The EPRI Asilomar Papers:
On the Possibility of Advanced Fuel Fusion Reactors
Fusion-Fission Hybrid Breeders
Small Fusion Power Reactors

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Prepared by
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ELECTRIC POWER RESEARCH INSTITUTE

THE EPRI ASILOMAR PAPERS

ON THE POSSIBILITY OF ADVANCED FUEL FUSION REACTORS, FUSION-FISSION HYBRID BREEDERS, SMALL FUSION POWER REACTORS,

EPRI ER-378-SR

Special Report

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Prepared by

An EPRI Ad Hoc Advisory Panel

Prepared for

Electric Power Research Institute
3412 Hillview Avenue
Palo Alto, California 94304

ABSTRACT

An EPRI Ad Hoc Advisory Panel met in Asilomar, California for a three day general discussion of topics of particular interest to utility representatives. The three main topics considered were: (1) the possibility of advanced fuel fusion reactors, (2) fusion-fission hybrid breeders, and (3) small fusion power reactors. The report describes the ideas that evolved on these three topics.

An example of a "neutron less" fusion reactor using the p-^{11}B fuel cycle is described along with the critical questions that need to be addressed. The importance to the utility industry of using fusion neutrons to breed fission fuel for LWRs is outlined and directions for future EPRI research on fusion-fission systems are recommended. The desirability of small fusion power reactors to enable the early commercialization of fusion and for satisfying users' needs is discussed. Areas for possible EPRI research to help achieve this goal are presented.

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Section 1

INTRODUCTION

The Electric Power Research Institute, on behalf of the U. S. electric utility industry, is funding a small but broadly based fusion R&D program in support of the National fusion power program. Research and assessments in physics, engineering and system aspects of both magnetic and inertial confinement are being conducted.

On December 15, 16, and 17, 1976 a twelve-man EPRI ad hoc advisory panel met at Asilomar, California for a general discussion of topics of particular interest to utility representatives who are following the fusion program. The three main topics considered were:

- Advanced Fuel Fusion Reactors
- Fusion-Fission Hybrid Breeders
- Small Fusion Power Reactors

The panel members were diverse in background, consisting of theoretical and experimental plasma physicists, engineers of various backgrounds and electric utility personnel. In addition, the members came from various organizations--universities, national laboratories, public and private utilities, and EPRI. With this diversity there was an extremely lively and active exchange of ideas. A constructive and creative atmosphere rapidly developed. The end result was the writing and general agreement, in spirit, of the description that follows. I hope that the ideas that evolved in this three-day period and expressed in these "Asilomar Papers" will prove stimulating and useful to you.

William C. Gough
Program Manager for Fusion Power
Electric Power Research Institute

Section 2

ON THE POSSIBILITY OF ADVANCED FUEL FUSION REACTORS

Acceptance of new power systems by the public, by governmental legislative, executive and regulatory agencies, and by the utilities has become more difficult because of concerns with cost, emissions of various kinds, possibility of large accidents, limited resources, and waste heat. No system will overcome all these objections; only fission (with breeders or fissionable fuel factories), solar electric, or fusion have the capability to supply the need for the long term.

The problems with fusion as presently conceived are:

- Establishing scientific proof of principle.
- Solving large engineering problems arising from the fact that: the D-T reaction yields most of the fusion energy as 14 MeV neutrons, which leads to profound difficulty with material integrity, breeding tritium, thermal loading, etc.; and large and complex magnetic structures are needed.
- Difficult accessibility and maintenance problems caused by a structure that will be highly activated, unless high performance, low activation structural materials can be developed.

These problems lead us to to re-inspect the so-called "neutronless" fusion reactions, where all particles are charged, to see if some attractive scientific, technological, and engineering option space exists. Much of the power output emerges as moderately hard electro-magnetic radiation (x-rays), and the rest as charged particles. These circumstances lead to the possibility of a very high efficiency of energy conversion but, as might be expected, certain design difficulties exist.

The most likely reaction of this sort is

$$p + {}^{11}B \rightarrow 3\,{}^{4}He + 8.7\ \text{MeV},$$

although there are others. We shall use this as the main example. All these reactions have the physical disadvantages of:

- Significant reaction cross section only at very high energies, for example ≃ 0.9 barns at 675 KeV.
- Low power density, unless a very favorable confinement scheme can be found.

These difficulties have hitherto tended to minimize interest in such reactors.

It would be premature to disregard the possibilities existing with these reactions. For the utilities, the operational benefits from reactors working on these principles would be:

- The substantial reduction of radioactivity and neutrons should drastically reduce public concern (diversion, accidental releases, wastes, etc.). This probably the weightiest single reason.
- For at least the p-^{11}B reaction, fuels are practically inexhaustible and cheap; and the amount required for fusion is small compared to that already used in commerce.
- There being virtually no radioactivity, accessibility and servicing is much simplified.

For manufactuers and utilities the technical benefits include:

- The vacuum wall problem is entirely changed. This is very important because the vacuum wall-blanket is one of the worst problems in developing a D-T reactor. In particular, vacuum walls connected to external cooling systems (the qualification is important, as we shall see below) can be designed with much more confidence now. This compares strikingly with D-T reactors, where a satisfactory solution is not yet in sight.
- The blanket that surrounds the plasma is virtually eliminated, replaced by much thinner x-ray absorbing plates and heat transfer media.
- The structural integrity and life might be much improved.
- Easier access to the machine makes possible different methods of plasma manufacture, heating, etc.
- The appearance of the energy in the form of moderately hard x-rays ($\sim$ 110 keV) and charged particles presents opportunities to achieve exceptionally efficient conversion and use.

Against these advantages lie disadvantages, some of which have been mentioned. Others are:

- High particle energy and low power density per unit volume.
- The probable (but not certain) need for high β (β = plasma pressure/ magnetic pressure). This restricts the choice of the confinement systems likely to be useful.
- A good confinement geometry is needed, with low interior field, in order to reduce synchrotron radiation losses from the plasma. This circumstance may require the system to have internal conductors--unthinkable for D-T fusion systems, but conceivable for neutronless systems.
- Probable high circulating power, hence possible high cost.
- Sputtering and other material damage by heavy plasma ions.

- The necessity of a very high quality divertor, which acts not only to protect the vacuum wall, but also may be a direct electric converter.
- Much less knowledge, hence possible severe downstream problems not yet known.

Now follows a rough description of one possible reactor bodiment, using p-^{11}B and floating magnetic multipoles (to achieve high β and acceptable radiation rate).

AN EXAMPLE: A p-^{11}B SYSTEM WITH FLOATING MULTIPOLES

This example appears to be an attractive possibility, but the reader must realize that it (and all similar ideas) have thus far received little attention; hence the analysis to data is skimpy. As a corollary, this state of affairs bears out our contention that a class of potentially valuable concepts exists that needs more detailed exploration.

The attached figures show the geometry and principal parameters. Several key items are not shown, e.g., a divertor (or plasma pump), fuel injection, and mechanisms to cool or periodically replace some of the magnetically floating rings.

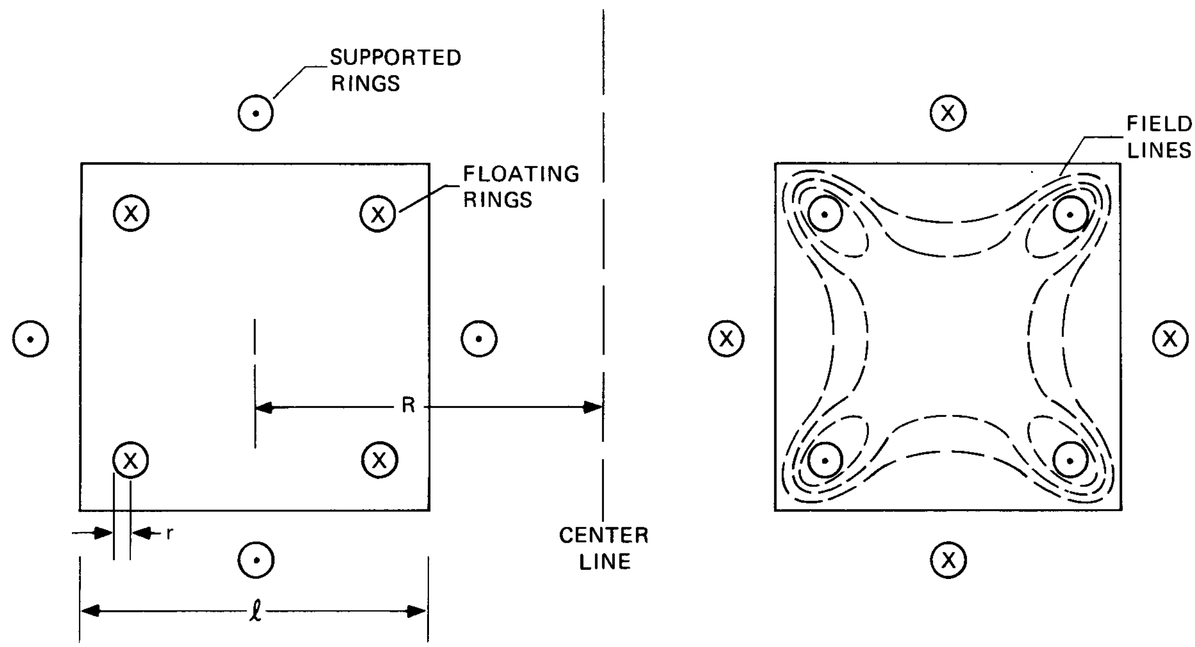
Shown successively are a cross section of a p-^{11}B floating multipole fusion reactor with parameters (Figure 2-1); one concept of how the wall can be designed (Figure 2-2); one concept for a high efficiency heat engine ($\simeq$ 65%) (Figure 2-3); a comparison of fusion reaction rates, as presently known (Figure 2-4); a concept of how the floating rings (which carry in excess of one million amperes each) might be designed (Figure 2-5).

CRITICAL QUESTIONS THAT NEED TO BE ADDRESSED FOR p-^{11}B FUELED MULTIPOLES

The questions in this area can be broken up into two convenient categories: (1) those which are critical to the successful operation of such a device and not subject to a straightforward, elementary analysis; and (2) those which are important or critical, but which could be addressed by analytical or simple experimental means. Answers to the latter questions could be available in a matter of a year or two; the former category could require five to fifteen years. Let us discuss the critical but not easily answered questions first.

Areas not Subject to Simple Straight-forward Analyses

Helium Builders. The build-up of the helium (or foreign impurity) concentration in the plasma can be very detrimental in that it reduces the fuel atom density that



TYPICAL DIMENSIONS

R = 10-15 m
ℓ = 6-10 m
r = 30-50 cm
VOLUME = 10^9-10^{10} cm^3
POWER PRODUCTION = 0.1-1 Watt/cm^3
TOTAL POWER PRODUCTION = 10^9 Watts
PLASMA DENSITY = 3×10^{13}-10^{14} cm^{-3}
T_i = 250 keV
T_e = 130 keV
MAGNETIC FIELD = 25-50 kg

Figure 2-1. p-^{11}B floating multipole fusion reactor.

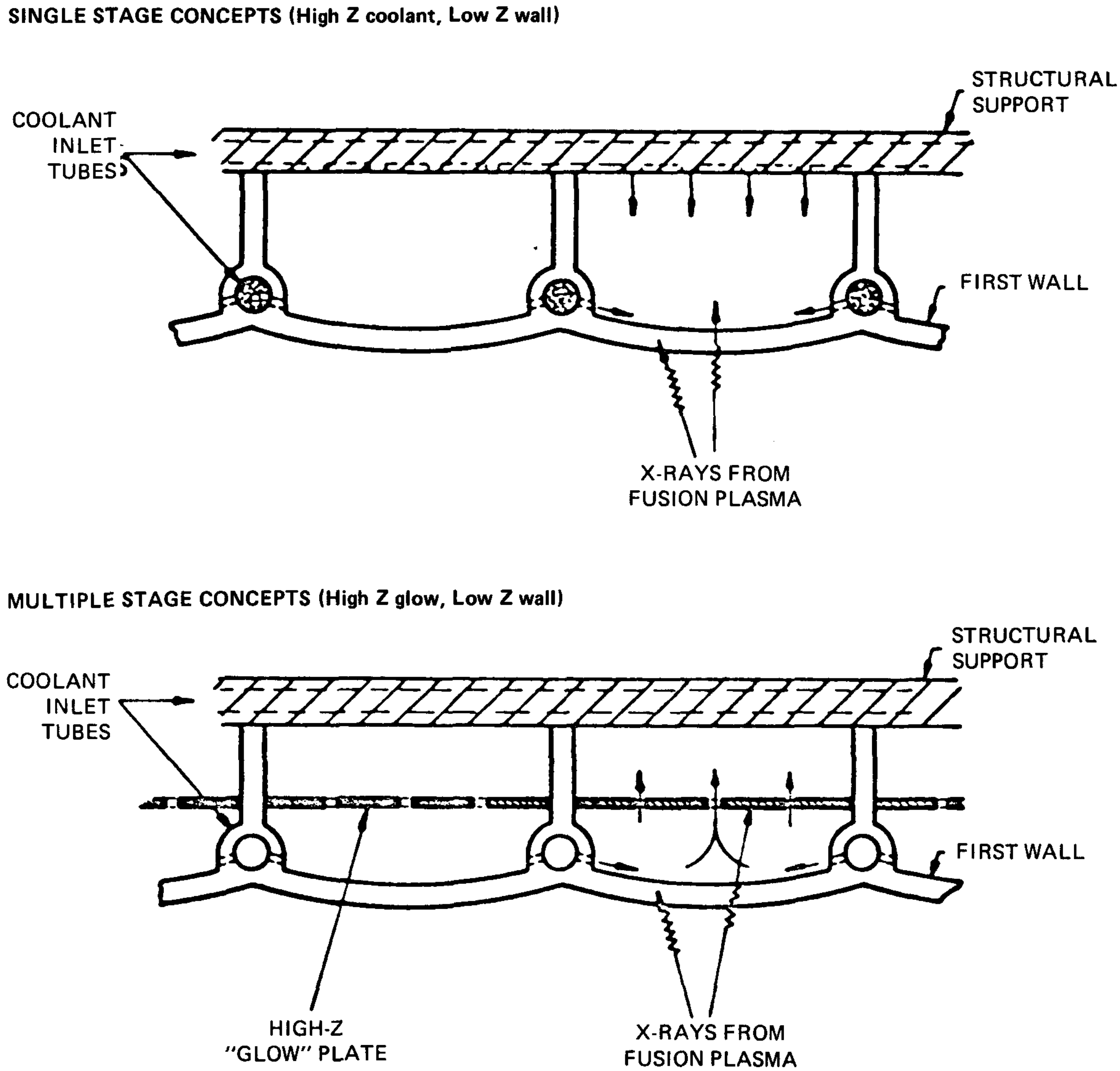


Figure 2-2. Radiation boiler/fusion reactor first wall concepts.

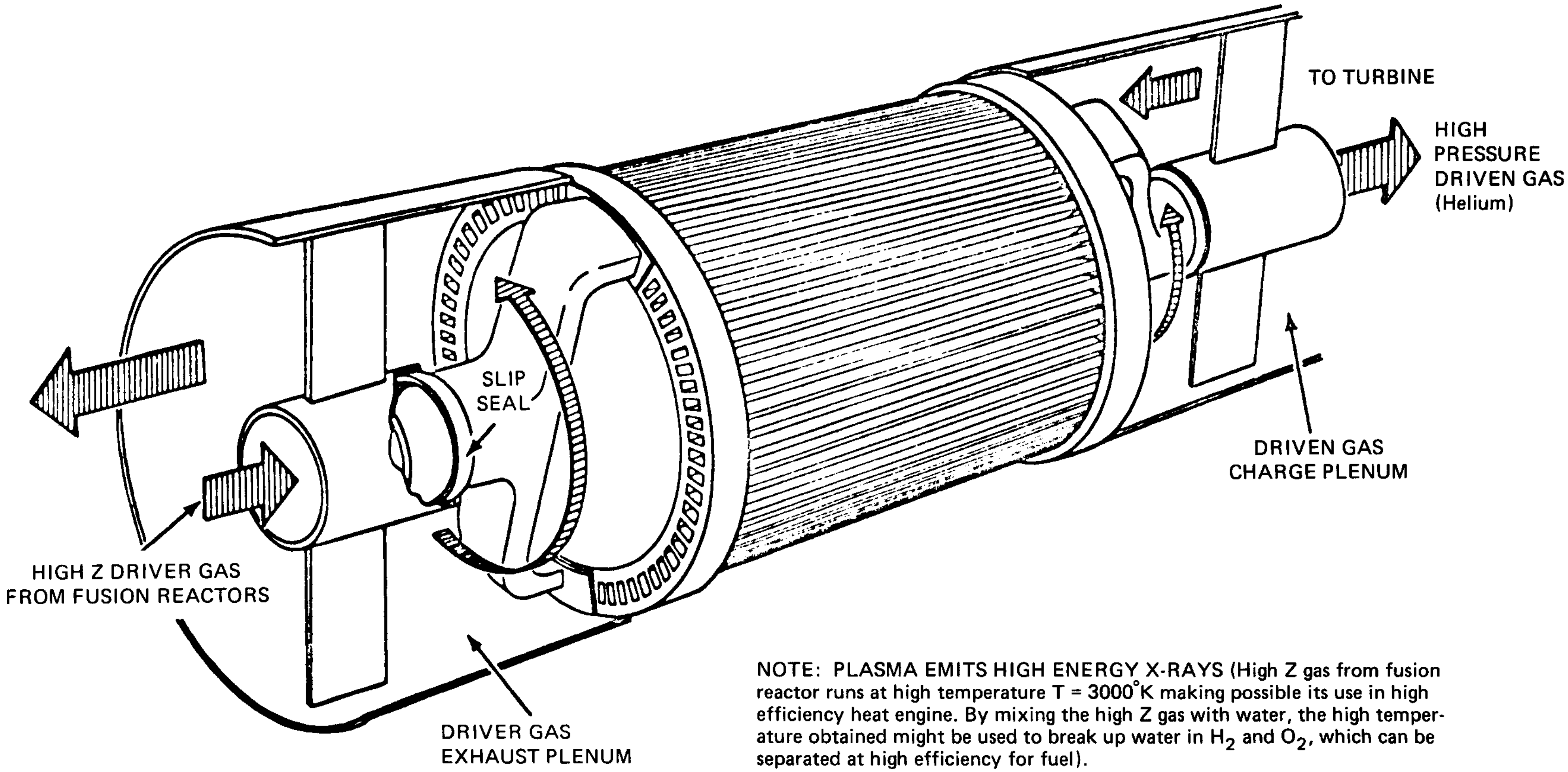


Figure 2-3. Energy exchanger with fixed tubes for high efficiency heat engine.

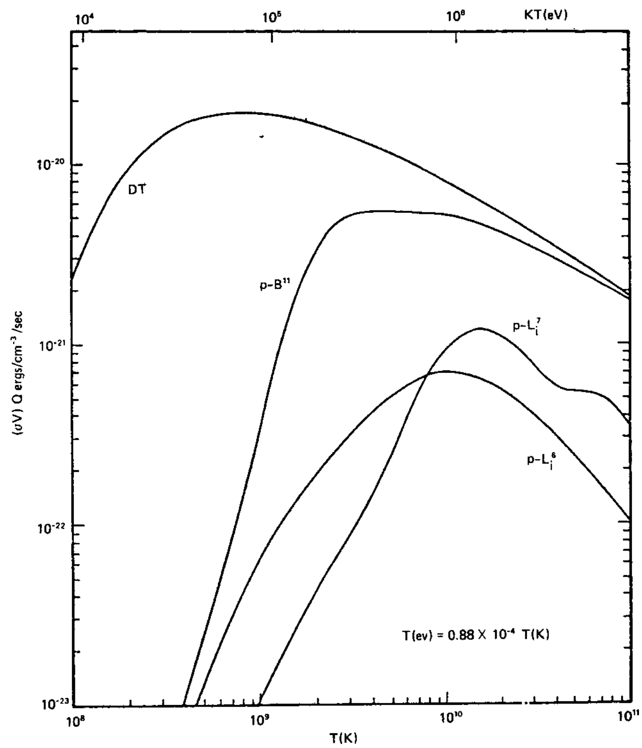


Figure 2-4. Comparison of fusion reaction rates.

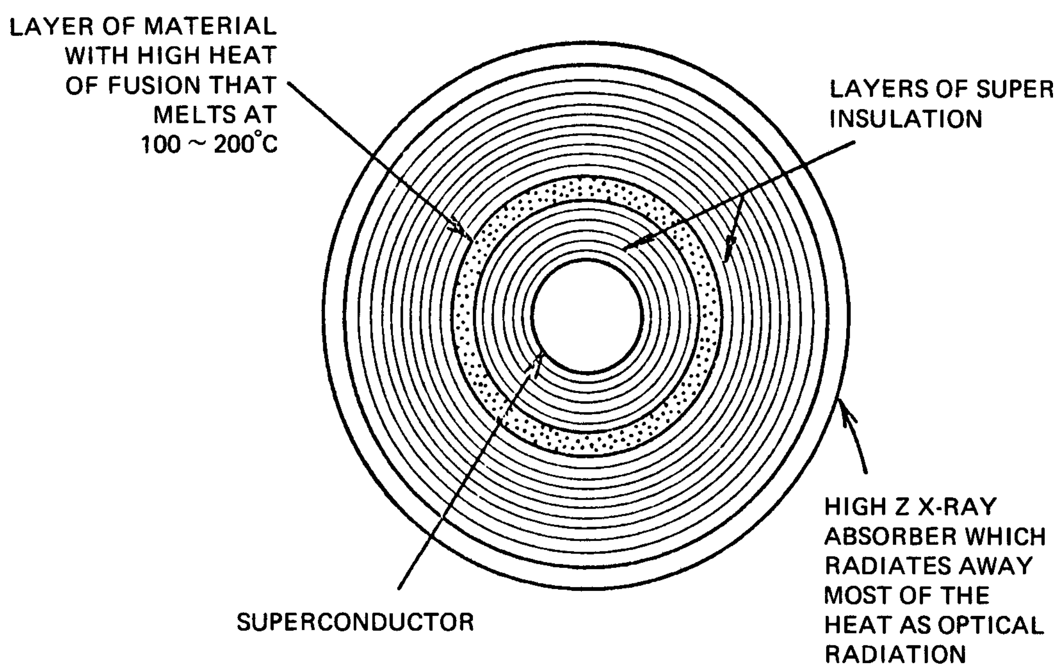


Figure 2-5. Concept of a floating ring.

can be maintained in the plasma at a given beta. Some method of impurity atom removal will have to be developed; otherwise, the burn time will be reduced to minutes instead of the hours required for economical power production. Methods such as bundle divertors have been suggested, but the problem of handling high heat loads (10-30 MW/m^2) and the possibility of plasma instabilities have not been fully defined, let alone solved.

Plasma Temperature. It will be necessary to keep the temperature of the plasma at 200-300 keV during the burn. It is suggested that this could be done by injecting very high energy (1-2 MeV) proton beams, or by the use of RF heating. However, the exact mechanism of particle entry or the auxiliary power required from the line have not been specified (nor has it been, for that matter, for any other fusion system).

Size. It is not yet clear how big this reactor must be to achieve a net power producing capability. Such a determination can only come after the high temperature confinement scaling for multipole devices has been established experimentally. It is also unclear whether a beta approaching unity can be achieved, and hence what maximum fuel density can be maintained.

Internal Conductors. The exact number of internal conductors required for plasma stability and their method of deployment (stabilized levitation or supports) has to be established. If the number of required hoops increases, the total sputtering increases, as does the amount of very expensive material (W, NbTi, etc.). Such a determination of magnet deployment should evolve from physics experiments in the next five to ten years.

Areas That Could Be Addressed in the Near Term by Experimental or Analytical Means

Advanced Fusion Fuels. Cross section verification of p-^{11}B and other possible advanced fusion fuels is necessary. There is some uncertainty about the exact values of the p-^{11}B cross section in the 50-2000 keV range. Calculations now show that if the cross section is a factor of two smaller, the energy recovery and recirculation must be much more efficient and the prospects correspondingly dim. If the cross section is a factor of two higher, the energy balance problem is much alleviated. It is also necessary to determine accurately the branching ratios for the α-^{11}B neutron producing reaction. Current estimates place the neutron production at 10^{-3} per reaction, and possibly much less. It is critical to check this number and to investigate other potential neutron-producing side reactions, because one of the greatest advantages of this system is its "neutronless" nature.

Heat Leakage. Associated with the internal rings are a number of crucial problems, some of which could be addressed here on the basis of the present conceptual designs. The exact temperature of the outer surface must be carefully established, because at the estimated value of 2000-3000^{o}K, vapor pressure, creep strength of outer shell, and heat leakage to the internal liquid helium superconductor are very critical to the duration of the burn. Methods of mechanically coupling the superconductor to the outer wall will also provide heat leaks that will eventually cause the superconductor to go normal. The specific electrical and thermal insulation materials need to be identified before a realistic assessment of coil lifetime can be made.

Particle Sputtering. The sputtering of particles from internal rings and their subsequent disposition must be calculated. It is currently estimated that the plasma flux to the outer ring surfaces must be 10^{10} to 10^{11} $cm^{-2}s^{-1}$ if a reasonably clean plasma is to be maintained. It is not clear how stringent these conditions are until more calculations are done.

Cool-down Time. Detailed calculations of the cool-down time of internal hoops is required. The duration of the burn time in a multipole system is determined by the time required for the heat leaking into the coil to cause the superconductor to go normal. Present calculations indicate that this time could be as long as one day, but more detailed analysis is required with neutron heating and heat leaks along supports included. Once the critical temperature is reached, the reactor will have to be shut down to cool the magnet well below the critical temperature. This could be done by circulating coolant through leads into the coils or by removing the coil from the reactor and replacing them with fresh coils. Both of these methods have serious implications on the down-time, hence the availability of the reactor. Further work is needed to identify a credible cooling scheme and to assess its impact on the duty cycle of the reactor.

Synchrotron Radiation. Accurate calculations of synchrotron radiation and reflectivities from internal hoops and walls must be carried out. The plasma temperature is high enough that synchrotron losses could be significant if the walls do not have good reflectivity. Various surfaces can be assessed as to their compatibility with the temperature and radiation environment. Of course, the determination of synchrotron radiation from this complex plasma geometry will be difficult, but it is amenable to advanced computational methods.

High Energy Conversion. Techniques for high efficiency energy conversion (from x-rays) must be analyzed. One of the attractive aspects of this concept is its potential for producing electricity at high (50-65%) efficiencies. However, such a process has to be established experimentally, and conceptual designs should be assessed with regard to chemical and mechanical compatibility.

Section 3

FUSION-FISSION HYBRID BREEDERS

An EPRI study performed by Westinghouse was based on the design goals of the next generation of fusion experiments. The study has shown that even if the pessimistic assumption is made that the TCT or T-20 plasma physics design conditions are essentially unalterable and not free to be optimized for fusion-fission, an attractive breeder can be designed to produce 2.5 metric tons of Pu^{239}/yr, sufficient to fuel 4.6-5.0 LWR's of an equivalent thermal power rating. Plutonium recycling is assumed. These results are consistent with other hybrid studies done for linear solenoids, mirror systems, inertial systems, and other tokamaks.

Although engineering studies now exist that appear to combine the technologies of fusion and fission successfully into a hybrid reactor, there is still considerable concern that the combination would raise new and unexpected problems; for instance, in fuel management, compatibility of gas cooling with neutron efficiency, blanket design, potential accident scenarios, and plant factor. Fusion-fission systems can be designed to produce large net power or only a small amount of fission power by suppressing fission reactions through the use of the $Th^{232} \rightarrow U^{233}$ cycle. There is disagreement over the reputed superiority of the fission-suppressed hybrid, but clearly an optimization has to be made. Studies of laser heated and E-beam heated solenoids show that, if these new systems work, small hybrids that do not require a great extrapolation of present technology can be designed.

UTILITY IMPACT*

Efficient hybrid fuel breeders would enable utilities to continue a well-developed LWR economy. Hybrids offer the possibility of an early impact of fusion on utility planning. The hybrid/LWR system could be an attractive economic alternative to the LMFBR/LWR system. If hybrids can be made to work, they could make higher quality fissile fuel than an LMFBR, they appear to come in a convenient size range, and they could utilize familiar fission technology. Because of the much reduced plutonium inventory, hybrids might be safer than LMFBRs.

*No single utility viewpoint exists; this represents the view of only a few informed representatives of the industry.

Since one modestly sized hybrid breeder can supply fuel for 4-5 LWR's, or a larger number of HTGR's (for equal plant factors), fewer breeding sites are required. Hybrids may allow the fusion program to gain power plant experience at the earliest time. In the eyes of the utilities, a hybrid breeder is a fusion product that has a clearly identifiable economic value. Hybrid breeders can provide LWR fuel virtually inexhaustibly with a minimum number of production plants. In fact, such fuel breeders need not be operated by the utilities.

IMPACT ON FUSION PROGRAMS

Hybrids offer a possibility for showing a visible product from the fusion program, and this may help assure continued funding. The great efficiency of fuel production by hybrids (4-5 LWR's fueled by one hybrid), if realized, would be a clear example of the practical benefit of fusion to long-term energy production.

A major commitment to hybrids might divert the pure fusion effort and/or bring the full weight of fission-related difficulties down on fusion. Hybrid development could add to the fusion program a subprogram fully comparable in difficulty to the LMFBR development program. A contrasting viewpoint is that while it is possible for the diversion of effort from the pure fusion program to imperil it, the early introduction of hybrids would help develop the technology base for any fusion device. Therefore, these programs could proceed along independent lines and still advance to join the pure fusion effort without loss of momentum. In addition, hybrids might represent an important spin-off of the fusion program.

DIRECTIONS FOR EPRI RESEARCH

Device-dependent studies have already been done for many concepts: Tokamaks, mirrors, theta-pinch, linear solenoids, etc. These typically include a hybrid study as an appendix. We now need an impartial study which emphasizes the breeder aspect specifically and seeks to determine the most favorable configuration taking into account the entire breeder/LWR system.

From the viewpoint of hybrid development, the collaboration with the Soviet Union through the T-20 program should be pursued on a priority basis. Once the "best" breeder concept has been identified and experience with fusion-fission modules has been gained in TFTR or T-20, steps should be taken toward achieving a demonstration device.

Section 4

SMALL FUSION POWER REACTORS

There is a two-fold purpose in exploring possibilities for practical small fusion reactors:

- To enable fusion to be brought into the commercial energy supply picture at the most rapid pace.
- To satisfy users' need for energy supply concepts which solve or ease some of the problems which hold up or limit the use of available energy supply projects, particularly nuclear fusion.

ENABLING EARLY COMMERCIALIZATION OF FUSION

Fusion commercialization presently is expected to require very large initial projects. This course would require covering substantial uneconomic project costs for several projects extending over a period of perhaps 15 to 20 years. Voluntary investments from capital markets might be expected to cover only a very small portion of the uneconomic costs. The use of substantial taxpayer funds may not be possible for such apparent commercial ventures. Thus, if fusion goes as presently perceived, there is a good chance that its commercialization will prove difficult. Even with adequate funding, there is little hope of commercializing fusion expeditiously if large projects are required.

To bring commercial energy suppliers into fusion in a significant way during the introductory period would require a series of quick, low cost steps. History of other major commercial developments, such as fission, appears to bear this out. Such steps could be possible if introductory fusion power projects were small and required minimal dollar commitment. For examples:

- Experience with fission shows that to carry out commercialization in a reasonable time at reasonable cost requires a series of relatively quick, low cost project steps.
- There also appear to be advantages in a final commercial product which is small enough to gain the benefits of volume production, quick turnaround, and statistically significant experience.

SATISFYING USER'S NEEDS

What users need from fusion are concepts which solve or ease some of their energy supply problems. Among the utility problems which small fusion systems might help address are the following:

- Excessive demands on utility credit. Utility regulations have lately shown an unwillingness to have ratepayers pay any investment costs which might logically be postponed. The principal device for shifting the investment burden from the ratepayers to the utility is by excluding construction work in progress from the rate base. In an inflating economy a utility with commitments to large projects may have half its power plant investment entirely self financed and producing no income. This is not a financially sound way for a utility to operate because there is no guarantee that the full costs will ever be allowed in the rates by tomorrow's rate regulation. The utility must borrow money to pay the interest on money already borrowed and the compounding effect is sizeable. This unhealthy situation runs high risk that future energy supply needs will not be met due to financial inadequacy. The need to reduce the credit demands on a utility by reducing the time projects that are under construction is urgent.
- Project justification. It has become nearly impossible to prove that an energy project that takes 10 to 12 years to carry out is really needed. By the time a clear need appears the long term concept is too late and something that offers a temporary fix must be used instead. What is needed is a concept that can be put together quickly after a commitment is made from largely factory manufactured components. It takes small projects to get expeditious treatment in acquiring necessary project approvals, permits, and financing.
- Siting and cooling. Site-related issues such as cooling, seismology, and transmission are a major hurdle for large projects and often are the cause of project termination or unanticipated expense. Small projects can greatly reduce the difficulty and risks and increase the site and cooling options.
- Inadequate plant availability. Large projects have an operating availability problem because each such project tends to be "first of a kind" with corresponding shakedown and ongoing forced outages. Small projects might have a 50% higher availability factor which translates into an inverse economy of scale factor of 1.5.
- Lack of simplicity. Simplicity ceases to be an objective when projects are so large that complexity can be accommodated. Large nuclear plants are already reaching the point of excess complexity. Emphasis on achieving acceptable cost in small size plants mandates achievement of simplicity in design and operation.
- Inadequate experience feedback. Large concepts may require a decade to experience the result of a design decision. People forget and move on and requirements change in that length of time so that much of the original logic is lost by the time a single result is obtained. If changes are made for subsequent units, another decade may pass before a second result is obtained. Such time periods and the lack of a statistically significant number of units of any particular design make experience feedback into new units practically nonexistent.

Thus, large concepts give little opportunity for improved performance through repetitive doing.

- Technological stagnation. Large concepts, once established are nearly impossible to change. They trend toward highly perfected obsolescence. Small size would make the introduction of beneficial technological changes practical.

RECOMMENDATIONS FOR EPRI

The present fusion program has no emphasis on small reactors, and we suggest that at least a portion of the program be devoted to this goal, difficult though it may be. The ideal would be to work toward something in the 150 MWe or so size with precursors as follows:

- A greater than 5 MWe net power producing experiment costing less than $100M and designed to operate greater than 5 years.
- A greater than 50 MWe net pilot plant costing less than $400M and designed to operate more than 15 years.

These precursor parameters are chosen on the assumption that projects which meet these conditions could be committed and financed.

EPRI should convene a suitable panel to study in detail and evaluate possible methods of achieving the goals cited above for small fusion power reactors. Both mainline and alternative fusion concepts should be considered. Specific plans for carrying out precursor steps associated with the most promising concepts would be the principal study results.

To open up a route to a viable option for small fusion reactors will require the combination of innovative ideas. Therefore, such a study should consider approaches which take advantage of one or more of the general concepts which might lead to smaller systems. Examples of such concepts or system elements include: high beta configurations; end-plugged linear systems; neutron-poor reactions; low B field configuration; high gain, small pellet inertial systems; and technology advances such as:

- neutral beam control
- low activation materials
- modularization
- cheaper high B fields
- high efficiency energy conversion.

Proponents of configurations embodying such concepts could be invited to propose greater than 5 MWe and greater than 50 MWe implementations.

Section 5

MEMBERS OF THE ASILOMAR AD HOC EPRI ADVISORY PANEL

A. EPRI FUSION ADVISORY COMMITTEE

F. Chen, Chairman
B. Fried
D. Rose
G. Kulcinski

B. EPRI FUSION PROGRAM COMMITTEE

C. Ashworth
W. Wolkenhauer

C. EPRI SPECIAL CONSULTANTS

J. Dawson
A. Hertzberg
J. Powell

D. EPRI STAFF

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N. Amherd
F. Scott