July 17, 1996

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

Dear Dr. Krebs:

In your March 25 charge letter you asked FESAC to carry out an Alternative Concepts Review and in particular to "consider the fundamental investment strategy that we should use in funding alternative concepts." You specifically asked that the following issues be addressed:

1) Review the present status of alternative concept development in light of the international fusion program;
2) Produce an overall plan for a U.S. alternative concepts development program including experiments, theory, modeling/computation and systems studies, which is well integrated into the international alternative concepts program; and
3) Provide an interim assessment of the readiness of the spherical tokamak concept to move to "proof-of-principle" level experimentation.

Interim findings and recommendations with regard to the spherical tokamak assessment were provided in a letter to you in May. This letter and the report to be transmitted to you under separate cover respond to the first two alternative concepts charges.

In response to the charges, our Scientific Issues Subcommittee (SciCom) established in March an Alternative Concepts Review Panel, chaired by Professor Farrokh Najmabadi and including seven members of

SciCom plus additional experts from national laboratories and universities. Three prominent scientists from the international fusion community served as consultants to the Panel. The panel interacted with proponents of the various alternative concepts through a variety of solicited written input and presentations, and welcomed unsolicited input as well at a sequence of four meetings of the panel. They also set up a world-wide-web home page of alternative concepts assessment papers and input from the community. The FESAC wishes officially to thank the members of the panel for their work, and the alternative concepts researchers who provided such extensive input on relatively short notice.

As pointed out in FESAC's January 27 report on a restructured fusion program, the history of alternative concepts research has been rich in discoveries and innovations of significance to fusion plasma physics in general and tokamaks in particular. In addition, in a science-driven program with a constrained budget in the coming years, research on alternative concepts provides a special niche for the U.S. helping us maintain excellence and leadership in fusion research within the worldwide fusion program.

The Panel finds that a sound investment strategy for the fusion program includes a Concept Development Program (inclusive of tokamaks and alternatives) with emphasis on science and innovation. In order to develop an overall strategy, the panel developed four criteria to measure the benefit of the research. They are:

1) advancement of general plasma physics;
2) advancement of fusion plasma physics;
3) contributions to fusion energy development; and
4) development of candidates for fusion power plants.

The panel also provides a classification of alternative concept programs based on their maturity and size:

1) Concept Exploration;
2) Proof of Principle;
3) Proof of Performance and Optimization;
4) Fusion Energy Development; and
5) Fusion Demonstration Power Plant.

They also identified the required mix of experimental facilities, theory and modeling, and concept evaluation and power plant studies efforts at each level. The Panel notes that for programs at early stages of development, the major benefits of research are in advancing general and fusion plasma physics. At more mature stages, the emphasis shifts towards contributions to fusion energy development and power plants.

In devising an implementation of the envisaged strategy for alternative concepts research the Panel finds that such a program must consider many concepts, each of which has its own unique and challenging issues. These concepts span a wide range in terms of level of development. In such a program there is a need to base the program priorities on a strong scientific foundation. To this end, the Panel recommends forming a “Concept Development Panel” (CDP). This CDP can be a subcommittee of the FESAC to provide consensus scientific input and recommendations on the directions and priorities of alternative concepts research. This process is used in parts of NSF and NIH, and represents an experiment in community governance. If successful, it can be extended to cover the entire concept development program (including both tokamaks and alternatives).

The Panel reviewed the status of alternative concepts and provided detailed reports on five of the more developed ones. Until the CDP is constituted and charged with providing scientific input on priorities, the Panel provides the following recommendations for fiscal year 1997 (not in priority order):

1) Expansion of the Concept Exploration Activities to encourage science and innovation in alternative concepts;
2) Initiation of a proof-of-principle program in the spherical tokamak (ST) area, and construction of new ST experimental facilities;
3) Strengthening and broadening of the existing reversed field pinch (RFP) program;
4) An expanded stellarator program including theoretical studies, concept development, and collaborations on international experiments; and
5) Establishment of a vigorous theory activity in alternative concepts.

The Panel reiterates the point made in the FESAC report of January 27, 1996 that any alternative concept experiment “should be operated with healthy funding to operate cost-effectively.” This policy coupled with the recommended activities for fiscal year 1997 has the potential to result in exciting scientific discoveries of significance for the mission and goals of the restructured fusion program.

Lastly, the Panel notes that programmatic and cultural distinctions exist between alternative and mainline concepts. These distinctions serve no useful scientific purpose and have caused considerable difficulties. The Panel and FESAC recommend that the OFES and the fusion community eventually remove these distinctions and focus on a seamless concept development program (including tokamaks and alternatives), with the decision to expand or reduce the research effort in any concept based solely on its contributions to the goals of the restructured fusion program.

The FESAC endorses the principles, processes and recommendations cited above and will transmit the full Panel report to you under separate cover.

Sincerely,

Robert W. Conn,
Chairman on behalf of the
Fusion Energy Sciences
Advisory Committee

Charge to the Fusion Energy Advisory Committee for an Alternative Concepts Review

In its report to DOE of January 27, 1996, the Fusion Energy Advisory Committee (FEAC) recommended that a review of Alternative Concepts be carried out as part of making the transition to a Fusion Energy Sciences Program. This review should fundamentally be directed at recommending an investment strategy for funding alternative concepts. What criteria, in addition to scientific excellence, should determine the effort devoted to the Alternative Concept Program (for example, similarity to or difference from the tokamak, power density, size, etc.)? Within the general guidelines of this recommendation, the Department requests the FEAC to organize and conduct such a review as expeditiously as possible, using whatever approach it deems most appropriate. Although FEAC recommended that inertial fusion energy (IFE) should be considered as part of the alternative concepts review, the Department recognizes the distinct characteristic of IFE and will request a review of IFE in a separate charge.

It is generally recognized that the various alternative concepts are at significantly different levels of development. Within this context, the review should address the following:

1. Review the present status of alternative concept development in light of the international fusion program. As part of this review, consider not only the prospects for alternative concepts as fusion power systems but also the scientific contributions of alternative concept research to the Fusion Energy Sciences Program and plasma science in general.

2. The review should produce an overall strategy for a U.S. alternative concepts development program including experiments, theory, modeling/computation and systems studies, which is well integrated into the international alternative concepts program. The U.S. plan and supporting documentation should include but not be limited to:

   o recommendations on how best to collaborate in alternative concepts where our international partners already have large experiments (e.g., the stellarator),

   o recommendations for encouraging new innovations in alternative concepts,

   o a methodology for assessing on a comparative basis the scientific progress of alternative concepts in their early stages of development, and

- a set of criteria for use in determining when an alternative concept is ready to undertake a "proof-of-principle" scale experiment. For this purpose, consider the Princeton Large Torus as the proof-of-principle experiment that validated the tokamak concept.

3. The spherical tokamak is recognized to be a scientifically advanced alternate. Based on the FEAC recommendations to enhance research on alternative concepts, the FY 1997 budget request contains proposed funding for the National Spherical Tokamak Experiment (NSTX) at Princeton. An experiment of this size and scope could be considered a "proof-of-principle" for this concept. There are several ongoing spherical tokamak programs and several new grant applications also under review. We are not asking you to review any specific proposals. Rather an assessment of the readiness of this concept to move to "proof-of-principle" experimentation would provide a useful example to be carried out early in the overall review process. This assessment should specifically address, in the international context, the present theoretical understanding and experimental data base of the spherical tokamak concept. In addition, the potential for such spherical tokamak research to resolve key physics and technology issues of importance to both the conventional tokamak and the spherical tokamak as a reactor in its own right should be considered.

The FEAC's findings and recommendations with regard to the spherical tokamak assessment should be delivered to the Director of Energy Research by mid-April. The overall review of alternative concepts should be delivered by mid-July.

ALTERNATIVE CONCEPTS

A Report to the Fusion Energy Sciences Advisory Committee

by

The FESAC-SciCom Alternative Concepts Review Panel:

Prof. Farrokh Najmabadi* (Chair)	University of California, San Diego
Prof. James Drake	University of Maryland
Prof. Jeffrey Freidberg	Massachusetts Institute of Technology
Dr. David Hill	Lawrence Livermore National Laboratory
Prof. Michael Mauel	Columbia University
Prof. Gerald Navratil*	Columbia University
Dr. William Nevins*	Lawrence Livermore National Laboratory
Dr. Masayuki Ono	Princeton Plasma Physics Laboratory
Prof. Stewart Prager*	University of Wisconsin, Madison
Prof. Marshall Rosenbluth*	University of California, San Diego
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July 1996

* Member of FEAC-SciCom

† Dr. Solano resigned from SciCom and this Panel before this report was completed.

Contents:

Executive Summary

The term "alternative concepts" refers to plasma confinement configurations other than the standard or advanced tokamak that is the focus of the worldwide tokamak program. The most important reason for studying various concepts (alternatives in addition to tokamaks) 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 in alternative concepts of significance to tokamaks and fusion plasma physics in general are numerous. They include the discovery of 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. In fact, a fusion power plant will likely draw on the broad-based fusion sciences foundation that comes from experimental and theoretical studies in a variety of plasma confinement approaches including "alternative concepts."

Alternative concept research should be pursued even in a schedule-driven program because of the time-scale of fusion energy development. Long-term research and development programs like fusion must retain breadth and flexibility to incorporate changes that will certainly occur. It is premature to narrow the options to one concept even in a schedule-driven program. We, therefore, find that *a sound investment strategy for the fusion program focuses on a concept development program which includes both tokamaks and alternatives with emphasis on science and innovation. The decision to expand the research effort in any concept should be solely based on its contributions to the goals of the restructured fusion program and on the evaluation of specific proposals.*

Given the scarcity of resources, the plan for the concept development program must include a methodology to prioritize among many scientifically interesting and worthwhile proposals for research so that maximum benefit for the fusion program can be obtained, *i.e.,* an investment strategy. In order to develop an overall strategy, the Panel developed four criteria to measure the benefit of the research; they are: 1) advancement of general plasma physics; 2) advancement of fusion plasma physics; 3) contribution to fusion energy development; and 4) development of candidates for fusion power plants. We have also developed a categorization of the stages of development of concepts based on their level of maturity and program size, and identified the mix of experiments, theory and modeling, and power plant and design studies for each stage. They are: 1) Concept Exploration; 2) Proof-of-Principle; 3) Proof of Performance and Optimization; 4) Fusion Energy Development; and 5) Fusion Demonstration Power Plant. We note that for programs at early stages of development, the major benefits of research are in advancement of general and fusion plasma physics. At more developed stages, the emphasis shifts toward contributions to fusion energy development and power plants.

The peer-review process is the most objective way to review and judge the scientific merits of each proposal on its own right and should always be used. The difficulty lies in that each concept is unique, has its own set of challenging physics and technology issues, and different concepts are in vastly different stages of development. It is essential to set up a mechanism to periodically review and refine the status of each alternative concept, update its development plan, judge if the concept is ready for further development or should be terminated, and provide scientific recommendations on priority and balance in research among various concepts. This is the best way to ensure maximum return on the investment of talent and resources. To this end, the Panel recommends that *a continuing "Concept Development Panel" (CDP) should be constituted under the auspices of FESAC to provide consensus scientific input and recommendations on the direction and priorities of the concept development program research in the United States to FESAC and DOE.* This model parallels processes used in parts of NSF and NIH. In addition, community involvement: 1) will help avoid miscommunications between the OFES and the community (such as the perception that alternative-concept research has a low priority); 2) will be widely perceived as open and receptive to innovation and new ideas; and 3) will act as an experiment in community governance which can be extended if it proves to be successful. We also believe that *establishment of yet another subcommittee of FESAC would be unnecessary if SciCom is charged to also act as the CDP.* This is consistent with SciCom mission, "to provide an important channel of communication from the full breadth of the fusion community to FESAC, and to provide the best possible scientific input for priority setting." In addition, because of SciCom overview of all scinetific issues in the national fusion program, it is the logical choice as the concept development program is extended to include all confinement concepts, as recommended by FESAC and this Panel.

In developing the process for providing scientific input to the planning and implementation of the concept development program, we have taken every step possible to avoid unnecessary duplication of effort and additional bureaucracy. We have left, therefore, a large amount of discretion to the CDP in the process described below. On the other hand, we believe that the CDP should not replace the normal peer review of proposals; rather it should set the priorities after the peer-review process has established proposals to be scientifically sound. We understand that this process cannot be implemented immediately since there are a large number of procedural issues to be resolved (such as dates of reviews, proposal solicitations, *etc*.). We, therefore, recommend *that the DOE ask FESAC to establish a CDP (or charge SciCom) as soon as possible so that a smooth transition can be arranged*. Our recommendation on the role of the CDP is given in Section 5.3.

As part of the charge we were asked to review the status of alternative concept research but not review specific proposals. In order to focus the discussion, the Panel generated a set of standard questions (Section 6) for each alternative concept and asked presenters to provide written answers to those questions in the form of assessments which included information on the status of the concept, the critical issues, a research plan, and the benefits of the research. For more developed concepts, the Panel has provided a summary and critique of these assessment papers. We found this information very useful. The collection of these assessment papers (further refined to include references to publications, for example) provide a complete summary of the current status of alternative concept research. We recommend that *the CDP update these papers on a yearly basis with the CDP having the option of endorsing those prepared by the proponents and/or providing a critique*.

In Section 6, we have provided reviews of five of the more-developed concepts: spherical tokamaks, stellarators, reversed-field-pinches, field-reversed configurations, and spheromaks. While we have not provided a summary for each less-developed alternative concept that was presented to us, the presentation by the community clearly demonstrated that there exists a large number of interesting and intriguing ideas to be studied at the concept exploration stage.

Until the CDP is constituted and charged with providing scientific input on priorities for the concept development program, we provide the following interim recommendations:

A healthy alternative concepts program requires an increase in funding as proposed in the FY97 Presidential budget and should include in FY97 (not in priority order):

1) Expansion of the Concept-Exploration Program to encourage science and innovation in alternative concepts;

2) Initiation of a spherical tokamak proof-of-principle program and construction of new spherical tokamak experimental facilities;

3) Strengthening and broadening of the existing reversed-field pinch (RFP) program;

4) An expanded stellarator program including theoretical studies, concept development, and collaboration on international experiments; and

5) Establishment of a vigorous theory program in alternative concepts research.

We have made specific recommendations for the spherical tokamak, RFP, and stellarator concepts among the large array of alternative concepts because of their relative scientific maturity, recent advances, and identified approaches for near-term progress. Less-developed concepts should be considered under an expanded Concept-Exploration Program. We also note that existing alternative concept experiment should be operated with adequate funding to operate cost effectively, as recommended in the FESAC January 1996 restructuring report.

1. INTRODUCTION

In the letter of March 25, 1996, The Department of Energy (DOE) Director of Energy Research, Dr. Martha Krebs, asked the Fusion Energy Sciences Advisory Committee (FESAC) to organize and conduct a review of alternative fusion concepts and report to the Department by mid July 1996. Dr. Krebs asked that "This review should fundamentally be directed at recommending an investment strategy for funding alternate concepts." In addition, she asked that "the review should address the following:

1. Review the present status of alternative concept development in light of the international fusion program.
2. The review should produce an overall plan for a United States alternative concepts development program including experiments, theory, modeling/computation and systems studies."

Recommendations were also sought for (a) international collaborations, (b) encouraging new innovations, (c) a methodology for assessing the progress of alternative concepts, and (d) a set of criteria for proceeding to a "proof-of-principle" scale experiment.

The full text of the charge letter is given in the Appendix. The charge letter also asked for an interim report on the status of the spherical-tokamak concept by mid April. The text of the response to the interim Spherical Tokamak charge is also given in the Appendix.

In response, the continuing Subcommittee on Scientific Issues (SciCom) of the Fusion Energy Sciences Advisory Committee (FESAC) established an Alternative Concepts Review Panel which included seven members of SciCom and additional members from universities and national laboratories. Three prominent scientists from overseas participated in panel deliberations; they reviewed and commented on panel writings.

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

Throughout this report, we use "Alternative Concepts" to refer to confinement configurations other than the standard and advanced tokamaks that are the focus of the worldwide tokamak program. Inertial Fusion Energy was excluded from the charge to FESAC because a separate panel is charged in this area. The Alternative Concepts Review Panel, however, heard several presentations on magnetized target plasmas that marry certain aspects of magnetic and inertial confinement fusion.

2. PANEL ACTIVITIES

The Panel met three times: on March 26-27 (Washington DC), April 23-24 (Chicago), and June 6-7, 1996 (UC San Diego). In addition to panel discussions on devising an overall plan for an alternative concepts development program, about half of each meeting was devoted to reviewing the status of selected alternative concepts. In order to focus the discussions, the Panel generated a set of standard questions (Section 6) for each alternative concept and asked presenters to provide written answers to those questions in the form of assessment papers to the Panel. In the March meeting, we reviewed the spherical tokamak concept in response to the interim charge on spherical tokamaks. At the April meeting we heard presentations on stellarator, field-reversed-configuration (FRC), spheromak, and reversed-field pinch (RFP) configurations. The June meeting was devoted to less-developed concepts and nine presentations were made to the Panel. (Agendas for the three meetings are also included in the Appendix). The Panel would like to thank everyone who provided input to us.

The Panel maintained a World Wide Web site for the Panel activities. The fusion community was thereby kept informed of Panel activities and directed to the Web site for up-to-date information. We solicited input and indeed received many written comments and assessment papers; they are listed in the Appendix. The full text of these comments and assessment papers can be found on the Panel Web Site (http://aries.ucsd.edu/SCICOM/AC-PANEL/index.html)

3. BACKGROUND

Fusion research in the United States and worldwide has historically pursued many approaches to magnetic confinement. The tokamak concept in the late 1960's proved to have superior confinement compared to other experimental devices at that time and became the focus of fusion research worldwide. Research on alternative concepts, however, was continued. Over the intervening years, some of these concepts proved to be unsuccessful and were terminated. In addition, as the real Research and Development (R&D) resources declined in the late 1980's, it became increasingly difficult to maintain a wide spectrum of alternative concepts. Still, a healthy but modest level of research in alternative concept was carried out in the United States through the 1970's and 1980's.

In the fall of 1990, faced with a Congressional cut of $50M in the FY 1991 budget, the alternative concepts program was essentially terminated in favor of a schedule-driven development of the tokamak concept. Although $25M of the cut was restored, the DOE Office of Fusion Energy (which has recently been renamed the Office of Fusion Energy

Sciences, OFES) followed through with its original decision. In 1992, OFES was reevaluating its policies regarding alternative concepts and the DOE asked the FEAC for recommendations on alternative concept research. Subsequently, FEAC Panel 3 prepared a report on concept improvement. An excerpt from this report (on its page 2), summarizes the status of alternative concept research in the United States at the time, following the OFE decision to essentially eliminate the alternative concepts program. “Subsequent statements and communications by the Department led to the perception in the fusion community that proposals for research on non-tokamak concepts would not be supported by OFE, and should not be submitted. The only way that proposals on non-tokamak devices would be accepted for consideration was if the work was cast in the form of direct support for tokamak research. The rationale given was that research on competing concepts could not be supported, since, even if the research were successful, no funds would be available to develop the concept to its next, more expensive stage; thus it would be best not to begin."

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

While certain elements of the FEAC Panel 3 recommendations were implemented, such as the call for "a small, but formal and highly-visible annual competition to foster new ideas," (one competition was held and three proposals were selected and funded), the community retained the above impression *(i.e.,* alternative-concept research has a low priority) until the FESAC report in January 1996. Subsequent decisions by OFES to allocate increased funding for alternative concept research and to proceed with a proof-of-principle-class spherical tokamak device in the FY 97 Presidential Budget Request for magnetic fusion energy has helped the situation.

The FESAC, in its January 1996 report, "A Restructured Fusion Energy Sciences Program," recommended a healthy alternative concepts program and that a review of Alternative Concepts be carried out as part of making the transition to a Fusion Energy Sciences Program. The FESAC report states, "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 Presidential 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'." The current charge to FESAC is in response to FESAC recommendations in its report, "A Restructured Fusion Energy Sciences Program," and we have relied considerably on that report.

4. GENERAL PRINCIPLES

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 most important reason for studying 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 in alternative concepts of significance to mainline tokamaks and fusion plasma 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). In fact, a fusion power plant will likely draw on the broad-based science foundation that comes from experimental and theoretical studies in a variety of plasma confinement approaches, including "alternative concepts."

Alternative concept research should be pursued even in a schedule-driven program because of the time-scale for fusion energy development. Comparing our understanding of plasma physics and the status of enabling technologies with what was available even 20 years ago underscores the fact that fusion science and technology 20 years from now will certainly be quite different from today. Long-term research and development programs like fusion must retain breadth and flexibility to incorporate changes that will certainly occur. It is premature to narrow the options to one concept even in a schedule-driven program.

As stated in the FESAC report, "Re-initiation of an alternative concepts research program will increase the breadth of plasma research and the emphasis on science and innovation." This helps on several fronts. First, the resulting diversity will increase the visibility and impact on the larger scientific community. Second, under the constrained budgets anticipated in coming years, alternative concepts research is an area in which the United States can maintain excellence within the world context, with modest expenditures. Third, long-term programs like fusion depend on a continual inflow of new and younger talent. A broad program that encourages innovation causes the fusion program to be clearly seen as exciting and inspiring to new generations of scientist and engineers.

We, therefore, find *that a sound investment strategy for the fusion program must focus on a concept development program which includes both tokamaks and alternatives with emphasis on science and innovation. The decision to expand the research effort in any concept should be based solely on its contributions to the goals of the restructured fusion program and the evaluation of specific proposals.*

As mentioned in the FESAC report (page 21), the division of fusion research into mainline tokamaks and alternatives is historical and problematical. It is historical since during the 1970's and early 1980's, this distinction was made to "protect" research in new concepts from mainline approaches at the time (tokamaks and mirrors). It is problematical since it understates the strong plasma 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. It is of interest to note that second-stability tokamaks (currently a version of advanced tokamaks) were considered an alternative concept in the early 1980's. The distinction between alternative and mainline concepts serves little useful purpose and indeed has caused considerable difficulties. We, therefore, recommend that *the fusion energy sciences program and the fusion community strive to remove any programmatic and cultural distinctions between confinement concepts as mainline and alternatives and focus on a concept development program (including tokamaks and alternatives). The decision to expand the research effort in any concept should be based solely on its contributions to the goals of the restructured fusion program and the evaluation of specific proposals.*

The above principle has also been recommended by the FESAC restructuring report, which supports a programmatic unification of research on all confinement concepts. The FESAC report states that "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." The FEAC Panel 3 also included alternative concept research as part of the "Concept Improvement" program.

We have made every effort to ensure that the overall plan for alternative concepts research we have developed can be readily extended to a concept development program, which includes tokamaks and have used the phrase, "concept development program," instead of "alternative concept program" whenever possible. The Panel appreciates that this transition to a "seamless" concept development program may take two to three years.

5. CONCEPT DEVELOPMENT PROGRAM STRATEGY

The charge to the Panel asked for recommendations on "an overall plan for a U.S. alternative concepts development program," which includes developing: (a) a methodology for assessing alternative concepts at early stages of development; (b) a set of criteria for determining when an alternative concept is ready to undertake a 'proof-of-principle' scale experiment; and (c) ways to encourage new innovation in alternative concepts. Given the scarcity of resources, the plan for the concept development program must include a methodology for prioritizing among many scientifically interesting and worthwhile proposals for research so that maximum benefit for the fusion program can be obtained, *i.e.,* an <u>investment strategy</u>.

In order to devise a sound strategy for concept development research, the anticipated scientific benefits should first be stated (Section 5.1). Because the confinement concepts are in different stages of developments, a categorization of these stages is needed (Section 5.2) to identify the best mix of facilities and activities for the program. The peer-review process is the most objective way to review and judge the scientific merits of each proposal in its own right and should always be used. However, in a science-oriented program involving many new concepts that span a wide range in their level of development, there is a need to base the overall program priorities on a strong scientific foundation. This is the best way to ensure maximum return on the investment of talent and resources. It is essential to set up a mechanism to periodically review and refine the status of each alternative concept, update its development plan, judge if the concept is ready for further development or should be terminated, and provide scientific recommendations on priority and balance in research among various concepts. We recommend that a continuing committee of experts from the community be set up in order to provide the needed scientific recommendation to OFES (Section 5.3). This is consistent with FESAC recommendations in the "A Restructured Fusion Energy Sciences Program" report (page 12) which states that the governance system for the restructured Fusion Energy Sciences Program needs to "establish an open process for obtaining scientific input for major decisions, such as planning, funding, and terminating various facilities, projects, and research efforts."

5.1. Anticipated Benefits

In order to devise a sound strategy for concept development research, the anticipated scientific benefits should first be stated. The mission and intent of the restructured fusion program, as highlighted in the FESAC report, guided us in this area. We have divided the anticipated benefits from the concept development research into four broad criteria:

(1) Advancement of general plasma physics;

(2) Advancement of fusion plasma physics, including addressing issues specific to a concept as well as generic issues applicable to many or all fusion concepts;

(3) Contribution to fusion energy development, including addressing issues such as burning plasma physics and development of fusion technologies; and

(4) Development of candidates for fusion power plants.

The Panel does not believe that the potential to become an attractive fusion power plant should be used as a litmus test for fusion concepts that are at early stages of development. First, given the vastly different degrees of understanding between different concepts and degrees of extrapolation required to estimate the potential of a concept as a fusion power plant, such a test is arbitrary and not useful. Second, even those concepts that may prove to be unattractive as fusion power plants may provide understanding of key issues that may help other concepts mature. Rather, in early stages of development of concepts, the major benefits of research are in advancing general and fusion plasma physics (the first two criteria). At later stages of development, the emphasis gradually shifts towards fusion energy development and power plants (the latter two criteria).

While advancement of general plasma physics is included as a criterion in assessing the contributions of research to the goals of the fusion program, the Panel believes that research which is aimed solely at advancing general plasma physics should be funded under "basic plasma physics" research of OFES.

5.2. Stages of Concept Development

We envision that each concept will pass through five stages of development:

1) Concept Exploration;

2) Proof-of-Principle;

3) Proof of Performance and Optimization;

4) Fusion Energy Development; and

5) Fusion Demonstration Power Plant.

Scientifically, these stages of development of a concept represent points on a continuous scale. However, pragmatically, the boundaries between various stages usually represent quantum changes in the cost of program, in the level of commitment to that concept, and in the focus of the program. In each stage, the research program contains experiments, theory, and power-plant studies elements. The mix of these elements vary in each stage, but at least one main experiment is needed, *i.e.*, a Proof of Performance and Optimization Program for a concept contains at least one Proof-of-Performance-class experiment, and possibly some Proof-of-Principle-class and Concept-Exploration-class experiments and an array of supporting theory, power-plant and design studies, and technology development necessary for that concept.

These stages of concept development are defined in detail below. The decision to proceed from one stage to the next should be based on the maturity of the concept in order to be reasonably confident that: 1) the next stage of the program will be successful; and 2) the anticipated benefits of the next stage of the research justifies the increased level of effort.

Concept Exploration

These programs are aimed at innovation and basic understanding of relevant scientific phenomena. They consist of experiments (costing typically less than $5M/year per device) and/or theory and strive at establishing: 1) the basic feasibility of a concept (for a toroidal confinement system, these issues include basic existence of equilibrium and gross stability, rough characterization of confinement, initial demonstration of heating, existence of particular magnetic topologies for power and particle control, *etc.*); and/or 2) exploring certain phenomena of interest and benefit to other concepts. Power plant scoping should be limited to demonstration of net energy gain in a fusion plasma and identification of potential advantages/disadvantages since reliable scaling information for extrapolation to fusion plasmas would not be available.

Many independent experiments and theory activities are preferred at this level and can be attempted in parallel, each focusing on a small set of issues. High risk, large payoff research is desirable and should be encouraged. Activities should be of short duration (less than 3 years, requiring renewal after a 3 year period) in order to allow for a high turnover rate.

The major benefits of these programs are in encouraging innovation and advancing general and fusion plasma physics.

Proof-of-Principle

This is the lowest cost program aimed at developing an integrated and broad understanding of basic scientific aspects of the concept which can be scaled with great confidence to provide a basis for evaluating the potential of this concept for fusion energy applications. Experimental activity in this step requires at least one device with a plasma of sufficient size and performance ($5 to $30M/year) that a range of physics issues can be examined. For example, for a toroidal confinement system, the plasma should be hot enough and large enough to generate reliable plasma confinement data, explore MHD stability, examine methods for plasma sustainment, and explore means of particle and power exhaust. The diagnostic set must be comprehensive enough to measure the relevant profiles and quantities needed to confront the physics. Proof-of-Principle experimental results are probably far from the fusion-relevant regime in absolute parameters but provide initial data for scaling relationships useful in establishing a predictive capability for the concept. It is beneficial for the Proof-of-Principle program to include Concept-Exploration-class experiments which focus on certain key issues of the concept and help promote further innovations. Theory, modeling, and benchmarking with experiments should be vigorously pursued in order to provide a theoretical basis for scaling the physics of the concept and evaluating its potential. Power-plant studies, including in-depth physics and engineering analysis, should be carried out to identify key physics and technological issues and help define the research program. Any technological issue specific to the concept should also be addressed during the Proof-of-Principle stage.

The construction, operation, and analysis of a Proof-of-Principle-class experiment takes roughly eight to ten years which sets the lower bound on the duration of a Proof-of-Principle program. Furthermore, substantial resources are necessary to operate a Proof-of-Principle-class experiment. These programs, therefore, should be national endeavors, drawing expertise from many institutions. Sufficient resources should be committed both to the Proof-of-Principle-class device as well as the supporting smaller experiments, theory and modeling, and power-plant studies in order to ensure a healthy return on the investment of the talent as well as resources in such an activity.

The major benefits at this stage are advancement of fusion plasma physics with some contribution to fusion energy development and power plants.

Proof-of-Performance and Optimization

The Proof-of-Performance programs explore the physics of the concept at or near the fusion-relevant regime in absolute parameters albeit without a burning plasma. This stage aims at generating sufficient confidence so that absolute parameters needed for a fusion development device can be achieved and a fusion development program with a reasonable cost can be attempted. At this stage, the physics of the concept and the scaling information is refined further, new physics in fusion-relevant regimes is examined, and the performance of the concept is optimized. Because of the demand on absolute performance, usually a large single device ($50-100M per year) is needed which is equipped with a variety of auxiliary systems for control and operational flexibility as well as extensive diagnostics providing complete coverage in space and time. This program should contain Concept-Exploration-class and possibly Proof-of-Principle-class experiments to help in optimization of the concept. Extensive theory and modeling activities should exist to analyze the experimental results on all issues and start providing a predictive capability for the concept. Both power-plant and design studies, including in-depth physics and engineering analyses, should be carried out to focus on critical issues, help in optimizing the physics regimes, and evaluate the potential of the concept for fusion development and power plants. As with the Proof-of-Principle program, this must be a national endeavor, which should include expertise from many institutions and sufficient resources allocated for supporting activities.

The major benefits at this stage are contributions to fusion energy development and power plants, and advancement of fusion plasma physics.

Fusion Energy Development

This program is aimed at developing the technical basis for advancing the concept to the power plant level in the full fusion environment. It includes devices such as ignition experiments, volume neutron sources, or pilot plants. The physics research is mainly connected with charged fusion products and the production of substantial fusion power (high stored energy, disruptions, high-power exhaust, steady-state particle and power control, *etc.*). Fusion technology issues (blankets, activation, maintenance, to name a few) should be resolved by this program in a way that is directly applicable to a power plant. These devices must also develop the data base on operational reliability and maintainability, safety and licensing, and costing to justify a demonstration power plant.

The major benefits at this stage are contributions to fusion energy development and power plants, as well as some advancement of fusion plasma physics.

Fusion Demonstration Power Plant

The device(s) at this stage is constructed to convince the electric power producers, industry, and the public that fusion is ready for commercialization. These are effectively scaleable power plants with the same physics and technology as envisioned for a commercial power plant. There should be no remaining physics issues to be addressed in these devices and their operation should demonstrate that technological development of previous stages has been successful.

5.3. Scientific Planning of the Concept Development Program

As mentioned before, the peer-review process is the most objective way to review and judge the scientific merits of proposals and should always be applied. However, peer-review of one proposal does not provide sufficient information on the relative priority among many proposals, especially those of different concepts with different scientific issues and at different stages of development. It is, therefore, essential to set up a mechanism to periodically review and refine the status of each alternative concept, update its development plan, judge if the concept is ready for further development or should be terminated, and provide scientific recommendations on priority and balance in research among various concepts. We recommend that a continuing committee of experts from the community be set up in order to provide the needed scientific recommendations to OFES. This is consistent with FESAC recommendations in its January 1996 report, "A Restructured Fusion Energy Sciences Program" (page 12) which states that the governance system for the restructured Fusion Energy Sciences Program needs to "establish an open process for obtaining scientific input for major decisions, such as planning, funding, and terminating facilities, projects, and research efforts." In addition to providing up-to-date scientific assessments, community involvement will help avoid miscommunications between the OFES and the community (such as the perception that alternative-concepts research has a low priority), will be widely perceived as open and receptive to innovation and new ideas, and will act as an experiment in community governance that can be extended if it proves to be successful.

To this end, the Panel recommends that *a continuing "Concept Development Panel" should be constituted under the auspices of FESAC to provide consensus scientific input and recommendations on the direction and priorities of the concept development research in the United States to FESAC and DOE.* This model parallels processes used in parts of NSF and NIH. Membership of the Concept Development Panel (CDP) should be for 3 years, with one-third of the members changing each year to provide both continuity and new ideas. This is similar to the model adopted for SciCom. We also believe that *establishment of yet another subcommittee of FESAC would be unnecessary if SciCom is charged to also act as CDP.* This is consistent with the SciCom mission, "to provide an important channel of communication from the full breadth of the fusion community to FESAC, and to provide the best possible scientific input for priority setting." In addition, since SciCom has a broad overview of the national fusion program, it is the logical choice as the concept development program is extended to include all confinement concepts as recommended by FESAC and this Panel.

In developing the process for providing scientific input to the planning and implementation of the concept development program, we have taken every step possible to avoid unnecessary duplication of effort and additional bureaucracy. We have left, therefore, a large amount of discretion to the CDP in the process described below. On the other hand, we believe that the CDP should not replace the normal peer review of proposals; rather, it should set the priorities after the peer-review has established the proposals to be scientifically sound. We understand that this process cannot be implemented immediately since there are a large number of procedural issues to be resolved (such as dates of reviews, proposal solicitations, *etc*.). We, therefore, recommend that DOE ask FESAC to establish the CDP (or charge SciCom) as soon as possible so that a smooth transition can be arranged.

During the activity of our Panel, we generated a "standard set of questions" to be addressed by various presenters to the Panel (included in the Appendix). For each concept, proponents produced an assessment paper that included information on the status of the concept, the critical issues, a research plan, and the benefits of the research. For more developed concepts, the Panel provided a summary and critique of these assessment papers. We found them to be very useful. The collection of these assessment papers (further refined to include references to publications, for example) provide a complete summary of the status of alternative concept research. We believe that it would be relatively easy to update these papers on a yearly basis (if the status of the concept has changed) with the CDP having the option of endorsing the paper prepared by the proponents and/or providing a critique. These yearly documents will become a record of alternate concept research and could serve many useful purposes, including providing research plans for various concepts, lists of critical issues, and a historical record of progress for each concept. We, therefore, recommend that *the Concept Development Panel maintain a set of assessment papers on each concept, published annually as a document on the status of concept development program.* Obviously, the extent of these assessment papers depends on the maturity of the concept and the size of the research program.

Lastly, in developing the process for the CDP activity, we have limited ourselves to Concept-Exploration and Proof-Of-Principle programs since almost all of the alternative concepts fall in those categories. We believe that this process can be readily extended to review Proof-of-Performance programs. However, the decision to embark on new Proof-of-Performance Programs and beyond (*i.e.,* construction of large facilities) are of such magnitude that a mechanism other than CDP (such as FESAC or special panels of FESAC) should be sought.

In the following sections, we further elaborate the goals and characteristics of these two classes of programs, and establish a set of recommendations for the processes by which proposals will be reviewed and the CDP arrives at its recommendations.

5.3.1. Process and Criteria for Concept-Exploration Programs

A. General Principles

1. Proposals should focus on experiments and/or theory and strive at establishing the basic feasibility of a concept and/or exploring certain phenomena of interest and benefit to other concepts. Pure theory proposals should be accepted.

2. The Concept-Exploration program should be dynamic with a rapid turnover to ensure continuing innovations and new ideas. Therefore, each study should be of a limited duration (1 to 5 years) which is clearly stated in the original proposal. Milestones for progress should be identified. During the program life, continuing proposal and review are needed to monitor scientific progress on milestones during the project period. Projects reaching the end of their initial proposed life can be renewed. However, the application for renewal should be evaluated competitively with new proposals, so that the renewal process is qualitatively different from the continuing proposals and review.

3. It is expected that a portion of projects which did not meet expectations would be terminated each year in order to allow room for innovation and new ideas.

B. Review and Selection Process

1. Proposals for exploratory experiments or paper studies are submitted to OFES as is the case now. Proposals should contain an estimated lifetime for the work, milestones by which progress can be judged and continuation granted, and an assessment paper.

2. The OFES organizes peer reviews of these proposals as is the case now, with at least one member of the CDP participating in each review. The type of review (written or oral presentation and number of reviewers) should be governed by the size of the request. The outcome of the reviews are passed on to CDP for the overall program review, and funding decisions are deferred until the CDP recommendations are available.

3. The CDP meets once or twice a year to rank proposals for Concept Exploration which have been peer-reviewed during the previous period. Proponents of proposals with a cost exceeding $1M are allowed to make an oral presentations directly to the CDP. For review of proposals

of lower cost, the CDP can rely on written materials from the peer reviews and information from the CDP member which took part in the specific review.

4. The CDP ranks the new and renewal proposals and provides a consensus recommendation to FESAC and DOE as to which should receive funding and at what level so as to maintain the desired emphasis among different approaches to concept development.

5.3.2. Process and Criteria for Proof-of-Principle Programs

A. General Principles

1. Experimental activity in this step requires at least one device with a plasma of sufficient size and performance along with supporting Concept-Exploration-class experiments, theory and modeling, and power-plant studies.

2. The construction, operation, and analysis of a Proof-of-Principle-class experiment takes roughly eight to ten years, which sets a lower bound on the duration of a Proof-of-Principle program. Sufficient resources should be committed both to the Proof-of-Principle-class device, as well as the supporting smaller experiments, theory and modeling, and power-plant studies, in order to ensure a healthy return on the investment of the talent and as resources in such an activity. Once a decision is made to proceed with a Proof-of-Principle program, the OFES should seek to ensure that it receives adequate funding (barring a severe reduction of the national funding), even if this means delaying other Proof-of-Principle programs.

3. As with the Concept-Exploration programs, the Proof-of-Principle programs should include clear milestones for progress. During the program life, continuing proposals and reviews are needed to monitor scientific progress on milestones during the project period. Projects reaching the end of their proposed life can be renewed. However, the application for renewal should be evaluated competitively with new proposals, so that the renewal process is qualitatively different from the continuing proposals and review.

B. Review and Selection Process

1. In its annual report on the status of concept development research, the CDP provides a recommendation that a concept is ready for a Proof-of-Principle Program. If funding permits, OFES then issues a call for proposals, allowing open competition for participation in all elements of the new proof-of-principle program.

2. OFES organizes peer reviews of these proposals as is the case now, with at least one member of the CDP participating in each review. The outcome of these reviews are passed on to the CDP for the overall program review and funding decisions are deferred until the CDP recommendations are available.

3. The CDP reviews these proposals and provides a scientific assessment of each. The CDP also provides recommendations for an implementation strategy or strategies depending on available funding. The goal is to craft a Proof-of-Principle program that obtains complete resolution of the issues that must be resolved at this stage. In some cases, for example, it may be found that more than one experiment must be funded in order to obtain complete coverage of proof-of-principle issues. In the event that the proposals brought forward are collectively deficient in leaving some subsets of the issues unaddressed, the CDP will note these in its report and advise if further proposal solicitations are recommended.

6. STATUS OF ALTERNATIVE CONCEPTS

As part of the charge we were asked to review the status of alternative concept research, but asked not to review specific proposals. In the March meeting, we reviewed the spherical tokamak concept in response to the interim charge on spherical tokamaks. In the April meeting, we heard presentations on the stellarator, field-reversed-configuration (FRC), spheromak, and reversed-field pinch (RFP) research programs. The June meeting was devoted to less-developed concepts and nine presentations were made to the Panel. In order to focus the discussions, the Panel generated a set of standard questions for each of the alternative concepts and asked presenters to provide written answers to these questions in the form of assessment papers provided to the Panel. The questions were:

A) What is the current worldwide status of research and achievements:

- A1) What are the present levels of experimental achievements?
- A2) What is the present level of theoretical understanding?
- A3) Do theory, modeling, simulations, and empirical scalings fit the experimental observations?

B) What is the appropriate level of research for this concept:

- B1) What are the major experimental and theoretical issues that should be addressed?
- B2) Do the above issues require:
  - (a) launching new experimental facilities and/or theoretical activities?
  - (b) expanding the current experimental and theoretical activities?
  - (c) exploration at the present level of research?
  - (d) or can they be addressed at a lower level of research?
- B3) What is an appropriate mix of research activity for this concept among large facilities and mix of small supporting experiments, theory and modeling, and concept design and evaluation studies?

B4) What is the worldwide research plan (outside U.S.) to address the above issues?

B5) What is the proper level of U.S. research within the context of the international program? In particular:

(a) Is it necessary to have more than one new international experimental facility?

(b) Given the worldwide plan, which areas should the U.S. program focus on?

C) What is the potential impact of research on this concept on:

C1) increasing our knowledge of general plasma physics?

C2) increasing our knowledge of fusion plasma physics (of this concept as well as the physics of other confinement concepts)?

C3) helping develop fusion as an energy source (help develop the data base for fusion development steps such as burning plasmas, volumetric neutron source, *etc*.)?

C4) developing this concept as a candidate for a fusion power plant?

As mentioned before, for each concept, proponents produced an assessment paper which included information on the status of the concept, the critical issues, a research plan, and the benefits of the research. For more developed concepts, the Panel provided a summary and critique of these assessment papers. We found them to be very useful. The collection of these assessment papers (further refined to include references to publications, for example) provide a complete summary of the status of alternative concept research. We believe that it would be relatively easy to update these papers on a yearly basis with the CDP having the option of endorsing the paper prepared by the proponents and/or providing a critique. In the following sections, we have provided reviews of five of the more-developed alternative concepts, namely, stellarators, spherical tokamaks, reversed-field-pinches, field-reversed configurations, and spheromaks, including recommended programs for the U.S.

While we have not provided summaries for each of the less-developed alternative concepts that were presented to us, the presentation by the community clearly demonstrated that there exists a large number of interesting and intriguing ideas to be studied at the concept exploration stage. The full text of these assessment papers that we received from the community can be found on the Panel Web Site (http://aries.ucsd.edu/SCICOM/AC-PANEL/index.html).

Until the CDP is constituted and charged with providing scientific input on priorities for the concept development program, we provide the following interim recommendations which are based on detailed programs outlined in the next few sections:

A healthy alternative concepts program requires an increase in funding as proposed in the FY97 Presidential budget and should include in FY97 (not in priority order):

1) Expansion of the Concept-Exploration Program to encourage science and innovation in alternative concepts;

2) Initiation of a spherical tokamak proof-of-principle program and construction of new spherical tokamak experimental facilities;

3) Strengthening and broadening of the existing reversed-field pinch (RFP) program;

4) An expanded stellarator program including theoretical studies, concept development, and collaboration on international experiments; and

5) Establishment of a vigorous theory program in alternative concepts research.

We have made specific recommendations for the spherical tokamak, RFP, and stellarator concepts among the large array of alternative concepts because of their relative scientific maturity, recent advances, and identified approaches for near-term progress. Less-developed concepts should be considered under an expanded Concept-Exploration Program. We also note that existing alternative concept experiment should be operated with adequate funding to operate cost effectively as recommended in the FESAC January 1996 restructuring report.

6.1. Spherical Tokamak

The spherical tokamak (ST) is a low aspect ratio (A), axisymmetric torus. It has both a toroidal and a poloidal magnetic field with profiles qualitatively similar to a standard tokamak (although RB_ϕ is not approximately constant). The primary difference is geometrical, the ST having an aspect ratio $A \sim 1.3$ while in a standard tokamak $A \sim 3$. The long term motivation for considering low aspect ratio is the possibility that such configurations will lead to smaller, more compact fusion development steps and possibly reactors. Thus, the developmental path to fusion as well as the capital cost to build such reactors may be considerably reduced from the standard tokamak approach. The scientific attractiveness of the spherical tokamak is a consequence of its anticipated favorable MHD equilibrium and stability properties. This follows from the results of existing, small ST experiments, well established MHD theory, and the similarity of ST to the standard tokamak. In fact, standard tokamak MHD scaling laws indicate that higher MHD performance may be achieved at low aspect ratio. The ST approaches the low aspect ratio asymptotic limit of the generic tokamak configuration. A qualitative comparison of spherical and standard tokamaks is as follows.

Scientific advantages of the ST over the standard tokamak: The ST is expected to have higher MHD β limits. This follows because of the favorable aspect ratio scaling of β_{crit} the larger values of stable κ due to the natural elongation, and the increase in β_N with decreasing aspect ratio. There may, in addition, be an improvement in confinement near the outer portion of the plasma core because of the suppression of certain electrostatic and electromagnetic modes as the local value of *A* decreases.

Scientific disadvantages of the ST over the standard tokamak: Because of the low aspect ratio, the ultimate ST power plant will have no room for an ohmic-heating (OH) transformer. Thus, one must develop efficient techniques for non-inductive start-up, a requirement not relevant for standard tokamaks. Currently, helicity injection is being suggested, but the transition from this to a clean, high temperature, bootstrap-dominated equilibrium is at this point an unknown and untested approach. A second issue is that even with a high bootstrap fraction some steady state current drive and current profile control will be required. This is more uncertain at the plasma densities and magnetic fields characteristic of low aspect ratio where standard radio frequency (RF) wave current drive methods are ineffective. High harmonic fast waves have been suggested, but this too is a largely untested approach. Since standard tokamaks also require current drive, the ST disadvantage is not fundamental (as it is for non-inductive start-up) but rather reflects the fact that the suggested methods have yet to be proven experimentally.

Technological advantages of the ST over the standard tokamak: The main technological advantage is the achievement of high beta in a compact, low aspect ratio geometry. This feature can lead to improved safety margin against disruptions, higher power density, or a combination thereof. Equally important, compactness leads to a smaller unit size which reduces the overall developmental costs. Existing spherical tokamaks, START in particular, demonstrate a surprising aversion to hard disruptions, at least in the ohmic heating regime. This would be an important technological advantage should it carry over to future, larger, auxiliary heated STs. A further advantage is that while standard tokamaks can achieve values of $\kappa \sim 2$, a *naturally* elongated ST achieves the same values with substantially reduced requirements on the poloidal field (PF) system.

Technological disadvantages of the ST over the standard tokamak: Since the core of an ST power plant contains no blanket and a minimal, if any, shield, the toroidal field (TF) magnet, (at least its central leg) must be made with normal conductors, not superconductors. The central leg (conductor and insulator, if required) must be able to withstand the intense neutron wall loading for an economically adequate lifetime. Also, there will be significant joule heating of the coil that requires careful consideration since this can lead to a high recirculating power and a corresponding economic problem with the overall power balance. An equally important problem is enhanced heat load removal, a consequence of the anticipated higher power densities and compact divertor configuration. The heat load problem is common to most alternate concepts since, like the ST, they aim to achieve high power density.

Based on this evaluation, and the summary below describing the status of ST research in the U.S. and worldwide, the Panel agreed that the spherical tokamak is ready for a proof-of-principle experiment to be built in the U.S. with the goal of addressing the following issues:

1) Extension of the data base to determine the dependence of plasma confinement on aspect ratio and auxiliary heating;
2) Achievement of high beta by auxiliary heating;
3) Development of techniques for clean, efficient, non-inductive start-up;
4) Development of efficient current drive techniques for low aspect ratio;
5) Achievement of high bootstrap fraction in advanced operation; and
6) Long pulse, fully relaxed operation.

A. Worldwide Status of Research and Achievements

Experimental Achievements: Experimental progress in the spherical tokamak concept can be assessed by examining two main sources of information. First, ten experiments have been operated whose geometry is directly relevant to spherical tokamak physics. These are small experiments, equivalent to the "concept exploration" stage. The larger of these consist primarily of START (Culham), CDX-U (Princeton), and HIT (U. Washington), and there is the smaller MEDUSA device (U. Wisconsin). Although the results from these experiments are promising they would probably not by themselves justify proceeding to a "proof-of-principle" program. This decision is instead substantially motivated by the second source of information, the vast wealth of data accumulated over 25 years of tokamak research. The point is that even though the spherical tokamak has a tighter aspect ratio, it still shares many common features with standard tokamaks Thus, one expects that a great deal of the favorable physics of standard tokamaks would either carry over directly or perhaps in some cases even be improved upon.

Of the three experiments listed above, START is the largest and has produced the best results in terms of absolute performance [1]. The basic parameters of the experiment are $B = 0.5$ T, $I = 250$ kA, $R = 0.3$ m, $R/a = 1.35$ and $1.6 < \kappa < 4$. In terms of performance, START, which operates as an ohmically heated tokamak, has achieved peak electron temperatures of about 500 eV and line averaged densities of about 5×10^{19} m^{-3} with an overall pulse duration of about 40 ms. For short periods of time, high elongations corresponding to $\kappa \sim 4$ have been obtained, although 1.6 - 2 is a more typical range. The confinement data, particularly with regard to scaling, is limited in extent, but seems to match best with the Rebut-Lallia relation and is similar to several of the other familiar empirical scaling relations. In short, in terms of confinement, START behaves more or less like a standard tokamak. Because of the natural elongation, START has been able to operate in a double null divertor configuration with a much less sophisticated PF system than in other double null experiments (*e.g.*, DIII-D and JET). With regard to current-driven disruptions, theory indicates that at low aspect ratio the limiting q value increases from the usual value of 2 to approximately 4. Consistent with this, START typically operates with edge q values in the range of 5 - 6 although values as low as 4 have been obtained. A perhaps unexpected and desirable feature of START operation concerns disruptions. For over 20,000 discharges with $R/a < 1.8$, no hard disruptions have been observed. Instead, these are replaced by internal reconnection events (IREs) which degrade performance by means of a thermal quench, but not a rapid current decay, both of which are observed nearly simultaneously in hard disruptions in standard tokamaks. An important issue is whether or not this desirable behavior extends into the regime of high auxiliary heating power. In summary, START observes many of the favorable features of standard ohmic tokamak operation while exhibiting several improvements with regard to elongation, divertor implementation, and disruption immunity.

The CDX-U experiment is a smaller (in terms of toroidal field strength) spherical tokamak [2] with the following parameters: $B = 0.1$ T, $I = 100$ kA, $R = 0.32$ m, $R/a = 1.5$, $\kappa = 1.6$. Typical operation achieves peak electron temperatures of 100 eV and pulse lengths of about 10 ms. As with START, the CDX-U experiment observes no hard disruptions for low aspect ratio discharges. Instead, resistive MHD activity leads to IREs. Discharge programming has resulted in periods of quiescent operation with no IREs and enhanced central confinement (by a factor of 2 - 3). The implication is that for longer time scale experiments, current profile control may be a desirable feature. CDX-U has also made progress on the problem of non-inductive start-up, which is not required in a standard tokamak. This has been achieved by a combination of helicity injection start-up and an ECH sustained pressure gradient. Although peak performance is not achieved during this operation it is nevertheless an important demonstration of feasibility.

The HIT experiment is a coaxial helicity facility [3] that can be operated as a spherical tokamak with similar engineering parameters to START. Its parameters are $B = 0.5$ T, $I = 200$ kA, $R = 0.3$ m, $R/a = 1.5$ and $\kappa = 2$. Typical operation is characterized by peak electron temperatures of about 100 eV and pulse lengths on the order of 10 ms. The interesting feature of this experiment from the viewpoint of spherical tokamak research is that it has been able to achieve non-inductive start-up and current sustainment by means of helicity injection without an ohmic transformer. This technique must now be demonstrated to be consistent with low impurity content, at least in the plasma core, in order to be generally adopted as the preferred start-up procedure. Also, the efficiency may be an issue since 15 MW of power is required to create a 150 kA plasma.

The MEDUSA experiment is a small ST with B Û 0.3 T, I = 40 kA, R = 0.12 m, R/a = 1.5 and κ = 1.5. It was funded as an undergraduate research project by University of Wisconsin, Madison. Key results obtained to date include confirmation that IREs are a ubiquitous feature of low-A plasmas with peaked current profiles; observation of a rapid inward plasma motion during an IRE; and internal magnetic measurements showing broad current profiles during the current rise phase and subsequent rapid redistribution into a peaked current profile.

In summary, the spherical tokamak has achieved promising performance, quite comparable to standard tokamaks of similar scale. One difficulty is that with only a few small dedicated facilities available, there is a lack of data with regard to transport scaling. Equally important, none of the existing facilities has operated with substantial auxiliary power, so the questions of heating, current drive, and beta limits have yet to be addressed. Nevertheless, the wealth of data from many years of standard tokamak research is expected to carry over to the spherical tokamak, thereby significantly reducing the level of uncertainty regarding the performance of future larger devices.

Theoretical Achievements: Theoretical understanding of the spherical tokamak is relatively advanced with respect to other alternate concepts, largely because of its similarity with standard tokamaks and the availability of corresponding theoretical analyses and numerical tools. A summary is as follows.

A major motivation for spherical tokamak research results from MHD equilibrium and stability studies. At low aspect ratio one expects to achieve higher values of beta based on the simple scaling relation $\beta \sim \kappa/A$. Detailed MHD studies show that the improvement is greater than this scaling would indicate for two reasons. First, at low aspect ratio, ST equilibria exhibit natural elongation. This results in passively stable values of $\kappa \sim 2 - 3$ which are higher than for standard tokamaks which have $\kappa \sim 1.4 - 1.6$. Second, and somewhat surprising, the multiplying coefficient, β_N, also increases as A decreases, which leads to further gains. Combining all these features leads to β_{crit} on the order of 20% to 40% depending upon whether or not a conducting wall is included in the calculation. These high beta limits have not as yet been tested experimentally in STs because of the absence of auxiliary heating.

Because of the low aspect ratio and the corresponding need to minimize and ultimately eliminate the OH transformer, non-inductive current drive is an essential element of ST research. This difficult problem can be eased if discharges can be created with a substantial fraction of the current being due to the bootstrap current. Theoretical studies show that it is possible to achieve 80% bootstrap fraction at high β_{crit} ~ 44%, high edge q ~ 16 and moderate central q ~ 2.5. These profiles, however, require both current and pressure profile control. At lower values of beta, but still high $\varepsilon\beta_p$, high bootstrap fraction is possible, perhaps requiring only pressure profile control. This issue has also not been addressed in existing ST facilities because of the absence of auxiliary power to maintain low collisionality at high epsilon beta-poloidal.

Even assuming a large bootstrap fraction, substantial non-inductive current drive is still required. Standard techniques such as lower hybrid and electron cyclotron current drive have difficulties because of the high density and low magnetic field. One proposed alternative is high harmonic fast wave current drive. These waves have good accessibility and strong single pass absorption. One simulation [4] predicts that 6 MW of power can drive 1.5 MA of on-axis current for an NSTX plasma. Alternatively, off-axis-currents of 0.5 MA can be driven with the same system. Another possibility is neutral beam current drive which is well established for standard tokamaks but has not received detailed attention for the ST. Note that while the individual components of high β, high bootstrap current, profile control, and non-inductive current drive have all been investigated theoretically, an integrated start-up and evolution to flat-top scenario remains to be carried out.

A final topic of interest concerns transport, both in the core and the scrape-off-layer (SOL). Since transport in both regions is likely to be anomalous, theoretical studies, in analogy with those for standard tokamaks, will likely involve sophisticated, nonlinear micro-turbulence analysis and simulations. Consequently, the resulting predictions will not be treated with the same confidence that is afforded to MHD predictions. Following the traditional approach, one will rely instead on empirical scaling relations as imperfect as they may be. Still, there are two promising points with regard to the theory of ST transport. First, in the outer portion of the core (where A is small), theoretical and numerical studies have shown that at low collisionalities, certain classes of electrostatic and electromagnetic modes have dramatically reduced growth rates as A is decreased [5]. This may lead to transport barriers and an overall increase in core confinement. Second, experimental observations in START indicate that the width of the SOL is larger than would be predicted by Bohm diffusion. If the larger width scales to future experiments, this would be a desirable feature, since the ST is expected to have high heat loads resulting from high beta and small major radius. Any mechanism that helps spread the heat load onto a wider area of the target plate is beneficial.

In summary, a substantial amount of theory has been carried out, mainly focused on the design of a proof-of-principle experiment. The results are promising for MHD. In other areas the theory suggests ways to overcome the difficulties associated with current drive and possible mechanisms for improving transport over standard tokamaks. With the theoretical tools available today, many of the remaining unanswered questions can be addressed once the resources are provided.

Comparison of Theoretical Modeling with Experiment: In many ways the overall operation of existing spherical tokamaks parallels that of standard tokamaks. Attempts to make detailed comparisons between theory and experiment have been reasonably successful in the MHD area. In the areas of core confinement, divertor physics, and start-up the comparisons are much less clear. A summary is given below.

The best agreement between theory and experiment concerns MHD equilibria and natural elongation. There is a good correlation between elongations in the range from $1.4 < \kappa < 4$ and the corresponding values of l_i which represent current profiles varying from hollow to peaked, respectively [6]. Consistent with the idea of natural elongation, high κ equilibria are found to be stable to vertical instabilities, at least on the 10 ms time scale. Regarding disruptivity, the absence of hard, current-plus-thermal quenching disruptions and their replacement with milder thermal quenching IREs is not well understood.

Core confinement comparisons have been limited to the ohmic heating regime. In terms of absolute numbers, best agreement is found with the Rebut-Lallia scaling, indicating that the ST behaves essentially like a standard tokamak. However, when comparing the predictions of several of the standard empirical scaling relations to a next generation proof-of-principle experiment, there are substantial variations in the predicted confinement time; that is, the auxiliary power required to achieve a given beta-normal can vary by a large factor.

In the area of divertor physics the observed larger energy e-folding width of the SOL in spherical tokamaks is not well understood theoretically although some initial ideas related to MHD pressure driven modes have been suggested. Also, a theoretical explanation for the wide imbalance in the heat fluxes on the inboard and outboard divertor plates has only recently been developed [7], but has yet to be fully embraced by the experimental fusion community.

In summary, there are some convincing comparisons between theory and experiment, but many areas still need further analysis.

B. Appropriate Level of Research

Major Experimental and Theoretical Issues: The issues that must be addressed fall into four categories: MHD, transport, divertors, and non-inductive operation. In the MHD area, theoretical and experimental studies are needed to simultaneously optimize the configuration with respect to beta limits, natural elongation, and the achievement of high bootstrap fraction profiles. The absence of hard disruptions and the appearance of IREs must also be understood. Ultimately, one must learn to eliminate IREs as well as hard disruptions since, in a reactor environment, thermal quenches by themselves can threaten the physical integrity of the machine. Also, experiments should be designed with some flexibility to vary the aspect ratio to test the various scalings with respect to A.

In the transport area, a major goal is to extend the data base at low aspect ratio into the auxiliary power regime. Also, one must learn how to produce transitions from L-mode to H-mode and, once achieved, to evaluate the desirability of H mode operation at low A. This research will rely predominantly on new experiments although some experiments may be possible on existing facilities.

The divertor area has several important issues. First, a more detailed experimental and perhaps theoretical understanding of the enhanced energy e-folding width of the SOL is required. Second, research needs to address the adequacy of divertor operations with and without dedicated divertor coils and X-points. Divertor research presently ongoing at high aspect ratio on highly radiative divertors and mantles needs to be carried out in the low aspect ratio regime. Third, the theory explaining the imbalance between inboard and outboard heat fluxes on the divertor plates must be confirmed and/or improved upon. Although observed in many standard tokamaks, the imbalance is particularly important for the ST because of the anticipated higher heat loads. These issues suggest that configurational optimization (*i.e.* double null, single null, natural divertor, *etc.*) studies may be of value.

Non-inductive start-up, current sustainment and profile control have only a very limited data base. Experimental and theoretical investigations are required in the areas of helicity injection start-up and current-drive sustainment, perhaps by high harmonic fast waves or neutral beam injection.

One should keep in mind that while start-up, current drive, and transport are often interrelated from an operational point of view, they are actually three separate issues from the physics point of view. To help isolate these phenomena, and improve understanding both singly and collectively, the proof-of-principle experiment should contain a robust OH transformer. This would enable measurements of transport with and without auxiliary heating, independent of the details of non-inductive start-up and current drive.

Each of the above issues should be tested on a facility capable of sufficiently long pulse length to achieve a fully relaxed equilibrium. This would allow a more reliable assessment of the ultimate viability of the ST concept.

Appropriate Mix of Research Activity in the U.S.: Because of the promising results so far attained, and the close relationship to standard tokamak research, new, larger spherical tokamak facilities, at the proof-of-principle level, are required (worldwide and within the U.S.) in order to mount a program that can resolve each of the above issues. These issues cannot be addressed on existing facilities which lack auxiliary heating and are characterized by relatively short pulse and high collisionality.

The proof-of-principle-class ST experiment will be of sufficiently large scale that, for the sake of economy, it should probably be located at a site with substantial site-credits as well as an existing scientific and engineering staff. National laboratories, most industry, and several universities satisfy this requirement. Furthermore, a concept at this stage of development requires the support, innovation, competitiveness, and community involvement arising from several smaller concept-exploration-class experimental facilities. These would most appropriately be located at sites elsewhere from the proof-of-principle experiment. Universities would be ideal for such experiments.

As part of this program there should be a corresponding increase in the level of theoretical support, support of smaller facilities, and power-plant studies. In addition, the continued support of existing small STs in the U.S. is nonetheless highly desirable in order to address specific issues and to investigate quickly and inexpensively innovative new ideas. These include non-inductive start-up, current drive, the influence of conducting walls on IREs, suppression of IREs, limits to elongation, the effects of toroidal velocity and velocity shear on MHD stability, and divertor magnetic configurations. Furthermore, without the scientific input from several smaller facilities, the ST community may shrink below critical mass which would greatly reduce the rate of progress of the ST concept.

The Worldwide ST Research Plan: There will likely be several new small spherical tokamaks constructed in Europe, Russia, Japan, and Brazil as the ST concept gains worldwide acceptance. The major new facility of interest is the MAST experiment at Culham. MAST is a 1 MA experiment which can address many, although not all, of the issues described above. Specifically, MAST does not have as primary goals the investigation of non-inductive start-up, long pulse, and wall stabilization for advanced performance. Even so, it must still be considered in the class of proof-of-principle experiments. The EURATOM has recently given approval for construction of MAST. A worldwide program consisting of MAST, a U.S. proof-of-principle experiment, and a number of small supporting experiments constitute a critical mass capable of testing and advancing the ST concept in an efficient manner.

The Proper U.S. Role in Worldwide ST Research: The U.S. should play an active role in the international ST program and strive to be its leader. Of the alternate concepts considered, the ST is certainly near, if not at the top of the list in terms of concept advancement. The U.S. initiated the concept and has been a strong, intellectual proponent of the ST concept. Experimentally, the concept has been most successfully advanced by our colleagues at Culham. It is one of the most interesting and exciting areas of fusion research and the U.S. should be anxious to participate. We should pursue the opportunity aggressively in order to not fall behind the growing worldwide ST research effort and because a concept with this potential warrants more than one proof-of-principle experiment worldwide. The U.S. has a long tradition of being a leader in the area of advanced and innovative tokamak operation and this tradition should serve as a focus for the U.S. contribution to the worldwide ST program. Moreover, the ST meets a particular need of the U.S. fusion program for small, low cost, market entry vehicles.

C. The Potential Impact of ST Research

Spherical tokamak research will make potentially important scientific contributions in the areas of basic plasma physics, fusion plasma physics, assessment of the ST as an energy source, and assessment of the ST as a fusion power plant. These contributions are summarized sequentially below.

In the area of general plasma physics, operation of an ST in a high β, high bootstrap current regime will allow investigation of such phenomena as: 1) increased orbit-averaged good curvature for suppression of electrostatic and electromagnetic turbulence; 2) effects of reduced trapped particle fraction due to omnigenity near the plasma core; 3) effects of high trapped particle fraction and high mirror ratios near the plasma edge; 4) absorption of high harmonic fast waves; and 5) effects of strong magnetic curvature, long connection length, and large mirror ratios on energy e-folding width of the SOL. These are generic issues of interest to many magnetic configurations, not only the ST.

A major contribution of ST research is in the area of fusion plasma physics. Experimental and theoretical investigations of MHD equilibrium should contribute greatly to our knowledge of beta limits, q limits, and the limits of natural elongation. In addition, techniques developed to reduce or eliminate IRE's will be important for conventional tokamaks as well as the ST. The ultimate goal is to learn how to operate in a high performance mode without disruptions. A second area of major impact is the development of techniques for achieving long-pulse current-profile control and non-inductive start-up. The ST may contribute to and benefit from related research on conventional tokamaks and stellarators. In terms of fusion plasma physics, ST research extending the confinement data base to low aspect ratio will be of prime importance. It will indicate the desirability of the ST approach to ignition and fusion energy production as well helping to narrow down the uncertaincies in the scaling of conventional tokamaks to future missions. Each of the contributions above represent have a direct and large impact on fusion plasma physics.

The compactness of the spherical tokamak combined with the high power density associated with high beta offer the possibility that the ST can make valuable contributions to the problem of making an economical fusion energy source. Preliminary designs for a volume neutron source and a pilot plant look attractive (small size, economical developmental program), but are based on confinement times predicted from an optimistic scaling law choice. Pessimistic choices lead to less attractive designs. This issue would not be resolved until after the proof-of-principle experiment has been completed and the relevant data assimilated into the modeling.

The benefits of compactness and high power density carry over to a commercial power plant. Smaller size ultimately leads to a smaller capital cost, an important problem facing the standard tokamak reactor. The compactness also introduces new technological problems that must be addressed in the future including higher neutron wall loadings (6 - 10 MW/m^2), and development of a low-loss central leg of TF coils resistant to neutron damage and generating modest activation over a reasonable lifetime.

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6.2. Stellarator

A. Worldwide Status of Research and Achievements

Experimental Achievements: Stellarator plasmas have ion temperatures up to 1.6 keV, electron temperatures up to 3.5 keV, densities to 3 x 10^{20} m^{-3}, volume-average beta values greater than 2%, and an energy confinement time greater than 40 ms. Plasma heating with neutral beams and ECH has been developed; heating efficiencies are similar to tokamaks. A divertor concept is being developed and tested in existing devices.

Theoretical Understanding: Reliable codes have been developed for design and interpretation of the equilibrium, stability, and neoclassical transport properties of stellarators over the last 15 years. Analytic expressions for neoclassical transport coefficients have been derived and fundamental understanding of equilibrium and neoclassical transport properties has been developed. The understanding of anomalous transport remains a challenge.

Agreement with Experiment of Theory and Empirical Scalings: Codes accurately predict the shape of the 3-D pressure surfaces. The neoclassical theory can be consistent with the empirical ion transport coefficients at low collisionality and the measured radial electric field in the plasma core. Evidence of H-mode behavior has been seen, starting a line of confinement improvement research. The empirical magnitude and scaling of transport are similar to tokamaks and to a gyro-reduced Bohm Lackner-Gottardi scaling $(\rho_i/qR)(T/eB)$ with ρ_i the ion gyroradius, q the safety factor, and T the temperature.

B. Appropriate Level of Research

Major Experimental and Theoretical Issues: The major issues are confinement understanding and improvement, development and study of practical particle and power handling schemes, understanding of operational limits, development of optimization principles, and exploration of optimum configurations. In the longer run it will be necessary to demonstrate stability of alpha particle confinement, plasma heating, alpha particle distributions, and alpha ash removal.

Experiments should: 1) test neoclassical transport and investigate the role and control of radial electric fields at lower collisionality; 2) study the sensitivity of turbulence and anomalous transport to magnetic configuration, plasma parameters, and wall conditioning; 3) further develop the particle and power handling concepts; 4) investigate the limiting plasma behavior as beta is raised; and 5) test key optimization principles and techniques for confinement improvement.

Theory issues include: 1) clarification of the constraints on the magnetic configuration imposed by adequate neoclassical confinement; 2) modification of the tokamak linear and gyrokinetic codes for application to stellarator configurations; 3) development of techniques and codes for studying stellarator divertors; 4) augmentation of equilibrium codes to incorporate new effects such as the improvement in the magnetic surface quality in the presence of plasma rotation; 5) exploration of new stellarator configurations that maintain desirable properties but are consistent with smaller power plants; and 6) investigation of alpha confinement and stability.

Research Program Outside the U.S.: The two major facilities under construction in the world program, LHD (1998) in Japan and W7-X (2004) in Germany, will provide integrated tests of two different stellarator configurations using superconducting coils for long pulse/steady-state capability. Divertor, transport, and beta limit issues are being studied on present medium scale stellarators: CHS in Japan and W7-AS in Germany. Also, TJ-II in Spain (1997) and H-1 in Australia will focus on beta limit issues. Stellarator research is also being pursued in Russia and the Ukraine. The theory programs associated with major stellarators are focused on support for the experiments. Longer range projects include a better free boundary package for the MHD stability codes, which is under development at Garching. Studies are also starting on the implications of different stellarator configurations for a fusion power plant.

Recommended U.S. Program: In regard to its development status, the stellarator as a concept is in the transition phase between proof-of-principle and proof-of-performance. Very large devices (LHD and W7-X) are under construction. Medium sized devices such as W7-AS and

W7-A in Germany, CHS in Japan, and ATF in the United States have been operated and provided data confirming the essential physics of the stellarator approach in stability, confinement, and heating. While the confinement data from these machines fit a scaling that connects to tokamak data, the confinement data has all been obtained on machines with minor radii less than 25 cm. There is concern that in plasmas this small, the edge region, as defined by neutral penetration, is too large a fraction of the radius. In the tokamak, consistent confinement scaling of the predictive value did not emerge until data from the machines with minor radii in the 40-50 cm range and above became available. Consequently although it is encouraging that a systematic confinement scaling has been constructed for the stellarator on relatively smaller machines than the tokamak, the confinement data from LHD and W7-X will prove crucial in establishing the validity of the scaling. In terms of confinement, the stellarator lies somewhere between proof-of-principle and proof-of-performance. In the area of stability, reactor levels of beta have not yet been achieved; this is a physics element one would expect to be done at the proof-of-principle stage. The issue of particle and power handling (divertors) has just begun to be investigated in stellarators and becomes urgent and unavoidable in the long pulse/steady-state devices LHD and W7-X. Hence, in power and particle control, the stellarator is closer to proof-of-principle level, but the required data should be obtained in LHD and W7-X.

<u>The U.S. can play a valuable role in stellarator concept development at the concept exploration level.</u> Stellarator geometries are particular; tests of new geometries generally require a new device and the concept exploration level is the place to start. An example of a currently funded effort in this vein is the HSX device at the University of Wisconsin which is testing the quasi-helically symmetric stellarator configuration. The HSX configuration cannot be duplicated in any other device in the world, including LHD and W7-X. To some extent, radical new departures in geometry in the general stellarator class (*e.g.*, very low-aspect-ratio stellarators) should be considered individually as entirely new alternate concepts and should progress through the various stages of concept development, beginning at the concept exploration stage.

<u>An appropriate U.S. focus area is in the effort to reduce the size of stellarator fusion power systems.</u> The physics basis obtained from the stellarator proof-of-principle experiments is sufficient to project the concept to the power plant scale. The projected devices are about the same size as the mainline tokamak power plant projections. Accordingly, in analogy with the Advanced Tokamak thrust, a concept improvement thrust for the stellarator is an appropriate area of interest. Important issues like more compact systems, the minimum aspect ratio, confinement improvement, and beta optimization should be key goals of the U.S. stellarator effort.

In view of the planned operation of two large, ongoing proof-of-performance level devices in the world and the limited resources available in U.S., there is little motivation for the U.S. to build proof-of-performance devices similar to LHD and W7-X. Within the world stellarator program, the possibility exists for additional interesting experiments in the proof-of-principle class. Such experiments have not yet been proposed, but interesting theoretical ideas for new stellarator geometries are now coming forward. If one or more of these ideas develop into proposals, such proposals should be considered as candidate elements of a balanced U.S. concept development program. Owing to the general maturity of the stellarator field, it is possible to consider starting a new stellarator concept, which has a strong theoretical basis, at the proof-of-principle level, although the normal course for a completely new concept would be to begin at the concept exploration level.

In order to maintain beneficial contact with the large stellarator efforts abroad and to gain knowledge from those important experiments, the U.S. should:

Seek to gain a support role on LHD and W7-X. This role should consist of an experimental physics and diagnostic contribution (or similar scale hardware) on both LHD and W7-X. This diagnostic contribution will allow meaningful participation of U.S. scientists in the stellarator research on LHD and W7-X.

Seek to provide substantial theory support to LHD and W7-X. A core of theorists could contribute to the interpretation of results from LHD and W7-X. This core of theory competence in the stellarator field would be the key to the U.S. program being able to absorb the results from LHD and W7-X to provide the basis for possible future U.S. reentry into stellarator experimental initiatives at large scale.

This core of theorists could also stimulate domestic initiatives by elucidating aspects of stellarator optimization needed for incisive tests of physics or for power plants such as the practical definition of the optimization criteria and the search for configurations that satisfy these criteria.

C. Potential Impact of U.S. Stellarator Research

General Plasma Physics: Because naturally occurring plasmas are fully 3-D, the theoretical techniques developed for stellarators have application to a broad range of plasma problems, for example, electron orbits in the magnetosphere.

Fusion Plasma Physics: Stellarators are a strong driver for the development of 3-D plasma physics and help define the possibilities and limitations of toroidal confinement systems. 3-D equilibrium theory developed for stellarators provides insights and computational techniques for resistive instabilities, wall modes, and field error effects. Transport and particle losses due to symmetry breaking had a natural development within the context of stellarators. Comparison between stellarator and tokamak experiments have broadened the understanding of bootstrap currents, edge velocity shear layers, and the role of field errors in both systems. Stellarators continue to provide unique plasma configurations and tests of physics; trapped particle instability theory will be tested on W7-X in which most trapped particles are in a region of good curvature. Also, stellarators can maintain a reversed q profile across the entire plasma and thereby test effects of globally reversed shear (or low shear). A quasi-toroidal stellarator could test tokamak physics without a net plasma current. Quasi-toroidal and quasi-helical stellarators have different signs of the bootstrap current, allowing tests of stabilization and destabilization of magnetic-island producing perturbations.

Development of Fusion Energy: By requiring no net current, the stellarator avoids problems associated with current drive requirements, control with a high bootstrap current fraction, major disruptions, and positional control systems and instabilities. The stellarator may lead to a technically more attractive reactor (than a tokamak) because it is intrinsically steady-state, can have low recirculating power, and has a robust magnetic configuration. It may also have low power density, which leads to a large, and costly system.

Developing the Concept as a Power Plant: The recent U.S. Stellarator Power Plant Study has shown that modern stellarator designs are similar in scale and cost to the projections of the mainline tokamak to the power plant scale. Design optimization studies are needed to obtain more compact configurations with good confinement properties and higher beta. For instance, a consideration may be the aspect ratio of the device: the higher the aspect ratio, the easier stellarators are to design for high beta and good confinement but the larger the minimum size power plant.

Summary of Findings

1. In regard to its development status, the stellarator as a concept is in the transition phase between proof-of-principle and proof-of-performance.

2. The U.S. can play a valuable role in stellarator concept development. An appropriate U.S. focus area is in the effort to reduce the size of stellarator fusion power systems.

3. In view of the planned operation of two large, ongoing proof-of-performance level devices in the world and limited resources available in the U.S., there is little motivation for the U.S. to

build proof-of-performance devices similar to LHD and W7-X. Within the world stellarator program, the possibility exists for additional interesting experiments in the proof-of-principle class. Such proposals should be considered as candidate elements of a balanced U.S. concept development program, although the normal course would be to begin at the concept exploration level.

4. In order to maintain beneficial contact with the large stellarator efforts abroad and to gain knowledge from those important experiments, the U.S. should: 1) seek to gain a support role on LHD and W7-X; and 2) seek to provide substantial theory support to LHD and W7-X. This core of theory support could also stimulate domestic initiatives.

6.3. Reversed-Field Pinch (RFP)

Like the tokamak, the RFP plasma is confined by a combination of toroidal, B_ϕ., and poloidal magnetic fields, B_θ, in an axisymmertic toroidal geometry [1]. Unlike the tokamak, the toroidal and poloidal field strengths are comparable, $B_\phi \approx B_\theta$, and the toroidal field in the RFP is largely generated by currents flowing within the plasma. As a consequence, the safety factor, $q \approx aB_\phi/RB_\theta$, for an RFP is always less than unity while for the tokamak, $q > 1$. The RFP concept derives its name from the fact that the direction of the toroidal field is reversed in the outer region of the plasma (and q vanishes at some minor radius), and this reversal corresponds to a relaxed state of minimum energy [2]. As a fusion concept, the RFP has some advantages relative to the tokamak. The magnetic field at the coils can be low, and the plasma current can be increased sufficiently (at least in principle) to allow ohmic ignition.

In the following, the status of RFP research is summarized. Since the RFP concept originated more than 30 years ago, a history of the development the RFP plasma confinement concept is presented first. Secondly, key research accomplishments from the RFP program are listed. The scientific and technical issues facing the RFP are described next. Finally, we discuss the appropriate level of research for the RFP and conclude by noting the research impact on plasma and fusion science resulting from RFP research.

A. Worldwide Status of Research and Achievements

The RFP concept evolved from toroidal pinch research at the beginning of world fusion program. This early pinch research was motivated by the desire to achieve conditions for ohmic ignition with high engineering beta. Fast growing sausage-type and kink instabilities were overcome by applying a toroidal field and a close-fitting conducting shell; the stabilized toroidal pinch was able to achieve gross stability at high-current. Nevertheless, the toroidal pinch had relatively poor confinement, and worldwide pinch research was temporarily abandoned except for the large Zeta device (R = 3 m, a = 1 m, $I_p \leq 0.5$ MA) built in 1958 at Harwell, U.K. [3,4].

By the mid 1960's, the persistent investigations using the Zeta device paid off when Zeta "spontaneously" entered a quiescent phase having reduced fluctuations, improved confinement, and a reversed toroidal field at the plasma edge [5]. This transition to improved confinement occurred when Zeta operated within a restricted neutral pressure range having reduced collisionality, which allowed turbulent relaxation to the RFP configuration [6]. Self-reversal of the toroidal field was later observed in many RFP experiments [7-12], and this fundamental process was later explained by Taylor as the natural tendency of a plasma discharge surrounded by a flux-conserving shell to relax towards a state of minimum energy [2,13]. Taylor's theory was able to explain two critical observations from Zeta: the relaxation to the field-reversed state was independent of the initial state of the discharge or the discharge history, and the final relaxed state depended upon the pinch parameter, $\Theta \equiv B_{\theta,w} / \langle B_\phi \rangle$.

The improved understanding of MHD relaxation and the encouraging results from Zeta justified a large worldwide effort on RFP physics in the 1970's. Several small-scale concept exploration experiments were constructed (for example, HBTX-1 [10,14], ZT-1 [9], ZT-S [15,16], ETA-BETA [17], TPE-1 [12], OHTE [18]), and several medium-scale experiments (and upgrades) were operated into the 1980's (including ZT-40 [20], ZT-40M [21], TPE-1R, HBTX-1B, HBTX-1C, ETA-BETA-II). The first two decades of RFP research resulted in considerable experimental experience and theoretical understanding. These included programmed start-up [22] (including the use of pellet fueling [23]), generation of partial current drive (about 5% of the total current) by applying oscillating external fields [24], and (perhaps most importantly) considerable experimental experience contributing to a database of RFP confinement scaling and beta limits [25].

By the end of the 1980's, the world RFP program entered a new stage of development. Construction of two large experiments began in order to test RFP confinement scaling in reactor-like conditions. Construction of the ZTH device [26] began at LANL, and RFX [27] was constructed at Padua, Italy (previously the location of the ETA-BETA experiments). These facilities required funds on the order of $100 M per device. At the about the same time, the MST device [28] was constructed at the University of Wisconsin. Unfortunately, budget constraints and policy decisions in the U.S. forced the cancellation of ZTH towards the end of its construction period. These budget cuts changed the RFP program from one containing "proof-of-principle" or confinement-scaling devices into a program emphasizing the investigation of scientific issues of a more fundamental nature.

The few RFP experiments which operated during the 1990's produced major scientific advances. The source of magnetic fluctuations within the RFP have been identified [29,31], and a theoretical understanding of the experimentally measured fluctuation-induced transport has been developed [31].

Perhaps the most significant new development in RFP research is the reduction of fluctuations and associated confinement improvement as a result of transient current profile control [32]. Magnetic diffusion due to finite resistivity causes the current profile in RFPs to evolve away from the Taylor minimum energy state. In MST an induced poloidal electric field was used to transiently drive the current profiles back towards the Taylor state. Magnetic fluctuations decreased by a factor of two, RFP "sawtooth oscillations" were eliminated, beta increased from 5% to 9%, the central electron temperature increased from 0.4 keV to 0.6 keV, and global energy confinement increased from 1.3 ms to 6 ms. The improved performance of the RFP with current profile control is analogous to similar progress made in tokamak confinement beginning about five years ago which focused worldwide attention on "advanced tokamak" concepts.

The MST results have motivated consideration of "advanced RFP" concepts. The RFP, having evolved from early pinch experiments, was at least in part driven by the desire for pulsed, ohmic ignition in a device with low magnetic field strength at the conductors. In contrast, "advanced RFP" concepts use (as yet not fully developed) current and pressure profile control techniques to improve confinement and beta limits and to operate steady-state. Although the early RFP has an extensive database resulting from more than a dozen small and medium-sized experiments, "advanced RFP" concepts are, by comparison, still immature.

Several outstanding reviews describe the early RFP program up to 1990 [1,19]. Nearly 20 RFP devices have been constructed with plasma currents ranging from 50 kA (*e.g.,* ZTP at LANL) to 0.5 MA (*e.g*., HBTX, MST and RFX), major radii ranging from 0.45m to 2.0m, and confinement times as high as 6 ms (in the recent MST experiments). The "best confinement" gathered from different RFP devices shows a favorable "constant poloidal beta" scaling of global energy confinement [25]. Provided the poloidal beta, $\beta_\theta \sim 0.1$, is constant as the size and current of an RFP increases, data indicate $\tau_E \propto I_p^3/(an^{1.5})$. Impurity puffing experiments [33] support the assertion of constant β_θ; however, plasma current scaling within a single device does not. Within a single RFP the poloidal beta decreases with increasing current [34], and confinement degrades.

The observed favorable scaling at fixed β_θ can be compared with two theoretical studies of RFP confinement scaling [35,36]. Connor and Taylor were able to reproduce a constant-β_θ scaling by considering transport driven by electrostatic interchange modes. Carreras and Diamond proposed a resistive-interchange turbulence model for RFP confinement which includes transport due to magnetic fluctuations. In the Carreras-Diamond model, β_θ is no longer constant, and $\tau_E \propto I_p^2/(a^{0.25}n)$. Both scalings can fit the present "best confinement" database [25]; however, when RFX operates at its design current of 2 MA, the RFP performance database will be sufficiently wide to distinguish between the favorable (Connor-Taylor) and the unfavorable (Carreras-Diamond) confinement scaling predictions.

Although the RFP program has made steady progress towards documenting RFP confinement scaling, the dominant experimental achievements in RFP confinement research have been in the area of fluctuations and associated transport. A partial list of key scientific achievements follows:

<u>Identification of the cause of magnetic fluctuations</u>. The dominant magnetic fluctuations in the RFP are low order resistive MHD modes. The spectrum of fluctuations calculated from nonlinear resistive MHD simulations agree well with experimental measurements.

<u>Magnetic fluctuations are the cause of core transport</u>. Direct measurements of energy and particle flux from the core (*i.e.,* within the reversal surface) are clearly accounted for by magnetic fluctuations. Outside the reversal surface, magnetic fluctuations drive little transport.

<u>Electrostatic fluctuations are the cause of edge transport</u>. Direct measurements of the energy and particle fluxes at the edge are shown to be caused by electrostatic fluctuations.

<u>Identification of the MHD dynamo</u>. Self-driven currents in the RFP are produced spontaneously by the so-called "dynamo effect." At the extreme edge of an RFP, the fluctuating **v x B** has been measured directly and shown to account for the edge dynamo current.

<u>Observation of resistive-wall stabilization</u>. The external kink is stabilized by a close-fitting, thick conducting shell in an RFP. Experiments with relatively resistive shells have observed both the external kink and the resistive, "dynamo" modes which grow with a growth time of several wall penetration times, in agreement with theory.

<u>Oscillating field current drive observed</u>. An initial test of OFCD sustained 5% of the total plasma current

Confinement improvement observed through profile modification. As mentioned above, when the current profile of the RFP is driven externally, fluctuations decrease significantly and as much as a five-fold confinement improvement has been observed.

Research Issues

Several RFP reactor studies [37, 38, 39] have examined the critical issues facing the RFP fusion concept. The most recent and extensive of these studies is the TITAN study [39]. In TITAN, an attractive reactor concept was presented emphasizing a very high power density and oscillating field (helicity injection) to maintain a steady-state plasma current. The fusion power density was chosen to be very high in these studies, and problems associated with high power and particle fluxes were solved by radiating more than 70% of the core fusion power through injection of xenon impurities. The TITAN study both listed several important research issues facing the RFP and illustrated the reactor potential of the RFP if these issues could be resolved favorably.

Probably the most important issue facing the RFP is confinement scaling. Although numerous RFP devices have been built and achieved $0.02 < \tau_E < 2$ ms without transient current profile control and $\tau_E \sim 6$ ms with current profile control, there remains significant uncertainty of the level of energy confinement expected in reactor-sized RFP devices [43]. A major assumption in projecting the reactor performance of RFPs is that at reactor level temperatures the resistive diffusion is sufficiently small that the current profile would remain very close to the Taylor minimum energy state and that current-driven resistive MHD modes would not cause significant transport [42]. If this assumption is not correct, then current profile control will be required as in the MST experiment and current drive efficiency becomes a major issue.

In the absence of large scale MHD modes transport will very likely be dominated by resistive interchange modes. The Connor-Taylor confinement scaling, $\tau_E \propto I_p^3$, is favorable for reactor projections. Based on this scaling, compact RFP reactors with high mass power density are projected to have large ignition margins. On the other hand, with the Carreras-Diamond confinement scaling, $\tau_E \propto I_p^2$, future RFP reactors would be much larger with a mass power density comparable to conventional tokamaks.

Related to confinement scaling is the issue of beta limits for the RFP. Theoretically, specific RFP profiles have been shown to be stable to ideal MHD instabilities up to $\beta_\theta < 0.5$ [40], and resistive MHD stability has been constructed for profiles having $\beta_\theta < 0.2$ [41]. The observed beta of an RFP is typically $\beta_\theta \approx 0.1$, and it is not known whether or not an RFP can operate consistently above this level. Furthermore, non-ohmic heating has never been applied to an

RFP. This is significant since auxiliary heating can be an effective tool for exploring beta limits and confinement, and the absence of auxiliary heating data adds greatly to the uncertainty of the effects of alpha heating in potential RFP fusion power sources.

The second key issue identified by the TITAN study is power and particle handling. TITAN adopted the use of three “open geometry” toroidal divertors. Even with the use of impurity injection to enhance radiative losses, the usual poloidal, radiative, pumped limiters envisioned for tokamaks would encounter serious erosion in a compact RFP reactor. Since all RFP devices have to date operated with short-pulse-length, limiter-defined plasmas, the physics of toroidal-field divertors and the presence of magnetic separatricies must be investigated.

The final key research issue for the RFP fusion concept is steady-state current drive (and potentially profile control). In the TITAN study, it was determined that the RFP should operate steady-state in order to maintain its economic attractiveness. Although predicted to be efficient [24], oscillating field current drive (OFCD) has yet to be demonstrated in high-temperature plasmas or to be shown to contribute significant fractions of the plasma current. Although transient current profile control has been induced magnetically, the basic concepts for practical current profile control that may control steady-state profiles have not yet been defined.

B. Appropriate Level of RFP Research

Presently, worldwide there are four RFP laboratories. The MST and RFX devices are the two largest having $R \approx 1.5$ m, $a \approx 0.5$ m, and $I_p \geq 0.5$ MA. RFX is roughly the same size as MST but it is designed for higher current and a longer pulse length. Two smaller RFP devices are located at the Electrotechnical Institute, Tokyo, Japan, (TPE1RM-20 and TPE-2M), and the T2 device (formerly OHTE) is located in Sweden. These smaller RFP experiments are focused on confinement studies and the effects of graphite and resistive walls. In Japan, a new RFP device is under construction (TP-RX) having a conventional RFP design but allowing currents up to 1 MA. The worldwide RFP research program is the third largest fusion concept development program, and it has been highly productive, contributing significantly to the advancement of RFP plasma and fusion science.

Historically, the RFP program entered a “proof-of-principle” developmental stage at the end of the 1980's. Although all of the key issues facing the RFP were not being investigated with the same degree of effort, large experiments were being constructed in order to evaluate confinement and beta scaling up to the 4 MA level [26]. Today, the RFX device in Padua, Italy holds the promise of investigating confinement and beta scaling up to current levels of 2MA. With proper support, the other RFP devices worldwide are capable of investigating both conventional and “advanced” RFP concepts. A possible limitation of the present RFP devices is the inability to address issues related to magnetic divertors.

Although aspects of the RFP program are focused on the scaling issues usually associated with “proof-of-performance” fusion programs, the Panel finds that the Reversed Field Pinch (RFP) concept is best considered as a "proof-of-principle" stage program. This reflects the lack of understanding of key issues associated with beta limits, current drive, and power handling. On the other hand, after more than 30 years of research using nearly 20 experimental devices, the RFP has certainly passed the “concept exploration” stage of concept development. This conclusion is based on:

1) A large experimental database from a variety of devices demonstrating gross MHD stability for many energy confinement times and a favorable confinement scaling with increasing device size;

2) The presence of several operating RFP experiments including a proof-of-principle scale device, RFX, in Italy and more modest programs, such as the similarly-sized (but lower current) MST device located within the U.S. With proper support, the world RFP laboratories can explore the broad range of scientific issues necessary for the further advancement of the RFP fusion concept; and

3) A developed theoretical and computational understanding of many key experimental observations including equilibrium formation, MHD dynamo, resistive MHD fluctuations in the core, and the relationship between confinement and fluctuations.

Although the Panel classifies the RFP program as consisting of "proof-of-principle" stage activities, these activities do not necessarily require the construction of large new experimental facilities. Instead, the RFP program can address the major issues outlined above through:

1) A broader experimental investigation of advanced RFP issues, such as profile control, confinement enhancement, auxiliary heating, and beta-limits within the U.S. program;

2) Increased collaboration with the RFX device in Italy; and

3) Increased support for RFP theory and computation.

Based on future more extensive reviews of proposals, the funding of the RFP program within the U.S. should be increased as we proceed in the exploration of “proof-of-principle” stage fusion concepts. For example, the MST facility is capable of hosting outside collaborators which could bring advanced plasma profile diagnostics, auxiliary heating systems, and current drive techniques. A reinvigorated experimental program should be accompanied by an increased theory and computational effort in order to keep pace with experimental discoveries and to interact with our international partners in the other RFP programs.

C. Research Impact on Plasma and Fusion Science

Spanning more than three decades, RFP research has had considerable impact on plasma and fusion science. Important scientific accomplishments include the understanding of MHD minimum energy states, observations of the plasma dynamo, and investigations of nonlinearly coupled tearing modes. Research on RFPs has direct relevance to other confinement concepts. For example, the MHD activity in spheromaks may be related to that in RFPs because of the similarities in their q profiles. Techniques developed in understanding and controlling plasma transport in the RFP will also very likely have significant spin-offs in other areas and will more generally advance fundamental plasma and fusion science.

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6.4. Field Reversed Configuration (FRC)

The Field Reversed Configuration (FRC) is a compact toroidal system in which the magnetic field lines lie in the poloidal plane while all currents (both currents within the plasma and those in flux conservers or equilibrium field coils) flow in the toroidal direction. FRCs range from systems in which the ion gyro-radius is small compared to the radius of the plasma, to systems with large-orbit ion “rings” in which the orbit size of an important class of particles is comparable to the plasma radius. FRC research offers possibilities for advancing plasma science in the areas of high-β systems, large orbit effects, and magnetic reconnection. In addition, FRCs may shed light on important uncertainties about burning plasmas concerning phenomena associated with energetic, large-orbit fusion products. Because FRCs possess a magnetic topology that is singular for its lack of a rotational transform and magnetic shear, they offer a data point for the equilibrium and stability of plasmas at this extreme.

The FRC shows promise as a candidate fusion reactor system because there is no mechanical structure in the center of the torus, while an engineering beta near unity makes maximum use of external magnets. The absence of toroidal magnetic field coils allows for reactor designs in which the scrape-off layer carries the power and particle exhaust outside the coil system, thereby easing the engineering constraints for particle pumping, impurity control, and power exhaust. If questions regarding formation, stability, sustainment, and confinement are successfully resolved, then FRCs may offer a high-power-density and easily maintainable alternative approach to fusion power production.

A. Current Status of FRC Research.

While the experience with FRCs to date has been limited, FRC experimental results have been generally favorable, raising hopes for its ultimate development into a practical fusion system. Previous reviews of FRCs and FRC-related research include a review of FRC/ion ring research [1], a review of compact system physics and technology [2] a comprehensive review of FRC experiments and theory presented in 1988 [3], and a recent brief review of progress since then [4]. Experiments have achieved the following ranges of FRC parameters:

Density	$0.5 – 50 \times 10^{20}\ m^{-3}$
Ion temperature:	50-3000 eV
Electron temperature	50-500 eV
Particle confinement time	≤ 1.0 ms
Energy confinement time	≤ 0.3 ms
$<\beta>$	0.75-0.95
Separatrix radius	3-20 cm
Separatrix length	20-400 cm
Poloidal magnetic flux	< 10 mWb.

These parameters have been achieved using θ-pinch formation techniques with careful attention to the symmetry of the pre-ionization plasma [5] and the axial shock wave dynamics [6]. More recently, FRCs have been formed by merging two spheromaks with opposite helicity [7]. Notable experimental achievements include: stabilization of the rotational instability that ordinarily appears in FRC experiments with multipole fields [8]; detection of global internal MHD modes [9]; translation of FRCs along a guide field from a θ-pinch formation region to a mirror field where the FRC is stopped and the translation kinetic energy is converted to thermal energy [10]; and studies of transport in FRCs showing that both the particle content and magnetic flux decay faster than would be expected from classical theory [11].

While FRCs have proved remarkably stable in experiments, a satisfactory theoretical explanation has not been found. A convincing stability theory is needed to gain confidence for extrapolation to the fusion regime. The essential problem here is that FRCs have unfavorable flux-surface-averaged curvature without magnetic shear, so FRCs should be unstable to high-*n* ideal MHD modes, and possibly unstable to low-*n* ideal modes as well. Many analytical and numerical treatments have addressed the (n=1) tilting mode. Ideal-MHD theories generally predict instability. Recent work which considered equilibria with a more blunt separatrix shape and a hollower current profile than has been achieved in experiments to date suggested that FRC configurations exist that are stable to ideal-MHD tilt modes [12]. The most successful tilting theories have included finite Larmor radius (FLR) effects, using either kinetic ions [13] or a gyroviscous fluid [14]. The latter led to the prediction of marginal stability conditions consistent with experimentally observed stable FRCs. The FLR stability explanation, however, fails to explain the experimental evidence of robust stability since the inclusion of FLR in stability calculations has the effect of transforming the unstable MHD mode into a negative-energy wave that can be destabilized by almost any residual dissipation mechanism. Other effects that might be important to the experimentally observed stability of low-*n* modes in FRCs are plasma flow [15], shear in plasma flow [16], or effects related to the rapid drift of electrons in high curvature regions [17]. Resolution of the physical mechanism underlying the experimentally observed FRC stability is critical to the reactor prospects for FRCs (*i.e.,* will the stabilization extrapolate to reactors?) and to developing a strategy for future FRC research (*e.g.*, if FRC stability is governed by FLR effects, then future experiments must investigate limiting behavior at large $S \sim a/\rho_i$, while if FRC stability is governed by plasma flow, then future experiments should study generation and control of plasma flow in FRCs).

Alternatively, FRC stability questions might be resolved by the stiffening effect of an energetic ion ring carrying a substantial fraction of the current. An ion ring system may be a "hybrid" system (ring plus background plasma) [18] or a ring-dominated system (very little background) [19]. Studies of plasmas with a significant fraction of large orbit ions have shown the stabilizing potential of such rings [20].

Field Reversed Configurations are a very challenging system to model. Theory and modeling have been successfully applied to the simulation of FRC formation employing the θ-pinch technique. There has been a substantial effort aimed at understanding the stability of FRCs, including both the work mentioned above and more recent efforts in which hybrid simulation codes [21] have been used to study ion ring stabilized FRCs [22]. An alternative approach has been an effort to extend Taylor's theory of minimum energy states, which has been so successful in describing RFPs and spheromaks, to finite beta systems, like FRCs, by including plasma flows in a two fluid theory [23].

The presentations to the Panel did not include any empirical scalings fit to the experimental data. Such efforts in the published literature [3] tend to focus on the particle decay time, τ_n, and the flux decay time, τ_ϕ. The general trend of particle decay times observed in experiments is captured by the expression $\tau_n \sim R^2/\rho_i$ μs/cm (where ρ_i is the ion gyroradius in the external field). However, there are significant (more than a factor of 3) deviations from this estimate. Generally, the decay times for flux and plasma energy are found to be similar to the particle confinement time. While efforts have been made to develop scaling laws for τ_ϕ, there does not appear to be any single empirical scaling law for τ_ϕ that applies over many devices.

B. Appropriate Level of Research for the FRC.

The major issues in FRC research presented to the Panel were: 1) developing a satisfactory understanding through experiment and theory of global stability which includes kinetic effects (in particular, in the $S \sim a/\rho_i >> 1$ regime), plasma flow, and flow shear; 2) the demonstration of a high-quality FRC plasma ($n\tau \geq 10^{17}$ s/m^3 and $T_e+T_i \geq 1$ keV); 3) developing an understanding of transport and flux decay; 4) FRC sustainment and current drive; and 5) development of a fusion-relevant start-up method.

The experimental observation of robust global stability of FRC plasmas is not presently well understood. The panel believes that it is vital to resolve the physical mechanism underlying experimentally observed FRC stability. To investigate the finite Larmor radius effects on stability, the future experiments should be extended to larger S regimes with sufficiently low collisionality. To assess the effects of plasma flow and/or flow shear, flow diagnostics should also be implemented. The FRC theory effort in the U.S. suffered a severe decrease as a result of the five year hiatus in alternate concept research. In spite of this, substantial progress has been made in addressing the stability of FRCs. However, more work is required in this area. The Panel encourages the utilization of the theoretical tools developed for tokamak MHD stability for FRCs with appropriate modifications. The existing theory effort should be expanded, particularly to include the effects of large orbit ions (and ion rings) and plasma flows on FRC stability.

The experimental demonstration of a high-quality FRC is closely related to the problem of understanding transport of particles, energy, and magnetic flux in FRCs. The Panel believes it appropriate to put more emphasis on developing such an understanding than on achieving specific goals in $n\tau$ and/or (T_e+T_i). Substantial experimental progress toward this goal can be achieved with existing facilities, provided they are adequately funded. Particular emphasis should be given to improved diagnostics and controls to allow a investigation of the connection between plasma transport and those parameters that have been identified as important to FRC equilibrium and stability. This includes: determination (and modification) of the magnetic structure of the FRC; the profiles of density, electron and ion temperature; and information about (and modification of) plasma flows. Analysis tools need to be supported so that experimental observables can be used to infer values of parameters (*e.g.*, diffusion coefficients) in theoretical models.

The theory of FRC sustainment and current drive appears to have received inadequate attention. In most magnetic confinement systems (*e.g.*, tokamaks) one discusses schemes for driving force-free currents (**j** parallel to **B**). However, in FRCs what is required is sustainment of currents perpendicular to the magnetic field. Such currents are intimately related to the overall force balance (the Grad-Shafranov equation) and particle orbits (*e.g.*, diamagnetic currents). The Panel is not aware of any adequate theory dealing with the sustainment of such currents in FRCs and recommends a concerted effort to develop such a theory. In particular, this effort should aim at developing quantitative predictions regarding current driven by the injection of neutral beams, ion rings, RF power, and rotating magnetic fields (RMF). It has been suggested that RMF current drive is a leading candidate for sustainment of FRCs. The near term priority for resource allocation to experimental investigations of RMF current drive should be lower than that for stability investigations. However, the efficiency of RMF as well as its possible effects on plasma confinement needs to be tested eventually. Once a viable FRC current drive candidate emerges, its feasibility at reactor parameters should be examined theoretically since the FRC reactor prospect hinges largely on the practicality of the plasma sustainment.

While the development of a fusion relevant start-up method will ultimately be important if FRCs are to form the core of fusion power reactors, the Panel judged it premature to invest substantial resources in such an effort. The more basic issues of FRC stability and transport should be addressed first.

In addition to the U.S. effort in FRCs, there are three experiments in Japan (NUCTE-3, a θ-pinch facility at Nihon University; FIX, a θ-pinch source and translation experiment at Osaka University; and TS-3 at Tokyo University in which FRCs were formed by merging two spheromaks with opposite helicity) and three θ-pinch experiments (BN, TL, and TOR) at the TRINITI Research Center in Troitsk, Russia. This international effort is not appreciably better funded than the U.S. effort.

C. Potential Impact of FRC Research

FRC research offers possibilities for advancing plasma science in the areas of high-β systems, large orbit effects, and magnetic reconnection. FRCs may shed light on important uncertainties about burning plasmas concerning phenomena associated with energetic, large-orbit fusion products. Because FRCs possess a magnetic topology that is singular for its lack of a rotational transform and magnetic shear, they offer a data point for the equilibrium and stability of plasmas at this extreme. If questions regarding formation, stability, sustainment, and confinement are successfully resolved, then FRCs may offer a high-power-density and easily maintainable alternative approach to fusion power production.

Finding

The Panel concludes that FRCs are an interesting plasma configuration at the concept exploration level. Stability to large scale MHD-like modes remains a critical issue both in conventional FRCs and in ion ring stabilized configurations. Because global stability is a potential show-stopper for these configurations, the U.S. program should focus on this issue prior to addressing confinement and sustainment.

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6.5. The Spheromak

The spheromak, like the reversed field pinch, belongs to the class of self-organized plasma containment devices whose magnetic confinement fields form a "Taylor-like," near-minimum-energy configuration. Benefits of the spheromak include a simple, compact configuration that projects to an economic, high-mass-power-density reactor system under the assumptions of stable and "adequate" plasma performance.

In the 1980’s a significant experimental effort (~$100 M in time integrated dollars) was supported to explore the spheromak. This research was centered at PPPL on S-1, University of Maryland on PS, LLNL on Beta-II, and at LANL on CTX. In these experiments**,** attaining hot plasma confinement was difficult. Issues such as wall stabilization, magnetic field errors, plasma-wall conditioning, and magnetic-fluctuation-driven transport all contributed to the spheromak confinement performance. However, in the early 1990’s the Los Alamos CTX experiment did demonstrate that a well-designed, clean, low-field-error spheromak could achieve "high-temperature" performance ($T_{e,core}$ = 400 eV, at a magnetic field of 3 T). CTX also demonstrated the ability to sustain the spheromak configuration against resistive diffusion using electrostatically driven magnetic helicity injection.

A. Status of Research

Global spheromak behavior agrees well with the Taylor minimum energy principle for magnetoplasma relaxation within a conducting boundary. The basic concepts of magnetic helicity dissipation and injection are well understood and in agreement with experiment. Modifications to Taylor's theory to account for driven, dissipative, non-force-free configurations with a view to confinement scaling is more elusive. The stability theory for the tilt and shift (n = 1) modes is well developed (and agrees with experiment) for time-scales short compared with the flux conserver resistive times and for spheromaks with open (passive) conductors. Stability in the presence of resistive walls is poorly understood and is judged by this Panel to be the principal issue for next step spheromak research. The stability of pressure-driven modes, is well developed, but there are few comparisons with data. Generally, the spheromak has been observed to confine plasma with beta much greater than predicted from Mercier limit. Experimental results are also in good agreement with theoretical stability predictions of current driven modes although the nonlinear behavior is not well modeled. Magnetic turbulence, presumed to be from tearing modes, is also poorly understood and in need of detailed experimental quantification.

B. Appropriate Level of Spheromak Research

It is the opinion of this Panel that the spheromak is at the concept exploration stage of development. Considerable experimental data already exists in short-pulse exploratory experiments at the few hundred eV range, where equilibrium and stability are passively provided by a close-fitting conducting boundary. Demonstrating reasonable confinement in experiments where the equilibrium and stability is controlled by externally imposed magnetic fields remains an important milestone for concept exploration.

Addressing next-step spheromak issues will require at least one experiment that can achieve high-temperature (0.5 keV) in quasi-steady-state with externally imposed magnetic field control. In this quest, cost could likely be minimized by taking advantage of the many existing site credits at various institutions. With such new experiments, and in view of the minimal international effort in spheromak research, the U.S. would take the international leadership role in this concept area.

Developing experiments that can address next-stem spheromak issues would necessitate a substantial increase in research funding devoted to this concept (to the level of order $3M to $5M/year). Important issues to be studied, both experimentally and theoretically, include:

1) Equilibrium and stability in externally imposed magnetic fields for timescales exceeding the resistive time of the flux conserver;

2) Energy confinement during helicity sustainment;

3) Investigation of alternatives to gun sustainment of the plasma current;

4) Magnetic turbulence;

5) Profile control;

6) Transport and beta limits (scaling); and

7) Edge plasma engineering (edge conditioning and divertors).

In addition to the next step experimental program, a proper mix of research activities would require contributions from the broader plasma community with expertise in "self-organized" plasma systems such as the RFP. This is particularly relevant to theory and plasma diagnostics, where similar properties of the RFP could improve the economy of scale for spheromak theory and experimentation.

C. Research Impact on Plasma and Fusion Science

When viewed from the broad perspective of plasma and fusion science, the physics basis of the spheromak concept has significant overlap with the very low aspect ratio physics of the spherical torus (ST) and the self-organized magnetoplasma physics critical to the RFP. In the pursuit of this research, there exists great potential for expanding knowledge in the areas of plasma relaxation, dynamo regeneration, turbulence, and transport. Spheromak, RFP, and ST science could be strongly coupled in terms of theory, diagnostics, and experimental technique (profile control, edge plasma conditioning, *etc*.). Thus, spheromak research can contribute greatly to the fusion science base of these other alternative concepts and can gain significantly from progress made in these other concepts as well.

With respect to fusion science, the spheromak projects to an economic fusion reactor due to its simplicity and high-mass-power-density characteristics. Evolving the spheromak concept would provide timely information on this approach to economic fusion energy.

APPENDIX

Appendix

charge letter

Response To ST Charge

Meeting Agendas

List of Interactions,