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Fusion Energy Sciences Advisory Committee

Advice and Recommendations to the U.S. Department of Energy

In Partial Response to the Charge Letter of March 25, 1996, and in Response to the Charge Letter of April 8, 1996

July 22, 1996

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

FUSION ENERGY SCIENCES ADVISORY COMMITTEE
Advice and Recommendations to the U.S. Department of Energy

In Partial Response to the Charge Letter of March 25, 1996, and in Response to the Charge Letter of April 8, 1996

Members of FESAC

Robert W. Conn, Chairman
John F. Clarke
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July 22, 1996

Table of Contents

Preface

This document is a compilation of the written records that relate to the Fusion Energy Sciences Advisory Committee's deliberations with regard to the Letters of Charge received from the Director, Office of Energy Research, dated March 25 and April 8, respectively.

The Letter of Charge dated March 25 requested that the Committee undertake reviews of: (a) the major U.S. fusion facilities; and (b) of alternative confinement concepts. In May, 1996 the Committee responded to the first charge by submitting to the Director, Office of Energy Research, a report entitled: "The Fusion Science Research Plan for the Major U.S. Tokamaks".

During the meeting of July 1996, which is the subject of this report, FESAC provided a detailed response to the charge that requested review of alternative confinement concepts. In particular, it responded to the sentences:

> "I would like the committee to consider the fundamental investment strategy that we should use in funding alternative concepts. In the near term, however, we would like you to provide us with an assessment of one element within the category of alternative concepts, that of spherical tokamaks."

The Committee had responded previously to the request for an early report on the findings and recommendations with regard to the spherical tokamak assessment in two letter reports that were forwarded to the Director, Office of Energy Research, by the Chairman of FESAC on June 5, 1996. The letter of transmittal and both letter reports are included in this document as Appendix I.

In order to respond to the balance of the charge in a timely manner, FESAC established a working group which reviewed the status of alternative concepts on a world-wide basis and prepared background material, included in this report as Appendix II, to help FESAC in its deliberations.

Also during the July 1996 meeting, FESAC provided a detailed response to the charge in the letter of April 8 that requested review of the inertial fusion energy program. In particular, it responded to the sentences:

> "Questions of scientific merit and appropriate energy relevance have been address positively by the previous reviews. For the near term, however, we would like you to provide us with an assessment of the content of the inertial fusion energy program that advances the scientific elements of the program and is consistent with the Fusion Energy Sciences Program, and budget projections over the next several years."

Again, FESAC established a working group which reviewed the status of the U.S. inertial fusion energy program and prepared background material, included in this report as Appendix III, to help FESAC in its deliberations.

Department of Energy
Washington, DC 20585

March 25, 1996

Dr. Robert W. Conn, Chair
Fusion Energy Advisory Committee
School of Engineering
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0403

Dear Dr. Conn:

This letter forwards two charges intended to follow up on specific recommendations made by your Committee in its Advisory Report on "A Restructured Fusion Energy Sciences Program." The report calls for expeditiously conducting two specific programmatic reviews to help the Department set the technical priorities of the restructured program:

- A Major U.S. Facilities Review
- An Alternative Concepts Review

The first review should be dealt with directly. As indicated by the enclosed charge, the second review is a little more involved and may require a longer time scale to fully address. I would like the committee to consider the fundamental investment strategy that we should use in funding alternative concepts. In the near term, however, we would like you to provide us with an assessment of one element within the catagory of alternative concepts, that of spherical tokamaks. Although the Fusion Energy Advisory Committee (FEAC) has suggested that the Alternative Concepts Review should also encompass inertial fusion energy, DOE is preparing a separate charge on that topic.

Please carry out the Facilities Review and the Alternative Concepts Review in parallel, using additional expertise outside of the FEAC's membership as necessary, so that the restructuring process may proceed. I would like to have your recommendations regarding facilities and, at least, the spherical tokamak aspects of the alternative concept review by mid-April.

The Department is most appreciative of the continued dedication shown by all FEAC members and your willingness to provide advice on important issues as we enter a period of unprecedented changes in the U.S. fusion science program. I will look forward to hearing the Committee's recommendations on these matters.

Sincerely,

Martha Krebs

Martha A. Krebs
Director
Office of Energy Research

Enclosures

Charge to the Fusion Energy Advisory Committee for a Major Fusion Facilities Review

In its report to DOE of January 27, 1996, the Fusion Energy Advisory Committee (FEAC) recommended that a major U.S. fusion facilities review be immediately carried out as part of making the transition to a Fusion Energy Sciences Program. The purpose of this review is to examine the progress, priorities, and potential near-term contributions of TFTR, DIII-D, and Alcator C-MOD (and other facilities as appropriate), and produce an optimum plan for obtaining the most scientific benefit from them. This optimzation should be within the context of the overall recommendations of the report on "A Restructured Fusion Energy Sciences Program" and should work within the funding level for these three facilities in the President's FY 1997 Budget Request.

The Department therefore requests the FEAC to organize and conduct such a review as expeditiously as possible, using whatever approach it deems most appropriate. In carrying out the review, the FEAC is encouraged to involve foreign participants in the review process.

There are specific points that the review should address:

o What are the highest priority near-term (~2 years) scientific objectives to be accomplished with these facilities to advance the goals of the U.S. Fusion Energy Sciences Program?

o What actions could be taken to more effectively use these facilities to address the objectives identified above? For example, changes in theory and modeling collaborations, in international collaborations, in enabling technology capabilities, in operating schedules, and in the allocation of resources among the facilities should be considered.

o In the case of TFTR, if the resources are available to permit operation of TFTR through FY 1997, what are the specific scientific objectives that would merit continuing operations through FY 1997 and into FY 1998? How would you measure progress toward such objectives in a review in mid FY 1997?

The FEAC's findings and recommendations in response to this charge should be delivered to the Director of Energy Research by mid-April.

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:

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

   - recommendations for encouraging new innovations in alternative concepts,

   - 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.

Department of Energy
Washington, DC 20585

APR 08 1996

Dr. Robert W. Conn, Chair
Fusion Energy Advisory Committee
School of Engineering
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0403

Dear Dr. Conn:

This letter forwards the charge that follows up on a specific recommendation made by your Committee in its report, "A Restructured Fusion Energy Sciences Program." The report calls for a programmatic review to assist the Department in setting technical priorities for the Inertial Fusion Energy (IFE) Program.

Inertial fusion has been reviewed often in the past decade, including the Fusion Policy Advisory Committee in 1990, the Fusion Energy Advisory Committee (FEAC) in 1993, as well as two reviews by the National Academy of Sciences during the 1980s. Questions of scientific merit and appropriate energy relevance have been addressed positively by the previous reviews. For the near term, however, we would like you to provide us with an assessment of the content of an inertial fusion energy program that advances the scientific elements of the program and is consistent with the Fusion Energy Sciences Program, and budget projections over the next several years.

Please consider augmenting the expertise of FEAC with appropriate individuals from inertial fusion programs that are active in this country, as well as foreign participants that would be helpful.

I would like to have your recommendations regarding this program by July 1996.

The Department is appreciative of the time and energy provided by the members of FEAC in this continuing effort to improve and orient the fusion energy sciences program to the needs of the times. I will look forward to hearing the Committee's recommendations on this matter.

Sincerely,

Martha Krebs

Martha A. Krebs
Director
Office of Energy Research

Enclosure

Charge to the Fusion Energy Advisory Committee for an Inertial Fusion Energy Review

Since 1990, the fusion program has had a mandate to pursue two independent approaches to fusion energy development, magnetic and inertial confinement fusion. In magnetic fusion, our strategy is to continue to use international collaboration, especially participation in the International Thermonuclear Experimental Reactor, to pursue fusion energy science and technology. In inertial fusion, our strategy has been to assume the target physics is the highest priority activity and would be developed as a part of the weapons research program; and, indeed, the next step in the development of target physics, namely the National Ignition Facility, is proceeding into construction in Defense Programs.

Based on the Fusion Policy Advisory Committee report of 1990, we had taken as our highest priority in inertial fusion energy the development of heavy ion accelerators as the most desirable driver for energy applications. That development program has met all of its milestones and has received numerous positive reviews, including one by the Fusion Energy Advisory Committee (FEAC), which in 1993 recommended a balanced Inertial Fusion Energy program of heavy ion accelerator development, plus other smaller scale efforts, at $17 million per year.

The potential for inertial fusion energy has been judged to be real, but the fusion program no longer has as a goal the operation of a demonstration power plant by 2025. Given that the basic mission of the fusion program has changed from energy development to fusion energy science, and that funding for the entire fusion program will be constrained for some number of years, I would like FEAC to again consider inertial fusion energy and recommend what the new Fusion Energy Sciences program should be doing in support of this future fusion application, and at what level?

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The Honorable Hazel O'Leary
Secretary
U.S. Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585

July 16, 1996

Dear Secretary O'Leary:

The House and Senate Appropriations Committees recommended last week different budget levels, along with different appropriations bill language, for the Department's FY 1997 Fusion Energy Sciences Program. Both the House and the Senate Appropriations Committees commend the community for developing a restructured program plan that meets the objectives of the scientific community as well as the objectives of the Congress and Administration. However, the budget levels appropriated are, in our view, inadequate to implement the restructured program and endanger the community consensus built around this plan.

Your Fusion Energy Sciences Advisory Committee (FESAC) thanks you and the Department for your strong support and urges two actions:

First, that the Department argue vigorously for a budget of at least $250 million, and preferably approaching the President's request.

In our report, "A Restructured Fusion Energy Science Program," FEAC recommended a $275 million funding level and reported that the goals of the restructured program cannot be effectively implemented at budget levels below $250 million.

Second, that the Department seek language from the Congress to allow maximum flexibility in the implementation of the restructured fusion program plan.

In this regard, we note that the specific language in the House appropriations bill will have devastating effects on the program, as detailed in the letter to you from the chair of our Scientific Issues Subcommittee. The FESAC urges you to support language which is not restrictive, but rather which instructs the Department to work with FESAC and the scientific community to determine the allocation of funds that will be most effective in helping to implement the restructured fusion program.

We appreciate your continuing support and look forward to helping the Department implement a vigorous Fusion Energy Sciences Program.

Sincerely,

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

cc: Dr. John Gibbons
Dr. Martha Krebs

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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,

Bob Conn

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

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Dr. Martha Krebs
Director
Office of Energy Research
U.S. Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585

July 17, 1996

Dear Dr. Krebs:

The Fusion Energy Science Advisory Committee (FESAC) transmits to you the report of the FESAC Inertial Fusion Energy Panel, formed to address the issues you raised in your charge letter to me this past April. The Panel, chaired by Dr. John Sheffield, prepared this comprehensive report in a short time and we acknowledge with appreciation all the work of the Panel members.

The FESAC has reviewed and discussed the Panel's findings and funding recommendations, and we support them on the assumption that the President's budget request is approved. The Panel finds that Inertial Fusion Energy (IFE) research is scientifically and technically challenging and fits appropriately as a part of the restructured fusion program. The Panel also finds that the IFE program now conducted by the Office of Fusion Energy Sciences of Energy Research benefits from an "essential symbiotic relationship with the Inertial Confinement Fusion (ICF) program conducted by Defense Programs." The Panel recommends that a joint IFE steering committee between Energy Research and Defense Programs be formed to review the IFE program and related programs in Defense Programs on a regular basis, to ensure strong coordination.

The Panel accepts the findings and recommendations of earlier reports about the heavy ion beam development program. The Panel recommends that \$2 million to \$3 million per year be devoted to non-driver science and technology, with highest priority (beyond heavy ion driver development) being wall protection and cavity clearance schemes and confirmatory simulations of heavy ion driver target performance. The Panel notes that if the budget were to remain at the present level of about \$8 million per year, the pace of development of the heavy ion accelerator would be substantially slowed.

The Panel, while not unanimous about the appropriate budget level, indicates that the budget for the IFE program should be increased to about \$10 million per year for the next few years to resolve both driver and non-driver issues. This would allow the program to make an informed decision on whether to proceed with a full heavy ion driver and target experiment in three to four years while increasing the breadth of the program. FESAC recommends that a final judgement on the proper budget level and program balance await final resolution of the FY 1997 budget for OFES programs.

Sincerely,

Bob Conn

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

Appendix I

The Transmittal Letter and Letter Reports from FESAC to the Director, Office of Energy Research, Concerning the Spherical Tokamak

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June 5, 1996

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

Dear Martha,

In your letter of March 25, 1996, you asked FESAC to address issues related to the alternative concepts in the fusion energy sciences program of the Department. You specifically enclosed a charge to the committee. I in turn have asked the FESAC Scientific Issues Subcommittee (Scicom) chaired by Prof. Jim Callen of the University of Wisconsin to address the charge and prepare a report based on which FESAC could transmit to you our findings and recommendations. We shall have a report from Scicom by July 8 and will address your charge regarding alternative concepts at our FESAC meeting in Washington on July 16-18.

In addition, however, you asked for an early report on the "...findings and recommendations with regard to the spherical tokamak (ST) assessment...." The Scicom has addressed the ST assessment first and transmitted to me the findings of the Scicom Alternative Concepts Panel with regard to spherical tokamak research. I am forwarding their findings to you and trust that this early assessment is helpful to the Department in its planning with respect to ST research. A detailed assessment of U.S. alternative concept research within an international context will be provided as part of our report to you in July.

Sincerely,

Bob Conn

Robert W. Conn
Chair, Fusion Energy Sciences
Advisory Committee

Enclosure

cc:
Dr. Anne Davies, Associate Director, OFES
Prof. J. Callen, Chair - FESAC Scicom
Prof. G. Navratil, Vice Chair, Scicom
FESAC Members

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DEPARTMENT OF APPLIED PHYSICS
TEL (212)854-4457, FAX (212)854-8257

Seeley W. Mudd Building
500 West 120th Street

May 31, 1996

Dean R. W. Conn, Chair, FEAC
University of California - San Diego
Office of the Dean, School of Engineering
9500 Gilman Drive
La Jolla, CA 93093-0403

Dear Professor Conn:

In March you sent by FAX to the FEAC Scientific Issues Subcommittee (SciCom) a copy of a charge to the Fusion Energy Advisory Committee initiated by the March 25 letter from Dr. Martha Krebs to you involving Alternative Fusion Concepts and directed that the SciCom begin to address the issues involved in order to prepare a report to FEAC. In Dr. Krebs' letter of March 25, 1996, the DOE asked FEAC to organize and conduct a review of alternative concepts (charge letter is attached) and specifically to address the following: (1) Review the present status of alternative concept development in light of the international fusion program; and (2) Produce an overall plan for a United States alternative concepts development program including experiments, theory, and modeling/computation and systems studies, that is well integrated into the international alternative concepts program. The DOE asked that the overall review of alternative concepts be delivered to DOE by mid July. In addition, the DOE asked for an earlier report on the "...findings and recommendations with regard to the spherical tokamak assessment..."

In response, the SciCom established an "Alternative Concept" Panel including members from universities, national laboratories, and the international community. (Membership list of the panel is attached.) To date, this panel has met twice, once in Germantown, MD and once in Chicago. The meeting of the Alternative Concepts Panel in Washington, D.C. on March 26 and 27 included presentations from U.S. scientists on spherical tokamaks (agenda attached). More recently, this panel met in Chicago on April 23 and 24 with presentations on stellarators, reversed-field pinches, spheromaks, and field-reversed configurations (agenda attached). This panel is planning one additional meeting June 6 and 7 in San Diego and invitations have been extended for presentations on all other alternate concepts.

This letter transmits the Alternative Concepts Panel findings with regard to its assessment of spherical tokamak research. A detailed scientific assessment of U.S. alternative concept research within an international context will be provided in the panel's final report to be transmitted to you in July.

The FEAC-SciCom has reviewed this interim report of the Alternative Concepts Panel and voted to accept it with 12 in favor (1 opposed and 1 abstention). The SciCom also voted to endorse the panel findings with 12 in favor (1 opposed only to finding 3), 1 opposed , and 1 abstention. Key among these is that the panel finds that the spherical tokamak concept is scientifically ready to move to the "proof-of-principle" stage of development.

Sincerely yours,

Gerald A. Navratil, Vice-Chair, FEAC-SciCom,
on behalf of the Scientific Issues Subcommittee of the Fusion Energy Advisory Committee (FEAC-SciCom), and its Alternative Concepts Panel

Enclosure

SciCom Alternative Concepts Panel
Summary of Findings on Spherical Tokamak Research
April 1996

1. The Panel notes that it would be imprudent now to recommend the proper scope and funding level for spherical tokamak research without completing the review of all alternative concepts. That recommendation will be contained in our report in July. In that context, we expect that spherical tokamak research will be one part of a multi-faceted alternative concept research program.

2. The Panel finds that the spherical tokamak concept is scientifically ready to move to a "proof-of-principle" stage program. This conclusion is based on:

   (a) The growing data base from "concept-exploration" experiments such as START which shows that confinement in spherical tokamaks is "tokamak-like."

   (b) The concept-exploration research has not identified any physics "show-stoppers" to proceeding to the next stage of research.

   (c) A large body of tokamak theory and experimental data which can be extrapolated to lower aspect ratio providing a sufficient basis for proceeding to a proof-of-principle stage program.

3. The Panel finds that research in spherical tokamaks can make an important contribution to fusion plasma physics and fusion energy development. The spherical tokamak research can help resolve key issues of tokamaks because the spherical tokamak concept pushes the tokamak physics to the limit of extreme toroidicity. In this context, the spherical tokamak research fits well with the emphasis of the U.S. tokamak program on advanced tokamaks.

   Preliminary analysis indicates that spherical tokamaks with small size may be possible for fusion energy development and power plants. However, integration of plasma physics and technological issues such as MHD stability and current drive, design of the center-post, edge physics and divertor heat removal, and wall loading limitations set the optimum parameters of spherical tokamaks. These integration issues should also be addressed in a proof-of-principle spherical tokamak program.

4. Spherical tokamak research is moving into a proof-of-principle stage internationally and several proposals for proof-of-principle experiments are pending. One of these, the MAST experiment, is approved for construction in the United Kingdom. The panel notes that fusion research historically has shown there is great benefit in having more than one proof-of-principle-class experiment. Thus, from a scientific perspective, the construction of a proof-of-principle-class device outside the U.S. should not preclude construction of proof-of-principle-class experiments in the U.S. A programmatic decision to construct a U.S. proof-of-principle-class experiment should be based on the benefits anticipated from such an experiment for the U.S. fusion program.

5. The panel finds that new concept-exploration-class spherical tokamak experiments can provide significant cost effective contributions to key spherical tokamak physics issues. Such experiments may be required for a healthy proof-of-principle spherical tokamak program.

Appendix II

The Report from the Sub-Panel to FESAC Concerning Alternative Concepts

ALTERNATIVE CONCEPTS

A Report to the Fusion Energy Sciences Advisory Committee

NAJMABADI @ FUSION.UCSD.EDU

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
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Prof. Gerald Navratil*	Columbia University
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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 <u>investment strategy</u>. 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 investment strategy.

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 $B \leq 0.3$ T, $I = 40$ kA, $R = 0.12$ m, $R/a = 1.5$ and $\kappa = 1.5$. 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 $\beta_{crit} \sim 44\%$, high edge $q \sim 16$ and moderate central $q \sim 2.5$. 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.

The U.S. can play a valuable role in stellarator concept development at the concept exploration level. 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.

An appropriate U.S. focus area is in the effort to reduce the size of stellarator fusion power systems. 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:

Identification of the cause of magnetic fluctuations. 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.

Magnetic fluctuations are the cause of core transport. 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.

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

Identification of the MHD dynamo. 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.

Observation of resistive-wall stabilization. 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.

Oscillating field current drive observed. 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	$\leq$ 1.0 ms
Energy confinement time	$\leq$ 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

Department of Energy
Washington, DC 20585

March 25, 1996

Dr. Robert W. Conn, Chair
Fusion Energy Advisory Committee
School of Engineering
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0403

Dear Dr. Conn:

This letter forwards two charges intended to follow up on specific recommendations made by your Committee in its Advisory Report on "A Restructured Fusion Energy Sciences Program." The report calls for expeditiously conducting two specific programmatic reviews to help the Department set the technical priorities of the restructured program:

- o A Major U.S. Facilities Review
- o An Alternative Concepts Review

The first review should be dealt with directly. As indicated by the enclosed charge, the second review is a little more involved and may require a longer time scale to fully address. I would like the committee to consider the fundamental investment strategy that we should use in funding alternative concepts. In the near term, however, we would like you to provide us with an assessment of one element within the catagory of alternative concepts, that of spherical tokamaks. Although the Fusion Energy Advisory Committee (FEAC) has suggested that the Alternative Concepts Review should also encompass inertial fusion energy, DOE is preparing a separate charge on that topic.

Please carry out the Facilities Review and the Alternative Concepts Review in parallel, using additional expertise outside of the FEAC's membership as necessary, so that the restructuring process may proceed. I would like to have your recommendations regarding facilities and, at least, the spherical tokamak aspects of the alternative concept review by mid-April.

The Department is most appreciative of the continued dedication shown by all FEAC members and your willingness to provide advice on important issues as we enter a period of unprecedented changes in the U.S. fusion science program. I will look forward to hearing the Committee's recommendations on these matters.

Sincerely,

Martha Krebs

Martha A. Krebs
Director
Office of Energy Research

Enclosures

Charge to the Fusion Energy Advisory Committee for a Major Fusion Facilities Review

In its report to DOE of January 27, 1996, the Fusion Energy Advisory Committee (FEAC) recommended that a major U.S. fusion facilities review be immediately carried out as part of making the transition to a Fusion Energy Sciences Program. The purpose of this review is to examine the progress, priorities, and potential near-term contributions of TFTR, DIII-D, and Alcator C-MOD (and other facilities as appropriate), and produce an optimum plan for obtaining the most scientific benefit from them. This optimzation should be within the context of the overall recommendations of the report on "A Restructured Fusion Energy Sciences Program" and should work within the funding level for these three facilities in the President's FY 1997 Budget Request.

The Department therefore requests the FEAC to organize and conduct such a review as expeditiously as possible, using whatever approach it deems most appropriate. In carrying out the review, the FEAC is encouraged to involve foreign participants in the review process.

There are specific points that the review should address:

- o What are the highest priority near-term (~2 years) scientific objectives to be accomplished with these facilities to advance the goals of the U.S. Fusion Energy Sciences Program?

- o What actions could be taken to more effectively use these facilities to address the objectives identified above? For example, changes in theory and modeling collaborations, in international collaborations, in enabling technology capabilities, in operating schedules, and in the allocation of resources among the facilities should be considered.

- o In the case of TFTR, if the resources are available to permit operation of TFTR through FY 1997, what are the specific scientific objectives that would merit continuing operations through FY 1997 and into FY 1998? How would you measure progress toward such objectives in a review in mid FY 1997?

The FEAC's findings and recommendations in response to this charge should be delivered to the Director of Energy Research by mid-April.

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:

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

   - recommendations for encouraging new innovations in alternative concepts,

   - 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.

Columbia University in the City of New York | New York, N.Y. 10027

DEPARTMENT OF APPLIED PHYSICS
TEL (212)854-4457, FAX (212)854-8257

Seeley W. Mudd Building
500 West 120th Street

May 31, 1996

Dean R. W. Conn, Chair, FEAC
University of California - San Diego
Office of the Dean, School of Engineering
9500 Gilman Drive
La Jolla, CA 93093-0403

Dear Professor Conn:

In March you sent by FAX to the FEAC Scientific Issues Subcommittee (SciCom) a copy of a charge to the Fusion Energy Advisory Committee initiated by the March 25 letter from Dr. Martha Krebs to you involving Alternative Fusion Concepts and directed that the SciCom begin to address the issues involved in order to prepare a report to FEAC. In Dr. Krebs' letter of March 25, 1996, the DOE asked FEAC to organize and conduct a review of alternative concepts (charge letter is attached) and specifically to address the following: (1) Review the present status of alternative concept development in light of the international fusion program; and (2) Produce an overall plan for a United States alternative concepts development program including experiments, theory, and modeling/computation and systems studies, that is well integrated into the international alternative concepts program. The DOE asked that the overall review of alternative concepts be delivered to DOE by mid July. In addition, the DOE asked for an earlier report on the "...findings and recommendations with regard to the spherical tokamak assessment..."

In response, the SciCom established an "Alternative Concept" Panel including members from universities, national laboratories, and the international community. (Membership list of the panel is attached.) To date, this panel has met twice, once in Germantown, MD and once in Chicago. The meeting of the Alternative Concepts Panel in Washington, D.C. on March 26 and 27 included presentations from U.S. scientists on spherical tokamaks (agenda attached). More recently, this panel met in Chicago on April 23 and 24 with presentations on stellarators, reversed-field pinches, spheromaks, and field-reversed configurations (agenda attached). This panel is planning one additional meeting June 6 and 7 in San Diego and invitations have been extended for presentations on all other alternate concepts.

This letter transmits the Alternative Concepts Panel findings with regard to its assessment of spherical tokamak research. A detailed scientific assessment of U.S. alternative concept research within an international context will be provided in the panel's final report to be transmitted to you in July.

The FEAC-SciCom has reviewed this interim report of the Alternative Concepts Panel and voted to accept it with 12 in favor (1 opposed and 1 abstention). The SciCom also voted to endorse the panel findings with 12 in favor (1 opposed only to finding 3), 1 opposed , and 1 abstention. Key among these is that the panel finds that the spherical tokamak concept is scientifically ready to move to the "proof-of-principle" stage of development.

Sincerely yours,

Gerald A. Navratil, Vice-Chair, FEAC-SciCom,
on behalf of the Scientific Issues Subcommittee of the Fusion Energy Advisory Committee (FEAC-SciCom), and its Alternative Concepts Panel

Enclosure

SciCom Alternative Concepts Panel
Summary of Findings on Spherical Tokamak Research
April 1996

1. The Panel notes that it would be imprudent now to recommend the proper scope and funding level for spherical tokamak research without completing the review of all alternative concepts. That recommendation will be contained in our report in July. In that context, we expect that spherical tokamak research will be one part of a multi-faceted alternative concept research program.

2. The Panel finds that the spherical tokamak concept is scientifically ready to move to a "proof-of-principle" stage program. This conclusion is based on:

   (a) The growing data base from "concept-exploration" experiments such as START which shows that confinement in spherical tokamaks is "tokamak-like."

   (b) The concept-exploration research has not identified any physics "show-stoppers" to proceeding to the next stage of research.

   (c) A large body of tokamak theory and experimental data which can be extrapolated to lower aspect ratio providing a sufficient basis for proceeding to a proof-of-principle stage program.

3. The Panel finds that research in spherical tokamaks can make an important contribution to fusion plasma physics and fusion energy development. The spherical tokamak research can help resolve key issues of tokamaks because the spherical tokamak concept pushes the tokamak physics to the limit of extreme toroidicity. In this context, the spherical tokamak research fits well with the emphasis of the U.S. tokamak program on advanced tokamaks.

   Preliminary analysis indicates that spherical tokamaks with small size may be possible for fusion energy development and power plants. However, integration of plasma physics and technological issues such as MHD stability and current drive, design of the center-post, edge physics and divertor heat removal, and wall loading limitations set the optimum parameters of spherical tokamaks. These integration issues should also be addressed in a proof-of-principle spherical tokamak program.

4. Spherical tokamak research is moving into a proof-of-principle stage internationally and several proposals for proof-of-principle experiments are pending. One of these, the MAST experiment, is approved for construction in the United Kingdom. The panel notes that fusion research historically has shown there is great benefit in having more than one proof-of-principle-class experiment. Thus, from a scientific perspective, the construction of a proof-of-principle-class device outside the U.S. should not preclude construction of proof-of-principle-class experiments in the U.S. A programmatic decision to construct a U.S. proof-of-principle-class experiment should be based on the benefits anticipated from such an experiment for the U.S. fusion program.

5. The panel finds that new concept-exploration-class spherical tokamak experiments can provide significant cost effective contributions to key spherical tokamak physics issues. Such experiments may be required for a healthy proof-of-principle spherical tokamak program.

FESAC

Science Committee

Alternative Concepts Panel

First Meeting: March 26 & 27, 1996, DOE Headquarters, Germantown

Tuesday March 26 (Morning: Room A-419, Afternoon: Room E-401)

9:00-12:30 Closed Panel Meeting

12:30-1:30 Lunch

1:30-2:00 Masa Ono: "Review of START and CDX experiments"

2:00-2:30 Ray Fonck: "View on Spherical Tokamak Physics"

2:30-3:00 Stan Kaye: "Fusion Physics Issues of ST - Status and Future Investigations"

3:00-3:30 Martin Peng: "The ST Vision: Motivations and Development Path"

3:30-3:45 Break

3:45-4:15 Alan Wootton: "Observations on spherical Tokamaks"

4:15-4:45 Phil Edmonds: "The experimental Requirements of Low Aspect Ratio Tokamaks"

4:45-5:15 Tom Jarboe: "The HIT Program and Spherical Tokamak Issues"

5:30-6:30 Closed Panel Meeting

Wedensday March 27 (Auditorium)

9:00-12:00 Round table discussion with interest parties

12:00-1:00 Lunch

1:00-6:00 Closed Panel Meeting

FESAC
Science Committee

Alternative Concepts Panel

Second Meeting: April 23 & 24, 1996, Hilton O'Hare, Chicago

Tuesday April 23 (Dublin/London Room)

8:30-12:00 Closed Panel Meeting

12:00-1:00 Lunch

1:00-2:15 Stellarators, Jim Lyon et al.

2:15-2:30 FRC, Masaaki Yamada

2:30-3:30 FRC, Alan Hoffman, et al.

3:30-3:45 Break

3:45-4:00 Spheromaks, Masaaki Yamada

4:00-5:00 Spheromaks, Bick Hooper et al.

5:00-5:15 General Comments, Dick Siemon

5:15-6:30 RFPs, Kurt Schoenberg, Stewart Prager, et al.

Wednesday April 24 (Chicago Room)

8:30-5:30 Closed Panel Meeting

FESAC

Science Committee

Alternative Concepts Panel

Third Meeting: June 6 & 7, 1996, UC San Diego, La Jolla, CA

Thursday June 6 (5101 Eng. Building Unit I):

8:30-12:00 Closed Panel Meeting

*** All speakers leave at least 1/4 of alloted time for questions and
*** discussion by the panel.

1:00-1:15 J. Kesner, "Plasma confinement in a Levitated Dipole"

1:15-1:30 M. Mauel, "Experiments to explore the dipole confinement concept".

1:30-2:00 P. Moroz, "The Spherical Stellarator Concept"

2:00-2:30 F. Wessel, "Staged-Pinch for Thermonuclear Fusion"

2:30-3:00 N. Rostoker, "Fusion Reactors based on Colliding Beams in a Field-Reversed Configuration"

3:00-3:30 Discussion

3:30-3:40 Break

3:40-4:10 R. Taylor, "The Electric Tokamak"

4:10-4:40 R. Siemon/K. Schoenberg, "Magnetized Target Fusion"

4:40-5:10 D. Bachelor, "Small Aspect Ratio Toroidal Hybrid, SMARTH"

5:10-5:40 A. Hassam, "Centrifugally Confined Plasmas"

5:40-6:10 G. Miley, "Inertial Electrostatic Confinement Fusion,"

Friday June 7 (584 Eng. Building Unit II):

8:30-5:30 Closed Panel Meeting

FESAC

Science Committee

Alternative Concepts Panel

Comments or E-mails from the Fusion Community

- Charles Hartman, LLNL, General Comments (6/4/96)
- George Miley, Comments on advanced fuels, direct energy conversion, ... (6/3/96)
- George Miley, Comments on inertial electrostatic confinement fusion (6/3/96)
- Ron Miller, General Comments (5/31/96)
- Adil Hassam, General Comments (5/29/96)
- Paul Bellan, General Comments (5/29/96)
- Nick Krall, General Comments (5/16/96)
- Paul Garabedian, Comments on new stellarator configurations (5/16/96)
- Robert Hirsch, General Comments (5/16/96)
- Dick Siemon, Los Alamos, General Comments (4/21/96)
- Ben Carreras, General Comments (4/8/96)
- George Miley, General Comments (3/25/96)

FESAC

Science Committee

Alternative Concepts Panel

Assessment Papers

These "Assessment Papers" are produced in response to a standard set of questions:

- Magnetized Target Fusion by Seimon (3 June 1996).
  (binhex'ed Mac MS Word file).
- SMall Aspect Ratio Toroidal Hybrid - SMARTH by Batchelor (3 June 1996).
  (binhex'ed Mac MS Word file).
- Dipole Fusion Concept by Kesner & Mauel (3 June 1996).
  (Latex file).
- The Electric Tokamak by Taylor, et al. (3 June 1996)
  (binhex'ed Mac MS Word file).
- Colliding Beams in a Field Reversed Configuration by Rostoker, et al. (3 June 1996)
  (binhex'ed Mac MS Word file).
- High Density Magnetic Fusion by Hammer, et al. (3 June 1996)
  (binhex'ed Mac MS Word file).
- Staged-Pinch Fusion Concept by Wessel (25 May 1996).
- Centrifugally Confined Plasmas by Hassam (23 May 1996).
- The Reversed Field Pinch by Prager (April 23, 1996).
  (binhex'ed Mac MS Word file).
- The Spheromak by Hooper & Barnes (April 1996)
  (binhex'ed Mac MS Word file).
- Stellarator by Boozer, Lyon, Shohet, et al. (18 April 1996)
  (binhex'ed Mac MS Word file).
- Field-Reversed Configuration by Steinhauer et al. (March 1996)
- Spherical Tokamaks by Kaye, Ono, Goldston and Peng (27 March 1996)

Appendix III

The Transmittal Letter and Report from the Sub-Panel to FESAC Concerning Inertial Confinement Fusion

July 17, 1996

Professor Robert W. Conn
Dean
School of Engineering
University of California, San Diego
9500 Gilman Drive
La Jolla, California 92093-0403

Dear Professor Conn:

In May, you sent me by fax a copy of the charge to the Fusion Energy Sciences Advisory Committee (FEAC) from Martha Krebs, regarding the Inertial Fusion Energy (IFE) program of the Office of Fusion Energy Sciences. Enclosed is a copy of the Charge.

The panel of technical experts (see Enclosure 2) that I chaired held two meetings in June, one at the Lawrence Berkeley National Laboratory and one at the Lawrence Livermore National Laboratory. We received input from DOE/OFES and DOE/DP/ICF and from numerous experts from the many institutions involved in inertial fusion research.

The new mission of the OFES is to "Advance plasma science, and fusion technology-- the knowledge base necessary for an economically and environmentally attractive energy source for the nation and the world".

Because of the short time given to respond to this charge, we decided to rely on background information contained in the FEAC-7 report of a more extensive review of this subject published in 1993, and to hear mainly about programs since that time.

Our panel has the following findings:

(1) Progress in the IFE program since the 1993 FEAC-7 review has been good, despite its being funded at the $8 million per year level, rather than the then-recommended $17 million level.

(2) A strong IFE program is a proper and important component of the restructured OFES/DOE program. Challenging and relevant scientific issues need to be resolved, notably in collective effects in high current accelerators and beam-plasma interactions.

(3) With DP/ICF physics development and supporting science and technology and the high repetition rate driver development in the OFES/IFE program, the United States is positioned to lead the world in IFE science and technology.

(4) There has been significant progress since 1993; a substantial declassification in the DP/ICF area allows wider participation and more rapid scientific progress; in progress in preparation for the National Ignition Facility (NIF); in target physics; heavy ion accelerator technology; in operation of improved laser systems; operation of light-ion systems; and in improved understanding of power plant issues.

(5) The inertial fusion program involves much exciting science and technology, and there are opportunities because of declassification to broaden the work in the IFE program. The work of LBNL, LLNL and the institutions is of high scientific gravity.

(6) There are numerous challenges in physics and technology but there are no show-stoppers.

(7) The time frame is set by a succession of anticipated events in the DP and the OFES programs. In the restructured OFES program, it is envisaged that there will be "a growing portfolio of new experiments". By 1999, the International Thermonuclear Experimental Reactor Engineering Design Activities will be complete, if the presently proposed schedule is followed the NIF should be well advanced in its construction phase , and the Tokamak Fusion Test Reactor program at the Princeton Plasma Physics Laboratory will be completed. This is a period in which some new initiatives--including one in IFE--should be ready for consideration by OFES. The NIF program is designed to have the capability to ignite a D-T target in the 2005 time frame.

(8) The heavy ion driver is the most promising for energy applications because of its greater efficiency, about 3 times greater than laser driver candidates. Further, the induction linac approach is the most likely to meet performance/cost targets.

In the longer term, breakthroughs in the development of laser systems could change these conclusions, and reassessments should be made on a regular basis.

(9) There is a need for an Integrated Research Experiment (IRE) to have in one facility the ability to resolve basic beam dynamics, final beam focusing and transport issues in a reactor relevant beam parameter regime, and to evaluate the target heating phenomonology. Progress in beam development encourages the belief that the conceptual design of a 3kJ-30kJ, ~100 MeV driver could be developed around 1999, provided there is continued support for accelerator development.

(10) Target physics will not be tested conclusively before the experiment on NIF. LLNL has just completed an integrated simulation of a heavy ion driver target. It is important for other groups to develop new codes and to perform independent confirmatory simulations. Such efforts, would provide an important link between the MFE and the IFE communities.

(11) Several comprehensive conceptual design and system studies have been completed. They show the potential for and the requirements for IFE to provide competitive power plants. The IFE program within OFES should have sufficient breadth beyond driver development to cover those other areas that are critical to its feasibility and competitiveness.

As a first priority, we suggest work on wall protection scheme evaluations and development and confirmatory simulations of heavy ion driver performance. As a second priority, there should be work on cavity clearing technologies at IFE repetition rates and the development of final focusing optics for lasers (we assume that focusing and transport work for beams will be undertaken as a part of the accelerator development program.) As a third priority, work on target factory studies, rep-rated laser systems (a promising area but the present funding level will only support development of the most promising driver), shielding, blanket and tritium studies, and further detailed power plant conceptual design studies.

(12) We suggest that a joint IFE steering committee, between ER and DP, consisting of all interested parties, should review the program on a regular basis, and define the expectations for the ER and DP parts of the program. In addition, this steering committee could facilitate international collaboration.

(13) The position of the Panel is that there should be an increase in the non-driver part of the IFE program, raising it from the present ~$1M per year to $2-3M per year. It is noted that if this were done at a constant level of about $8M per year it would substantially slow the pace of accelerator development. In fact, the FEAC-7 report identifies the $5M per year case as one in which there is no credible program for the development of a heavy ion fusion energy option. The following finding, concerning funding for the IFE program, represents a medial opinion of the Panel. A minority of the Panel would support a more aggressive approach and a comparable minority, a less aggressive approach.

The medial opinion is that funding for the IFE program should be increased to about $10M per year for the next few years to strengthen the scientific and technological understanding of the prospects of IFE and to involve a wide range of institutions in these efforts. Such an annual budget would allow maintaining the pace of heavy ion accelerator development. In total, the program would provide the breadth of support necessary for initiation around the year 2000 of a construction project for an integrated research experiment using a multi-kJ heavy ion driver with a target chamber. An increased budget in the 1999 time frame would be required for developing such a proposal.

Sincerely,

/S/

John Sheffield
Chair, on behalf of the FEAC/IFE panel

Enclosures

Charge from Martha Krebs, Director of DOE Energy Research

"Charge to Fusion Energy Advisory Committee
for an Inertial Fusion Energy Review

Since 1990, the fusion program has had a mandate to pursue two independent approaches to fusion energy development, magnetic and inertial confinement fusion. In magnetic fusion, our strategy is to continue to use international collaboration, especially participation in the International Thermonuclear Reactor to pursue fusion energy science and technology. In inertial fusion, our strategy has been to assume the target physics is the highest priority activity and would be developed as a part of weapons research program; and, indeed, the next step in the development of target physics, namely the National Ignition Facility, is proceeding into construction in Defense programs.

Based on the Fusion Policy Advisory Committee Report of 1990, we had taken as our highest priority in inertial fusion energy the development of heavy ion accelerators as the most desirable driver for energy applications. That development program has met all of its milestones and has received numerous positive reviews, including one by the Fusion Energy Advisory Committee (FEAC), which in 1993 recommended a balanced Inertial Fusion Energy program of heavy ion accelerator development, plus other smaller scale efforts, at $17 million per year.

The potential for inertial fusion energy has been judged to be real, but the fusion program no longer has as a goal the operations of a demonstration power plant by 2025. Given that the basic mission of the fusion program has changed from energy development to fusion science, and that funding for the entire fusion program will be constrained for some number of years, I would like FEAC to again consider inertial fusion energy and recommend what the new Fusion Energy Sciences program should be doing in support of this future fusion application, and at what level."

FESAC/IFE REVIEW

PANEL MEMBERS

Mohamed Abdou	University of California Los Angeles
Richard Briggs	Science Applications International Co.
James Callen	University of Wisconsin
*John Clarke	Pacific Northwest National Laboratory
*Harold Forsen	Bechtel (retired)
*Katharine Gebbie	National Institute of Science and Technology
Ingo Hoffman	Gesellschaft fuer Schwerionenforschung Darmstadt, Germany
John Lindl	Lawrence Livermore National Laboratory
*Marshall Rosenbluth	University of California, San Diego
John Sheffield (chair)	Oak Ridge National Laboratory
William Tang/ Ernest Valeo	Princeton Plasma Physics Laboratory

*Members of FESAC

REPORT OF THE FESAC/IFE REVIEW PANEL

July 19 1996.

Panel members.

Mohamed Abdou	University of California Los Angeles.
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*Katharine Gebbie	National Institutes of Science and Technology.
Ingo Hoffman	Gesellschaft fuer Schwerionenforschung, Darmstadt, Germany.
John Lindl	Lawrence Livermore National Laboratory.
Earl Marmar	Massachusetts Institute of Technology
William Nevins	Lawrence Livermore National Laboratory.
*Marshall Rosenbluth	University of California, San Diego.
John Sheffield (chair)	Oak Ridge National Laboratory.
William Tang/ Ernest Valeo	Princeton Plasma Physics laboratory

* Members of FESAC.

TABLE OF CONTENTS

ACRONYMS

DOE	Department of Energy
DP	Defense Programs
DPSS(L)	Diode Pumped Solid State (Laser)
ER	Energy Research
ESTA	European Science and Technology Assembly
EU	European Union
FEAC	Fusion Energy Advisory Committee
FESAC	Fusion Energy Science Advisory Committee (The renamed FEAC June 1996)
G	Target Energy Gain
GIMM	Grazing Incidence Metal Mirror
HIF	Heavy Ion Fusion
ICF	Inertial Confinement Fusion
ICFAC	Inertial Confinement Fusion Advisory Committee
IFE	Inertial Fusion Energy
ILSE	Induction Linac System Experiments
IRE	Intermediate Research Experiment
ITER	International Thermonuclear Experimental Reactor
kJ	kiloJoule
KrF(L)	Krypton Fluoride (Laser)
LANL	Los Alamos National Laboratory
LBNL	Lawrence Berkeley National Laboratory
LLNL	Lawrence Livermore National Laboratory
MFE	Magnetic Fusion Energy
MIT	Massachusetts Institute of Technology
MJ	MegaJoule
NIF	National Ignition Facility
OFES	Office of Fusion Energy Sciences
PPPL	Princeton Plasma Physics Laboratory
RF	Radio Frequency
SNL	Sandia National Laboratories
TFTR	Toakamak Fusion Test Reactor

A. CHARGE TO PANEL

This report provides an analysis by a Fusion Energy Advisory Committee (FEAC) Panel, of future program options for the Inertial Fusion Energy (IFE) component of the Fusion Energy Sciences Program of the Office of Fusion Energy Sciences. The report is in response to the following request to FEAC from the Director of the Office of Energy Research:

"Charge to the Fusion Energy Advisory Committee
for an Inertial Fusion Energy Review.

Since 1990, the fusion program has had a mandate to pursue two independent approaches to fusion energy development, magnetic and inertial confinement fusion. In magnetic fusion, our strategy is to continue to use international collaboration, especially participation in the International Thermonuclear Reactor, to pursue fusion energy science and technology. In inertial fusion, our strategy has been to assume the target physics is the highest priority activity and would be developed as a part of the weapons research program; and, indeed, the next step in the development of target physics, namely the National Ignition Facility, is proceeding into construction in Defense programs.

Based on the Fusion Policy Advisory Committee Report of 1990, we had taken as our highest priority in inertial fusion energy the development of heavy ion accelerators as the most desirable driver for energy applications. That development program has met all of its milestones and has received numerous positive reviews, including one by the Fusion Energy Advisory Committee (FEAC), which in 1993 recommended a balanced Inertial Fusion Energy program of heavy ion accelerator development, plus other smaller scale efforts, at $17 million per year.

The potential for inertial fusion energy has been judged to be real, but the fusion program no longer has as a goal the operation of a demonstration power plant by 2025. Given that the basic mission of the fusion program has changed from energy development to fusion science, and that funding for the entire fusion program will be constrained for some number of years, I would like FEAC to again consider inertial fusion energy and recommend what the new Fusion Energy Sciences program should be doing in support of this future fusion application, and at what level."

B. REVIEW PROCESS

The panel was briefed by Dr. N. Anne Davies, Director of the Office of Fusion Energy Sciences (OFES) of the Office of Energy Research, and by Dr. David Crandall, Director of the Office of Inertial Confinement Fusion (ICF) and the National Ignition Facility (NIF) of Defense Programs, on the roles of IFE and ICF in the Department of Energy. A summary was given of previous reviews of the IFE program, including that of the Fusion Policy Advisory Committee (1990) and the FEAC Panel 7 (1993). The panel was asked by Dr.Davies, and agreed to assume, that NIF would be built and that the IFE mission belonged in OFES. Presentations were also heard on the progress and prospects in the various areas of the program from a number of the collaborating institutions. Written comments were received from experts in the field. The agendas of the meetings and a list of contributors are provided in Appendix A.

It was agreed that, given the short timescale for conducting this review, the panel would rely on the extensive technical background provided in the FEAC Panel 7 report, supplemented by the more recent information given in presentations and written comments. Updates to some of the appendices of the Panel 7 report are appended -- Target Physics for IFE (Appendix B), and IFE Power Plant Issues and Needed Breadth of Research (Appendix C).

C. OVERVIEW

Inertial confinement of plasmas provides an important fusion option with the potential for a competitive power plant. There are two inertial fusion program elements. The OFES/OER/DOE has the mandate to support energy applications through its Inertial Fusion Energy (IFE) program. The ICF program in DP/DOE is motivated by science based stockpile stewardship. The DP program is funded in FY 1996 at about $240 M/year, about 30 times the OFES inertial fusion energy program. Obviously, much of the key reseach will be undertaken in the DP program. The IFE program must concentrate on energy issues not covered by DP, and try to position itself to apply the results of DP research in the energy area. Significant developments in the ICF program continue to provide crucial scientific and technical results that support the IFE component. It is important to capitalize on this symbiotic relationship between IFE and ICF. Further, progress in the IFE program since the 1993 FEAC-7 review has been good, despite its being funded at the $8M per year level rather than the then-recommended $17 M level.

A strong IFE program is a proper and important component of the restructured OFES/DOE program. Challenging and relevant scientific issues need to be resolved, especially in the areas of collective effects in high current accelerators and beam-plasma interactions. With the ICF physics development in Defense Programs, and supporting science and technology and the high repetition rate driver development in the OFES program, the United States is positioned to lead the world in developing IFE science and technology.

The following finding, concerning funding for the IFE program, represents a medial opinion of the Panel. A minority of the Panel would support a more aggressive approach and a comparable minority, a less aggressive approach. The medial position of the Panel is that there should be an increase in the non-driver part of the IFE program beyond the present level to strengthen the scientific and technological understanding of the prospects of IFE and to involve a wider range of institutions in these efforts. The medial opinion is that, to achieve this goal, the funding for the IFE program should be increased to about $10M per year for the next few years. Such an annual budget would allow maintaining the pace of heavy ion accelerator development. In total, the program would provide the breadth of support necessary for initiation around the year 2000 of a construction project for an integrated research experiment using a multi-kJ heavy ion driver with a target chamber.

D. FINDINGS

1. Progress Since 1993.

• An opportunity for wider participation and more rapid scientific progress has been created by a substantial declassification in the ICF area funded by DOE's Defense Programs;

• The progress in the preparation of the National Ignition Facility (NIF), for which the Inertial Confinement Fusion Advisory Committee (ICFAC, November 1995) indicated that "as far as ignition is concerned there is sufficient confidence that the program is ready to proceed to the next step in the NIF project....";

• Excellent progress in:
- the understanding of target physics through the NOVA program;
- heavy ion accelerator technology;
- operation of improved, fusion relevant, laser systems -- KrF (Nike at NRL), the new Omega Upgrade Direct Drive Facility (U. Of Rochester) and diode pumped solid state development (at LLNL);
- operation of light ion systems that support some beam-target interaction assumptions; and
- improved understanding of power plant issues and refinements that could lead to competitive fusion power plant prospects.

2. Science and Technology.

The inertial fusion program involves much exciting science and technology, as seen in the continuing developments in the target physics area. Although most of the science of target design and implosion is undertaken in the ICF Program, there are opportunities, because of declassification, for a broadening of the work in the IFE Program. The development of energetic, high current density, space-charge-dominated beams and their focussing onto a target involves fundamental science -- instabilities, beam-plasma interactions, plasma lenses, etc. -- and a great opportunity to compare sophisticated computer models with experiments. These developments will have importance broadly across the accelerator field. The development of the drivers and of power plant systems requires innovative new technologies. Work to date has already led to some significant advances.

The panel finds the work at LBNL to be of high scientific quality and was impressed that the ongoing theory and experiments, even at present funding levels, will contribute significantly to the science base required for heavy ion driver development and beam propagation. The complementary IFE programs at LLNL and other institutions have also made impressive progress.

3. **Challenges.**

Many scientific and technological challenges remain to be overcome before the goal of an economic power plant can be realized. Success is not assured although we see no show stoppers. In rough order of importance, the most critical of these are:

- Overcoming the hydrodynamic instabilities (and possible laser-plasma or beam plasma instabilities), and obtaining adequate symmetry to produce a high gain target yield. We must rely on NIF for the basic experimental proof or disproof.

- Providing viable protection of the target chamber against the X-rays, neutrons, blast, and debris to be expected from the pellet explosion. This may be particularly critical for the final focusing optics of a laser system. An analogous issue for heavy ions is finding an adequate mode for beam transport, compatible with the chamber environment that is present with various wall protection schemes.

- Development of a driver with adequate efficiency, rep-rate, and reliability.

- Mass producing targets at a cost of about $0.25 apiece, including their injection and accurate positioning in the target chamber.

All of the above must of course be done at a cost compatible with economic electricity production.

4. **Timeframe.**

The pace and content of the IFE program is driven by a succession of anticipated events in the DP and OFES programs:

- In the Restructured Fusion Energy Sciences Program, it is envisaged that there will be "a growing portfolio of new experiments . . . "

- By 1999, the International Thermonuclear Experimental Reactor Engineering Design Activity will have been completed, the NIF should be well advanced in its construction phase, assuming the presently proposed schedule is met, and the Tokamak Fusion Test Reactor program at PPPL will be completed. This is a period in which some new initiatives -- including one in IFE -- should be ready for consideration by OFES.

- The proposed NIF program is designed to have the capability to ignite a D-T target in the 2005 timeframe.

5. **Opportunity for the U.S. in IFE.**

A strong IFE program is a proper and important component of the restructured OFES/DOE program. Challenging and relevant scientific issues need to be resolved, especially in the areas of collective effects in high current accelerators and beam-plasma interactions. With the ICF physics development in Defense Programs and supporting science and technology and the high repetition rate driver development in the OFES program, the United States is positioned to lead the world in developing IFE science and technology.

6. **Logic for Heavy Ion Accelerator Driver.**

In agreement with previous reviews of inertial fusion energy by the National Academy of Sciences and two FEAC panels, we consider the heavy ion accelerator to be the most promising driver for energy applications. The reasons include the relatively high efficiencies that are possible with accelerators, exceeding 30%, and the demonstrated high reliability of high power accelerators operating at rep rates of several Hz. In contrast, the best laser options - KrF and DPSS - have efficiencies less than 10%. Among the alternatives for heavy ion accelerators, the induction linac (or possibly the recirculating version) is well matched to the multikiloamp currents and submicrosecond pulse lengths required for inertial fusion.

An alternative accelerator approach is the rf/storage ring driver. This approach fits well within the existing European accelerator programs, and is a valuable complementary program. In a presentation at the review meetings, our European panel member agreed that the induction linac has potential cost advantages in comparison with the rf linac/storage ring approach they are exploring.

In the longer term, breakthroughs in the development of laser targets, including direct drive and other approaches (such as the fast ignitor described below) could modify the decision on drivers. Reassessment of the driver and target should be made on a regular basis.

7. Need for Integrated Research Experiment.

Excellent progress has been made in the past by the IFE Program in accelerator development on key issues (e.g., beam bending, merging, pulse compression, final transport) through a series of small scale experiments - closely coupled with theoretical modeling - to understand fundamental aspects of the basic beam phenomenology. These innovative small scale experiments and associated theoretical modeling should continue. However, progress at the level needed to fully evaluate the HIF approach to IFE will also require an integrated experiment capable of resolving the basic beam dynamics issues in the accelerator, studying the final focusing and transport issues in a reactor-relevant beam parameter regime, and evaluating the target heating phenomenology.

With a succession of delays in the funding of the (less ambitious) ILSE project, the IFE team believes a more comprehensive "Integrated Research Experiment" (IRE) should be the focus of the next decade of IFE research and development. The IRE is discussed in more detail in section IID. The overall objective of IRE is to provide the data base needed to support a decision to proceed with the construction of a full scale IFE driver, on a time scale consistent with NIF demonstrations of fusion target performance.

While various options for such a facility have been considered over the years, no particular option has been selected. Consequently, the Panel received only limited information on this topic. Nevertheless, it seems clear that trade studies of various options leading to the development of a conceptual design for the IRE should be a major focus of the heavy ion program over the next two to three years.

8. **Target physics.**

The key scientific issue for any IFE system is target physics. This will not be tested conclusively before the experiments on the NIF. Nonetheless, the best possible simulations are indicated for a program of this importance and scientific value. LLNL has just completed the first succesful "integrated" simulation of a heavy ion driven target. We believe it is important for other groups to develop new codes and perform independent confirmatory simulations as one element in a driver decision. We believe that the recent declassification makes this feasible, and that this essential task could be undertaken by an MFE theory group, providing an important link between the MFE and IFE communities with eventual mutual enrichment. Developing new target physics codes is a challenging multiyear project. In the interim, MFE theorists could contribute to such issues as beam propagation, and participate in target design using existing codes.

9. **Program Needs Derived from Power Plant Studies.**

Several comprehensive, conceptual design and systems studies have been completed. They show the potential for and requirements for IFE to provide competitive power plants. Other than development of the driver, the key issues are:

- Demonstration of high gain at moderate driver energy.
- Development of chamber technology, including wall protection and cavity clearing schemes at power plant repetition rates.
- Development of power plant technologies to provide tritium self-sufficiency, radiation shielding, radiation resistant materials, and low-cost target production.

The IFE program within OFES must have sufficient breadth, beyond driver development, to cover those other areas that are critical to its feasibility and competitiveness. Progress in these areas will influence driver research priorities and should provide the data needed in the near term to perform meaningful experiments on NIF that are important to IFE.

10. Priorities Outside Heavy Ion Accelerator Development.

The panel suggests the following priorities for the broader program:

First priority:

- Wall protection scheme evaluations and development.
- Confirmatory simulations of heavy ion driver target performance.

Second priority:

- Cavity clearing technologies at IFE repetition rates.
- Development of the final focussing optics for laser systems. (It is assumed that final focussing and transport studies for heavy ion beams are undertaken as a part of the accelerator development program.)

Third priority:

- Target factory studies.
- Work on rep-rated laser systems. This is an important area but until IFE funding increases substantially, development of only the presently most promising driver can be afforded.
- Shielding, blanket and tritium studies.
- Detailed power plant conceptual design studies. The extensive studies made in recent years have identified the principal issues for IFE. It is time now to concentrate the scientific and technological studies on these specific issues.

11. Roles of DOE/Energy Research and DOE/Defense Programs, and International Collaboration.

This Panel has reviewed and commented on the IFE program conducted by the OFES of Energy Research. The program benefits from an essential symbiotic relationship with the ICF program conducted by Defense Programs. The Panel notes that the NIF program expects to offer testing time to a range of institutions and program interests. A 1994 workshop, organized by DP, identified a wide range of IFE relevant issues that could be addressed by NIF. The Panel is not in a position to comment on the balance between the various elements of the DP program, but feels strongly that greater clarification is needed regarding possible implementation of these

IFE relevant elements of the DP-supported ICF program.

A joint IFE steering committee between ER and DP, consisting of all interested parties, should review this program on a regular basis.

In addition, such a committee might be used to facilitate international cooperation in IFE. This FESAC/IFE panel did not review the foreign programs, except for a brief discussion of some European developments (see IIC). We note, however, that the French are building a NIF-scale facility, that there is a proposal in Europe to expand IFE, and that there are significant IFE programs in Japan and Russia.

12. **Budgets.**

The position of Panel is that there should be an increase in the non-driver part of the IFE program beyond the present level to strengthen the scientific and technological understanding of the prospects of IFE and to involve a wider range of institutions in these efforts. We believe that this is needed even though there is a large measure of breadth because of related DP-funded efforts. For a total OFES/IFE budget in the range of $8M or greater, this total investment in non-driver science and technology should be $2M - $3M per year.

The following finding, concerning funding for the IFE program, represents a medial opinion of the Panel. A minority of the Panel would support a more aggressive approach and a comparable minority, a less aggressive approach. The medial opinion is that funding for the IFE program should be increased to about $10M per year for the next few years to strengthen the scientific and technological understanding of the prospects of IFE and to involve a wide range of institutions in these efforts. Such an annual budget would allow maintaining the pace of heavy ion accelerator development. In total, the program would provide the breadth of support necessary for initiation around the year 2000 of a construction project for an integrated research experiment using a multi-kJ heavy ion driver with a target chamber. An increased budget in the 1999 timeframe would be required for developing such a proposal.

At the present OFES/IFE budget level of $8M, a significantly increased investment in program breadth is desirable but would be achieved at the expense of a substantial slowing of the pace of development of a heavy ion accelerator. At lower budget levels, the elements of the program would have to be done serially rather than in parallel, delaying the pace of the program beyond that needed to meet the goals above. At some lower level, it would be impossible to mount a coherent driver development program. The FEAC Panel report identified the $5M/year case as one in which "there is no credible program for the development of a heavy ion fusion energy option."

II BACKGROUND INFORMATION

A. Target Physics.

The gain required for an ion-beam power plant can be estimated from the requirement that the recirculating electrical power should be limited to about 25%, and hence 10% of the output fusion thermal power. For an assumed accelerator efficiency of 35%, gains of about 30 are needed.

Recent LLNL integrated calculations of 2-sided, indirectly driven ion target designs predict a gain of 40-50 with a 6-7 MJ driver capable of focussing to a 6 mm radius spot size. These calculations consider the ion energy conversion to X-rays in the target, and the subsequent radiation transport and pellet implosion. Most of the calculation involves the same physics as that involved in the LLNL NIF laser implosion predictions, which have been verified by LANL simulations. The validity of these codes has been tested against Nova experiments and judged (by ICFAC for example), to provide an adequate basis for proceeding with NIF. We conclude by analogy that an adequate basis of target physics exists for proceeding with consideration of other aspects of an HIF design. A wide variety of possible target designs for HIF requires further study. It is very likely that more optimum designs are feasible. We believe that it would be desirable if independent propagation and target physics codes would be implemented and we recommend that the participation by scientists from one or more MFE groups be encouraged.

There are alternative concepts for IFE reactors. Direct drive targets, while requiring very high uniformity, allow better coupling of driver energy to compressed fuel (by a factor of 2-5) and hence potentially higher gain. Such advantages in gain might allow KrF lasers or DPSSL's to overcome the large efficiency advantages of HIF. Experiments on the Omega facility (University of Rochester) and NIKE facility (NRL) should give some quantitative data on these prospects in the next several years. Direct drive HI targets are in principle feasible, but questions regarding deposition nonuniformity from such sources as beam overlap and multiple-beam interactions have not been adequately evaluated.

Still more drammatic improvements in gain or minimum size may be available with the fast ignitor. Many physics and technology issues remain to be explored, and the first significant data base on this exciting new prospect will become available in the next 2-3 years on Nova.

We conclude that indirect-drive HIF remains the driver option of choice. Enough data should be forthcoming on direct drive and fast ignitor prospects in the next 3-4 years, that it should be possible to better evaluate the prospects of IFE with lasers at that time.

B. **Heavy Ion Accelerator (Progress, Issues and Prospects).**

1.) Progress since 1993 on issues identified by FEAC panel 7 (page 7)

The LBNL injector program has demonstrated the production and acceleration of a single driver-scale ion beam, in a linear geometry. The parameters of the beam are 2MeV, 0.25mC/m (790 mA) of K^+, with emittance of 1mm-mr. Beam energy variation (< ±0.15%) is also consistent with the full-scale driver requirements. The goal of producing a multi-beam injector was not met because funding was not provided. A schematic diagram of an accelerator experiment, indicating issues and progress, is shown in the figure below.

Matching a high-current beam into an alternating gradient (quadrupole) channel is important. Experiments are beginning with a 6-quadrupole matching section; 3-D computer simulations project succesful operation.

Transverse beam combining is considered advantageous because it allows for electrostatic quadrupole transport of many beams (at low energy) with small apertures. Once combined (at about 100 MeV), subsequent acceleration and transport is carried out with magnetic quadrupoles that have large apertures. Beam combining experiments have begun at LBNL.

Transport of a low-current space charge dominated beam (mAs) through a 7-quadrupole magnetic focussing system has been achieved successfully at LLNL. Construction has started at LBNL of a high current (800 mA) system.

Recirculation is being investigated; potential advantages include reduced total length, saving on total number of induction modules, and allowing smaller individual induction modules. An overall reduction in cost could thus be realized. Many issues must be dealt with here: beam control is likely to be more difficult; emittance growth in a curving beam with space charge effects needs evaluation; the pulsers must be programmed with a different time history for each pass of the beam; energy recovery from dipoles appears necessary; and vacuum requirements are significantly more stringent (~ 2 orders of magnitude). A prototype recirculator is being developed at LLNL to address many of these issues experimentally; it is not expected to have a functioning 360 degree ring before FY98.

Final focusing of the beam onto target presents numerous scientific and technical challenges. Preliminary experiments have begun at LBNL; on self-focusing (*plasma lens*), have led to a 20-fold increase in beam intensity; and on laser-induced plasma channel guiding; much more work needs to be done in this area in the future.

In parallel with the experimental investigations, theoretical modeling of beam transport and dynamics has made excellent progress in the last few years. Highlights include: particle in cell simulations of beam merging results; detailed modeling of beam transport through electrostatic quadrupoles, with space-charge effects; simulations of the recirculator approach, which are used to help design the experiments; evaluation of resistive wall mode effects on longitudinal beam stability; numerical studies of chamber focusing and transport, including effects of charge and beam neutralization; investigation of beam-beam interactions for multiple beams converging near the target.

There has been a number of hardware developments. Lower cost ferromagnetic materials have resulted from making better use of industrial products. High repetition rate, reliable, flexible waveform controllers and generators have been developed for beam acceleration. Low-cost pulsed magnetic quadrupoles and a high gradient (100 kV/m) electrostatic quadrupole system has been developed.

The studies described above were carried out primarily to support the design and experimental program of the Induction Linac Systems Experiments (ILSE) accelerator. The advances described above would allow an ILSE-type accelerator to have twice the performance at a similar cost to the original proposal. This experience leads to the expectation that much larger gains in performance will be achieved in the proposed program over the next few years. For these reasons the program is considering an integrated experiment with a 3-30kJ accelerator as the next step.

2.) Issues in the near term program.

- Continued development of ion sources to achieve longer life and lower emittance is needed.
- Development should continue on compact multi-beam, high current injectors.
- A demonstration is required of the injection and multi-pass recirculation of a space charge dominated beam, while maintaining beam quality.
- The maximum transportable current density limits should be determined.
- Validation of beam simulation codes for 100's of lattice periods is required.
- Demonstrations of beam combining are required with validation of codes, and of beam focussing with and without neutralization.
- Development is also required of low cost components and assembly techniques.

3.) Feasibility of Heavy-Ion-Beam-Drive for High-Gain Targets:

It must be demonstrated that high-gain targets can be driven by heavy ion beams. Some modeling has been carried out to investigate this very broad issue, and there is some related information from light ion target designs and simulations. Recent simulations from LLNL, using the modeling developed for NIF, predict adequate gain for ion-beam indirect-drive targets. These simulations are supported by a wide variety of data from the NOVA laser at LLNL. Much of the detailed experimental evaluation of the prospects for ion-driven ignition and gain must await results from NIF. In the meantime, development of indirect drive target designs for NIF, which are ion-beam relevant, should continue.

4) Additional Science & Technology Questions.

a) *Focusability*: The ability to maintain beam quality (focusability) at high current is the principal scientific challenge for the development of HIB drivers. In addition to the topics and progress noted in section 1 above, some additional physics issues worthy of consideration include:

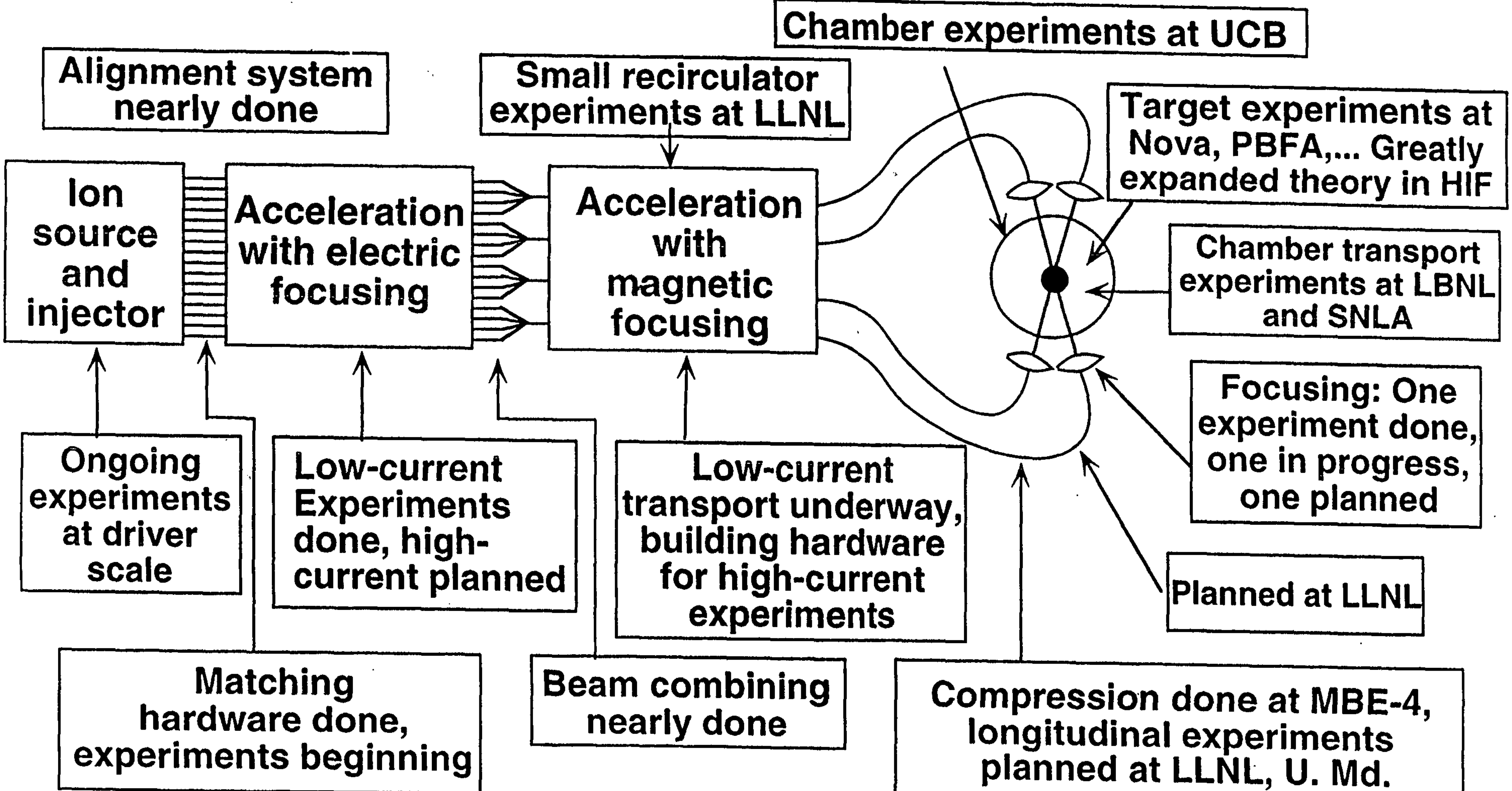
(i) The goal of developing a capability to do "end-to-end (of the accelerator)" simulation of beam propagation is expected to play a key role in optimizing the MJ driver design. A linear driver will pass the beam through of order a thousand lattice periods. Therefore, experimental validation of code accuracy over long times will be important. Existing particle-in-cell (PIC) methods have shown good agreement in short experiments, and have been used to obtain converged results over hundreds of lattice periods. However, maintaining a sufficiently low noise level for long-time accuracy will be computationally challenging. The much longer beam path in a recirculator driver makes it even harder to model. Intermediate tests of understanding in this key area of long-time transport are expected to come from the small recirculator experiments (of order 300 lattice periods) and possibly more efficient "reduced" description simulation methods. Experimentation should help to determine whether piecing together results from separate analyses of carefuly selected elements/accelerator modules is adequate to accurately describe an entire machine.

(ii) The physics and feasibility of self-pinched propagation in the chamber remains an important and open issue. Experiment/theory tests on this subject would be valuable.

(iii) The filamentation of an HIB driver for ICF is an important issue that could benefit from some reexamination. Earlier studies [E. P. Lee, et al. Phys. Fluids 23, (1980) 2095] considered the growth of filaments in a charge-neutralized ion beam propagating through a resistive plasma medium. They concluded that filamentation required higher pressure than the ~ 1 mtorr present in current fusion chamber designs. Although these results are reasonable, powerful new computational capabilities can profitably be used to examine higher density regimes of interest.

b.) *Beam-target interaction*: Intense radiation from the target, produced when the target is heated by the early time portions of the beam, can affect propagation of the remainder of the beam. Langdon et al's calculations [A.B.Langdon, Nucl. Instr. and Methods in Physics Res. A 278, p 68, 1989, and also Carlo Rubbia, Nucl. Instr. and Methods in Physics Res. A278, p 253, 1989] indicates that "photoionization of half the beam by the time it propagates to within 20 cm of the target is likely." A later more accurate kinetic calculation following a slice of ion beamlets, as they merged and hit the target, showed a 5% loss of ion deposition within the intended 3 mm radius spot (A.B.Langdon, Particle Accelerators, Vol. 37-38, p ,175-180, 1992). This calculation assumed no neutralization due to collisional effects and photoionization of vapor in the chamber. Such neutralization effects further reduce the electric field and the trajectory changes. This issue should be included in the examination of all potential focussing schemes.

Experiments addressing nearly all subsystems and manipulations are in progress or are being designed



These scaled experiments do not have enough current or length to address some important issues.

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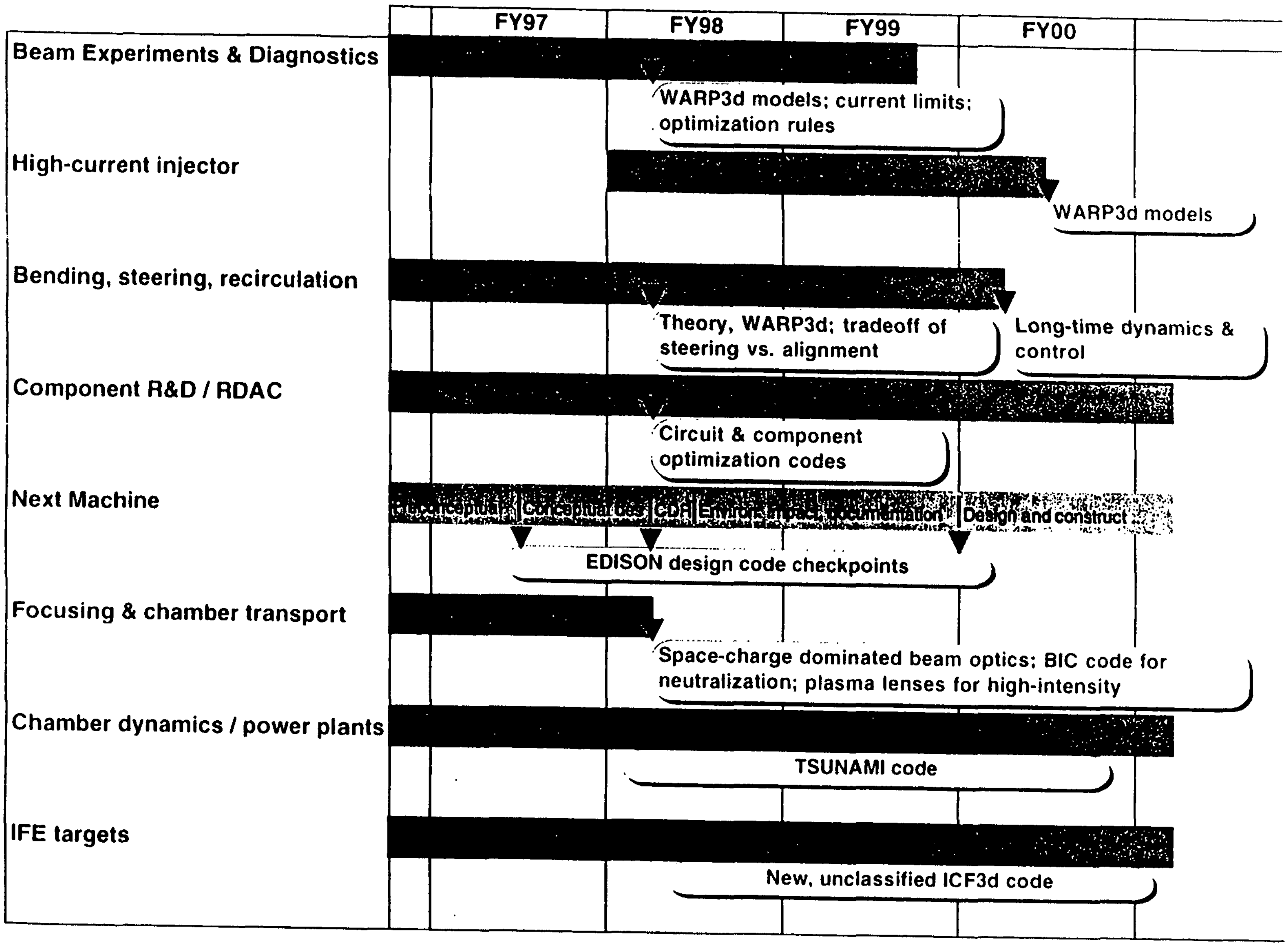
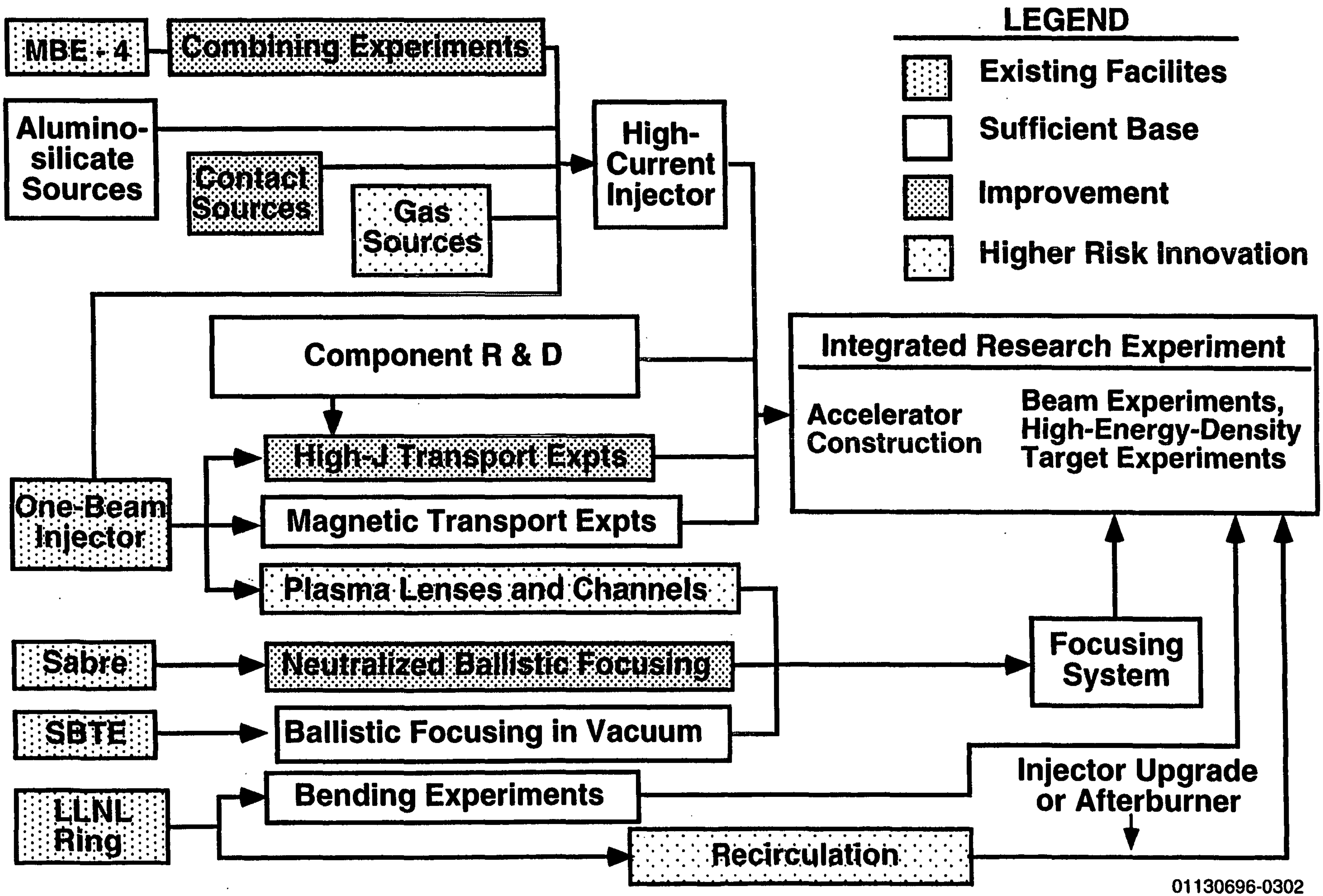
Key validations of theory and simulation



Structure of near-term experimental program



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IFE National Plan

ID	Task Name	Cost
1	TOTAL 4-YEAR PLAN (in $M)	$48.70
2	Plan Approved	$0.00
3	Accelerator Theory, Simulation	$16.40
4	General	$6.20
5	Target Design	$2.40
6	Induction	$6.20
7	Foreign Exchanges, R.F.	$1.60
8	Beam Experiments, Diagnostics	$1.00
9	Combining	$0.40
10	Matching	$0.40
11	Plasma Lens	$0.00
12	Magnetic Transport	$0.20
13	High Current Injector	$5.10
14	Ion Sources	$0.80
15	Alumino-silicate	$0.00
16	Contact	$0.00
17	Gas	$0.00
18	Injector	$4.30
19	High-J Transport	$1.70
20	Component R&D	$10.60
21	Industrial Collaboration	$2.80
22	Vacuum, Alignment, Cavities...	$1.20
23	Insulators	$0.80
24	Metglas and Cores	$1.50
25	Pulsers	$1.10
26	Electrostatic Quads	$0.80
27	Superconducting Quads	$2.40
28	Accelerator Project, Expts.	$4.00
29	Preconceptual Design	$0.00
30	Conceptual Design	$0.40
31	Write CDR	$0.40
32	Project Prepration	$1.20
33	RDAC	$2.00
34	Construction	$0.00
35	Acceptance	$0.00
36	Experiments	$0.00
37	Bending and Recirculation	$4.50
38	Bending and Steering	$1.50
39	Recirculation	$3.00
40	Chambers and Power Plants	$5.40
41	Focusing Experiments	$1.10
42	Ballistic Focusing	$0.30
43	Neutralized ballistic	$0.00
44	Plasma Lens (cost only)	$0.40
45	Channels	$0.40
46	Chambers and Power Plants	$4.30
47	Target Inj. and Tracking	$0.30
48	Chamber Dynamics	$4.00
49	Defense ICF Program	$0.00
50	Complete Program	$0.00

Project: IFE Program
Date: Mon 7/1/96

Task		Summary		Rolled Up Progress	
Progress		Rolled Up Task			
Milestone	◆	Rolled Up Milestone	◇		

C. A **European Perspective. Ingo Hofmann, GSI Darmstadt.**

At GSI Darmstadt (a major German national laboratory in heavy ion nuclear and applied research) there exists a laboratory commitment to develop heavy ion drivers and beam physics as well as plasma physics experiments (with heavy ions) towards the goal of IFE based on the RF Linac & Storage Ring concept. This is complemented by a basic science program funded by the Federal Ministery of Research on "High Energy Density in Matter" since 1980 (beam plasma experiments, target theory and driver development), which supports primarily University groups, again with GSI in a lead lab role. Both programs add up to approx. 2 Mio. DM/y. [An addendum: as far as the relatively "low-level" funding of HIF in Europe one should keep in mind that, generally, salaries of scientific staff are not included and that the GSI facility is a large investment (300 Mio. DM) which came from other sources].

In other European countries (except Russia) there are smaller groups and individuals in a number of institutions who work on different aspects of HIF. I estimate these efforts as presently < 0.5 Mio. DM/y. The feasibility study proposal "Ignition Facility" submitted to the European Union would allow establishment of a formal European collaboration within the "keep-in-touch" position towards ICF (in total 1% of the yearly 200 million ECU fusion budget). Although a "Study Group" has been inaugurated in March 1995, the decision on behalf of the EU is still pending. It should be mentioned here that the report of the recent ESTA (European Science and Technology Assembly) working group, established by the previous Commissioner for Energy Research as a consulting body, was in favour of gradually raising the 1% level for ICF to 10% of the total fusion budget. This is to be seen in part as a consequence of the US declassification in energy related ICF.

In Russia there is a collaboration between Arzamas (their former weapons lab) and ITEP/Moscow with the purpose of using the existing proton/heavy ion synchrotron at ITEP for target experiments at the kilojoule level, which requires some hardware extension to implement a foil stripping device. According to unofficial information this project expects funding at the $10 million. level (in total).

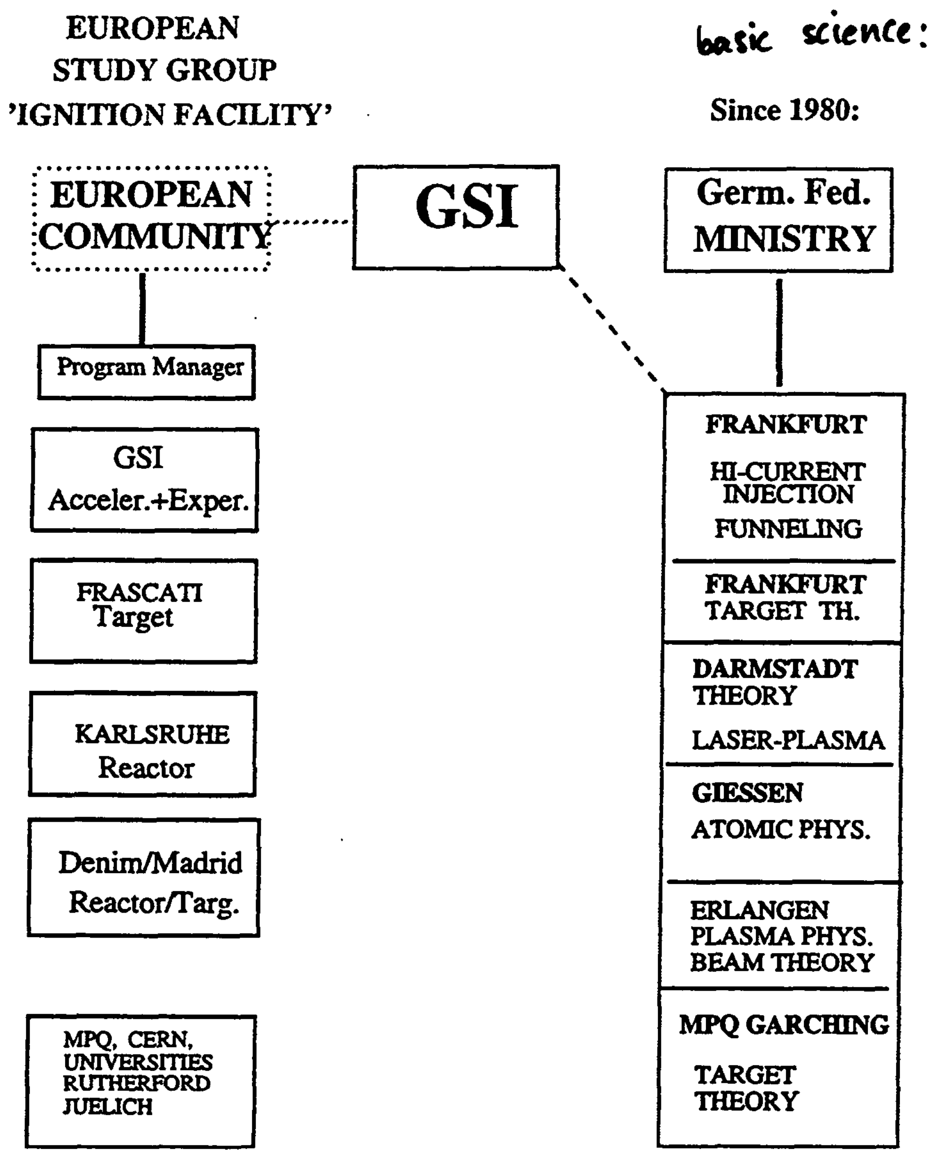
2. Technical Prospects RF vs. Induction Approach

The RF approach is based on broad experience with linacs and storage rings, however not under the extreme beam power density conditions required for HIF. In the European Study we are not yet in a position to say how many storage rings and final beam lines are really needed for a reactor driver. The induction approach is highly innovative and appears to have a larger cost saving potential due to its very high current capabilities. Since both schemes are still in a research phase they need to be pursued as complementary approaches. There is a lot of synergism which opens possibilities for effective collaboration in a number of beam physics issues, including final focusing.

3. Beam Physics - a Science?

In my estimate the LBNL/LlNL beam physics group is doing excellent work and has developed capabilities which are unique in their kind. The codes are used under the special technical boundary conditions of injectors and the induction acclerator, where they have developed an extremely high standard of modeling. Applying their 3-D simulation tools to areas of concern in the larger accelerator community (including the RF approach to HIF) would be an excellent opportunity to foster the links with the broader field and give the group the recognition it truly merits. At the same time, confidence in their simulation tools would build up in the accelerator community. I believe that it is largely the detachment from too specialized an accelerator environment (especially at low energy) which is a condition for recognition of beam physics as science.

HIF PROGRAM



I. Hofmann

D. Integrated Reasearch Experiment.

The overall objective of an Integrated Research Experiment (IRE) is: to provide the capability to investigate the science of heavy ion beam/target interactions; and to provide a data base that, together with the results from the broadened base program and NIF, will be sufficient to support a decision to proceed with the construction of a full scale heavy ion IFE driver. The design parameters for this proposed experimental facility are not fixed at this point, although a number of representative examples of facilities at about the right scale have been studied in the past.

The overriding issue in the development of heavy ion accelerators is the transport and beam control of very high power, high brightness ion beams. The generation, axial compression, and merging of multi-beam, high-current, heavy ion beam pulses in the presence of strong electromagnetic interactions with the accelerator structures must be carried out, while maintaining a good beam emittance (brightness). There are no fundamental impediments, but it is clear that a variety of passive and active beam control systems are needed. Experiments at the scale of the IRE are essential to develop the experience and understanding needed before a full scale driver can be designed with confidence.

The induction accelerator technology has demonstrated adequate reliability, rep-rate capability, and efficiencies in moderate scale experiments. The main issue in the technology area is achieving these performance capabilities at a low enough cost to meet the economic goals.

The committee concurs with the IFE Program's description of the science and technology elements that should be included in this integrated experiment:

- The IRE should provide the experimental capability for resolving the basic beam dynamics issues involved in the generation, acceleration, and pulse compression of a heavy ion beam, through the accelerator and through the beam transport to the target chamber.
- It should be capable of studying experimentally a wide range of schemes for focusing and transporting the heavy ion beam onto the target, including vacuum ballistic transport, plasma neutralization, plasma channel transport, and self-focused transport.

- It should provide an experimental evaluation of energy deposition and target heating with heavy ion beams in hot ionized matter, in the temperature regime of a 100 eV or more, including any effects that radiation from the target might have on the focusing and steering of the ion beam passing through various background gases in the target chamber.
- The operation of this facility, at a rep rate of several Hz, will also provide engineering data on the efficiency, reliability, and costs at a scale that will allow meaningful extrapolation to a full scale induction linac driver.

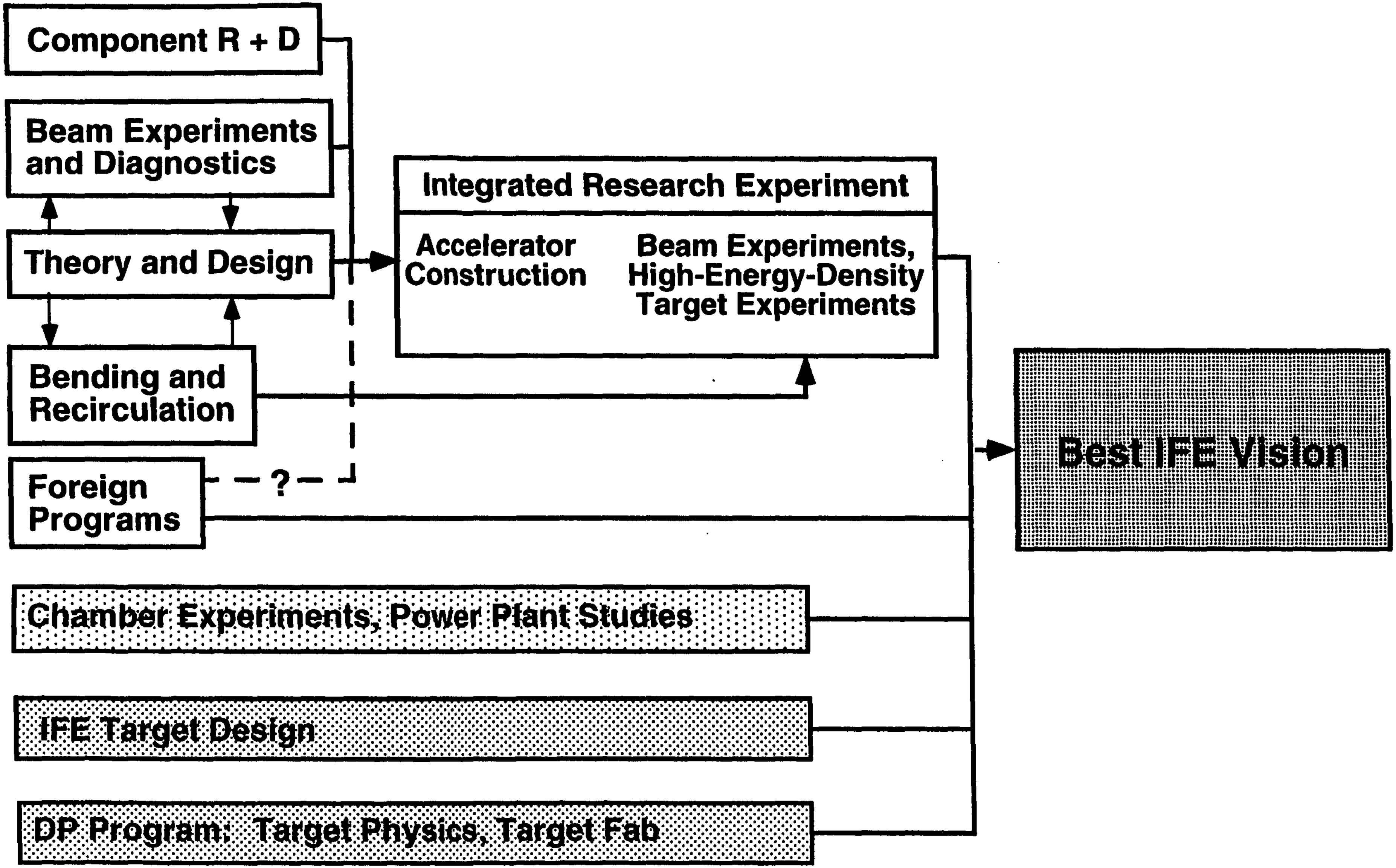
To achieve these objectives, the IRE should be designed with the flexibility for experimental studies over as wide a range as practical, both in the operational modes of the beam in the accelerator as well as the beam parameter variations possible for final focusing, transport, and target heating studies. For example, with plasma-based ion sources, a range of ion masses is possible in principle, if the appropriate flexibility is provided in the beam transport system.

The challenge faced by the IFE Program in the design of the IRE is how to achieve these objectives at an affordable cost. The general parameter range under consideration is a pulse energy in the range 3 to 30 kilojoules, at a beam voltage of 100 to 300 MeV (with singly charged K, for example). At a pulse length of order 10ns (after compression, at the target) the beam current is several kiloamps. The beam current in the accelerator should then be several hundred amps, sufficient to reach the "heavy" beam loading regime necessary for high efficiency operation of the accelerator cells. It is also necessary to be in this regime to fully evaluate the longitudinal dynamics of the beam in the presence of significant feedback from beam loading of the accelerator cells. This feedback is especially important in understanding the amplification of current waveform fluctuations (klystron-like bunching modes), and the viability of various correction schemes for maintaining smooth pulse waveforms.

To accurately model the phenomenology of a full scale driver in a machine that is about 10-20 x smaller, scaling of several of the key parameters is necessary. Major variables that have a significant effect on the cost include the final beam voltage, the pulse length (or the joules in the pulse), and the ion mass. Over the next two years, trade studies to identify the most promising parameter sets for the IRE should have a very high priority.

Previous designs of a so-called "High Temperature Experiment" (HTE), with many of the same objectives, explored a similar parameter regime, see for example, "Accelerator Inertial Fusion -- A National Plan for the Development of Heavy-ion Accelerators for Fusion Power", Los Alamos National laboratory Report LA-UR-81-370, Dec. 10, 1981, and "Heavy Ion Accelerator Research Plan for FY84-FY89", Los Alamos national laboratory Report LA-UR-83-1717, May 1983.

IFE Program Structure



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E. **Progress on potential laser drivers for IFE.**

Both KrF and Diode-Pumped Solid-State Lasers (DPSSL's) have potential as drivers for IFE. Although both laser systems have projected laser efficiencies of less than 10% for IFE applications, the projected target gains for Direct Drive targets could be high enough for economical energy production. Although quite speculative, the potential enhanced gain of direct drive targets ignited by a fast ignitor laser beam could further relax the laser efficiency requirements, or reduce the laser energy required for IFE.

Since 1993, significant progress in the ICF Program has been made in developing both the target physics and technology required for Direct Drive IFE with lasers . The NIF is being designed to allow testing of Direct Drive targets. Programs to establish the laser requirements for laser beam smoothing and hydrodynamic instability control are being actively pursued on the recently completed Omega glass laser at the University of Rochester and the KrF Nike laser at the Naval Research Laboratory.

The 60 beam Omega laser is capable of delivering 30-45 KJ of laser light at 0.35 mm in a flexible pulse shape. Omega is the principal U.S. facility for exploring direct drive implosions and will be used for establishing the requirements for direct drive ignition on the NIF.

The 56 beam KrF Nike laser can deliver 2-3 kJ of energy at 0.248 mm to planar targets. Nike will be used primarily for the study of imprinting (target perturbations created by laser intensity variations in the laser beam), and subsequent hydrodynamic instability growth. Individual Nike beams have achieved spatial intensity uniformity of about 1% when averaged over the 4 ns duration of the laser pulse. This a factor of several better than can currently be achieved with glass lasers although improvements planned for Omega are expected to significantly improve its beam quality.

System studies of KrF lasers have concluded that 5-7% efficiency is feasible (perhaps somewhat more if waste heat from the amplifiers is recovered). The Nike laser, which was not designed for efficiency or high repetition rate, operates at about 1.7% efficiency. For IFE, amplifiers would need to be developed which demonstrate the required efficiency, repetition rate and durability.

Flashlamp-pumped Solid-State lasers do not have the efficiency or heat handling capability required for IFE. For example, the NIF, as designed, will operate at about 1/2 % efficiency. However, solid state lasers which use a gas-cooled crystal gain medium, pumped with efficient diode lasers have projected efficiencies near 10%. Many elements of such a system have been demonstrated on a small scale at LLNL. A 2 joule DPSSL at LLNL, which used the crystal Yb:S-FAP as the gain medium, has operated at 25 Hz with gas cooling and has demonstrated an ability to handle heat fluxes in excess of those required for IFE. Larger scale DPSSL lasers would take advantage of the technology developed for the NIF. A major issue for DPSSL's is the cost of diodes. For IFE applications, diode costs of $ 0.10/watt or less are required. Current diode costs are about $10/watt and the cost goal for diodes to be used on the NIF is $1/watt. Diodes have a variety of commercial and military applications and their price is projected to decrease as these markets grow.

A generic issue for laser IFE is protection of the final optics against neutrons, X-rays, and debris from the target and chamber. Grazing incidence metal mirrors (GIMM's) and self-annealing fused silica optics operated at several hundred degrees Centigrade have been proposed as solutions. An OFES sponsored program to further evaluate possible optics protection approaches could help establish criteria for determining laser requirements.

DP is supporting a modest development effort on DPSSL's and a research program on the fast ignitor. At present there is no funding for KrF rep-rated high power amplifier development. Although we are not recommending an OFES program on laser driver development at this time, we do recommend that OFES continue to evaluate progress on laser drivers and direct drive targets in DOE Defense Programs. We also recommend that OFES act to encourage international collaborations with the U.S. on laser driver developments directed toward IFE.

F. IFE Power Plants (Progress and Needs)

A number of excellent, comprehensive, conceptual design and system studies for IFE power plants have been completed over the last few years. Innovative concepts have been developed through these studies, and they have contributed to providing a greater understanding of the prospects and issues for IFE. These studies have shown the promise of IFE as a competitive energy option. The key technical issues, derived from this work, are listed in Table 1.

The target physics and performance, and target-beam interactions will be addressed primarily by the DP program, partly in the R&D for NIF, and then through experiments on NIF.

Several issues affect the viability of fusion chamber designs for IFE. The first issue concerns the feasibility and performance of a viable wall-protection scheme. A practical IFE system requires protection of the solid chamber wall from rapid degradation due to the extremely high instantaneous heat and particle loads associated with the X-rays and debris from the target explosion. While researchers agree on the need to protect the solid chamber wall, there is no consensus on the best means to achieve this. The two leading schemes proposed for wall protection are : 1) thick liquid layer, and 2) thin liquid layer. In the first scheme, a thick layer of a liquid, e.g. flibe, is formed inside the chamber solid walls to form a "pocket" surrounding the microexplosion. This scheme has the added advantage of also protecting the first wall from neutron damage. Examples of key issues associated with this scheme are:

1) the ability to form a stable and uniform thick liquid layer so as to fully cover the interior surfaces of the first wall,;

2) the feasibility of forming the liquid layer so as to allow holes for the driver beams without exposing the first wall to unacceptable levels of X-rays and debris;

3) the ability to re-establish the wall protection layer after the microexplosion; and

4) the need for this liquid to contain lithium to provide adequate breeding and the ability to clear the chamber from a multi-species liquid (e.g. the molten salt flibe).

Another scheme for wall protection relies on a thin liquid metal film wetting the first wall. This concept allows greater control over liquid feeding and uniformity of the liquid layer. It can use a single-element liquid; for example, lead, which is a neutron multiplier that can also enhance tritium breeding. Examples of issues with this scheme are: a) blast effects, b) flow around geometric perturbations, c) neutron damage and activiation, and d) protection of inverted surfaces. Only a very small effort has been devoted to this critical issue of wall protection. Experiments and modelling are needed to evaluate the scientific and technological issues - fluid mechanics, thermomechanics, and materials response - of the various wall protection schemes

The second IFE issue is cavity clearing at IFE pulse repetition rates. Following each pellet explosion, the cavity (chamber) fills with target debris and material evaporated or otherwise ejected from the cavity surfaces. This material must be removed from the cavity before the next target is injected. This generally requires recondensing condensable gases onto the surfaces of the first wall (or more specifically the surfaces of the wall protection layer) and by pumping non-condensable gases out through large ducts. Power reactors require a repetition rate of ~3-10 pulses per second. Evacuation requirements depend on propagation limits for both targets and driver energy. Base pressure requirements: determine 1) the time to evacuate the chamber; and 2) the level of protection to the first wall (and final optics) afforded by the cavity background gas. Research is needed to better understand clearing requirements, the recondensation process, and to develop design solutions. Some small scale experiments are being planned at universities.

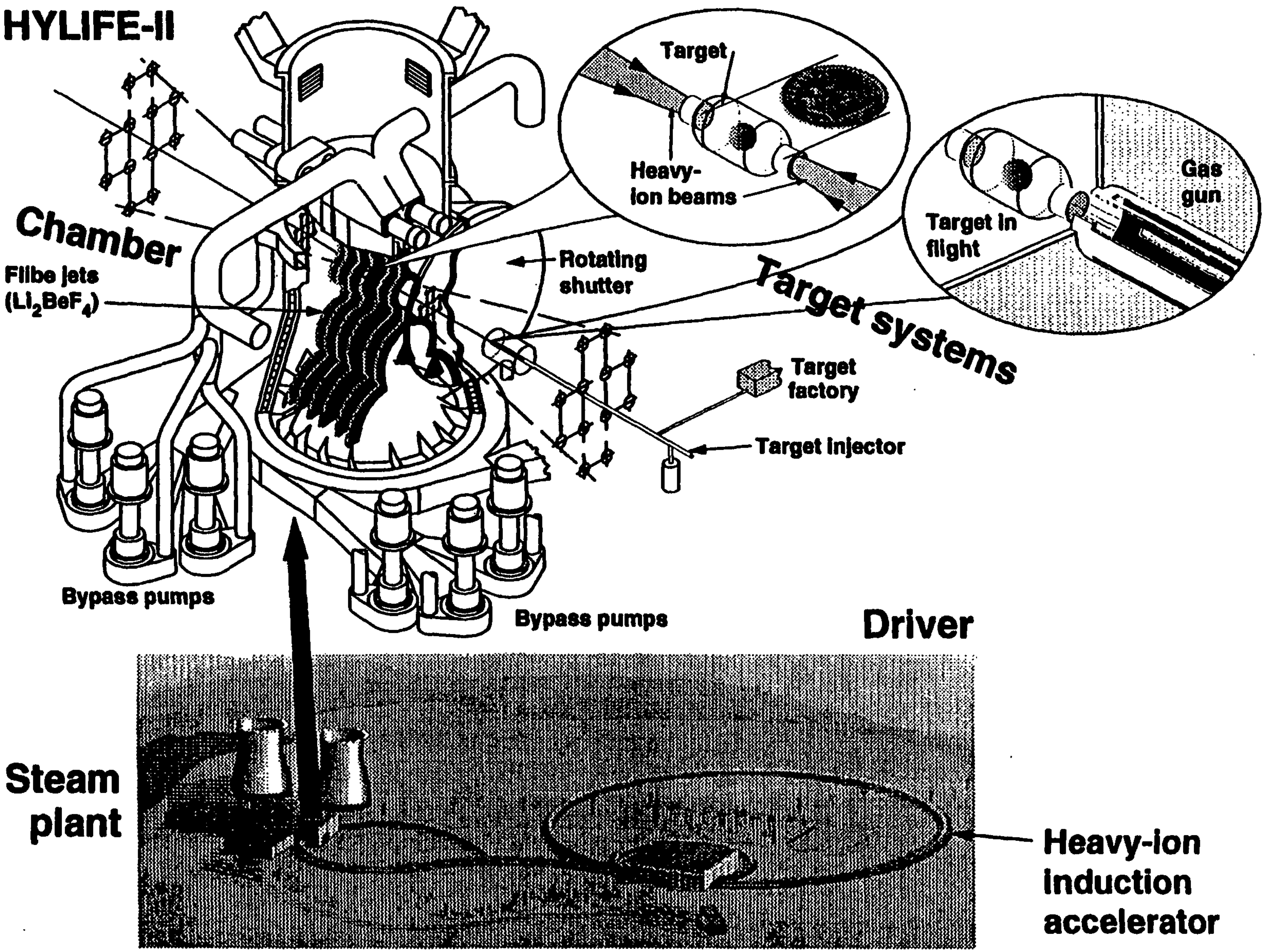
The remaining fusion chamber and target fabrication issues in Table 1 are related strictly to power plant technology feasibility, safety, and economics. They include: demonstration of tritium self-sufficiency in a practical IFE system; demonstration of adequate radiation shielding of all components; thermo-mechanical response and radiation damage of the first-wall/blanket; and demonstration of low cost, high volume target production techniques. The required R&D and the resolution of these last four issues will be greatly influenced by the results of research to resolve the previous issues.

Table 1

Top-Level Issues For
Inertial Fusion Energy

1	Sufficiently High Target Gain at Economical Driver Size: a) G > 30 for indirect drive with ion beams. b) G ~ 100 for direct drive with lasers.
2	Driver cost, efficiency, reliability, and lifetime: a) Demonstration of the required performance of a driver operated in a repetitive mode. b) Performance, reliability and lifetime of final optics.
3.	Fusion Chamber: a) Feasibility and performance of a viable wall-protection scheme. b) Cavity clearing at IFE pulse repetition rates. c) Tritium self-sufficiency in a practical IFE system. d) Adequate radiation shielding of all componenets. e) Pulsed radiation damage and thermomechanical reponse of first wall/blanket, particularly for concepts without thick liquid protection.
4.	Sufficiently low cost, high volume, target production system.

A number of integrated IFE power plant designs exist



Liquid-jet protected fusion chambers for long lifetime, low cost, and low environmental impact

G. **Synergy of IFE/ICF and MFE.**

- There is an important synergy in plasma theory and computer modeling as seen in the numerous books on plasma physics; e.g., in such areas as Particle-in-Cell simulations and intense radiation-plasma interactions

- Non-linear plasma instabilities, shock waves and implosion codes, non-neutral plasmas, plasma-wall interactions, and intense ion-beam physics are important common interests

- There is much in common in atomic physics and diagnostic needs, notably in the radiation detection area—mirrors, photo-detectors and lasers.

- Common technology interests include neutron damage resistant materials development and tritium breeding blanket technologies.

The IFE Program is synergistic with other DOE programs

IFE TASK	MFE	Other ER	ICF	Other DP	Other DOE
Theory, Simulation, Small Experiment	PS	HENP	PS, TD	RAD	HAP
High Current Injectors and Transport	NBH, MT	HENP, SNS	HED	RAD	HAP
Ion Sources	NBH, DIAG	HENP, SNS	HED	RAD ?	HAP
Components			HED		
Electrostatic Quadrupoles	NBH	HENP, SNS			
Magnetic Lenses	MT	HENP, SNS		RAD	HAP
Pulsers	MT	HENP, SNS		RAD	HAP
Magnetic Materials	MT	HENP, SNS		RAD	HAP
Insulators	NBH, MT	HENP, SNS		RAD	HAP
Integrated Facility		HENP, SNS	HED	RAD	HAP
Bending and Recirculation	PS	HENP	PS		
Focusing			HED, LIF		
Chambers and Power Plants	MR		MR		HAP

BNCT = Boron Neutron Capture Therapy, DIAG = Diagnostics, HAP = High Average Power (e.g.waste management), HED = High Energy Density Physics (creates LMF option?), HENP = High Energy and Nuclear Physics (e.g. relativistic klystrom), LIF = Light Ion Fusion, MR = Materials Research, MT = Materials Testing, NBH = Neutral Beam Heating, PS = Plasma Simulation, RAD = Radiography, SNS = Spallation Neutron Source, TD = Target Design.

APPENDIX A. Charge to Panel, Meeting Agendas, and Contributors.

1. Charge to Panel.

Department of Energy
Washington, DC 20585

APR 08 1996

Dr. Robert W. Conn, Chair
Fusion Energy Advisory Committee
School of Engineering
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0403

Dear Dr. Conn:

This letter forwards the charge that follows up on a specific recommendation made by your Committee in its report, "A Restructured Fusion Energy Sciences Program." The report calls for a programmatic review to assist the Department in setting technical priorities for the Inertial Fusion Energy (IFE) Program.

Inertial fusion has been reviewed often in the past decade, including the Fusion Policy Advisory Committee in 1990, the Fusion Energy Advisory Committee (FEAC) in 1993, as well as two reviews by the National Academy of Sciences during the 1980s. Questions of scientific merit and appropriate energy relevance have been addressed positively by the previous reviews. For the near term, however, we would like you to provide us with an assessment of the content of an inertial fusion energy program that advances the scientific elements of the program and is consistent with the Fusion Energy Sciences Program, and budget projections over the next several years.

Please consider augmenting the expertise of FEAC with appropriate individuals from inertial fusion programs that are active in this country, as well as foreign participants that would be helpful.

I would like to have your recommendations regarding this program by July 1996.

The Department is appreciative of the time and energy provided by the members of FEAC in this continuing effort to improve and orient the fusion energy sciences program to the needs of the times. I will look forward to hearing the Committee's recommendations on this matter.

Sincerely,

Martha Krebs

Martha A. Krebs
Director
Office of Energy Research

Enclosure

Charge to the Fusion Energy Advisory Committee for an Inertial Fusion Energy Review

Since 1990, the fusion program has had a mandate to pursue two independent approaches to fusion energy development, magnetic and inertial confinement fusion. In magnetic fusion, our strategy is to continue to use international collaboration, especially participation in the International Thermonuclear Experimental Reactor, to pursue fusion energy science and technology. In inertial fusion, our strategy has been to assume the target physics is the highest priority activity and would be developed as a part of the weapons research program; and, indeed, the next step in the development of target physics, namely the National Ignition Facility, is proceeding into construction in Defense Programs.

Based on the Fusion Policy Advisory Committee report of 1990, we had taken as our highest priority in inertial fusion energy the development of heavy ion accelerators as the most desirable driver for energy applications. That development program has met all of its milestones and has received numerous positive reviews, including one by the Fusion Energy Advisory Committee (FEAC), which in 1993 recommended a balanced Inertial Fusion Energy program of heavy ion accelerator development, plus other smaller scale efforts, at $17 million per year.

The potential for inertial fusion energy has been judged to be real, but the fusion program no longer has as a goal the operation of a demonstration power plant by 2025. Given that the basic mission of the fusion program has changed from energy development to fusion energy science, and that funding for the entire fusion program will be constrained for some number of years, I would like FEAC to again consider inertial fusion energy and recommend what the new Fusion Energy Sciences program should be doing in support of this future fusion application, and at what level?

2. Meeting Agendas.

a.) FESAC/IFE Review LBNL, June 3-5, 1996.

Monday, June 3, 1996

	Welcoming remarks	Director, Charles Shank
8.30 am	Executive Session	
10.15 am	*Break*	
10.30 am	Public Session (all day)	N.Anne Davies (DOE) David Crandall (DOE)
11.15 am	History of IFE: FEAC 7 Panel Report	 Bill Hermansfeldt (SLAC)
12.00 pm	Program Overview	Roger Bangerter (LBNL)
12.30 pm	*Lunch*	
1.30 pm	Overview (cont)	
2.15 pm	IFE Target Physics	John Lindl (LLNL)
3.30 pm	Tour of Experiments	LBNL Staff
4.30 pm	Beam Physics Experiments	Simon Yu (LBNL)
6.00 pm	*Adjourn*	

Tuesday, June 4, 1996

8.30 am	Beam Theory	Alex Friedman (LLNL)
9.45 am	IFE Power Plants	Ralph Moir (LLNL)
10.30 am	*Break*	
11.00 am	The European Program	Ingo Hofmann (GSI)
11.45 am	Synergism of IFE, MFE and other ER programs	 Grant Logan (LLNL)
12.15 pm	*Lunch and Executive Session*	
1.45 pm	Summary (part 1)	Roger Bangerter (LBNL)
2.15 pm	Invited Comments	John Sethian (NRL) Bill Barletta (LBNL) Stephen Dean (FPA)
3.30 pm	*Break*	
4.00 pm	Invited Comments (cont)	Craig Olson (SNL) Mike Campbell (LLNL) Ken Schultz (GA)
5.00 pm	Public Comments	None
5.05 pm	Summary (part 2)	Roger Bangerter (LBNL)
6.00 pm	*Adjourn*	

Wednesday, June 5, 1996

8.00 am	Executive Session	
12.30 pm	*Adjourn*	

b.) FESAC/IFE Review LLNL, June 24-26, 1996.

Monday, June 24, 1996

8.30 am	Executive Session	
12.30 pm	*Lunch*	
1.30 pm	Tour of LLNL facilities	LLNL Staff
3.00 pm	Executive Session	
4.00 pm	Discussion of proposed heavy ion accelerator budgets	Roger Bangerter (LLBL) Alex Friedman (LLNL)
4.45 pm	Executive Session	
6.00 pm	*Adjourn*	

Tuesday, June 25, 1996

8.30 am	Executive Session	
12.30 pm	*Lunch*	
1.15 pm	Fast Ignitor	John Lindl (LLNL)
2.15 pm	Executive Session	
5.45 pm	*Adjourn*	

Wednesday, June 26, 1996

8.00 am.	Executive Session	
12.30 pm	*Adjourn*	

3. **Written Contributions.**

a.) Recommendations for Inertial Fusion Energy from the Naval Research laboratory, Stephen Bodner and John Sethian.

b.) Comments on the IFE program from the University of Wisconsin, Robert R. Peterson and Gerald L. Kulcinski.

c.) Comments from the University of Maryland, Martin Reiser and Terry Goodlove.

Appendix B - Status of Target Physics for IFE

1. Summary of 1993 FEAC Panel 7 target physics findings

Although there has been major progress in ICF target physics since the 1993 FEAC panel 7 report[1], the two principal findings of that report remain true:

The primary approach to heavy ion fusion (HIF) and the glass-laser-based NIF is the indirect-drive approach. For indirect drive, the capsule implosion and burn physics are the same for both HIF and laser-driven hohlraums. For ion-driven hohlraums heated to the same radiation temperature (T_R), the HIF requirements for hydrodynamic instability, implosion uniformity, and pulse shaping can be investigated directly with laser-driven targets. In addition, at the same radiation temperature, x-ray hohlraum wall losses, radiation-driven hohlraum wall motion, and radiation transport for laser-driven hohlraums are directly applicable to HIF. These are the primary issues which affect coupling efficiency and hohlraum symmetry for the baseline HI hohlraums. Because of these similarities, the DP target physics program on the Nova laser at LLNL provides a solid base for calculating most critical elements of HI targets.

Success of the ignition objectives on the NIF will substantially reduce the risk for heavy ion inertial fusion energy (IFE), and these results will play a major role in any decision to develop a full scale HI driver. We believe that the success of the Nova laser target physics program, coupled with the Halite/Centurion[2] underground test results, provide a sufficient target physics base for proceeding with the development of the technology and physics base for HI drivers.

2. Progress on Indirect Drive ICF since 1993

2.1 Declassification

Of major importance to the general availability of the target physics basis for ICF was the Dec 1993 decison by DOE to largely declassify ICF. Since that time, a large number of articles as well as a comprehensive review of Indirect Drive ICF[3] have appeared in the scientific literature.

2.2 ICFAC review of Indirect Drive Ignition Laser Targets for the NIF

The 1990 National Academy of Science review ICF[2] established the Nova Technical Contract (NTC) as a set of target physics goals which would form the basis for a decision to proceed with the NIF. These goals are also largely applicable to HIF, as summarized by the 1993 FEAC Panel 7 report. Since the 1993 FEAC Panel 7 met, the Defense Program advisory committee for ICF, the ICFAC, has extensively reviewed (8 full ICFAC and 4 subcommittee meetings) the ICF Target Physics Program. An extended review of progress on the NTC, which the ICFAC has concluded is essentially complete, is available[4]. In its letter report following it final meeting in November 1995, the ICFAC concluded[5]:

"The overall impression of the committee on the target physics is that there has been remarkable progress in the last six months. During the three years of ICFAC reviews of ICF, the ICF target physics program for ignition has identified and resolved many potential target physics issues. The peer review and collaboration between the two nuclear weapon design laboratories has been largely responsible for the rate of progress in addressing Nova Technical Contract goals. Without major roles for both laboratories in target physics, the credibility of reaching ignition will be significantly reduced. There is a much larger base of attractive designs than at the time of KD1 (decision to proceed with preliminary engineering design of the NIF) and the case for achieving ignition on NIF has been significantly strengthened since that decision. The program has developed a broader set of tools. In all of the critical areas - cryogenic layer production , hohlraum laser plasmas, and implosions - committee members believe that the probability of ignition has increased above 50%, and some believe that it is well above this level. As one committee member put it, the situation has changed from risk reduction to confidence increasing. Although new problems may appear, the committee has seen a high level of ingenuity in the personnel in the program and has confidence that solutions will be found."

2.3 Integrated Calculations of NIF Ignition Targets

One of the significant advances of the past 3 years, has been the development, by both Livermore and Los Alamos, of integrated calculations of NIF ignition targets that employ full radiation transport[3,6,7]. These calculations model the laser propagation and absorption, the full hohlraum and the implosion as a single integrated entity. Fig 1 shows the NIF point design which has had the most intensive analysis. Fig. 2 shows the numerical grid at the beginning and at the peak of the laser pulse. The snapshot at peak power also shows the laser rays. Figure 2 does not show the detailed zoning of the fuel capsule but it was included in the calculation. These calculations use the 2D LASNEX computer code which is the workhorse of the ICF indirect drive modeling program. Figure 3 shows the gain obtained from the integrated calculations for different size targets that can be tested on the NIF. These gains are consistent with analytical scaling curves that are indicated for two different hohlraum coupling efficiencies. As indicated in Figure 1, the NIF point designs have about an 11% coupling efficiency. Although such integrated calculations have been a prominent feature of the ICF program since its beginning, the Nova experiments on symmetry demonstrated that it was necessary to utilize full radiation transport, rather than diffusion, in order to accurately calculate implosion symmetry. Although aglorithms for solving the transport equation have been available for many years, significant improvements were required to achieve the required accuracy with reasonable amounts of computer time. With these improvements, it has been possible to do ignition calculations which routinely utilize full radiation transport. These calculations, and the Nova experiments, which have been used to verify the accuracy of the computational techniques, have resulted in a significant improvement in the confidence of the accuracy of the NIF ignition designs noted by the ICFAC report. These techniques have been applied to HIF high gain targets as described below.

2.4 Development of 3D codes

Another major advance in the past 3 years has been the development and utilization of 3D codes for hydrodynamic instability and implosion calculations. The nonlinear evolution of the Rayleigh-Taylor instability is inherently 3D, and various features of the radiation asymmetry onto the capsule, particularly on Nova, must sometimes be modeled in 3D. Figure 4 shows the results of planar Rayleigh-Taylor Instability experiments and 3D calculations which correctly model the dependence of the late phase nonlinear evolution of perturbations with different shapes[8]. Fig 5 shows the calculated and measured yield degradation for implosions which used capsules with deliberately perturbed surfaces. The observed yields require 3D calculations to accurately model the results. These results apply directly to indirect drive implosions driven by ion beams.

3. Target Gain Requirement for IFE

For an inertial fusion energy (IFE) application, the target/driver combination must meet a minimum product of driver efficiency times gain (hG) where h is the efficiency of converting electrical energy into the driver beam energy and G is the target gain, the ratio of thermonuclear yield to driver energy delivered to the target. For IFE power production, we have:

$$P_{net} = P_{gross} - P_{aux} - P_{driver} = P_{gross}[1 - f_a - 1/(\eta G M \varepsilon)]$$

where P_{net} is the net power, P_{gross} is the gross power, P_{aux} is tht power required to run auxilliary systems, and P_{driver} is the power required to run the driver. Also M is the fusion blanket multiplier which is slightly greater than unity because tritium production in lithium is exothermal, and e is the efficiency of converting thermal energy into electricity. The product Me~0.4 in typical power production studies. Since P_{aux} is usually only a few percent of the gross power produced, the fraction of the gross power used to power the driver is approximately 1/hGMe. If this is more than 25-35% of the total power, the cost of electricity increases rapidly. Hence we require hG>7-10. Since ion beam drivers can have an efficiency of 35% or potentially more, we only require a target gain G>20-30. Since currently proposed laser systems, such as KrF or diode-pumped solid state lasers (DPSSLs), have efficiencies of 10% or less they require a target gain G>70-100. At this stage of planning, a saftey margin in the potential target gain of a factor of two or more is important for making a case that can be strongly defended. Although Indirect Drive laser driven targets can potentially reach the lower end of the required gain at a laser energy of about 10 MJ as indicated in Fig. 3, there is no margin for error and the laser size is very large. Direct Drive targets, which will also be tested on the NIF, have potentially higher gain which makes this type of target more attractive for power production with lasers. Potential issues for Directd Drive targets are discussed at the end of this appendix.

4. Ion Beam Target Designs

A wide range of ion beam targets, such as those shown in Fig 6, can achieve the required gain and can be matched to accelerator and fusion chamber requirements. The two sided targets in Fig 6 have received the most attention in the HI program because they are well matched to attractive fusion chamber approaches which utilize a first wall protected from neutrons. Such fusion chambers utilize a thick blanket of neutron absorbing material, which also breeds the required tritium, inside of the chamber first wall[9].

4.1 Localized Radiator Designs - Two-Sided

Fig. 7 shows the analytically estimated gains[10] (as a function of ion beam focal spot radius for two typical heavy ion ranges) for targets with localized radiators such as those in Fig 6a. These calculations are based on capsule designs being developed for the NIF, and data on radiation transport and hohlraum energetics obtained from Nova experiments. Also shown in Fig. 7 are the capsule energies and required hohlraum temperatures. Capsules with the smallest energy indicated, 0.2 MJ, can be directly tested on the NIF. Symmetry is obtained in these localized radiator designs by using symmetry shields to remove long wavelength variations in the radiation flux. Similar approaches to controlling symmetry have been successfully tested on Nova[3].

As shown in Fig. 7, the gains are critically dependent on the spot size of the ion beam when it is focused on the target radiators. The HI driver energy required to drive a fuel capsule of a given size depends inversely on the efficiency with which the ion beam energy is converted to x rays. This in turn depends on the focal spot size and range of the ions which determines the mass of material heated by the ions. As the ion range is reduced, less mass is heated for a given spot size. This results in a higher gain for a given spot size or a larger tolerable spot size for a given gain. For idealized radiator designs, 50-80% of the driver energy can be converted to X-rays[1]. Recent more detailed calculations which include full radiation transport and radiator wall motion obtain conversion efficiencies of about 50%. These calculations indicate the radiators with very small spot sizes are likely to suffer from closure due to wall motion. More work is required to fully optimize radiatior designs for these localized radiator designs.

The targets in Fig. 7a are readily adaptable, in principle, to single sided irradiation. If the radiators are constructed with a 90 degree bend prior to entering the hohlraum, the ion beams can come in from a single side while maintaining basically two-sided axisymmetric irradiation of the capsule.

4.2 Distributed Radiator Designs - Two-Sided

The distributed radiator design shown in Fig 6b, is suitable for relatively short range ions. This design uses the same capsules as the localized radiator design and NIF, but symmetry is obtained by locating the radiating material where it is required for symmetry. This can be achieved by varying the density and radiator material. Fully integrated design calculations, similar to those that have been done for NIF targets, have been successfully carried out. Fig 8 shows the materials and densities used at the beginning of a particular series of calculations which achieves adequate symmetry and gain of 40 with about 7 MJ of 3.5 GeV Pb ion beam energy[12]. This design uses low density high-z materials for the hohlraum wall in order to maintain near pressure equilibrium between the walls and the foam radiator material. Fig 8 also shows the density contours near peak compression. Such calculations have been made possible by the developments in modeling for the NIF but much less effort to date has been devoted to optimizing the HI targets. When optimized, targets like those in Fig 8 are expected to have gains of 50-70 when drive by 5-7 MJ of ions.

Integrated calculations are also being carried out on the localized radiator designs, but these designs have complicated hydrodynamics in the radiators and internal symmetry shields which has not been fully modeled.

4.3 Spherical Target Designs

A range of "symmetrically" irradiated targets such as the target shown in Fig 6c, is also feasible. The potential gain of these targets depends on the degree of "direct coupling" of the ion beam to the fusion capsule. Two designs which indicate the range of target sizes and gains are shown in Fig. 9.

The light ion program at Sandia National Laboratories (SNL) has examined the design shown in Fig 9a. In this design, the ions are absorbed entirely in the high-z shell and the low density foam outside the fuel capsule[13]. The high-z shell and foam produce x rays which then implode the fuel capsule. The capsule design is largely similar to the NIF target designs, with the exception that the Sandia design has an outer layer of BeO to help provide "internal pulse shaping". This layer can help relax the accelerator pulse-shaping requirements. Because there must be enough material to stop the ions over then entire surface of the target, there is a larger heat capacity of radiator material in these spherical targets than in two-sided designs. This results in lower x-ray production efficiency, and relatively low gain at a large driver energy.

The other extreme in symmetrically illuminated targets is the direct drive target ion target·. In the example indicated in Fig. 9b., the pressure which drives the implosion of the DT layer is generated in the CH_2 layer which is directly heated by the ion beams[14]. At early time, there is very little smoothing of nonuniformities which arise because of the overlap of a finite number of ion beams. At later times in the pulse, the CH_2 generates enough radiation that radiation smoothing is significant. If sufficient uniformity can be achieved[15], such targets can have very high gain for relatively small drivers. Because both the symmetry and hydrodynamic instability characteristics of this target depend sensitively on details of the ion beam and the illumination geometry, relevant experiments will require a significant scale ion beam machine with many beams.

A 3D radiation transport capability is probably required to accurately calculate the number of ion beams required for symmetry in both of the above designs. The Indirect Drive symmetric target will require fewer beams than the directly driven design. Using a 2D diffusion approximation, SNL has estimated that 12-20 beams will be adequate for their target design. Full transport calculations in 2D are now possible. Further development of the 3D codes mentioned above, planned for next few years, will allow 3D calculations of these ion targets.

4.4 Ion Beam Coupling Experiments

The issue of x-ray production using ion beams is currently being addressed by experiments on the PBFA II light ion accelerator at SNL. On PBFA, 1-2 TW/cm^2 lithium ion beams have been focused on conical gold targets filled with low-z low density foam. Althoug the temperature achieved in these experiments is less than 100 eV, the measured radiation temperature and x-ray spectrum, as well as the tamping of the gold wall expansion by the foam, are in agreement with calculations.
LASNEX calculations indicate that fusion relevant matter conditions can be achieved with a heavy ion accelerator delivering as little as 1 KJ of energy Experiments at GSI Darmstadt have produced a 400 mm diameter focal spot uisng an approximately 10 cm focal length z-pinch plasma focus. Using this focal diameter, LASNEX calculations, using 1 KJ of ions with a range of 0.03 g/cm^2 delivered in 2 ns, predict temperatures of 250 eV in a gold lined Be cylinder. A wide range of experiments could be carried out with such plasmas. The effect on beam focusing of photo-ionization of the incoming ion beam, caused by target radiation emission, could readily be addressed.

5. Direct Drive Laser Targets for the NIF

Although Indirect Drive is the baseline approach to ignition and gain on the NIF, sufficient progress has been made on Direct Drive with lasers over the past 3 years that the NIF the target area is also being configured for Direct Drive as shown in Fig. 10. By moving 24 of NIF's 48 beam clusters, it is possible to achieve the geometric irradiation uniformity of better then 1% required for Direct Drive. The proposed beam arrangement is shown in Fig. 10. The geometric placement of the laser beams, as well as beam power balance and pointing accuracy primarily affects the long wavelength perturbations on the fusion capsule. This geometry is relatively straightforward to specify. The principal target uncertainties for Direct Drive are the imprinting of short wavelength perturbations onto the outside surface of the fusion capsule, and the subsequent growth of these perturbations by Rayleigh-Taylor Instability. This imprinting occurs because all techniques currently used for beam smoothing require some time to become effective. During this startup phase, residual intensity variation across beams imprint surface modulations on the target. The physics of this imprinting is quite complex and is one of the principal research topics for Direct Drive. As the target is accelerated, these modulations are amplified by Rayleigh-Taylor growth. The growth of all perturbations from both target fabrication and laser imprinting grow more rapidly for Direct Drive targets than for Indirect Drive targets of a given compressibility. This difference is related to the much higher ablation rates of Indirect Drive[3]. To reduce the growth rate of instabilities in Direct Drive, the targets are deliberately preheated. However this approach also reduces the possible gain by reducing the compressibility as shown in Fig. 11 for calculations from the University of Rochester[16]. In this Fig, a is the ratio of the pressure in the shell to the Fermi degenerate pressure at the same density. The current baseline target for the NIF has a=3 with a gain of 30 at 1.5 MJ. If a scheme can be developed for reoptimizing the laser focal diameter near the peak of the laser pulse, the gain increases to about 50. Under the same set of assumptions, the gain is estimated to be 130-150 at 10 MJ. Depending on the feasible laser efficiency, this gain is adequate for energy production although the laser is quite large.

The recently completed 60 beam Omega Nd-glass laser at the University of Rochester will be used to establish the understanding required to accurately specify the smoothing requirements and instability growth for Direct Drive on the NIF. The Nike facility at the Naval Research Laboratory will address these issues in planar geometry for a KrF laser.

Direct Drive targets require a uniform distribution of beams over the entire surface of the target as indicated in Fig. 10. Unless some approach can be developed which relaxes this requirement, Direct Drive is incompatible with the protected wall fusion chamber designs discussed above. A major issue for laser driven fusion chambers is survivability of the final optics to x-rays, neutrons, and debris. This issue will be addressed to some extent on NIF, but for a much smaller number of shots than is required for IFE.

Although driver beam imprinting and subsequent hydrodynamic instability growth are common issues for both ion beam and laser beam direct drive targets, the specific mechanisms for imprinting are unique to each driver. Hence the information learned for Direct Drive with lasers will not significantly increase the understanding of Direct Drive ion beam targets.

6. Fast Ignitor approach to ICF

A still more speculative approach to ICF, which has potentially high leverage for high gain, is the fast ignitor approach[17]. In the standard approach to ICF, fusion fuel is imploded and subsequently compressed in such a way that a relatively low density hot spot is formed in the center of a dense shell which contains most of the fuel. The hot spot must be large enough to capture the alpha particles and initiate a self propagating burn wave into the main fuel. The performance of these targets is very sensitive to the mix of cold fuel from the surrounding dense shell into the hot spot or asymmetry in the implosion, both of which can quench the burn.

In the fast ignitor approach to ICF, the compression and ignition steps are separated. A conventional driver is used to compress the fuel, but no attempt is made to produce the central hot spot. This relaxes the sensitivity of the implosion to asymmetry and mix. The energy required to ignite the compressed fuel must then be delivered to the compressed core by a separate beam before the core has a chance to expand. While the compression beams can deliver their energy in nanoseconds, the ignitor beam must deliver its energy in about 10 ps into a spot of about 10 mm radius. Because targets which are uniformly compressed require lower density for good burn efficiency, such targets can have a gain which is a factor of several higher than that of standard ICF targets.

The achievable gain will depend on the efficiency with which the fast ignitor beam is capable of delivering its energy to the compressed core. The intensities involved in the fast ignitor pulse are 10^{19}-10^{20} W/cm^2. At these intensities, the laser plasma interaction is highly relativistic.[18] A laser beam capable of delivering greater than 600 joules in 500 fs has recently been completed on Nova. This laser will be used to test key physics issues associated with delivering the ignitor energy to a compressed ICF target.

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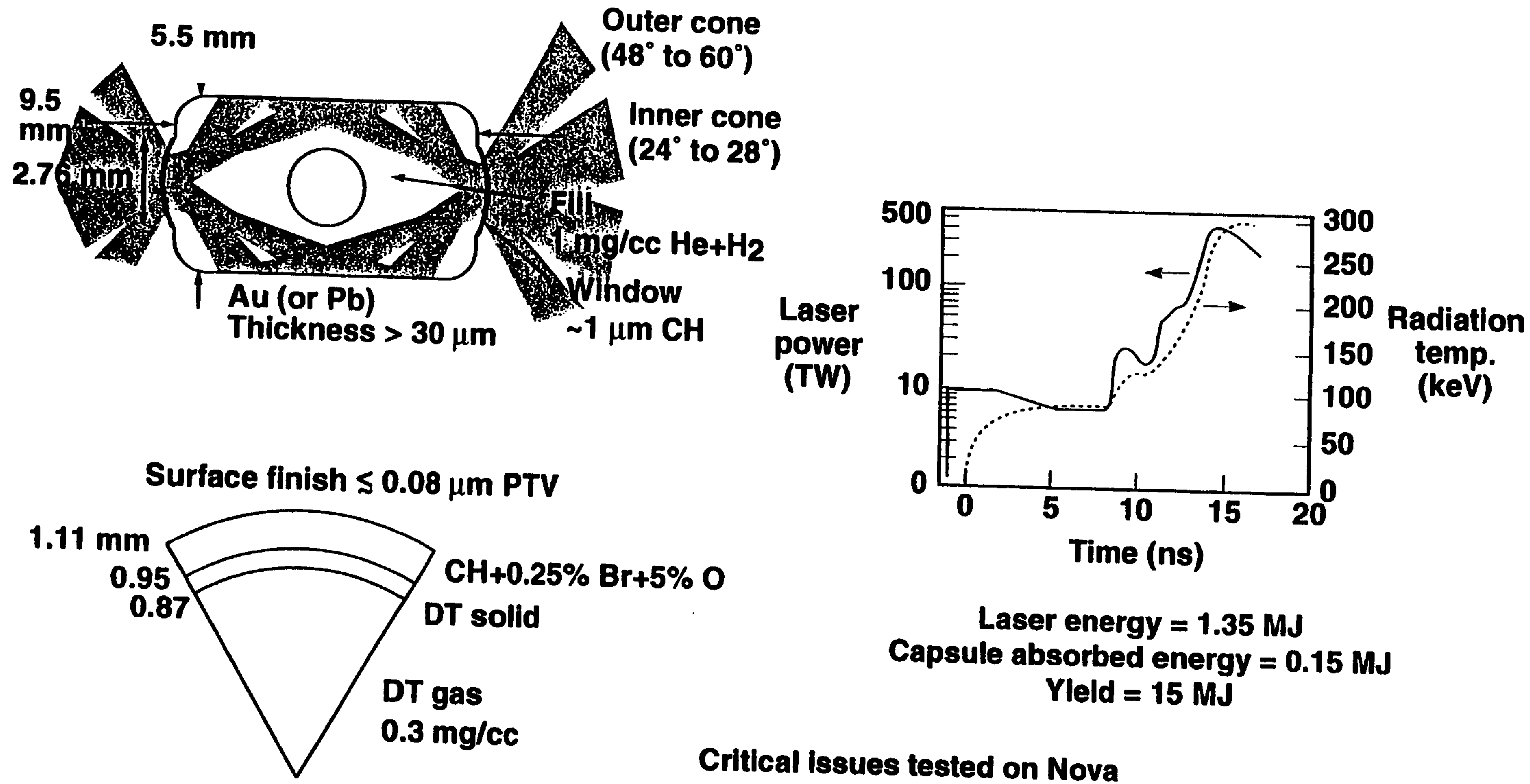
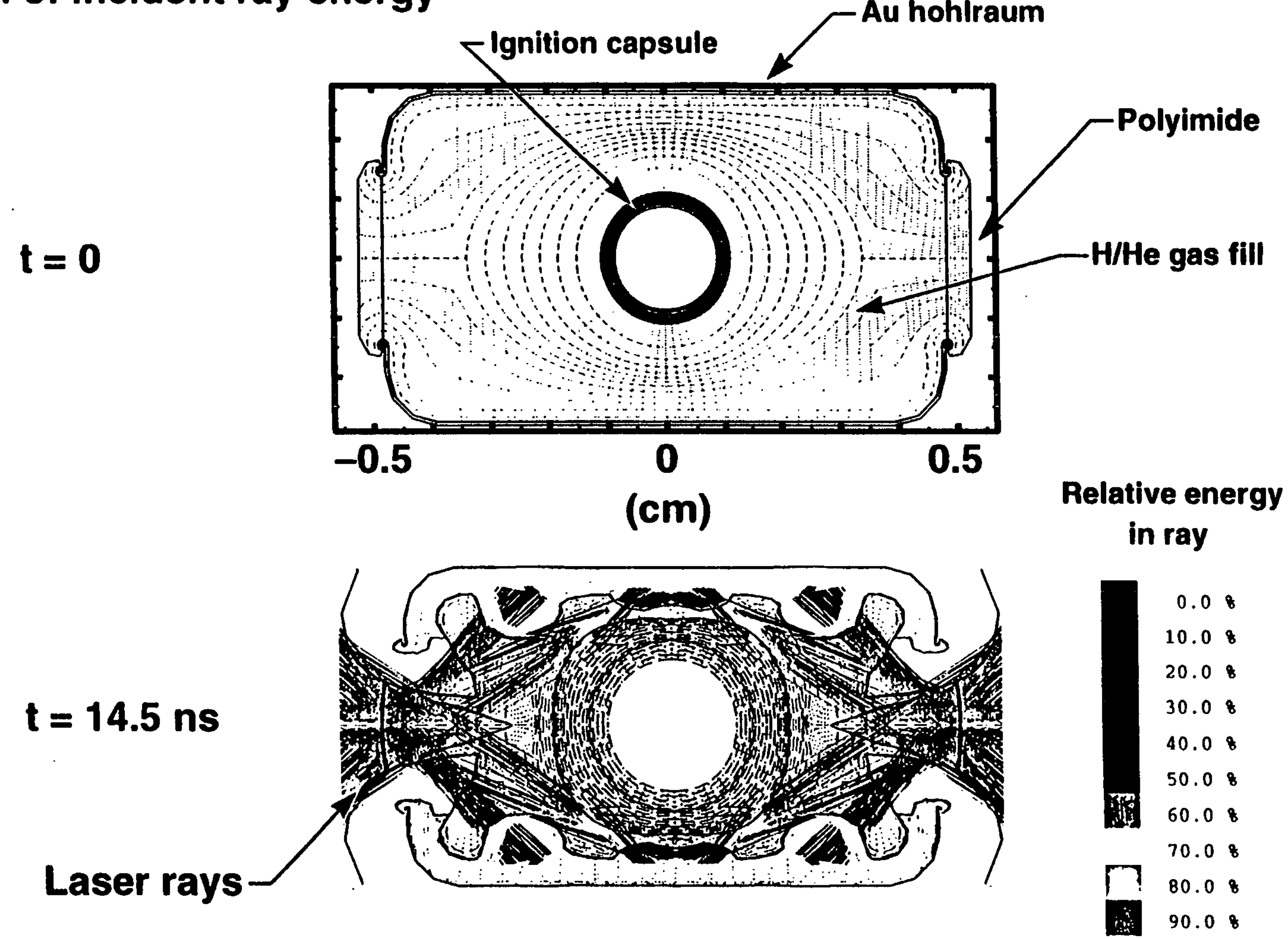


50-05-0494-1808Cpb01

Figure 1. NIF baseline targets achieve ignition in both LLNL and LANL detailed calculations which use physical models tested on Nova experiments.

30JDK/sae

Laser ray paths contoured along the path by remaining fraction of incident ray energy

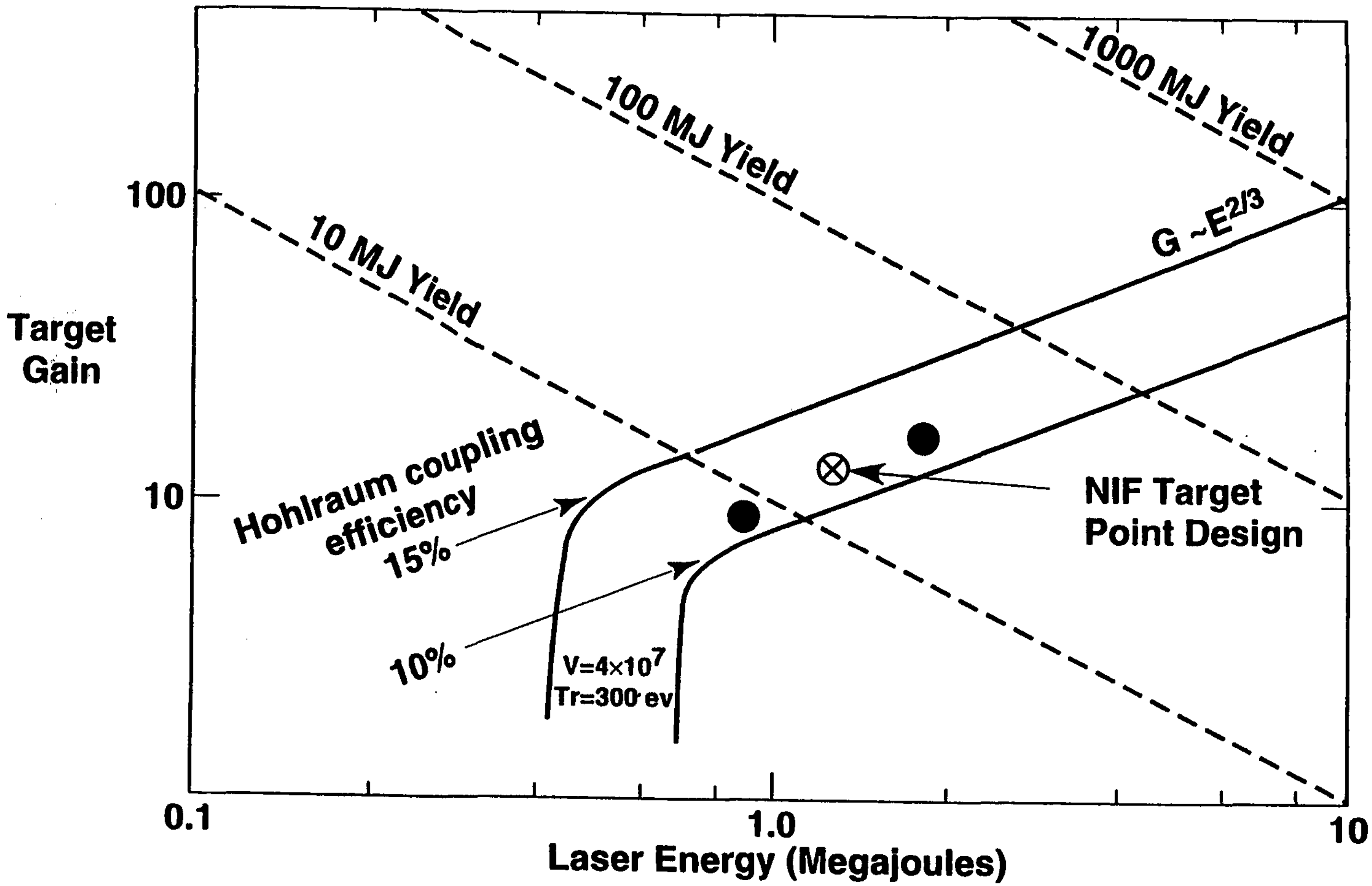


05-00-0496-0659pb02

Figure 2. We perform 2D calculations of NIF ignition targets to model accurately the coupling of the laser rays, the hohlraum, and the capsule.

Los Alamos

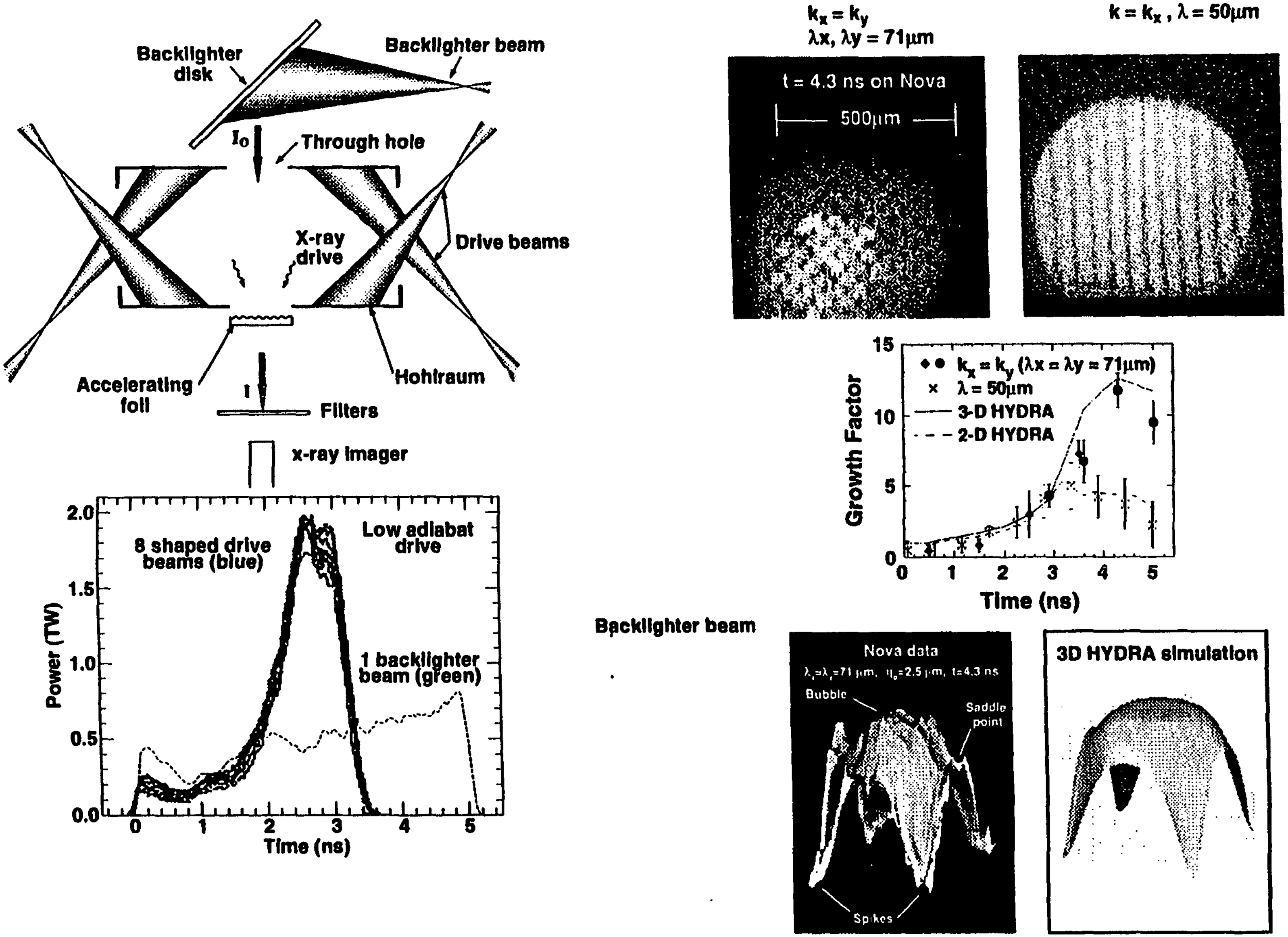
02JDL/pdd



08-00-1093-3819B

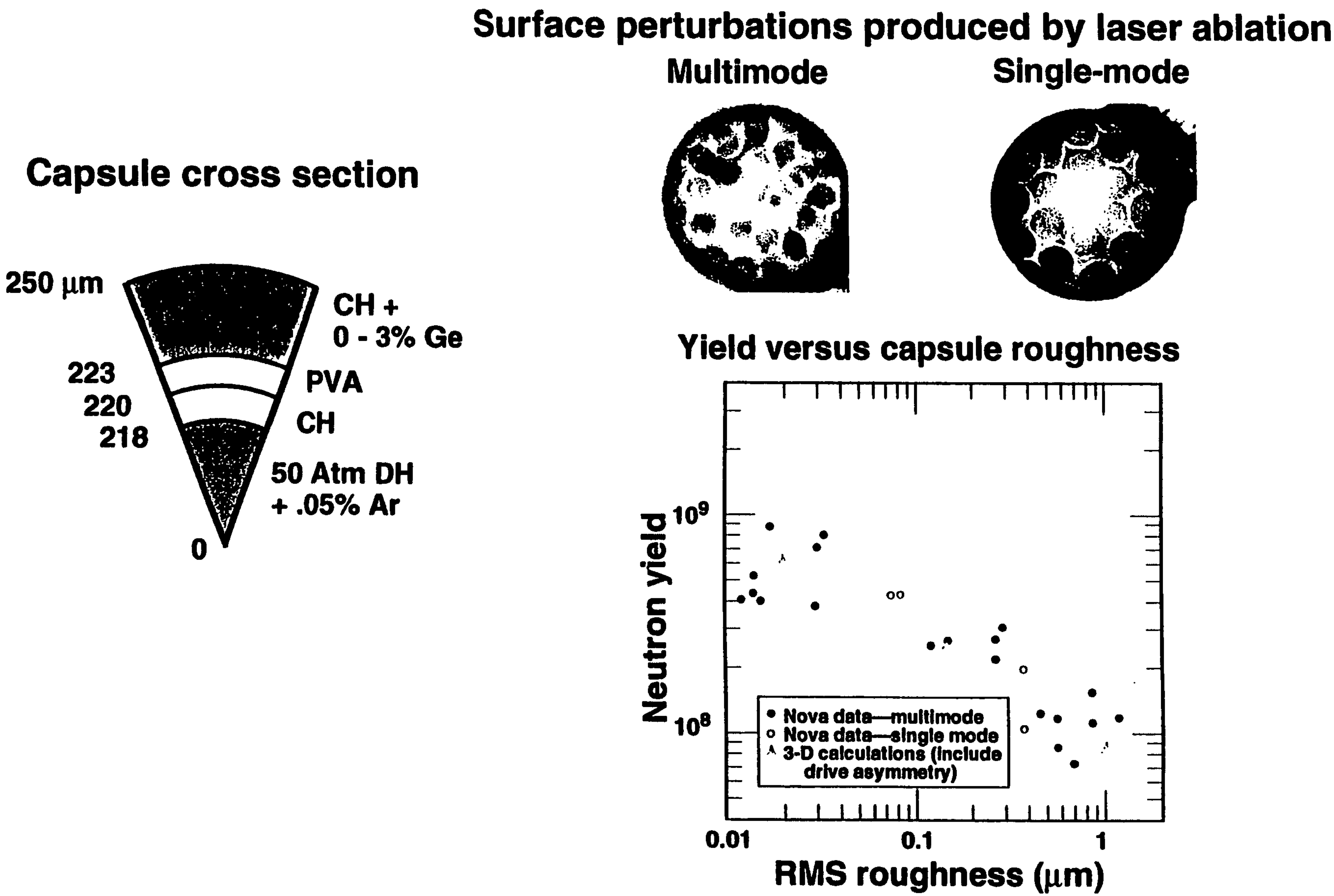
Figure 3. The National Ignition Facility (NIF) is being designed to demonstrate ICF capsule ignition and propagating burn.

13JDL/sae



05-00-0396-0634pb03

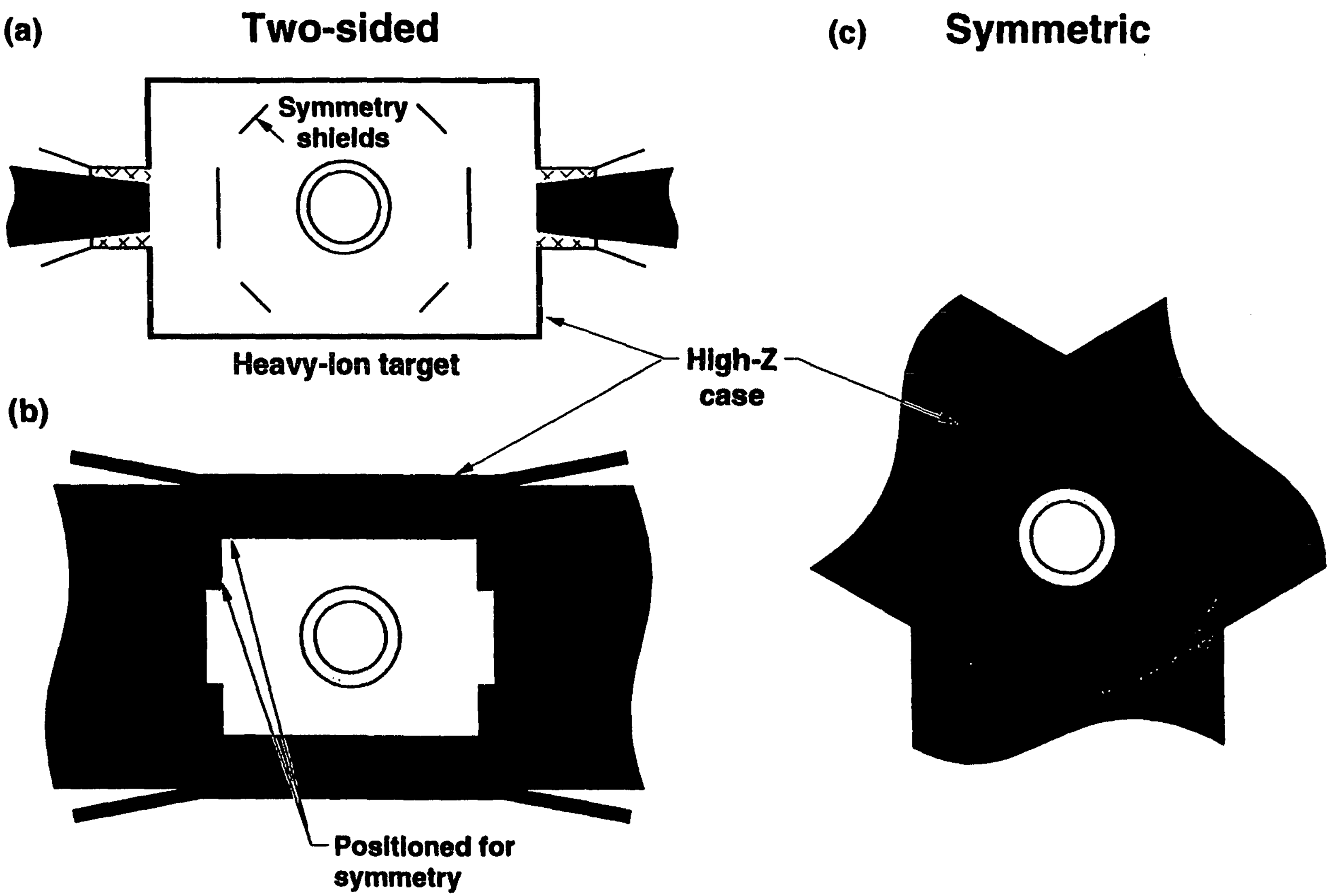
Figure 4. The measured growth of planar hydrodynamic instabilities in ICF is in quantitative agreement with numerical models.



08-00-0695-1445B

Figure 5. Capsules with deliberately perturbed surfaces have degraded fusion neutron yields as a result of the growth of these perturbations.

02JDL/gbh

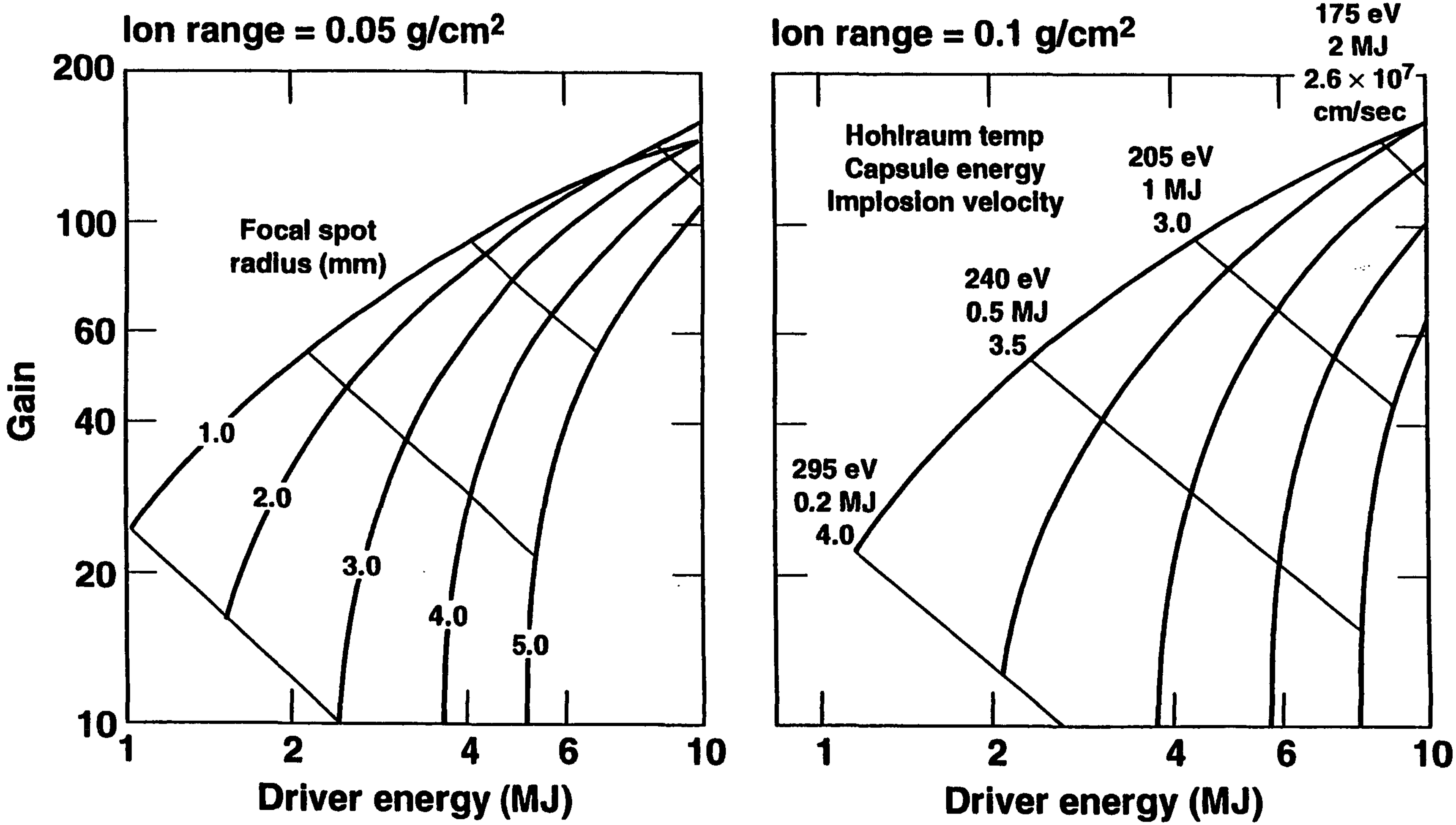


02-07-1192-3485pb01

Figure 6. A wide range of ion beam targets can be matched to accelerator and fusion chamber requirements.

12JDL/pdd

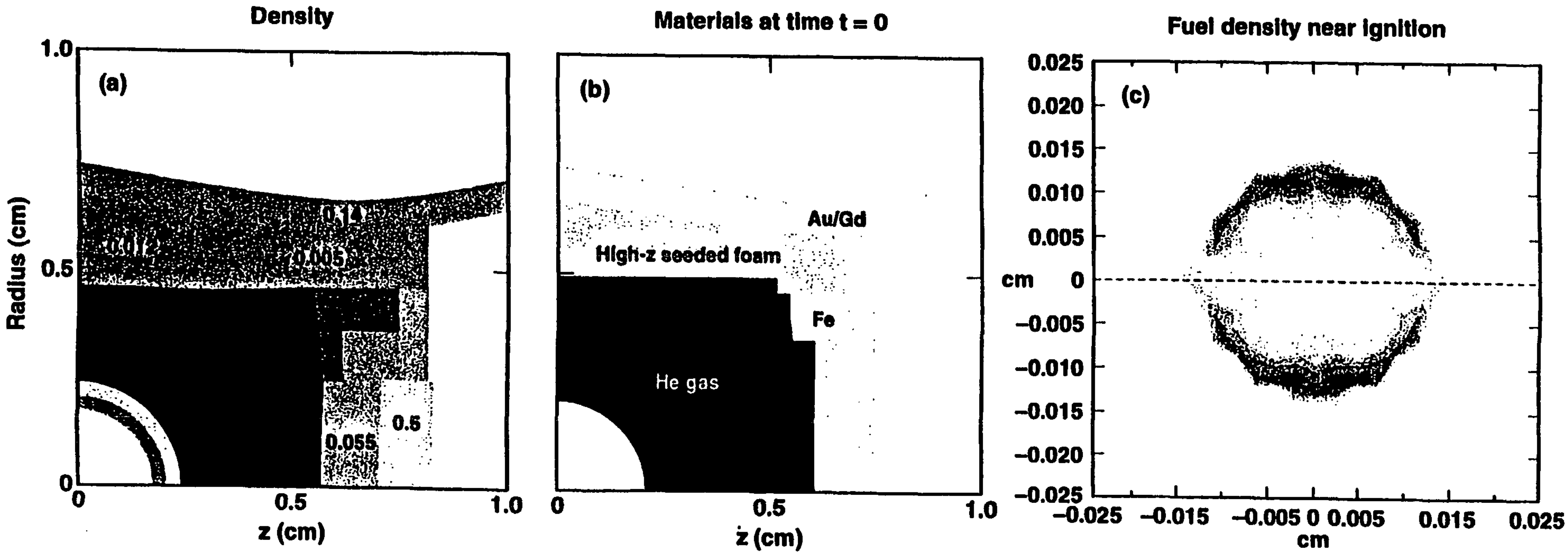
• Analytic estimate of potential heavy-ion target gain for two-sided targets



02-08-1192-3488pb01

Figure 7. Heavy-ion target gain depends on ion beam range, spot-size, and hohlraum coupling efficiency.

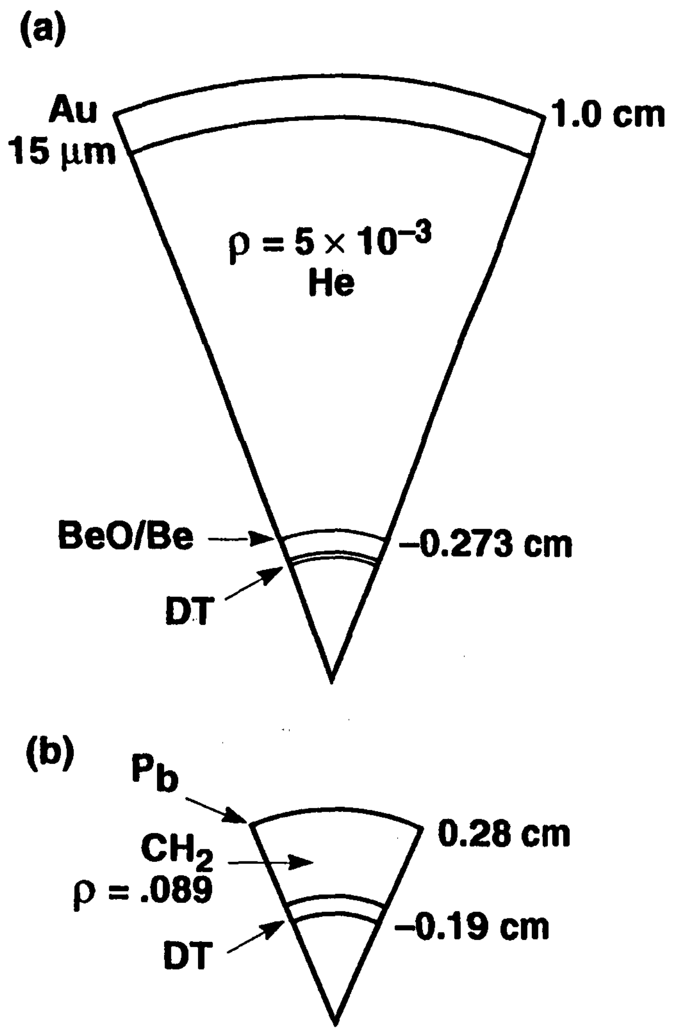
12JDL/mcm



05-00-0596-1259pb01

Figure 8. (a) and (b) In the distributed radiator design low density walls are in approximate pressure balance with low-Z fill. (c) Implosion symmetry in the distributed radiator design is adequate for ignition and gain.

28JDL/dsm



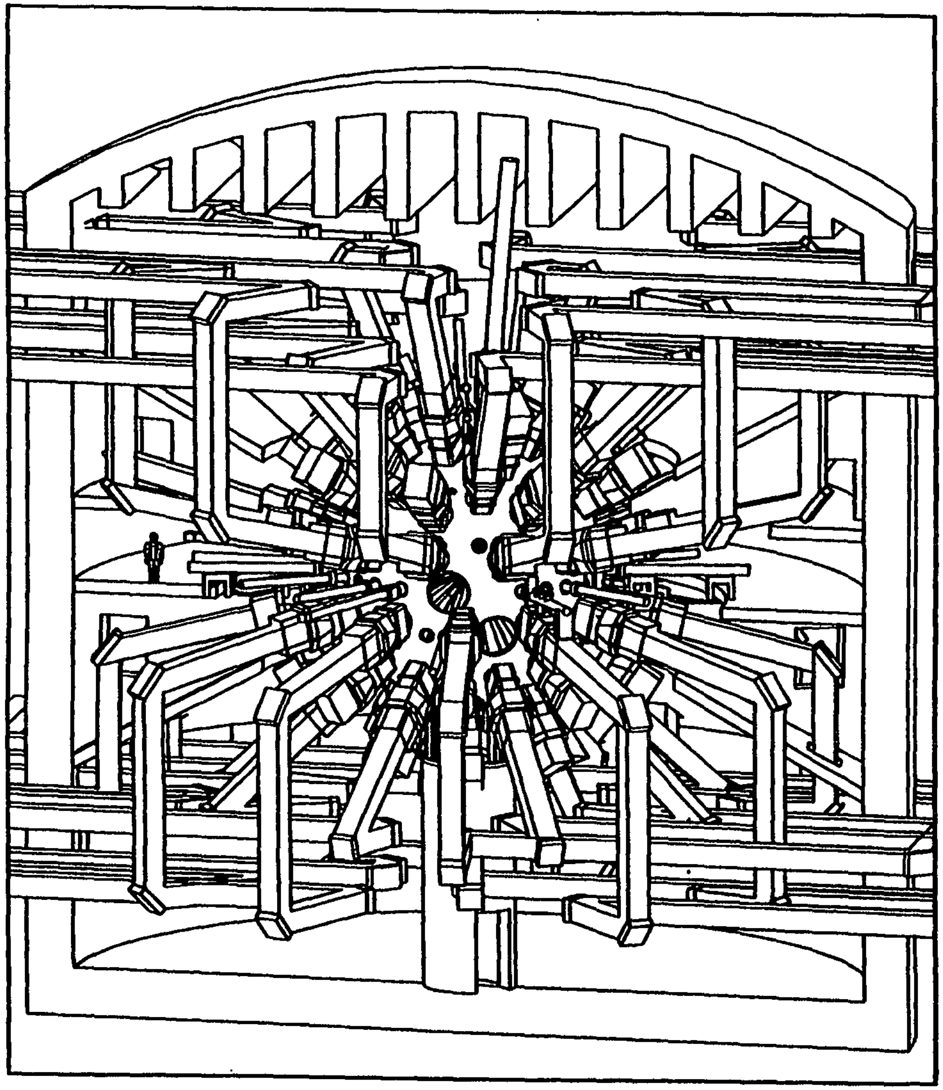
Sandia indirect-drive "light-ion" design
14 MJ absorbed
600-MJ yield
ion range ~0.03 g/cm^2
$G = 43$

Direct-drive ion design
1.67 MJ absorbed
220-MJ yield
ion range ~0.05 g/cm^2
$G = 130$

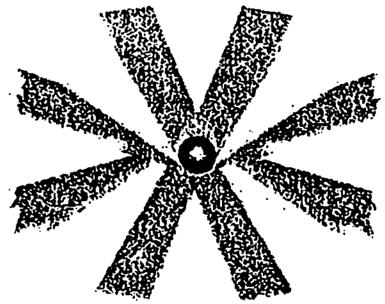
05-00-0596-1247pb01

Figure 9. The gain of "spherical" ion beam targets depends on the degree of "direct coupling" of the ion beam to the fusion capsule.

24JDL/gbh



Reposition 24 4-beam clusters



Laser target direct drive

40-00-0894-3298pb01

Figure 10. The NIF target area building and beam transport system can be reconfigured for direct drive.

29JAP/dsm

UR/LLE 351-nm gain curves

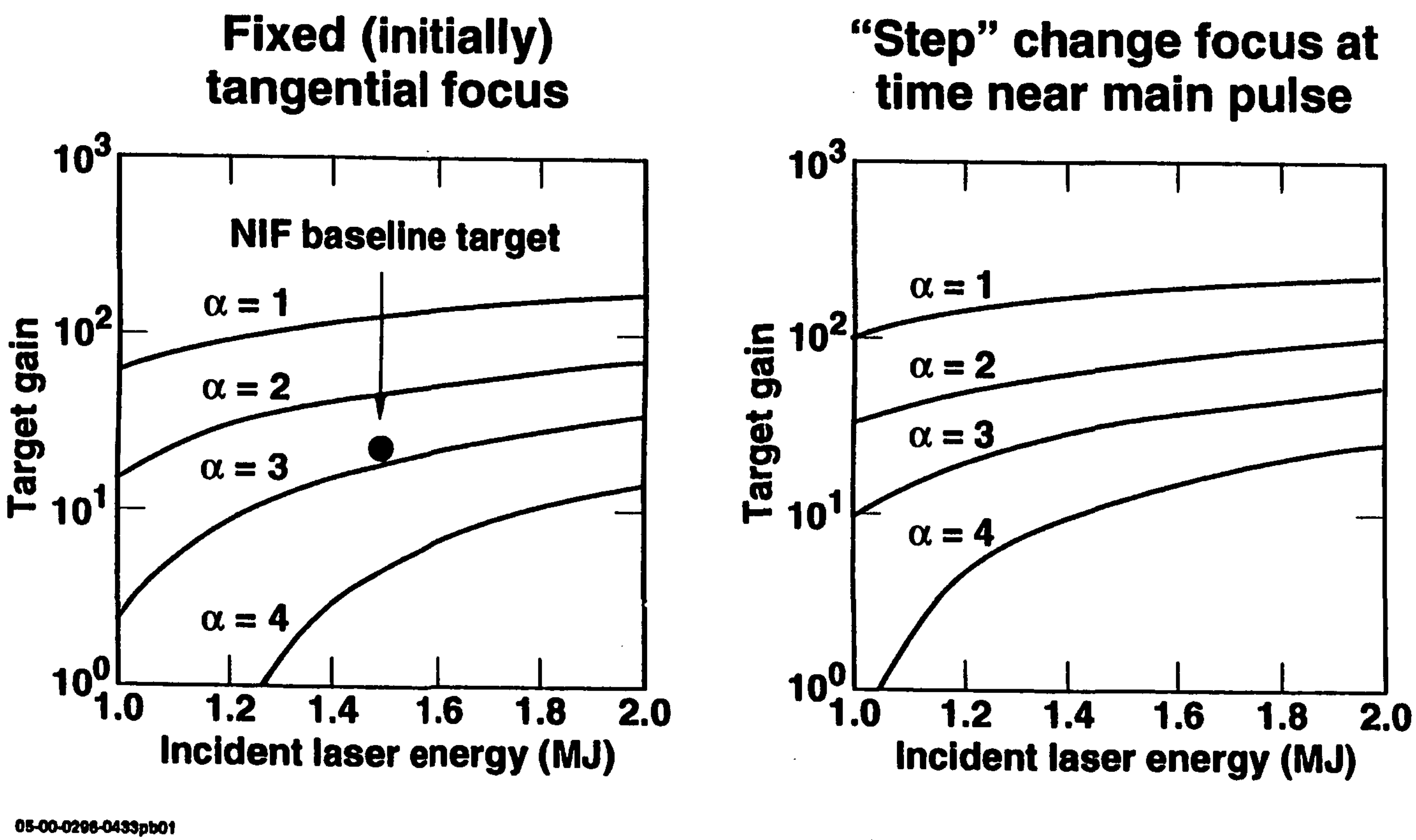


Figure 11. Hydrodynamic and laser-plasma instability constraints will determine the performance of high-gain direct-drive capsule design.

27JDL/sas

Appendix C

IFE Power Plant Issues and Needed Breadth of Research

About 50 conceptual design and system studies for IFE power reactors have been carried out over the past 25 years. Eleven of these were driven by heavy ion beams. The most recent studies, PROMETHEUS and OSIRIS were published in 1992 by two industrial and university teams. Each team developed two conceptual designs, one with heavy-ions and the other with a laser-beam driver. Table 1 shows some of the major parameters of several heavy-ion IFE reactor studies.

These studies make it possible to identify the key technical issues for inertial fusion energy power systems. Table 2 lists the key top-level issues. A brief discussion of these issues is given below followed by the subpanel's views on near-term research priorities.

The first issue is demonstrating high gain at moderate driver energy. Most studies require a gain in the range of 70-120 for a driver output energy (transmitted to the target) of ~ 4-7 MJ. It should be noted that reactor design studies have typically focused on high-gain, multi-megajoule incident energy target concepts that are appropriate for economic power production. However, engineering development is cost limited. It therefore is worthwhile to consider if target designs that provide moderate gain (20-50) at low driver energy (1-2 MJ) are justified. Such targets would lower the facility cost associated with IFE engineering testing and fusion power demonstration.

The second issue concerns the feasibility of the indirect drive (ID) targets for heavy-ion and laser-drivers. For heavy-ion drivers some of the issues include: a) the properties of the method used to transport and focus the HI beam to the target, b) the accuracy and reproducibility of the repetitive HI target launch system which injects the ID targets to the center of the target chamber, and c) the ability of the high-z hohlraum cavity to efficiently convert and smooth the radiation incident on the DT capsule.

The issues of imploding an ID target with laser beams include: 1) plasma closure of the entrance apertures to the hohlraum, 2) accurate target tracking and pointing of the multiple laser beams to coincide with the entrance apertures of the moving ID target, and 3) accurate and reproducible indirect drive target propagation from the pellet injector to the center of the target chamber.

The third issue is the feasibility of direct drive targets. There are strong incentives to consider direct-drive (DD) targets because of higher gains. However, the *feasibility and performance characteristics of DD* targets are presently uncertain.

The fourth key top-level issue relates to the cost, efficiency, reliability and lifetime of the driver. The specific issues for heavy ion drivers are vastly different from those for laser drivers. The attraction of the HI approach to IFE has always been related to the fundamental technical feasibility of building a system with the required properties to drive a pellet to ignition. The basic accelerator technology is well developed, the beam physics is tractable, and existing accelerators have exhibited 25-year lifetimes with 95% availabilities. The key problem for HI is cost. Key issues associated with a HI cost reduction strategy include: a) space-charge limited transport of a bunched beam, and b) high current storage rings for heavy ion beams.

The key issues for the laser driver include:

1) obtaining an adequately high overall efficiency for the laser driver

2) performance, reliability and lifetime of the final laser optics

3) reliability of various components of the laser driver.

The above four issues are concerned with the target and driver. The remaining key issues relate to providing the proper chamber environment and reactor technologies related to energy conversion, fuel (tritium) generation and adequate radiation protection in a viable, reliable, and efficient high temperature system.

The fifth issue concerns the feasibility and performance of a viable wall-protection scheme. A practical IFE system requires protection of the chamber solid first wall from rapid degradation due to the extremely high instantaneous heat and particle loads associated with the X-rays and debris from the target explosion. While researchers agree on the need to protect the chamber solid wall, there is no consensus on the best means to achieve this. The two leading schemes for wall protection are : 1) thick liquid layer, and 2) thin liquid layer. In the first scheme, a thick layer of a liquid, e.g. flibe, is formed inside the chamber solid walls to form a "pocket" surrounding the microexplosion. This scheme has the added advantage of also protecting the first wall from neutron damage. Examples of key issues associated with this scheme are: 1) the ability to form a stable and uniform thick liquid layer so as to fully cover the interior surfaces of the first wall, 2) the feasibility of forming the liquid layer so as to allow holes for the driver beams without exposing the first wall to x-rays and debris, 3) the ability to re-establish the wall protection layer after the microexplosion, and 4) the need for this liquid to contain lithium to provide adequate breeding and the ability to clear the chamber from a multi-species liquid (e.g. the molten salt flibe).

Another scheme for wall protection relies on a thin liquid metal film wetting the first wall. This concept allows greater control over liquid feeding and uniformity of the liquid layer. It can use a single-element liquid; for example, lead, which is a neutron multiplier that can also enhance tritium breeding. Examples of issues with this scheme are: a) blast effects, b) flow around geometric perturbations, and c) protection of inverted surfaces.

The sixth IFE issue is cavity clearing at IFE pulse repetition rates. Following each pellet explosion, the cavity (chamber) fills with target debris and material evaporated or otherwise ejected from the cavity surfaces. This material must be removed from the cavity before the next target is injected. This generally requires recondensing condensable gases onto the surfaces of the first wall (or more specifically the surfaces of the wall protection layer) and by pumping non-condensable gases out through large ducts. Power reactors require a repetition rate of ~3-10 pulses per second. Evacuation requirements depend on propagation limits for both targets and driver energy. Base pressure requirements determine 1) the time to evacuate the chamber, and 2) the level of protection to the first wall (and final optics) afforded by the cavity background gas. Research is needed to better understand clearing requirements, the recondensation process, and to develop design solutions.

The seventh issue is concerned with demonstration of tritium self sufficiency, which is an absolute requirement for an IFE system operated on the DT cycle. Fuel cycle analysis shows issues associated with: a) the magnitude of the required tritium breeding ratio (TBR), and b) the magnitude of the achievable TBR. The required TBR is most sensitive to:

- tritium fractional burnup in the target
- the tritium mean residence time in the target factory
- the number of days of tritium reserve on site
- the doubling time

Studies show the required TBR is in the range of 1.05 to 1.25 depending on the specific value of the above parameters. The achievable TBR will depend on the specific design and materials of the first wall protection scheme, structural and breeding materials and void spaces occupied by penetrations (e.g., for beams).

The eighth issue is demonstration of low cost, high volume target production techniques. Target production for IFE reactors will require technologies which are presently either nonexistent or insufficiently developed for such application. A typical 1000 MW IFE reactor requires on the order of 10^8 targets per year. Hence, the cost per target needs to be in the range of 0.15 to 0.3 dollars for economic viability.

The ninth issue is demonstration of adequate radiation shielding of all components. The present codes and data provide adequate predictive capability. The issue, therefore, relates more to the ability to design and develop a fully integrated system in which all components are adequately protected from radiation.

The last issue concerns pulsed radiation damage and the thermomechanical response of the first wall/blanket. The severity and nature of this issue will depend, to a large extent, on the viability and specific characteristics of the wall protection scheme. If a thick liquid layer for wall protection proves feasible, then radiation damage and heat loads in the first wall/blanket will be moderate and can easily utilize technologies developed in magnetic fusion. A unique issue in this case may be the need to enhance tritium breeding. On the other hand, if the first wall protection scheme does not prove feasible, then the first wall/blanket issues such as radiation damage and thermomechanical response will become exceedingly critical.

Table 1

Major Parameters of Several Heavy Ion IFE Reactor Studies

Parameter	HIBALL-II	Cascade	HYLIFE-II	Prometheus-H	Osiris
Year Publ.	'84	'90	'91	'92	'92
First Surface	PbLi	C Granules	FLiBe	Pb	FLiBe
1st Surf. Radius, m	5	5	.05	4.5	3.5
Breeding Blanket	PbLi in porous SiC tubes	Flowing Li2O granules	FLiBe jet array	Li2O in SiC structure	FLiBe in porous C cloth
Primary Coolant	PbLi	C and LiAlO2	FLiBe	Pb & He	FLiBe
Vacuum Vesel	Ferritic steel	Al	Stainless st.	Ferritic st.	C/C compos.
Accelerator type	RF Linac	Induct. Linac	RIA	Induct. Linac	Induct. Linac
Driver Energy, MJ	5	5	5	7	5
Illumination	Cyl. sym.	2-sided	1-sided	2-sided	2-sided
Target Gain	80	75	70	103	87
Yield, MJ	400	375	350	720	430
Rep-Rate, Hz	5/chamber	5	8.2	3.5	4.6
Gross Th. Eff., %	42	55	46	43	45
Driver Eff., %	27	20	20	20	28
Net Power, MWe	946 x 4	890	1083	1000	1000

Table 2

Top-Level Issues For Inertial Fusion Energy

1	Sufficiently High Target Gain at Economical Driver Size: a) G > 30 for indirect drive with ion beams. b) G ~ 100 for direct drive with lasers.
2	Driver cost, efficiency, reliability, and lifetime: a) Demonstration of the required performance of a Driver operated in a repetitive mode. b) Performance, reliability and lifetime of final optics.
3.	Fusion Chamber: a) Feasibility and performance of a viable wall-protection scheme. b) Cavity clearing at IFE pulse repetition rates. c) Tritium self-sufficiency in a practical IFE system. d) Adequate radiation shielding of all componenets. e) Pulsed radiation damage and thermomechanical reponse of first wall/blanket, particularly for concepts without thick liquid protection.
4.	Sufficiently low cost, high volume, target production system.

Reasons for IFE Focus on Heavy Ion Driver

Reactor studies have examined fusion energy systems with both heavy-ion and laser drivers. At this stage of inertial fusion R&D, the data base is not sufficient to conclusively select a driver that will ultimately be proven to be the most attractive for fusion energy system application.

However, there are compelling reasons why the IFE program within OFES should focus only on heavy-ion drivers. The key reasons are:

1. the constrained IFE budget permits only partial development of one driver concept

2. many of the issues of the laser-driver are being addressed by the Defense Program (DP) within DOE. HI development is not supported by any program other than IFE.

3. the current data base, albeit limited, indicates that heavy-ion drivers have greater potential for IFE application than laser drivers because: a) HI drivers have much higher efficiency than lasers, b) HI beams have a much higher reliability than laser systems, and c) the feasibility of the final optics for a laser system remains a major feasibility issue.

For the above reasons, it appears prudent to focus the limited IFE resources on the driver to R&D of heavy ions. However, future research results may warrant a new assessment of the driver selection. In particular, if Direct Drive Targets prove feasible, higher gains will be possible and the potential of laser drivers will vastly improve. Such results coupled with advances in laser system performance, e.g. in Diode-pumped solid state and KrF lasers, will make it necessary to reevaluate the selection of the best driver for IFE applications.

Breadth of the IFE Program

The IFE Program within OFES should not be limited to only the driver. IFE effective research requires devoting a part of the resources to some of the other critical scientific and technological issues such as chamber technology because: 1) these issues are critical to the feasibility and attractiveness of IFE, 2) the research results will greatly influence future research priorities for the driver and the driver-target coupling, and 3) data is needed in order to design meaningful experiments on NIF that are of relevance to IFE.

Appendix IV

Minutes of the FESAC Meeting of July 16-17, 1996

FUSION ENERGY SCIENCES ADVISORY COMMITTEE

Hilton Hotel
620 Perry Parkway
Gaithersburg, MD 20877
July 16-17, 1996

MINUTES

Present:
Robert W. Conn, Chair
Thomas B. Cochran
Harold K. Forsen
Joseph G. Gavin, Jr.
Katherine B. Gebbie
George R. Jasny
Michael N. Knotek
Stephen L. Rosen
Marshall N. Rosenbluth
P. Floyd Thomas, Jr.
Demetrius D. Venable

DOE Representatives:
N. Anne Davies
James Decker
Milton D. Johnson

Ex-Officio
Terrence A. Davies

Introduction

In the interests of brevity, remarks made during the various presentations both by the speakers and by members of the Committee have been included in each section without reference to source.

Tuesday, July 16, 1996

Welcome/Remarks - Robert W. Conn (University of California, San Diego), Chair, FESAC

The Chairman welcomed the committee members to the meeting. He reviewed the agenda, which is attached as Appendix I, and pointed out that Dr. Martha Krebs would meet with the Committee during the afternoon of the second day of the meeting to receive the reports of the Committee. Dean Conn reminded the Committee that they had been provided with three charges, relating to a review of the major U.S. fusion facilities, of alternative concepts, and of the Inertial Fusion Program, respectively. He pointed out to members that this would be the last meeting of the Committee with the current membership. New members and a new chairman have been nominated by the Secretary of Energy. Their term of office begins on August 18, 1996. The members of the new committee had been invited to attend this meeting, and eleven of them were present.

Dean Conn emphasized the point that every review that had been undertaken regarding the fusion program, whether it be of part of the program or of the entire program, always resulted in an excellent report and suggestions that more funding be made available. In accepting reports and recommendations relating to portions of the program, it was therefore necessary for the Committee to consider and maintain a balance within the overall program.

The Chairman stated that after hearing the presentations of Dr. Jim Decker and Dr. Anne Davies, it would be appropriate for the Committee to draft its reaction to the budget process and to forward its thoughts and recommendations to the Secretary of Energy.

DOE/ER Perspective - James Decker (DOE-OER)

Dr. Decker welcomed the new members to FESAC, indicating that their duties as committee members would start, officially, in August. He expressed his thanks to Mike Knotek and Jim Callen for their work with restructuring of the fusion program. He noted that the language in the FY 1997 Appropriations Bill in the Senate had described the new restructured program as well planned, as a result of which their budget mark had been made as high as possible within their budget constraints.

Dr. Decker pointed out that DOE, with the help of the Committee, had substantially complied with the request of Congress to restructure the program. He reviewed the House and Senate FY 1997 budget marks, pointing out the very restrictive language that accompanied the House mark. Dr. Decker stated that due to a lack of champions in Congress, non-defense discretionary funding had been squeezed this year, although science-related programs had done quite well. Energy supply, on the other hand, was not perceived as a problem at the moment. He emphasized that OFES needed relief from the very restrictive language that accompanied the House mark for the fusion program.

Implementation of FEAC Restructuring Recommendations and FY97 Budget Situation - N. Anne Davies (DOE-OFES)

Dr. Davies explained the DOE responses to the restructuring recommendations made by FEAC. DOE had accepted the recommendations, which had formed the basis of the FY97 funding request, and had produced a strategic plan to implement the program. Whereas the previous program had relied upon modest advances in science and major developments in technology, the new program emphasizes advances in science. in support, Dr. Davies compared the FY96 budget with the FY97 request.

Dr. Davies explained the new Plasma Science Initiative, which was a program for university participation only. OFES had set aside $4 million for this program, although there was a possibility that NSF might supplement it for basic plasma research. This funding would form the backbone of the Young Investigators Program and of the Opportunities in Basic Plasma Research Program. Dr. Davies emphasized that the focus of the new program was on innovation and science. She outlined the future plans for facilities, for alternative concepts, for theory and modeling, and for materials.

In referring to the change in advisory committee membership, Dr. Davies pointed out that the Committee was being enlarged and that the new membership will reflect the change in program emphasis. She explained the new organizational structure at OFES, stating that it is still emerging.

Dr. Davies analyzed the House mark as specified by the accompanying language. $209 million was ear-marked for specific programs, leaving just $16 million to cover $55.2 million of other programs that had been contained in the President's request. While the Senate language was much more flexible, either mark would result in a slowing of the restructuring process plus the loss of an additional 200 persons from the program.

Discussion of Options for Dealing with the FY97 Budget Marks - FESAC

Following a discussion of the shortcomings of the prescriptive language that had accompanied the House mark, Dean Conn stated that it would be appropriate for the Committee to write to the Secretary of Energy forwarding their views. He said that a draft letter had been prepared for the Committee's review which requested that the Secretary do two things: Seek to increase the overall funding level; seek removal of the prescriptive language. Dr. Conn added that he would like the Committee's letter to be brief and to the point, indicating that fusion community leaders were writing additional letters to the Secretary, some of which would be detailed and thus would supplement FESAC's. Several suggestions for modification of the letter were made and discussed. It was suggested that the level of overseas competition should be called out in the letter since it was felt that the Committee should let Congress know that the U.S. had relinquished the lead in fusion. This recommendation was not acted upon. After modification of the draft letter, a "straw" vote was taken, which was unanimously in favor of the modified wording. A request was made, and accepted, that a further review of the "final" letter be undertaken after re-typing before a formal vote was taken and recorded.

Office of Science and Technology Policy Perspective - Ernest Moniz (OSTP)

Mr. Moniz reviewed the budget marks and emphasized the commitment of OSTP to the fusion program. He said that it was important that the credibility of FEAC/FESAC be maintained, and that the Committee must have the support of the fusion community.

Mr. Moniz provided a comparison of Administration and Congressional out-year assumptions, indicating that the future outlook was dependent upon the accuracy of many assumptions and upon the desire to balance the budget by 2001. Irrespective of whether the Administration or the Congressional path is pursued, things will still be difficult.

Referring to ITER, Mr. Moniz stated that he would like to see the EDA through to completion but that he had difficulty in seeing how the U.S. could be a full partner in construction. He emphasized that there were a large number of good programs competing with fusion for a shrinking pool of dollars. Future fusion funding will depend upon the perceived value of the program. The differences in allocations between the House and Senate adds to conferencing difficulties. Mr. Moniz acknowledged that lower fusion budgets would erode the U.S. competitive position internationally, and that continuing reductions in funding would mean that the U.S. could not be an international player at all, and could lead to erosion of the current community unity.

Continuing Discussion of the Letter to the Secretary of Energy - FESAC

Upon resumption of discussions, the Committee reviewed the final version of the letter to the Secretary of Energy. The motion was made and seconded that the letter be sent to the Secretary as written. The motion was passed by unanimous vote.

Highlights of FESAC Report on the Major Facilities Charge - James D. Callen (University of Wisconsin), Hutch Neilson (ORNL)

Drs. Callen and Neilson reviewed the findings and recommendations of the panel that had undertaken the facilities study, explaining the scientific issues, their relative importance, and how each facility will contribute to their understanding. They thanked all who had taken part in the review for their help. The panel's report had been forwarded to Dr. Conn who, in turn, had forwarded it to DOE with the Committee's letter of transmittal.

In answer to questions, the presenters indicated that the issue of participation by industry, other than by General Atomics, had not been taken up specifically during the facilities review. Neither was the review undertaken in the context of a formal assessment of worldwide facilities, although two of the panel members had been selected from overseas programs to ensure that the panel as a whole clearly understood the capabilities of overseas facilities. The panel had not reviewed what should happen to the facilities under other budget scenarios: This had not been within the scope of the charge. Very difficult decisions would have to be made at lower budget levels and these should be tackled by the new FESAC, not by the panel, but with advice and input from Scicom if requested. All facets of the program that had been suggested by the panel were important to ITER and could potentially provide the U.S. with ITER credit.

Scicom Report to FESAC on the Alternatives Charge - James D. Callen (University of Wisconsin), Farrokh Najmabadi (University of California, San Diego)

Dr. Conn discussed the charge stating that the review was directed at recommending an investment strategy for funding alternative concepts, in particular taking cognizance of the international fusion program. The objective was to produce an overall strategy for a U.S. alternatives concept development program that would include experiment, theory, modeling and system studies, to recommend how best to collaborate internationally, to encourage new innovations, to assess scientific progress, and to determine the criteria to be used in determining when a concept was ready to move on to proof-of-principle. Dr. Conn reminded the Committee that the panel had delivered an interim report dealing with the spherical tokamak to FESAC in May.

Dr. Callen described the make-up of the panel, which had been chaired by Dr. Najmabadi, and had included consultants from the fusion programs of Japan, the United Kingdom and Germany.

Dr. Najmabadi described the panel's activities and provided the background to the review. The term alternative concept had been taken as referring to magnetic configurations other than the standard or advanced tokamaks. The panel had found that a programmatic as well as a cultural distinction existed between mainline and alternative research. Alternatives and tokamaks are viewed by OFES and by part of the fusion community as competitors rather than as being complementary, thus ignoring the strong connection that exists between most magnetic confinement approaches. Dr. Najmabadi outlined the concepts research plan that had been suggested by the panel, together with a strategy for its implementation, described the anticipated benefits, and defined the various stages of development that a new concept was likely to go through. While it was agreed that peer reviews should be a part of the process, the danger exists that these could squeeze out highly innovative concepts. The establishment of a Concept Development Panel was recommended, with this role possibly being played by Scicom. Typical review and selection processes, applicable respectively to the concept exploration and proof-of-principle stages, were presented.

The status of spherical tokamaks was reviewed and the suggestion made that this concept is ready to move to the proof-of-principle stage. The status of stellarators and that of other magnetic confinement techniques were discussed. A series of interim recommendations, pending the establishment of a Concept Development Panel, was presented.

Scicom's Future Role - James D. Callen (University of Wisconsin)

Dr. Callen took the opportunity to ask if Scicom was still needed, now that a larger, more fusion-oriented FESAC was being formed. He pointed out that the community, including many panelists, were unhappy with the trend towards the establishment of many, smaller panels, and would prefer that one, larger body, review everything. He suggested that this matter should be dealt with by the new committee and new chairman.

Discussions on Alternatives Report - FESAC

The Committee discussed the report of the panel and reviewed Dr. Callen's letter to Dean Conn on the report. Dr. Conn pointed out that the letter contained six major points and recommendations. Again, each individual topic had been pronounced good and in need of increased funding, but the funding simply was not available. He suggested that the Committee should strive to achieve a balance. After further discussion of the letter, it was agreed that it should be modified overnight and reviewed again in the morning.

Wednesday, July 17, 1996

Report to FESAC on the Inertial Fusion Energy Charge - John Sheffield (ORNL), John D. Lindl (LLNL), Mohamed A. Abdou (University of California, Los Angeles)

Dr. Sheffield reviewed the charge, presented the make-up of the panel, and outlined it's meeting agendas. He described the basic principle behind the technology that was under review, illustrated a basic system, and detailed the technical issues involved. The major uncertainties needing resolution include the driver, beam focusing, pellet manufacture, pellet positioning, and clearing of the chamber after a fusion reaction.

Dr. Sheffield described the changes and progress that had occurred since the heavy-ion driver program had last been reviewed, during the seventh meeting of the initial FEAC. He outlined the challenges, the future needs, the structure of an integrated research experiment, and the criticality of the time-frame. Target physics issues were presented, together with a list of future priorities. Budget implications were analyzed.

Dr. Sheffield contrasted Defense Program applications with energy applications. Defense applications require that a single pulse of energy only hit the target, and are such that ample time can be left for the chamber to clear before firing the next pulse. On the other hand, energy production requires a continuous procession of pulses, typically at 4 Hz, and the chamber needs to be cleared in-between each pulse. He expressed concern that in the present budget circumstances the panel had recommended increasing the funding for this program, and speculated that Defense Programs might wish to help by contributing to it.

The Committee discussed the chamber exhaust problem, and raised concern over the very large diameter needed for the heavy ion accelerator ring.

Dr. John Lindl pointed out the synergism between laser target designs and heavy-ion target designs. He described and contrasted current drive technologies, and compared their coupling to the targets. He emphasized the value of modeling in this work, describing targets in more detail and outlining what still needs to be achieved. Dr. Lindl explained the importance of energy gain and how it affected the economics of the process. Greater gain was needed for laser energy production than for heavy-ion driver energy production, since the efficiency of the laser beam production process was significantly lower than that for heavy-ion beam production. He stated that all the critical target issues were being addressed. 10^{10} energy pulses will be required from the driver over the life-time of a commercial power plant. Heavy-ion drivers have demonstrated the required longevity, reliability and desired repetition rate, as well as exhibiting better initial efficiency.

Dr. Lindl expressed uncertainty over the level of funding that might be available for this program in the out-years. The presently envisaged funding projection calls for a substantial increase in funding in FY99. That budget will be in preparation in a year's time and since it appears unlikely that the political climate will have changed by then, funding of the increase will be extremely difficult.

In answer to questions, Dr. Lindl pointed out that the size of laser needed for energy production will depend upon the eventually-realized target gain. It is possible that direct drive on the target would improve overall laser system efficiency. An on-going watch will be kept on laser experiments, and especially results from the National Ignition Facility (NIF), where both direct drive and indirect drive experiments will be performed.

Dr. Abdou described the general characteristics of IFE power plants and summarized the many conceptual design studies that had been carried out to date over a 25 year span. He outlined the potential for IFE, and enumerated the top-level issues that need resolution. These include target gain versus driver energy, the efficiency, reliability and cost of the driver, fusion chamber robustness, and the development of an economical target production system. He indicated that determining the economics of a power plant operating at 1,000 MW was not relevant, since over the time-scale of the envisaged development, production units of 2,000 MW were more likely to be needed. The IFE process will exhibit a large gain in economy as the output increases.

Dr. Abdou pointed out that next to ignition, chamber wall protection is likely to be the next most important issue. High instantaneous loads of X-rays, target debris and neutrons can lead to serious ablation of surfaces surrounding the micro-explosion. Dr. Abdou indicated that liquid wall protection schemes were being considered. He described one such system with the aid of diagrams: It utilized thick liquid, and would permit shallow land burial of the chamber and supporting structure even after 30 years of operation. A thin-liquid system is also being explored. Chamber clearing issues are important, as are target injection challenges.

A discussion of the explosive force associated with the pellets indicated that it was the X-ray portion of the yield that was most damaging. Hence the need for wall protection by a liquid that must be kept at high temperature. The choice of material for the hohlraum also presents an important issue for resolution.

During the ensuing discussion, the incompatibility of producing large amounts of energy in a system that employed extensive miniaturization was pointed out. The issue of show-stoppers was raised, but it was agreed that these, and the budgets needed to resolve them, had been adequately dealt with during the presentation.

Finalize FESAC Letter Reports on the Alternatives Charge and on the Inertial Fusion Energy Charge - FESAC

The Committee reviewed and refined the letters of transmittal that would accompany the panel reports on alternative concepts and inertial fusion energy. The motion to accept the final version of the IFE letter was made, seconded, and passed by unanimous vote. The final version of the alternative concepts letter was also agreed to unanimously.

Executive Summary for Dr. Martha Krebs and Dr. James Decker - FESAC

Dr. Krebs thanked the Committee for its work in assisting the Department with restructuring the fusion program, indicating that special thanks were due to two persons. She then presented Mike Knotek with a plaque containing the Distinguished Associate Award, which had been signed by the Secretary of Energy. Finally, she gave a special vote of thanks to Bob Conn, not just for his chairmanship of the restructuring process, but for the five years that he had served as Chairman of the Committee.

In referring to the appropriations marks, Dr. Conn informed Dr. Krebs that FESAC had developed a response that was directed to the Secretary. In essence, the response urged two actions: That the Secretary seek improved funding; that the Secretary request removal of restrictive language.

Dr. Conn then reviewed the IFE transmittal letter, and followed this with a review of the alternative concepts letter of transmittal. He stated that the response to the spherical tokamak charge had been forwarded previously and had not been dealt with specifically at this meeting. However, it had been integrated into the final transmittal letter. Dr. Conn emphasized that distinctions between mainline and alternative concepts could become poisonous, that it was highly desirable to promote the change in culture that had been recommended in the panel's report, and that the Committee had agreed with the notion of a Concept Development Panel.

With respect to the IFE program, Dr. Conn pointed out that one recommendation had been to appoint a joint Defense Programs/Energy Research steering committee, to coordinate those declassified activities that were common to both programs. This might be difficult since Defense Programs had just dissolved its ICF advisory committee. Nevertheless, optimization of both programs needs to be assured, and overlap and redundancy between them eliminated.

Terrence A. Davies
School of Engineering
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July 22, 1996

Appendix I

FUSION ENERGY SCIENCES ADVISORY COMMITTEE MEETING
July 16-17, 1996

Hilton Hotel
620 Perry Parkway
Gaithersburg, MD 20877
July 16-17, 1996

AGENDA

Tuesday, July 16, 1996

9:00 AM	Welcome/Opening Remarks	Conn
9:15 AM	DOE/ER Perspective	Decker
9:45 AM	Implementation of FEAC Restructuring Recommendations	Davies
10:15 AM	Discussion of Options for Dealing with the FY97 Budget Marks	FESAC
11:00 AM	Office of Science and Technology Policy Perspective	Moniz
11:30 AM	Continue Discussion of the Letter to the Secretary of Energy	FESAC
12:30 PM	Lunch	
2:00 PM	Highlights of FESAC Report on the Major Facilities Charge	Callen/ Neilson
2:30 PM	Scicom Report to FESAC on the Alternatives Charge	Callen/ Najmabadi
	Scicom's Future Role	Callen
4:00 PM	Discussions on the Alternatives Report	FESAC
5:30 PM	Adjourn	

Wednesday, July 17, 1996

9:00 AM	Report to FESAC on the Inertial Fusion Energy Charge	Sheffield/ Lindl/Abdou
11:00 AM	Finalize FESAC Letter Reports on the Alternatives Charge and on the Inertial Fusion Energy Charge	FESAC
12:00 Noon	Lunch	
1:30 PM	Continue Work on Letters	FESAC
3:30 PM	Executive Summary for Dr. Martha Krebs and Dr. James Decker	Conn/ FESAC
5:30 PM	Adjourn	