UNIVERSITY OF WASHINGTON
SEATTLE, WASHINGTON 98195

College of Engineering
Department of Nuclear Engineering

September 8, 1988

Dr. Robert O. Hunter, Jr.
Director, Office of Energy Research
U.S. Department of Energy
1000 Independence Avenue
Washington, D.C. 20585

Dear Dr. Hunter:

The Magnetic Fusion Advisory Committee met on September 7-8, 1988 in Los Alamos, New Mexico to complete its response to Dr. Decker's charge letter of June 7 concerning Fusion Program Planning for the Early-to-Mid 1990's. As preparation for its response, the MFAC met as a panel with leaders of the fusion program for a week-long summer study. The MFAC has reviewed the report of the Summer Study Panel which had been submitted earlier to the membership. With one dissent, MFAC endorses the report as providing the basis for the following conclusions and recommendations.

In reviewing the current status of fusion research and its place in the world energy picture, we find compelling reasons for a renewed thrust in the quest for fusion development. Issues such as the greenhouse effect, acid rain and forest depletion are attracting widespread concern, shifting the focus from issues of energy supply to those of the harmful effects of the use of present fuels. The situation is increasingly regarded as a long-term global problem, requiring international cooperative action. Fusion power developed early in the next century could be an important part of the solution.

International cooperation in a large scientific endeavor is never easy to achieve. Fortunately, cooperation is already functioning well in fusion research. This cooperation is now focussed on the ITER design of an engineering test reactor. During the past eighteen months, the ITER process has received enthusiastic support from all of its participating parties, at both the working and government levels.

The ITER design, scheduled for completion in 1990, presents a challenge to us to make sure that the product meets the criteria important to the US and to help shape what follows. This is true regardless of whether the design is implemented on a unilateral, bilateral or multilateral basis. Meeting the challenge will require strengthening the domestic program upon which such a role depends.

The US has recently identified the Compact Ignition Tokamak (CIT) as the major new program initiative needed in the 1990's. The MFAC, as did the Summer-Study Panel, strongly supports this initiative within a balanced US fusion program. The time has come for the world program to initiate experiments with ignited plasmas.

At present, the US base scientific and technology programs are operating at budget levels too low to permit them to support effectively the CIT and ITER programs and to exploit new ideas enhancing the commercial acceptability of fusion. Additional funds are needed to carry out the

Benson Hall, BF-10 / Telephone: (206) 543-2754

CIT on a timely basis within a program that adequately addresses the other areas necessary for fusion development. The specific activities required are detailed in the Summer-Study Panel Report in Figure 2 of the Overview Subpanel findings.

These activities incorporate the two historical aspects of fusion development: demonstration that fusion is a feasible energy source, and demonstration that economic, safety, and environmental conditions can be met. The activities are also organized to show directly their support of the CIT and ITER programs and the R&D programs necessary to identify an attractive reactor. The Panel Report does not assign priorities to these activities on the grounds that all will be required. However, MFAC emphasizes the need for increased attention to two issues: understanding tokamak energy transport and developing low-activation materials.

We urge the Department of Energy to seize the initiative in promoting a new US thrust in the development of fusion power. The additional financial needs represent relatively modest increments to the present program. The thrust should be shaped around timely completion of the CIT project, optimization of the ITER design, and development of a reactor concept with attractive economic, safety, and environmental characteristics. The need and timing for this initiative derive from concerns about the undesirable side effects of fossil fuels.

I look forward to discussing these matters with you at your earliest convenience.

Sincerely yours,

ORIGINAL SIGNED BY
F. L. RIBE

Fred L. Ribe
Chairman
Magnetic Fusion Advisory Committee

FLR:ll

Attachment: Report on Fusion Program Planning for the Early-to-Mid 1990's presented to the Magnetic Fusion Advisory Committee by the MFAC Summer-Study Panel, September 1, 1988.

GENERAL ATOMICS

September 12, 1988

Professor Fred L. Ribe
Chairman, Magnetic Fusion
Advisory Committee
Nuclear Engineering
Benson Hall, BF-20
University of Washington
Seattle, WA 98195

Dear Fred:

At the MFAC meeting in Los Alamos on September 7-8, 1988, I was the sole dissenter who did not endorse the summer study panel report. I would like to clarify the reason of my dissent.

The official charge for this MFAC deliberation was to recommend initiatives in the 1990's of the U. S. fusion program. The gist of the summer study panel report is essentially to follow the present program plan and to do more faster with an increased funding level. The present program plan is the one I take issue with. I see an opportunity to take a new program direction by proposing new initiatives in the 1990's without abandoning the present program direction overnight. Since I found during the summer study week that some people share my view, I think it is worthwhile to let my view be known.

Let me start from the following observation. The fusion program has made impressive progress in confining high temperature plasma and a significant amount of thermonuclear reaction is taking place in present-day devices. As a result, attempts to proceed to the fusion test reactor phase have been made for the past several years, presently ITER. The design efforts showed that these test reactors must have a large power throughput and are very expensive. Some people say that they are so expensive that they can only be built internationally. The reason for this is the physics constraints, the energy confinement time in particular. In other words, the progress of plasma science is good enough to demonstrate burning plasma in TFTR and JET but not good enough to embark on a test reactor phase.

The purpose of R&D for a product is removal or reduction of technological constraints. The wide design window allowed by the R&D effort leads to a product that the market accepts and desires. The R&D effort may be divided into two phases: scientific and engineering. The goal of the scientific phase is not only the demonstration that it can be done but that the scientific constraints are reduced to the extent that the engineering phase can proceed expeditiously. To minimize the total development time, the relative lengths of the scientific phase and the engineering phase have to be balanced. Too early a transition to the engineering phase prolongs the development time and also increases the total cost.

The main goal of the fusion program is the generation of electric power. The present fission and fossil power stations are currently 1000 to 3000 Mwth in size. According to the history of these energy technologies, many test reactors of various types were built before commercialization. They were of output power smaller than that of the eventual commercial plants. The number of fission test reactors exceeded one hundred world wide. The problem the fusion program faces presently is that physics dictates the minimum size of a test reactor and it is of the same size as the anticipated commercial plant. A test reactor may cost $5 B and take over ten years to build. If this situation is not changed, the commercialization of fusion power may not come until the latter half of the 21st century.

A new product must compete with existing products. Fusion power is a case where high technology is used to generate a low technology product, namely energy. It has plenty of competition. The competition is, of course, decided on economics, including the social cost. The translation of the social cost into dollars will depend on the slope of the utility curve at the risk aversion side. Based on the experience of fission energy, the slope is very steep as far as radio-activity and safety are concerned. The public will form an opinion about fusion power by watching the developmental phase of the fusion program. One of the goals of the test reactor phase must be the minimization of the real and perceived social costs.

The fusion program has not paid enough attention to the activation of the structural material. The present conceptual test reactor designs including ITER are of high activation. The safety issue associated with after heat and the disposal of radio-active material are very important factors in determining the cost of fusion power. It is beyond comprehension that the fusion program wants to have a highly visible embodiment of a fusion reactor with all the negative factors associated with activation.

The above considerations point the way to the strategy for the next ten years. The aim is to come up with fusion test reactors of affordable cost and benign environmental characteristics.

The program emphasis should be changed immediately towards this goal. It can be accomplished without major disruption because many of the program elements necessary for the task are already in place. It is largely a matter of emphasis.

The first priority should be to complete the scientific phase to the extent that the test reactors become affordable. More specifically, the energy confinement must be improved since it is the high-leverage issue constraining the test reactor design. One might ask what is the probability of achieving this. I would like to point out that the program faced this question more than twenty years ago. Many experiments showed that plasma transport was governed by Bohm diffusion which is too fast for fusion reactors to be practical. Some people believed that nature is unkind to magnetic fusion. Subsequent experiments with the internal ring devices proved otherwise.

Experiments with the present tokamaks show steady improvement in confinement; the H-mode and the supermode are examples. In some plasma regimes the theory appears to explain the transport behavior. There have been proposed new ideas that the tokamak configuration can be shaped in such a way as to mimic the configuration of the internal ring configurations. These developments convince me that the prospect for improvement is good.

Priority in the development and technology area should be on the enhancement of social acceptability. The design studies of test reactors, especially the ITER, should be directed to sensitivity tests on physics constraints and the incorporation of low activation material. The sensitivity tests will tell the scientific program how much confinement improvement is needed.

If the new emphasis described above is implemented immediately, the U. S. fusion program would be in a position to enter the test reactor phase with affordable and environmentally attractive reactor designs sometime in the 1990's.

The major initiative in the 1990's should then be the initiation and construction of a U. S. fusion test reactor. As the U. S. program regains world leadership in the test reactor phase, the necessity of the ITER will diminish. A more fruitful and productive international cooperation should replace it.

Sincerely,

T. Ohkawa

Tihiro Ohkawa
Vice Chairman
General Atomics

Report* on

FUSION PROGRAM PLANNING FOR THE EARLY-TO-MID 1990'S

presented to the

Magnetic Fusion Advisory Committee

by

the MFAC Summer-Study Panel

September 1, 1988

*This report was prepared by a panel established by the Magnetic Fusion Advisory Committee (MFAC). The findings and recommendations presented herein are presented to the MFAC as the basis for its findings and recommendations.

TABLE OF CONTENTS

REPORT OF THE MFAC PANEL ON
FUSION PROGRAM PLANNING FOR THE EARLY TO MID 1990'S

1. FOREWORD

Worldwide concern about current trends in energy consumption and the associated environmental impact have led to the establishment of a major joint international effort in controlled fusion research. The US should strive to play a strong role in this effort. This requires certain timely initiatives to strenghten the domestic program upon which this role depends.

The goal of the US fusion program is the development, by early in the next century, of the science and technology data base necessary for an assessment of fusion as an energy option. The key technical issues for the assessment are the completion of the plasma physics data base, especially that of the physics of burning plasmas; development of an economically attractive reactor concept; and development of the necessary nuclear technologies and materials. An important feature of this program has been the recognition that international cooperation will permit pooling of world fusion resources.

The Magnetic Fusion Energy program has launched the Compact Ignition Tokamak (CIT) as the major new US fusion program initiative for the 1990's. Our readiness for this grew out of a series of important fusion initiatives launched in the 1970's. Several MFAC findings and meetings have endorsed the CIT decision. The present Panel, formed with broadly based representation from the US fusion community, strongly reiterates support for the CIT and its place in the international fusion program, believing that the time has come for the world program to initiate experiments with ignited plasmas.

An important area of US fusion research, and one in which it has held a position of world leadership, is in the assessment of reactor concepts and development of an advanced concept having attractive reactor features. These efforts optimize features of plasma, materials, and nuclear technology to make possible a realistic and supportable statement of the economic, safety and environmental attractiveness of fusion.

The US has also played an important role in establishing international cooperation in fusion, now focused on the ITER process. US participation in such a world program has received strong MFAC endorsement. During the past eighteen months, the ITER process has received enthusiastic support from all of its participating parties, at both the working and government levels, so that international cooperation is now likely to play an increasingly important rôle in fusion development.

While the CIT and ITER initiatives have been launched, the US base fusion scientific and technology programs are operating at budget levels too low to permit them to support these lead programs most effectively or to pursue advanced concepts. Modest increases are urgently needed in many areas to utilize existing facilities and to exploit new ideas.

The completion in 1990 of the ITER conceptual design phase presents a challenge for the US to help complete the ITER data base in science and technology and to help assure the best possible ITER design. US research has already influenced the ITER process in important ways. It has also introduced ideas for improving the ultimate reactor product. Our ability to influence the ITER process depends on continuing the strength of our fusion scientific and technological base. New research initiatives will be needed to reinforce and advance our earlier contributions and to otherwise strengthen the US position in the world fusion research program.

Because of growing international concern about present-day fossil fuels, the issue of environmentally acceptible, long-term energy supply has been recognized as a global problem, requiring global cooperation for solution. In light of this, cooperation in fusion and the time scale set by the ITER process acquire new significance. If taken now, the new initiatives presented in this report will assure continuation of a strong US position in this worldwide effort in the 1990's toward a fusion contribution to the energy supply problem.

2.0 INTRODUCTION

2.1 Charge to the Panel

At its June, 1988 meeting the magnetic Fusion Advisory Committee was asked by the DOE Acting Director of Energy Research, James F. Decker, to address the issue of Fusion Program Planning for the Early-to-Mid 1990's and to respond by the end of September, 1988. Identifying the compact Ignition Tokamak (CIT) as the major initiative for the 1990's, he asked the MFAC to consider what new initiatives the fusion program should be prepared to undertake in the early-to-mid 1990's and in what order. Consideration should be given to the issues requiring resolution prior to an assessment of the feasibility of fusion and the plans of other national programs with the goal of positioning the US program to contribute to and benefit from the world program.

A copy of Dr. Decker's charge letter is appended to this report.

2.2 The MFAC Summer Study

The suggestion of an MFAC summer study was made prior to the June, 1988 MFAC meeting and presented in preliminary form to the MFAC at its March, 1988 meeting by Dr. John F. Clarke of the Office of Fusion Energy of the US Department of Energy. A steering committee for the summer study was appointed by MFAC chairman, F. L. Ribe, and its initial suggestions were reviewed at an informal meeting on the evening before the June, 1988 MFAC meeting. Arrangements were then made to hold the summer study at the Coolfont Conference Center, Berkeley Springs, West Virginia on August 1-5, 1988. The study was constituted as an MFAC panel to provide written response to Dr. Decker's charge of June 7, 1988.

Table 1 shows the personnel taking part in Summer Study. The MFAC panel was divided into four subpanels chaired as follows:

Overview	S. O. Dean
Confinement	J. Sheffield
Plasma Science	H. Weitzner
Technology	M. Gottlieb

Table 2 shows the Schedule of Events of the Summer Study. During the plenary session of the second day Dr. Decker interacted with the summer-study membership, explaining the charge and associated policy considerations. He also expressed misgivings about the preliminary preconceptual design of the ITER device. In addition to the schedule shown in Table 2, there were numerous informal discussions, and the afternoon of Aug. 4 was spent in almost continuous sessions by all of the subpanels.

In this report the preceding foreword statement represents the themes that emerged from the considerations of all the subpanels and those of the other participants during the discussions in the plenary sessions. The findings of the Overview Panel in Section 3 of this report represent a summary and evaluation of the action initiatives developed during the Summer Study by the other three subpanels. The action initiatives and their rationales are given in Sections 4 through 6.

MFAC Summer Study, August 1-5, 1988

The study is constituted as an MFAC panel to provide written response to Dr. Decker's charge of June 7, 1988. The constitution of the four working subpanels is as follows:

Overview	Confinement	Plasma Science
S. O. Dean*, FPA	J. Sheffield*, ORNL	H. Weitzner*, NYU
R. J. Briggs, LLNL	B. Coppi, MIT	D. E. Baldwin***, UT
R. C. Davidson, MIT	W. E. Drummond, UT	D. B. Batchelor, ORNL
T. K. Fowler, UCB	R. E. Siemon, LANL	A. Bers, MIT
H. P. Furth, PPPL	B. G. Logan, LLNL	J. D. Callen, UWisc
J. E. Leiss, Consultant	J. F. Lyon, ORNL	H. H. Fleischmann, Cornell
R. K. Linford, LANL	D. M. Meade, PPPL	R. A. Gross, Columbia
D. O. Overskei, GA	T. Ohkawa, GA	A. L. Hoffman, STI
	D. R. Parker, MIT	P. H. Rutherford, PPPL
	K. F. Schoenberg, LANL	S. C. Prager, UWisc
		L.C. Steinhauer, STI

Technology	Chairman	DOE Representatives
M. Gottlieb*, GSS	F.L. Ribe**, UWash	J. F. Decker
M. A. Abdou, UCLA		J. Clarke
C. C. Baker, ANL		N. A. Davies
L. A. Berry, ORNL		J. J. Auchmoody
E. E. Bloom, ORNL		D. H. Crandall
R. W. Conn, UCLA		R. J. Dowling
W. B. Gauster, SNL		M. Roberts
D. F. Holland, INEL		J. Willis
R. A. Krakowski, LANL		
G. L. Kulcinski, UWis		Observers
D. B. Montgomery, MIT		
K. I. Thomassen, LLNL		B. Oran, HSSTC

* Subpanel Chairman
** MFAC chairman
*** MFAC vice chairman

Table 1. Persons attending the MFAC Summer Study, August 1-5, 1988.

MFAC Summer Study - 1988
Schedule of Events

	Morning Sessions 9 - 12 am	Evening Sessions 7:30 - 10:00 pm
Aug. 1		Plenary organizing session subpanel sessions
Aug. 2	Plenary session, discussion of charge - Decker and others	Subpanels meet
Aug. 3	Subpanel sessions	Plenary session - Midpoint evaluation
Aug. 4	Subpannel sessions	Plenary session
Aug. 5	Plenary session - conclusion, written subpanel reports	

Breakfast	7:30 - 8:45	(except Monday)
Luncheon	12:00 - 1:30	(except Monday)
Afternoon	Free discussion and recreation	
Social hour	5:00 - 6:00	
Dinner	6:00 - 7:15	(except Friday)

Subsequently: From the subpanel reports, Ribe and Baldwin will produce a draft report to Dr. Decker to be made final and adopted at the Sept. 7, 8 LANL MFAC meeting.

Subpanel chairs:		
	Program Overview	S. Dean
	Confinement Systems	J. Sheffield
	Plasma Science	H. Weitzner
	Technology	M. Gottlieb

Table 2

3. OVERVIEW SUBPANEL FINDINGS

As illustrated in Fig. 1, the development of fusion energy requires:

(1) adequate understanding of the underlying plasma science
(2) development of the necessary technologies
(3) integration of the fusion science and technology into appropriate test facilities which demonstrate the required systems performance.

Systematic progress has been made in all three areas. However, additional improvements are required before sufficient information will be available to allow a definite assessment of the potential of fusion, and there are still many possible development paths from today's status to an ultimate commercial fusion energy source.

The purpose of the fusion initiative described in this report is to ensure vigorous US participation in the world effort to develop a safe, environmentally acceptable and economic energy source. The proposed fusion initiative, illustrated in Fig. 2, consists of three important program thrusts and seven specific action initiatives. The three program thrusts are:

(1) the US program to construct a compact ignition tokamak (CIT)
(2) the international design of an International Thermonuclear Experimental Reactor (ITER)
(3) R&D programs to ensure realization of the environmental, safety, and economic potential of fusion as a commercial energy source.

A set of detailed initiatives are described in the findings of the plasma science, confinement and technology subpanels. The proposals of the subpanels are listed in Table 3. These have been grouped to support the seven action initiatives illustrated in Fig. 2 and discussed below. The selection of these action initiatives from a much larger set of possible new activities is based upon both their technical significance and upon broad programmatic and national policy considerations. The nature of these considerations is outlined in Table 4.

<u>The action initiatives</u> The first action initiative shown in Fig. 2 is to <u>provide for the timely construction of CIT</u>. Although CIT has long been recognized as a central thrust of the US program, funds have not yet been authorized to complete the construction on a timely basis. The DOE charge setting up the present MFAC panel implies that CIT is an already approved project. However the panel participants

noted that construction funds for hardware procurement have not yet been authorized by Congress. In addition the DOE five year "flat budget" plan results in an undesirably long construction schedule for CIT. Consequently members of the summer study pointed out that additional funds are needed, over and above the current DOE budget, to expedite the construction of CIT. Expediting CIT construction not only will result in obtaining important information on ignition physics at an earlier date but also will ensure that the U.S. will be ready to focus its attention on ITER construction on a timely basis.

The second action initiative shown in Fig. 2 is to improve predictive capability for tokamak confinement. Facilities like CIT and ITER might be made smaller and less expensive if we could extrapolate plasma transport with higher confidence from present experiments. In addition better understanding of the basic principles contributing to transport may lead to development of means for reducing transport, resulting in yet smaller devices. This action initiative is described in detail as initiative 1 of the confinement systems subpanel and as initiative 2 of the plasma science subpanel. The primary recommendation here is the immediate establishment of a <u>National Confinement and Transport Task Force</u>. The specific objective of this task force would be to mount a coordinated effort to characterize and understand the processes of cross field transport in tokamaks, with a view to identifying ways to reduce heat transport and improve overall energy confinement. This action initiative should include the development and application of special purpose diagnostics to existing experiments, new, small scale experiments when appropriate, and theoretical support to these efforts.

The third action initiative shown in Fig. 2 is to <u>optimize first stability tokamaks, including all necessary science, technology and systems analysis tasks</u>. It is aimed at development of more complete understanding of tokamak physics in the "first stability" regime in which these devices presently operate. This initiative is described in detail by the confinement systems subpanel (Sec. 4.5). Its near-term objective is to provide physics support to the design and construction of CIT and ITER. Studies of the effects of current, density and temperature profile control are important elements of this initiative, as are the effects of high fields and also of high and low aspect ratio.

The fourth initiative shown is to <u>develop attractive fusion reactors, including second-stability and alternate concepts</u>, and all necessary science, technology and systems-analysis tasks. This initiative could result in greater improvements for an end product and possibly for ITER. This initiative corresponds primarily to initiatives 3 and 4 of the confinement systems subpanel and initiatives 1 and 2 of the technology subpanel. The fusion program requires a coordinated science, technology and systems-analysis attack on what is required for fusion to be commercially attractive. Concepts must be analyzed through design studies; the physics of high power density must be proven and efficient, reliable technology developed.

The fifth initiative presented is to develop advanced plasma heating technologies, both for CIT and ITER. This initiative corresponds to parts of technology subpanel initiative 3 (Table 3) specifically relating to electron cyclotron heating and negative ion beam technologies.

The sixth action initiative shown in Fig. 2 is a major technology initiative which would improve ITER and also demonstrate features of an improved fusion reactor. This initiative is to develop and test high performance nuclear components with attractive environmental features, including low activation materials development and 14-MeV neutron source definition. This initiative corresponds primarily to technology subpanel initiative 1 (Table 3).

A seventh initiative is presented in Fig. 2 to design and build a tokamak for integration of steady-state (as opposed to pulsed) physics and technology. This is important both to ITER and the fusion end product. This initiative corresponds primarily to confinement systems subpanel initiative 5 (Table 3).

As part of the above seven action initiatives, support activities will be required, including a series of medium scale experiments and enhanced use of existing facilities. The justification for such activities is presented by the plasma science subpanel in describing their initiatives 1 and 3 (Table 3). Finally, it is important that effective use of international collaboration and of US industry, university, laboratory and government resources be sought as part of the implementation of all the action initiatives.

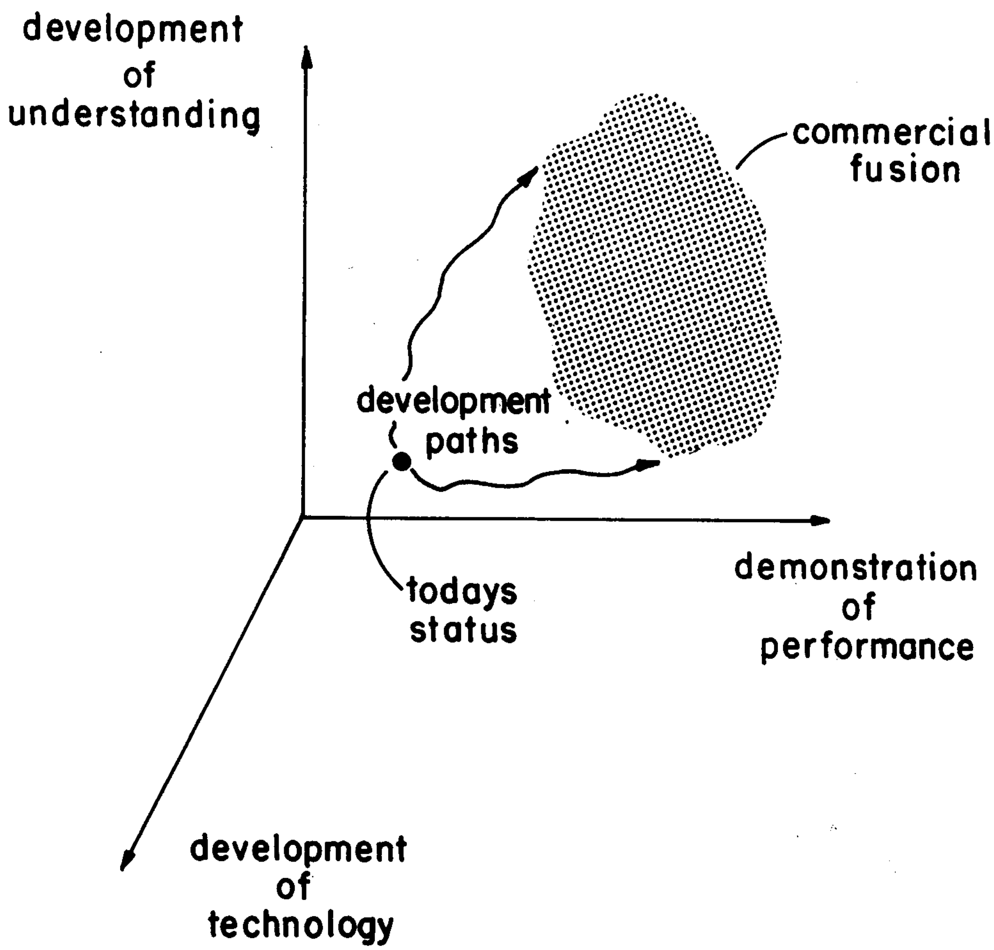

Fig. 1 Factors in the development of fusion energy

PURPOSE	VIGOROUS US PARTICIPATION IN THE WORLD EFFORT TO DEVELOP A SAFE, ENVIRONMENTALLY ACCEPTABLE, ECONOMIC ENERGY SOURCE		
PROGRAM THRUSTS	SUPPORT CIT	OPTIMIZE ITER	IMPROVE END PRODUCT
ACTION INITIATIVES	Provide for timely construction of CIT		
	Improve predictive capability for Tokamak confinement		
	Optimize first-stability Tokamaks. Include all necessary science, technology and systems-analysis tasks		
		Develop attractive fusion reactors, including second-stability and alternate concepts. Include all necessary science, technology and systems-analysis tasks.	
	Develop advanced heating technologies for CIT and ITER		
		Develop and test high performance nuclear components with attractive-environ-features, including low activation materials development-and 14-Mev neutron source definition.	
		Design and build Tokamak for integration of steady state physics and technology.	
SUPPORTING ACTIVITIES	A series of medium scale experiments and enhanced use of existing facilities will be required.		
	Effective use of international collaboration and of US industry, university, laboratory and government resources should be sought.		

FIGURE 2 PROPOSED FUSION INITIATIVE

Table 3
Initiative Areas Proposed by the Subpanels

Confinement Systems Subpanel

1. Improving predictive capability for confinement enhancement.
2. Optimization of the first stability regime tokamak.
3. Initiative in the advanced toroidal area: second stability.
4. Alternate concepts.
5. Steady state/high duty factor toroidal devices.

Plasma Science Subpanel

1. Experimental initiative in medium scale experiments.
2. A task force approach to critical problems in plasma science, in particular confinement and transport.
3. Enhancement of the scientific output of state-of-the-art devices.

Technology Subpanel

1. Feasibility of a safe, environmentally attractive power conversion system.
2. An attractive commercial and product.
3. Technology to improve plasma performance.

TABLE 4

JUSTIFICATIONS OF INITIATIVES

A. RESULT IN EFFICIENT USE OF RESOURCES

1. Provide optimum funding profile for construction
2. Provide adequate operating funds
3. Make efficient use of international collaboration
4. Make effective use of US institutions

B. IMPROVE SCIENCE AND TECHNOLOGY BASE

1. Improve predictive capability
2. Demonstrate required technology

C. STRENGTHEN US NATIONAL PROGRAM

1. Attract new people/institutions into fusion
2. Result in US nuclear fusion capability
3. Result in US leadership

D. IMPROVE ATTRACTIVENESS OF FUSION

1. Lower development cost
2. Result in more economic reactor design
3. Enhance fusion safety and environmental characteristics

4. CONFINEMENT PANEL FINDINGS

4.1 Introduction

Substantial progress has been made in recent years in the confinement program: $T_i \approx 30$ keV, $T_e \approx 10$ keV, $\langle\beta\rangle \approx 6\%$ in tokamaks, and steadily improving parameters in the alternative concepts, comparable with those in tokamaks at a similar stage of development. The understanding of transport and MHD has improved steadily as a stronger connection has been made between theory and experiment, and advances in diagnostics and plasma modeling have been developed and applied. The plasma technology tools which permit better control and optimization of the plasma have been a major part of these advances. Since 1980 many innovative initiatives have been developed - in fundamental physics, concept improvement, and burning-plasma physics. These, during the next 5-10 years, offer the possibility of substantially improved understanding and performance, in support of the assessment of fusion's potential.

The Subpanel is pleased to have the opportunity to recommend action initiatives, not funded presently, to further enhance this program.

The initial deliberations of the Subpanel were used to provide a common background and an approach to evaluating confinement initiatives. Specifically, the following areas were reviewed: the program; burning plasmas; ITER; and attractive reactor concepts. A major topic of the discussions was the need to optimize transport (minimize the thermal diffusivity χ), in order to provide not only the best fusion reactor, but also the best path to the reactor. For example, as indicated in supporting work for the TPA, if a 1200 MW electric reactor requires $\chi \approx 1.2\ m^2\ s^{-1}$, the precursor ETR at 500 MW thermal will require $\chi \approx 0.5\ m^2\ s^{-1}$, and the precursor Compact Ignition Device will require $\chi \approx 0.2\ m^2\ s^{-1}$.

From these discussions, four global areas emerged in which action initiatives are proposed:

(1) Toroidal physics and, specifically, transport
(2) Improved confinement systems
(3) Plasma control
(4) Steady state/High duty factor testing

Brief talks were given on: the principles and issues of confinement; the goals and programs in the areas of the dense Z-pinch, reversed field pinch (RFP), compact torus (CT), stellarator, and tokamak; burning plasmas, D-^{3}He; and plasma control (heating, current drive, impurity control); and steady state/high duty factor testing.

A number of issues emerged from the first two days of the meeting which were important to establishing and evaluating the confinement initiatives.

4.2 Program Development and Rationale for Initiatives

During the past few years the program has started a number of exciting new initiatives: DIII-D, PBX-M, Alc-C Mod, MTX, ATF, CPRF-ZT-H, MST, LSX, and the CIT. The confinement subpanel supports the goal of studying the burning plasma issue expeditiously through the CIT program. Following the spirit of the charge letter we assume that the CIT is a part of the ongoing program.

Decreasing budgets have led to a squeezing of these programs. Consequently, maximum use of this substantial investment is not being made, and many existing initiatives are languishing.

An experimental test reactor of the type under design in the ITER activity is a key element of the burning plasma path in a progression to a reactor. The Panel is concerned that the best information should be available to the ITER design process to ensure success. At the same time developments which could improve or complement ITER should be pursued vigorously to maintain a secure balance in the program.

Broadly speaking, the confinement program has two elements: <u>Burning Plasmas</u> and <u>Concept Improvement</u>. Both of these elements are essential components of the strategy for providing a fusion assessment.

The above points led us to consider the initiatives in terms of their support for these two program elements, and also in terms of the time for their deployment. By <u>near term</u> we mean the next 4-5 years, in which initiatives could be undertaken to support CIT and ITER, and provide a strong basis for concept improvement. By <u>long term</u> we mean those initiatives which, to some extent, are conditional upon the information from the near term program.

4.3 Summary of the Action Initiatives

The initiatives are discussed in detail below. This section summarizes them in an abbreviated form. Examples of specific opportunities to accomplish them are given below. It is to be noted that these were not rated by the subpanel. This partial list of opportunities does not incorporate all of the relevant initiatives from the Plasma Science and Technology Subpanels. The five initiatives summarized below are not in priority order, except for the case of confinement enhancement. All of the initiatives from this and the other two subpanels have been unified by the Overview Subpanel in Section 3.

Near Term Initiatives:

(1) Confinement Enhancement (4.4): $5M - $20M/yr
-Task force
-Diagnostics, experimental time
-New (small) experiments

(2) Optimization of the First-Stability Regime (4.5): $10M - $20M/yr
- Improve Configuration, Enhance Plasma Control
- Supporting Technology (Technology Panel)

(3) Advanced Toroidal Research - Second Stability (4.6): up to $33M/yr
- Present and new toroidal devices (PBX-M, ATF/SRX, V-Mod)
- $q_0 >> 1$, D-^{3}He (CANDOR)

(4) Alternate Concepts (4.7): $10M - $33M/yr
- RFP
- Stellarator
- CT
- Dense Z-Pinch

(5) Steady State/High Duty Factor Experiments (4.8): $5M to $20M - $40M/yr
- Superconducting Tokamak (STE)
- ATF
- ZT-H
- (Tore Supra)

Long Term Initiatives (4.9):

(1) Advanced Tokamak $50M/yr per facility

(2) ATF-II $30M/yr

(3) ZT-I $200M - $335M capital cost

Figure 3 is a time bar chart showing possible implementation of these initiatives.

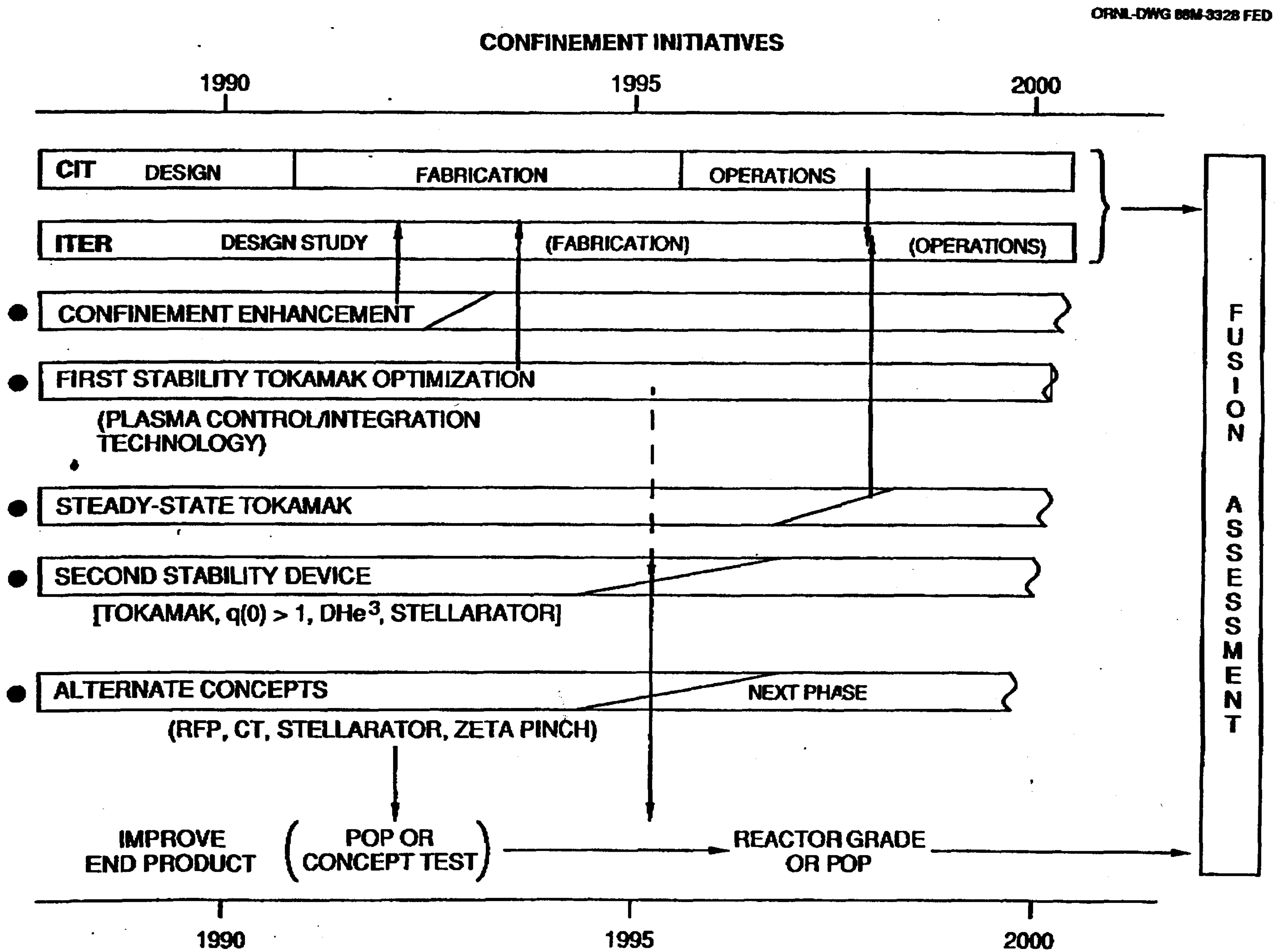

Fig. 3 Time bar chart of confinement initiatives.

4.4 Improving Predictive Capability for Confinement Enhancement

4.4.1 Background It seems to be generally agreed that energy confinement must be significantly improved if fusion is to become a viable energy alternative. One approach to this involves seeking innovative device concepts which solve the problem without the need for a detailed understanding of turbulent transport. Another approach is to try to understand the dominant physical phenomena which determine turbulent transport with the hope that this will lead us to ways of significantly mitigating this transport. This approach is discussed below.

4.4.2 Comments on Turbulent Transport: For the last thirty years, the U.S. Department of Energy has with remarkable consistency supported a theoretical effort aimed at understanding turbulent transport, and it is now beginning to bear fruit. It is giving us the tools, in the short-run, to improve our predictions of confinement for CIT and ITER and, in the longer run, to give us the physics insight to develop devices which will optimize confinement.

It is recommended that a new initiative be undertaken to make use of these new tools in the main line devices so that we can understand the origins of turbulence in different configurations. Just how such a program would fit into and function within the fusion program is discussed below.

Historically, turbulent transport has been described in terms of experimentally determined transport coefficients, e.g. χ_E. As the experimental data base grew, empirical sealing laws for χ_E, etc., were developed for different devices and operating conditions. These have become the principal tools in predicting the performance of new tokamaks, such as CIT. Unfortunately, this does not allow us to make reliable predictions for new tokamaks in which different physical phenomena become significant. For example, change in edge conditions may change the underlying turbulent processes and thus alter the transport coefficients resulting from the turbulence.

What is needed is some insight into the mechanisms which control the underlying turbulent processes. To this end there is a growing body of turbulence theory, and a systematic program of theoretically-based transport modeling and comparisons with experiments is well under way. Significant progress has been made but we have a long way to go before we have the required theoretical understanding to predict reliable transport coefficients.

However, fitting transport coefficients to various data is not the most sensitive way to test and distinguish different theoretical models. Different types of turbulence have different "signatures" which can be measured experimentally; e.g. the spectra in both k and ω can be measured. Indeed, correlated measurements of both density and potential fluctuations in the body of the plasma have recently been made. The objective is to use those types of measurements to guide theorists in developing and testing their models.

The logic is thus: 1) experiments on turbulence (signatures) guide the development of theoretical models and 2) those models are used to predict turbulent transport.

These measurements are not easy (neither is the theory), and to be meaningful a large set of different data must be taken simultaneously, e.g., full turbulent spectra and accurate ion density and temperature profiles, to get η_i. More and better diagnostics are needed to do this. More over, it must be done on a variety of tokamaks, both large and small, with and without divertors, with and without auxiliary heating, etc. In addition, over a period of years, a number of new small scale devices may be required to facilitate this program.

4.4.3 Specific Proposals

(1) A National Confinement Task Force

(a) Description Regarding improvement of the general relationship of theory and experiment, we fully support the concept of a national task force (also proposed by the Plasma Science Subpanel) charged with the mission of providing more fundamental understanding of transport in reactor-relevant regimes. This task force would presumably contain individuals who are resident in each of the major experimental facilities, and these individuals would facilitate preparation and transmittal of fairly complete data sets from their devices to the entire group. In addition to analyzing data and comparing theoretical predictions to the experimental results, the task force would also suggest experiments designed to shed light on theoretical issue, to the planners at the major experimental programs.

(b) Output Expected In addition to providing a better scientific basis for the goal of assessing fusion's ultimate potential, it is expected that this initiative would both increase our predictive capabilities regarding confinement in future large devices, narrowing the margin that must now be built into those designs, and indicate or suggest methods of optimization which, if followed, could result in a more attractive fusion reactor. It is worth noting that transport, or global confinement, is the single most important characteristic of plasma behavior which drives the size and cost of a reactor. It also drives the cost and complexity of current drive required to produce steady state in the tokamak. Consequently, improving our predictive capability regarding transport, and identifying methods of optimization is an extremely high-leverage initiative which could lead to a greatly improved vision of a tokamak reactor.

(c) Cost The formation of a national Confinement Task Force has minimal cost, probably in the range of $1 M/yr, since it will primarily involve the reprogramming of the activities of personnel already in the fusion program.

(d) Schedule The Task Force can be constituted immediately and begin its work during FY'89. The overall goals of the Task Force are ambitious and results will probably initially be slow in coming. Major output from this initiative would be expected in early-to-mid 1990's.

(2) Special-Purpose Diagnostics

(a) Description A second aspect of improving our understanding in the confinement area is the development of special purpose diagnostics which will shed light on key theoretical issues. Specific examples which come to mind are measurements of the poloidal field, plasma potential and the turbulent ω, k spectrum in drift-wave and MHD ranges. Others will be identified by the National Confinement Task Force. To be effective, new diagnostics will need to be developed and the capabilities of existing diagnostics extended. In addition, significant machine time, even on the major devices, must be dedicated to carrying out the relevant measurements.

(b) Output Expected The output expected is much the same as that anticipated from the National Confinement Task Force initiative, namely, an increase of our predictive capability leading to the capability for optimization of the design of future steps. As a result of carrying out measurements of fundamental quantities, the relevance of a particular line of theory can be tested. Also by providing direct feedback to the theoretical community, improvements in theoretical understanding will occur.

(c) Cost The cost of developing new diagnostics specifically to shed light on key theoretical issues concerning transport is estimated at $5M/yr. An additional $5-10 M may be required for increasing machine time specifically to be made available for this purpose.

(d) Schedule Development of the special-purpose diagnostics will take on the order of 3-5 years.

Utilization of these diagnostics on the major facilities would produce important new information during the mid-1990's.

(3) Small-Scale Experiments

(a) Description The improvement of confinement and attaining higher efficiency of current drive are two key elements leading toward better tokamak reactors. These are not merely wishful thinking and several new ideas have been proposed. To test these ideas, new small-scale facilities as well as modifications to old facilities may be required. Several examples of such small-scale experiments follow.

It was found many years ago in the floating ring devices that plasma confinement deteriorates when the trapped particles move to the bad curvature region. Theory suggests that the tokamak plasma is susceptible to various trapped particle instabilities. The tokamak plasma can be shaped in such a way that the trapped particles are in a magnetic well; i.e. the configuration becomes a "maximum-J" configuration. Theory predicts the reduction or the elimination of trapped particle turbulence. If further analysis, presently underway at General Atomic, supports this concept, a small-scale experiment should be built to test the basic idea.

Another idea is to utilize a high β_p value in the second stability region. MHD activities are expected to be quite different and the transport associated with the MHD turbulence may be reduced. While this initiative overlaps with that of the second stability tokamak, and may be included there, the emphasis would be quite different. Here, the main thrust is on eliminating the main physics of transport by radically changing the MHD environment in which it is studied.

A third small-scale initiative example is in the area of current drive. The present current drive schemes have low efficiencies resulting from the fundamental relationship between the wave energy and the wave momentum. By using the fact that a circularly polarized transverse wave carries helicity, the absorption of which results in the transfer of the helicity to the plasma, the plasma current can be comparable to ohmic heating current drive. It warranted after further theoretical study, this idea could be tested on an existing machine by the addition of RF power.

(b) Output These new ideas are examples of concepts which are to be tested on existing devices or future small-scale facilities. The main output would be fundamental understanding, as well as the improvement in the tokamak concept. There certainly are more ideas to come, and it is an exciting prospect that by developing ideas such as those described above, a steady-state tokamak with good confinement may be on the horizon.

(c) Cost The cost of these relatively small-scale facilities or add-ons to existing facilities is in the range $5-10 M/yr.

(d) Schedule Facilities such as described above take 2 to 3 years to construct. It is expected that several of these will be initiated at staggered intervals during the next decade. New results will become available beginning in the 1990's.

4.5 Optimization of the First Stability Regime Tokamak

4.5.1 Background: The near term objective of this area is to extend the performance of the first-stability regime tokamak so that it becomes an attractive reactor configuration for the ITER. The most important feature is to increase confinement for a given plasma current. The longer-range objective is to develop an attractive configuration for a fusion reactor. In this case it is also important to increase beta for a given plasma current.

4.5.2 Initiative to Enhance Plasma Control

(1) Description The MHD and microstability of a tokamak depends on the plasma profiles of current, pressure, density and temperature. This initiative will use various techniques to determine the optimum profiles for plasma confinement. Previously, edge control of magnetic fields and plasma recycling has produced the High (H) mode, and central fueling with edge recycling control has produced the "super shot" mode. Extensions of these successful examples, guided by theory, should lead to an improved confinement regime.

(2) Plasma Current Profile Control Techniques now exist for modifying the plasma current profile using non-ohmic current drive such as lower hybrid waves and neutral beams. In addition, there is a significant effect on the current profile when beta poloidal is large. The large tokamaks (TFTR, Doublet III-D, and Alcator C-Mod) should each be equipped with at least one technique for non-ohmic current drive.

(3) Plasma Density Profile Control Theory indicates that the density profile is important for determining microstability. In addition, experiments indicate that peaked density profiles have good confinement properties. Plasma fueling techniques to modify the density profile include: cold gas, beams, and pellet fueling. These techniques are suitable for present-day devices. However,

more penetrating beams and pellets must be developed for the large plasmas of ITER. Edge density control techniques include divertors and conditioned graphite and pump limiters. The large tokamaks should have at least one controlled fueling techniques and one edge-control technique to provide density control.

(4) Plasma Pressure (Temperature) Profile Control Controlled heating profiles would provide control of pressure and temperature profiles. Technology needs to be developed to provide central heating in ITER-sized plasmas. In particular ECH at 280 GHZ should be developed with as much as 10 MW for an experiment such as Alcator C Mod leading to CIT. Using negative ion beams to control profiles would require beam energies of the order of 1 MeV.

(5) Results Expected The optimum profiles would be determined, and would provide maximum confinement times for first-stable-regime tokamaks.

(6) The Cost is estimated to be approximately $10 M per year, plus the earlier development costs. This does not include costs associated with high-power facilities.

(7) Schedule These activities should continue through 1993. If significant advances are made in a particular area, the work should continue toward a fusion reactor. Gradually, other areas would be phased out.

4.5.3 Initiative to Improve the Configurations of First-Stability-Regime Tokamaks

(1) Description Previous experiments have investigated the effect of geometric parameters on confinement. This initiative would extend those studies.

(2) High Magnetic Field The development of high-field materials and configurations would benefit the tokamak concept significantly. The large-aspect-ratio configuration should be evaluated.

(3) Low Aspect Ratio A low aspect ratio tokamak has been proposed as a means of increasing current and beta. This concept should be evaluated.

(4) The Cost is estimated to be approximately $10 M/year.

(5) Schedule complete initial tests by 1993.

4.6 Initiative in the Advanced Toroidal Area: Second Stability

4.6.1 Use of Present and New Toroidal Devices

(1) Description The key issue is improvement of attainable beta and transport through access to the second stability region. The existing initiatives are PBX-M and the ATF stellarator. The new initiatives would be the SRX and V-MOD.

(a) PBX-M: Study access to the second stability region in tokamaks via plasma shaping and current profile control.

(b) ATF: Study direct access to the second stability region in a stellarator using the Shafranov shift and edge shear to achieve the desired configuration.

(c) SRX: Access second stability using high aspect ratio, $q_o > 1$, and current profile control with off-axis neutral-beam injection.

(d) V-MOD: Access second stability at lower aspect ratio with $q_o \sim 2$, a conducting shell, current profile control, and 3 MW of ICH.

(2) Output Expected Success would lower the requirements on heating power, magnetic field, and plasma current, and would improve prospects for D-He^3 operation. Results from these initiatives would impact decisions on next-generation advanced toroidal experiments in the tokamak (Advanced Tokamak Experiment and/or STE) and stellarator (ATF-II) areas.

(a) PBX-M: Higher beta operation and longer maintenance of the second stability region with current profile control.

(b) ATF: High beta operation ($\langle\beta\rangle \geq 5\%$) and reduced transport in the second stability region; optimization and control of the second stability plasma for long pulses (5 - 20s).

(c) SRX: Study of second stability in a high-aspect-ratio tokamak with high β_p, control of MHD instabilities and the current profile with off-axis neutral beam injection.

(d) V-MOD: Study of second stability in the usual tokamak aspect ratio range and current profile control with lower hybrid current drive (HCD).

(3) Cost per Year (in the 1990-1993 period)

(a) PBX-M: Approximately $8M to continue operation and extend current profile control with ion Bernstein wave heating and LHCD.

(b) ATF: Approximately $3M for long-pulse, high-power RF heating and vacuum vessel cooling.

(c) SRX: Approximately $4M for construction and operatio

(d) V-MOD: Approximately $3M for construction and operation.

(4) Schedule

(a) PBX-M: Continue operation through the planned 1990-91 hiatus.

(b) ATF: Install high-power long-pulse ICH in 1990 and install vacuum vessel cooling, power supply modifications in 1991.

(c) SRX: A decision to proceed in 1989, construction in 1990-93, and operation in 1993.

(d) V-MOD: This would have the same schedule as SRX.

4.6.2 An Additional Initiative in the Advanced Toroidal Area:

(1) Description The CANDOR device with R ~ 1.7 m and I_p = 18 MA would combine high density, high magnetic field, and $\beta_p \sim 1$,

(2) Output Expected The object is to first achieve D-T ignition and then to proceed to study D-He^3 burning conditions. This is an Ignitor/CIT-like tokamak device which would be a modification or replacement of CIT.

(3) Cost per Year $15 more than the presently conceived CIT program in the near term.

4.7 Alternate Concepts

4.7.1 Background Work on magnetic configurations other than tokamaks is motivated by the desire for the best possible reactor product from the fusion program. Improvements in power density, simplicity, smaller unit size, and ultimately reduced cost of electricity are anticipated. Furthermore, a confinement concept capable of smaller unit size might be developed for less cost, which will be a major consideration as the program moves from the scientific feasibility phase into the engineering development phase.

4.7.2 Initiative AC1: Reversed-Field Pinch (RFP) Integrated Proof-of-Principle Experiment Description Experimental and theoretical results combined with systems studies indicate a strong potential for this high-beta, ohmically heated system. Favorable peer review has led DOE to invest in a major US facility (the Compact Physics Research Facility (CPRF) at Los Alamos) that will allow an RFP device (ZTH) to seek significant advancement of plasma parameters towards reactor relevance. What is needed in the near term is a commitment of added resources to the Los Alamos ZTH experiment that would allow an RFP integrated proof-of-principle experiment (an "E2" decision in TPA terms).

(1) Output Expected:

(a) Improved plasma parameters by means of increased current capability from 2MA to 4MA ($20M)

(b) Acquisition of a pellet injector for density/profile control ($2M)

(c) Low frequency oscillating field current drive studies ($20M)

(d) Divertor for impurity control ($10M)

(e) Diagnostic upgrades ($10M)

(2) Total Cost: $62 M

(3) Schedule: Preparation would be done in phase with ZTH construction/operation. Approximately $3M/year would be needed from FY89 - FY93 for detailed designs. $10 M per year would be needed from 1994 - 1998.

4.7.3 Initiative AC2: Better Use of Existing RFP Facilities

(1) Description In the near term a number of existing RFP devices are available for study of issues critical to the RFP concept. The devices include the MST at the University of Wisconsin, the OHTE at General Atomic, the Reversatron at the University of Colorado, and the ZT-40 and ZT-P experiments at Los Alamos.

(2) Output Expected For example the OHTE experiment is particularly well suited for study of basic RFP stability and confinement properties with a resistive boundary. Additional operating funds for MST and ZT-40/ZT-P (unfunded after FY 89) would allow timely investigation of critical

issues such as RFP transport, edge plasma, and boundary conditions.

(3) Cost per Year Approximatly $3-5 M

(4) Schedule OHTE, which has ceased operation, could resume operation in FY 89 at a cost of $2-3 M per year. Approximately $1 M per year for MST is needed throughout the near term. ZT-40/ZT needs approximately $2 M per year from FY 90 - FY 91.

4.7.4 Initiative AC3: Stellarator Development

(1) Description This would provide exploration of basic stellarator physics and configuration optimization in a small, university sized, low-aspect-ratio stellarator. Parameters would be $R < 1$ m, $a \sim 0.2$ m, $B \sim 1T$, $P_{heat} < 1$ MW, pulse length < 1 S.

(2) Output Expected Control of flux surfaces, study of particle transport and electric fields, divertor studies in a low-aspect-ratio configuration.

(3) Cost per Year Approximately $1 - 2 M

(4) Schedule With a decision in 1989, construction would take place in 1990-92, with operation in 1993.

4.7.5 Initiative AC4: Proof-of-Principle FRC Facility

(1) Description The key issues that have been identified for the intrinsically high-beta field reversed configuration (FRC) concept will be addressed in the LSX experiment under construction at Spectra Technology Inc. (operation in 1991) and an upgrade of the FRX-C facility at Los Alamos (operation in 1989). The evaluation of results from these experiments, anticipated about 1994, is expected to result in a major new proposal to improve plasma parameters.

(2) Output The FRC will be studied in a more reactor relevant regime ($n\tau_E \sim 10^{19} m^{-3}s$, $T \sim 4$ keV). However, the technology of formation might not be sufficiently developed to justify this as an E2 integrated proof-of-principle step.

(3) Cost Approximately $50 M for major device fabrication.

(4) Schedule Decision in 1994-95. Construction during 1995-1998. Operation beginning about 1999.

4.7.6 Initiative AC5: Fundamental Spheromak Studies:

(1) Description In addition to the existing University-scale experiments at Maryland and Berkeley, one or two more small-size experiments are needed to address issues of confinement and stability properties. Recent results indicate that reduced field errors can improve the global energy confinement by a factor of three and lead to increased T_e (> 150eV)

(2) Output Enough research activity to resume progress on the exploration of this type of compact toroid.

(3) Cost $2-4 M per year.

(4) Schedule To begin in FY 89.

4.7.7 AC6: High Density Z Pinch (HDZP)

(1) Description This novel and qualitatively different approach to magnetic fusion may provide the least expensive method of igniting small quantities of DT fuel. The combination of a low level of support from the Office of Fusion Energy (OFE)(~$150 K/year) and funds from other sources has allowed considerable progress. Both Los Alamos and NRL expect to conduct key scientific tests of the HDEP in 1989-90. If successful, this concept may lead in the near term to proposals to do DT burn experiments, leading to a pulsed 14-MeV neutron source.

(2) Cost A rough estimate is $1-2 M for the D-T Burn experiments.

(3) Schedule A decision would occur in 1990 with operation in about 1992.

4.8 Initiative Area: Steady-State/High Duty Factor Toroidal Devices

Steady-state, high power operation is a critical step in the development of all magnetic fusion approaches. Each approach has different steady-state development issues to be addressed, and different schedules:

(1) Tore-Supra (France): 30 sec pulse operation; first half of 1990's
(2) Steady-state tokamak Experiment (STE): operation in second half of 1990's
(3) ATF: steady-state operation in the first half of 1990's followed by ATF II
(4) ZTH: steady-state operation (F-θ pumping) in the second half of 1990's

4.8.1 <u>The STE Tokamak</u>

(1) <u>Objectives</u>

(a) To evaluate high power plasma-wall interaction in elongated, diverted tokamak geometry, for times long compared to surface-conditioning times.

(b) To control current profiles to enhance confinement and beta for times long compared to plasma L/R times.

(2) <u>Description</u> A medium size (R_o = 1.2 - 2.4 m) and current (I_p = 2-4 MA) subconducting tokamak, with hydrogen plasma, elongated-single/double null divertor geometry, reactor-grade plasma temperature and heat flux.

(3) <u>Relationship to Other Programs and Initiatives</u> The STE would extend long-pulse data from ATF and Tore-Supra to steady-state/high duty factor in an elongated, diverted tokamak geometry. Experience from STE would be vital to successful long pulse/steady-state operation of ITER. This role of STE is complementary to that of CIT. STE could also incorporate elements of other initiatives: ECH, high field, and access to second stability.

(4) <u>Expected Benefits/Outputs</u>:

(a) Improved high powered divertors and plasma-interactive components
(b) Enhanced beta and confinement through optimum current profiles
(c) Better tokamak reactors, conditional on inclusion of advanced features of ECH, high field, and second stability.

(5) <u>Cost and Schedule</u>

Design and R&D	FY 1989-91	$ 3-5M/year
Construction	FY 1992-96	$20-40M/year
Operation	FY 1996 on	$20-30M/year

4.9 Initiatives in the Mid-90's

The tokamak, stellarator and the RFP are in different phases of development. Accordingly the time schedules of the initiatives are different and are discussed separately.

4.9.1 <u>Advanced Tokamak Initiative</u>

(1) <u>Description</u> The near term initiatives on tokamak improvement are expected to yield definitive results on improved confinement, high β-value and better plasma control. Based on these results, one or more advanced

tokamaks with reactor grade plasma are needed in 1995-2000 time frame.

(2) <u>Output Expected</u> Integrated tests of better confinement (smaller χ), high β and steady state with high current drive efficiency and plasma control. These characteristics lead to economically and environmentally attractive tokamak reactors.

(3) <u>Cost</u> Approximately $50 M per year per facility.

(4) <u>Schedule</u> The first facility might start operation in 1994.

4.9.2 <u>Initiative on a Next-Generation Stellarator</u>:

(1) <u>Description</u> This would be a stellarator with superconducting coils and R ~ 2 m, a ~ 0.5 m, B = 4T, $P_{heating}$ = 10-20 MW, and divertor geometry. Expected parameters are $n_e > 10^{14} cm^{-3}$, T_i = 5-10 kV, $n\tau_e > 10^{13} cm^{-3}s$, $\langle\beta\rangle$ = 5-8%.

(2) <u>Output Expected</u> This would provide a demonstration of high-beta, steady-state operation in a low-aspect-ratio (tokamak range) stellarator with reactor-relevant parameters. It would also provide demonstration of steady-state heating, fueling, particle and power removal, impurity control and configuration optimization.

(3) <u>Cost per Year</u> Approximately $30M.

(4) <u>Schedule</u> A decision in 1990-91 would be followed by a construction start in 1993-94, and operation in 1997-98.

4.9.3 <u>An RFP Conditional Initiative</u>

(1) <u>Description</u> Projections of the present RFP database to a device with I_ϕ = 8-10 MA, a = 0.3 - 0.4 m, called ZTI, lead to an ohmically ignited D-T plasma. The ZTI device would demonstrate in a hydrogen plasma, the high beta (β_θ ~ 0.1 - 0.2) steady -state conditions needed in a commercial reactor. In addition, D-T operation would provide desirable neutron-source applications. The device would operate with efficient resistive (copper and aluminum alloys) magnets, a larmor radius of 1.8 - 2.0 m, a toroidal-field-divertor impurity control system and oscillating field current drive.

(2) <u>Output Expected</u> The following plasma parameters are expected to characterize ZTI: n_e = 5-6 $110^{20} m^{-3}$; $T_i \sim T_e$ = 5-10 keV; $n\tau_E > 10^{20}$ s/m^3 and $\langle\beta\rangle$ ~ 0.05 - 0.10. With D-T operation, fusion power output would be approximately 120 MW.

(3) <u>Cost</u> Conceptual design studies and associated cost estimates indicate a total cost of 335 M$ for a D-T facility and $200 M$ for a hydrogen facility.

(4) <u>Schedule and Cost Per Year</u> Assuming adequate support, research in the international RFP community through FY 87, including major confinement studies on RFX and XTH, should adequately address RFP proof-of-principle issues to allow the initiation of ZTI design. The design effort would continue through FY 2000, at which time a decision to proceed with construction would be made, pending full integrated proof-of-principle tests at 4 MA on ZTH. Construction is expected to take 4 to 5 years, with an associated budget of $40 - $50 M per year. For a "design-to-construct" project to be initiated in 1997, adequate proof-of-principle research on high current confinement, density control, current drive and divertor/impurity control are required.

4.10 Glossary of Acronyms

ITER	International Thermonuclear Experimental Reactor
ETR	Engineering Test Reactor
RFP	Reversed Field Pinch
CT	Compact Torus
DIII-D	Doublet III - D Configuration
PBX-M	Princeton Beta Experiment - Modification
Alc C-Mod	Alcator C-Modification
MTX	Microwave Tokamak Experiment
ATF	Advanced Toroidal Facility
ATF-II	Advanced Toroidal Facility II
CPRF-ZT-H	Compact Physics Research Facility Z-Pinch Toroidal-H
MST	Madison Symmetric Torus
LSX	Large s Experiment (a Field Reversed Configuration)
CIT	Compact Ignition Tokamak
SRX	Second Regime Experiment
V-Mod	Versator Modification
STE	Steady State Tokamak Experiment
TFTR	Toroidal Fusion Test Recorder
ECH	Electron Cyclotron Heating
AC1,2,3,4,5,6	Alternate Concept 1,2,3,4,5,6
OHTE	Ohmically Heated Toroidal Experiment (an RFP)
ZT-40	Z-Pinch Toroidal-40 cm radius
ZT-P	Z-Pinch Toroidal-P
ZTI	Z-Pinch Toroidal-I
FRC	Field Reversed Configuration
HDZP	High Density Z-Pinch
PWI	Plasma Wall Interaction

5. PLASMA SCIENCE SUBPANEL FINDINGS

5.1 The Role of Plasma Science

The fusion plasma program has developed sufficient experience and predictive capability for the community to conclude that a large, successful engineering test reactor almost assuredly could be built. The attractiveness of the present design, as well as that of reactors that would follow it, apparently needs to be improved considerably for economic and other reasons. With this awareness a new class of questions arises and the need for different attitudes appear. With our base of understanding in plasma science, we can plan satisfactorily for CIT, but we require much greater knowledge in order to plan beyond. Not only do we need to know more, but we must also modify our thinking about fusion problems to more traditional scientific lines, away from the dominantly phenomenological pattern that has served us well up to this time. The proposed initiatives described below are aimed at the two goals of providing additional scientific understanding and emphasizing a more traditional scientific approach.

The state of our knowledge and our ability to present our work as a solid scientific discipline also affect the perception of fusion by our scientific and engineering colleagues in other disciplines. As we enhance plasma science, we also improve the credibility of the fusion initiatives.

Finally, good work in plasma science also enables us to attract and retain outstanding young scientists into a field that otherwise may appear to have very long pay-off times.

5.2 The Action Initiatives

We present three initiatives in order of priority

(1) Experimental Initiative in Medium Scale Experiments
(2) A Task Force Approach to Critical Problems in Plasma Science, in particular Confinement and Transport
(3) Enhancement of the Scientific Output of State of the Art Devices

5.3 Medium Scale Experiments

5.3.1 Importance and Rationale A significant improvement is needed in the level and health of medium and small scale experiments. Such experiments are the most effective means for exploring, testing and developing innovative new fusion ideas and concepts. They are also critical for the development of the areas of fusion plasma science that can lead to new directions and increased performance of the front-line fusion experiments. Some examples of contributions of this type in the recent past are Alcator (high density, magnetic field and confinement), Octupole (bootstrap current), Versator (lower hybrid current drive), and ISX-B, TORS-II, D-III and PBX-M (high β).

As indicated below, there are many medium scale experiments that should be performed that would support CIT, help optimize ITER and develop a more attractive fusion end product. Plasma science research on this class of experiments provides the attraction and excitement that draws young scientists into the field and retains those presently involved, thereby continually rejuvenating fusion. However, the health of the area of medium and small scale experiments has become quite fragile, and it needs to be reinvigorated, as indicated below, so as to encourage innovation, to develop fusion plasma science, and to ensure the future intellectual vitality of the fusion community.

5.3.2 Funding Needs The budget for small and medium scale experiments has decreased appreciably. The FY 89 budget for innovative, medium scale experiments (PBX-M, MTX, TEXT, LSX, FRX-C, Phaedrus, ZT-40M, MST, MS, CCT, VERSATOR, PISCES) is about $44 M. Smaller experiments add another $12 M per year. The total budget for both categories has decreased by about $7 M per year for the past two years and is due to drop an additional $12 M in FY 90. These progressively decreasing budgets have considerably reduced the construction, operations and diagnostic complements of the medium scale devices, causing the time scales for them to achieve their objectives to be stretched out substantially. Coupled with the flat funding profile, in the present five-year DOE plan, there is no prospect for new starts or significant upgrades of present medium-scale devices for the next five years.

An increase of $25 M/year over the FY 89 budget of $55 M/year is needed for small and medium scale experimental initiatives. This increase allows both for some expansion of the operational effort and provides capital for new starts. The increase has the following components:

(1) Operations of present facility enhancements and new initiatives: Improved diagnostics ~$5M, experimental modifications ~$5M — $10M

(2) Construction of new facilities: support of basic fusion science, innovative approaches to fusion — $15M

About 20% of the operations increase should be used for increased theoretical support. The $10M increase for operations allows for an extension of present efforts. The $15M capital increase allows for continual replacement, and some small augmentation, of the approximately $100M of installed capital base (10 medium scale and a number of smaller scale experiments) at the rate of 1 or 2 moderate scale, and several small scale, experiments per year.

5.3.3 Needed Experiments Some examples of needed experiments illustrate their contributions both to the CIT and ITER projects, and to the development of a predictive plasma science capability and more attractive fusion reactor concepts. A list of sample topics well suited for investigation in medium sized experiments follows. The subjects are selected for their scientific value and their importance to CIT, ITER or the fusion end-product. The list is illustrative of the major contributions to fusion that can accrue from this class of experiments; it is not prioritized, nor is it complete. All experiments can influence the end-product, while many directly affect CIT or ITER, as indicated.

(1) High Beta Physics

(a) First Stability Boundaries: Determine the cause of the first stability beta limit by investigating profile changes and internal instabilities in a device with an easily accessed beta limit (CIT- and ITER- relevant)

(b) Second Stability Physics: Through current profile tailoring and heating, produce plasmas in the higher beta second-stability regime (ITER-relevant)

(c) RFP Beta Limits: Through internal profile and fluctuation measurements, and controlled beta variation, determine the cause of the observed beta operational range

(2) Current Drive and Profile Control

(a) RF and Neutral-Beam Profile Control: Establish techniques for tailoring the current density profile (ITER-relevant)

(b) Bootstrap Current Experiments: Measure the self-induced current density profile under varying collisionality and pressure (CIT- and ITER-relevant)

(c) Profile Effects in RFP's: Measure density, temperature, and magnetic field profiles for evaluation of transport coefficients and current drive requirements

(3) Configuration Studies

(a) Energetic Particle Effects: In both tokamak and FRC devices, determine the effect of high energy or large gyroradius particles on equilibrium and stability (CIT- and ITER- relevant)

(b) High Energy Density Plasmas: Examine stability and assess the fusion potential of the dense z-pinch

(c) The Shell RFP: Determine the effect of a shell with short electrical penetration time on stability, transport, and sustainment

(d) High and Low Aspect Ratio Tokamaks: Assess effect on stability (e.g. beta limits) and transport of aspect ratio variation (ITER-relevant)

(e) Low-Aspect-Ratio, Nearly-Hellically-Symmetric-Stellerator: Assess equilibrium, stability and transport in optimized configurations.

5.4 Task Force Approach to Critical Plasma-Science Problems of Fusion Development

5.4.1 Importance and Rationale An initiative is proposed to form a number of task forces to address critical problems of magnetically-confined plasmas, whose resolution would have major impacts on fusion development.

As an initial step, a Task Force on Energy Confinement and Cross-Field Transport in Tokamaks should be formed as soon as practicable.

Although confinement and transport have been major components of toroidal research from its outset, the underlying plasma science has not yet advanced to the point where reliable predictions can be made for devices on the scale of the proposed ITER. Moreover, lack of understanding of the process of cross-field transport is, in many cases, the principal obstacle to progress with present-day tokamaks. However, the increased ability of front-line tokamak experiments to provide the detailed data needed for confinement studies, for example, ion temperature profiles and q(r)-profiles, and the availability of fluctuation spectrum measurements from moderate sized tokamak suggest that a coordinated attack on the confinement problem may prove fruitful.

The Task-Force Approach could be extended in the future to other areas of plasma science critical to fusion development. Examples of these include Tokamak Configuration Optimization (identifying the optimum magnetic geometry and plasma-control techniques for achieving a high value of beta at moderate plasma current), and Current Drive (identifying techniques for improving the efficiency of non-inductive current drive).

5.4.2 The Task Force on Confinement and Transport

(1) Objectives The specific objective of the Task Force on Confinement and Transport will be to mount a coordinated effort to characterize and understand the processes of cross-field transport in tokamaks, with a view to identifying ways to reduce heat transport and improve overall energy confinement. The long-term objective will be to develop a physics-based predictive capability that can be used to reliably determine the confinement requirements in future devices such as ITER. A shorter-term objective will be to conduct a critical assessment of present theoretical models in relation to available experimental data on confinement. As improved theories become available, these also will be tested in a systematic and critical way against confinement data.

(2) Composition of the Task Force The Task Force on Confinement and Transport will be multi-institutional, and it will include experimentalists engaged in data analysis, theorists working on cross-field transport problems and computer-modellers engaged in transport-code development and application.

There will be a small core-group whose members will devote their research activities predominantly to the work of the Task Force. The core group will coordinate the work of a number of specialized task groups whose members will participate on a fractional-time basis in particular activities of the Task Force. There may be an Advisory Committee composed of senior members of the scientific community.

(3) Reporting by the Task Force The Task Force will report to the Directors of the Confinement Systems Division and of the Applied Plasma Physics Divisions of the Office of Fusion Energy, jointly.

(4) Activities of the Task Force The Task Force will coordinate and lead a focused national effort on the issue of cross-field transport in tokamaks, and it will work to promote an increased awareness of relevant problems within the experimental and theoretical communities.

The Task Force will encourage and solicit experimental investigations aimed at shedding light on transport issues in tokamaks and it will seek to advance the development and application of appropriate diagnostics. The Task Force will also encourage relevant theoretical developments, as well as the extension and application of appropriate transport codes. The Task Force will seek to influence the direction of experimental and theoretical work on tokamaks and to provide increased focus on the physics of cross-field transport.

Using its capabilities in data-analysis and transport-modelling, the Task Force will attempt to compare the experimental data with the predictions of various theories of cross-field transport. The Task Force will coordinate the enlargement of computational capabilities, if found to be needed, to accommodate the relevant experimental data-base or the theoretical models. The Task Force will attempt to stimulate the development of new theoretical work, as appropriate.

The Task Force will transmit its recommendations on experimental and theoretical priorities in parallel to the relevant program leaders or laboratory directors and to the DOE Office of Fusion Energy.

(5) <u>Resources Required</u> The Task Force will request the release of members of the core group from other substantial research responsibilities. Members of the specialized task groups should be released on a fractional-time basis for specific periods of time. Access to all available data-analysis and transport-modelling codes must be provided. In many instances, it may be necessary to provide programming support to modify these codes to add additional physics or to test new theoretical models.

The commitment of program leadership to the importance of this initiative must be sufficient to ensure an accommodating response to requests for experimental run time for specific confinement studies. For maximum effectiveness, all of the U.S. tokamaks should participate in the experimental studies. In many cases, dedicated experimental run-time will be required. In other cases (especially TFTR), there will be compilations of confinement data that have been obtained on the basis of non-interference with other experimental objectives.

The Task Force will seek to obtain relevant experimental data from foreign tokamaks, but it should not be expected that this will be provided in as great detail, or in as timely a fashion, as the data from U.S. devices.

Direct funding will be required for incremental travel expenses of Task Force members, especially those from university groups and possibly also for support staff. The needed funding is not likely to exceed $1.0M, annually.

5.5 Initiative to Enhance the Scientific Output of State-of-the-Art Machines

5.5.1 Use of Mainline Devices for Basic Measurements The missions of the largest, state-of-the-art machines tend to emphasize reaching new parameter regimes. Consequently comparatively few resources are available to do the detailed studies needed to understand the physical processes which actually determine these parameters. The emphasis of the mainline program needs to be shifted to apply more resources to the scientific issues. Areas in which more detailed understanding and a more reliable predictive capability would impact the CIT and ITER programs include: transport, beta-limits, configurational optimization, and current drive. To increase the scientific effectiveness of the programs more time should be made available for experiments which contribute to understanding the dominant physics. To realize increased scientific productivity on the mainline experiments (D-IIID, Alcator C-MOD, ATF, ZT-H) would entail increased running time and increased scientific staff, including theory and computation.

Funding: Estimated funding for these purposes is about $10 million/year.

5.5.2 Extended Use of Mainline Devices Scientific studies can also be performed very efficiently on state-of-the-art machines after they complete their original mission. By that time a great deal of operational experience and a large existing data base have been built up. Consideration should be given to maintaining such machines as experimental user facilities for detailed physics studies over a period of perhaps three years. As an example, D-IIID could provide a useful facility for confinement, configurational and rf-heating physics studies at a budget reduced from its current level. Over the next decade a number of additional machines will be in a similar situation, e.g. Alcator C-mod, ATF, and ZT-H.

Funding: Approximately $20 million/yr should be allocated to extended lifetime of mainline experiments for predominantly scientific purposes.

5.5.3 Increased Diagnostics For the currently operating and oncoming experiments, increased support should be directed toward diagnostic needs. The diagnostics required to elucidate the underlying physical processes and make meaningful comparisons with theory must be more complete and more specialized than are typically used for measurement of gross plasma parameters and for plasma control. Examples of needed new or improved diagnostics are: measurement of current density profiles (polarizations of impurity radiations, Zeeman splitting, etc.); measurement of ambipolar potentials using heavy-ion beam probes, and measurement of fluctuation spectra and correlation functions (laser and microwave scattering).

Funding: Anticipated funding needs are approximately $5 million/year.

5.5.4 Existing Diagnostics Support should also be given to provide increased availability of existing diagnostics. This would build a sufficiently complete data base to allow observation of correlations between various effects, framing of hypotheses, and meaningful comparison with theory.

Support is also needed for transfer of diagnostics from the tokamak program to the alternate concept programs so that sufficiently reliable information can be obtained to understand the underlying physics.

Funding: Availability of existing diagnostics: Approximately $5 M/year. Transfer of diagnostics: Approximately $3 M/year.

6. FINDINGS OF THE TECHNOLOGY SUBPANEL

6.1 Introduction

The present focal points of the US technology program are the US CIT ignition tokamak and the International Tokamak Engineering Reactor ITER.

In this report we consider additional action initiatives in the technology program that will provide maximum leverage for an attractive fusion reactor in terms of cost, safety and environmental features. This can be achieved by:

(1) An improved vision of an economic, safe and benign fusion energy source; i.e., by product optimization in terms of commercial attractiveness and advanced materials for fusion, and

(2) The development of enabling technologies, involving

(a) Technology test facilities
(b) Advanced materials
(c) A high-energy neutron source
(d) Technology for plasma profile control and improved stability
(e) Steady state physics and technology integration

6.2 Improvements in Commercial Attractiveness

The mission is to expand the fusion systems design effort to show the potential safety, environmental and economic aspects of the commercial end product.

Technical features are: selected commercial reactor studies including the high field tokamak, a D-^{3}He reactor study, and reactor confinement concept improvements.

As background, a continual and integrated national systems design effort to exploit the compelling advantages of fusion energy is essential. Such a program is necessary in order to impact the direction of fusion research in the next decade (e.g., low activvation, advanced fuel, improved confinement concepts, etc.) There is a need to counter the impression that the current fusion program may not be leading to the most attractive commercial end product.

6.3 Advanced Materials for Fusion

The mission is to identify and develop advanced materials which will support more aggressive designs and improve the safety, environmental and economic attractiveness of fusion as an energy source.

Present fusion devices and reactor concepts have very limited utilization of advanced materials such as structural ceramics, composites, high strength-high temperature alloys, etc. Development of such materials would significantly improve the attractiveness of fusion as an energy source. Reducing size (higher allowable wall loadings, higher-field magnets, etc.) and increasing potential thermodynamic efficiency (materials with higher temperature capability) will improve the economics. Low activation materials will improve the safety and environmental attractiveness.

Technical Features are to:

(1) Develop structural materials and superconductors suitable for magnets operating at 15-20 Tesla.

(2) Develop structural ceramics, composites, and other advanced tailored materials for blanket structure, HHF and PIC components.

(3) Develop very low activation blanket structural materials.

A schedule of approximately five years is required for "proof-of-principle" experiments in the development of new materials. Early initiation of this activity will allow testing of concepts utilizing advanced materials in Phase 2 of the ITER program and support development of improved reactor designs.

6.4 Technology Test Facilities

The use of existing facilities for the development of blankets, tritium technology, high-heat-flux components and high field magnets has proceeded as far as possible. A limited number of new specialized test facilities is required in the early-to-mid 1990's to make possible the development of enabling technology, and to carry out safety-related experiments. Maximum use of existing equipment and international collaboration is made to minimize cost.

Specific proposals include the following:

(1) Nuclear Test Facilities

(a) Liquid metal MHD and heat transfer test stand
(b) Solid-breeder module test in a fission reactor
(c) Engineering-scale tritium breeding/neutronics experiment
(d) Breeding-blanket interface upgrade of TSTA
(e) Activation product release and transport tests.

(2) High Heat Flux (HHF) Plasma Materials Interaction (PMI), and Impurity Control Tests

(a) Prototype tests of high-heat-flux components
(b) Upgrade plasma surface test stands to reactor conditions.

(3) A Magnet Test Stand

Upgrade HFTF using MFTF-B choke coils to provide a 14 Tesla large volume.

In the area of <u>international participation</u>, we expect significant contributions from the European Community (EC), Canada, and Japan in the nuclear and HFF/PMI area.

6.5 High Energy Neutron Source

The mission is concept selection, preliminary conceptual design, and scoping R&D of a neutron source for the development and qualification of materials for fusion.

Technical features include:

(1) High energy neutrons in an appropriate fusion operation (i.e., 14-MeV) to produce radiation damage as in the fusion environment

(2) Sufficient flux and experimental volume to support materials development and qualification (i.e., 100 dpa/y in 500-1000 cm^3)

This is critically important to the development of low activation alloys, ceramics, and other advanced materials for fusion. It is also necessary for the development of a data base to support engineering design and licensing. This has been identified as a high priority, required facility by three international panels in the last decade as well as by MFAC. The proposed study should lead to a 1991 decision to proceed with an engineering design.

International aspects A three year study under auspices of the IEA is presently in progress. The goal is to identify, evaluate and select a concept and to undertake a multinational preconceptual design to support a decision to proceed on engineering design and construction. There are excellent prospects for international collaboration.

6.6 Profile Control and Improved Stability

The mission is to improve confinement concepts with new technologies for profile control and improved stability.

Technical features are "smart" heating and current drive, with central fueling, which should allow discovery and exploration of new, better (less restrictive) regimes of operation. Specific technologies include high frequency, high power microwave sources; MeV ion beams; and high velocity pellets or plasmoids for fueling.

International participation is anticipated as follows:

(1) Microwaves; none anticipated.

(2) Neutral beams; ion beams: substantial cooperation is possible.

(3) Fueling: some help is possible.

6.7 Steady State Physics Technology Integration

The availability of a steady state tokamak with reactor level heat flux and plasma conditions would be extremely useful for plasma interactive tests of materials and concepts consistent with edge physics phenomena. It would also provide a test bed for various profile and stability control technologies in an appropriate environment.

6.8 The Technology Initiatives

6.8.1 Feasibility of a Safe, Environmentally Attractive Power Conversion System

Mission: Activities Leading to the ability to build an attractive, low activation test module.

Elements:

(1) Designs of safe, environmentally attractive modules
(2) Selected materials research
(3) Approprate test facilities (small scale)
(4) Studies of a neutron test facility

Cost:

	New	Base
First year	$14 M	$15 M*
Second year	14	15
Third year	14	15
Fourth year	13	15
Fifth year	7	15

*(Materials 7M, Nucl. Tech 7M, Sys., Safe 1M (1/4))

International Aspects: Close collaboration is fruitful and desirable.

6.8.2 Initiative 2: An Attractive Commercial End Product

Mission: To show potential economic, safety, and environmental attractiveness of fusion through selected, in depth system studies.

Elements:

(1) High field tokamak reactor
(2) $D-{}_3He$ reactor
(3) Reactor confinement concept improvement

Cost:

	New	Base
First year	$ 3 M	$ 3 M*
Second year	3	3
Third year	4	3
Fourth year	4	3
Fifth year	4	3

*(3/4 Systems, Safety, Environmental)

International Aspects: This effort uses international data bases but asserts U.S. leadership in innovative approaches.

6.8.3 Technology to Improve Plasma Performance

Mission: To develop experimental tools to discover and explore new and better (less restrictive) regimes of plasma operation.

Elements:

(1) Heating and current profile technologies
(2) High field conductor test/development
(3) Particle and power control technologies

Cost:

	New	Base
First year	$ 6 M	$ 30 M*
Second year	14	30
Third year	13	30
Fourth year	12	30
Fifth year	6	30

(Magnetics 7.5M, Heating, Fueling 16M, PIC 6.5M)

International Aspects: A mixture of U.S. dominated and internationally intensive activities. Coordination will be very beneficial.

6.9 Incremental Cost Schedule

	Base	1st Yr	2nd Yr	3rd Yr	4th Yr	5th Yr
<u>Initiative 1</u>						
Nuclear facilities		6.0	6.0	6.0	6.0	0.0
Advanced materials		6.0	6.0	6.0	6.0	6.0
1/4 Systems/ Safety		1.0	1.0	1.0	1.0	1.0
Neutron Source		1.0	1.0	1.0	0.0	0.0
Subtotal	15.0	14.0	14.0	14.0	13.0	7.0
<u>Initiative 2</u>						
3/4 Systems/ safety	3.0	3.0	3.0	4.0	4.0	4.0
<u>Initiative 3</u>						
ECH		1.0	2.0	3.0	3.0	1.0
Negative-Ion beams		1.0	3.0	5.0	5.0	3.0
Fueling		0.5	2.0	3.0	3.0	2.0
Magnetics		1.5	2.0	0.0	0.0	0.0
PIC		2.0	5.0	2.0	1.0	0.0
Subtotal	30.0	6.0	14.0	13.0	12.0	6.0
TOTAL	48.0	23.0	31.0	31.0	29.0	17.0

Department of Energy
Washington, DC 20585

JUN 7 1988

MEMORANDUM FOR Dr. Fred L. Ribe
Chairman, Magnetic Fusion
Advisory Committee
University of Washington, Benson Hall BF-10
Seattle, Washington 98195

SUBJECT: Charge to MFAC on Fusion Program Planning for the Early-to-Mid 1990's

I request that the Magnetic Fusion Advisory Committee address the issue of fusion program planning for the early-to-mid 1990's. The Department of Energy has proposed to begin construction of the compact ignition tokamak (CIT) focused on the issue of the physics of burning plasmas. This is the major initiative envisioned during the early 1990's. Considering the other issues requiring resolution prior to an assessment of the feasibility of fusion, what new initiatives should the program be preparing to take in the early-to-mid 1990's and in what order? Consideration should be given to plans of other national programs, with the goal of positioning the U.S. program to both contribute to and benefit from the world program.

Given the extensive documentation of the TPA Reports, a lengthy MFAC panel report is not necessary; a letter report should suffice. I would appreciate having your response by the end of September 1988.

Sincerely,

James F. Decker
Acting Director
Office of Energy Research

Appendix A