

Fusion Energy Sciences Advisory Committee Reports on

Review of the Fusion Materials Research Program,
Review of the Proposed Proof-of-Principle Programs,
Review of the Possible Pathways for Pursuing Burning Plasma Physics,
and
Comments on the ER Facilities Roadmap

July 1998



U.S. Department of Energy
Office of Energy Research

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Section 1

POST OFFICE BOX 2009
OAK RIDGE, TN 37831-8070

August 3, 1998

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

Dear Dr. Krebs:

The FESAC met on July 30-31 and considered the charge in your letter of January 14, 1998, regarding the Materials Research Program. The FESAC accepted the report on the U.S. Fusion Program on Structural Materials, prepared by the subcommittee co-chaired by Dr. S. Harkness and Dr. C. Baker.

FESAC appreciates the very considerable work by the subcommittee, which has resulted in a helpful and articulate report. We particularly endorse the subcommittee's central recommendations that the materials program be strongly coupled with an overall systems approach, and broadened to allow increased modeling and innovative exploratory research on novel materials. The new initiatives outlined in the report represent significant scientific opportunities for the program, and should be supported if possible.

FESAC does believe that a continuing and broadening systematic exploration of a wide variety of materials in the context of both the magnetic and inertial fusion requirements is appropriate and consistent with the restructured fusion program's focus on science, innovation and long-term optimization of the fusion product. Such examination will require regular peer review to maintain balance among various candidate materials. The re-examination should take into account developments in other fields and seek opportunities to benefit from other, larger government and industrial materials programs. The fusion program will remain highly dependent on such leveraged activities as long as it lacks sufficient resources to fully qualify materials unique to fusion.

Sincerely,



John Sheffield, Chair
on behalf of the Fusion Energy
Science Advisory Committee

Enclosures

cc: N. A. Davies, DOE-OFES
J. Decker, DOE-ER
A. Opdenaker, DOE-OFES
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July 15, 1998

John Sheffield

cc: Mohamed Abdou Bill Hogan Carl McHargue Paul Shewmon Dick Briggs Nat Fisch Joe Johnson Earl Marmar Ned Sauthoff Anne Davies	Charlie Baker Gerry Kulcinski Bob Odette Stu Zweben Jim Callen Katherine Gebbie Charles Kennel Bruce Montgomery Tony Taylor Al Opdenaker	John Davis Mike Mauel Dave Petti Ira Bernstein Bob Conn Dick Hazeltine John Lindl Marshall Rosenbluth Nermin Uckan John Galambos
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Attached is the Review of the Fusion Materials Research Program that was requested by Dr. Martha Krebs. This Review was discussed at the FESAC meeting of May 26. Several requests for additional information arose from that meeting to which the Panel has attempted to be responsive. While the major conclusions and recommendations of the Report have mainly stayed the same, it is hoped that the logic for them is now more clearly explained. A strengthened recommendation is that the Fusion Materials budget be increased by one or two million dollars per year to support the proposed new initiatives in modeling and exploratory concepts.

A "roadmap" to the Panel response and the major comments from the May meeting is as follows:

- Expanded Discussion of Modeling; p. 16; p. 23
- International Efforts and Collaborations; p. 22; Appendix 1
- Logic for Balance of Effort Between Materials Systems; p. 11, 12, 23
- Logic for New Effort on Exploratory Materials, p. 18, 19
- Use of Accelerator Based Neutron Sources; p. 20, Appendix 2

I look forward to answering any further questions at our upcoming meeting of July 30.

Sincerely,

**Samuel D. Harkness
Co-chair, Fusion Materials Panel**

Att.

Executive Summary

The Fusion Energy Science Advisory Committee was asked to conduct a review of Fusion Materials Research Program (the Structural Materials portion of the Fusion Program) by Dr. Martha Krebs, Director of Energy Research for the Department of Energy. This request was motivated by the fact that significant changes have been made in the overall direction of the Fusion Program from one primarily focused on the milestones necessary to the construction of successively larger machines to one where the necessary scientific basis for an attractive fusion energy system is better understood. It was in this context that the review of current scientific excellence and recommendations for future goals and balance within the Program was requested.

Scientific Quality of Present Program

The scientific quality of the work was judged to have elements of very high scientific quality and all elements are being conducted in a competent manner. Metrics evaluated included evidence of recognition, publications per worker, new people attracted to the work and significance of recent accomplishments.

The current Program funding supports ~15 FTE's and represents about 3% of the DOE's Office of Fusion Energy Sciences FY 1998 Budget. It is, therefore, obviously not adequate to undertake the qualification of any new materials system. The Program has used the resources to focus on materials performance questions, particularly those associated with radiation damage.

Situational Analysis

In looking ahead at recommended future goals and the balance required to achieve them, it is important to consider the assessment of the current situation for fusion energy to provide a context for the Panel's suggestions and comments. Key aspects of this situational analysis included:

- Fusion energy R&D is inherently a long-range endeavor aimed at the development of an attractive power systems concept by the middle of the next century. The results of focused materials research and development will be determinant to the realization of the potential of fusion energy as well as important to the design and construction of nearer term experimental facilities.
- The U.S. Fusion Energy Sciences Program has been restructured to focus on science and technology innovation, while pursuing energy production technology through international collaboration.
- The use of fusion power by the World's societies will ultimately be determined by its relative attractiveness to other energy alternatives. While the exact weighting of decision factors is impossible to establish at this time, it is clear that capital

cost, operating expense, plant availability, worker and public safety and final decommissioning and waste disposal costs will all be considerations.

- It is prudent to assume that the funding resources for materials R&D will continue to be constrained (as will be the overall U.S. program) at about present levels. Some modest growth may be possible. In the long-term, significantly more resources will be required to develop the materials and technology for fusion energy.
- In the near-term the materials R&D efforts will emphasize issues related to deuterium/tritium fusion. At the same time, the program needs to account for various possible magnetic and inertial confinement approaches. In the long-term alternate fusion fuel cycles should also be considered.
- One should anticipate continued, significant enhancement in computing capability as it relates to modeling of materials performance.

Key Issues

The materials feasibility issues for fusion energy are motivated by the long-term view of the needs for attractive, commercial power systems. From a users' perspective (e.g. electric generating companies) the major requirements are economic competitiveness, safety (e.g. no accidents which lead to public evacuation near the site), a closed tritium fuel cycle, ability to maintain the power core, reliable operation (very low number of unscheduled shut-downs per year), and no, or at least, minimal radioactive material greater than Class C waste storage. Achieving economic competitiveness implies more compact reactors which usually means higher power densities, high temperatures for high thermal efficiencies, modest component fabrication costs, low failure rates, high reliability, low unscheduled outages, and acceptable lifetimes to minimize scheduled replacements. Unfortunately, a clear materials/design path has not yet emerged that can provide a balanced combination of these important attributes.

Perhaps the most important "key issue" for fusion materials development is the recognition that the fusion environment and needs for an attractive power system concept present a major challenge involving a wide variety of complex, interacting phenomena and conditions. This implies directly that materials R&D activities should be considered as part of an integrated program along with engineering science research, technology/component development and advanced design and systems assessments.

Goals and Objectives

The recommendation of the Panel, based on the current situation, is that the Fusion Materials Program adopt as its goal:

To provide the materials science knowledge base that will enable the utilization of fusion energy. The near-term emphasis will be on feasibility issues for in-vessel components for deuterium/tritium systems.

The Panel further recommends the following as the main supporting objectives of the program:

- Based on an integrated effort including materials research, advanced technology and design studies, identify candidate material systems for meeting the needs of fusion energy.
 - More fully integrate the planning and conduct of the materials R&D activities with engineering science research, technology/component development and advanced design and systems assessments. System roadmaps are useful tools for defining the feasibility questions, necessary skills and required resources.
- Provide the knowledge base on the effects of the fusion environment on materials performance.
 - For each of the three materials approaches currently under study (ferritic alloys, vanadium alloys and silicon carbide based composites), focus on the property limitations and the system level questions that could prevent an attractive design from being developed.
 - IFE concepts may require special emphasis on thick liquid walls (may also be applied to MFE) and optical materials. An assessment which will identify the key materials feasibility issues related to IFE systems is needed.
- Identify the performance-limiting phenomena and apply the materials science principles to the development of improved properties and the expansion of performance windows.
 - The combined advances in material science understanding and computing capability extend the ability to develop meaningful physically-based, semi-empirical models that can guide and help interpret experimental information. These should be applied to the key identified feasibility issues. Crosscutting concerns such as the overall effects of a combined high generation rate of gas and atomic displacements are particularly good candidates.
 - Exploratory approaches and materials should be identified and pursued if they offer greater potential to satisfy the combined requirements of performance, reliability, safety and long term waste disposal.

- Develop performance data and provide materials input for fusion design studies.
 - It is recognized that most of the extensive generation of detailed, system specific engineering design data will not be needed until such a time as the overall program is prepared to commit to an attractive energy system. It must be recognized that the development of the required information will require years to accumulate.
- Pursue fusion energy materials science and technology as a partner in the international effort.
 - It is very important to coordinate the U.S. effort with others based on the complexity of the materials challenge and the level of resources available in the U.S.

Balance of the Program

The Panel considered the appropriate balance between current programmatic elements in the context of the suggested goal and objectives and the overall international effort on materials. The intermediate term, and ultimately long term program objectives, would be enhanced by greater emphasis on data analysis and modeling and pathways to introduce exploratory and innovative materials concepts. These exploratory efforts should consider the needs of innovative magnetic confinement concepts as well as the needs of inertial fusion energy.

The fusion materials program should maintain a focus on key issues related to in-vessel structures in a D-T fueled reactor, with significant emphasis on irradiation experiments. Those issues that threaten the viability of a design concept should be addressed first. It is recommended that the fraction of research related to basic understanding of materials performance in the fusion environment be increased. This increase is motivated by increased capability and sophistication of computer modeling, the need to make the most effective use of expensive and difficult-to-obtain materials data, and the desire to form stronger connections with the greater scientific community. There is also a great opportunity to develop and apply modern computational tools to engineering design, analysis and simulation of in-vessel components. Such studies would benefit the materials research program by sharpening the understanding of performance requirements and promoting the development of advanced design methods needed to assure structural integrity without undue conservatism.

Introduction of new ideas and people to the fusion materials program should be encouraged. Modest increases in funding and yearly opportunities for competitive, peer reviewed proposals are possible mechanisms to support such renewal. Innovative approaches to promoting sustained and mutually beneficial collaborations between laboratories, universities and industry should be developed. Standards of quality, performance and progress towards program goals should also be more fully developed and used to prioritize the investments made by the Office of Fusion Energy Sciences.

Specialized Facilities for Materials Research and Development

As part of the overall charge, the Panel was asked to review the program efforts aimed at a fusion neutron source test facility including US involvement in the international fusion material irradiation facility (IFMIF). The Panel also considered the general topic of specialized facilities for materials research and development.

In the absence of a test facility with a prototypical fusion environment, fission reactors will remain the primary test facilities for fusion material irradiation. The value of fission reactor irradiation can be enhanced considerably by: 1) continued emphasis on innovative techniques to better simulate helium, hydrogen and other transmutation rates; and 2) more emphasis on modeling to better understand and extrapolate radiation damage results to those expected in fusion spectra.

The panel did not make a technical assessment of the merits of an accelerator-based neutron test facility. Because of the current fusion budget situation, there are considerable uncertainties in the decision to construct IFMIF. Given such uncertainties, it does not seem prudent to assume that an IFMIF-type facility will be available over the next decade. If funding for a materials test facility becomes available, the panel believes that a review of the relative merits of different materials and blanket test facilities concepts should be made at that time. Both plasma-based volumetric sources and accelerator systems should be considered.

Resource Allocation Perspective

The current Fusion Materials Program is funded at about an annual budget of \$6M. The activities that are covered by these resources are currently focused on advanced ferritic alloys (~25%), vanadium alloys (~50%), and SiC composites (~25%) but with smaller efforts on modeling, neutron source studies and insulating ceramics. These funding levels must be considered in the context of the world-wide effort that is shown in Appendix 1. For the total international effort, roughly 45% is on ferritic alloys, 14% on silicon carbide composites and 22% on vanadium alloys. The Panel felt that the U.S. effort should continue to remain involved with the ferritic alloys program while exploring the potential for vanadium alloys and silicon carbide composites. The Panel believes that some enhancement is needed in modeling and materials to support exploratory concepts for both MFE and IFE that are emerging in the restructured Technology Program. It is recommended that a modest increase be made in the Fusion Materials Budget of 1 to 2 million dollars per year to support these new initiatives.

Additional modeling and knowledge-base development support should be sought from outside the fusion materials program (such as combined efforts with the BES materials program, the ER strategic simulation initiative and the Stockpile Stewardship materials modeling effort).

At the modest level of the current base program, there are few resources available beyond what are needed to address the identified materials system feasibility issues.

However, if a flat materials budget is necessary, then the Panel recommends that the SiC program be reduced and, if necessary, a smaller reduction of the vanadium program to allow the recommended initiatives in modeling and exploratory approaches. This recommendation on funding priorities is based on the following rationale:

- there are serious issues related to the ability to fabricate large complex components out of an inherently brittle material like SiC composites for use in the fusion environment even if the irradiation performance problems are solved
- the vanadium program is the largest U.S. program and would therefore be the least impacted by the modest funding reduction
- the level of funding for advanced ferritics is the minimum necessary to leverage the larger programs in the EU and Japan

Panel Report

Background

The Fusion Energy Science Advisory Committee was asked to conduct a review of the Fusion Materials Research Program (the Structural Materials portion of the Fusion Program) by Dr. Martha Krebs, Director of Energy Research for the Department of Energy. This request was motivated by the fact that significant changes had been made in the overall direction of the Fusion Program from one primarily focused on the milestones necessary to the construction of successively larger machines to one where the necessary scientific basis for an attractive fusion energy system is better understood.

It was in this context that the review of current scientific excellence and recommendations for future goals and balance within the Program was requested. The last Review of the Fusion Materials Program (though broader in charter than this Review) occurred in 1993.⁽¹⁾ The scientific excellence of the current Program was considered in the context of the conclusions reached by that review. Key results from 1993 included: that low/reduced activation materials offer the potential to improve the safety and environmental performance of fusion energy; that preparation for building a Demonstration Reactor requires that both ITER and a 14 MeV neutron source proceed on a similar schedule; that fission-reactor testing is a crucial element of any viable strategy; that ultimately the needed materials qualification effort will require an investment of several hundred million dollars; and, that an evolutionary rather than revolutionary introduction of new materials is suggested.

In general, the current review found that the materials work sponsored by the Office of Fusion Energy Sciences (OFES) was responsive to these major conclusions. The Program is almost exclusively focused on the development and preliminary qualification of low activation materials. About 60% of the budget is directed toward understanding their response to neutron irradiation.

The make-up of the Committee is included as Attachment 1 and comprised a mixture of materials scientists familiar with the requirement of advanced nuclear systems as well as physicists and nuclear engineers knowledgeable of the needs of both magnetic and inertial fusion systems.

Background information was provided through the Office of Fusion Energy Sciences (Dr. F. W. Wiffen) and at a Panel meeting held in Pittsburgh on March 2-3, 1998, where many principal contributors provided expert opinion on both the current status and the future needs for the Program. The agenda for this meeting is included as Attachment 2.

⁽¹⁾ DOE/ER-0593T, Fusion Energy Advisory Committee, April 1993.

A second meeting of the Review Panel was held in San Diego on April 16-17, 1998 to formulate a position on the key Panel findings. The agenda for this meeting is included as Attachment 3.

Scientific Quality of Present Program

The Panel developed four metrics to assess the scientific quality of the current program. These were:

- (1) publications and peer recognition;
- (2) an evaluation of recent technical achievements;
- (3) interest by other segments of technology as evidenced by technology transfer; and
- (4) the ability to attract young professions to work on the program and university interactions.

Based on these metrics, the Panel concluded that:

The present program was judged to have elements of very high scientific quality and all elements of the program are being conducted in a competent manner.

Highlights from this review included:

- (1) Publications and Peer Recognition:
 - 300 papers published between 1995 and 1998.
 - Publications prolific; rate has been 4.5/full time equivalent/year.
 - 50% of publications in Journal of Nuclear Materials.
 - Four members of the community have been named Fellow of the American Society of Metals, two of the American Nuclear Society.
- (2) Recent Technical Achievements:

Six were noted to be particularly significant. These were:

- The superior resistance to radiation-induced shifts in the Ductile-Brittle Transition Temperature (DBTT) and the superior swelling resistance of the low activation 9Cr-2WVTa ferritic/martensitic steel is a major finding that has very important implications for performance of devices in radiation environments. This alloy is the basis for Japanese development efforts on the F82-H steel.

- The success of molecular dynamic (MD) simulations in describing cascades in iron and in demonstrating that the primary radiation damage event is similar for fission neutrons and 14 MeV neutrons allows both further modeling and simulation and fission reactor-based experiments to proceed with greater confidence.
- The development of physically-based micromechanical models for failure in the ferritic/martensitic steels and the progress in developing methodologies to relate small specimen property measurements to full size components greatly increases the confidence in the results of tests on irradiated specimens and in radiation environments where the physical facilities limit the size of specimens. These results should find widespread applications since almost all mechanical property testing is conducted on samples that are small relative to the size of structural components.
- The controversy surrounding the claim of permanent electrical degradation of ceramic insulators exposed to radiation that generates point defects was resolved by a series of cleverly-designed and carefully-conducted experiments in collaboration with Japan.
- The SiC/SiC composite program element has established a strong interaction with U.S. industry and other DOE programs. It has made effective use of the SBIR program.
- Significant progress has been made in developing the technology base for vanadium alloys. A commercial heat of 1200 kg of the alloy V-4Cr-4Ti has been successfully produced with impurity control and has been formed into a variety of product forms (plate, sheet, rods). Advances in both Gas Tungsten Arc (GTA) and electron beam welds have produced weldments with acceptable unirradiated DBTT.

(3) Transfer of Technology

While technology transfer is not a main thrust of this program, the extent to which its findings are adopted by the commercial section is an indicator of the quality of work. The panel identified at least four areas in which significant interaction has occurred. The steel industry has adopted many of the developments associated with the ferritic/martensitic steels and there now exists a good industrial base for these materials. Secondly, the procedure for MD calculations have been adopted by segments of the semiconductor industry to predict and model the irradiation effects associated with ion implantation during fabrication of device. The third example is the use of fracture mechanic tools to allow the extension of results from small test specimens to the prediction of the performance of large components. Finally, it was noted that the interaction with industry on the SiC composites has been extensive.

(4) Attraction of Young Professionals and University Interactions

Another indicator of quality is the ability to attract highly qualified, recent graduates to the program activities. The recent additions to the research staff have doctorates from respected universities and have outstanding records in their graduate studies. They have blended with the staff who have extensive experience in irradiation testing and alloy development to form strong teams.

University interactions include the support of doctorate research at University of California-Santa Barbara, RPI, University of Tennessee-Knoxville, Northwestern University, and Auburn University. In many cases, investigators at one of the laboratories have acted as co-advisors for the graduate study. This activity provides input and guidance for the next generation of materials scientists/engineers.

Situational Analysis

In looking ahead at recommended future goals and the balance required to achieve them, it is important to consider the Panel's assessment of the current situation for fusion energy to provide a context for our suggestions and comments. Key aspects of this situational analysis included:

- Fusion energy R&D is inherently a long-range endeavor aimed at the development of an attractive power systems concept by the middle of the next century. The results of focused materials research and development will be determinant to the realization of the potential of fusion energy as well as important to the design and construction of nearer term physics experiments.
- Work to date has illustrated the challenges presented to a first wall and blanket system by a 14 MeV neutron spectrum. System solutions such as thick liquid walls that reduce the neutron damage to the structural components might expand the performance of IFE or MFE systems. Thin liquid walls are also of interest even though they would not reduce the radiation damage significantly since they would sharply lower the thermal and fatigue loading due to the incident heat flux.
- The use of fusion power by the World's societies will ultimately be determined by its relative attractiveness to other energy alternatives. While the exact weighting of decision factors is impossible to establish at this time, it is clear that capital cost, operating expense, plant availability, worker and public safety and final decommissioning will all be considerations.
- The U.S. Fusion Energy Sciences Program has been restructured to focus on science and technology innovation while pursuing energy production technology through international collaboration.

- It is prudent to assume that the funding resources for materials R&D will continue to be constrained (as will be the overall U.S. program) at about present levels. Some modest growth may be possible. In the long-term, significantly more resources will be required to develop the materials and technology for fusion energy.
- In the near-term the materials R&D efforts will emphasize issues related to DT fusion. At the same time, the program needs to account for various possible magnetic and inertial confinement approaches. In the long-term alternate fusion fuel cycles should also be considered.
- One should anticipate continued, significant enhancement in computing capability as it relates to modeling of materials performance.

Key Issues

The materials feasibility issues for fusion energy are motivated by the long-term view of the needs for attractive, commercial power systems. From a users' perspective (e.g. electric generating companies) the major requirements are economic competitiveness, safety (e.g. no accidents which lead to public evacuation near the site), a closed tritium fuel cycle, ability to maintain the power core, reliable operation (very low number of unscheduled shut-downs per year), and no, or at least, minimal radioactive material greater than Class C waste storage. Achieving economic competitiveness implies more compact reactors which usually means higher power densities, high temperatures for high thermal efficiencies, modest component fabrication costs, low failure rates, high reliability, low unscheduled outages, and acceptable lifetimes to minimize scheduled replacements.

Unfortunately a clear materials/design path has not yet emerged that will allow DT-based fusion to become an attractive energy source with a balanced combination of the system attributes just described. The Fusion Materials Program has focused on ferritic steels, vanadium alloys and silicon carbide composites in recent years.

It is generally believed that ferritic steels represent the lowest risk in building a demonstration plant and the first commercial reactors. It has a fabrication base, a good unirradiated database, and can be provided by a number of companies worldwide. For magnetic fusion, the risk is its ferro magnetic characteristic and if that limits plasma control then it cannot be used, though this is obviously not a limitation for inertial systems. A second issue is the ductile to brittle transition temperature (DBTT). Since the DBTT temperature after irradiation is on the order of 100°C, the design would have to insure that the structural temperature is always above 100°C. Another major issue is a temperature limitation of about 550°C (creep limit) that may reduce the reactor efficiency relative to other alternatives.

Vanadium alloys represent a longer range option than ferritic alloys since there is currently no fabrication experience equivalent to ferritic steel. Currently there is only one supplier in the U.S. and one in the Russian Federation. It would take a major

investment to develop a commercial infrastructure of an equivalent level to the investment the government made in the titanium industry for application to aircraft. Before vanadium alloys can be deployed, it may be necessary to find applications other than fusion, unless fusion can invest \$100M (see vanadium assessment report done by DOE) to develop the necessary commercial infrastructure. Until this infrastructure is built, it is difficult to estimate the eventual materials costs though it is reasonable to assume they will be several times that of the ferritic alloys. The fusion designer might consider putting it on non-critical fusion components to develop confidence in the material. The main issue with vanadium is that it may be only compatible with a single coolant material (Lithium) and therefore lacks flexibility in design. The magnetohydrodynamic issues associated with using a liquid metal coolant will require the development of a workable insulator coating.

Silicon carbide composites are truly exploratory materials at this time. It is expected that they will be the most expensive of the three materials to fabricate based on current experience in the aircraft industry where composites have been difficult to economically include despite ten years of effort due to design and fabrication issues. The inherent low energy absorbing capability of the structure represents a key design hurdle. Other major challenges are the reliable transfer of mechanical loads to a primary structure and the inherent permeability of the material system. If this latter issue prevents helium being used as the coolant, it is not clear what other alternatives exist.

Therefore, perhaps the most important "key issue" for fusion materials development is the recognition that the fusion environment and needs for an attractive power system concept present a major challenge involving a wide variety of complex, interacting phenomena and conditions. This implies directly that materials R&D activities should be considered as part of an integrated program along with engineering science research, technology/component development and advanced design and systems assessments. Thus, it is important to think in terms of material systems that meet several operational requirements such as acceptable impacts on plasma performance, heat extraction at high temperatures, structural integrity under normal and off-normal condition, tritium breeding, radiation shielding, and adequate reliability, maintainability and repairability to achieve acceptable availabilities. The choice of materials to meet such requirements must result in a compatible combination of coolant, structure, and other materials as required (e.g. lithium-containing and neutron multiplication materials for tritium breeding, electric insulators, tritium barriers, etc.).

The Panel finds that the development of materials for attractive fusion power systems is a major challenge involving a wide variety of requirements, complex phenomena and conditions. It is necessary to continue to consider material systems in an integrated fashion.

Based on this finding, the Panel recommends that materials R&D activities in the U.S. Fusion Energy Sciences Program should be developed and managed as part of an integrated effort involving materials research, advanced technology and component development and advanced design activities. The approach to this integrated effort should include the development of "roadmaps" to guide program implementation and

should account for the inherent different time scales for research and design activities. We note that "advanced design" includes a variety of studies including detailed engineering efforts (e.g. ITER), conceptual reactor studies (e.g. ARIES) component evaluations (e.g. APEX) and benchmark calculations.

For almost all fusion energy concepts, including both magnetic and inertial fusion, there are systems-level issues that require the combined efforts of materials and engineering science research and component development. The key issues are:

- Identify materials and design concepts with higher power density capability.
- Identify materials and design concepts that can achieve high temperatures resulting in high thermal efficiencies.
- Identify materials and design concepts with sufficiently low failure rates, which when combined with operation and maintenance concepts for sufficiently fast change out times, that will result in an acceptably high availability.
- Identify materials and design concepts that provide for tritium self-sufficiency for deuterium/tritium concepts.
- Identify materials and design concepts that do not require public evacuation under hypothetical accidents.
- Identify materials and design concepts that minimize radioactive waste storage and disposal (e.g. meet Class C requirements and consider recycling).
- For the various materials and design concepts identified above, realistically estimate the effects of the high neutron fluxes and other factors in the operating environment on important system attributes that may degrade continuously with time, like operational limits availability, reliability, as well as ultimate component lifetimes. Other factors in the operating environment like chemical compatibility and optical properties are expected to be important to particular concepts.
- For the various materials and design concepts identified above, define criteria for establishing materials performance limits and identify those material key issues and properties of highest importance.

Many fusion power plant concepts have been studied. The authors of such studies have chosen different balances among the issues identified above. The materials research program should increase the level of understanding of materials limiting phenomena to the point that fusion energy program leaders have the information necessary to make decisions among the various options at appropriate times in the future. This understanding should come from a balanced combination of theory, computer simulation, and materials data.

Proposed magnetic fusion energy (MFE) and inertial fusion energy (IFE) power plant designs sometimes employ in-vessel components and reaction chambers that have some similar issues. Thus, the materials R&D program should study the range of data applicable to both MFE and IFE cases (e.g. increasing the range of He/dpa ratios). However, some very different materials issues exist that will be important to any determination of their feasibility. The materials research program should expand its focus to include the following:

- Working with the design community as concepts are developed, the program should assess what type of materials R&D program is required to support concepts containing thin and/or thick liquid first walls to protect structures from very high neutron wall loadings.
- The program should expand its list of materials receiving attention to include those of interest in IFE reaction chambers and innovative magnetic confinement concepts and include consideration of materials compatibility issues. Examples include copper and other alloys that may be required for high-heat flux components and/or copper magnets.
- The program should resolve what differences, if any, in material damage may exist because of the pulsed vs steady state operation of MFE concepts as well as the extreme pulsed nature of the IFE neutron flux and how best to obtain data necessary to resolve any uncertainties.
- There are a variety of special purpose material needs related to diagnostics and optical equipment for MFE and IFE. For example, the performance of the optical interface between the driver and the reaction chamber in laser driven power plants is critical and will strongly affect inertial fusion energy feasibility decisions in the next decade. The materials research program should model and measure how optical properties of candidate materials degrade in the pulsed IFE neutron environment and if and how to mitigate this degradation (e.g. high-temperature self-annealing).

Goals and Objectives

The recommendation of the Committee, based on the current situation and the key issues, is that the Fusion Materials Program adopt as its goal:

To provide the materials science knowledge base that will enable the utilization of fusion energy. The near-term emphasis will be on feasibility issues for in-vessel components for deuterium/tritium systems.

The Panel further recommends the following as the main supporting objectives of the program:

- Based on an integrated effort including materials research, advanced technology and design studies, identify candidate material systems for meeting the needs of fusion energy.
 - More fully integrate the planning and conduct of the materials R&D activities with engineering science research, technology/component development and advanced design and systems assessments. System roadmaps are useful tools for defining the feasibility questions, necessary skills and required resources.
- Provide the knowledge base on the effects of the fusion environment on materials performance.
 - For each of the three materials approaches currently under study (ferritic alloys, vanadium alloys and silicon carbide based composites), focus on the property limitations and the system level questions that could prevent an attractive design from being developed.
 - IFE concepts may require special emphasis on thick liquid walls (may also be applied to MFE) and optical materials. An assessment which will identify the key materials feasibility issues related to IFE systems is needed.
- Identify the performance-limiting phenomena and apply the materials science principles to the development of improved properties and the expansion of performance windows.
 - The combined advances in material science understanding and computing capability extend the ability to develop meaningful physically-based, semi-empirical models that can guide and help interpret experimental information. These should be applied to the key identified feasibility issues. Crosscutting concerns such as the overall effects of a combined high generation rate of gas and atomic displacements are particularly good candidates.
 - Exploratory approaches and materials should be identified and pursued if they offer greater potential to satisfy the combined requirements of performance, reliability, safety and long term waste disposal.
- Develop performance data and provide materials input for fusion design studies.
 - It is recognized that most of the extensive generation of detailed, system specific engineering design data will not be needed until such a time as the overall program is prepared to commit to an attractive energy system. It must be recognized that the development of the required information will require years to accumulate.

- Pursue fusion energy materials science and technology as a partner in the international effort.
 - It is very important to coordinate the U.S. effort with others based on the complexity of the materials challenge and the level of resources available in the U.S.

Balance of the Program

Based on the information on the U.S. and International Fusion Materials Programs that is included as Appendix 1, the Committee considered the appropriate balance between current programmatic elements in the context of the suggested goal and objectives and reached the following conclusions.

- The fusion materials program would benefit from an increase in the fraction of research related to the modeling of materials behavior.
 - The fusion materials program should maintain a focus on key issues related to in-vessel structures in a D-T fueled reactor, with significant emphasis on irradiation experiments. However, the fraction of research related to basic understanding of materials performance in the fusion environment should be increased. This increase is motivated by increased capability and sophistication of computer modeling, the need to make the most effective use of expensive and difficult-to-obtain materials data, and the desire to form stronger connections with the greater scientific community. This basic research should develop mechanistic, micro-structurally based models of irradiation effects on material properties. Such semi-empirical models can be used to evaluate, correlate and extrapolate engineering data; and to provide insight on pathways to improved materials. The modeling approaches should include direct simulation methods, like those based on molecular dynamics and Monte Carlo techniques, which are rapidly developing to link macro and meso size scales with rigorous treatments of key phenomena that occur at the atomic scale.
- In recognition of the new direction in the fusion energy sciences program, additional emphasis should be placed on developing the knowledge base for fusion materials.
 - The fusion materials research program should emphasize: a) innovative experiments to address key common and long-term issues; b) assessment of information to provide the best estimates of stress-temperature-displacements per atoms (dpa) -corrosion limits that can be systematically refined and improved with continuing research; c) increasingly reliable property predictions; and d) mechanism based approaches to improved materials. In the near and intermediate term, development of this knowledge base will primarily rely on intermediate dose fission reactor irradiation experiments coupled with an expanded basic research and

modeling effort. This effort should be coupled with the recommended increased efforts to develop micro-mechanical predictive models.

- An increase in involvement with integrated component modeling would be beneficial to the fusion materials program.
 - There is a great opportunity to develop and apply modern computational tools to engineering design, analysis and simulation of in-vessel components. Such studies would benefit the materials research program by sharpening the understanding of performance requirements and promoting the development of advanced design methods needed to assure structural integrity without undue conservatism. These studies will also lead to major improvements in the engineering science underlying advanced in-vessel designs. While the fusion materials program should not take the lead in these studies, it should increase its participation in these activities.
- An increased emphasis should be placed on resolving the key feasibility issues raised by each materials systems in conjunction with other parts of the Fusion Program. Examples for each of the three materials systems which are currently under some level of development include:
 - Ferritic Steels
 - * The suitability of the application of ferromagnetic materials for in-vessel components in magnetic confinement devices (in collaboration with researchers outside the materials program and with some emphasis on Tokamaks). This should include an analysis of the perturbations of the device's magnetic fields and issues of dynamic control of the plasma.
 - * The effect of irradiation, including the influence of helium, on fracture toughness, ductility and constitutive properties to better determine performance constraints over the temperature range of interest.
 - * Alloy development and design concepts resulting in higher, maximum operating temperatures in a fusion irradiation environment.
 - Vanadium Alloys
 - * The viability of electric insulators for candidate liquid metal/vanadium systems and the suitability of coolants other than liquid lithium and possible designs to accommodate the chemical reactivity of lithium (in collaboration with researchers outside the materials program).

- * The effect of irradiation, including the influence of helium, on fracture toughness, ductility and constitutive properties to better determine performance constraints over the temperature range of interest.
- * High temperature limitations with particular emphasis on the role of helium in creep and creep rupture and alloy development strategies to expand these limits.

- SiC/SiC Composites

- * Structural joining methods and identification of properties and methods for designing thermal-mechanically loaded, inherently brittle structures.
- * Irradiation effects on thermal conductivity, the stability of fibers and fiber coatings, including the effects of transmutations.
- * Coatings and claddings to provide adequate hermeticity for helium coolants.

- Opportunities for new and innovative approaches to fusion materials research should be expanded.

- The motivation for new approaches is illustrated by Table 1, which shows the results of initial calculations done for the APEX study. The allowable neutron loads are sensitive functions of the assumed fraction of the charged particle energy that reaches the first wall, the thickness of the wall, the coolant temperature and the results from detailed analysis of the specific design. While, therefore, the absolute estimates of allowable neutron wall loadings would be expected to change in other studies, the general trends would be expected to be maintained. The preliminary APEX calculations shown in Table 1 indicate the possible limitations for any of the current candidate materials in allowable neutron wall loading relative to the level of 7 MW/m^2 , which is viewed as an attractive design target. Future inclusion of materials which had previously been eliminated based on long term activation level considerations alone may open up the system design window. These exploratory efforts should consider the needs of innovative magnetic confinement concepts as well as the needs of inertial fusion energy.

- Therefore the introduction of new ideas and people to the fusion materials program should be encouraged. Modest increases in funding and a yearly opportunity for competitive, peer reviewed proposals are possible mechanisms to support such renewal. Innovative approaches to promoting sustained and mutually beneficial collaborations between laboratories, universities and industry should be developed.

Material (Interface Temperature)	Allowable Peak Neutron Wall Loading (MW/m ²)
Ferritic Steel (500°C)	1.5
V-Cr-Ti (600°C)	3.2
SiC-SiC (700°C)	2.5
Oxide-Dispersion Strengthened Ferritic (ODS) (600°C)	2.6
Nb1Zr (700°C)	6.6
Tungsten (800°C)	8.8
TZM (800°C)	13
T111 (800°C)	11.6

- Peer review should be expanded along with the use of well-defined measures of quality and progress towards program goals.
 - Standards of quality, performance and progress towards program goals should be more fully developed and utilized at both universities and national laboratories. These metrics should be used to facilitate an ordering of programmatic priorities toward the more promising new developments in material systems. Periodic expert peer reviews in the context of these metrics should be supported. The review panels should include leading materials scientists from outside the program.

Specialized Facilities for Materials Research and Development

As part of the overall charge, the panel was asked to review the program efforts aimed at a fusion neutron source test facility including US involvement in the international fusion material irradiation facility (IFMIF). The Panel also considered the general topic of specialized neutron irradiation facilities for materials research and development.

The fusion environment in which the materials of the in-vessel system have to function is complex and arguably more challenging than faced by any other potential power generation concept.

*Information taken from "Exploring Novel High Power Density Concepts for Attractive Fusion Reactors," APEX Studies, M. Abdou, et al.

The time dependent change in mechanical properties and dimensional stability of candidate structural materials that result from the high rate of creation of both point defects and gas atoms increase the difficulty of designing a structure that can reliably sustain the primary and secondary loads imposed by high heat fluxes, coolant circulation and magnetic forces. In addition, there are complex thermal/ chemical/ mechanical/electrical/magnetic interactions that must be accommodated while the system converts the nuclear energy to a useable form of heat and breeds the tritium required by the fuel cycle.

In a realistic R&D program, particularly for fusion where no appropriate facilities now exist, tests proceed from simple measurements to more complex prototypes in order to reduce cost and facilitate understanding of basic effects and phenomena. Generally, tests can be classified into: basic single effect, multiple effect/multiple interactions, and integrated tests.

The structural material program has focused on: 1) basic property measurements on non-neutron test stands, 2) exploring radiation effects on structural materials. The only available irradiation facilities are fission reactors. These have been used extensively by the US materials program as well as by the rest of the world (including numerous international collaborations).

While fission reactors generally have relatively large volume and high neutron flux, the neutron spectra are much softer than those encountered at the first wall in "dry-wall" design concepts. In order to better simulate fusion neutron damage relevant parameters such as the helium-to-dpa ratio, special techniques such as helium charging and boron and nickel doping were used. The program should investigate a range of helium/dpa levels between 0.1 and 15 to encompass the various design possibilities.

In the absence of a test facility with a prototypical fusion environment, fission reactors will remain the primary test facilities for fusion material irradiation. The value of fission reactor irradiation can be enhanced considerably by: 1) continued emphasis on innovative techniques to better simulate helium, hydrogen and other transmutation rates, and 2) more emphasis on modeling to better understand and extrapolate radiation damage results to those expected in fusion spectra.

An additional possibility considered was a spallation neutron source such as is planned to be built at Oak Ridge (Appendix 2). Unfortunately it has been concluded by other Review Panels (Reference 8 of Appendix 2) that the pulse nature of the beam, the uncertain He/dpa ratio resulting from the high energy tail of the neutron spectrum and the low rate of dpa generation rate minimizes the usefulness of spallation sources to fusion materials research.

It should be noted that current designs for in-vessel components can be classified into: a) "evolutionary" and b) "revolutionary" concepts. The "evolutionary" concepts have a "dry" solid first wall. They are based on: 1) self-cooled liquid metal breeder or 2)

ceramic solid breeders cooled by helium (or water). These "evolutionary" concepts are relatively better understood because they have been the focus of the world R&D program for over 20 years. In these concepts, the "dry" solid first wall is exposed to an intense neutron flux that includes a large high energy (14 MeV) neutron component. The helium-to-dpa ratio is typically in the range of about 4 to 15 for vanadium alloys and ferritic steels, depending on breeder, coolant, and design. Helium production in silicon carbide is typically an order of magnitude higher than in steels. Deeper into the blanket, this ratio drops rather rapidly because neutron-induced helium production reactions have a typically high energy threshold. So, fission reactor simulation becomes increasingly more relevant in the deeper regions behind the first wall.

In a "revolutionary" or exploratory concept for magnetic and inertial fusion, liquid layers flow in front of the first wall. Such liquid layers will remove the surface heat flux and reduce thermal stresses in the first wall. If these liquid layers are thick, the neutron flux at the first wall changes in two ways: a) reduction in magnitude and hence in the neutron irradiation dose for the first wall and b) softening the spectrum. A reduction in both the flux and the helium-to-dpa ratio of more than an order of magnitude is predicted in some recent, but yet unproven, designs. Recent work on these "revolutionary" designs is motivated by the desire to increase the power density (high neutron wall loading) capability in fusion systems. But they also appear to reduce the radiation damage problem in the first wall to a more manageable level. Another important observation is that fission reactor irradiation becomes more relevant, and in some cases ideal, for simulation of the radiation environment in the structural materials of these "revolutionary" concepts.

The fusion program has long considered the need for specialized facilities for neutron irradiation of materials as well as other facilities for the R&D needs of fusion technology and material systems.

The Panel received a presentation on the results of the Conceptual Design Activity (CDA) of an accelerator-based neutron source, called IFMIF. The CDA was an international collaboration that involved Europe, Japan, USA, and Russia and it was concluded in December 1996. The IFMIF-CDA design would provide a testing volume in excess of 100 liters over a wide range of neutron flux. About 0.5 liter of test volume is at a neutron flux equivalent greater than 2 MW/m². This high flux region is for post irradiation tests of miniaturized specimens of first wall and blanket structural materials. Additional test volume of more than 5 liters will provide a neutron flux ranging from 2 MW/m² down to 0.1 MW/m². In addition, more than 100 liters of volume is available at low flux below 0.1 MW/m². This volume at low flux conditions may be useful to test a variety of different materials including fully instrumented specimens under various loading conditions. The total estimated capital cost of IFMIF is about \$800M.

The Panel did not make a technical assessment of the merits of an accelerator-based neutron test facility. Nor did the Panel review the details of the CDA design. Because of the current fusion budget situation, there are considerable uncertainties in the decision to construct IFMIF. This budget situation is further complicated by the delay in the decision to construct ITER.

Given such uncertainties, the Panel recommends that the Program continue to develop a knowledge-base on materials properties and system feasibility issues that does not assume that any non-reactor irradiation facilities are available over the next decade. If funding for a materials test facility becomes available, the panel believes that a review of the relative merits of different materials and blanket test facility concepts should be made at that time considering the multitude of needs of the entire fusion technology program. Both plasma-based volumetric and accelerator systems should be considered.

At this time the U.S. materials program funds the accelerator-based IFMIF activity at the low level of \$300K. The Committee recommends that continued support for this activity be considered in the context of international activities on the subject.

To support the recommended goal of establishing the feasibility of one or more in-vessel materials systems, it is noted that the appropriate gas/dpa ratios and levels can most easily be approximated in fission reactors for ferritic steels which commonly include small levels of nickel.* The Committee, therefore, suggests that the Program consider as one possibility a strategy which emphasizes the development of ferritics for the design of the first DT power producing device which in turn would be used to evaluate more revolutionary, ultimately more attractive materials. At that point in the maturation of fusion energy, a separate construction of a dedicated accelerator or plasma-based source to achieve high fluence irradiation of materials to support future commercial reactor designs would likely be warranted.

Resource Allocation Perspective

The current Fusion Materials Program is funded at about an annual budget of \$6M. The activities that are covered by these resources are currently focused on advanced ferritic alloys (~25%), vanadium alloys (~50%), and SiC composites (~25%) but with smaller efforts on modeling, neutron source studies and insulating ceramics. These funding levels must be considered in the context of the world-wide effort that is shown in Appendix 1. For the total international effort, roughly 45% is on ferritic alloys, 14% on silicon carbide composites and 22% on vanadium alloys. The Panel felt that the U.S. effort should continue to remain involved with the ferritic alloys program while exploring the potential for vanadium alloys and silicon carbide composites. The Panel believes that some enhancement is needed in modeling and materials to support exploratory concepts for both MFE and IFE that are emerging in the restructured Technology Program. It is recommended that a modest increase be made in the Fusion Materials Budget of 1 to 2 million dollars per year to support these new initiatives.

* The presence of nickel allows the high levels of helium expected in a DT fusion machine to be generated in a fission reactor.

Additional modeling and knowledge-base development support should be sought from outside the fusion materials program (such as combined efforts with the BES materials program, the ER strategic simulation initiative and the Stockpile Stewardship materials modeling effort.

At the modest level of the current base program, there are few resources available beyond what are needed to address the identified materials system feasibility issues. However, if a flat materials budget is necessary, then the Panel recommends that the SiC program be reduced and, if necessary, a smaller reduction of the vanadium program to allow the recommended initiatives in modeling and exploratory approaches. This recommendation on funding priorities is based on the following rationale:

- there are serious issues related to the ability to fabricate large complex components out of an inherently brittle material like SiC composites for use in the fusion environment even if the irradiation performance problems are solved
- the vanadium program is the largest U.S. program and would therefore be the least impacted by the modest funding reduction
- the level of funding for advanced ferritics is the minimum necessary to leverage the larger programs in the EU and Japan

Attachment 1

FESAC Materials Review Committee Members

Abdou, Mohamed A.

University of California, Los Angeles
Mechanical, Aerospace & Nuclear Eng. Dept.

Baker, Charles C. (Co-Chair)

University of California, San Diego
School of Engineering

Davis, John W.

The Boeing Company

Harkness, Samuel D. (Co-Chair)

Westinghouse Electric Company
Science and Technology Center

Hogan, William

Lawrence Livermore National Laboratory

Kulcinski, Gerald (Prof.)

University of Wisconsin, Fusion Tech. Institute

Mauel, Michael

Columbia University, Applied Physics

McHargue, Carl (Dr.)

University of Tennessee

Odette, Robert

University of California, Santa Barbara

Petti, David A.

Idaho National Engineering Laboratory
Fusion Safety Program

Shewmon, Paul

Ohio State University (Retired)

Zweben, Stewart J.

Princeton University, Plasma Physics Laboratory

Attachment 2

Agenda for FESAC Review of the Fusion Materials Monday, March 2, 1998

8:00 a.m. Welcome and Introductions
Sam Harkness (W) and Charlie Baker (UCSD)

CURRENT PROGRAM REVIEWS

8:45 DOE Perspective on Overall National Program Strategy Budget
Environment Program Interactions and Coordination, Management
Issues
Mike Roberts (DOE)

9:15 Overview of the Materials Program - Goals, Strategy, Tasks,
Institutions, International Collaborations, Overview of Foreign
Research Programs, Interfaces and Synergism with Other Material
Programs (BES, EE, NE, NRC, etc.)
Bill Wiffen (DOE)

10:00 Discussion of Current DOE Program

10:15 Break

RESEARCH NEEDS

10:30 MFE Reactor Perspective on Materials Needs, Definition of Direction
from the Advanced Design Studies
Mike Billone (ANL) and Mark Tillack (UCSD)

11:15 IFE Reactor Perspective on Materials Needs
Ralph Moir (LLNL)

11:45 Lunch - (Rooms 401-2C6 and 401-2C14)

12:30 Fusion Safety and Environmental Perspective of Material Issues/
Perspective on Disposal Requirements for Activated Materials
Kathy McCarthy (INEL)

1:00 Discussion of Research Needs

RESEARCH PROGRAMS - CURRENT STATUS

- 1:30** **Strategy for Materials Development, Including Facilities, Techniques and Neutron Sources for Fusion Materials Development**
 Everett Bloom (ORNL)
- 2:15** **Radiation Effects in Fusion Materials, Including Modeling of the Radiation Damage Process**
 Steve Zinkle (ORNL)
- 3:00** **Progress and Future of Modeling Radiation Effects in Materials**
 Bill Wolfer (LLNL)
- 3:30** **Vanadium Alloys**
 Dale Smith (ANL)
- 4:15** **Ferritic/Martensitic Steels**
 Ron Klueh (ORNL)
- 4:45** **SiC/SiC Composites**
 Russ Jones (PNL)
- 5:15** **IFMIF - The IEA Conceptual Design of an Accelerator Based Neutron Source**
 Tom Shannon (UTK)
- 5:45** **Depart for Dinner – (D’Imperio’s)**

Tuesday, March 3, 1998

- 8:00 a.m.** **IFE Requirements for Optical Systems**
 Steve Payne (LLNL)
- 8:30** **Summary of Research Programs**
 Arthur Rowcliffe (ORNL)
- 9:00** **Discussion of Review Approach; Outline and Schedule**
 Sam Harkness (W) and Charlie Baker (UCSD)
- 9:30** **Break**

- 9:45** **A Perspective on Design Requirements for Fusion Systems**
Mohamed Abdou (UCLA)
- 10:15** **Goals for the Fusion Structural Materials Program**
Group Discussion
- 11:00** **Breakout Sessions for Sub-Panel Groups**
(Rooms 401-2C3; 401-2C7; 401-2C10)
- **Scientific Quality of Current Program**
Carl McHargue (UTK), Coordinator

 - **Recommendations on Balance of the Materials Research Program to Support the Qualification Requirement**
 - **Special Requirements for Ferritic, Vanadium and Other Candidate Systems**
 - **Component vs. Material Testing Needs**
 - **Necessary Mix of Research Performers**
 - **Involvement of International Efforts**
Bob Odette (UCSB), Coordinator

 - **Required Specialized Facilities**
Mohamed Abdou (UCLA), Coordinator
- 12:00** **Lunch – (Rooms 401-2C6 and 401-2C14)**
- 1:30** **Report of Planning Efforts**
- 2:30** **Summary**
- 3:00** **Depart**

Attachment 3

FESAC Meeting Agenda
Thursday, April 16, 1998

- 8:30 a.m. Introduction and Welcome**
S. D. Harkness
- 8:45 Review and Discussion of Goals Statement**
C. Baker
- 9:30 Review of "Scientific Excellence" Section**
C. McHargue
- 10:15 Review of "Balance" Section**
R. Odette
- 11:15 Review of "Facilities" Section**
M. Abdou
- 12:00 Lunch**
- 1:00 Break-out Groups**
- Outline of Report Section
- Writing Assignments
- First Drafts (if possible)
- 6:00 No Host Dinner, place to be announced**

Friday, April 17, 1998

- 8:30 a.m. Review of Revised Goals Statement**
- 9:00 Review of "Scientific Excellence" Progress**
- 10:00 Review of "Balance" Progress**
- 11:00 Review of "Facilities" Progress**
- 11:30 Plan for Next Actions**
- 12:00 Adjourn**

Appendix 1: Review of Programmatic Elements of the US Fusion Materials Program

The Committee was presented with a review of the current technical status of the fusion materials program including a budget breakdown of the current programmatic elements, a comparison of the US and foreign programs, and an overview of how the fusion program fits within the context of other US materials R&D.

The US fusion materials program currently is focused on three material systems: vanadium alloys, advanced ferritic alloys, and SiC composites. These materials were chosen based on a combination of performance potential, low activation (safety and environmental concerns), and technical feasibility as a material in a fusion power system. The degree to which each material system satisfies these criteria differs and current R&D is focused on solving radiation-related issues associated with each material system. Table 1 presents some of the pros and cons for each material along with the critical development issues for each material system.

Table 2 compares the budget of the US and foreign materials programs and the distribution of that budget across the different alloys. The table indicates that the US spends ~ 50% of their ~ \$6 M budget on vanadium alloys, and 25% on SiC composites and 25% on ferritic/martensitic steels. This is quite different than in the EU, where 90% of the effort is on ferritic/martensitic steels. In Japan about 40% is spent on ferritic/martensitic steels, with roughly equal amounts on SiC composites, vanadium alloys, and other materials. Russia spends about 40% of their budget on vanadium alloys and 45% on other materials. Very little is spent on ferritic/martensitic steels and SiC composites.

The US augments its program by two bilateral agreements with Japan and Russia. Funds are received from Japan (~ \$2.8 M/year) to support collaborations using US facilities. Funds go to Russian for access to their liquid metal fast spectrum fission reactor irradiation facilities. International steering committees coordinate the bilateral agreements and additional international coordination is probably the executive committee for the Fusion materials agreement of the IEA.

Table 3 presents the distribution of the current US effort among the different materials technology development areas. Irradiation testing and evaluation makes up between 60 and 85% of the total expenditures for each of the three materials (V alloys, SiC composites, ferritic/martensitic steels). Significant effort is devoted to baseline property development and other critical materials technology areas if warranted by the material (e.g., welding Vanadium alloys).

The linkages that the three major fusion materials development laboratories (ANL, ORNL, PNNL) have with other materials programs is shown in Table 4. A check mark indicates that there is sharing of staff and facilities between those programs and the OFES fusion materials program. By the number of check marks in the table, there is a broad range of interaction at the working level between these programs. The committee feels that this list is impressive and that the leverage currently afforded by the linkages to these programs is quite high.

Table 1 Comparison of candidate fusion material systems

Vanadium Alloys	Pros	<ul style="list-style-type: none"> • Shows evidence of radiation resistance • Low activation if impurities are controlled • Offers temperatures of 650°C or higher
	Cons	<ul style="list-style-type: none"> • May only be limited to use with liquid metal coolant • Currently a small volume technology
	Technical Feasibility Issues	<ul style="list-style-type: none"> • Use with other coolants like He? • Viability of insulator coatings for Li/V system • Effect of irradiation including influence of He on fracture toughness, ductility etc. • High temperature limits with emphasis on role of helium in creep and creep rupture
Advanced Ferritic Alloys	Pros	<ul style="list-style-type: none"> • Established technology • Reduced activation compositions match establish alloys behavior • Shows high fluence resistance to irradiation
	Cons	<ul style="list-style-type: none"> • Fracture toughness • Temperature may be limited to 550°C
	Technical Feasibility Issues	<ul style="list-style-type: none"> • Use of ferromagnetic materials for in-vessel components in magnetic fusion • The effect of irradiation, including the influence of helium, on fracture toughness, ductility and constitutive properties • Alloy development and design concepts resulting in higher, maximum operating temperatures in a fusion irradiation environment.
SiC/SiC Composites	Pros	<ul style="list-style-type: none"> • Offer potential environmental and safety advantages • Offer temperature capability to 950°C
	Cons	<ul style="list-style-type: none"> • Unacceptable irradiation damage to first generation SiC fibers • May only be viable with He coolant
	Technical Feasibility Issues	<ul style="list-style-type: none"> • Structural joining methods • Methods for designing thermal-mechanically loaded, inherently brittle structures. • Irradiation effects on thermal conductivity, the stability of fibers and fiber coatings, including the effects of transmutations. • Coatings and claddings to provide adequate hermiticity for helium coolants.

Table 2. Comparison of US and Foreign Fusion Materials Programs

Party					
	US	EU	Japan	Russia	TOTAL
Budget	~\$6 M (+\$0.5 M on Cu and SS for ITER)/15 FTEs	10 MECU/40? FTEs	61 FTEs	30? FTEs	
Distribution by Alloy (%) / FTT					
Ferritic/Martensitic	23/3	90/36	38/23	10/3	45/65
SiC/SiC Composites	26/4	6/2	23/14	5/1	14/21
Vanadium Alloys	50/7	2/1	20/12	40/12	22/32
Other	1/1	2/1	19/12	45/14	19/28
					100/146

Table 3. Distribution of US efforts among different materials technology development areas

	Vanadium alloy	SiC Composites	F/M Steels
Fabrication and Baseline Properties	19	7	15
Welding/Joining	10	5	2
Corrosion, Compatibility	11	0	0
Test Methods Development	0	5	0
Irradiation Effects Testing and Evaluation	39	13	48
Irradiation Experiments and Facilities Operation	21	70	35
Total	100	100	100

Table 4. Linkages between fusion materials development laboratories and other US materials programs.

Program	ANL	ORNL	PNNL
Other Fusion Work (e.g. blanket)	√		
DOE-BES (e.g., radiation effects, microscopy, alloy development, x-ray research)	√	√	√
DOE-Fossil Energy (e.g. SiC composite development)	√	√	√
DOE-EE (high temperature materials)		√	
NRC (irradiation effects)	√	√	√
DOE-Accelerator Production of Tritium	√	√	√
DOE - Naval Reactor	√		√
DOE - EM			√
DOE Joining Program	√		√
LWR programs (EPRI, Japan)	√		√

Appendix 2: Options for Accelerator-based Neutron Sources for Fusion Materials Work Sources for Fusion Materials Work*

Introduction

One of the major materials issues to be faced in developing attractive fusion power is the effect of the intense neutron fluxes associated with DT fusion concepts. The first-wall neutron spectrum which contains a large 14 MeV component not only results in very high displacement rates (~ 20 -dpa/yr at 2MW/m²) but also causes much higher transmutation rates than is experienced in fission reactors. The elements He and H are of particular concern, but other impurities can also be important. The influence of these transmutation products on property changes has been very well established, the obvious example being the role of He in swelling behavior.

While fission reactors are likely to provide the bulk of irradiation facilities for the foreseeable future, accelerator-based neutron sources have been proposed to provide the spectral response, high fluxes, and high fluences needed for the investigation of fusion materials. Such a source would be needed to validate and extrapolate the results from fission irradiations to fusion conditions, for materials development, and to provide design data for future high-fluence fusion devices (e.g., DEMO). Until suitable materials are developed in this way, the technology to design and build fusion-based facilities will not exist.

Accelerator/target options

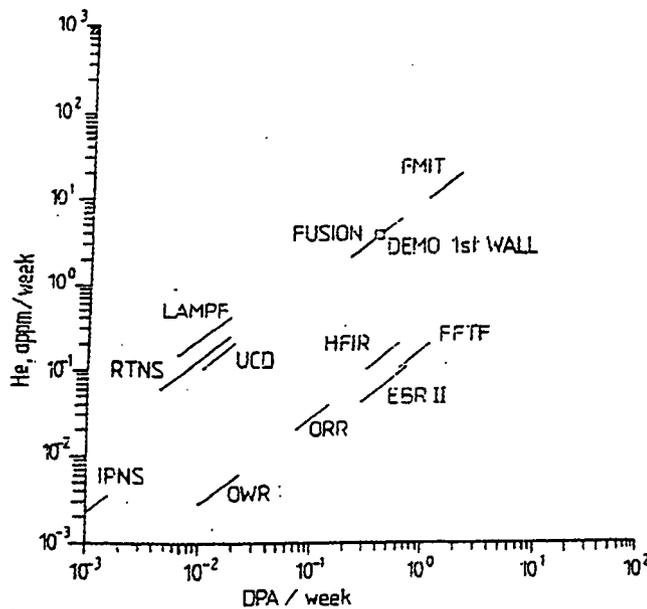
D-T sources based on beams colliding with solid or gaseous targets, e.g., RTNS II. Such sources produce essentially mono-energetic 14 MeV neutrons, and because of target heating and lifetime issues are generally limited to low fluxes (0.01-1 dpa/yr) and fluences, and small volumes. A 1970's project (the Intense Neutron Source)⁽¹⁾ would have allowed fluxes up to 10 dpa/yr but with a usable volume of only a few mm³.

Stripping sources based on the interaction of 30-40 MeV deuterons with low-Z targets.⁽²⁾ Such systems provide neutron spectra which approximately match those of a fusion reactor as measured by transmutation rates (particularly He production) to dpa ratios. Credible designs exist which would provide fluxes > 20 dpa/yr using existing accelerator technology (beam-powers ~ 5 -10 MW).⁽³⁾ Irradiation volumes at these fluxes would be about 0.5 liters, with more available at lower fluxes. Such designs do provide for steady-state operation, with intrinsic time structure periods in the range of a few nanoseconds or less, well below the range of concern for radiation damage effects.⁽⁴⁾ There are two intrinsic issues with the D-Li sources. First the existence of a high energy tail in the neutrons ($E_n \sim 20$ -50 MeV) for which essentially no neutron cross-section data exist. This causes some uncertainty in calculated transmutation rates. Second is the relatively small volume available at high flux/fluence. Both these issues have been judged to lead to acceptable levels of compromise.⁽⁵⁾

*Appendix 2 was prepared by Dr. M. Saltmarsh of Oak Ridge National Lab.

Spallation sources which use high energy (~1 GeV) protons on a thick high-Z target. Spallation sources are the most efficient way to produce neutrons with an accelerator, in terms of neutrons/unit of beam energy. The target volume is intrinsically larger than in the D-Li case, leading to larger irradiation volumes, but smaller peak fluxes for the same beam power. The neutron spectra contain a high energy tail extending all the way to the incident beam energy which creates a substantial uncertainty in calculating most transmutation rates. As measured in terms of the He/dpa ratio, the spectrum is a closer match to fusion than to fission, as can be seen in Figure 1 which compares a variety of sources including two spallation sources (LAMPF and IPNS).⁽⁶⁾ Taking LAMPF as indicative of modern spallation sources, the He/dpa ratio is seen to be high by a factor of two or so while the attainable fluences are too low by a factor ~50. Furthermore the intrinsic time structure of the beam (pulses every 8.3 ms) is likely to effect the radiation response in some materials, particularly at elevated temperatures. He/dpa ratios closer to the fusion value can be attained by moderating the neutrons, but this tends to lower the available fluxes.⁽⁷⁾

Comparison of the weekly DPA and helium production rates in iron for various irradiation facilities



Original data from Greenwood et al., ANL/FPP/TM-197

An evaluation committee chaired by D. Doran concluded, 'There is no sustained mission for LAMPF in the metals and alloy segment of the fusion materials program; the damage rates are too low, even with a significant upgrade, and there is a question of pulse effects at elevated irradiation temperatures. If damage rates were raised to 3 to 5 dpa/yr, useful mechanical property data might be obtained in the 40 to 360°C temperature range; however, a research program to evaluate effects of the higher energy tail would have to precede heavy program use.'⁽⁸⁾

Similar considerations apply to use of the proposed ORNL Spallation Neutron Source (SNS), which would be a machine somewhat more powerful than LAMPF. It will be designed for an average beam power of 1 MW, as compared to LAMPF's nominal maximum power rating of 0.8 MW. The highest displacement rates due to neutrons would be on the stainless steel jacket surrounding the mercury target and are estimated to be in the range of 0.1-2.5 dpa/yr.⁽⁹⁾ To serve the primary mission of neutron scattering, the beam time structure is planned to be microsecond pulses at 50 Hz, unsuited to studies of magnetic fusion structural materials.

While spallation sources have been judged unsuitable for examining fusion specific issues in metals and alloys, there is a potential mission for basic studies of radiation effects, and for fusion-specific issues in some other materials, particularly where the pulsed nature of the beams can be put to advantage. These were examined and discussed in reference 8.

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- 4) E. H. Lee et al, "Effects of Rapidly Pulsed Ion Bombardment on Microstructure and Phase Stability in a Ti-Modified Stainless Steel," Nucl. Inst. & Meth. B16, 1986, 251
- 5) IEA Working Group report, Karlsruhe, 1992
- 6) D. E. Greenwood et al., ANL/FPP/TM-197
- 7) D. G. Doran et al., Journal of Nuclear Materials 174, 125-134, 1990
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OAK RIDGE, TN 37831-8070

May 28, 1998

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

Dear Dr. Krebs:

The Fusion Energy Sciences Advisory Committee (FESAC) heard a presentation on the draft report from the Materials Program Review Panel, which has responded to your charge of January 14, 1998.

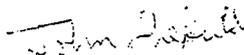
The draft report makes some very important recommendations about the fusion structural-materials program.

- * There should be a greater emphasis on modelling of materials, using the growing capabilities in this area, to complement the experimental program in developing optimized materials. This area is a very good candidate for inclusion in the Strategic Simulation Initiatives.
- * More emphasis should be given to understanding the complete systems into which the structural materials must fit to help in prioritizing the elements of the development program.

The FESAC agrees with these recommendations.

The FESAC has requested that the Review Panel consider certain modifications and add some clarifying sections to the draft report. The FESAC will review the final report in its July 30-31 meeting.

Sincerely,



John Sheffield, Chair
on behalf of the Fusion Energy
Science Advisory Committee

Enclosures

cc: N. A. Davies, DOE-OFES
J. Decker, DOE-ER
A. Opdenaker, DOE-OFES
FESAC Committee
J. Galambos, ORNL
W. Houlberg, ORNL



Department of Energy

Washington, DC 20585

January 14, 1998

Dr. John Sheffield, Chair
Fusion Energy Sciences Advisory Committee
Energy Technology Programs
Oak Ridge National Laboratory
Bethel Valley Road
Oak Ridge, TN 37831

Dear Dr. Sheffield:

This letter provides a charge to review a specific portion of the Office of Fusion Energy Sciences (OFES) program -- our Materials Research Program. The OFES materials program was last reviewed by Panel 6 of FEAC, with FEAC reporting in April 1993 on the Neutron Interactive Materials Program. With the recent restructuring efforts conducted throughout most of the program, now including the interim FESAC report on ITER participation, the materials activities represent a principal program element remaining to be considered from this new point of view.

This review is focused on the Materials Research Program, (a separately defined budget line) which is comprised principally of the low activation structural materials research and our efforts aimed at the associated neutron source test facility. The applications of these low activation structural materials, e.g., blankets and first walls, should be considered to provide the context for this review.

Please address these topics:

1. goals for this element of our program,
2. scientific quality of the work,
3. balance in the various dimensions of the materials program, e.g., analysis, theory and modeling vs. empirical approaches for predicting performance; engineering component vs. material sample approach to testing; and single issue focus such as irradiation vs. systems approach with multiple environmental conditions such as corrosion and compatibility as well as irradiation,
4. mix of research performers,
5. balance of effort on candidate materials systems,
6. coordination, collaboration and balance among domestic and international participants, including our involvement in the international fusion materials irradiation facility activities, and
7. outreach activities.

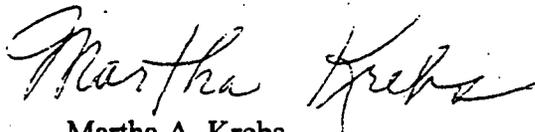
The FESAC review should provide both an evaluation of the program strategy (including scope and priorities for future work) and a technical review/evaluation of the ongoing research in this program element.

We are organizing an overview presentation on the materials program for the January 22 FESAC meeting, as was noted at the last meeting. After this date, members of OFES and materials program researchers will be prepared to present to FESAC or a FESAC panel the details of the current program goals, approach, and content.

I would like to have the FESAC evaluations and recommendations on the fusion materials program by May 15, 1998. This advice will be important for planning and decisions on future work on materials in FY 1999 and later years.

I appreciate the time and energy devoted by the members of FESAC and the FESAC panels to this continuing effort to evaluate, orient and improve the OFES program. I will look forward to hearing the Committee's recommendations on the fusion materials research activities.

Sincerely,

A handwritten signature in cursive script that reads "Martha Krebs".

Martha A. Krebs

Director

Office of Energy Research

Section 2

August 3, 1998

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

Dear Dr. Krebs:

The FESAC met on July 30-31 and considered the charge in your letter of July 29, 1998, regarding the recent peer reviews of three proposed proof-of-principle (PoP) programs. You asked FESAC's advice on how to incorporate such new and expanded programs into the Fusion Energy Sciences program, given the current fiscal constraints, commenting that a complete answer to the question requires an in-depth review of the entire fusion program.

The FESAC is encouraged by the exciting possibilities embodied in these proposals. The diversity of scientific work among the proposed programs is precisely what was envisioned when the recommendations for restructuring the program were made. The peer review process, using a single review committee, and the iterative approach, allowing the proposers to provide written responses, were particularly informative.

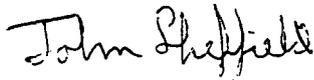
As anticipated in your charge letter, the FESAC is not able to make specific recommendations regarding these proposals without an in-depth review of the entire Fusion Energy Science program. In this situation, the FESAC recommends that the DOE provide sufficient support in FY 1999 to allow all three innovative programs to maintain their momentum and be ready in FY 2000 for implementation, if selected in the broader program review proposed for the next fiscal year.

The FESAC strongly supports the DOE proposal to conduct a study of the whole Fusion Energy Sciences program, which would provide a basis for addressing such opportunities. The DOE also proposes to undertake a number of other reviews of the Fusion Energy Sciences program in FY 1999. In support of all these reviews, we recommend strongly that the OFES commission a data gathering exercise on the various opportunities for on-going and proposed research, to meet the goals identified in the restructured program, that could be undertaken in the future Fusion Energy Sciences program. In addition, a synthesis document should be written to show how each potential opportunity fits the goals. An excellent model for such documents is the recent report 'Technology Opportunities to Reduce U.S. Greenhouse Gas Emissions' (http://www.ornl.gov/climate_change). In parallel with this exercise, the OFES should prepare a list of the objectives and criteria for judging all the major areas of the program. Useful contributions to this list are in the FEAC restructuring report of January 27, 1996, and in the FESAC report of July 22, 1996, 'Advice and Recommendations to the U.S. Department of Energy'.

Dr. Martha A. Krebs
Page 2
August 3, 1998

The discussions in our meeting highlighted the exciting progress and opportunities for scientific advances in the Fusion Energy Sciences program. In addition to learning about the three innovative PoP proposals, we heard about major opportunities for computer modeling and development of advanced materials. We also heard about significant progress on the DIII-D in developing the physics basis for advanced tokamaks. Finally, we are aware of the major advances in the inertial fusion program which has resulted in supportive language from the Congressional committees. We strongly encourage the DOE to provide a level of funding in the FY 2000 and subsequent budgets to allow the Fusion Energy Sciences program to take advantage of these important scientific opportunities.

Sincerely,



John Sheffield, Chair
on behalf of the Fusion Energy
Science Advisory Committee

Enclosures

cc: N. A. Davies, DOE-OFES
J. Decker, DOE-ER
A. Opdenaker, DOE-OFES
FESAC Committee
J. Galambos, ORNL

Consensus technical summary of the members of the review panel for Proof-of-Principle Proposals in Fusion Energy Science, June 8-11, 1998

Stellarator Proof of Principle (PoP) Program Proposal

1. Summary

The panel members conclude that the stellarator community is ready for a PoP program with a lead experiment based on the "quasi-axisymmetric (QA) stellarator," which is a concept based on a new direction, rather than a refinement of more standard directions. The members are concerned that the cost of the proposal is high in the context of the Fusion Energy Program and that the construction time for the lead experiment is long enough to slow down progress on the concept.

This lead experiment will focus on: (1) the role and usefulness of bootstrap currents in this version of the compact stellarator ($A = 3-4$); (2) beta limits; (3) the avoidance of disruptions; (4) demonstration of the control of neoclassical transport by proper configuration design; (5) control of turbulent transport, e.g. using enhanced confinement techniques ("transport barriers") developed in the tokamak program; and (6) the role of bootstrap current and magnetic shear in suppressing or enhancing magnetic islands and tearing modes. The closeness to standard tokamak operation of the main concept gives the expectation of achieving a high quality plasma in a new configuration a high probability of being achieved.

In endorsing the technical merit of the proposed lead experiment, the National Compact Stellarator Experiment (NCSX), the panel members recognized: (a) the significant innovative components of the proposal, (b) the strong theoretical basis for the design, (c) the experimental relationship to the tokamak which has a large data and theoretical basis, and (d) the strengths of the stellarator team in physics and engineering.

The proposed program also includes Concept Exploration (CE) experiments, theory, systems studies, and international collaborations. CE experiments include the Helically Symmetric Experiment at the University of Wisconsin, an upgrade of the Compact Auburn Toratron, and possibly a new quasi-omnigenous (QO) stellarator experiment. The panel members endorse the OFES plan that all new stellarator CE experiments and those with significant upgrades be reviewed as part of the broader CE process.

The US community has interacted closely with the international stellarator research community in developing new concepts for the present proposal. It is expected that the new directions will be viewed as complements to the presently established directions of the world program. A close international collaboration will enhance progress in the field.

2. Background Discussion on the Basis of the Proposal

The stellarator proposal is an outgrowth of commonly perceived weaknesses in the tokamak program which has been the mainline magnetic fusion program for about three decades. The tokamak has had an advantage over other magnetic fusion concepts in that it can produce plasma traps in a relatively simple magnetic configuration with confinement properties that has brought some experiments to regimes that are close to that needed to produce net fusion power. However, several limitations have been of concern in tokamak operation that may affect its viability as a power source. These include:

- (1) The possibility of discharge termination by unplanned and perhaps wall damaging disruptions;
- (2) The difficulty of achieving steady state current operation which can lead to premature fatigue due to stress from continual start-up;
- (3) The likelihood of inducing current driven instabilities (from so-called neoclassical tearing modes) at high beta in the current profiles that are easiest to establish and sustain.

These crucial difficulties appear to be solved in typical stellarator designs. Sudden plasma disruption has not been observed in stellarator operation and the intrinsic property that rotational transform is produced by current in external coils allows for natural steady state operation. Further it is relatively straightforward to design for radially increasing rotational transform profiles that are not susceptible to neoclassical tearing mode instability. These three features of a stellarator provide for viable fixes to significant difficulties in tokamak operation.

The "price" for these fixes is the complexity that results from a stellarator magnetic field configuration. The shape of the magnetic fields is intrinsically 3-dimensional, and thus difficult to envision. More importantly, stellarator magnetic fields are not perfect traps. The magnetic field lines need not form surfaces, and a fraction of the collisionless particles orbits may not be contained due to intrinsic helical ripple (this latter situation is particularly relevant to charged fusion products that when lost would directly impinge on plasma facing surfaces, at perhaps selective "hot spots"). Hence a viable magnetic field design is quite crucial for favorable energy containment, both to contain thermal particles to low neoclassical losses as well as to contain energetic particles that arise from beam injection and fusion products.

Mainline stellarator research projects have existed in Japan and Germany for over two decades. They have demonstrated that energy containment in stellarators in properly designed magnetic fields produce thermal containment properties comparable to tokamak L-mode. Modest improvement of containment has been achieved in stellarator H-mode discharges, and recently W7-AS has achieved a confinement enhancement factor of 2.5 over standard operating conditions. As in tokamaks, the search for improved confinement regimes is of high priority in stellarators.

The main direction in stellarator research in Germany has been to develop a near-omnigeneous configuration where the collisionless motion of particles hardly deviates from a flux surface. Such a property suppresses bootstrap current, which is essential for these machines as they are designed with hardly any magnetic shear. They also seem to need large aspect ratio, which, together with beta stability limits, appears to lead to power producing systems with low surface power density.

An alternate tack that has been developed in recent years has been the study of a quasi-symmetric configuration. Two types of symmetry can be exploited, quasi-helical symmetry and quasi-axisymmetric symmetry. Both symmetries limit the deviation of charged particles from closed flux surfaces. An experimental program that will study helically symmetric systems is now in place at the University of Wisconsin. The main thrust of the present proposal is to investigate quasi-axisymmetric (QA) configurations at the Princeton Plasma Physics Laboratory.

There are several striking features in the QA configuration:

(1) A compactness that allows for an aspect ratio of ~ 3 which is typical of a tokamak, and which can lead to higher wall loading capability than high aspect-ratio stellarators.

(2) Tokamak-like magnetic fields, which allow the rotational transform producing coils to be saddle coils, rather than coils that thread around the machine. This feature allows for more flexibility in determining parameters for optimal operation.

(3) Compatibility of internal bootstrap (or ohmic) currents with external currents for generating desirable rotational transform profiles. The two current sources augment each other. The induced bootstrap current that produces rotational transform "heals" rather than destabilizes magnetic islands in rotational transform profiles that increase with radius.

(4) Stability studies indicate that beta values in up to 5% can be MHD stable in a compact QA configuration. This value is even higher than is planned in many tokamak reactor scenarios; nonetheless a rather large reactor size is still envisioned and even higher beta limits may be needed to obtain the flexibility to have smaller power plants.

These features by and large make the QA stellarator quite promising as a mainline experiment. The proposal exploits in a natural way the expertise of the Princeton group in tokamak operation. Operation of the QA should be very similar to tokamak operation. Indeed the concept can be viewed as an extension of the tokamak concept to a region where stellarator and tokamak fields interact in a mutually beneficial way to solve some of the traditional shortcomings that otherwise exists in both these concepts. There is a high degree of confidence among committee members that interesting plasma parameters would be achieved by the Princeton group on the QA experiment. Nonetheless, there was a realization that the design step from tokamak to stellarator is rather ambitious and unforeseen problems may arise.

The second concept that is being proposed for stellarators is a compact quasi-omnigeneous device. The thrust of the concept is to achieve quasi-

omnigenity and compactness in a configuration that can attain stability at relatively high beta. This concept has made rapid progress during the past year and it is seen as a prime candidate for PoP level of support after the conclusion of the present QA experiment and if the results of the CE investigation are favorable. The quasi-omnigenity allows for weak neoclassical effects so that the bootstrap current is not high (hence the rotational transform profiles remain under external control even at high beta) and the neoclassical diffusion is quite modest. Calculations of fusion particle losses indicate that about 10% of the alpha particles have prompt loss, indicative that most of the alpha particles will be trapped in the quasi-omnigeneous fields. This configuration can have substantial shear, and hence is not subjected to large island formation, as low-shear stellarators are when resonance arises. In this concept, many different parameters are optimized simultaneously.

3. Basis for Support Level

The panel explored the issue of whether the important physics for the QA concept should first be explored at the CE level before undertaking a PoP experiment. Several issues led to the conclusion that the PoP level is proper for this experiment. These included the aforementioned contact with the tokamak data base, which provides both data and theory to guide the QA stellarator. Also, the international stellarator program has provided codes and data which help establish the design point of the proposed experiment. On the experimental physics side, the panel noted that the beta limit is likely to be "soft," and thus its study requires both the proposed heating power and the ability to do a good job on the power and particle balance. Other physics issues, e.g. evaluation of the role of magnetic islands, will require extensive diagnostics and operational pulse length.

The committee also examined the question of whether the PDX facility is the proper facility if a QA experiment is approved. Concern was expressed that this facility, and in particular the vacuum vessel, might be so constraining as to limit the physics issues addressed, e.g. involving the aspect ratio or in the ability to use simple modular coils which could provide a more robust configuration. In the end, the panel concluded that although the vessel is not ideal, the cost saving are probably significant enough to offset any limiting constraints. The poloidal field coils, together with the proposed saddle coils to generate magnetic shear, should provide a significant degree of flexibility for the experiment. The power and other facilities available are a significant asset which should be utilized by the US program.

Although the facility should be capable of conversion to a QO geometry several years downstream (as discussed in the proposal) the decision and specifics of such a conversion should undergo review by the fusion community before it is undertaken. This decision should be made in the light of the knowledge gained from all QA and QO experiments.

4. Technical Issues

The panel members had several technical concerns which need to be addressed as the stellarator program proceeds. These included possible limits due to MHD activity during plasma startup, when the magnetic configuration may have a hill, and whether power and particle handling will be adequate as the experiment moves to higher power and longer pulse lengths. It is recommended that the stellarator community review such issues as it proceeds with a detailed experimental proposal.

The possibility of Alfvén instabilities effecting beam containment when heating is attempted was brought up, as it has been found to be deleterious in some stellarator experiments. Though such phenomena does not appear to be the rule, better theoretical understanding of this phenomena should be developed.

Numerical studies of stellarators are mathematically extremely subtle especially when islands develop at finite beta. Continued verification of known experimental results with theoretical code predictions should continue to ensure that the conceptual understanding is accurate.

5. Cost and Schedule Issues

The proposed stellarator program is rather large with total annual funding ramping to \$30M by FY2003 (i.e. by the fifth year of the POP program). This amount is equivalent to the largest POP funding recommended by the FESAC POP guidelines. The scale of the stellarator POP is determined by the NCSX experiment. The four year construction cost of NCSX is \$35M. After construction, the annual cost for operation and upgrades is \$20M/year. Slightly less than \$10M/year is requested for the costs of supporting theory, stellarator CE experiments, and international collaborations. Since the total cost of the stellarator POP is substantial, the panel members urge that the stellarator community seek ways to reduce costs.

Another concern of the stellarator program is the relatively long time needed to establish experiments in the NCSX facility. Since the scientific results from NCSX are important for the evaluation of this confinement concept, the time required to begin experimental operation should be shortened if possible.

6. Reactor Issues

There are still concerns about the viability of a stellarator reactor that might result from the present concepts. If the physics issues in the stellarator are solved, a stellarator reactor will have several advantages over the tokamak: lack of disruptions, steady-state operation, and the lack of auxiliary current drive. The cost savings of the latter, both in capital and operation, may balance added costs from the complexity of the magnetic coils. However, the beta of the concept as presently proposed is about 5%, and thus on the low

side. It is the panel's understanding that the presently estimated cost of electricity is comparable to that of the advanced tokamak, and thus higher than the US market will accept today. Further innovation and simplification of the stellarator concept may still be needed for it to be a commercially successful fusion energy reactor.

Consensus technical summary of the members of the review panel for Proof-of-Principle Proposals in Fusion Energy Science, June 8-11, 1998

Magnetized Target Fusion: A Proof-of-Principle Research Proposal

1. Summary

The Committee concludes that the MTF Concept qualifies for Proof-of-Principle status. However, it is thought unlikely that this concept will ultimately result in a commercial fusion reactor. Nonetheless, the possibility that MTF could produce plasmas with $Q > 1$ in less than 10 years at relatively modest cost was considered an attractive fusion energy application which warrants PoP status.

This is an innovative proposal that represents a true alternative to existing magnetic and inertial fusion concepts. The proposed approach constitutes an exciting scientific opportunity to study plasmas in a density regime that is intermediate between conventional inertial and magnetic fusion experiments. The site credits associated with the DOE Defense Program are substantial.

2. Introduction

The basic idea behind the MTF proposal is to create an FRC, translate it into a chamber with a cylindrical liner, and electromagnetically implode the liner, thus compressing and heating the FRC to temperatures high enough that fusion gain results in the "dwell time" before turnaround or breakup of the liner. The goal is to achieve a D-T equivalent $Q \sim 0.1$ in three years.

The magnetic confinement configuration known as the FRC has been studied in the laboratory for 40 years. At Los Alamos, an extensive research program culminated in the FRX-C in the early 80's. A thorough review of the FRC concept was published by a member of the proposing team. In the last decade, experimental FRC research in the US has continued at the University of Washington.

Experimentally, the macroscopic stability of FRCs is limited by the $n=1$ tilt instability to values of S^*/E of less than about 3.5, where S^* is the ratio of the separatrix radius to the ion collisionless skin depth, c/ω_{pi} , and E is the elongation of the device. (Here, c is the speed of light and ω_{pi} is the ion plasma frequency.) This experimental finding is inconsistent with ideal MHD, which predicts instability to the tilt mode. ("Ideal MHD" refers to the one-fluid system of equations in the limit of very small gyroradius with isotropic pressure and no dissipative effects.) However, kinetic, FLR, and other non-MHD effects may explain this discrepancy. As far as transport goes,

empirical confinement from FRC experiments scales as R^2/ρ_{ie} , where R is the major radius and ρ_{ie} is the ion gyroradius based on the external magnetic field.

Compression of solid liners also has been studied in the Defense Programs context for several decades. Initial studies utilized high explosives to drive the compressions. Recently, advances in pulsed power technology have made intense electromagnetic compression feasible. This technology is widely employed to create dense plasmas for intense X-ray sources and other applications.

3. Experimental and Theoretical Basis for the MTF Scheme

The experimental basis for the proposed scheme is reasonably sound. Liners that are nearly as elongated as the proposed liners have already been compressed, although some technical issues need addressing. Cylindrical liners enclosing a vacuum have been compressed by factors of 5 or more and, in the course of the compression, no evidence of any gross instability is seen (the proposal seeks a factor of 10 in compression). The FRC proposed as the pre-implosion plasma is twice as dense and about 3 times hotter than in previously recorded experiments but this is not expected to pose serious difficulty. The heart of the experiment - compression of the initial FRC by the liner with almost adiabatic increases in heating and density - has not been tested at the required levels. In related experiments, namely compression by flux ramp-up or by FRC translation, modest compressions have been obtained. However, the proposed MTF scheme, as such, requires significant extrapolation beyond previously demonstrated results.

The theoretical basis of the compression and heating supports the proposed extrapolations mentioned above. The liner implodes on a timescale that is long compared with the Alfvén times of the FRC. On these timescales, the quasi-static MHD equilibrium of the compression has been worked out - the FRC compresses and actually contracts axially as the implosion proceeds. Leaving aside the issue of gross stability for the moment, if adiabatic compression is assumed, it should be more than sufficient to bring the final fusion triple product, $nT\tau$, (density, temperature, and confinement time) to the proposed value as well as to breakeven conditions in future experiments, given the available and future power. Departures from adiabaticity due to particle and energy transport losses have been assessed by O-D calculations using the empirical FRC confinement scaling laws mentioned above. Needless to say, use of these scaling laws is an educated guess - whether these laws continue to hold in the large compressions required remains to be seen. Nonetheless, departures from adiabaticity deduced from these laws, while significant, do not alter the conclusion that enough power will be available to access the desired $nT\tau$. A 2D MHD and transport simulation, including adiabatic and ohmic heating, particle and energy transport losses, and eddy

current heating at the liner during implosion, would greatly enhance the theoretical basis of the compression. Such a simulation might also serve as a starting point towards addressing a major concern, namely the introduction of impurities from the liner material during compression.

There is, of course, the crucial issue of whether the implosion will be stable to gross instabilities on the Alfvén timescale. Indications are that the compression should be stable to these gross instabilities. FRCs are known to be experimentally stable for the S^*/E parameter less than 3.5. For the proposed experiment, this parameter starts off less than 3.5 and the trajectory in the S^*-E parameter space naturally stays less than this value and is chosen to peak at 3.5. (This assumes the static FRC scaling laws will continue to hold). This circumstance addresses the primary concern in MHD plasma compressions, namely the pervasive sausage and kink instabilities. The $n=2$ rotational instability could be an issue. However, in FRC experiments to date, this instability is seen to appear only at the end of a relatively long quiescent period.

4. Reactor Issues

The aspect of the MTF scheme that generates the most discussion is its viability as a fusion reactor. Most panel members think it is unlikely that this concept will ultimately result in commercial fusion energy production. Producing liners inexpensively, rapidly evacuating the exploded debris, and disposing of the radioactive waste are difficult challenges. On the other hand, the MTF plasma resides in between in the vast parameter space spanned by magnetic fusion and inertial fusion. As such, reactor visions are unique: one concept involves vaporizing the first wall and blanket and turning these into a working fluid for an efficient MHD generator. This holds the possibility of mitigating the first wall problem while making a reactor that bypasses the steam cycle.

Consensus technical summary of the members of the review panel for Proof-of-Principle Proposals in Fusion Energy Science, June 8-11, 1998

The Reversed Field Pinch (RFP): A Proposal for RFP Research in the U.S.

1. Summary

The committee believes that the RFP is ready for Proof-of-Principle status. The concept has seen some excellent scientific progress in recent years and there is an enthusiastic community with many new ideas for achieving further progress. Solutions are proposed to several highly challenging problems that require solution before the RFP is viable for a Proof of Performance program and ultimately a reactor. The primary focus of the program should remain upon confinement issues. Progress in this area will determine the best means of designing the experiments needed for establishing such future steps as current drive and higher beta.

2. Introduction

The RFP is a relatively mature concept with many years of experimental and theoretical experience. The USA has been a major player in RFP research for many years. Recently, interest in RFPs has been revived largely motivated by the outstanding work of the MST group at the University of Wisconsin. During this time the international program has grown substantially with the construction of large experiments in Japan and Italy. These experiments provide a multiple-machine database and serve as an experimental basis for evaluating the RFP. Given the long history, international interest, and recent technical progress in the RFP concept, it is natural that the USA program should be elevated to Proof of Principle status.

3. The Advantages

The RFP has a number of potential advantages as a reactor as compared to tokamaks and these serve as motivation for pursuing RFP research as an alternate magnetic fusion concept. These advantages include (1) the use of normal magnets, (2) high beta, (3) weak fields at the coils, (4) the possibility of ohmic heating to ignition, and (5) the potential absence of disruptions. There is a similar list of disadvantages, described below, which limit progress towards a reactor.

The RFP concept also has substantial advantages with regard to basic plasma science. It offers an excellent test bed for examining the generation

and influence of magnetic MHD turbulence on plasma confinement. This is an issue of interest to the entire fusion program and hence is much broader than only the RFP.

Another issue worth noting is beta. The RFP is inherently a high beta device and typically operates in the range of 5 – 10% and has achieved 15% beta. This is essentially the regime of reactor relevance, though the typical values are somewhat on the low side. Thus, substantial beta has been achieved in an RFP, and though improved beta is advantageous, the viability of an RFP for energy applications relies on achieving substantial reduction of transport and in demonstrating that steady state operation can be obtained.

The committee, as well as essentially the whole USA fusion community, believes the quality of research carried out on the MST experiment is exceptional. We have a great deal of confidence and respect for the MST team. They have introduced several highly innovative ideas including the idea of reducing MHD turbulence, and the resulting degradation in transport, by means of current profile control. Although USA support for RFP research is relatively modest, the MST group is arguably considered to be the world leaders in the program.

This combination of advantages -- reactor benefits, broad contributions to fusion plasma science, and an outstanding team — motivate our recommendation for Proof of Principle status.

4. The Challenges

Engineering issues aside, there are three fundamental scientific problems that must be solved in order for the RFP to proceed to the Proof-of-Performance stage as defined in the USA alternate concept program. These can be summarized as follows.

- (1) Confinement: Energy confinement in RFPs has been traditionally poorer than in a tokamak because of the presence of substantial MHD turbulence. Typical thermal diffusivities in an RFP are between 20 – 100 m²/sec. In a comparably size tokamak, the corresponding value is approximately 5 m²/sec. A related observation is that the relative magnetic field fluctuation level is on the order of 0.01 as compared to 0.0001 in a tokamak. Interestingly, there is an empirical scaling law satisfied by many different RFP experiments. This scaling is unfavorable at low current but becomes favorable at high current. It actually represents the “best” performance observed in each given device. To their credit the RFP community is not relying on this scaling law to predict improved transport in future devices but is planning to investigate, at a basic level, the source of fluctuations and the resulting anomalous transport. In this connection the use of current profile control to reduce turbulence has already resulted in an increase in confinement time by a factor approaching 5. Further increases are necessary. This will require new

tools, such as the proposed lower-hybrid current drive. Understanding and improving transport is the most critical problem facing the RFP concept and should receive primary emphasis in the PoP program.

- (2) **Current Drive:** An RFP by its very nature requires a large toroidal current. In a steady state reactor, which requires at least 20 MA, this current must be driven externally by non-inductive means. Standard, tokamak RF methods are too inefficient to be of interest. The RFP community is advocating oscillating field current drive (OFCD) which is theoretically predicted to be much more efficient but which has not as yet been demonstrated experimentally. Furthermore, as much as 20% of the total current must be driven by tokamak RF methods for current profile control. The success of OFCD, or some other equivalently high efficiency method is a critical step on the pathway to a steady state reactor.
- (3) **Resistive Wall Instabilities:** The RFP, by virtue of its low safety factor, is theoretically predicted to be unstable to external resistive wall MHD modes. Standard tokamaks and stellarators are typically designed to be stable even with the wall at infinity. Advanced tokamaks are often unstable to a single resistive wall mode, and stabilization of this mode is an important component of AT design. In fact the problem of resistive wall modes is of importance to the entire fusion community, affecting tokamaks, stellarators, RFPs, spheromaks and FRCs. The problem is more difficult in an RFP where several modes are unstable simultaneously. Various forms of stabilization have been proposed (e.g. feedback, smart walls, rotation) but there has been only limited experimental verification. Stabilization of resistive wall modes is another critical problem that must be solved on the path to an RFP reactor.

5. The PoP Proposal: Suggestions

The committee believes that the RFP PoP proposal has correctly identified the major issues to be addressed and has suggested a strategy for solving each of the related problems. The plan is logical and well thought out and the budget requested is appropriate for the tasks. Furthermore, the total funds requested are relatively modest because of the clever use of MST as the base facility.

Nevertheless, the committee feels that the proposed plan is too ambitious. A more appropriate strategy is to make progress on the three critical problems in a sequential manner focusing initially on the problem of transport. Ultimately, the proposed solutions must be shown to work in an integrated manner. At present though, it is premature to worry about integration and the need for a very aggressive program is unwarranted. The following guidelines represent a more serial approach to the RFP PoP. The

time scale and funding are spread out and there are ample opportunities to alter the strategy mid-stream before too many funds have been committed.

- (1) Confinement is the first priority. The relevant equipment (primarily the lower hybrid power system and related diagnostics) should be purchased and installed on MST. A detailed physics investigation should be carried out as proposed with the goals of understanding transport and improving confinement by means of current profile control.
- (2) The next priority is current drive. The principles of OFCD must be tested and compared to theory. The goal here, as stated in the proposal, is to develop an efficient method for driving the large amounts of current required in an RFP.
- (3) The resistive wall problem is also important, but it must be recognized that this is a community wide problem. The most appropriate experimental plan must be developed within the context of the entire fusion program. The MST team does not feel that their facility is the appropriate place to study feedback stabilization of this problem, but they are properly encouraging their associated community to investigate this problem.
- (4) The measurement of beta limits as suggested in the proposal should be the lowest priority. The reason is that beta achievement is not the key issue for RFP viability, while the other problems are make or break for RFP viability.

The proposal suggests carrying out steps 1,2 and 4 more or less in parallel. The committee suggests prioritizing the experimental effort in accordance to the programmatic importance of the issues.

Section 3

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**Report to the
U.S. Department of Energy
Office of Fusion Energy Sciences
on
Possible Pathways for
Pursuing Burning Plasma Physics
and
Fusion Energy Development
July 17, 1998**

ACKNOWLEDGEMENT

This report represents the combined efforts of a large number of people in the U.S. fusion community in terms of their participation in workshops and in providing input to this report. The nature of this report deals with a broad range of programmatic issues and represents a wide spectrum of opinion. It is almost certain that no single individual, including all the contributors, will agree with everything in this report. Hopefully, however, people will find their point of view represented in a generally fair manner. Individuals who have contributed (with apologies to anyone who has been omitted) directly to this report are listed below.

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Coordinated by C. Baker and prepared by many contributors

POSSIBLE PATHWAYS FOR PURSUING BURNING
PLASMA PHYSICS AND FUSION ENERGY DEVELOPMENT

1.0 Introduction

1.1 Purpose and Background

This report has been prepared in response to a request from the U.S. Department of Energy's (DOE) Office of Fusion Energy Sciences (letter sent by Dr. A. Davies to Dr. C. Baker, January 28, 1998) to consider possible alternatives on reduced cost options for "next-step" devices. A central focus of next-step devices is the study of "burning" plasmas which explore the impact of substantial fusion energy production via the deuterium-tritium reaction.

An important part of the U.S. Fusion Energy Sciences Program is its participation in the International Thermonuclear Experimental Reactor (ITER) program. Taking into account the international situation and U.S. domestic issues, the ITER process is exploring reduced-cost options to the present ITER device. A Special Working Group, reporting to the ITER Council, has been formed to explore these issues on behalf of the ITER Parties, i.e. the European Union, Russian Federation, Japan and the U.S. This report, and its related activities, will aid the U.S. in the international process.

1.2 Community Process

This report is the result of a broad-based U.S. community effort to discuss, debate and work together on the crucial issues involved in considering next-step options. The main content of this report is based on three potential pathways identified at a broadly-attended community Forum for Next-Step Fusion Experiments (University of Wisconsin, Madison, April, 1998) organized principally by the University Fusion Associates and by the work of the ITER Steering Committee—US (ISCUS) on reduced cost ITER options. The Madison Workshop was followed by a smaller Workshop on Next-Step Options (University of California, San Diego, June, 1998) to focus on preparing this report. A broadly-announced Web Site was established to facilitate access to documents related to this process.

Summary

The mission of the Fusion Energy Sciences Program is to advance plasma science, fusion science and fusion technology - the knowledge base needed for an economically attractive fusion energy source. The policy goals that support this mission include:

- advance plasma science in pursuit of national science and technology goals;
- develop fusion science, technology and plasma confinement innovations ; and

- pursue fusion energy science and technology as a partner in the international effort.

A key aspect of the third goal in particular is the study of the physics of burning plasmas. (This is sometimes referred to as the "third leg" of the program). This report describes potential pathways towards this goal that have been identified within the U.S. fusion community (most notably at the Madison Forum for Next-Step Fusion Experiments, April, 1998). There is a strong linkage between this element of the program and the other "two legs" of the U.S. program, i.e., to advance plasma science and concept innovation. The pathways described herein depend directly on the continuation of a viable and strong base program with adequate resources. This base program provides the underlying science and technology critical to the development of fusion energy and the study of burning plasmas. This report is predicated on the presumption that the present base program funding level of somewhat more than \$200M/yr. will be continued.

The purpose of this report is to describe pathways for the U.S. to pursue burning plasma physics and fusion energy development. The first pathway features continued participation in the ITER design effort, now focused on a reduced-cost device, "ITER-RC". This device would be designed to achieve the overall programmatic objective of the original ITER device, but with somewhat reduced baseline plasma performance goals within nominal physics assumptions and the possibility of achievement of the full ITER mission if advanced physics performance can be realized. The ITER-RC tokamak would integrate a moderate-to-high energy gain plasma with a steady-state system including superconducting magnets as well as some nuclear technology testing. The second tokamak pathway would include two devices rather than the ITER-RC: a copper-coil device, capable of DT burning plasma experiments, and a steady-state, advanced device with superconducting coils operating predominately DD plasmas. (In previous extensive design studies, substantial effort has been devoted to the design of devices suitable for both these two pathways.) The third pathway chooses to delay the burning plasma and steady-state steps to focus resources on enhancing concept innovation to prepare for later, but hopefully more affordable steps to the burning plasma and fusion energy development stage. Besides the facilities described above, all three pathways will require, to differing degrees, additional facilities for fusion technology development, such as a point neutron source and/or a volume neutron source.

The ITER-RC pathway puts stronger emphasis on physics and technical integration, facing many important and difficult integration problems in the near term. The modular pathway separately addresses the physics issues of burning plasmas and steady-state and also addresses some issues of integration (e.g. remote maintenance in a DT environment, superconducting coils in a tokamak magnetic environment) prior to initiating an integration step. The ITER-RC pathway is the shortest pathway for fusion energy development but is also the pathway which would require the largest initial funding outlay. The separate devices in the modular approach may provide greater flexibility for concept innovation allowing a more advanced integration step to follow. It is important to note that both ITER-RC and the modular pathway contribute physics and technology information to other lines. The Enhance Concept Innovation pathway puts increased emphasis on concept improvement in the tokamak, related concepts, and concepts more distant from the tokamak, including inertial fusion energy (IFE) prior to initiating either the modular pathway or the integrated machine pathway. This pathway is already a key aspect of the base program and thus also contributes to the possibility of pathway 1 and 2. Each of these pathways are aimed at the same long-term goal but will arrive there with different time scales, costs and degrees of technical challenges and risks.

The Promise of Fusion

The benefits of Fusion R&D were clearly seen by the President's Committee of Advisors on Science and Technology (PCAST) in the 1995 Report of the Fusion Review Panel. Their stated views are increasingly valid today and are quoted below.

"The principal objective of the U. S. program of fusion energy research and development is to provide this country and the world with an abundant, safe, environmentally attractive, and cost-competitive new energy source. Achieving this objective would bring large benefits almost irrespective of how the energy future unfolds; and achieving it could be crucial if society finds it necessary, for environmental or political reasons, to reduce sharply the currently dominant role of fossil fuels in world energy supply."

"In the course of pursuing this energy goal, fusion R&D yields an immediate and continuous additional benefit by nourishing an important branch of basic science - plasma physics - and the technologies related to pursuing it. This field of research, for which nearly all of the funding comes from fusion energy R&D budgets, has been prolific in the production of insights and techniques with wide applications in other fields of science and in industry."

"Finally, for a variety of reasons, fusion energy R&D has evolved a higher degree of international scientific and technological cooperation than any other field of scientific or technological research. This cooperation - entailing not only extensive exchanges of personnel and information but also full-fledged international collaboration in design, construction, and operation of some of the largest experiments - is in itself a valuable model and precedent for internationalization of R&D in other fields. Such cooperation is likely to become increasingly important as the costs of cutting-edge R&D continue to grow in relation to the capacities of individual nations to pay for it."

Progress in Fusion

The research field of Fusion Energy Science has in the last two decades made major scientific advances. Supported by large investments in the late 70's and early 80's, facilities capable of producing and sustaining plasmas with fusion relevant parameters were built and successfully operated. The science of plasma measurement techniques was developed; today nearly all quantities of relevance needed to compare to theories are measured with sufficient precision. Plasma stability limits have been explored experimentally and the results are quantitatively predicted with high accuracy. The theoretical minimum in cross-field plasma ion transport has been reached in some circumstances. Plasmas with high degrees of recombination before reaching a material surface have been produced, fulfilling the simplest vision of magnetic confinement as using the magnetic field to prevent hot plasma from touching the material wall of the confining chamber. Methods to drive the current in low-to-moderate density plasmas with auxiliary means have been successfully employed; the efficiencies of these methods are in accord with theory and code modules exist to calculate the driven currents. The theoretically predicted self-driven or bootstrap current has been confirmed and has significantly enhanced prospects for steady-state operation. The result of these advances has been a confirmation of theory in most areas and a computational basis of plasma understanding sufficient for predictive projection of the performance of next step devices. These recent advances also show the commonality of the issues for fusion concepts (for example, various toroidal concepts) and how advances in one concept (e.g., tokamaks) can be exploited in alternate configurations.

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This advance in scientific understanding was made possible by supporting advances in fusion technologies. Magnetic coil systems and their associated feedback control systems were developed to stably confine the plasma equilibrium and produce many variations of plasma shape for optimization of plasma performance. Plasma heating technologies were developed and deployed at the tens of megawatt level; these systems were indispensable in the studies of plasma stability and current drive and also now are the basis of many important plasma measurement techniques. Superconducting coils have been used in magnetic confinement systems and pulse lengths exceeding two hours have been produced, clearly showing the potential for steady-state. Tritium fueling systems were implemented and safely operated resulting in the large scale production of fusion power (11 MW in the TFTR tokamak and 16 MW in the JET tokamak) and over 1 gigaJoule of fusion energy produced.

The Stages of Fusion Development

In order to discuss the status of fusion progress and future pathways, it is useful to introduce the five stages of fusion concept development as described in the 1996 report by the Alternative Concept Panel formed from the Science Committee of the Fusion Energy Advisory Committee. There are five development phases applicable generally to all fusion concepts:

- concept exploration
- proof of principle
- proof of performance
- fusion energy development
- fusion power plant deployment (begins with a DEMO plant).

The concept exploration phase is generally implemented on small experiments aimed at particular new innovations. The concept exploration phase can also include particular new developments on larger facilities.

A proof-of-principle program has as its main goal the resolution of key scientific issues in depth and on a broad front. A proof-of-principle level program is generally implemented in intermediate sized devices which are capable of investigating a complete set of key issues in depth, of producing plasma parameters approaching reactor conditions, of achieving extensive control capability and of using a comprehensive set of diagnostics. In the United States, examples of proof-of-principle level devices are the Alcator C-Mod and DIII-D tokamaks.

The defining feature of the proof-of-performance level phase is the need for plasma parameters needed to minimize the extrapolation to the following more costly fusion energy development steps. The clear examples of devices in this class are the tokamaks JET in the EU, JT-60U in Japan, and TFTR in the U.S. The DIII-D and Alcator C-mod are sufficiently capable devices technically to make some contributions at this program level.

The fusion energy development phase is mainly defined by devices which can study deuterium-tritium burning plasmas and integrate reactor relevant technology. Another goal that often enters at this stage is steady-state operation with its associated technology implications such as plasma power exhaust and superconducting magnets. These two sets of issues and their integration cannot usually be addressed in devices associated with the proof-of-performance level research. It is a strategic issue of critical importance that the scientific issues of burning plasmas and steady-state cannot be addressed at the proof-of-principle level, but require facilities of significant scale and cost.

Role of the Base Program

The scientific and technological progress in the program cited above has been and will continue to be derived from a strong and healthy Fusion Energy Science base program. This base program encompasses the embryonic concept exploration stage up through the proof-of-principle stage. The scientific progress to date gives good confidence that many concepts can and should be brought through the proof-of-principle stage. The efforts on concept improvement are also contained in the base program. Given the large number and diversity of fusion approaches, *an essential element of fusion strategy is the view that sequentially and over a period of time selected concepts may be moved up through the five development phases.* Although the advances in the field cited above were primarily made using the tokamak confinement device, these general advances engender the anticipation that a similar level of scientific maturity can be realized for a number of other fusion approaches. The Base Program mission to bring a number of fusion concepts through the proof-of-principle stage is an essential and enduring fusion strategy element that will be pursued as a component of any larger strategy. Which concept will make the ultimately best fusion power system is a question that will be answered over time. *The immediate strategic issues revolve around which concepts to advance in what order and at what pace.*

Fusion Development Steps

Most of the scientific and technology progress cited above has centered around the tokamak concept. This concept was seen in the late 70's as the concept most likely to be capable of producing high performance plasmas. The research done with the tokamak has borne out this early view. The fusion program strategy has been to advance the tokamak through the development stages at the most rapid possible pace. In the 1970's a sufficient proof-of-principle basis was developed to motivate the construction of the proof-of-performance level tokamaks. These tokamaks have achieved performance levels that give the required confidence for the tokamak program to move on to the fusion development phase. It has been the U.S. and international view for the past decade that burning plasma physics is the next frontier of fusion plasma physics, and we should pursue this science as soon as practical in a tokamak device. The cost of the devices for the fusion development phase has motivated an examination of whether the proof-of-principle basis arrived at in the 80's could be improved upon. This new thrust, generally called the Advanced Tokamak (AT) program, represents renewed research at the proof-of-principle level aimed at finding the upper bounds to the potential of the tokamak as a magnetic confinement system.

The essential strategic question for the fusion program at this time in regard to the tokamak is whether to proceed to build tokamak devices in which we have confidence in a basic level of performance in order to get on now to the issues of burning plasmas and steady-state that are not readily addressable with proof-of-principle level facilities or to accelerate the development of more advanced operating modes to incorporate in the fusion development steps to come. A further aspect of this strategic question is whether to accelerate the development of concepts alternate to the tokamak that might be advanced into the development phase.

Three Pathways for Pursuing Burning Plasma Physics and Fusion Energy Development.

There is a strong consensus in the international fusion scientific community that the tokamak is technically ready for the steps to burning plasma physics and steady-state operation. There are, however, a range of opinions (hence different pathways) about the most cost-effective and technically sound approach at this time. This has led us to define three potential pathways:

1. Integrated: a single device like ITER-RC: a reduced-cost ITER-like tokamak;

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2. Modular: two separate tokamak devices to demonstrate DT burning and long-pulse/steady-state operation; and
3. Enhanced Concept Innovation.

The cost and technical challenge of the ITER-RC step is considerable. The combined cost of a smaller copper-coil burning-plasma and steady-state superconducting device is probably comparable to, perhaps less than, the cost of an ITER-RC. However, when the cost of an ITER-like 'integrating facility (which would have to follow these two devices) is included, the total cost of the modular pathway to a DEMO is probably larger than the ITER-RC pathway. The U.S., at present budget levels, cannot proceed alone down either of these two paths, so the decision as to which pathway will be pursued is necessarily an international one. Indeed, all three strategies will be heavily dependent on, and would benefit greatly from, international collaboration.

The three pathways differ in the number of remaining sequential development steps and the total time to a DEMO step, as depicted in Figure 1. Pathway 1 addresses the major physics and technology issues of fusion energy development in an integrated manner and provides the most timely pathway for the development of fusion energy to the demonstration stage. Pathway 2 addresses the major physics issues separately in less expensive devices and then addresses physics and technology integration in a subsequent advanced integration facility to arrive at the demonstration stage. Pathway 3 delays addressing the burning plasma physics and integration issues of fusion energy in the manner of either pathways 1 or 2 until other confinement concepts have been developed through the Proof-of-Principle and/or Proof-of-Performance steps. Table 1 provides a summary of the principle advantages of each pathway. The choice which is ultimately made will depend on national and international factors, as well as the technical issues outlined in this report. We have confidence, however, that whichever path is pursued, fusion can and will play an important role in the world's energy future.

The main part of this report are the following sections which describe three pathways in detail:

1. Integrated: a single device like ITER-RC: focused on a reduced-cost version of ITER;
2. Modular: focused on separate-mission tokamak technology; and
3. Enhanced Concept Innovation: focused on concept innovation leading to the study of burning plasmas at a later time.

Each section describes the pathway and its rationale, implications and advantages/disadvantages. Some topics, such as the potential contribution of the Strategic Simulation Initiative and fusion Technology and materials issues, apply to all pathways but are placed for now in the chapter on the Modular Pathway. This report does not attempt to make value judgments of choice among those pathways.

Table 1: Candidate Pathways - Principal Advantages

<u>Pathway</u>	<u>Advantages</u>
(1) ITER-RC	<ul style="list-style-type: none">• Early study of integration of burning plasmas, long-pulse/ steady-state operation and fusion technology.• Minimizes number of steps (and time) to tokamak-based, demonstration power plant. No additional integrating facility needed.• Consistent with strategic plans of ITER Partners.• Makes maximum use of the leveraged U.S. investment and results of the ITER-EDA.
(2) Modular	<ul style="list-style-type: none">• Early study of burning plasmas and long-pulse, steady-state operation.• Reduces initial facility investment costs and provides optimization for separable missions.• Provides further optimization before integration step, allowing perhaps a more advanced integration step to follow.• Provides multiple options for location of major facilities.
(3) Enhanced Concept Innovation	<ul style="list-style-type: none">• Provides for enhanced concept improvement leading to possibly the development of less expensive, more attractive fusion concepts.• Reduces near-term facility investment costs.• Provides further opportunities and time to optimize concept(s) for burning plasma, integration, and demonstration.• Stimulates breadth of plasma science development (This is an enduring base program value common to all pathways).

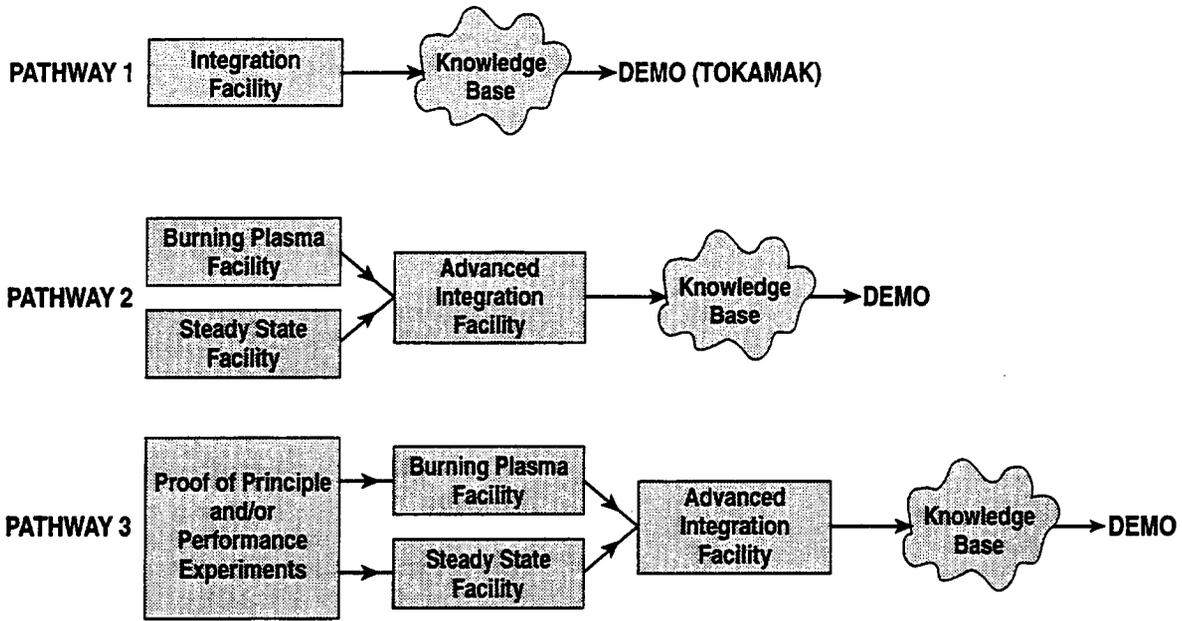


Fig. 1 Potential sequence of steps in the three pathways

2.0 Reduced Cost ITER Integrated Step

2.1 Pathway for a Reduced Cost ITER

The "Reduced-Cost ITER" pathway exploits the existing situation in which, based on strong progress in world-wide tokamak research, the world program has decided that the tokamak concept is technically ready to proceed to a physics and technology integration step in which the next major physics issues of burning plasmas and steady-state will be explored. Implementation of the strategy requires that funding can be secured to exploit this opportunity to follow what appears to be today the most direct, economical and timely pathway for fusion energy development. The reduced-cost ITER strategy will accomplish most, perhaps all, of the ITER mission in a less costly experimental facility. The four ITER Parties have agreed to develop a reduced cost design during the EDA extension period, with the objective of reaching a construction agreement by the end of the three-year extension in July, 2001. If the reduced cost ITER is constructed, the next -step program would combine in a single major facility

- the creation and experimental investigation of self-heated burning plasmas,
- the demonstration of long-pulse advanced tokamak operating modes in burning plasmas,
- the integrated exploration of related tokamak plasma physics issues,
- the integration of fusion reactor-relevant technologies, and
- the integrated testing of fusion reactor components in a single major facility.

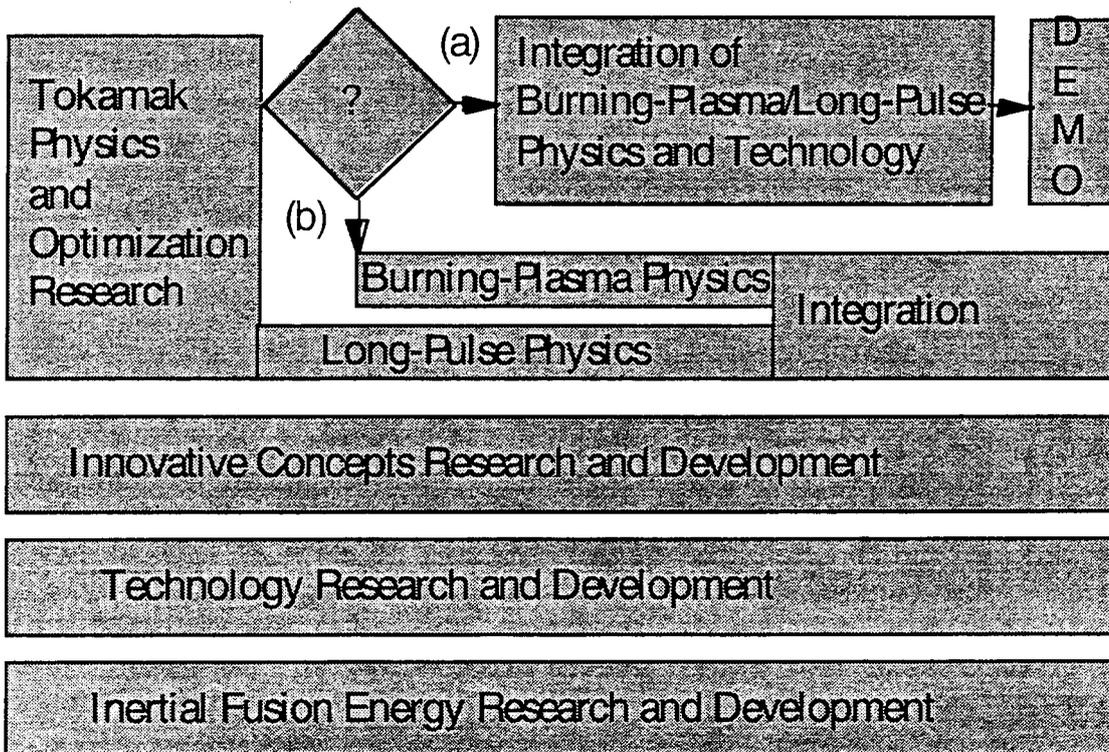


Figure 2.1. Pathway Roadmap. The tokamak program is at a decision point regarding readiness to proceed to two possible next steps: (a) an integrated device that achieves both the physics integration of burning plasmas and long-pulse operation and the physics/technology integration of burning plasmas and reactor-relevant technology, or (b) a program of separate burning plasma and long-pulse devices whose physics results would be combined in a subsequent integration device, which would also integrate the technology. Other elements of the overall program strategy would include the

innovative concepts program, the technology program, and the inertial fusion energy program, which proceed in parallel with the tokamak program.

The RC-ITER experimental program would be phased, with the early phases emphasizing physics explorations, the intermediate phases emphasizing the achievement of reliable quasi-steady-state operation with moderate neutron flux, and later phases emphasizing the accumulation of neutron fluence and operating time for fusion nuclear and materials science studies and for integrated component and system testing. The device is being designed with sufficient flexibility to operate under a variety of experimental scenarios and to allow modifications during operation that would take advantage of results from earlier phases. The nuclear testing capability of ITER, implemented mainly through the introduction of blanket test modules, would reduce the need for a Volume Neutron Source.

The reduced-cost ITER pathway has a high probability of sustaining the four-party ITER collaboration on a next-step tokamak. It is consistent with the collaboratively evolved strategy of the ITER project and with the fusion development strategies of our ITER partners. While the parties differ on some of the detailed specifications for the DEMO step following ITER, they envision ITER and a "point neutron source" facility (for lifetime materials testing) as providing the technical basis for the DEMO. Figure 2.1 illustrates the combination of near-term research programs feeding into the ITER step, which would lead to a tokamak DEMO when combined with the point neutron source; the base program of advanced tokamaks, other innovative concepts and technology programs would feed into the ITER and DEMO programs throughout the periods of design and operations. (Some of our ITER partners also see ITER as providing the basis for a non-tokamak DEMO, especially one using a closely-related confinement approach such as the stellarator.) The Inertial Fusion Energy Program is envisioned as progressing in parallel with the magnetic fusion energy program. Under currently limited US fusion budgets, the need to preserve a strong base program will constrain the US's ability to contribute to ITER; however, the return on investment for the US's contribution will be enormous due to the larger investments of the international partners.

The reduced-cost ITER will be designed with the flexibility required to accomplish a two-fold mission:

- a reduced physics and testing mission (e.g. $Q \geq 10$, moderate pulse length of 300-1000 seconds, $\Gamma_n = 0.5 \text{ MW/m}^2$) when operating in the basic physics performance mode, and
- the full ITER mission (e.g., ignition, steady-state, $\Gamma_n = 1.0 \text{ MW/m}^2$) when operating in the enhanced physics performance mode to demonstrate the upside potential of tokamak power systems, but with increased technical risks.

The mission reductions inherent in the de-scoping to the reduced-cost ITER eliminate the commitment to ignition (reducing mandatory performance to $Q=10$), and reduce the nuclear testing capability due to reduced inductive pulse length and reduced neutron flux when operating in the basic performance mode. With the modified $Q \geq 10$ performance objective, there would be less physics margin at the pessimistic end of the range of physics performance, but ignition might still be possible at the optimistic end of the range. Investigation of thermal control of an ignited discharge would be deferred in the base physics performance mode, and staging would allow delaying specific upgrade capabilities until experiments on ITER itself establish the requirements. The technologies and engineering design solutions developed during the ITER EDA will be adapted for the reduced-cost ITER design, including such improvements and innovations as are consistent with the ongoing ITER R&D program.

The basic physics performance mode will use projections of the established ELMy H-mode database that are the basis for the ITER EDA design. The reduced-cost ITER will be designed to achieve the reduced mission based on ITER EDA physics rules and consequent cost/benefit design optimizations. However, the design will also include features that permit exploration of advanced tokamak (AT) modes, including shaping ($\kappa_{95} \geq 1.6$ and

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$\delta_{95} \geq 0.3$, possibly within the constraint of a single null configuration), n=0 internal coils if needed for vertical position control, current profile control, flexible heating/current drive systems for pressure-profile/transport-profile control, and real-time profile diagnostics. The enhanced physics performance mode will be based on the database for advanced tokamak operation that will be established within the next few years.

It is envisioned that, during the design and construction phases, the US would be involved in design, diagnostics, and manufacture of high-technology tokamak components and systems, targeted at achievement of US science and technology goals while emphasizing dual-use activities which both benefit ITER and achieve US program goals outside ITER. In the operations phase, the US would be involved in the scientific and technological aspects of ITER's experimental program, addressing the "third leg" of the US fusion program — the pursuit of burning plasmas and technology through international collaboration.

2.2 Rationale for Reduced Cost ITER Pathway

The primary rationale for the reduced-cost ITER pathway is to exploit the present status of the world-wide tokamak program:

- burning plasma physics is the next frontier of fusion energy science;
- all Parties and pathways eventually require such an integrated physics/technology step to develop fusion energy;
- the tokamak concept is technically ready to proceed with this step; and
- the cost to the Parties will be reduced if we proceed on a cost-shared international basis.

The reduced-cost ITER would build on the existing international collaboration in design and R&D. This international collaboration allows the US to highly leverage its contribution to accomplish the next stage of magnetic fusion science exploration and magnetic fusion energy development in a highly cost-effective manner. The reduced-cost ITER is consistent with the fusion development plans of the international partners.

The tokamak concept is highly developed, and ongoing tokamak research is very promising with respect to future performance enhancements. The tokamak has already demonstrated reliable and sustained operation in a physics mode that extrapolates to achievement of a burning plasma in the reduced-cost ITER. The world tokamak program has achieved further enhancements in plasma performance for short periods during a pulse, and the tokamak program is presently focused on the sustainment of this enhanced performance for extended periods and on the achievement of high levels of performance in several parameters simultaneously. The reduced-cost ITER pathway will address essentially all the major next-step issues in tokamak physics and fusion technology and their integration in a single device. The reduced-cost ITER pathway will maximize use of the fusion-reactor-relevant technologies and design solutions that have been developed in the ITER EDA and will establish the knowledge base for the demonstration of fusion power in the most timely and cost effective manner.

While there are uncertainties about the levels of enhanced performance that could be achieved and sustained in the reduced-cost ITER, the design will incorporate those features that are expected to be needed to obtain optimized reactor-scale plasma performance. The planned flexibility in the design is intended to respond to the uncertainties and to provide a range of physics operating modes that will permit both broad scientific research at the forefront of tokamak plasma science and improved likelihood of mission success. The world's tokamak research

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program is now focused on the resolution of high-priority physics R&D issues that relate to the design of ITER; in particular, an ITER Topical Group on Advanced Tokamaks has been established to resolve the shape issue and current-profile/pressure-profile control for the reduced-cost ITER, and there is increased attention of all Physics Expert Groups to AT needs.

2.3 Technical Contributions of a Reduced Cost ITER

As stated in the formal ITER Agreement, "The overall programmatic objective of ITER is to demonstrate the scientific and technological feasibility of fusion energy for peaceful purposes". A Special Working Group (SWG) has been charged with proposing "technical guidelines for possible changes to the current detailed technical objectives and overall technical margins, with a view to establishing option(s) of minimum cost still satisfying the overall programmatic objective of the ITER Agreement". The formal report of the SWG addressing this charge has been approved by the ITER Council. The physics benefits of the reduced-cost ITER assumes that the device would:

- achieve extended burn in inductively driven plasmas with $Q \geq 10$ for a range of operating scenarios and with duration sufficient to achieve stationary conditions on the time-scales characteristic of plasma processes, and
- aim at demonstrating steady-state operation using non-inductive current drive with the ratio of fusion power to input power for current drive of at least 5.

While these levels of plasma performance are less than those targeted for the present ITER, as described in the Final Design Report (FDR), the SWG expresses the view of the four ITER Parties that this reduced level still satisfies the overall programmatic objectives of the ITER EDA Agreement. Operation in enhanced performance modes in the reduced-cost ITER might permit the achievement of ignition.

A device meeting these technical and plasma performance objectives would permit studies not only of reactor-scale burning plasma physics and long-pulse physics separately, but also their integration. While aspects of the relevant individual physics phenomena could probably be studied separately on somewhat smaller devices, the integrated combination of long pulse and reactor-scale burning plasmas together with the relevant technology is key to the mission of a device such as ITER.

2.3.1. Burning plasma physics, steady state physics, and advanced tokamak physics

Experiments in the reduced-cost ITER will explore the physics issues of "burning plasmas", in which the heating is dominated by alpha-particles created by the fusion reactions themselves, as distinct from an "ignited" ITER plasma, in which the heating is only by alpha particles. At $Q=10$, the power from the alpha particles would be two-thirds of the total heating power. Burning plasma physics issues will include new plasma-physical effects on the Alfvén eigenmodes made unstable by the presence within the plasma of a population of super-Alfvénic alpha particles. Although the relative population of super-Alfvénic particles is expected to be smaller than in present experiments, theoretical studies of energetic-particle modes in plasmas such as ITER's predict that new phenomena will arise: for example, ITER-scale burning plasmas would be able to address nonlinear collective effects in which many toroidal Alfvén eigenmodes are unstable and drive fundamentally different loss mechanisms, such as stochastic diffusion due to overlapping resonances, whereas present-day ($Q < 1$) DT plasmas can study only coherent single modes of instability and particle trapping in a resonant drift island. While detailed studies of these effects in a $Q=10$ (rather than ignited) ITER plasma remain to be carried out, it is clear that there will be very little difference between ignition and $Q=10$ in regard to the alpha particle population, so that a reduced-cost ITER plasma will be fully representative of a reactor plasma in this regard.

The reduced-cost ITER will also permit studies of the physics of very-long-pulse/steady-state plasmas, in which much of the plasma current is self-generated and which will require effective control of plasma purity and plasma-wall interactions. Achievement of a large fraction of self-generated current in a high-performance plasma will require sufficient plasma shaping, plasma stability at high normalized beta-values (or active stabilization of wall modes), and control of the profiles of quantities such as plasma current, pressure, density, and flows. The reduced-cost ITER design is aimed at incorporating increased configurational flexibility, as well as features specifically chosen so as to permit the achievement and study of steady-state operational modes. In addition, the reduced-cost ITER will address issues of steady-state control of plasma purity and plasma-wall interactions, including the physics of a "radiative divertor" designed for handling high power flow for long pulses, and would allow studies of novel plasma and atomic-physics effects, as well as advancing the materials science of surfaces subject to intense plasma interaction. Increasing triangularity of the core plasma affects the divertor magnetic configuration in a way that reduces the flexibility to accommodate uncertainties in divertor physics. The replaceable divertor cassette developed for the present ITER design provides an opportunity for a trade-off of core plasma shaping versus divertor flexibility. In addition, the reduced-cost ITER gives greater emphasis to AT modes; the SWG statement on $Q_{\text{current_drive}} \geq 5$ (the same as in the original ITER guidelines) documents the commitment to exploration of AT and steady-state physics and technology.

Since even the reduced-cost ITER has three times more gyro-radii in its plasma minor radius than does the largest present-day tokamak, it will allow studies of size-scaling of transport which should resolve issues of the dependencies on relevant dimensionless parameters. The different scalings of the edge and the core would be studied. For the first time, the core-plasma and the edge-plasma would be simultaneously in a reactor-like regime; in particular, the size-scalings of confinement in the core and in the edge may give rise to fundamentally different density limits than in present-day tokamaks. Transport barrier studies would utilize a variety of auxiliary heating and current-drive techniques to create, control, and sustain localized regions of significantly reduced transport.

In either the present or reduced-cost ITER, studies of size-scaling of stability would include both the scaling of MHD modes with number of gyro-radii at relevant collisionality and effects of significantly larger energies and plasma currents during plasma transients, including so-called "disruptions". Size-scaling of both ideal and non-ideal beta-limits would be addressed. Feedback stabilization of neoclassical tearing modes and profile control would be studied and utilized to overcome long-pulse beta limits.

At reactor-scale levels of plasma current (i.e., in either the present or reduced-cost ITER), disruptions may induce the new phenomenon of avalanching of runaway electrons by hard collisions, as well as competing mechanisms for the loss of energetic electrons by fluctuations and non-axisymmetries. Physics studies of mitigating power flows during disruptions would address the feasibility of reducing the plasma-wall loads, complementing the technology program's work on handling these loads.

2.3.2 Physics Integration

Most importantly, the reduced-cost ITER would allow the integration of burning plasmas with long-pulse/steady-state operation. This integration will involve the following complex interplay of transport, stability, and an internal self-generated heat source:

- The evolution of plasma profiles would no longer be dominated by external heating; internal self-generated feedback loops would be prominent, with the self-heating affecting temperature and density profiles, that in turn modify transport and stability and hence affect the self-heating. The important time-scales, in ascending order, are the energy confinement time, the time for significant accumulation of thermalized alpha particles

(helium ash), the "skin time" that characterizes the slow evolution of the current profile within the plasma, and the time-scale for plasma-wall interactions and for wall temperatures to reach equilibrium. Since transport and stability are dependent on the current profile, ITER's pulse length (which is longer than the relevant skin time in both the present and reduced-cost designs) is an important measure of its ability to study the approach to steady-state profiles.

- With strong self-heating, profile control will most effectively take the form of control of local transport, by creating transport barriers that serve as tools for profile control. Such studies would be key to developing an attractive tokamak reactor concept, where the alpha power could be utilized to create pressure profiles that drive consistent self-generated current profiles. As such, ITER would be key to the optimization of the tokamak concept, adapting the advanced tokamak features and techniques developed throughout the world program on non-burning plasmas and applying them to the control of an optimized self-heated burning-plasma configuration.

For these key studies, an ignited plasma is not essential: it is sufficient for the self-heating to be dominant -- as it would be in the Q=10 reduced-cost ITER. The emphasis on research into integration of burning-plasma physics and long-pulse physics in advanced tokamak modes provides an opportunity for developing the basis for an even more attractive tokamak reactor concept.

2.3.3 Technology Integration

The integration of technology has been a clearly stated programmatic objective of the ITER agreement, "... by demonstrating technologies essential to a reactor in an integrated system, and by performing integrated testing of the high-heat-flux and nuclear components required to utilize fusion energy for practical purposes." There is universal agreement that an integrated device will be required prior to a demonstration reactor -- the current argument is over whether to proceed with the integrated device on the present database, or to proceed first with a modular approach, returning to an integration step only after further concept improvement.

The technology integration objectives for a reduced-cost ITER should be adequately achievable even at baseline-physics neutron flux half that of the current ITER. The physics/technology integration objectives would be even better satisfied in the reduced-cost option, provided advanced tokamak operation can be achieved and sustained in steady state under conditions of strong self heating.

The compatibility of the "nuclear" components (divertor, limiters, blanket, tritium extraction, shield, etc.) with the tokamak environment will be an important driver for design and R&D. Making design choices that are consistent with nuclear technology and remote-maintenance requirements is an imperative for ITER, but would be very unlikely to happen in a cost-conscious design of a non-nuclear or very-low-fluence device.

In particular, studies aimed at mitigating the power flows during disruptions would address the influence of melting and erosion of significant amounts of wall material and the influx of this material into the plasma. Although the energy deposited in a disruption would be substantially smaller in the reduced-cost ITER than in the full ITER, the surface area on which it is deposited is also smaller, so that erosion and melting are likely to be similar. Co-deposition of tritium with disruption-induced or steady erosion of wall material can adversely affect safety-related tritium in-vessel inventory ceilings. Tritium recovery technology can be adequately demonstrated on a reduced-cost ITER.

The compatibility of the "standard tokamak" components (superconducting magnets, vacuum chamber and pumping ducts, heating and current drive, and diagnostics) with a nuclear environment will also be a design driver absent from non-nuclear devices; an example is the need to avoid paths which irradiate components intolerant to too

many unimpeded neutron flights. The design choices will in turn be confirmed by operation in the nuclear environment.

Reality provides an over-arching discipline to design solutions. The emphasis on failure modes, reliability and maintainability of components necessary in a major integrated nuclear facility will drive design and R&D to a much greater extent than in non-nuclear devices. The high availability goals of the later engineering-oriented phase of an integrated device will also be a strong driver.

The nuclear testing role of ITER is fulfilled mainly through the installation of blanket test-modules, introduced through ports specifically allocated for this purpose. The reduced-cost ITER could have the same nuclear testing capability as the original ITER, if modestly enhanced performance can be achieved with advanced physics, but would have a reduced neutron flux and fluence capability under ELMy H-mode operation. Even the reduced capability (e.g., fluence of about $0.3 \text{ MW}\cdot\text{yr}/\text{m}^2$), when combined with a point neutron source, could provide a sufficient basis for a DEMO design.

2.4 Fusion Development Pathway Implications of a Reduced Cost ITER

The primary advantages of the reduced-cost ITER pathway to fusion development are threefold:

- it minimizes the time and number of steps needed before a magnetic fusion demonstration reactor can be built,
- it shares the costs and risks internationally, providing a large return on investment to any one Party, and
- it provides for the involvement of the world's fusion experts and the Parties' industries in a coordinated world-wide program to achieve ITER's objectives.

The ultimate objective of a government-funded fusion energy development program is to bring one or more fusion reactor concepts to the stage at which the concept is sufficiently demonstrated to provide an assurance that electricity can be generated at an affordable cost with acceptable environmental consequences. This is generally agreed to require demonstration of:

1. reliable, controlled operation of a D-T fusion plasma under reactor-relevant conditions;
2. reliable operation, to some significant fraction of their anticipated lifetimes, of reactor-extrapolatable technologies, components and systems under fusion reactor conditions;
3. reliable operation of an integrated fusion reactor at availabilities ($> 50\%$) that are extrapolatable to commercial requirements;
4. tritium fuel self-sufficiency;
5. net electrical power production at significant levels ($> 100\text{s}$ of MW);
6. the safety of fusion reactors;
7. the feasibility of economically competitive fusion reactors; and
8. the feasibility of environmentally benign fusion reactors.

The collection of sequential and parallel research and development steps that lead to these demonstrations is the fusion development pathway for a given fusion concept. The culmination of the development pathway for a given fusion reactor concept is generally agreed to be a demonstration plant (DEMO) in which the simultaneous demonstration of most, if not all, of the above requirements is achieved.

The ITER mission was defined with the objective that ITER, its supporting R&D programs, and the nuclear and materials testing programs that would be carried out, both as part of ITER and in supporting national R&D programs, would provide the design basis for a DEMO with respect to requirements #1-6. It was envisioned that advanced physics research would be carried out, both as part of the ITER experimental program and in parallel with ITER on other devices, to develop a tokamak physics concept for the DEMO in support of requirement #7. The development of advanced blanket, structural and other materials in parallel with ITER is necessary to provide the design data base for a DEMO that can satisfy requirement #8, consistent with all the other requirements. These major elements, together with the base tokamak plasma physics and fusion engineering science research programs, constitute the "ITER" fusion development pathway. To the extent that the reduced-cost ITER is successful in accessing advanced tokamak operation to accomplish the full ITER mission, the "reduced-cost ITER" development pathway is identical with the "ITER" development pathway. The "reduced-cost ITER" development pathway is probably the shortest and least expensive development pathway for tokamaks.

2.5 Pros & Cons of a Reduced Cost ITER Strategy

The advantages of the reduced-cost ITER (ITER-RC) strategy include the following:

- The ITER-RC strategy exploits the advanced status of tokamak development worldwide, including the readiness to move to burning plasmas and system integration. The ITER-RC step will be a dramatic advance in the development of fusion and will move fusion research much closer to the production of practical fusion power.
- The ITER-RC strategy will permit early study of the integration of burning plasmas, long-pulse/steady-state plasmas, and fusion technology. It addresses the major next-step tokamak physics and technology issues and their integration in a single device.
- The ITER-RC strategy, because of its early integration of physics and technology, minimizes the time and number of steps needed before a demonstration magnetic fusion reactor can be built; and it makes possible the earliest possible implementation of magnetic fusion energy.
- The ITER-RC strategy has a sound physics basis, well-supported by ongoing world tokamak programs. With basic-level plasma performance, the fusion alpha power will be dominant; with enhanced performance, ignition may be achieved. The reduced-cost ITER should have about the same probability of accomplishing its reduced physics ($Q = 10$) and nuclear ($\Gamma_n \approx 0.5 \text{ MW/m}^2$) missions under ELMy H-mode operation as the ITER EDA design was judged by the international fusion community to have of achieving the full ITER mission. Moreover, the reduced-cost ITER would have a possibility of accomplishing the full ITER mission under modestly advanced physics assumptions.
- The ITER-RC strategy brings the world's technical and financial resources to the task, with internationally shared benefits and risks. The ITER project (including INTOR before it) represents almost 20 years of collaboration on the definition, design and supporting R&D for a next-step tokamak experiment by the international partners. This collaboration and its products are generally held in high regard by the involved governments; for example, in the G8 Communique following the recent Birmingham summit (May 15-17, 1998), world leaders stated, "We acknowledge successful cooperation on the pilot project of the International Thermonuclear Experimental Reactor (ITER) and consider it desirable to continue international cooperation for civil nuclear fusion development". The ITER-RC strategy is consistent with the collaboratively evolved strategy of the ITER project and with the fusion development strategies of the other ITER partners. The ITER-RC strategy is strongly favored by all of the non-US ITER parties.
- The ITER-RC strategy would maximize use of the technologies and engineering design solutions that have been developed already for ITER, thus utilizing fully the results of a six-year technology R&D program and a nine-year engineering design effort.
- With US participation, the ITER-RC strategy enables US industry to maintain parity in fusion technology with competitors in other Parties.
- The ITER-RC program provides valuable physics and technology information for other magnetic fusion concepts.
- The ITER-RC strategy demonstrates full-scale integrated reactor technology, much of which is generic to all toroidal fusion concepts.
- The ITER-RC strategy reduces the need for a Volume Neutron Source.

- The ITER-RC strategy fits well within the tritium availability window and benefits from civilian supplies of tritium.
- Successful ITER-RC operation will give the Parties the option to proceed with a fusion demonstration plant based on their respective energy needs and utility industry circumstances.

The disadvantages of the reduced-cost ITER strategy include the following:

- The ITER-RC strategy must confront all of the major next-step physics and technology issues and their integration in a single device.
- The consequences of technical failure of a single device that addresses all next-step issues together would certainly be greater than the consequences of technical failure of a single less-expensive device that addresses a single issue.
- If ITER-RC's physics performance does not meet expectations, then the credibility of magnetic fusion would be damaged.
- Ultimately, a better concept may emerge and, because of overall resource limitations, ITER-RC might have delayed implementation of this better concept.
- An international agreement on siting and cost-sharing is required.

2.6 Near Term Actions

Assuming that the ITER-RC strategy is adopted, world efforts over the next few years should be focused on the following tasks:

- the completion of the ITER EDA technology R&D projects;
- development of an attractive reduced-cost ITER design; and
- the development of the physics and engineering databases to support an advanced design.

As part of this effort, the US should:

- fully participate in the completion of its assigned role in the ITER EDA technology R&D projects, which also have intrinsic value for the US domestic program;
- participate vigorously in the design of the reduced-cost ITER, which accommodates and exploits AT features, since the US has strongly advocated incorporating advanced features in a reduced-cost ITER;
- focus significant capabilities of the US base tokamak experimental program and of the supporting theory and computational programs on the development of the physics basis needed to support an attractive reduced-cost ITER design; and
- direct appropriate parts of the technology program at cost reduction for key ITER components.

2.7 Summary of Technical Appendices on Reduced Cost ITER

2.7.1 Advanced Physics Assumptions

The world tokamak program is making steady progress in understanding Advanced Tokamak (AT) operating modes that have the promise of significantly enhanced performance and potentially lead to attractive fusion power plants (e.g., the ARIES-RS study, and the Galambos et al. 1995 Nuclear Fusion paper). However, the highest performance results to date are transitory and are not yet achieved with all the relevant dimensionless parameters simultaneously. Demonstration and understanding of long pulse AT discharges is the challenge being pursued by the international tokamak community; tokamak facilities are implementing rf current-profile control, plasma density control (e.g., divertor and fueling), and exploring ideas for internal transport barrier control (e.g., rf flow drive). Given the promise of these emerging AT modes, we believe that they should be a central ITER research objective and design driver. A key question is the level of advanced performance that reasonably can be expected to be established in the next 2-4 years. Our judgment is that confinement 50% better (i.e., $H_H = 1.5$) than the present ITER ELMy H-mode database and stability 50% better (i.e., $\beta_N = 3.5$) than the ITER EDA design basis are plausible. Such performance has already been sustained for several energy confinement times (duration $\sim 5\tau_E \sim 1$ sec), but, for lack of current drive capability and other reasons, such performance has not been sustained for several current relaxation time scales. It is reasonable to design a reduced size ITER to achieve a reduced mission under present ITER physics projections ($H_H \approx 1$, $\beta_N \leq 2.5$) but with the capabilities (e.g., high plasma shaping, and current profile control) to utilize AT performance ($H_H \sim 1.5$, $\beta_N \sim 3.5$) modes to achieve ignition and the full nuclear mission.

2.7.2 Systems and Transport Studies

Any further substantial reduction in the ITER capital cost will be achieved only by reducing some of the mission requirements and/or adopting less conservative physics/engineering guidelines. Systems code studies have examined the options for reduced cost ITER designs. Using ITER EDA technology and physics/engineering guidelines, it should be possible to design a ($Q = 10$, $R = 6.0-6.5$ m, $I = 12-14$ MA, $\kappa \leq 1.7$, $\Gamma_n \approx 0.5-0.9$ MW/m²) device which would be able to explore high-Q, steady-state and AT physics operation, which would have a significant, albeit somewhat reduced, nuclear testing capability and which would have a cost about 60-70% that of the ITER EDA design. Projected performance of such designs under modest AT physics assumptions, such as should be supported by the experimental database within 2-4 years, include ignition and the full nuclear mission capability. Using modest AT physics and more innovative engineering design guidelines results in even smaller size designs, with size and cost saturating at $R \approx 5$ m and 50% of the ITER EDA design cost. The smaller devices have larger divertor heat loads than the ITER EDA design under AT operating conditions.

A series of 1-D transport simulations have been performed to assess the performance of a representative reduced-cost ITER design point. The results indicate that an $R = 6$ m ($Q = 10$) design based on the present ITER ELMy H-mode physics design guidelines is possible. Ignition and full nuclear mission capability are predicted for modest advanced physics assumptions ($H_{97} \geq 1.3$, $\beta_N \geq 2.5$).

2.7.3 Illustrative AT ITER Design Point

An ITER-like conceptual design has been developed at $R = 5.6$ m with an estimated machine cost that is 45% that of the ITER EDA, when full advantage is taken of various mission and engineering implementation cost reductions. It has been demonstrated that the combination of reducing the fusion power to high-Q operation ($10 < Q < 20$) and reducing the shield thickness so that neutron-gamma heating is absorbed inertially in the first layer of the magnet can reduce the size of a next-step "ITER-like" machine to less than half the volume of the current ITER. In this inertial regime, TF insulation radiation allowables would be reached after 60,000 pulses of 300 second duration and overall magnet system refrigerator requirements can be decreased by a factor of four (from 80 kW to 20 kW). A full steady-state regime can be achieved at reduced power, or at full-power as a refrigerator upgrade option. The

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single most important cost reduction is the reduction in physics performance and plasma power, which reduces the cost of a new ITER to 70% of its baseline value. The most important engineering idea for cost reduction and the second most important overall is the reduction of the shield thickness by 34 cm and the radial build by 50 cm. The overall cost savings of adiabatic operation is 19.5%. Cost improvements resulting from the use of more recent conductors, the use of quench detectors as internal dump resistors, and more realistic scaling algorithms for previously fixed costs result in a total potential savings of 55 % relative to the ITER EDA cost.

2.7.4 Device Capability Requirements for Accessing AT Modes

Advanced tokamak operation is a subject of current research. As such, it is difficult to make definitive statements regarding design requirements for advanced tokamak operation in ITER. However, we can point out design features that are likely to be important for advanced tokamak operation. We list these design features in roughly the order in which they must be addressed during the design of a reduced cost ITER.

- (1) Strong Plasma Shaping. That is high elongation ($\kappa_{95} \geq 1.6$), high triangularity ($\delta_{95} \geq 0.3$), and/or lower aspect ratio ($A \leq 3$). Plasma shaping is a strong driver to the design of a reduced cost ITER since the achievable plasma shaping will be determined by basic device parameters [shape of the plasma chamber, location of the divertor(s), location and capability of PF coils] which cannot be modified easily either after construction or even during the detailed design process.
- (2) Internal Control Coils are desirable in that they allow higher elongation. Active control of ($n \neq 0$) resistive wall modes and/or tearing modes might also be achieved with internal coils. Such a system would have to be included in the initial machine design.
- (3) A real-time diagnostic capability for measuring temperature, density, current, and rotation profiles is required for AT operation. Hence, diagnosticians should be involved early in the design process to insure adequate diagnostic access.
- (4) Central Heating and Current Profile Control. Auxiliary heating and current drive systems mainly impact the design of the ports. Neutral beams for current (or rotation) drive require tangential ports, while RF systems require horizontal ports. We commend the good example set in the ITER FDR design, which included many ports for the RF heating and current drive, each with a common interface suitable for any of the candidate systems.
- (5) Pressure Profile Control is the key issue for advanced modes. Schemes for active control of the pressure profile that we are aware of involve controlling transport through control of the velocity profile.
- (6) Rotation Control. Advanced operating modes may require overall plasma rotation for stabilization of the resistive-wall mode and/or neoclassical tearing modes and the introduction of velocity shear to produce (and control) transport barriers. While some progress has been made (particularly with IBW rotation drive), there is still much to learn about rotation drive in tokamaks. A vigorous physics R&D program will be required. A common port interface will allow the system(s) for driving sheared rotation to be added after the physics R&D effort has defined the requirements for rotation drive.
- (7) Central Fueling would allow control the density profile, and thereby the pressure profile. Unfortunately, we do not yet have any proven means of getting fuel to the center of a reactor-like plasma. R&D is required to support inside pellet launch and alternative schemes for central fueling (like compact toroids). The only impact on the device design (as opposed to the supporting R&D program) is a possible increase in demand for port space.

- (8) Advanced Divertor Techniques to allow highly dissipative divertor and/or core plasma operation in regard to confinement quality, tolerable impurity levels, and density limits. High performance core plasmas are likely to call for increased plasma triangularity and perhaps some form of double null operation, features that demand reexamining the divertor solutions that need to be employed.

The full text of these appendices can be found at the Web Site address: <http://nso.ucsd.edu>

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3.0 Modular Program Pathway

3.1 Pathway Overview

The major issues in fusion R&D can be described as: (1) the achievement and understanding of self-heated plasmas with high energy gain that have characteristics similar to those expected in a fusion energy source, (2) the achievement and understanding of sustained self-heated plasmas with characteristics (steady-state or high duty factor pulsed systems) similar to those expected in a competitive fusion system and (3) the development of the nuclear technologies needed for fusion energy sources. These general categories can be used to describe both the magnetic and inertial fusion R&D programs which have historically pursued a modular approach with the individual modules focused on the technical issues described above. The 1995 PCAST review of Magnetic Fusion recommended that the modular strategy be continued with programs and facilities specialized to address the ignition, steady-state and technology issues. This modular pathway, with burning plasma physics as the highest priority element, was the central recommendation of the Gruber FESAC Panel (January 1998) and was the option that was preferred by many of the fusion community researchers at a workshop on approaches to burning plasma physics held in Madison, Wisconsin (April 1998). The continuation of the modular approach for the next major steps in magnetic fusion enhances the likelihood of successfully realizing a viable fusion power source.

The proposed Modular Program Pathway to Magnetic Fusion (Fig. 2.1) would have four major initiatives aimed at: (1) developing innovations in steady-state advanced magnetic confinement configurations, (2) exploration, optimization and understanding of strongly burning plasmas, (3) development of technologies and materials needed to make magnetic fusion an economically and environmentally attractive energy source, and (4) a Strategic Simulation Initiative to facilitate the fundamental science understanding in each of the first three initiatives and to then serve as a mechanism to intellectually integrate the science of these initiatives.

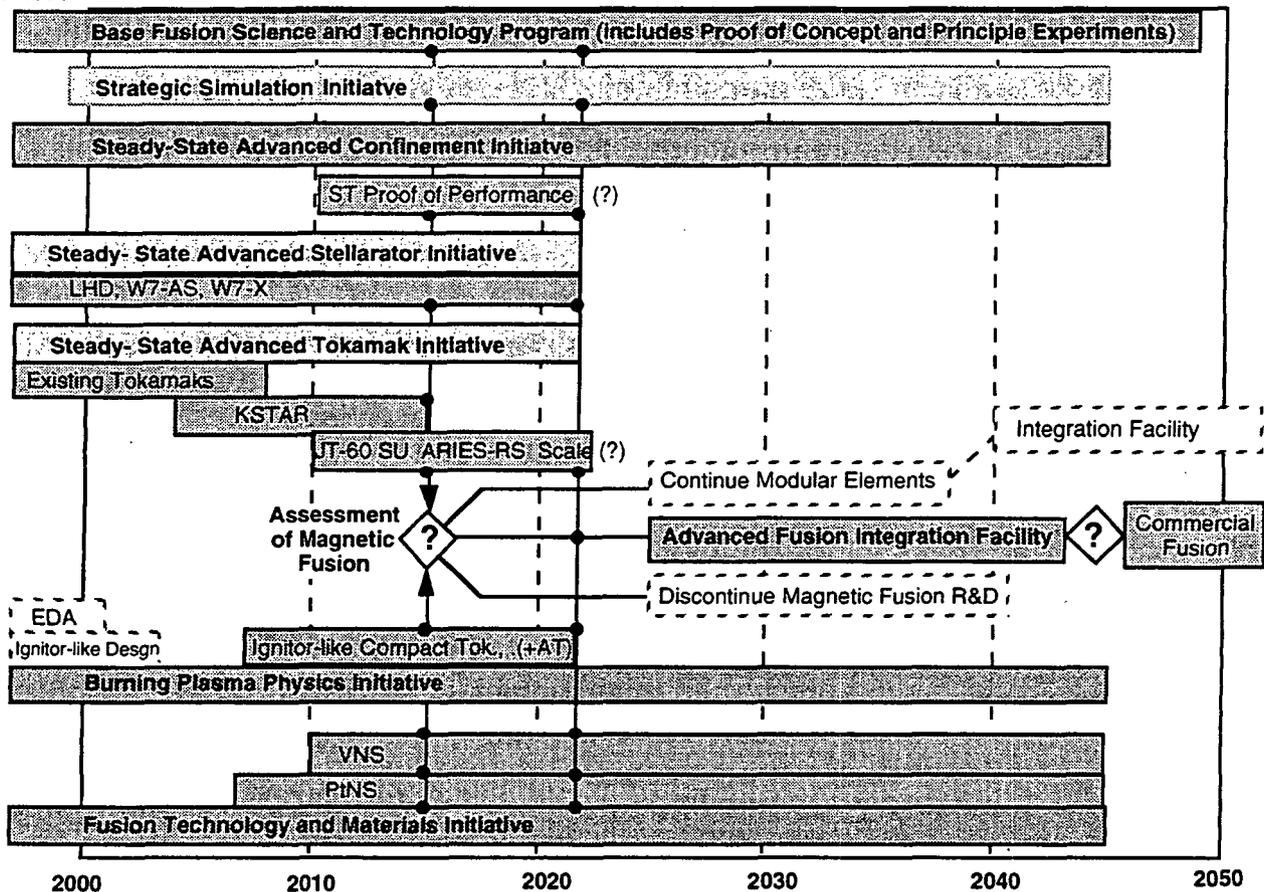


Fig. 3.1 Modular Strategy for the Magnetic Fusion Program (the grey bars correspond to operation)

The Steady-State Advanced Confinement Initiative would be addressed by extensions of ongoing research with advanced tokamaks (DIII-D, C-Mod, JET and JT-60 U), very long pulse superconducting tokamaks (Triam and Tore Supra), new superconducting tokamaks under construction (KSTAR, SST-1), long pulse stellarators (W-7AS), two new \$1B class superconducting stellarators (LHD, W-7X) and new facilities in the spherical torus and advanced stellarator configurations. A possible major new facility in this Initiative is the JT-60 SU, which if constructed would be capable of addressing fully the physics of steady-state advanced tokamak physics at the reactor plasma scale of ARIES-RS. Many of these advanced toroidal configurations are able to take advantage of fundamental toroidal plasma physics that was first developed and understood using the pulsed tokamak as a research tool to access and understand fusion plasma conditions. It is probable that the Steady-State portion of the modular pathway which carries forward the superconducting tokamak development path will be implemented by the machines listed above that will be built outside the United States.

There are currently no facilities in the world magnetic fusion program capable of the study of high-energy-gain, burning plasma issues. TFTR and JET carried out successful initial experiments with weakly burning D-T plasmas that were limited in plasma duration in 1993-97. JET is scheduled to carry out another series of weakly burning D-T experiments near the end of 2002. The TFTR and JET experiments have not only produced D-T fusion plasmas with Lawson parameters ($n_i \tau_E T_i$) within a factor of 10 of that required for ignition but most importantly confirmed that D-T experiments could be carried out safely in the laboratory. The magnetic fusion program is technically ready today to begin construction of a \$1B scale Ignitor-like compact ignition tokamak. The major thrust of the proposed Modular Pathway is to build a burning plasma facility at the earliest possible time as recommended by the Grunder FESAC Panel. The objective for the burning plasma initiative is to achieve, explore, understand and optimize strongly burning plasmas in a toroidal magnetic configuration. An analysis using the present tokamak data base indicates that a compact tokamak configuration would achieve the desired burning D-T plasma performance ($Q \geq 10$) for pulse duration (\gg energy confinement time and \sim plasma current redistribution time)

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needed to satisfy the burning plasma physics objectives. An important characteristic of the compact tokamak is that ignition can be achieved in a physical size much smaller than the final power plant such as ARIES-RS. Therefore, the incremental construction cost of this facility might be minimized to $\sim \leq \$1B$ with construction taking $\sim 7 - 8$ years. The generic toroidal burning plasma physics information from this initiative would provide a foundation for understanding burning plasmas in the advanced tokamak, advanced stellarator and spherical torus configurations.

The Strategic Simulation Initiative (SSI) is a key element of the Modular Strategy. First, the SSI will be a powerful capability in developing the fundamental physics understanding of the Steady-State Advanced Confinement Initiative (Advanced Tokamaks, Advanced Stellarators and Spherical Tori) and in the Burning Plasma Initiative which uses the pulsed tokamak to cost-effectively access burning plasma conditions. The major advantage of the SSI will be to intellectually integrate the fundamental burning plasma physics understanding from the Burning Plasma Initiative and the fundamental physics understanding from the Steady-State Advanced Magnetic Confinement Initiative that will allow the development of an optimized step forward in magnetic fusion, the Advanced Fusion Integration Facility. The SSI is expected, in fact, to play a key role in all three pathways discussed in this report.

The Fusion Technology and Materials Initiative would focus on the critical task of developing and testing advanced materials that would lead to an attractive fusion power plant. An essential capability needed in this area is an intense neutron source capable of irradiating candidate materials to power plant scale fluences. A conceptual design for the Point Neutron Source (PtNS) has been developed through an IAEA collaboration and is estimated to cost $\sim \$0.8B$. A volume neutron source would test larger size ($\sim 10m^2$) sub-components to prior to reactor scale integration and is expected to have a construction cost in the range of $\$1-2B$. Such facilities will be needed in the other two pathways described in this report.

Because the costs of the various facilities all exceed the amount that would be available in the US fusion budget under the present constraints, international collaboration would be required to implement this modular strategy.

The Three Major Fusion Initiatives would be carried forward to a Magnetic Fusion Assessment Check point in ~ 2015 which would review the status of magnetic fusion and decide whether to (1) proceed forward to an Advanced Fusion Integration Facility, (2) extend the Modular phase or (3) move to another innovative confinement concept.

3.2 Rationale for the Modular Program Strategy

3.2.1 Hardware Integration Strategy

The fusion R&D program has used the modular approach for the first decades of research and has understood that these program modules would be integrated near the final stages of fusion development. However, fusion is still in the research phase at this time. Significant progress has been made in producing reactor plasma conditions for short durations in the laboratory that gives encouragement that a solution is possible, but the knowledge base does not exist at the present time to build an attractive fusion power system.

The most efficient approach to pursue fusion R&D objectives at this time is to focus on critical issues in each sub-area, and to develop the knowledge in each sub-area to near that needed for integration at the energy production scale. The advantages of this approach are:

- allows the flexibility needed in a research program,
- reduces cost and time for individual steps, and
- allows innovation to be incorporated earlier.

Systems studies which have been used to evaluate linkages between sub-areas and the Strategic Simulation Initiative will be utilized to provide a "virtual" integration of the research modules. Physical hardware

integration of the three main research modules should be done only when needed to address issues in those sub-areas, or when the status is near reactor levels and integration is the main objective.

Fusion has a particular challenge at this time to not only demonstrate the scientific and technological feasibility of magnetic fusion, but to also develop economically and environmentally attractive fusion power systems. Keys to this are advanced magnetic confinement systems with high fusion gain, high power density and high duty cycle preferably steady-state plasmas, the corresponding enabling technology and the necessary nuclear technologies with attractive environmental characteristics such as low activation and the ability to withstand the neutron fluence. The Modular Program Pathway has focused program elements or initiatives that are targeted on addressing these issues.

3.2.2 Toroidal magnetic confinement systems have generic physics and technology issues, and the pulsed tokamak is an effective tool for developing the generic physics and technology.

Now that fusion plasmas have been produced in the laboratory using the pulsed tokamak configuration, the emphasis is broadening to develop features for improving the characteristics of toroidal magnetic confinement as an economic and environmentally attractive energy source. The advanced tokamak, the spherical torus and the advanced stellarator all emphasize features which are based on the fundamental science of toroidal plasmas developed by the conventional tokamak such as shaping of plasma and magnetic profiles for increased β , utilization of the self-produced bootstrap current to optimize the magnetic configuration, and sheared plasma flows to reduce losses due to plasma turbulence. The initiatives underway and proposed for spherical tori and stellarators emphasize the common features and generic nature of the physics and technology of toroidal magnetic systems. The stellarator will explore configurations with low recirculating power that are expected to avoid plasma disruptions. The spherical torus may offer a cheaper next step in the development path since it allows smaller burning plasma devices than can be built using superconducting coil technology. While the spherical torus and the advanced stellarator are presently not as well developed as the advanced tokamak, they are expected to benefit greatly from the tokamak knowledge and infrastructure base.

Two major issues for toroidal magnetic confinement are: (1) the scaling of confinement in alpha heated plasmas and (2) the effect of dominant alpha heating on the magnetic configuration, plasma energy confinement and potential alpha driven instabilities. The basic physics of these processes has been studied using neutral beams or radio-frequency waves to simulate the effects of alpha heating. Information on the scaling of confinement during strong alpha heating and the magnetic configuration parameters required for ignition is central to developing magnetic fusion. In addition, alpha heating depends on the local plasma parameters, which in turn depend on local plasma confinement and alpha heating. Understanding and controlling this complicated non-linear feedback loop is a critical issue for all advanced toroidal magnetic systems - advanced tokamak, spherical torus and advanced stellarator, and experiments with high gain plasmas are needed. The basic strategy for the Burning Plasma Physics Initiative is to continue to use the pulsed tokamak as a research tool to cost effectively access strongly burning plasmas and to address these fundamental burning plasma issues for all toroidal configurations.

Plasma heating, current drive, fueling, particle and power exhaust are also generic and common plasma technology issues for the advanced tokamak, spherical torus and advanced stellarator. Tritium retention and handling, remote maintenance and blanket technology are closely related nuclear technologies for all toroidal systems as well. Detailed systems studies of potential power plants based on the advanced tokamak, spherical torus and modular stellarator confirms that these toroidal systems are almost identical in their capital cost and cost of electricity (COE), and are very similar in other characteristics such as plasma volume and magnet energy as shown in Table I.

Table I. System studies of advanced tokamak, spherical torus and stellarator power plants. Advanced Reactor Innovation Evaluation Study (ARIES)

	Spherical Torus (A = 1.6)	Modular Stellarator	Advanced Tokamak
Power (Thermal), GW	3.8	2.3	2.6

Power (Net Elec), GW	1.0	1.0	1.0
Capital Cost, \$B(1992\$)	4.3	4.3	4.2
COE, mil/kWh (1992\$)	84	75	76
Plasma Volume (m ³)	581	735	349
Magnetic Energy, GJ	76	80	85
Plasma Current, MA	29	~0	11.3

3.3 Technical Contributions of the Modular Plan

3.3.1 Burning Plasma Physics Initiative

The fusion program needs the capability to extend the frontiers of fusion plasma physics that will enable discoveries in previously unexplored parameter space that have the possibility to lead to more attractive fusion regimes. The coupling of advanced toroidal physics with strongly alpha-heated plasmas is a key issue for the development of attractive toroidal magnetic reactors whether they are classified as advanced tokamaks, spherical tori advanced stellarators or reversed field pinches. The achievement of an ignited ($Q \geq 10$) plasma will allow these scientific objectives to be achieved.

Objectives for the Burning Plasma Physics Initiative

- Determination of the conditions required to achieve high Q energy producing plasmas.
- Control of high Q plasmas through modification of plasma profiles and external sources.
- Determination of the effects of fast alpha particles on plasma stability.
- Sustainment of high Q plasma - high power density exhaust of plasma particles and energy and alpha ash exhaust, some evaluation of alpha heating on bootstrap current profiles.
- Exploration of high Q burning plasma physics in some advanced configurations/operating modes that have the potential to lead to attractive fusion applications.

These objectives would be pursued with a phased operating program very similar to that proposed for the Reduced Cost ITER program.

Phase 0 : Evaluate plasma regimes and confirm hardware capability using deuterium plasmas.

Phase I : Demonstrate, control and optimize strongly burning D-T plasmas ($Q \geq 10$) for an extended duration. Assume base line ITER performance [$H_{95} \sim 0.85 \cdot \text{ITER 93-H (Elm-free)}$, $\beta_N \leq 2.5$] in line with the present conventional tokamak data base.

Phase II: Demonstrate, control and optimize enhanced performance (e.g. gyroBohm) or advanced physics modes (e.g., TPX/ARIES-RS) in strongly burning plasmas for an extended duration.

Phase III: Demonstrate controlled ignition ($Q \geq 10$) and extended burn.

Coupled technology issues (e.g., plasma exhaust/plasma facing components, tritium handling and remote handling) will also be addressed at conditions approaching those anticipated in a fusion system with the exception of some steady-state requirements.

Physics Requirements for an Advanced Burning Plasma Experiment

The physics of a burning plasma can be explored if the parameters listed below are attained.

- | | |
|--|--|
| $Q \geq 10$, $P_{\alpha}/P_{Heat} > 66\%$, | - alpha heating dominant but still easily controlled |
| Burn time $\geq 10 \tau_{\alpha s}$ | - alpha heating, fast alpha effects (e.g., TAE) |
| $\geq 10 \tau_E$ | - pressure profile evolution due to alpha heating |
| $\geq 3 \tau_{He}$ | - helium ash accumulation |
| $\geq 3 \tau_{cr}(\tau_{current\ redistribution})$ | - evolution of bootstrap current |

The initial D-T experiments on TFTR and JET confirmed the single particle confinement requirements for alpha particles and were able to detect weak alpha heating in agreement with expectations (TFTR-1995, JET-1997). At $Q > 10$, alpha heating will dominate the plasma heating and the effect on energy confinement and pressure profile can be determined. The alpha slowing down time is in the range of 0.1 to 0.5 s for the Burning Plasma experiments to be discussed and is sufficiently short so that the alpha distribution is in equilibrium. The energy confinement time ranges from 0.6 to 3 s for experiments to be discussed and is short compared to burn times anticipated. The alpha ash confinement time is expected to range from 4 to 10 τ_E or from 2.4 to 30s. The devices with shorter pulse lengths operate at higher densities which means the τ_E is shorter for the same $n\tau_E$. The normal conductor devices under consideration are expected to have burn times of several helium ash transport times. The current redistribution due to alpha heating modifications of the bootstrap current profile is a key issue for advanced burning plasma experiments. This requirement is more difficult to satisfy and must be determined for each device and specific operating mode. The current redistribution time, τ_{cr} , is $\approx 22 \kappa a^2 T_e^{3/2}$ s where κ is the elongation, a is the minor radius in meters, T_e is the average electron temperature in 10 keV and τ_{cr} is in seconds. The larger devices tend to have longer burn times but τ_{cr} increases at roughly the same rate. A lower temperature obtained by operating at higher density (bounded by the Greenwald limit) allows τ_{cr} to be reduced. For baseline physics assumptions and full magnetic fields the typical pulse lengths correspond to $\sim 1 \tau_{cr}$. Fortunately, as advanced tokamak performance is attained the plasma current and magnetic field can be reduced allowing a very substantial increase in pulse length for normal conductor devices so that pulse lengths of several τ_{cr} can be attained.

Possible Facilities for the Burning Plasma Initiative

A normal conductor burning plasma device has the advantage of providing high magnetic fields and plasma currents at a reduced size and cost relative to a superconducting system since a neutron shield is not required to protect the toroidal and poloidal coils thereby allowing the major radius to be reduced. A significant cost savings is also realized by using copper alloys and inertially cooled cryogenic technologies. The copper coil systems could also allow for stronger and more flexible plasma shaping which is desirable for advanced burning plasma experiments.

A number of copper coil burning plasma devices have been studied including Ignitor (1978-98), CIT (1986-89), BPX(1990-91), BPX-AT(1991-1998) which are precooled to cryogenic temperatures prior to the pulse, HLT(1990) which was actively cooled with LN and PCAST(1996) which was actively cooled with water. The general physical parameters of these devices are summarized and compared with ARIES-RS and ITER-EDA in the following table. Projections of D-T performance for these facilities using the same methodology as ITER RC is given in a later section.

Table II. Parameters of Burning Plasma Facilities

	R (m)	a (m)	κ	A	B (T)	W_{mag} (GJ)	I_p (MA)	FlatTop (s)	Pfus (MW)	Cost (\$B)
TFTR	2.5	0.9	1.0	2.8	5.6	1.5	2.7	2	11	~1
JET	2.85	0.85	1.6		3.8	1.5	4.2	5	16	~1
Ignitor	1.32	0.47	1.85	2.8	13	4	12	5	150	0.5

Spherical Torus	1.05	0.75	3.0	1.4	2.6	0.5	16	steady	800	0.8
BPX-AT	2.00	0.50	2.0	4.0	10	4	6.25	12	150	0.70
CIT	2.14	0.65	2.0	3.3	10	4	11	10	150	0.73
BPX	2.59	0.79	2.0	3.3	8.1	9	10.6	10	400	1.63
Gyro-Bohm JET	3.0	1.0	2.0	3	6		10		360	~3
HLT	3.40	1.20	2.0	2.8	8	15	20	40	500	NA
PCAST	5.00	1.50	1.75	3.3	7	35	15.3	120	400	5.3
ARIES-RS	5.52	1.38	1.7	4.0	7.98	85	11.3	steady	2170	4.6
ITER-EDA	8.14	2.80	1.6	2.9	5.68	120	21	1000	1500	10.7

The cost estimates are in FY1995\$ and are for the construction project not including site costs.

The construction costs of fusion confinement facilities are a strong function of physical size, plasma current and energy stored in the magnets as suggested in Table II. An empirical fit to the cost estimates for several devices yields $\text{cost} \sim B^{0.87} R^{1.85}$ (J.Schmidt, 1995 PCAST Machine Study) and Aamodt (Madison Forum) has found a similar relation with $\text{cost} \approx 25BR^2$. The costs for CIT and BPX are the cost estimates at the end of their Preliminary Design Phase escalated to FY1995\$. The BPX-AT cost estimate is based on a Conceptual Study during the 1991 New Initiatives Task Force activity and was a detailed cost estimate based on scaling down from BPX. The Ignitor cost estimate of \$0.5B is on the high end of various cost "estimates". Ignitor, CIT, BPX-AT and BPX were costed on the basis of siting at an existing large tokamak or equivalent site. The PCAST and ITER cost estimates do not include site costs outside the fusion facility and its direct facilities costs. The ARIES cost is the total direct cost for the 10th of a kind. Consideration should be given to special studies that look for new design features or manufacturing techniques to reduce the unit construction costs for the tokamak core. The general conclusion is that compact tokamak facilities (Ignitor, BPX-AT and CIT) with major radii $\leq 2\text{m}$ have construction costs in the $\leq \$1\text{B}$ class, while larger superconducting devices necessarily have major radii $\geq 5\text{m}$ with costs in the \$5B range.

Estimated Fusion Performance of Normal Conductor Burning Plasma Experiments

The performance of PCAST, Ignitor, CIT, BPX and BPX-AT was estimated using a zero-D model assuming ITER-93H (ELM-free) confinement scaling with alpha heating and fuel depletion due to alpha ash accumulation calculated self-consistently. The plasma profiles were taken to be the same with a flat density profile ($\alpha_n = 0.1$) and modestly peaked temperature profile ($\alpha_T = 1$). The impurity levels were taken to be 3% Be and the alpha ash was assumed to have a confinement time $\tau_{\text{He}} = 5 \tau_E$ resulting in $Z_{\text{eff}} \approx 1.5$. These assumptions are the same as the modeling assumptions made for the Reduced Cost ITER with the exception that ITER-RC assumes $\tau_{\text{He}} = 10 \tau_E$ for the baseline performance mode and $\tau_{\text{He}} = 5 \tau_E$ for advanced performance mode. For BPX-AT at $Q = 10$ increasing τ_{He} from 5 to 10 τ_E increases the required H93-Elmfree H factor by 10%. Some of these calculations are summarized in Fig. 3.2 below.

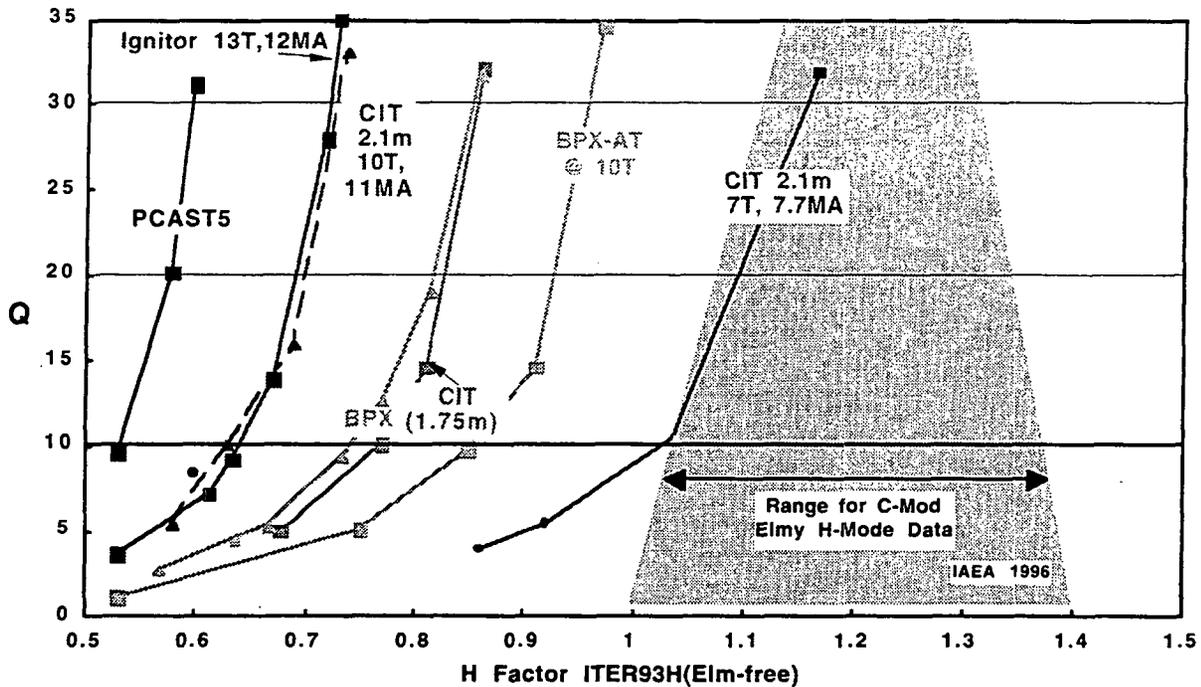


Fig. 3.2. Fusion power gain estimates for several burning plasma experiments.

The existing confinement data base from all tokamaks is centered about an ITER93(Elm-free) H factor of ≈ 0.85 while the confinement data from Alcator C-mod, a prototype for the compact ignition tokamaks, has ITER93(Elm-free) H factors of ≈ 1.2 . Ignitor and CIT are projected to ignite for ITER93(Elm-free) H factors of ≈ 0.85 which is the same requirement as for ignition in the ITER-EDA. Other Ignitor-like devices with somewhat lower field and plasma current such as BPX-AT are projected to achieve $Q \geq 10$ ITER93(Elm-free) H factors of ≈ 0.85 and would ignite at ITER93(Elm-free) H factors less than those achieved in Alcator C-mod. Note that the full field performance of Ignitor and CIT(2.1m) is nearly the same suggesting that Ignitor would ignite at $\sim 70\%$ of full field and current if the enhanced Alcator C-Mod performance could be attained in D-T. At this reduced field, the flattop of Ignitor would be extended from 5 seconds to 15s.

Compact Ignition Tokamaks

The compact tokamaks (e.g., Ignitor and BPX-AT) first advocated by Coppi with higher magnetic field and higher plasma densities have additional advantages with respect to beta limits, operating density limits, impurities and fast alpha particle limits and are well suited for studying burning plasma physics during the time scales of interest. Recent results from tokamak confinement experiments, in particular Alcator C-Mod, confirm the high field compact ignition tokamak design assumptions with regard to confinement, ICRF heating, power handling and impurity control. Alcator C-Mod with ELMing H-modes tends to operate with 20-30% higher confinement relative to ITER93H scaling than larger lower field lower density tokamaks and provides confidence that Ignitor-like compact tokamaks would achieve the performance required to achieve the burning plasma objectives.

Detailed engineering designs have been carried out for copper alloy coils cooled to cryogenic temperatures and significant operating experience has been obtained. Ignitor is designed to have copper alloy coils that are precooled to 30 °K with liquid hydrogen while BPX-AT, CIT, BPX designs have copper alloy coils that are precooled by LN to 77 °K. The pulse length is determined by the adiabatic temperature rise of the conductor/structure thermal mass during the pulse. A small reduction in the peak coil current allows the pulse length to be increased dramatically for the coil/cooling configuration. For example, reduction of the magnetic field in BPX-AT from 10 T to 7 T allows the magnetic field flat top to be extended from 12 s to 56 s. This feature of Ignitor-like compact tokamaks can be used to advantage for studying advanced tokamak regimes where improved confinement

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and β allow the plasma current and magnetic field to be reduced as shown in the example below for BPX-AT (Fig. 3.3). In order to exploit this capability the initial design would need to incorporate or allow upgrades for active cooling of internal divertor components (as in BPX-AT) and techniques to pump helium ash during the pulse.

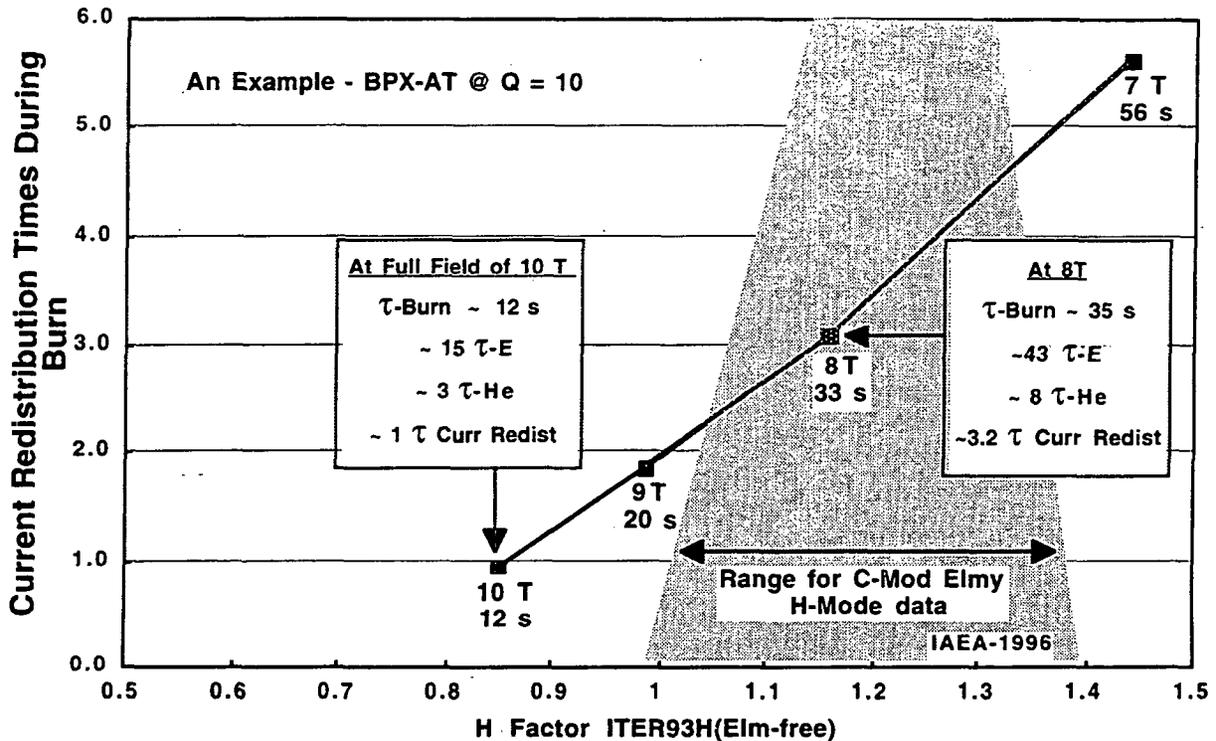


Fig. 5 Capability of an inertially cooled Ignitor-like tokamak to produce pulses with long burn (magnetic field flat top) times that would allow several plasma current redistribution times for studying advanced modes. Compact helium ash pumping systems would have to be provided in the divertor for the long pulses available.

An important point is that the Ignitor-like compact tokamaks can explore a broad range of experimental operating space by density variations and by reduction in the magnetic field/plasma current since they have large margins with regard to density, MHD beta and TAE limits.

Alpha Physics Considerations for the Burning Plasma Physics Initiative

After alpha heating, the primary alpha physics issue concerns alpha-driven toroidal Alfvén eigen (TAE) mode instabilities which could cause the loss of energetic alpha particles before effective alpha heating had occurred. Ideally, the burning plasma experiment should be able to avoid these instabilities while achieving high Q and to then controllably approach the stability boundary to determine the physics constraints for future devices. The TAE instability occurs when the alpha particle speed, V_{α} , is \geq the Alfvén speed, $V_{\text{Alfvén}}$, and $(R/a)\nabla\beta_{\alpha}$ exceeds a threshold that depends on details of the plasma and magnetic profile (e.g., β and shear). It can be shown that β_{α} depends on $\beta T_e^{5/2}$ and the temperature can be varied by adjusting the plasma density. The curves in Fig. 3.4 are density scans for 0-D calculations where Q was held constant (by adjusting H) while maintaining constant helium ash and impurity fractions. The Ignitor-like compact tokamaks can scan the same general range of TAE instability parameters space as the ITER-EDA and PCAST devices.

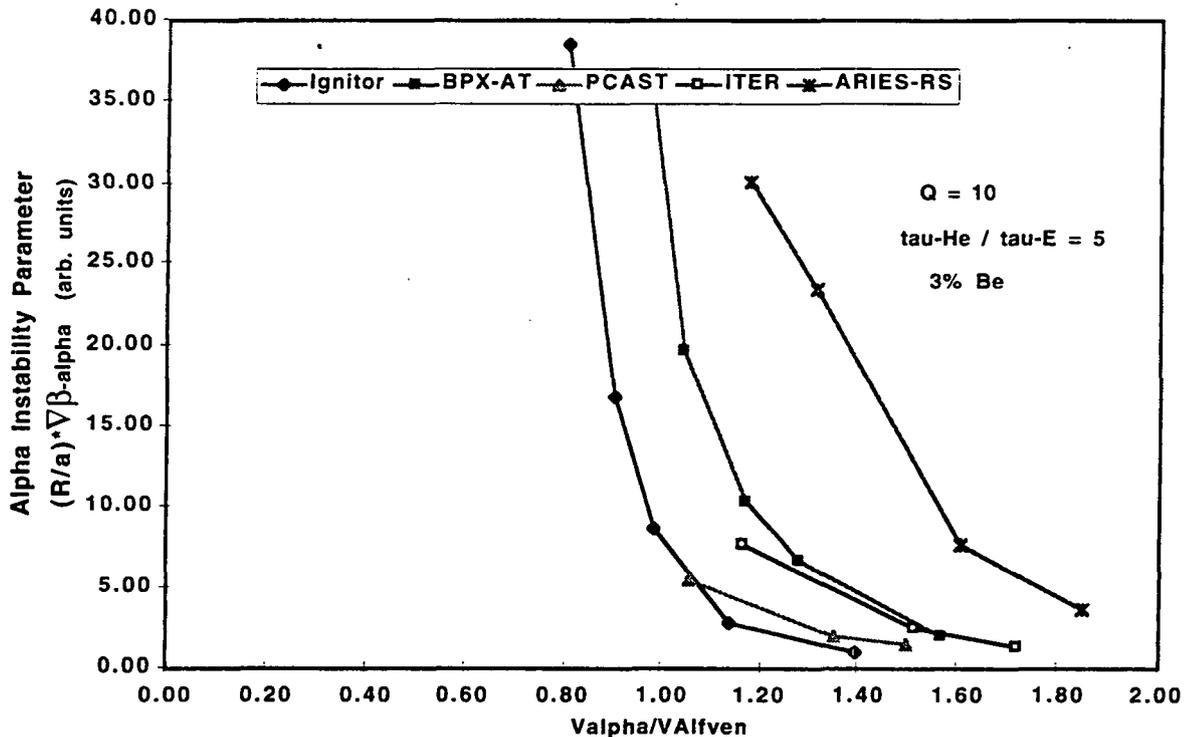


Fig. 3.4. Comparison of parameters that determine the instability boundary for alpha-driven toroidal Alfvén eigen (TAE) modes.

Intermediate Sized Ignition Experiment based on Gyro Bohm Scaling

A moderate size normal conductor tokamak ($R = 3\text{m}$, $B \sim 6\text{ T}$, $I_p \sim 10\text{ MA}$) experiment based on extrapolation of DIII-D/JET gyro-Bohm scaling experiments has been proposed. This physics mode is not considered to be an advanced tokamak mode, such as the reversed shear mode, and therefore enhanced performance is achieved without the need for strong active plasma profile control. The advantage is a more robust (i.e., reliable) plasma configuration with potentially fewer plasma disruptions. A concept study for a similar size tokamak has been carried out for a high-performance long-pulse tokamak (HLT) with parameters as shown in Table II. The HLT design, which is also inertially cooled, increases its adiabatic pulse length by using the thermal mass of an external liquid nitrogen reservoir which is initially subcooled to 63.5 °K prior to each pulse. The toroidal field coil and central solenoid are actively cooled during the pulse by circulating the LN through the cooling channels. This design allows the pulse length to be arbitrarily extended by enlarging the liquid nitrogen reservoir, but reduces the maximum attainable magnetic field to accommodate the cooling channels. Active LN cooling of this type is being evaluated as a possibility for extending the pulse length on compact tokamaks as well as moderate scale tokamaks. Further work is needed in a multiple machine program to test the gyro-Bohm scaling and to investigate pedestal scaling especially at higher β_N .

Findings on the Burning Plasma Physics Initiative

1. The compact high field tokamak utilizing cryogenic normal conductors is a potential pathway to access $Q \geq 10$ conditions and address burning plasma physics with a facility costing $\leq \$1\text{B}$. In addition, evaluation of burning plasma physics in an advanced configuration for up to several skin times is possible.

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2. Technological issues associated with the longer pulses (e.g. helium pumping and other internal components) in a compact high field tokamak need more detailed analysis and should be updated to include recent results.
3. Intermediate size (JET-scale) normal conductor tokamaks offer interesting possibilities for burning plasma physics research with costs in the ~\$1.5B range.
4. Larger burning plasma tokamaks utilizing superconducting coils while somewhat more capable have costs in the several \$B range.

3.3.2 Steady-state Advanced Confinement Physics Program Initiative

Objectives of the Steady-state Advanced Confinement Physics Initiative

The development, exploration and detailed understanding of high confinement, high fusion power density and high duty-cycle (steady-state) plasmas is needed for the development of economically and environmentally attractive applications of fusion power. This initiative includes subprograms on steady-state advanced tokamaks, steady-state advanced stellarators and other Proof of Performance experiments that might emerge from the ongoing Base Fusion Science and Technology Program such as the Spherical Torus.

Steady-state Advanced Tokamak Program Initiative

Outstanding progress in exploring and understanding advanced plasma regimes for short pulse magnetically confined plasmas has been made by a number of specialized medium-sized and several large size pulsed tokamaks. The next frontier in the advanced tokamak configuration is to extend high performance advanced tokamak regimes to near steady-state conditions in fusion-relevant plasmas so the plasma physics (with the exception of actual alpha physics) of a magnetic fusion plasma can be understood and optimized. The strategy is to use H and D plasmas rather than D-T plasmas to increase experimental flexibility and to reduce costs associated with tritium handling and neutron activation that are present at burning plasma facilities.

The steady-state advanced tokamak program will be dedicated to the development of the scientific basis for a compact and continuously operating tokamak fusion reactor. It will explore techniques for optimizing steady-state plasma performance through active control of the current profile, the pressure profile, the radial electric field profile, transport barrier formation, of plasma-wall interactions, and by advanced plasma shaping. Key areas to be optimized are the averaged plasma pressure (or β) through wall stabilization, the plasma confinement through transport barriers, and the current drive efficiency. This will involve making efficient use of the self-driven bootstrap current to provide a substantial fraction of the total plasma current. The integration of optimized plasma performance and efficient continuous operation will be a key issue, as will be control of major plasma disruption. Advanced tokamak operation will be at reduced current levels compared to conventional tokamaks and this will reduce the impact of disruptions.

Requirements for the Next Step Steady-state Advanced Tokamak Initiative

The plasmas needed to resolve the steady-state advanced confinement issues should have physics phenomena similar to that projected for a fusion plasma. This requirement would be satisfied if the dimensionless plasma parameters ρ^* , ν^* and β^* are comparable to those in a strongly burning (e.g., $Q > 10$) D-T plasma. Kadomtsev (Nuclear Fusion 1975) has shown that plasmas can have the same ρ^* , ν^* and β^* if they have the same similarity parameter, $Ba^{5/4}$. In addition, the pulse length must be sufficient to allow a thorough study and controlled modification of plasma current evolution in advanced tokamak modes (e.g., pulse length $\geq 10 \tau_{cr}$), and to allow the plasma wall/divertor interaction to come into equilibrium. Some aspects of alpha particle physics (e.g., TAE modes in reversed shear magnetic configurations) can be simulated using the injection of high power beams and RF power. The physics issues and criteria have been previously discussed in detail in the Tokamak Physics Experiment (TPX) conceptual design. (e.g., IAEA paper 1994)

In addition to having dimensionless physics parameters similar to those in a strongly burning plasma and having long pulses for current control and wall equilibration, there are several other physics issues for this initiative. An advanced flexible divertor configuration is required to study the interplay between edge conditions needed for enhanced confinement modes and the requirements for heat dispersal and limited recycling back to the main plasma. The poloidal field system and divertor configuration must allow a wide range of plasma shapes and plasma current profiles to be explored. In addition, detailed steady-state current profile control is needed, necessitating the requirement for multiple current drive systems. Plasma diagnostics are particularly important in this experiment, both for fundamental understanding and for active control.

Possible Facilities for the Steady-State Advanced Tokamak Initiative

During the next 5 years the existing tokamak facilities will be exploited to address the steady-state advanced tokamak issue over the parameter space available to those devices. A number of facilities could be upgraded to extend their capabilities in this program element. However, significant new facilities are needed in the world program to fully address this issue for an advanced tokamak reactor as exemplified by ARIES-RS. The possibilities that have been considered are listed in Table III. The parameters shown are the nominal full field parameters with the exception of Alcator C-Mod.

Table III. Parameters of Steady-State (long-pulse) Advanced Confinement Tokamaks*

	R (m)	a (m)	κ	A	B (T)	a/ρ^* ARIES-RS	I_p (MA)	τ_{cr} (s)	FlatTop (s)	Cost (\$B)
Alcator C-Mod (Cu)	0.67	0.21	1.8	3.2	5	0.15	1	0.5	5	
DIII-D (Cu)	1.67	0.67	1.8	2.5	2.2	0.23	2	3.2	5-10	
JET (Cu)	2.85	0.85	1.6	2.8	3.8	0.41	4	10	10	~1
JT-60U (Cu)	3.1	0.9	1.7	3.5	4.1	0.45	4	14	10	~1
Triam (S/C)	0.8	0.12	1.5	6.7	8	0.13	0.03	0.2	7200	
Tore Supra (S/C)	2.3	0.80	1.0	4.0	4.2	0.42	2	5.6	120	0.5
SST-1 (S/C)	1.05	0.20	1.8	5.3	3	0.10	0.22	0.3	60	0.2
KSTAR (S/C)	1.8	0.50	2.0	4.0	3.5	0.25	2	3.2	300	~0.4
TPX (S/C)	2.25	0.56	2.0	4.0	3.35	0.27	2	4.3	1000	0.8
JT-60SU (S/C)	5.00	1.40	1.7	3.5	6.25	0.86	10(5)	64	1000(ss)	~3
ARIES-RS (S/C)	5.52	1.38	1.7	4.0	7.98	1.00	11.3	83	steady	4.6
ITER-EDA (S/C)	8.14	2.80	1.6	2.9	5.68	1.44	21	280	1000	~10

The cost estimates are in FY1995\$ and are for the construction project not including site costs.

The ability to address advanced tokamak issues depends on the range of dimensionless parameters that can be accessed. Table III assumes that β/v^* was constant for all devices when calculating the number of gyro-radii, a/ρ^* , and when calculating the plasma temperature to determine the current redistribution time, τ_{cr} . KSTAR and TPX would extend the plasma duration of JET/JT-60U plasma regimes with modest ρ^* , v^* and β^* ($a/\rho^* \approx 0.2$ to 0.4) from ~ 1 current redistribution time to $\gg 10$ current redistribution times allowing current profiles to approach equilibrium. JT-60SU would have the capability to address the steady-state advanced tokamak issue fully with a plasma duration of $14 \tau_{cr}$ at nearly ARIES-RS values for ρ^* , v^* and β^* .

An important point to note in this regard is that JT-60 SU has almost the same dimensional as well as dimensionless parameters as ARIES-RS, and hence one would expect the costs (exclusive of D-T and nuclear technologies) to be comparable. If the requirement is to study advanced tokamak physics at ARIES-RS dimensionless parameters then the test plasma must have $BR^{5/4} \approx$ to ARIES, and since the rough cost scaling for both superconducting and copper machines is $\text{cost} \sim B^{0.87}R^{1.85}$ (J. Schmidt), then the cost of the test plasma device will scale roughly as $\text{cost} \sim (BR^{5/4})^{1.48}/B^{0.61}$. A possible cost reduction could be realized by increasing the magnetic field and decreasing the major radius.

The non-burning steady-state advanced confinement experiments (e.g., JT-60 SU) would need extensive plasma heating power (~60 MW) to bring the reactor size plasmas to high temperature and flexible high power current drive systems. The high-performance long-pulse tokamaks would initially operate with hydrogen, and later transition to deuterium operation to minimize neutron activation of the structure near the plasma. KSTAR, TPX and JT-60 SU are designed with modest shielding between the plasma and the superconducting coils, and have incorporated low activation materials into the mechanical structure near the plasma.

Integration of the Burning Plasma and Steady-state Advanced Confinement Initiatives

The programs of the Advanced Burning Plasma experiment and the Steady-State Advanced Confinement initiatives would be coordinated and integrated programmatically. A comparison of plasma relevant parameters for possible devices in the two programs are shown in Table IV.

Table IV. Comparison of Potential Elements for Advanced Burning Plasma and Steady-state advanced confinement for the Next Step Modular Program.

	R (m)	a (m)	B (T)	Ba ^{5/4}	I _p (MA)	τ _{cr} (s)	Flattop (s)	Q _{DT}	P _{Fusion} (MW)	Cost (\$B)
Ignitor (full B)	1.32	0.47	13	5.1	12	8.4	5	ign	150	~0.4
BPX-AT (full B)	2	0.5	10	4.2	6.25	9.7	12	≥10	150	
BPX-AT (80%B)	2	0.5	8	3.4	5	7.7	33	≥10*	150	0.7
PCAST	5	1.5	7	11.6	15.3	85	120	ign	400	5.0
ARIES-RS	5.5	1.4	7.98	11.9	11.3	83	steady	ign	2150	4.6
JT-60 SU	5	1.4	6.25	9.5	10	64	1000	(5)*	(250)	~3
TPX	2.25	0.56	3.35	1.6	2	4.3	1000			0.8
KSTAR	1.8	0.5	3.5	1.5	2	3.2	300			~0.4

* JT-60SU and BPX-AT with H_{ITER-89P} ≈ β_N ≈ 3. In this table, the temperature was varied to keep β/v* constant for all devices. The plasma temperature for ignited plasmas in Ignitor can be reduced below this value by increasing the plasma density resulting in shorter current redistribution times at lower β/v*.

The burning plasma experiments can extend somewhat into advanced regimes while the advanced confinement experiments would extend into some limited burning plasma studies. JT-60 SU has considered a D-T option with additional shielding which could achieve Q ~ 5 (P_{fusion} ~ 250 MW) if H_{ITER-89P} ~ 3 and β_N = 3 could be attained. This direct integration of burning plasma physics and advanced tokamak physics is an important feature that gives additional flexibility and breadth to the modular approach.

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3.3.3 Strategic Simulation Initiative

The Fusion Energy Sciences leg of the Strategic Simulation Initiative will provide additional capability to integrate these two initiatives, and to transfer the generic information from the tokamak initiatives to the advanced stellarator and spherical torus confinement initiatives. This Strategic Simulation Initiative will develop and employ advanced computational methods and multi-teraflop computing resources to greatly accelerate the development of a comprehensive simulation capability for magnetically confined fusion plasmas. The elements of the Simulation Initiative include both microscopic modeling of plasma turbulence that leads to anomalous energy transport and macroscopic modeling of global plasma instabilities that lead to plasma disruptions. By calibrating this simulation capability against both the burning plasma experiment and the steady-state advanced confinement experiment, we will gain the confidence needed to use this as a design integration capability for use in defining the optimum advanced magnetic configuration and in designing the next step integration experiment. To the extent that these devices allow additional direct integration of burning plasma physics and advanced tokamak physics, it will be possible to perform additional calibrations with this simulation capability to gain additional confidence in their projections to the next step.

3.3.4 Fusion Materials and Technology

Fusion Technologies

Technology and materials will be the eventual keys to the realization of fusion's potential as an energy source. Achieving acceptable performance for a fusion power system in the areas of economics, safety and environmental acceptability, is critically dependent on performance of the blanket and divertor systems which are the primary heat recovery, plasma purification, and tritium breeding systems. A fundamental understanding of key materials and technology issues is required to develop a compatible combination of materials (breeder, coolant, and structure) for the blanket system. Key issues include chemical compatibility, neutronics, thermohydraulics and stress conditions in addition to the irradiation effects and tritium interactions. The development of blanket systems require integrated prototype module testing in a 14 MeV environment. Such testing could be conducted in the proposed Advanced Fusion Integration Facility, or in a smaller dedicated D-T test facility such as the Volume Neutron Source (VNS).

Materials

Design and performance of these key components rests in turn upon the properties and characteristics of the structural materials. Temperature limits imposed by the properties of materials are the major limitation in the quest for high thermal efficiency. The major in-vessel systems will have a finite lifetime and will require remote maintenance and replacement. Reliability and lifetime, which are primarily determined by the performance of materials, will have a significant impact on plant availability, a second major factor in the cost of energy equation. Mechanical properties of structural materials under conditions associated with off-normal events, radioactive isotope inventory, and release paths are key considerations in designing for safety. The initial levels of radioactivity of materials on removal from service and the rate of decay of the various radioactive isotopes, dictate acceptable storage and disposal methods and the possibility of recycle of materials, both being major considerations in the environmental acceptability of fusion. The development of low activation materials is a long-term endeavor and a critical issue on the path to fusion energy. Fission reactors are currently used for irradiation studies. However, a 14 MeV point neutron source (PtNS) will ultimately be needed to fully qualify materials for fusion reactor applications. Conceptual design of such a materials test facility is being pursued as part of an international collaboration.

Technology also plays a vital role in the continued progress in fusion science. Exploitation of existing and planned experiments will depend on further advances in certain technologies, particularly those related to manipulation and control of the plasma, extension to longer pulses, and operation in a radiation environment. Within the context of the fusion program's goals to develop a low-cost, next-step device and the knowledge base for a more attractive fusion power source, the likely reduction in the size and complexity envisioned to accomplish these objectives will require improved technologies to handle higher heat loads, produce lower cost magnet designs, develop safe and efficient tritium processing systems and develop more efficient and flexible heating, current drive, and fueling systems.

Synergy Between the Inertial Fusion Energy (IFE) and Magnetic Fusion Energy (MFE) Programs

The ICF community is currently developing an integrated plan for the development of all aspects of IFE including drivers, targets, target fabrication and injection, chambers, and final optical systems that will survive in a fusion environment. There are three driver options: heavy ion accelerators, diode-pumped solid state lasers and krypton fluoride lasers. The proposed plan calls for the construction of an Integrated Research Experiment based on one of these drivers beginning in FY 2003. The potential exists for synergy between magnetic and inertial fusion energy (IFE) technology development. Several of the technologies under development by the MFE program are applicable to IFE such as:

- Pellet/target technology and delivery systems
- PFC liquid metal surfaces / IFE chamber technology
- Tritium systems
- IFE driver technology
- Materials (radiation effects)

A natural overlap between MFE and IFE is that of liquid metal surfaces for the IFE chamber wall protection which is also applicable to divertor plate protection in MFE devices. The benefits of coordination in technology development are twofold; namely, (1) both programs can leverage technology advances made in each other's field of research and (2) a reduction in duplication of effort will lead to substantial cost savings in future development programs.

Major Technology Facilities

Fusion materials must operate in a very demanding environment which includes various combinations of high temperatures, chemical interactions, time dependent thermal and mechanical loads, and intense 14 MeV neutron fluxes. Neutron irradiation is a particularly important issue, due both to its effects on physical and mechanical properties as well as the production of radioactive materials, and is the most difficult to investigate with currently available facilities.

At present fission reactors are the primary means to investigate the effects of irradiation on fusion materials. In addition there are specialized studies which can be done in ion beams (REF). However the response of materials to these various radiation fields can be quite different from that due to a fusion neutron spectrum. Various techniques have been used to more nearly reproduce the fusion environment, but a 14MeV neutron source will ultimately be needed to develop and qualify fusion materials. The source must reproduce the fusion spectrum, particularly in terms of the ratio of transmutation products to atomic displacements, and must have flux and fluence capabilities sufficient to allow accelerated testing to fluences up to end-of-lifetime. The international community has proposed as a Point

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Neutron Source an accelerator facility based on the D-Li interaction (IFMIF). The characteristics of the IFMIF are summarized in Table V. The Final Report of the Conceptual Design Activity estimates the cost of the facility to be ~0.8B\$. Development and qualification of structural materials in such a device would be required before embarking on a high-fluence fusion-based system.

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Table V. Point Neutron Source Parameters

Neutron Flux	Volume	> 1 MW /m ²	>6.0 liter
		> 5 MW /m ²	0.4 liter
		> 20 MW /m ²	0.1 liter
Particle		D+	
Beam Current		2@125mA	
Beam Energy		32, 36 or 40 MeV	
Beam spot		5 cm x 20 cm	
Duty Factor		100 % (CW)	
Plant Factor		70 %	

The technologies needed to build a source like IFMIF are essentially in hand, and share much in common with high-power spallation sources, and the APT program. The primary uncertainties relate to the high reliability and availability requirements.

Engineering testing of nuclear components

The overall goal of the fusion technologies area is to develop a blanket system that meets the performance requirements of tritium self-sufficiency and efficient energy recovery, and at the same time achieves the desired level of safety and environmental attractiveness. While much work can be done without a high flux neutron environment, development and testing of fusion blankets and other nuclear components will eventually require a large volume plasma-based neutron source. The required technical features, summarized in Table VI, are taken from the IEA Study Summary which describes requirements for testing in fusion facilities, with emphasis on testing needs to construct a DEMO blanket.

Table VI. Fusion Nuclear Technologies Testing Requirements

Neutron Wall Load, MW/m ²	1 - 2
Plasma Mode of Operation	Steady-State
Minimum Continuous Operating Time, Weeks	1 - 2
Neutron Fluence (MW·y/m ²) at Test Module	
Stage I: Initial Fusion "Break-in"	0.3
Stage II: Concept Performance Verification	1 - 3
Stage III: Component Engineering Development and Reliability Growth	4 - 6
Total Neutron Fluence for Test Device, MW·y /m ²	> 6
Total Test Area, m ²	> 10

Fusion nuclear systems could be tested in any sufficiently sized facility, such as the Advanced Fusion Integration Facility, to demonstrate performance and reliability and activation features. However, the risk and cost of such tests are dependent on the cost and scale of the facility, leading to the idea of smaller options tailored to the testing task. A number of design studies have been performed that address the concept of VNS-like facilities, which are typically highly driven devices with $Q \sim 1-2$, and water cooled magnets. Examples include the Steady Burn Experiment work at MIT, the Spherical Torus concept at ORNL, the Small Fusion Development Plant at ORNL and two Small Business Innovation Research Projects focusing on Spherical and Conventional Tokamak confinement.

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A related but differently scoped facility, called a Pilot Plant, was pursued in 1988/89. These device concepts generally are small (e.g., R~2 meters or less), low power (e.g., 100 MW), normally conducting, jointed magnet tokamaks where the designs emphasize maximum access to the fusion core. Costs have been estimated in the 1-2B\$ range. While the plasma performance required is less than that for a high-gain burning plasma facility, significant technology and materials development would be needed before embarking on this step.

3.4. Pathway Implications for the Modular Plan Option

The elements of a Road Map for magnetic fusion based on the modular approach are shown in Fig. 3.1. The burning plasma (BP), steady-state (SS) and nuclear technology mission elements of ITER are separated and addressed by specialized focused modular initiatives. The modular initiatives have greater flexibility to explore and exploit innovations to improve the attractiveness of magnetic fusion. For example, the burning plasma physics element can begin studies of more conventional regimes and then incorporate advanced tokamak physics into the later phases at lower cost. In addition, the steady-state non-burning plasma element is relieved of nuclear technology constraints and has much more flexibility to pursue experiments dedicated to developing innovative advanced tokamak concepts. This pathway also allows the alternate magnetic configurations to be evaluated at the Proof of Performance level before the specifications of the Advanced Fusion Integration Facility are determined.

While both the steady-state advanced tokamak and the burning plasma physics elements will develop significant enabling technologies, this plan explicitly calls for facilities dedicated to the development of fusion technologies and materials. The need for a VNS has been identified by several studies (Finesse, IEA) which called for the capability to test components with ~10m² exposed to neutron fluxes of ~2 MWm⁻² and fluences up to 6 MWam⁻². In the modular plan, the focus is toward integrating advanced plasma physics, nuclear technology and advanced materials in an Advanced Fusion Integration Facility thereby allowing time for materials to be tested in the PfNS at high fluence.

The mission for facilities that follow the next stage of fusion R&D is a significant issue. The facility following the Next Steps is identified in Fig. 3.1 as a Advanced Fusion Integration Facility which would integrate and demonstrate all technologies and operating scenarios required for an attractive fusion power plant, and would therefore provide the scientific and technological foundation for a commercial fusion power plant. It is anticipated that the Advanced Fusion Integration Facility, which could be more advanced than ITER-RC, would demonstrate the entire fuel cycle and perhaps the production of electricity. The following Commercial Prototype would in turn be somewhat more advanced than the DEMO in the ITER-RC strategy and would have the important mission of demonstrating the high reliability needed for a commercial fusion power plant.

3.5 Advantages and Concerns for the Modular Program Pathway

The Modular Program Pathway follows from the viewpoint that magnetic fusion is still a research program and is not yet ready for a development step at the scale of a fusion power plant. The advantages of the Modular Program Pathway described in Section 3.1 include:

- The Modular Pathway allows the rapid development of toroidal ignition and burn physics in the only toroidal confinement device presently capable of reaching that physics regime – the pulsed tokamak. This can be done less expensively and with greater assurance of success than in an integrated facility such as ITER because it does not have to carry out other elements of the program.
- The critical magnetic fusion issues are naturally modular and are maturing on different time scales. The Modular Pathway allows programs and facilities to be optimized with focus on specific

technical issues and to have different time scales for resolution. The steps are more appropriate to our current level of knowledge than the "single step to DEMO" pathway.

- The Modular Pathway allows greater flexibility to explore and accommodate innovations and optimization of individual elements prior to a large scale integration facility. The Advanced Fusion Integration Facility (AFIT) would have more attractive features and less technical risk than the single step approach. Additionally, should the AFIT be designed to produce electric power, it could be considered a DEMO.
- The technical basis exists now to proceed with the burning plasma, advanced tokamak and materials development initiatives described. The Modular pathway allows for early investigation and resolution of burning plasma physics, steady-state advanced confinement physics and fusion materials and technology issues.
- The "advanced" tokamak is a very cost effective research tool for advancing generic toroidal plasma physics (enhanced performance, steady-state and burning plasma physics) and enabling technology that will benefit other innovative toroidal magnetic concepts that are being developed in the Steady-State Advanced Confinement Initiative.
- The Modular Pathway has multiple less-costly facilities which substantially reduces the technical risk associated with a single facility. Multiple facilities also offer the flexibility of multiple international locations, and allow financial commitments to be made in smaller amounts and phased over time.

The concerns about the Modular Pathway include:

- The integration of burning plasma physics, steady-state operation and nuclear technology at the energy scale is delayed.
- The Modular Pathway is a departure from the present international plan and is presently inconsistent with the national fusion strategies of our ITER partners. An explicit part of the European and Japanese national fusion plans, at this moment, is the concept of a single step to DEMO from JET and JT-60U.
- Some design solutions and technologies developed for copper magnet ignition experiments and D-D steady-state experiments may not extrapolate to an integrated facility.
- The Modular Pathway has "two steps" to a DEMO, rather than the "one step" to a DEMO in the ITER-RC Pathway. Thus, the cost and time of getting to the DEMO stage in the Modular Pathway will both be larger than the corresponding numbers for the ITER-RC Pathway, albeit the design basis for the DEMO will be superior in the Modular Pathway with separate physics and technology facilities followed by an Advanced Integrating Facility.

3.6 Near Term Actions for the Modular Strategy

1. Determine the "must have" physics requirements for the burning plasma physics initiative.
2. Carry out a detailed assessment/review of the physics, engineering issues, and costs for Ignitor-like compact tokamak burning plasma devices. Engage/inform the U. S. fusion community during this assessment.

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3. Hold an workshop with interested international participants on Burning Plasma Physics and Engineering Issues of compact tokamaks. There needs to be an assessment of Ignitor and similar compact tokamaks to evaluate what fraction of the goals of a burning experiment they will meet and how Ignitor-like compact ignition tokamaks fit into the longer range magnetic fusion program. This assessment should include a comparison to other burning plasma experiments such as BPX, the most similar design but a larger device with a divertor.
4. Determine the physics requirements for the steady-state advanced confinement initiative.
5. Carry out a detailed technical assessment/review of the physics and engineering issues for the steady-state advanced confinement element including an assessment of the relative capabilities of existing facilities, facilities now under construction and possible new facilities. Engage/inform the U. S. fusion community during this assessment.
6. Define the role and technical requirements for engineering testing of nuclear components in each of the three pathways.

These activities should be carried out with the goal of developing a plan within 6 months.

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4.0 Enhanced Concept Innovation Pathway

4.1 Pathway Overview

The previous two pathways focused on the tokamak next step, whether a single large facility integrating physics and technology in a DT device, or two facilities addressing separately burning plasma physics and non-inductive operation at high confinement performance on long time scales. Neutron irradiation facilities are assumed to be likely supporting facilities. Both pathways consider the same program elements and their associated theory and technology. This pathway also contains these program elements, but focuses on advancing a diverse portfolio of fusion concepts to bring forward the more attractive options for development. It suggests criteria for advancing these concepts, including the advanced tokamak, through successive stages of development. These criteria include cost, reactor attractiveness, and science value. In applying them we also strive to create a "balanced portfolio" of approaches to fusion power. Several concepts are now being pursued in the base program so this pathway should be viewed as already underway.

This pathway explicitly recognizes that technical readiness alone is not a sufficient basis for investing in a next step, and that any step can only be considered in the light of all other potential decisions in the near time frame. It is further recognized that there may be good reasons for modifying the criteria in particular cases. These reasons might include the desire to use the step to advance critical science, or that international participation reduces the cost to the U.S. However, in these cases the degree to which the criteria are modified should be justified by the value one expects from the step. This path will likely delay the availability of a commercial fusion energy system because the alternate concepts are in an earlier stage of scientific development but the resulting product, however, may be superior.

We will use the development stages defined in Chapter 1 to illustrate this pathway [See, Figure 4.1]. The development stages begin with concept exploration, and continue through proof of principle then proof of performance before entering the fusion energy development. The last stage explicitly requires international collaboration according to recent policy adopted by OFES in carrying out its "restructured" fusion program, as discussed in Chapter 1. Options for this stage are the focus of the preceding two pathways.

Pathway 3 is to emphasize and even maximize the number of fusion concepts for exploration. Selection based on technical success tends naturally to allow fewer concepts to reach successively higher levels of development. The Tokamak, Stellarator, and Inertial Confinement Fusion using lasers are the most advanced at this time. Among the variants to these approaches, the Compact Stellarator, the steady state Advanced Tokamak, and the steady state Spherical Torus (or the very low aspect ratio tokamak) are currently less advanced. Further, a number of other concepts are being explored experimentally, some of which are poised for proof of principle experiments.

With regard to decisions to advance a concept, we would impose four criteria; project cost, power plant attractiveness, science value, and impact on the balance of the portfolio. A discussion of each of these follows, but the detail for these criteria should be clarified by broader debates. These criteria define Pathway 3 and differentiate it from the other two pathways described in this report.

Appropriate Cost

Costs are expected to increase for devices at successfully higher stages of development. Cost curves are therefore expected to rise to the maximum for the fusion power plant. This maximum can be taken as an acceptable

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cost projected for a commercial power plant at a typical power rating such as 1000 MW in electricity produced. The cost criterion for a intermediate stage would be reduced accordingly, and should be cast in a well-considered range. Cost should not be a hard and fast criterion, nor be far exceeded except for adequate reasons, such as a compelling science value that

Roadmap to Attractive Fusion Power through Concept Development

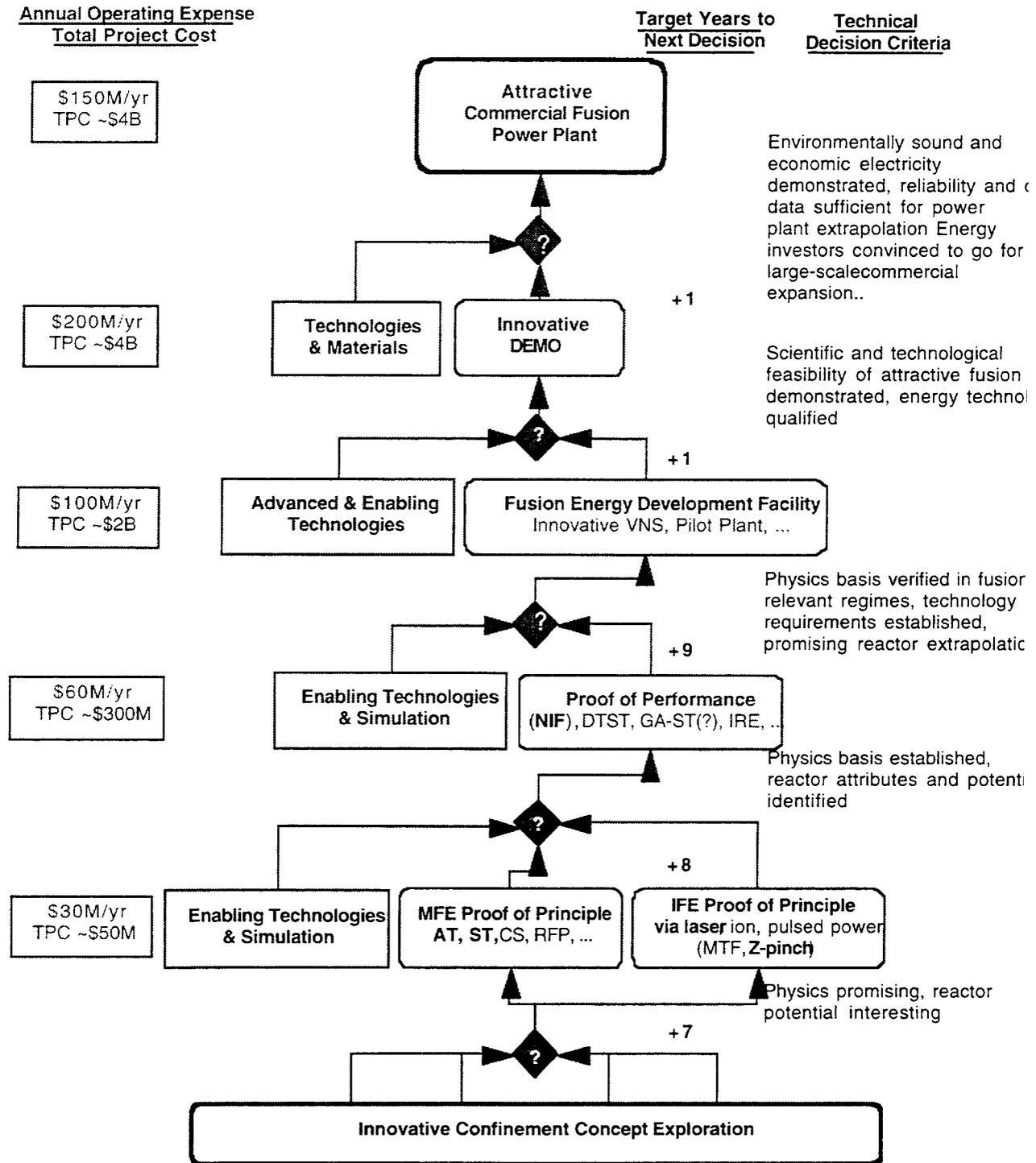


Figure 2.4.1. Concept Improvement Roadmap Toward Attractive Fusion Power within the next 40 year

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warrants additional investment. Such exceptions may be easier to justify at the lower rather than the higher stages of development.

Attractive Reactor

Attractive features projected to a power plant are of crucial importance. Application of this criterion depends on the development stage being proposed. Since these features are difficult to describe in detail during concept exploration, rough projections may be appropriate. For example, the FRC and the Spheromak possess an attractive feature by eliminating a large fraction of the magnets in contrast to the Tokamaks and the Stellarators where the magnets account for a large fraction of the cost and much of the complexity. The Spherical Torus and the Field Reversed Configuration project very high beta, an attractive feature for reducing the size and cost of the fusion core for a future power plant. As the concept progress to higher levels more of the reactor features and systems can be specified in increasing detail, rendering the criterion more stringent as well as easier to judge.

Science Value

Much of the magnetic fusion science base has been gained via the tokamak research, but different and very interesting science is to be explored and learned from other confinement approaches. For example, self-organized plasmas such as the Reversed Field Pinch and the Spheromak utilize for sustainment the plasma dynamos, a mechanism related to the formation of solar coronal loops. Fundamental properties of magnetic field reconnection in the Field Reversed Configuration are closely related to the field-reversed sheath observed in the solar down-wind tail of the magnetosphere. This criterion may thus play a large role, particularly at the early phases of development. Even for the stage of fusion energy development there is compelling incentive for a understanding of the physics of fusion ignition and burn to warrant some relaxation of the cost criterion. Evaluation of additional value (over that expected from this stage) for additional cost (over the metric for that stage) should be included in the decision.

Balanced Portfolio

Finally, it is the intent in this pathway to create and maintain a "balanced portfolio." We would define balance somewhat loosely as ensuring that each stage of development be populated with appropriate active options. We expect that a balanced portfolio include an expanded number of concepts at the exploration level, accompanied by diminishing numbers of concepts at progressively higher levels. Imagining a building block for each concept (including one for each major variant of the Tokamak, Stellarator, Inertial Fusion Energy, etc.), a vertical stack of blocks for each level would be roughly pyramidal. The balance needs to be managed through the decisions taken, since decision to build a particular device obligates the future and alters the potentially available funding for competing steps, as well as posit new advances that shed light on the relative merits of other possible steps. It implies that certain forward moves could be delayed in favor of others, even if technical readiness is demonstrated. We suggest that the criterion be applied more stringently at higher and more costly options of development, to avoid unwarranted negative impact on the balance of the portfolio and the negative image displayed for fusion energy by large and costly devices.

4.2 Rationale

Concept improvement has become a priority in the U.S. Fusion Energy Sciences Program. Efficient plasma production, containment, and fusion burn in modest size and simplified configuration characterize attractive

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fusion power. While the fusion research program has produced significant fusion power in the laboratory (in TFTR and JET), it must now innovate to produce the same efficiently, reliably and economically.

The U.S. does not perceive at the present time the need for rapid development of new energy sources. Thus, a compelling fusion concept will be required to kindle the necessary domestic interest and support, even at the reduced cost levels indicated here. Concept improvement in the U.S. fusion effort is therefore a necessity.

The Innovative Confinement Concepts (see Appendix 2.4-A) presently under consideration in the U.S. have a range of attributes for attractive fusion power and varying degrees of scientific and technological maturity. A broad portfolio for "Concept Exploration" enables reasonable balance of cost, risk, and benefit for an aggressive fusion energy sciences program. A balanced development in concept improvement would require that this portfolio contract as the concepts face the test for advancement.

4.3 Technical Contributions

Fusion confinement concepts naturally forms three groups: Magnetic Confinement (Pulsed Tokamak, Stellarator, Advanced Tokamak, Spherical Torus, Compact Stellarator, Reversed Field Pinch, Spheromak, Field Reversed Configuration, Electric Tokamak; Floating Multipole), Inertial Fusion (laser-driven and heavy-ion-driven Inertial Fusion Energy, Magnetized Target Fusion, Z-Pinch-driven Inertial Fusion Energy), and New Innovations (Inertial Electrostatic Confinement).

The first group of concepts relies on the magnetic field for containment. These concepts however span a wide range of different magnetic fusion plasma regimes, and utilize a plethora of techniques of plasma manipulation. This pathway addresses magnetic concepts other than the Pulsed Tokamak and the Stellarator. The second group relies on implosive compression of miniature frozen fuel capsules or small size magnetized plasmas via externally applied energy (photon pressure or mechanical force) to very high densities for extremely brief time scales (from Pico-seconds to μ s). A variety of drivers, mechanism and technologies are used for this purpose. The third group covers those concepts that are different in nature from the first two groups.

These concepts possess widely differing attractive features and concerns that form the basis for potential contributions, in the light of burning plasma physics, steady state advanced physics, technology and integration. Advances in one concept usually contribute to the understanding and progress of other concepts in the same group.

4.3.1 Magnetic Fusion Energy

The attractiveness of a magnetic fusion concept depends on MHD stability (or absence of disruptions) at high beta, absence of microinstabilities that degrade confinement, and simplicity of sustained operation of the fusion power core. These, as examples of attractive physics features, suggest potential contributions by this group of concepts.

Advances in understanding of MHD beta limits in tokamaks have given confidence of the high beta potential of the Spherical Torus. Neutral beam injection heating, proven effective for the tokamaks, has been used in a Spherical Torus to triple the Tokamak record toroidal beta. The beneficial effects of velocity shear in suppressing microinstabilities observed in the Tokamaks have provided a strong basis for expecting improved confinement in other concepts of this group. These potential improvements would reduce the unit fusion power while maintaining high neutron flux intensity needed in a fusion energy development device. Table 4.1 summarizes important potential contributions and concerns for these concepts.

Table 4.1. Attractive Features and Concerns of Magnetic Confinement Concepts

Concept	Development Stage	Attractive Features and Concerns
Advanced Tokamak	Proof of principle	<ul style="list-style-type: none"> - Improved confinement and β - Increased bootstrap current - <i>Disruption - stability and control</i> - <i>Projected high cost for Energy Development</i>
Spherical Torus	Proof of principle (to begin in or before May 1999)	<ul style="list-style-type: none"> - Much increased β, stability and controllability - Improved potential for high confinement - Improved current drive, bootstrap current, and its profile - Increased dispersion of plasma exhaust - Possible interim non-electricity applications of neutron produced along path to fusion energy - <i>Requires high plasma current with potentially high current drive recirculating power</i> - <i>Requires noninductive startup</i> - <i>Requires largely unprotected toroidal field central leg to ensure small size</i> - <i>Small normal conducting TF coils may project to large recirculating power</i> - <i>Disruption (possibly reduced or avoided via high safety factor or unobstructed vertical space)</i>
Compact Stellarator	Concept exploration (being reviewed for Proof of Principle test)	<ul style="list-style-type: none"> - Intrinsically steady state (no current drive) - Disruption-free, but dependent on bootstrap current fraction - Reduced aspect ratio from standard stellarators - <i>Trade-off between confinement and β uncertain at present</i>
Reversed Field Pinch	Concept exploration (being reviewed for Proof of Principle test)	<ul style="list-style-type: none"> - Simple, low-field, normal conducting toroidal coils - No disruptions - Inexpensive current drive - <i>Inductively driven</i> - <i>Magnetic turbulence-dominated transport - (reduced by edge current drive to approach $(j/B) = \text{constant}$)</i>
Spheromak	Concept exploration	<ul style="list-style-type: none"> - No toroidal field coils - <i>Potentially unstable to tilt instability</i> - <i>Magnetic turbulence-dominated transport - (reduced by edge current drive to approach $(j/B) = \text{constant}$)</i>
Field Reversed Configuration	Concept exploration	<ul style="list-style-type: none"> - No toroidal field coils - Near unity β - Simple and inexpensive magnets - Finite orbit effects prevent ideal instabilities - Potentially high power density - Natural open divertor geometry - <i>Global stability and sustainment for large device</i>
Electric Tokamak	Concept exploration	<ul style="list-style-type: none"> - Aims for near unity β stabilized by rotation - <i>Requires large drive for strong rotation</i> - <i>Global stability far beyond present understanding</i>

Floating Multipole	Concept exploration	<ul style="list-style-type: none">- No toroidal field coils- Better than unity β- Steady state; disruption-free- Potentially free of drift-wave turbulence and may obtain classical confinement- Low divertor heat load- <i>Requires levitated superconducting ring</i>
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4.3.2 Inertial Fusion Energy

While both magnetic and inertial fusion are at approximately the same stage of scientific understanding, the scientific and technological criteria by which these two distinct classes will be judged as power sources are very different. The various concepts under Inertial Fusion Energy (IFE) (see Table 4.2) offer routes to a fusion power plant which are paradigm shifts from the Magnetic Fusion Energy (MFE) concepts.

Net fusion energy gain Q , the ratio of output fusion energy to input drive energy, is a long-standing figure of merit for fusion research. Both MFE and IFE have progressed significantly in to approach systems for which gain exceeds unity, the so-called scientific break-even. In pulses of one to ten seconds, large Tokamaks such as TFTR and JET reported $Q=0.3$ and 0.9 , respectively. The National Ignition Facility (NIF) is under construction and preparing to test the gain of 10 in a single pulse.

The term ignition is used differently by MFE and IFE. For MFE, it defines the plasma condition where alpha heating is adequate to sustain fusion grade plasmas without auxiliary heating (Q of infinity). For IFE, which is inherently pulsed, ignition refers to establishing a propagating fusion burn wave from a central hot spot through the compressed fuel, similar to a miniature explosion. Once ignition occurs, target gain is determined by the fraction of fuel burned before the fuel disassembles.

Table 4.2. Attractive Features and Concerns for Inertial Fusion Concepts

Concept	Development Stage	Attractive Features and Concerns
Laser-driven	Proof-of-Performance (under construction)	<ul style="list-style-type: none"> - Modularized reactor components permits phased, cost-effective parallel development program - Modularity offers attractive availability and load-growth expansion. - High leverage from complementary Defense-funded Inertial Confinement Fusion program (NIF etc.) - <i>Ultimate driver costs must be ~\$500M (direct) for attractive COE (@ 1GWe)</i> - <i>IFE targets must be mass-manufactured at low cost (~30¢/target) and be injected with acceptable tracking accuracy (<1mm) at rep-rates of several Hz</i> - <i>Final focusing optics must achieve desired target spot sizes</i> - <i>Requires exceptional high-cycle (~10⁹) reliability of power components</i> - <i>Rapid cleansing of debris and residues from chamber at rep-rates of several Hz</i>
Heavy-ion-driven	Concept Exploration	<ul style="list-style-type: none"> - Reactor chambers could more readily use thick liquid walls, affording lifetime components and on-site, near-surface burial at end-of-life, than laser drive - Higher driver efficiency than laser drive - Final-focusing optics better protected than laser drive - Other attractive features as for laser IFE - <i>Other concerns similar to laser IFE</i>
Z-Pinch-driven	Concept Exploration	<ul style="list-style-type: none"> - Low driver costs per x-ray yield - <i>Closely coupled, invasive electrode</i> - <i>Reactor extrapolation and designs undeveloped</i> - <i>Other concerns similar to laser IFE</i>
Magnetic Target (compression) Fusion	Concept Exploration (being reviewed for Proof of Principle test)	<ul style="list-style-type: none"> - Small chamber size allows rapid test and development of concept, through proof-of-performance, at modest scale and cost - Highly leverages DP investment in facilities, diagnostics, and implosion science - Advances CT science development at fusion-relevant conditions - Reactor advantages similar to IFE, improved possibility for liquid walls - <i>Invasive magnetic coupling required in reaction chamber</i> - <i>Requires satisfying constraints on yield, rep-rate, and cost per target, similarly to laser drive and Z-Pinch drive</i>

4.3.3 New Innovations

Innovations that diverge qualitatively from the preceding two classes of fusion approaches should be encouraged by this pathway. Inertial Electrostatic Confinement based on a modified form of Penning discharge exemplifies this classification.

Table 4.3. Attractive Features and Concerns for New Innovations

Concept	Development Stage	Attractive Features and Concerns
Inertial Electrostatic Confinement	Concept exploration	<ul style="list-style-type: none"> - Adds to portfolio diversity - Confinement physics allows for very small-scale systems for development - Possible interim non-electricity applications of neutron produced along path to fusion energy - Net fusion energy gain at high surface to volume ratio: possible high mass power density with moderate wall loading - <i>Requires very-high spherical convergence Grid-less systems</i> - <i>Requires precise control of electron distribution function</i>

4.4 Advantages and Concerns

The advantages of this strategy include:

- Increased probability to develop the best fusion energy system
- Broadened physics investigation and utilization of differing plasma regimes
- Widened areas for potential spin-offs to other fields of science
- Minimized risk of failures in fusion energy development
- Complementarity to foreign fusion programs, which is studying a more limited range of concepts
- Improved basis for realistic cost targets for funding
- Extensive utilization of existing facilities
- Encouragement for U.S. to gain a leadership position in Innovative Confinement Concept fusion research

The concerns for this strategy include:

- Delay in burning plasma and integrated tests
- Reduced near-term focus
- Increased risks for longer time and more cost to develop practical fusion energy
- Reduced compatibility with existing international approach for ITER

4.5 Near Term Actions

Pathway 3 entails the consideration by the U.S. fusion community of the following actions:

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- Begin comprehensive roadmap assessment for Advanced Tokamak, Spherical Torus, and Inertial Fusion Energy immediately.
- Plan for similar assessment for Compact Stellarator, Reversed Field Pinch, and Magnetic Target Fusion when and if Proof of Principle facilities approach the start of experimentation.
- Clarify in 1999 the U.S. fusion energy sciences priorities and goals in the world fusion program.

Section 4

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Date: 6/30/98 3:07 PM
Priority: Normal
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CC: galambosjd@ornl.gov_at_internet at X400PO
Subject: Additional FESAC comments

----- Message Contents -----

To Anne Marie Zerega.
June 30, 1998.
From John Sheffield, chair, on behalf of the FESAC.

Additional preliminary Comments from FESAC on the document "Facilities for the Future: The Office of Energy Research."

D.) Comments on the OER future facilities report.

(1) In my opinion, Section 5. on fusion facilities is well written, and presents a reasonable balance between all the proposed facilities.

(2) While probably not our direct concern, in my opinion, some of the other sections are poorly written, and one in particular is out of balance with other sections.

I found the both the OBER and BES sections to be substandard, and the BES section to be too long relative to the other sections.

(3) If one had the opportunity to start over, the whole document would be improved by a single editor with some vision of a standard format, and a better vision of who is the principle customer for the document .

E.)

- page 1: in the final paragraph, there is no recognition that the scientific infrastructure includes the scientists, engineers, etc. - not

just the facilities.

- page 2: Section 1.1 - there is no sense of partnership with other agencies like NSF or international parties

- page 15 MICROCHARACTERIZATION - should the line really be "University of Illinois Frederick Seitz"?

- page 39 Section 5.1 - "on maintaining an involvement in fusion energy science AND TECHNOLOGY through international collaboration."

- page 40 top partial paragraph - facilities are also targeted to explore the science of regimes like "burning plasmas", not just to explore the potential of various concepts.

- page 40 first full paragraph - some scientific issues demand significant facilities - again, not just to test concepts through their stages.

- page 41 - second full paragraph - "reduced cost options" for ITER, not "cheaper".

- page 41 - section 5.2 - The DTST facility should be illustrative of proof-of-performance innovative concepts; the DTST's presence on the chart should not suggest a present-day plan that would be upset by delays or issues in the ST concept line; instead, innovative concepts will arise to the POP phase as they mature and demonstrate their suitability for POP. As such, the DTST should be addressed in section 5.6, not in its own section. (AS suggested by John Sheffield, this section should be merged with 5.6 and called something like innovative alternate concepts and moved to an early position in the the fusion report.)

- page 43 - section 5.3 - a multi-machine option for addressing the issues of burning plasmas should be described

- page 46 - section 5.5 - "Understanding the materials response and (DELETE THOSE) then"

- page 47 - there should be a line in the diagram showing Proof of Performance devices from innovative concepts beginning in the 2010 timeframe.

Author: oji@ornl.gov_at_internet at X400PO
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Priority: Normal
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Subject: FESAC comments

----- Message Contents -----

To Anne Marie Zerega.

June 30, 1998.

From John Sheffield, chair, on behalf of the FESAC.

Preliminary Comments from FESAC on the document "Facilities for the Future: The Office of Energy Research."

- 1.) There are concerns that the introduction is written at a low level. It is also noted that while attention is paid to the "Role of National Laboratories", it would be wise to add some comments on the role of universities and the private sector in hosting facilities e.g., in the fusion area, MIT and GA.
- 2.) It was noted that the goals of high energy physics and nuclear physics, as expressed in the first paragraphs of the respective sections, are essentially indistinguishable.

3.) In regard to the fusion section, the following comments were made.

A.) Section 5.1 Program overview

In the last paragraph on page 39, the first sentence seems to imply that fusion science is not science. Somewhere in the first 2 paragraphs of this section, the connection to "building the knowledge base needed for an economically and environmentally attractive fusion energy source" should be worked into the text. All parts of the program work toward building that foundation.

On page 40, in the 3rd paragraph, should read
... that past facility roadmaps were designed to address several fundamental scientific and technical issues that are still highly relevant to today's Fusion Energy Sciences Program with its emphasis on innovative concepts and alternatives to the CONVENTIONAL tokamak.

Section 5.3 Reduced Cost/Advanced Tokamak Physics Facility (ITER)

The strongest sentiment coming out of the Madison meeting, which reconfirmed the FESAC recommendation, was that we should be moving forward as quickly as possible, with international collaboration, to studying self-heating burning plasmas. If ITER does not go forward to construction, the best hope for that path is a compact, high-field, copper coil tokamak. Given what we know today,

it would probably be similar in size and field to the original CIT ($R \approx 2$ meter, $B > 10$ tesla), with a total construction project cost of about \$1 billion. This approach must, in my opinion, be added to the text, and to the roadmap, if community input means anything.

Section 5.4 Heavy-ion accelerator facility for inertial fusion energy

I believe it is premature to talk of construction of such a facility in FY2001, given the schedule for NIF. A schedule which is based on having positive results from NIF in hand makes much more logical sense to me.

B.)

1) To me it would be better if the fusion section began with a brief paragraph explaining what fusion is and why the fusion program is the steward of plasma science for the government, and a brief comment on the science and program evolution to our current state -- to provide the (nonexpert) reader with a context. This would then be similar in tone to the beginnings of the INTRODUCTION and the BER, HEP, and NP sections.

2) More generally, Section 5.1 reads a bit bureaucratic and management-oriented, and does not clearly articulate the scientific and energy goals of the fusion program, and the strong current emphasis on innovation in our science focus, energy goal program.

3) In the 5th line of the 3rd paragraph of 5.1, "concept exploration" should be listed as the first generic stage of concept development.

4) Since the fusion program is currently concentrating most of its "new" resources on alternate concepts and innovation, I would have expected that emphasis to emerge more clearly -- for example by the title of Section 5.6 being changed from "Other Fusion Energy Science Facilities" to something like "Innovation, Alternate Concepts," and then this section moved to 5.2 and being followed by ST-POP, ITER-RC, IFE Acc. and Neutron Source. Then the ST-POP Section should be changed to say that the ST presently looks like the best prospect for a next stage "proof of performance" device, but that we will have to assess this carefully after the year 2000, based on developments in this and other alternate concept research.

5) More generally, much of the fusion section seems to have a negative, whining tone. It should be written with a more positive, optimistic view toward the future -- rather than crying over spilt milk. We need to use this as an opportunity to say positively what exciting things we want to do and what new tools we need to do them.

6) In the Fusion Energy Sciences Roadmap at the end the Reduced Cost Advanced Tokamak ITER label should have an asterisk on it surely would be done internationally.

C.) Section 5.

1.) The section needs an overview showing the time and option space in which the various facilities sit - "proof-of-principle", "proof-of-performance," "full-scale-demonstration" + path through ITER, multi-device in parallel path, tokamak, non-tokamak magnetic, inertial, etc.

2.) It is clear that ITER, a 14MeV neutron source and STX are in the program. It is not clear where the strategy STX to a VNS (DTST) comes from, nor why the beam-driven IFE device is selected, other than that it was mentioned as an example in an earlier FEAC report (would it be highlighted as much today?). If these are mentioned it would be better to cite them as examples of possible options.

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June 30, 1998

Dr. Martha A. Krebs
Director
Office of Energy Research
U. S. Department of Energy
1000 Independence Way, SW
Washington, DC 20585

Dear Dr. Krebs:

Thank you for the opportunity to provide comments on "Facilities for the Future: the Office of Energy Research". We have some comments on Sections 4 and 5.

Comments on Section 4:

The present document on "Facilities for the Future" should better reflect the new DOE vision for large scale computation as a tool for discovery in support of basic science and better utilization of facilities. As such, we would suggest that the ideas contained in the following paragraph be appropriately integrated into Section 4 on Computing.

The Department of Energy has recently identified simulation science as a mission-critical activity. This activity seeks to accelerate progress toward scientific breakthroughs within its portfolio of research programs by taking full advantage of the dramatic increases in capabilities in high end computing in the next decade. As a full partner to scientific experimentation and theory, it will help revolutionize the way the DOE pursues many of its research and development programs that are central to accomplishing its energy, environment, science and national security missions. The simulation activity will build on the investments made under its defense mission for the Science-Based Stockpile Stewardship in the Accelerated Strategic Computing Initiative (ASCI). Unprecedented rapid advances are expected in the climate prediction combustion modeling and simulation activity, basic science modeling. Timely implementation here of revolutionary new resources is key to efficiently advancing DOE mission imperatives in areas such as Materials Science, Fusion Energy Sciences, High Energy and Nuclear Physics, Structural Biology, Subsurface Transport, and others. This will also provide valuable cross-cutting scientific support for the thrust areas as well as ASCI. Overall objectives of the scientific initiative on high performance computing include:

- To revolutionize the approach to solving scientific problems through enabling simulation on a scale never before possible;
- To accelerate fundamental new scientific discoveries and progress critical to DOE missions;

- To make high performance scientific computing more broadly available and accessible to DOE researchers as a principal tool for discovery;
- To make high-performance scientific computing a well-integrated component of the DOE science and engineering programs.

Comments on Section 5:

In previous FESAC presentations on the development of heavy ion beam drivers, a proposal was made to deliver 20-50 kJ on a target. Such an accelerator would not have "enough energy to burn a high-gain fusion target". Also such a facility would not be "heavily activated by D-T reactions".

Sincerely,



Richard J. Hawryluk
Deputy Director

cc: A. Davies
R. Goldston