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Final Report of the Ad Hoc Experts Group on Fusion

June 1978

**U.S. Department of Energy
Directorate of Energy Research**

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Final Report of the Ad Hoc Experts Group on Fusion

June 1978

**U.S. Department of Energy
Directorate of Energy Research
Washington, DC 20545**

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This report has been prepared for, and at the request of the
Fusion Review Committee of the Department of Energy R&D
Coordination Council.

FUSION REVIEW COMMITTEE

Chairman: Dr. John M. Deutch, Director of Energy Research

Members: Mr. Robert D. Thorne, Assistant Secretary for Energy Technology
Dr. Donald M. Kerr, Acting Assistant Secretary for Defense Programs

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Executive Secretary: Dr. Robert A. Summers, Department of Energy

With the assistance of the Fusion Review Staff:

Dr. Alexander J. Glass

Dr. Michael Roberts

Department of Energy
Washington, D.C. 20545

JUN 7 1978

MEMORANDUM FOR John M. Deutch
Director of Energy Research

FROM: Ad Hoc Experts Group on Fusion

SUBJECT: Report of Findings and Recommendations

On February 27, 1978, you requested the individuals identified below to conduct a brief review of the magnetic and inertial confinement fusion programs and report their findings and recommendations at an early date. We have met on two occasions--the first time for three days of briefings by headquarters and laboratory people, and the second time for two days to discuss our views and to generate this report. On each occasion, we were grateful for the opportunity to meet with you to clarify to us your interests and needs.

In addition, our sincere appreciation goes to the presenters from DOE headquarters and its laboratories for their efforts to improve our understanding of the fusion programs. In formulating our observations and recommendations, we were stimulated and benefited from discussions with Bob Summers, Alex Glass and Mike Roberts. Without their hard work we would not have pulled together this report.

In presenting this report it is important to recall that while we have been exposed to a wealth of information, it was not our desire nor your intention that we cover all of the important aspects of the programs which warrant careful review and assessment. Rather, as agreed, we concentrated our attention on just four areas: the objectives of the programs, the strategies being pursued, the program balance, and some aspects of management. In each of these areas, we found the need to advise you on our findings and suggest recommendations.

Tokamaks

At present, the U.S. magnetic fusion effort is dominated by Tokamak research. This is largely justified by the degree of success these machines have had in the past few years, as represented by the product of density and confinement time $n\tau \approx 3 \times 10^{13} \text{ cm}^{-3} \text{ sec.}$ and ion and electron temperatures $T_i \approx T_e \approx 2.3 \text{ KeV}$, - values which are within modest factors of those required for reactors. It appears that this approach has a high probability of achieving breakeven or with the Tokamak Fusion Test Reactor (TFTR) even net instantaneous fusion energy production (net with respect to the neutral beam power required to maintain the plasma); with very good luck ignition might even be achieved. Since a primary near-term goal of the fusion program is to achieve a reacting plasma, this effort should be pushed aggressively.

Despite their successes, some major questions remain for Tokamaks concerning the following issues:

1. The detailed physics of plasma and energy confinement times; theoretical explanation of the observed scaling laws.
2. The critical ratio of plasma to magnetic field pressure $\beta = (4\pi P/B^2)$ (P is plasma pressure and B is magnetic field strength) at which the plasma can operate.
3. Their behavior under long pulse conditions (can they be made steady state?).
4. Impurity control.
5. Lifetime of the first wall.
6. Practicality of Tokamak reactors. Are there significant ways to simplify them, reduce their size, improve their economics and engineering desirability?

Items (1), (2) and (4) are being attacked under present experimental programs. Addressing of points (3) and (5) requires the development of more advanced facilities. Point (6) is one of considerable concern to the utility industry and its supporting manufacturers and emphasizes the need for early engineering evaluation of reactor concepts.

Tokamaks may provide the earliest means to obtain a reacting or burning plasma on which physics studies can be made, and which could be used for materials and engineering tests. If present devices, as well as those under construction, i.e., TFTR (1981), Poloidal Divertor Experiment (PDX) (1979), Alcator C (1979) are pushed hard, a reacting plasma may be obtained in the early 1980's. To exploit these devices fully some upgrading of the machines may be required such as more beam power, higher energy beams, added radio frequency heating, or increased magnetic fields. While substantial burning may be obtained in devices presently under construction (TFTR and Joint European Torus, [JET]), they are not designed for the long-term burns suitable for engineering studies. Nevertheless, special Tokamaks may provide the earliest means for studying burning plasmas and long duration discharges at thermonuclear-equivalent conditions, and performing materials and engineering tests for reactor components.

The present profusion of Tokamaks which exists is probably the result of a "bandwagon" effect and represents an overextension of the program in this direction. Given fiscal constraints, the best possible use should be made of the existing facilities, including consideration of closing some facilities so as to extract the maximum information from others. If additional monies are available, these might be more productively spent on existing facilities than for initiating new devices.

Mirrors

Mirrors have achieved impressive plasma parameters, as represented by $T_e \approx 20$ KeV, $n \sim 1$. Mirrors have always been envisioned as energy multipliers. Their uncertainty lies in whether the marginal energy multiplication (Q) predicted for classical mirrors ($Q \sim 1$) can be improved upon significantly. There are at least two ingenious ideas to improve Q , namely:

- (1) The tandem mirror.
- (2) The field reversed mirror.

Critical questions for these devices are the stability of reversed field mirrors, which plasma losses will dominate in these devices when the more rapid mirror losses are reduced, the seriousness of electron heat conduction for tandem mirrors, what electron temperatures will be achieved in these devices, and how high a Q can be

achieved. The Tandem Mirror Experiment (TMX) (1979) and Mirror Fusion Test Facility (MFTF) (1983) facilities will provide tests for these ideas and may provide answers to these critical questions.

If either or both of these ideas succeeds, then mirrors may make attractive energy sources of modest size, capacity and expense relative to Tokamaks. It is also likely that they will be simpler to construct. If the fusion hybrid is considered, the relative simplicity of open magnetic confinement or linear machines may be particularly attractive.

To determine the feasibility of the mirror approach as an alternative reactor option, the TMX and the MFTF deserve an aggressive effort.

Alternative Concepts

There are many alternative magnetic concepts, in fact about a dozen or more (field reversed pinches, long solenoids, Linus, multipoles, etc.) It is clear that an attempt to support any large fraction of them will simply spread the money so thinly that none will have a significant chance.

However, there are two areas in which significant breakthroughs might be made which could change the whole picture. One is the area of "advanced" fusion fuels which could have considerable influence on environmental impact, engineering simplicity of the reactor, plant lifetime, etc. The second area involves devices which, with D-T fuels, could make a practical reactor considerably simpler to engineer, construct and maintain.

Using these considerations, only the most promising alternative concepts should be chosen and supported at a sufficient level (several million dollars/year) and for sufficient time (4 or 5 years) to test the concepts. They should then either be upgraded or dropped.

Plasma Physics

Plasma physics and our understanding of it is still a pacing item in fusion research (witness the relatively recent rise of Tokamaks as contenders for reactors and the recent and unexpected favorable dependence of Tokamak confinement time on density, $\tau \sim n$, found in Alcator). This effort could lead to improved machines with better confinement, concomitant reductions in size, and improvements in economy. It should receive substantial support which is shielded

against the funding needs of large hardware programs. Some support should be aimed at basic plasma physics to develop the understanding and the tools needed in applications to the fusion program. Perhaps such additional support should be provided by the Office of Basic Energy Sciences.

IV. The Inertial Confinement Fusion (ICF) Program

At present, the ICF program is less mature than the MFE program. It has fewer teams working in it, a less broad background of scientific effort and a smaller and less seasoned Washington leadership.

The principal reasons for pursuing the ICF program are that, (1) its scientific success seems likely and hinges on quite different physics than does that of MFE, and (2) the physical separation of the complicated and expensive driver mechanism from the target area is an attractive feature for reactor design.

The principal concerns are the energy/power requirements of the driver and the energy efficiency of the system.

Considerable progress has been made in the last few years toward a scientific feasibility experiment utilizing glass lasers, which are amenable to early and meaningful feasibility experiments but which have limited efficiency. Much less progress has been made toward providing the data to evaluate more efficient drivers and their target coupling characteristics.

The program consists of the following: (1) a large comprehensive project at Lawrence Livermore Laboratory (LLL) which is principally aimed at obtaining significant thermonuclear burn as soon as possible utilizing glass lasers (40% of total ICF program). This project includes calculational work on targets for several drivers, considerable diagnostic development, and a small effort in systems analysis and advanced laser development; (2) a large project at Los Alamos Scientific Laboratory (LASL) aimed at defining the potential of CO₂ lasers for inertial confinement fusion including CO₂ laser target design, and a small effort in systems analysis (21%); (3) a project at Sandia Laboratory (Albuquerque) aimed at the use of intense beams of high energy electrons and light ions to drive inertial confinement experiments (8%); (4) two other smaller experimental programs on inertial confinement using glass lasers (14% total); (5) an advanced laser technology program (10%); (6) a research program mainly involved with system studies and the development of ion beams (7%); and (7) less than 1% for long lead technology.

1. Both LLL and LASL need strong comprehensive target design capabilities to support their present programs. However, the target calculation, fabrication and diagnostics at LASL are not now adequate to support exploration of the CO₂ laser option. These should be strengthened as a matter² of high priority which, in our view, is more a matter of management than of additional money.

The LLL target design group which is doing excellent work in support of the ICF program has evaluated a range of possible drivers. However, we think that it is unwise to have only one group working on such a central problem and recommend that a second group be established which considers the full spectrum of possible drivers and associated targets. In view of their potential capabilities, the classification problem, and our recommendation in the previous paragraph, LASL is the appropriate place to establish this second group.

2. Both the LLL and LASL experimental programs with Shiva and the Eight-Beam System can go a long way toward evaluating the promise of larger, more expensive facilities. These experiments should be pursued as a matter of high priority. A portion of any funds that are added to the programs of the two laboratories should be devoted specifically to expanding these current investigations.
3. A facility larger than the Shiva or Eight-Beam System will be required to drive high gain targets. Both laboratories have begun the design of such facilities (Nova and Antares). Since there may be problems with coupling high power to targets (more so with Antares than with Nova), we have two recommendations: (1) Decisions to commit further significant funds to these facilities (aside from relatively small-cost, long lead items) should be delayed until the results of coupling experiments on the present facilities are in hand; and (2) in any event, such facilities should be designed so as to have the potential of growth to higher energy, if the growth is warranted.
4. The eventual feasibility and cost of the program depends on the driver energy at which high gain can be obtained from the target, which is, in turn, governed by the combined efficiencies of driver plus implosion geometry. Since all presently known lasers start out with relatively low efficiency, it is necessary to investigate other drivers. It is not sufficient to investigate only the technology of those drivers. The

drivers and implosion geometries must be investigated as systems. This part of the program needs considerable strengthening. Such investigations require careful calculations and diagnostics, including diagnostics of the details of the coupling mechanisms. The work should be carried out where the tools, expertise and support exist.

5. The work at Sandia includes a set of approaches that are promising from the point of view of overall efficiency, but its underlying science is particularly complex. The work is valuable, but considerations of driver requirements and target coupling are not well integrated. This warrants a special review by a group that would include experts on the specifications on driver requirements and the technologies to be utilized. Management steps should be taken to ensure proper program integration and balance.
6. The DOE laboratories, LLL, LASL, and Sandia, will continue to be national assets in this area, regardless of whether or not their present drivers are appropriate for the long term. Consequently, the program will benefit in long-term potential if these laboratories participate in the national program on other drivers, including but not limited to, advanced lasers.
7. Present classification policy may impede adequate assessment of new drivers and coupling systems at places other than the weapons laboratories. The classification policy should be reviewed now and on a periodic basis to make sure that no unnecessary hindrance is placed in the way of the program and that information which should be classified is properly protected.

V. Program Management

Implementation of the program strategy discussed above will require a more integrated headquarters management than has been necessary in the past. This need occurs because the recommended strategy must be implemented by efforts across a broader technical front involving more participants, and still provide timely data to support key program decisions.

The degree to which the management of the inertial and magnetic confinement programs should be combined depends chiefly on what is judged to be the primary motivation for the inertial confinement program. If the weapons technology applications of ICF are dominant,

then clearly the program management should remain separate as it is now. Should commercial energy rather than weapons technology application become the driving factor behind the ICF program, it would be preferable to have both ICF and MFE under common management. Even though the requirement for common management is not pressing in the near term, it does make a great deal of sense to develop, perhaps through the Fusion Review Committee, integrated program objectives, strategy and a coordination of critical milestones, since the potential of MFE and ICF for fusion energy should be judged on a common basis. Consequently, the pooling of data and cost information for reactor studies, and the establishment of common measures of performance is essential.

In addition to the above, we recommend the establishment of a unified fusion coordination committee involving the program leaders from the laboratories, external experts, and headquarters management to provide a continuing program review.

An explicit program participation policy should be developed clarifying and communicating the roles and responsibilities of headquarters, the DOE field offices, the national laboratories, industry, and universities. Because of the history of the relationship between the weapons laboratories and headquarters, some confusion exists and widely differing perceptions of what these roles should be are expressed by the various participants in the program. Clearly headquarters should be responsible for development and approval of program objectives, strategy, and plans, the monitoring and evaluation of results, and the presentation and defense of the program within the Department and to OMB and Congress. In carrying out these roles, the headquarters team should, of course, make extensive use of the technical expertise of the program participants. Where technical disagreement and competing views arise among the program participants, it should be the responsibility of the headquarters group to force the resolution of the issues by providing the forum for technical argumentation.

It was clear that such a headquarters group is established and functioning well in the magnetic fusion program. The equivalent group in the newer inertial confinement program is now evolving. The recommended strengthening of a second across-the-board center-of-excellence in the ICF program as well as involvement of other program participants in major subsections of the program will provide the basis for maturation of the proper headquarters role here.

While it was appropriate for the major activities in both programs directed toward the near-term scientific feasibility milestones to be pursued by the DOE laboratories, in light of the suggested strategy, the program would be strengthened by encouraging further participation by industry, universities, and the utilities. Where the DOE laboratories have unique capabilities, expertise, or facilities, or where control of classified information is essential, as in certain aspects of the ICF program, then the laboratories must play a dominant role. However, aggressive efforts should be made to take advantage of outside capabilities and potential wherever they exist to broaden and strengthen the scientific, technological and industrial base of the programs.

VI. International Cooperation

Since 1958, the magnetic fusion effort has been an exemplary one in terms of scientific cooperation among many nations. During this period, the U.S. has contributed its full share to this cooperation, contributing to international meetings and carrying on extensive scientific exchange programs with many countries, including those of Europe, the USSR, and Japan. This collaboration has been beneficial; many ideas have flowed back and forth which have significantly advanced progress. As we approach engineering test facilities and engineering prototype reactors whose expense will be large, more substantial collaboration may be desirable. The U.S. should continue to play a leading role in such cooperation. New initiatives, of course, should await the clear establishment and support of the direction of the U.S. program.

VII. Conclusions and Recommendations

1. Objectives

Observation:

There has been and continues to be considerable debate over the objectives and urgency of the fusion effort, the objectives and urgency of the nation's overall energy needs and the coupling of the two.

Recommendations

For the potential of fusion to be effectively determined over the coming years, an overall Departmental policy and consistent set of objectives should be established.

The objective of the programs should be to determine the highest potential for a commercial fusion energy source at the earliest practical date. To this end, a number of approaches to producing a reacting plasma must be evaluated, difficult engineering problems must be solved, the best combination of scientific and engineering promise must be selected, and the cost of a practical system must be determined.

Demonstration of scientific and technological feasibility should remain the near-term aim of the program. Its achievement should be a necessary, but not sufficient, step in the decision to proceed with the construction of an engineering prototype reactor. That decision should include the evaluation of the suitability of the various contending approaches for a reactor as well as the attainment of required technology.

2. Strategy

Observation:

The present fusion strategy is characterized as high risk; it emphasizes carrying the front running approach as quickly as possible to a demonstration of commercial feasibility while maintaining a vigorous backup program.

Recommendations:

Adopt a modification of strategy that reduces the risk by broadening and strengthening the technical base from which to choose the best fusion approach to practical energy production. This will surely require the commitment of significant funds within the next few years for facilities to obtain engineering and materials data as well as achieving scientific feasibility.

Pursue vigorously several physics approaches and carry out in parallel, engineering and materials test programs until at least one potentially economically competitive design is identified.

Lay the groundwork for one or more engineering test facilities to be committed as soon as practical after the potentially competitive designs are identified.

3. Programs

a) Magnetic Confinement

Observation:

The program has been successfully following a self-consistent logic supporting an internally generated objective and timing. With the recommended modification in strategy, there are associated changes in emphasis.

Recommendations:

Evaluate the critical physics and engineering questions, both individual and coupled questions, in Tokamaks and mirrors.

Obtain a thermonuclear burn in Tokamaks as quickly as practical. However, commitment to construction of a next generation Tokamak beyond TFTR, should not be made until results from TFTR and other related experiments justify it. A concern of the Group with the Tokamak concept was its apparent complexity as a reactor. Before commitment to a Tokamak-type engineering prototype reactor (an integrated energy-producing system), a convincing case should be made that Tokamaks can be engineered into attractive energy producers.

Select and support a few of the most promising alternative approaches to provide a real test of the concepts.

Observation:

Discoveries in plasma physics are still a pacing item in the magnetic fusion program as is demonstrated by numerous recent findings.

Recommendations:

Operate, modify, and exploit current facilities, both existing and under construction, to the greatest degree practical to accelerate the flow of essential physics and engineering information.

Insure a strong plasma physics foundation to support the fusion program; additional support should be made available, perhaps through the Office of Basic Energy Sciences.

b) Inertial Confinement

Observation:

The inertial confinement program is in a phase of rapid development with many driver-target options. This rapid change and the complications of classification form the basis for a set of specific suggestions.

Recommendations:

Operate, modify, and exploit current facilities, both existing and under construction to the greatest degree practical to obtain essential physics information.

Develop at LASL a fully competitive target design, coupling, and fabrication program.

Base further large commitments to planned facilities on formal reviews of the experimental results obtained with existing facilities.

Pursue the development of alternative drivers which offer the potential for achieving performance parameters required for eventual commercial use taking into account target coupling.

Review the objectives and technical status of the e-beam program at Sandia. Take management actions to assure program integration and balance.

Review the current classification policy, its impact on the program, and the protection of information which should be classified.

4. Management

Observation:

There are many participants in the fusion program management process with different perceptions of the various roles. When this complexity is coupled to the fact that the two fusion

programs have developed both separately and from different starting points in time and origin, there appear to be many areas for fruitful action.

Recommendations:

Implement a coordinated management of the MFE and ICF programs:

- o Develop a common basis for establishing program planning, a data base and ongoing evaluation.
- o Establish a unified fusion coordination committee involving the program leaders from the laboratories, external experts, and headquarters managers to provide a continuing program review.

Clarify, develop, and communicate the responsibilities and authorities of headquarters, laboratories, and the field offices, and develop the necessary competence in each group.

Expand and evolve appropriately the participation of universities, industry, and users as the program develops, e.g., in the areas of:

drivers/heaters;

systems engineering;

alternate concepts/drivers; and

physics investigations;

5. Items Not Fully Addressed

Observation:

In the five days of briefings and deliberations, it was simply not possible to deal adequately with every major issue in the complex fusion programs. However, four items have been identified as requiring a prompt and dedicated review.

Recommendations:

Determine the role of the fuel producing fusion hybrid in the overall fusion program.

Determine the potential changes in objectives of and limitations on the ICF program in the event of a comprehensive test ban treaty.

Conduct an in-depth review of the scientific and technological status of the fusion programs.

Determine the impact and potential of international cooperation on the development of the U.S. fusion program and recommend appropriate actions.

GLOSSARY

Fusion research, development, and commercialization can be divided into two principal stages:

Stage 1 - The determination of the potential of fusion as a practical source of energy (government as the primary sponsor); and

Stage 2 - The commercialization of fusion as an energy option (private sector as the primary sponsor)

Within Stage 1, there are two phases -

Phase 1 - Achievement of scientific feasibility and development of the foundation for Phase 2.

Phase 2 - Sequential achievements of technological and engineering feasibility.

Scientific Feasibility - Achievement of a reacting plasma in which fusion energy released exceeds the energy input to the plasma - is expected to require facilities such as TFTR/TFTR Upgrade, post-MFTF, Nova and Antares.

Technological Feasibility - Achievement of an elementary integrated system producing usable energy - is expected to require one or more engineering test facilities in which each of the technological requirements for a potentially economically competitive design has been met with at least the basis of a workable solution.

Engineering Feasibility - Achievement of an integrated system producing net usable energy in a manner indicative of an economically competitive design - is expected to require an engineering prototype reactor in which each of the technological and engineering requirements is met with a solution transferable to commercial designs.

Economic Feasibility - Achievement of an integrated system producing net usable energy in an economically competitive manner - is expected to require a commercial demonstration reactor in which each requirement is met with an economically competitive solution.

78 27 1978

Dr. John S. Foster, Jr.
Vice President, Energy R&D
TRW, Inc.
1 Space Park
Building R4-2004
Redondo Beach, CA 90278

Dear Dr. Foster:

The U. S. Department of Energy, through its Research and Development Coordination Council, intends to convene an Ad Hoc Experts Group to conduct a concise review of DOE's total fusion energy program, including both the magnetic confinement and the inertial confinement fusion programs. This will be an initial step toward establishing a DOE position on U. S. fusion energy research. As Chairman of the Fusion Review Committee of the DOE R&D Coordination Council, I am very pleased that you will be able to accept the invitation to organize and chair this Ad Hoc Experts Group.

The objective of the Ad Hoc Experts Group will be to review and present recommendations regarding the content and balance of the DOE fusion programs. For this purpose, the group will meet on March 14, 15, and 16, 1978 in Room 8222C, 20 Massachusetts Avenue, N. W., Washington, D. C. After a short Executive Session, the group will be briefed in some depth by the MFE and ICF Headquarters staff and by key participants from the national laboratories. Approximately one day will be allocated to each of the two programs, divided approximately equally between briefings and discussions. The third day will consist of a half-day of discussion followed by a half-day of drafting a brief written document. DOE staff will be available to assist in the documentation if the Group so desires. A list of Group members, a preliminary agenda for the meeting, and a list of some of the questions to be addressed are enclosed. We recognize that authoritative answers to all these questions will not be possible in the brief time available for this review. Additional background material will be forwarded to all members under separate cover.

Dr. John S. Foster

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I look forward to seeing you on March 14, 15, and 16. I have designated Dr. Robert A. Summers of my staff as my personal representative to work with you in carrying out this endeavor. He may be reached at the Office of Energy Research, Division of R&D Coordination (202/376-4051).

Sincerely,

"SIGNED BY JOHN M. DEUTCH"

John M. Deutch, Director
Office of Energy Research

Enclosures:

As stated