Controlled Nuclear Fusion: Current Research and Potential Progress : the Report of the Fusion Assessment Resource Group, Supply and Delivery Panel of the Committee on Nuclear and Alternative Energy Systems, National Research Council. (1978)

Pages
65

Size
8.5 x 10

ISBN
0309028639

Fusion Assessment Resource Group; Supply and Delivery Panel; Committee on Nuclear and Alternative Energy Systems; National Research Council

Study of Nuclear and Alternative Energy Systems

SUPPORTING PAPER 3

CONTROLLED NUCLEAR FUSION: CURRENT RESEARCH AND POTENTIAL PROGRESS

The Report of the
Fusion Assessment Resource Group
Supply and Delivery Panel
of the
Committee on Nuclear and Alternative Energy Systems
National Research Council

NATIONAL ACADEMY OF SCIENCES
WASHINGTON, D.C. 1978

International Standard Book Number 0-309-02863-9

Library of Congress Catalog Card Number 78-71304

Available from:

Office of Publications
National Academy of Sciences
2101 Constitution Avenue, N.W.
Washington, D.C. 20418

Printed in the United States of America

PREFACE

In June 1975, the National Research Council (NRC) undertook a comprehensive study of the nation's prospective energy economy during the period 1985-2010, with special attention to the role of nuclear power among the alternative energy systems. The goal of the study is to assist the American people and government in formulating energy policy.

The Governing Board of the National Research Council appointed an NRC-wide Committee on Nuclear and Alternative Energy Systems (CONAES) to conduct the study. CONAES consists of 15 members drawn from diverse disciplines and backgrounds. The committee developed a three-tiered functional structure for the study. The first tier is CONAES itself. The ultimate findings, judgments, and conclusions of the study will be embodied in its final report.

To provide scientific and engineering data and analyses, a second tier of four panels was formed to examine (1) energy demand and conservation, (2) energy supply and delivery systems, (3) risks and impacts of energy supply and use, and (4) syntheses of diverse models of future energy economies, respectively. Each panel, in turn, established a number of resource groups--22 in all--as the third tier, to address in detail an array of more particular matters, such as buildings and transportation systems, solar energy, breeder reactors, coal technologies, health and environmental implications, and alternative consumption patterns and economic models. In all, more than 200 informed individuals served on or contributed to the work of the panels and resource groups.

The National Research Council customarily publishes only the final reports of its committees--and then only after the report has been reviewed by a group other than its authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. However, because such a large volume of information and analyses was assembled for consideration by the committee, and because of the diversity and scope of that information and the accompanying judgments, the panel reports and approximately 10 reports by the resource groups are being published as supporting papers. Each of these has been considered and used by CONAES but has not undergone the critical review procedure normal to the NRC. The report of the Fusion Resource Group has, however, been subjected to a thorough and expert peer review for accuracy, consistency, and clarity.

It must be recognized that some conclusions of the panel and resource group reports may be at variance with the conclusions of the CONAES report. The findings reported in these documents are those of their authors and are not necessarily endorsed by CONAES or the National Research Council.

This report covers the work of the Supply and Delivery Panel's Fusion Assessment Resource Group. The report was designed to inform the Panel and CONAES of the current state of fusion technology and to provide an estimate of its future progress. It is published, with the other supporting papers, to enhance the general understanding of the intricate and wide-ranging implications of energy in the coming decades and to acquaint the reader with the variety and complexity of the material with which CONAES has had to deal.

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CONTENTS

BACKGROUND INFORMATION

FUSION, A FORM OF NUCLEAR ENERGY

It is commonly held today that the world's ultimate energy supplies will be provided either by the sun, by geothermal energy stored in the earth's interior, by nuclear fuels found on earth, or by some combination of these sources. There are two main variants of nuclear energy to be considered: fission and fusion. This report is concerned with the latter.

Fusion denotes a class of rearrangement reactions involving the nuclei of the lighter elements in the periodic table, reactions accompanied by a net release of large amounts of energy. Fission, on the other hand, denotes exothermic reactions following the neutron bombardment of the nuclei of heavier elements in the periodic table, principally certain isotopes of uranium and plutonium. A number of elements existing abundantly on earth can serve as nuclear fuels for either the fission or fusion process. Consequently, these fuels represent essentially inexhaustible sources of energy for the future.

Unlike fission, all fusion reactions require extremely high temperatures, tens to hundreds of million degrees Celsius. At these temperatures matter is gaseous and decomposes into atoms; the atoms, in turn, are stripped of their outer electrons and thus become ionized. We refer to this state as a plasma, that is, an ionized gas distinguished from ordinary gases by its ability to conduct electricity easily and to respond readily to electric or magnetic forces. For practical purposes, it will be necessary for a fusion reactor to achieve conditions where the appropriate fuel is raised to these elevated temperatures and held there long enough so that a significant fraction of the fuel can undergo fusion reactions. The amount of energy recovered in the process will have to exceed the amount of energy invested, and exceed it by some measure, in order for the fusion reactor to be of practical interest.

FUSION FUEL CYCLES

A variety of existing fusion processes, or fuel cycles, might be considered for terrestrial purposes.* The fusion energy will be released in a combination of three forms: radiation, kinetic energy of charged particles, and fast neutrons. The distribution of energy among these three forms depends on the fuel cycle selected and hence will affect the engineering aspects of a fusion plant as well as its potential applications. The reaction involving the deuterium (D) and tritium (T) isotopes of hydrogen requires by far the least stringent plasma conditions and is therefore receiving the most attention today. Deuterium is found in nature along with ordinary hydrogen in the proportion of 1 to 6,500, and is readily recoverable from the waters of the earth. Tritium is radioactive, decaying with the emission of a soft beta particle, and with a half-life of slightly more than 12 years. It does not occur naturally and therefore must be bred, that is, created artificially. The fusion reaction of D-T produces a nucleus of ordinary helium plus a neutron and 17.6 million electron-volts (MeV) of energy per event. Most of the energy (about 80 percent) is carried off by the 14.1 MeV neutron produced in the fusion events. In a suitably designed blanket, neutrons can be made to react with lithium (Li) to produce tritium. Consequently, the D-T fusion reaction involves a breeding fuel cycle of D-T-Li, with deuterium and lithium as the fuels ultimately consumed. The availability of lithium thus becomes an essential factor in considering the long-term viability of fusion, when operating on the D-T-Li fuel cycle.

Ideally, the D-T-Li cycle, relying entirely on the (n,α) reaction of $^6Li_3$ to produce tritium, yields 22.4 MeV of energy per one complete fusion-breeding event and translates into 8.32 megawatt days, thermal (MWD(th)) of energy per gram of tritium burned. The 14.1 MeV fusion neutrons are able to initiate $(n,\alpha n')$ reactions in $^7Li_3$ and produce tritium, as well. With natural lithium as the feed and a 40 percent conversion efficiency of thermal energy to electricity, one finds that the lithium makeup required by breeding amounts to approximately 1.8 kilos per megawatt electric year (MWe-year).

Current estimates indicate worldwide lithium reserves and resources on the order of 10 million tonnes. This is surely a conservative estimate and does not take into account the vast amount of lithium recoverable from sea water. The latter, because of the inherently low fuel-cycle costs of fusion power, could ultimately contribute to the reserve figures and increase them by many orders of magnitude. Nevertheless, the 10 million tonnes figure corresponds to about 5 billion MWe years of electrical energy, which could support a global population of 10 billion people for 500 years at a per capita electrical power demand of 1 kilowatt.

Early design studies on fusion reactors revealed that the reactor's lithium inventory would be difficult to recycle due to the buildup of excessive chemical and radioactive contaminants. This leads one to

*The well-known fusion reaction involving the common form of hydrogen, which is responsible for the energy production of most stars, including our sun, proceeds at too slow a rate to be of terrestrial interest.

consider the economic advantages of committing the entire inventory (in one design, 1.15 tonnes of natural lithium per MWe of installed capacity) over the lifetime of the plant. In the absence of recycling, lithium requirements would increase by at least an order of magnitude. Thus, one should expect that recycling and returning the plant inventory to next-generation reactors will become economically advantageous in the course of time.

Compared with other fusion fuel cycles, the D-T-Li fuel cycle has several disadvantages. It will require breeding and handling of radioactive tritium, as well as disposal of radioactive material produced within the plant by neutron activation. Due to the high background radiation in the plant, some functions will need remote operation and maintenance, making it somewhat more difficult to maintain high plant availability. The advantage of the D-T-Li fuel cycle is that it requires the least stringent plasma conditions for an operating reactor by a wide margin and leads to power densities one to two orders of magnitude greater than might be achieved with other fuel cycles. Advanced fuel cycles, by relying on deuterium, $^3He_2$, or such higher atomic weight elements as lithium, beryllium, or boron, tend to alleviate many of the above problems to varying degrees; these cycles may prove to be of practical interest at some future date, but are unlikely to compete with D-T-Li in early generation reactors.

Thus, the currently most promising fusion reaction, D-T, uses radioactive material in the form of tritium and produces neutrons that, in turn, will induce radioactivity in the structural and operating material contained within the reactor. Consequently, fusion reactors have their own unique radiation hazards, and these need to be evaluated carefully. There is reason to believe that the radiation safety and waste disposal problems peculiar to fusion reactors may be solved through the appropriate choice of materials, engineering, and design at lesser expense than for fission reactors. In addition, it must be noted that the neutrons created through fusion could be used, with some effort and redesign, to produce material for the manufacture of nuclear weapons. This raises a safeguard issue, although possibly of a less complicated nature than for fission breeders.

FUSION APPLICATIONS

So far, we have made no distinction between the use of a commercial fusion energy system to produce electricity and other possible applications. In fact, attention in this country has been devoted largely to developing a fusion reactor suitable for use as an electric power source by the electric utility industry. Since the principal direct output of a D-T reactor is energetic neutrons, however, other applications have been proposed. The main one is a scheme by which the neutrons, in combination with a blanket of fertile nuclear material, are used to produce fissile material for fission converters. Interest in this approach derives from the fact that each 14.1 MeV neutron could, in principle, lead to the production of up to five fissile atoms while at the same time satisfying tritium breeding needs. Each of the fissile atoms could then ultimately produce about 300 MeV consistent with light water reactor (LWR) performance and plutonium (Pu) recycling; advanced thermal converters could achieve higher yields. (See report of Reactor Resource Group.) The opportunities for energy multiplication, theoretically, are large and could lead to a significant reduction in plasma requirements within the scope of an economical commercial system. Elaboration of this concept will be given later.

Other uses for the neutrons and radiation produced in D-T reactions have been proposed. For example, by radiolysis of water to produce hydrogen, or of carbon dioxide to produce carbon monoxide, both products provide combustion fuels. Similarly, disposal of radioactive fission products could be achieved by transmutation to shorter lived or stable nuclei, etc. Although these applications could be of practical interest, such concepts have been explored only to a limited extent, and not sufficiently to determine whether they should become a significant objective in developing fusion for civilian applications.

Fuel cycles other than D-T exist that offer the possibility of increasing the proportion of fusion energy in the form of electromagnetic radiation and kinetic energy of charged particles. This could, in principle, lead to higher efficiency energy conversion options and unique applications for chemical production and materials processing. Although of potential use in fusion, such concepts must be considered highly speculative at this time.

METHODS OF APPROACH

Two main lines of approach towards developing a practical fusion reactor for civilian applications have evolved in the course of the past quarter of a century: the magnetic confinement approach and the inertial confinement approach

MAGNETIC CONFINEMENT

In a macroscopic sense, plasmas behave as if they were diamagnetic. Consequently, a properly designed magnetic field configuration will produce stresses to counterbalance the pressure of the plasma. In this manner the magnetic field can act as a thermal insulator and thus confine the plasma materially to a specific volume in space. Such configurations are termed magnetic bottles or containers. In principle, radiation pressure could achieve the same result, but in practice one finds magnetic fields that are either static or slowly varying in time to be the most effective. One measure of the economy by which magnetic fields confine a plasma is the parameter beta (β), defined as the ratio of plasma pressure to the magnetic pressure that would exist locally if the plasma were absent. By this simple measure, a beta of unity is the maximum achievable value. Confinement schemes tend to divide into low β (<0.1) or high β (>0.5) categories. Since for a given magnetic field strength and plasma temperature the power density in a reactor scales as β^2, the value of β achievable is an important design factor in magnetic confinement.

The usual figure of merit in the magnetic confinement approach, however, is the product of plasma density (n) and energy confinement time (τ). When n is measured in particles per cubic centimeter and τ is measured in seconds, it can be shown that at fusion reactor temperatures (on the order of 100 million degrees Celsius) most magnetic confinement schemes require $n\tau$ values in the range of 10 trillion to 100 trillion to reach the "energy break-even" conditions, where the energy released through fusion reactions just equals the energy invested in the plasma. An operating fusion reactor would have to achieve $n\tau$ values somewhat higher than the break-even condition (or Lawson criterion) and in addition, it would have to maintain temperatures high enough to burn a significant fraction of its fusion fuel. Certain early studies indicate that for economic net power production one is interested in operating the reactor in an "ignited" mode. Under these conditions the 3.5 MeV alpha particles (helium nuclei) produced in the fusion reaction are confined in the plasma long enough to heat the intially cold fuel. The nuclear process which then takes place is loosely analogous to chemical combustion. The ignition temperature for the D-T reaction is nominally 60 million degrees Celsius. It is also possible to "drive" a fusion reactor by injecting high-energy particles from an external source into the plasma. Such schemes may in fact be advantageous when one is concerned with optimum means for neutron production, rather than net power production.

A magnetic field confines charged particles to a small orbit in the plane normal to the field. In order to prevent them from leaking out along the field lines, one must either (a) use a toroidal geometry in which the field lines form closed-flux surfaces, or (b) rely on magnetic mirrors or other end-stoppering effects to reflect the particles. Thus, we can subdivide magnetic confinement schemes into closed and upon configurations, of which the Tokamak and mirror, discussed below, are representative.

The principal scientific limitation on fusion prospects is the multitude of plasma instabilities which may occur. These produce collective currents and charges and fluctuating fields which may destroy the confinement properties of the system. A vast amount of work has been done and considerable understanding gained on these matters over the years. The most dangerous and rapid instabilities, characteristic of early experiments, are now routinely avoided while some, leading to microturbulence, still remain and degrade confinement by a considerable factor. Nonetheless, it is now clear that a plasma which is magnetically confined in a closed system of sufficient size would exceed break-even, although we cannot state precisely at this time what the minimum size must be. Open systems have a smaller safety margin for confinement, but offer the possible advantages of higher β and simpler engineering.

Many different magnetic confinement schemes have been explored in the past. The one showing the greatest scientific promise today is a concept pioneered by workers in the Soviet Union, the Tokamak. Tokamak is a toroidal device in which a combination of externally applied toroidal magnetic fields and poloidal magnetic fields induced by toroidal currents flowing in the plasma create the desired magnetic configurations. Another promising confinement concept is the magnetic mirror, in its simplest configuration an open-ended device in which the confining magnetic fields are generated externally by suitably shaped coils.

Both the Tokamak and mirror are essentially long-confinement-time concepts, where in an operating reactor one would expect to achieve energy confinement times on the order of several seconds. The Tokamak would probably operate in a cyclic mode with a high duty cycle, while the mirror could operate in a stead state. As now conceived, the Tokamak will operate at lower β values than mirrors, which have operated in the high β regime.

One of the most important features of fusion physics is the enormous range of possible confinement variants. Several of the alternative magnetic confinement schemes (other than the Tokamak or mirror) have not been studied extensively enough to determine their prospective merits. Some, on the other hand, have encountered unresolved difficulties. With more ingenuity, experience, and understanding, it is conceivable that other fusion systems may evolve with better stability, confinement, and power density properties and lower plant capital costs than those under current investigation. Thus, it is very difficult to assess the ultimate character of magnetic confinement systems at this time.

INERTIAL CONFINEMENT

The range of plasma densities encountered in magnetic confinement schemes may vary from 100 billion cubic centimeters to 10 quadrillion per cubic centimeter; consequently, the critical confinement times for break-even may vary from several seconds to perhaps one-thousandth of a second. Inertial confinement, however, conceives of working at the extreme high density and very short time-scale end of the $n\tau$ range. Fuel, arranged in the form of a pellet, is rapidly compressed to densities of three to four orders of magnitude above liquid density values ($n \sim$ 10 septillion to 100 septillion per cubic centimeter) in this approach. During the compression stage, the fuel is adiabatically heated to ignition temperatures at the center of the fuel assembly. Thereafter, a fusion burn front propagates outward. The fuel continues to burn until the highly compressed plasma disassemblies, on time scales (R/v) comparable to the radius of the pellet at ignition (R) divided by the speed of sound (v) in the material. From the earlier $n\tau$ criterion, one may show that an equivalent figure of merit for inertial confinement is ρR, where ρ is the mass density and R is the pellet radius, both measured at maximum compression. For D-T the ρR value corresponding to break-even is on the order of 1 gram per square centimeter.

In order to achieve rapid compression, heating, and subsequent thermonuclear burn, it is necessary to find some means for delivering a short burst of energy to the pellet. This will cause the surface layer of the pellet to ablate, producing compression by the forces of reaction. The first energy source tried in an attempt to initiate these microexplosions was the laser; consequently, the inertial confinement approach is often referred to as laser fusion. More recently, energetic electron beams have also been used to drive the implosion; and ion beams, produced by technology borrowed from the electron beam work or by more conventional accelerators, have also been proposed as drivers. The driver energy required to reach break-even conditions depends in detail on the mechanism of energy deposition within the pellet and the efficiency with which this couples to the hydrodynamic phase of compression. Estimates ranging from hundreds of kilojoules to several megajoules have been obtained from computer codes which try to model the various physical processes taking place. The energy will have to be delivered over short enough times to reach power levels on the order of a hundred terawatts (>100 trillion watts) and focused to flux levels of 10 quadrillion watts per square centimeter.

An operating fusion reactor based on the inertial confinement approach would probably consist of a blast vessel, designed to withstand micro-explosive forces. It would be charged repeatedly with small fuel pellets which would then be irradiated and ignited by short bursts of energy from the driver. To this extent, the reactor cycle might be thought of as resembling the cycle in an internal combustion engine. The reactor, as in the magnetic confinement approach, would also include a blanket for breeding tritium and a coolant for converting fusion-product energy to thermal energy.

PROSPECTS FOR MAGNETIC CONFINEMENT

Some 25 years have elapsed since the U.S., Great Britain, and the Soviet Union each embarked on a serious program to harness fusion for civilian purposes. Many other nations joined in the effort after most elements of the program became declassified in 1958. Much progress has been made scientifically and a great deal learned about what has emerged as a new branch of physics, namely plasma physics. Nevertheless, as we shall indicate, it is still premature to judge the practical merits of fusion.

Three stages of accomplishment are necessary before a final determination can be reached:

1. Scientific Feasibility - wherein reactor-grade plasmas are attained; scaling laws are well understood; and break-even criteria, corresponding to $n\tau$ = 10 trillion to 100 trillion at plasma temperatures of 5 thousand electron volts (KeV), or 60 million degrees Celsius, and above, are demonstrated. We note that $n\tau$ values at the lower end of this range (i.e. $n\tau$ = 20 trillion) have been achieved in ALCATOR at 1 KeV ion temperature.
2. Engineering Feasibility - wherein it is demonstrated that a suitably designed power-producing reactor can be constructed and successfully operated, with due regard to safety and environmental impact.
3. Commercial Feasibility - wherein it is demonstrated that reactors of proper design will have all the features necessary to make them potentially competitive, in economic terms, with alternative commercial energy sources.

SCIENTIFIC FEASIBILITY

In spite of the very considerable progress made on what has proved to be a most difficult problem, fusion research has yet to complete the first phase, that of demonstrating scientific feasibility. There are strong indications, however, that this stage will be arrived at within the next several years, and that most likely it will be the Tokamak series of experiments in which initial scientific feasibility will be shown. The understanding of the behavior of plasmas in Tokamaks is well advanced, but experiments have been restricted to density and temperature ranges that are short of simulating reactor conditions. The particular forms of microturbulence which limit plasma lifetimes and density are strong functions of temperature and configurational detail. Consequently, it is particularly important to extend the experimental studies of scaling laws to the temperature regimes more representative of ignition conditions, and it is expected that this will be accomplished in the 1977 to 1978 time frame when the energetic neutral beam injectors come on line in the Princeton Large Torus (PLT) at Princeton Plasma Physics Laboratory (PPPL) and ORMAK Upgrade at Oak Ridge National Laboratory (ORNL) experiments. In the event that impurity control proves to be a determining factor in setting confinement times and $n\tau$ values, the above experiments may be of more limited value. Then, we shall have to await results from the Poloidal Divertor Experiment (PDX) at PPPL and from the Impurities Studies Experiments (ISX) at ORNL in the 1978-1980 time frame.

For purposes of economic reactor design, implying reasonable power densities, it is important that means be found to increase the β value in Tokamaks. The noncircular cross-section experiments in Doublet III at General Atomic Company (GA) and PDX at PPPL, as well as the flux-conserving mode of ORMAK Upgrade at ORNL, should shed some light on this subject during the 1978-1980 time frame. Finally, the Tokamak Fusion Test Reactor (TFTR) at Princeton, expected to be operating by 1980-1981, should be able to demonstrate plasma behavior under nearly fully simulated reactor conditions, although perhaps short of a true ignition mode.* Thus, it is quite likely that the scientific basis for a fusion reactor design based on the Tokamak principle will be in hand within the next 5 to 6 years, though it may still be short of the optimum design. Within this time frame, it may be expected that further confirmatory results will be obtained from similar large Tokamak experiments being planned in Europe, the Soviet Union, and Japan.

It may be of value when trying to form some outside judgment on the progress and rate of progress in fusion reactor development to note that the PLT, ORMAK Upgrade, PDX, Doublet III, and TFTR devices mentioned above are physics experiments. They are, however, somewhat unusual when compared to many scientific experiments outside the field of plasma physics. Each, with the exception of TFTR, is in the cost range of $10-$30 million, takes about 3 years to design and construct, and has a useful experimental life of perhaps 3 to 4 years. The operating costs of these experiments are in the range of several million dollars per year. TFTR is unique because of its large size and because it is designed to handle tritium. Its cost is one order of magnitude higher than that of the earlier and smaller devices. A small fraction of the total plant cost may be ascribed to the fact that TFTR will be able to handle tritium, in addition to hydrogen. In other words, significant

*As originally conceived, Doublet III has the design potential for reaching the ignition criterion; however, present plans do not call for fully implementing this potential advances in aspects of scientific knowledge may occur at 3-5 year intervals at costs in the range of 10 million to 100 million per experiment. Figure 1 represents some of the past and future milestones on the road to achieving scientific feasibility.

Results obtained in the magnetic mirror program from the 2X experiments at Livermore during 1975-1976 have been most encouraging. There seems to be convincing evidence that the observed scaling follows theoretical predictions and that confinement time increases with the three-halves power of the temperature. Theory also predicts that better control of instabilities will result from increased plasma radius. There seems to be a sufficient basis for constructing a larger device, designated as MX by the Livermore group, in order to extend the scaling studies to regimes where a higher value of $n\tau$ may be obtained. In order to make the magnetic mirror concept appear attractive for reactor applications, however, it will probably be necessary to find some way of decreasing end losses appreciably. Unless this can be done, the ratio of circulating power to net output power of a mirror reactor would be high and unattractive. Several proposals to reduce the losses inherent to open-ended magnetic mirror configurations are receiving consideration. For example, the tandem mirror scheme for reversing the ambipolar potential (proposed at Lawrence Livermore Laboratory (LLL) and Novosibirsk), the Elmo Bumpy Torus (operating at ORNL), or the field reversal concept under investigation (at Cornell University and LLL), represent, in principle, solutions of the end-loss problem.

We note again that while scientific feasibility may be demonstrated within the next several years in one or more devices, any reactor based on these results may turn out to be far from optimum in terms of the performance attainable eventually in practical fusion reactors. However, demonstration of scientific feasibility, besides providing a test bed for the performance of relevant physics experiments, should provide a realistic basis for further optimization of designs leading to economically attractive reactors.

ENGINEERING FEASIBILITY

Problems relating to the engineering feasibility of fusion reactors operating on the D-T fuel cycle are beginning to be addressed seriously. In this portion we merely summarize certain technological issues which will be discussed more fully in the chapter of technological considerations. A number of generic problems exist and are being studied in D-T fusion systems, independent of specific reactor design. These include the behavior of structural material in the intense radiation environment characteristic of a D-T fusion-reactor plasma; superconducting coil design on a large scale, tritium handling, and blanket design. Some amount of information useful to the fusion program is also becoming available from the materials studies that are part of the fast fission breeder program. Both Tokamaks and mirrors will require energetic neutral beam sources or radio-frequency sources, or both, for heating. Neutral beams are the current favorites, and it will become necessary to develop sources capable of operating in the 10-100 MW range at injection energies

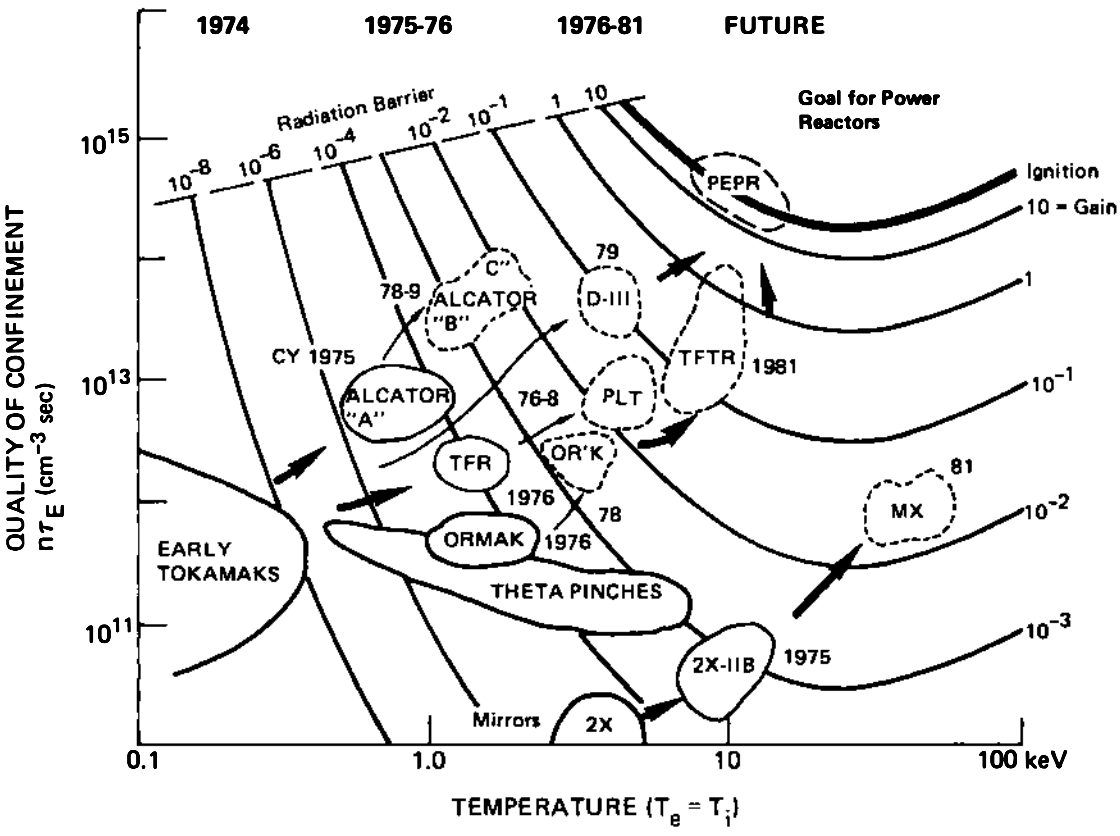

FIGURE 1 The path to scientific feasibility.

of 100-500 keV in pulsed and continuous wave (cw) modes. In addition, certain device-specific problems will have to be solved. For example, Tokamak reactors are conceived today to operate in a cyclic mode, with burn (reaction) times exceeding energy confinement times by large factors. The problem of fueling Tokamaks during the extended burn time remains to be solved.

If one were to embark on other than the D-T fuel cycle, recognizing the greater amount of physics uncertainty this would entail, many of the engineering problems listed above that result from tritium handling or neutron activation and damage could be eased or eliminated. At the same time, another set of engineering problems would be introduced, possibly of less severity.

PROSPECTS FOR INERTIAL CONFINEMENT

SCIENTIFIC FEASIBILITY

Research on inertial confinement started some dozen years after research began on magnetic confinement. Consequently, the state of knowledge in inertial confinement is far less advanced than in magnetic confinement. Offsetting this relative disadvantage is the rate of progress achieved in laser technology. In addition, much of the theory associated with inertial confinement relies on well-tested concepts in hydrodynamics, insofar as the concept is closely related to the behavior of dense matter being subjected to intense pressure pulses. Progress in understanding inertial confinement is being made rapidly now that suitably high-powered laser sources are becoming available and sophisticated diagnostics are being developed to obtain experimental data on the extreme short time scales (picoseconds to nanoseconds) characteristic of this approach. Although there is still a discrepancy in the degree of understanding extant between the two approaches, magnetic and inertial, it is conceivable that some measure of scientific feasibility will be demonstrated by inertial confinement in the same time frame as may be expected for magnetic confinement.

The inertial confinement program differs from the magnetic one in yet another respect, since it serves the dual goals of military and civilian applications. For this reason, portions of the program are classified. We shall have occasion to comment on this matter subsequently.

The driver source likely to be available first to workers in the inertial confinement program, with power capabilities approaching what would be needed to initiate an appreciable amount of thermonuclear burn in a target, is the neodymium (Nd) doped glass laser operating at a wavelength of 1.06 microns. These have now reached the terawatt (TW) level, and within the next few years facilities at Livermore and the University of Rochester should be able to reach the tens-of-terawatts range. Evidence of compression accompanying laser irradiation has already been obtained, but in targets where shock waves probably played the dominant role. These so-called exploding pusher pellets should be relatively free from instabilities, but limited in the density attainable, as confirmed by experimental observations. It will be necessary to show adiabatic compression at the higher power levels of the next generation of lasers in order to ensure that the inertial confinement approach can become feasible.

ENGINEERING FEASIBILITY

The very low efficiency of glass lasers tend to rule them out as candidates for commercial applications. At longer wavelengths, 10.6 microns, gaseous CO_2 lasers are available and their development has shown good promise. Their efficiencies may be adequate for commercial applications; however, it is not at all clear that a long wavelength laser source can serve as an adequate driver since the light is absorbed or reflected in the low-density wing of the plasma blown off from the target. Other laser systems are under development and, as mentioned earlier, electron beams are being explored now; ion beams may be explored in the near future. Whatever the ultimate driver source may be, it will require high conversion efficiencies and pulse repetition rates of 1 or more per second in order to be of commercial interest.

Ultimately, the attractiveness of fusion reactors based on the inertial confinement approach may hinge on the ease and cost of pellet fabrication. Assuming a hypothetical example in which the requisite driver energy for ignition is 1 megajoule, the energy multiplication from thermonuclear burn is 100 and the reactor operates on a cycle of 1 shot per second, the reactor will produce 100 MW (thermal) and every 10 shots will correspond to an energy yield of 1 million Btu. If, for the sake of argument, fuel costs are to be kept below $1 per million Btu, this implies each pellet must cost less than 10¢.

Based on information available from unclassified sources, it is somewhat doubtful that currently conceived pellet designs and driver sources will lead to commercially attractive pure-fusion power plants. On the other hand, it does appear that a fusion reactor based on the inertial confinement approach may have certain simplifying features, relative to one based on magnetic confinement approaches, which might enhance their attractiveness. It has been speculated, further, that the inertial confinement approach might lead to reactor designs that can be economical at a smaller power rating (~ 100 MWe) than some early design concepts based on Tokamaks (~ 1,000 - 2,000 MWe). At the present level of knowledge one can attach little confidence to such speculations, nor is there reason to rule out that magnetic confinement schemes might eventually prove attractive in smaller unit sizes.

Classified target information does tend to modify conclusions one may draw from information solely in the open literature. At the time of our assessment, another group, under the auspices of the Electric Power Research Institute (EPRI), was also engaged in reviewing the classified domain. Since their observations essentially reflect ours, and since the language of their remarks have met with classification approval, we reproduce them here.

> "The unpublished laser-fusion target designs suggested by recent LLL and LASL studies offer a very interesting and important possibility of pellet gain markedly exceeding that achievable with the presently published designs. The new design concepts, however, still require a very extensive experimental program. They depend on several aspects of the laser plasma interaction and pellet hydrodynamics which will be studied in

the planned ERDA programs within this decade, with some important results probably achievable by the end of FY 77.

"To explore these important possibilities on an optimum schedule appears to require some restructuring of the ERDA program and in particular much increased emphasis on target fabrication.

"The proposed targets are more complex and difficult to fabricate, but in compensation they offer an important trade-off in laser characteristics. The economic and technological optimization of a reactor may be altered in a fundamental way by this flexibility in design.

"Some possibly important results of these developments are not available to the open engineering and scientific communities because of the classification placed on the work. In any case, characteristics influencing reactor design, such as pellet yields, should be made available as soon as possible for use in unclassified reactor studies.

"No unusual problems in a fusion reactor appear to arise from the new target designs, aside from possible difficulties with pellet fabrication and cost. Several of the problems may in fact be alleviated by the expected changes in pellet output. We note however that very high pellet yields, requiring large containment vessels and possibly leading to marked variations in thermal output, may lead to difficulties in economics and in compatibility with power grid requirements."

Further elaboratorion of technological issues related to inertial confinement may be found in the next section.

TECHNOLOGICAL CONSIDERATIONS OF D-T FUSION

In this discussion the technological requirements for fusion power will be divided into three principal areas of concern: (1) the power balance, that is, the unique power-handling requirements associated with the production of electrical power by fusion; (2) reactor design, focusing primarily on the requirements imposed by a tritium-based fuel cycle, thermal-hydraulic considerations, and magnet systems; and (3) materials considerations, including surface erosion, radiation effects, and materials compatibility. The requirements discussed are based upon specific concepts, using current physics and engineering assumptions, the intent is to point out directions for improved or new plasma-confinement and engineering concepts. Therefore, the following discussions should be considered only as illustrative of the types of problems fusion will have to overcome or circumvent in the future.

THE POWER BALANCE

In contrast to fission reactors, fusion reactors will inherently require input power to establish the fuel conditions necessary for appreciable nuclear power release. Thus, fusion reactors can be viewed as energy-amplifying devices. The energy amplification achieved in a fusion reactor depends on both the plasma performance and the blanket design. The power-producing potential of a fusion reactor depends on the magnitude of the amplification, the efficiencies of the power-handling systems and the allowable capital cost of the power-handling systems. The technological requirements associated with power handling in a fusion reactor depend on the confinement scheme.

The Tokamak Reactor Concept

The Tokamak reactor operates in an ignited, quasi-steady-state mode. That is, plasma fueling and spent-fuel removal are required during cyclic burning phases. In current design studies it is generally assumed that: (a) fueling would be accomplished by injecting solid fuel pellets (with negligible power requirements) into the plasma; and (b) spent-fuel removal would be accomplished by guiding charged particles out of the plasma chamber along diverted magnetic fuel lines generated by coils called "divertor" coils. The feasibility of pellet fueling in Tokamak plasmas is currently being investigated at ORNL and GA. Stellerator confinement schemes have operated with divertors and experiments are now being constructed to investigate the behavior of Tokamaks with divertors, e.g., the PDX experiment at PPPL. Preliminary results obtained on a small Tokamak divertor experiment (DITE) at Culham, England, have been encouraging.

In the Tokamak, a changing magnetic flux induces an axial current in the plasma to provide: (a) a pulsed poloidal magnet field that works together with a steady-state toroidal field to confine the plasma; and (b) initial plasma heating that arises from the associated ohmic heating within the plasma. The power to establish and drive the axial current could be delivered by an energy storage and transfer system or by the combination of an energy storage system and a power supply taking power from the line. The energy required to establish the axial current in a Tokamak reactor will be in the vicinity of a few GJ (1 GJ = 1 billion joules), and the power requirements may approach 1000 MW at the peak power level. The TFTR at Princeton will employ motor-generator-flywheel sets to drive the axial current. Inertial energy storage could also be used in reactors, but inductive energy storage and homopolar generators are also being investigated for Tokamak applications.

It is generally assumed that the intrinsic ohmic heating process in Tokamaks will not provide sufficient heating to bring the plasma up to the ignition temperature and therefore auxiliary heating will be required to achieve ignition. A number of auxiliary techniques are currently being investigated, including neutral beam injection and radiofrequency (RF) heating. At present, neutral beam injection is considered the most promising method for reactor applications; however, more work needs to be done in the area of RF heating. For reactor applications it appears that injection powers between 50 and 100 MW will be required, with injection energies in the range of 100 keV to 500 keV in order to achieve sufficient beam penetration to heat the plasma center preferentially. The neutral beams will be on for about 5-10 seconds every burning cycle. In order to achieve good beam-system efficiencies it will be necessary to develop schemes based on negative ion acceleration and neutralization. Current Tokamak and mirror experiments employ neutral beam systems based on positive ion acceleration and neutralization. The production and acceleration of negative ions at high current density represents a new development area for fusion technology.

The power-producing potential of the Tokamak reactor depends on the fueling power requirements and on the achievable duty factor of the burn cycle. When the fueling power requirements are low (about 1 percent of the total nuclear power release) and the cycle duty factor is high (about 90 percent), the overall plant efficiency approaches that of the thermal converter, and the economic constraints on the power-handling systems are modest. It appears that it will be difficult to achieve Tokamak downtimes of less than about 1 minute for reactors. Therefore, it would also appear that burn times of the order of about 10 minutes must be achieved in order to get acceptable economics for Tokamak reactors. The length of the burning phase will be limited by either: (a) plasma-quenching resulting from enhanced plasma radiation associated with impurity buildup within the plasma; or (b) the available magnetic flux through the center of the torus, which determines the duration of the axial current. The magnetic flux limitation would easily allow burn times of 10 minutes and greater. Therefore, impurity control is essential to the economic viability of Tokamaks. It should be noted that with sufficient impurity control, it is in principle possible to operate a Tokamak in steady state, using beam injection and the bootstrap effect (the self-heating of plasma by alpha particles) to sustain the axial current.

Early Tokamak reactor studies were usually based on systems producing ~ 1000-2000 MWe. It has been mistakenly assumed by many people outside of the fusion community that these large output powers are required for Tokamaks. The electrical output of a Tokamak should, at this point, be considered a design variable whose limits have yet to be set by physics experiments and then optimized. There is no evidence indicating that the power of the Tokamak must be in excess of a 1000 MWe in order for Tokamaks to be economical power systems.

The Mirror Reactor Concept

The mirror reactor operates in a driven steady-state mode. That is, continuous energy input is required to sustain the plasma burn, because the mirror plasma does not ignite. The fusion power density is maintained at a steady state by continuously feeding fuel into the reacting plasma and by continuously removing spent fuel. Note that, in the mirror concept, plasma end losses provide the advantage of an inherent spent-fuel-removal mechanism. The energy required to start up and sustain the plasma burn would be provided by neutral beam injectors that would also serve as the fuel source in reactors. For mirror reactors operating with classical confinement (Q* ~ 1), the injected power requirements are comparable to the fusion thermal output of the device. Thus, a reactor releasing about 1000 megawatts of fusion power would require approximately 1000 megawatts of injected beam power. If end-stoppering research is successful and higher Q values become possible, the injected power requirements would be significantly reduced.

The power associated with fusion neutrons appears as heat in the blanket and converts to electricity by means of a thermal energy-conversion system. The power leaving the plasma in the form of energetic charged particles (this includes essentially all the power associated with the injected neutral beams) is fed to a direct energy-conversion system based on electrostatic concepts. The mirror operating with a Q ~ 1 requires that direct energy conversion be employed in order to achieve an acceptable overall power balance. The average energy of the injected particles would have to be in the range of about 300-600

$$^*Q = \frac{\text{Fusion Power Release}}{\text{Power Input Required}}$$

keV. It is emphasized that in a mirror reactor the neutral beam injectors must operate continuously during the burn as opposed to a Tokamak reactor in which the beams must be on only until ignition is achieved.

The power-producing potential of mirrors operating with classical confinement (that is, Q ~ 1) depends on the performance of the neutral beam injection and direct energy conversion systems. Even for relatively optimistic assumptions concerning the efficiencies of these systems, the overall plant efficiency of the classical mirror system is relatively low and the allowable capital costs for the power-handling systems appear to be extremely stringent. The power balance performance of mirror-based systems would be significantly enhanced if higher Q could be achieved. The enhancement of Q in mirror-based devices has become a major objective of the mirror program and is being pursued vigorously both at the Lawrence Livermore Laboratory and at the Oak Ridge National Laboratory.

The Inertial-Confinement Reactor Concept

In this concept, plasma heating energy in the form of laser beams, electron beams, or ion beams is delivered to a fuel pellet in about 1 nanosecond (10^{-9}s) within a cavity surrounded by a blanket. For a given fusion energy release per pellet microexplosion, the system output power depends on the achievable cavity pulse rate and on the number of reactor cavities. Cavity pulse rates in the range 1 shot per 10 sec per cavity to 100 shots per sec per cavity have been considered in reactor studies. The allowable pulse rate will be determined, to a large extent, by the time required to reestablish the necessary cavity environment permitting subsequent pellet injection and efficient beam penetration following each microexplosion. The number of reactor cavities that a single beam system can serve will be determined by pulse-rate capabilities and optical considerations.

The power-producing potential of inertial confinement reactors depends on the performance of the beam system. For example, current estimates indicate that attractive power production, assuming a pellet gain of 100, will require laser systems with an energy output of ~ 1 MJ, an efficiency approaching 10 percent, and a capital cost of ~ $200/J of laser energy output. The performance of available laser systems is currently far below that necessary for reactor applications, and new laser media may have to be identified to achieve acceptable performance. Also note that the energy storage equipment of the beam system must satisfy very stringent design requirements with regard to energy transfer times (~ 3 to 5 μsec) and repetition rates (~ 100 million to 1 billion pulses/year).

REACTOR DESIGN REQUIREMENTS

A tritium-based power economy is not feasible unless the rate of tritium production in the blanket exceeds its rate of consumption in the plasma. The requirement that the blanket breed tritium implies that lithium in some form must be present in the blanket. The tritium-breeding performance of the blanket must be such that the tritium doubling time is consistent with electrical-energy growth patterns, around 7 to 10 years by present standards. It appears that the required tritium-breeding performance can be satisfied by a variety of blanket configurations and material choices. It is also noted that the specific power (MW/kg of tritium inventory) anticipated in a fusion breeder reactor will be greater than the specific power (MW/kg of plutonium inventory) in fission breeder reactors. Therefore, the fuel-doubling times of interest require significantly lower breeding ratios with fusion reactors than with fission reactors.

The design of the blanket tritium recovery system is intimately related to the choice of materials, blanket cooling system, power-conversion system, and tritium-containment technology. The types of technology required for the blanket tritium recovery systems under current consideration are to a large extent available. However, specific schemes will require considerable development and demonstration. In addition there is need for data concerning: (a) permeability, with and without diffusion barriers, at low tritium partial pressures (in the range of 10^{-6} torr and lower); (b) diffusion coefficients for tritium in proposed breeding materials and coolants at low concentration (in the range of ~ 10 ppm and lower); and (c) equilibrium information on tritium-lithium systems and tritium-metal systems over a wide temperature range.

The technological requirements associated with the recovery of tritium from the plasma exhaust seem less formidable than those associated with the recovery of bred tritium. On the other hand, the plasma-exhaust tritium-recovery system will be required for tritium-burning experiments that do not employ a breeding blanket. Thus, demonstration of a plasma-exhaust tritium-recovery system is a nearer term objective than demonstration of a blanket tritium-recovering system. DOE is currently reviewing proposals for a tritium systems test facility, the major objective of which will be to demonstrate the plasma-exhaust tritium-recovering system for fusion devices.

Energy deposition at the first wall of the blanket will result in design limitations based on thermal stress considerations for all reactor concepts. The potential effects of energy deposition at the first wall appear to be most severe in the case of inertial confinement. In these concepts, the very short time scales for energy deposition would result in ablation of an unprotected metal first wall and in severe thermal cycling effects. Several alternative cavity first wall designs are being considered to ameliorate this problem.

Both mirror and Tokamak reactors will employ steady-state superconducting magnet systems for plasma confinement. The stored energy associated with these magnet systems falls in the range of about 100,000 to 200,000 joules per kWe. If capital cost allotments of around $200 per kWe are allowed for the steady-state magnet system, then the magnet cost must be in the range of about 1 to 2 mills per joule of stored energy. This allowable cost is in close agreement with projected costs for large superconducting magnet systems. It appears that force containment and mechanical design will be the limiting factors in the design of superconducting magnet systems for mirror and Tokamak reactors.

However, protection of the magnets in the event of a sudden plasma quenching also represents a major area of concern. DOE is currently involved in a superconducting magnet development program aimed at developing large coils for Tokamak and mirror systems.

MATERIALS CONSIDERATION

Surface erosion associated with plasma-particle bombardment can significantly limit the useful lifetime of components in fusion reactors. Moreover, in Tokamak plasmas the presence of impurities arising from first-wall erosion can severely affect plasma performance. It appears that the erosion processes of potential concern in fusion reactors will be plasma-particle sputtering and exfoliation resulting from the bursting of radiation-induced blisters. Currently, these phenomena are poorly understood in the context of a fusion reactor environment and, therefore, it is not possible to calculate accurate surface erosion rates in fusion reactors at present. A number of schemes are being pursued to protect both the plasma and the first wall from the consequences of surface erosion. Fundamental studies on sputtering and blistering are being conducted within the fusion program.

Neutron-induced atomic displacements and gas production can lead to deleterious changes in the structural properties of materials, and thereby limit the service life of structural components in fusion reactors. These radiation effects are expected to be most severe at high temperatures and in the vincinity of the blanket first wall. Calculations indicate that at the first wall (a) annual atomic displacement rates would be lower than those achieved in high-flux fission reactors, and (b) annual gas production rates would, with the exception of nickel-bearing materials, be substantially higher than those achieved in high-flux thermal fission reactors. Currently, intense sources of fusion-energy neutrons [$\sim 10^{14} n/(cm^2 sec)$] are not available; therefore, materials testing for fusion reactors must rely heavily on fission neutron irradiations and ion bombardments to simulate the fusion reactor radiation environment. Recent data obtained on the radiation performance of stainless steel under simulated fusion conditions suggest that the structural lifetime of the blanket first wall may be substantially increased (about 10-20 megawatt years per square meter) beyond that estimated several years ago (about 2 megawatt years per square meter) providing the operating temperature is lowered to 450°C or less.

The instantaneous displacement and gas-production rates during the plasma burn of inertial confinement reactor concepts are about six orders of magnitude greater than those associated with the mirror and Tokamak reactor concepts. The higher displacement rates represent a completely different damage regime than we are used to dealing with in fission reactors. Very little is known about the effects of enhanced recombination of defects under those conditions, but preliminary theoretical estimates reveal that such effects may even reduce the residual damage in metals at high temperature. However, there is no significant experimental effort being conducted at this time to substantiate these predictions. The rate dependence of the radiation effects must be investigated. At present, it appears that this rate dependence can be examined only by ion-bombardment techniques.

In general, the theoretical understanding of corrosion phenomena and kinetics is inadequately developed. Therefore, an extensive experimental program will be required to define the corrosion behavior of potential structural materials in lithium, lithium salts, and helium under the operating conditions and radiation environment expected in fusion reactors.

ENVIRONMENTAL ISSUES ASSOCIATED WITH PURE D-T FUSION

In assessing the environmental implications of D-T fusion power, we shall consider the following topics: radioactivity in effluents under normal operating conditions; reactor safety and waste disposal; thermal effects; and resource requirements. Further remarks on alternative fuel cycles are also included.

ROUTINE RELEASES

Under normal operating conditions, the primary radiological concern appears to be the escape of tritium to the environment. Current estimates suggest that a 1000-MWe fusion power plant would have a tritium inventory of about 3 to 30 kg, or about 30 million to 300 million curies. Because tritium permeates most metals at high temperatures, it can diffuse through containment walls, fluid piping, and heat exchanger tubing used for the blanket and its related systems. There are two primary paths by which tritium might eventually escape to the environment during normal operating conditions: through the blanket containment walls and fluid piping into the surrounding atmosphere, or through the coolant system into the steam cycle via the coolant-system heat-exchanger tube walls.

There are, as yet, no generally applicable standards concerning tritium release to the environment; nor can we say what the future limitations on tritium releases from fusion reactors should be. However, if light water fission reactor guidelines are applied to fusion reactors, the tritium leakage rate would have to be limited to a level of ~ 10 curries per day. The attainment of such a leakage rate seems technologically feasible; however, a major objective of fusion reactor technology will have to be the *demonstration* of adequate tritium containment at acceptable costs.

REACTOR SAFETY AND WASTE DISPOSAL

The magnitude and characteristics of the radioactive inventories induced by neutron interactions in the structural material of the blanket are major considerations in assessing D-T fusion reactor safety and radioactive waste disposal. The magnitude of induced structural and blanket radioactivity in a D-T fusion reactor is dependent upon the choice of material. For the wide range of materials and blanket designs being considered, the level of induced activity at equilibrium is generally in the range of about 1 billion to 10 billion curies for a 1000-MWe plant, clearly a significant level. To reduce the level of induced radioactivity, materials R&D programs on low-activation materials such as graphite, silicon, carbide, high-purity aluminum, and titanium are being conducted by the Electric Power Research Institute (EPRI). However, the level of activity by itself is not a meaningful measure of the technological problems posed by radioactive inventories; the associated nuclear afterheat, the time-dependent behavior of the activation products, and mechanisms for their release also need exmination.

At shutdown, the afterheat power density in the fuel of advanced fission reactors is anticipated to be at least one to two orders of magnitude greater than that expected in the structural components of fusion reactor blankets, even without the use of low-activation materials. For example, liquid metal fast breeder reactor (LMFBR) fuel would have an afterheat power density of ~ 100 watts per cubic centimeter while the first wall of a fusion reactor would have an afterheat power density no greater than ~ 1 watt per cubic centimeter. The conclusion is that afterheat removal will be less of a problem in fusion reactors than in fission reactors. Moreover, it appears that the engineered safety features necessary to limit biological impact in the event of an accident may have to satisfy less stringent requirements in fusion reactor design than in fission reactor design. This observation does not mean that fusion reactors will necessarily be safer than fission reactors, but rather that the technology and engineering necessary to achieve a given level of safety may prove less difficult and costly for fusion reactors.

A number of materials are candiates for fusion reactor structure. These include refractory (high-temperature) alloys based on niobium, molybdenum and vanadium, as well as conventional iron- and nickel-base alloys. Calculations suggest that, for most engineering alloys, long-term solutions to waste disposal similar to those sought for radioactive wastes from fission reactors may be required. On the other hand, the use of materials such as vanadium-base alloys might allow recycling of the blanket structure following a relatively short cooling period (of the order of 10 years). Should the low-activation materials mentioned previously prove practical, the decay period would be reduced even further. Within this context, it is possible that a fusion-power economy might not require the long-term radioactive waste disposal associated with fission power. This might represent a major societal advantage of fusion power relative to fission power. However, the mechanical behavior of vanadium alloys in a fusion reactor environment is unknown and there exists no industry (mining or fabrication) capable of producing large amounts of vanadium at present.

THERMAL EFFECTS

The D-T fusion reaction releases energy in two forms--neutron and charged-particle energy. The neutron energy, which eventually is manifested as heat within the blanket, would be recovered by a thermal energy-conversion system. The charged-particle energy could also be recovered as heat by a thermal energy-conversion system. However, direct recovery of this portion of the energy may also be possible. Because only about 20 percent of the energy released in D-T fusion appears as charged-particle energy, the impact of such direct energy conversion is marginal in a D-T fusion power economy. For example, consider a case in which a thermal energy-conversion system of 40 percent, efficiency is used for recovery of the neutron energy and a direct energy-conversion system of 70 percent efficiency is used for recovery of the charged-particle energy. The overall recovery efficiency for such a system would be 46 percent, about 15 percent higher than that for the thermal conversion system itself. However, fusion reactors inherently require input power to establish the fuel conditions necessary for fusion power production. Therefore, a fraction of the gross electrical output of the plant must be recirculated to sustain the fusion process. The amount of recirculating power required in fusion power plants can be appreciable. Thus, even when direct energy conversion is assumed for a portion of the fusion energy release, the overall plant efficiency (that is, the ratio of the net electrical power output to nuclear power release) of current D-T fusion reactor concepts is comparable to the overall plant efficiencies of fossil and fission power plants (i.e., in the vicinity of 30 to 40 percent), and there are corresponding thermal effluents as well.

RESOURCE REQUIREMENTS

The use of deuterium and lithium for fusion power should result in less stringent environmental impacts and economic constraints than those associated with the fuel requirements in fission power plants. On the other hand, the resource requirements for building the fusion reactor plant seem to be considerably more extensive. This is because estimated nuclear power densities in fusion reactor blankets, based on present understanding of permissible first-wall loading (2MW/square meter), are inherently lower (by one to two orders of magnitude) than those in fission reactor cores, which means that, for a given power generation level, a fusion reactor blanket will required significantly more structural material. For example, the stainless steel required for the nuclear island of the UWMAK-II Tokamak fusion reactor design, is about 5 to 10 times greater than that required for the nuclear island of a comparable liquid metal fast fission breeder reactor. By a factor of 2, UWMAK-II improvements attained might be in the β and in the first-wall performance. Although there do not appear to be any resource limitations to prevent fusion power development, the environmental and economic implications of the resource requirements must be examined carefully. For example, use of materials such as niobium, chromium, nickel, manganese and helium presents unique resource requirements for fusion power.

The nature of the problem could be reduced substantially if low-activation, recyclable material could be developed.

The principal use of helium in a fusion reactor will be in the cooling of superconducting magnets. On the basis of current reactor studies, the helium requirements for magnet cooling in fusion reactors would be ~ 0.05 metric tonnes per MWe. If helium were also used as a blanket coolant, the helium requirement would be increased by ~ 0.01 metric tonnes per MWe. Even if the present-value price of helium were to increase 100-fold relative to current helium prices (i.e., if we were forced to recover helium from the atmosphere), the associated increase in the capital cost of fusion power would only be on the order of a few percent. Therefore, the long-term implications of our current helium policies do not suggest any serious obstacles to implementating fusion power.

ALTERNATIVE FUSION FUELS

Several environmental drawbacks are commonly attributed to D-T fusion power. First, it produces substantial amounts of neutrons that result in induced radioactivity within the reactor structure, and it requires the handling of the radioisotope tritium. Second, only about 20 percent of the fusion energy yield appears in the form of charged particles, limiting the extent to which direct energy conversion techniques might be applied. Finally, the use of D-T fusion power is limited by lithium resources, which are less abundant than deuterium resources. As mentioned earlier, this limitation is on the time scale of 1000 years.

These drawbacks of D-T fusion fuel cycle have led to a number of alternative proposals, fusion-power reactors based only on deuterium being one possibility, the D-^{3}He fuel cycle being another. The goals of such proposals are to (1) reduce the magnitude of neutron production, as well as the need to handle large amounts of tritium, (2) produce more fusion power in the form of charged particles, and (3) reduce the system's dependence on lithium resources. It has also been suggested that materials with higher atomic numbers (such as lithium, beryllium, and boron)* be used as fusion fuels to provide power that is essentially free of neutrons and tritium and that releases all of the fusion energy in the form of charged particles. Many of the materials problems and reactor maintenance problems discussed earlier might be reduced or eliminated through the use of such fuel cycles. Although such alternatives to D-T fusion power appear very attractive, there are at least two important caveats:

1. Of all the fusion fuels under current consideration, the deuterium-tritium fuel mixture requires the lowest value of $n\tau$ by at least one order of magnitude and the lowest fusion temperatures by at least a

*An often-cited reaction is: $p + {}^{11}B = 3 \cdot {}^4He + 8.7$ MeV, which has a peak cross-section of approximately 0.9 barns at 675 keV, and is to be compared with the D-T peak cross-section of about 5 barns at 100 keV.

factor of 5. When the plasma *requirements* for significant power generation are compared with the anticipated plasma *performance* of current approaches to fusion power, it is apparent that fusion power must initially be based on a deuterium-tritium fuel economy.

2. The maximum attainable fusion power density is about one to two orders of magnitude greater with the D-T fuel mixture than with any of the alternative fuel mixtures. Thus, even if adequate confinement and heating could be achieved for implementing alternative fuels, such systems would face an economic disadvantage relative to the D-T system on the basis of power density.

Nonetheless, we would like to hold out the hope that at some future date technology may advance to the point where the use of such ideal fusion fuels will become practical.

OTHER APPLICATIONS OF FUSION

FISSILE FUEL PRODUCTION

It should be evident from the preceding discussions that the parameter Q, the ratio of nuclear power yield to power input, is of considerable importance to any fusion reactor design. Insofar as a fusion reactor may be viewed as an energy-multiplying device, practical considerations warrant as high a Q value as possible. For some time it has been recognized that one can achieve very high overall Q even though the fusion reactor, *per se*, operates at Q no greater than unity. The energy multiplication, then, results from conversion processes external to the fusion reactor, and is dependent largely on what applications one may foresee for the 14.1 MeV neutrons, which carry some 80 percent of the energy released in the D-T fusion reaction.

In a suitably designed blanket the fast 14.1 MeV neutrons can be made to multiply and produce a larger number of slower neutrons through n-2n reactions and fast fission. As a result, it is possible to design blankets containing lithium and natural (or depleted) uranium, with or without thorium, so that for every fast neutron entering from the fusion reactor core, one obtains one (plus a slight excess) tritium atom to replace the one lost by fusion, a number of fissile atoms of either plutonium or ^{233}U or both, plus an amount of heat. A blanket designed to optimize fissile fuel production is usually termed a fusion-fission symbiont, while one designed to optimize power production is termed a fusion-fission hybrid. Notice that up to five fissile atoms can be obtained in this cascade process for each fusion-produced neutron, and recalling that with fuel recycling each fissile atom can yield 300 MeV in an external fission burner (thermal reactor), we readily perceive that the total energy release by the hybrid or symbiont mode can be two orders of magnitude greater than by the pure fusion mode, for each D-T event.

Thus, for commercial introduction of early generation fusion, approaches with intrinsic Q near unity (for example, two-component Tokamaks, mirror devices, or pellet fusion based on simple targets) enjoy better prospects as hybrids or symbionts than they do as pure fusion reactors. Moreover, we might argue, as some U.S.S.R. spokesmen have, that the inherent advantages of hybrids or symbionts tend to rule out any practical interest in pure fusion development at this time. The opposite view, however, can also be readily stated: the engineering complexities and environmental problems that would result from adding fissioning or fissile-fuel-producing blankets to a fusion reactor are so formidable that the pursuit of fission-fusion concepts would constitute a counter productive diversion to the fusion program. It should be noted that fusion-fission systems based on the plasma design goals of the next generation Tokamaks appear to yield attractive fission fuel breeders.

The symbiont concept is clearly an alternative to the fission breeder, with possible advantages in terms of controlling accidental increases in reactivity, and with additional possible advantages under circumstances where fissile fuel production for an existing reactor complex is more important than the expansion of generating capacity.* In this context, it is also necessary to consider the "electrical breeding" or spallation concept. In the spallation process, high-energy accelerators are used to produce energetic beams of protons or deuterons, which are subsequently fired at liquid metal primary targets (lithium, bismuth, or lead being possible candidates), or appropriately cooled solid targets of uranium or thorium, to produce large numbers of neutrons. In effect, the neutrons "boil off" the target nuclei and continue to multiply in a suitably designed blanket or secondary target. The cascade process is somewhat similar to what has been described above, and it is possible to conclude that one fissile atom would be produced for each 10 to 20 MeV of beam energy in a suitably designed blanket. The concept dates back to the late 1940's and early 1950's, when E. O. Lawrence initiated the materials testing accelerator (MTA) project in the U.S. There has been a revival of interest in spallation, particularly among Canadian workers at Atomic Energy of Canada, Ltd. (AECL). Most current concepts envision ion beams of about 0.5 to 1 GeV in energy with currents ranging from 0.25 to 0.5 amperes. Advances in RF power supplies have contributed to the renewed interest in building such large (approximately 1 kilometer in length) ion beam accelerators.

Some rather preliminary estimates indicate that both symbiont and spallation approaches might lead to fissile fuel production at a cost of \$50 to \$100 per gram of ^{233}U or ^{239}Pu. As these estimates are in the potentially attractive range for future considerations, a more detailed discussion of this matter is included in a separate appendix. One needs to bear in mind the distinction, however, in that the "electrical breeder" using conventional accelerators has a considerably more advanced technological base than the fusion-fission symbiont, where the latter is based on as-yet-untested fusion reactor concepts.

*Since a fusion-fission symbiont, in principle, could fuel four to five LWR's, we can envision a converter reactor economy fueled by fusion-fission devices, *ad infinitum.* Whether this is on interest depends ultimately on the relative economics of such a hybrid complex *versus* a fission breeder economy.

CHEMICAL FUEL PRODUCTION

Some amount of attention has also been given to using the 14.1 MeV fusion neutrons to produce chemical fuels by radiolysis. For example, production of CO from CO_2 by neutrons, alpha particles, secondary gamma rays, and other secondary nuclear reaction products may be considered. The CO may then be used to generate hydrogen by the familiar water-shift reaction. Measured efficiencies for radiolytic production of CO from CO_2 by gamma radiation is 15 percent and by alpha radiation is 30 percent. There is no data available for radiolysis efficiencies by 14-MeV neutrons, though conservative guesses place it midway between the above two figures. Approximately, one estimates that a one meter thickness of CO_2 blanket at 200 atm (gas phase) would be sufficient to attenuate the 14.1-MeV neutrons.

Direct radiolysis of water by ultraviolet radiation to product hydrogen may also be considered, and more complex schemes using recyclable hydrogen halides have also been proposed. Advanced fusion fuel cycles, such as p - ^{11}B, in which much of the energy is emitted as hard x-rays, offer unique possibilities for chemical fuel production. Given our present state of understanding, it is difficult to draw any conclusions about the merits of configuring fusion reactors such that the bulk of their net energy output is realized through the production of chemical fuels. The proposition certainly deserves more careful analysis.

NUCLEAR WASTE DISPOSAL

In principle, any neutron-rich source might be used to transmute accumulated radioactive waste products to nuclei that either are stable or decay more rapidly to stable ones. Thus, transmutation provides a possible means of bringing the radioactivity levels of nuclear wastes down to values comparable to natural radioactive material. Were this to be economically feasible, transmutation would offer an alternative to disposal in geological formations.

It has been proposed that some fusion reactors could be dedicated to burning fission wastes. Preliminary studies indicate that for the transmutation of the actinides and most long-lived fission products, neutron wall loadings on the order of 5 to 10 MW per square meter would be required. Such values of wall loadings are about a factor of two higher than those proposed in current pure-fusion reactor designs; however, limitations on wall loading are yet to be determined experimentally and values of 5 to 10 MW per square meter might be feasible. Transmutation of ^{90}Sr and ^{137}Cs, on the other hand, would require on the order of 200 to 500 MW per square meter of neutron wall loading, and it is difficult to see how this can be sustained in currently conceived fusion reactors; consequently, new concepts, such as the Linus experiment at the Naval Research Laboratory, would have to be explored to meet this requirement.

SUMMARY

In conclusion, we do not believe that any of the alternative approaches to pure fusion have been examined in sufficient detail to warrant a marked change in present plans, which have as their primary goal the development of pure-fusion reactors.

CONCLUSIONS

DOE is currently the principal agent in the U.S. for the development of fusion power. This arrangement is likely to continue for some time into the future, until progress and incentives of sufficient magnitude to attract private investments become apparent. It is nevertheless essential that there be close interaction between DOE and the ultimate customer, the utilities, in the course of such a civilian development program. Both parties, the developer and the user, must learn to appreciate their respective requirements. Of course, there is a large worldwide effort on fusion, in addition to the U.S. program. The cooperation and complementarity of this global effort is perhaps unique in the history of technology; the U.S. program is surely a beneficiary of this state of affairs.

Nevertheless, DOE and its counterparts in other countries, principally the U.S.S.R., EEC, and Japan, face difficult choices in planning for the orderly development of fusion power. The near-term objectives are in a state of transition. Although scientific feasibility has yet to be demonstrated by any of the approaches now under consideration, there is a growing conviction that this will be achieved relatively soon. No fundamental conceptual difficulties seem to be evident that would indefinitely delay the demonstration of scientific feasibility.

The question that remains open is whether one or more of the approaches promising to achieve scientific feasibility in the near future will lend themselves to the development of a practical, commercial fusion-reactor technology. The principal concern revolves around capital cost, minimum plant output, plant availability, plant complexity, and environmental characteristics.

A reasonable approach to developing fusion power involves at least the following elements of a program plan:

1. Continue to concentrate on the main line approaches showing the greatest physics promise at this time. For magnetic confinement this is the Tokamak, backed up by the mirror, while for inertial confinement it is the laser-driven micro-pellet implosion, backed up by other potential energetic driver sources. It is by this route that we hope to gain further information rapidly on plasma behavior and scaling laws under conditions simulating the reactor regime.

2. Explore all promising routes to improving the potential reactor performance of the main line approaches. Certain undesirable features that may show up in early generations of conceptual reactor designs (based on extrapolations of our current state of understanding), will serve as valuable guideposts for further improvements.

3. Explore other physics and engineering options in sufficient depth over the next 5 to 10 years to determine how they might lead to fusion systems more desirable to a user in terms of cost, size, operational, and envionmental characteristics. This will require a number of intermediate-sized physics and engineering experiments and tests. Program planners will have the difficult task of selecting the areas that promise the necessary flexibility for a successful development program. Full but premature commitment to a single approach on the one hand, and unfruitful procrastination and dissipation of resources on the other, will detract from an orderly development schedule and should be avoided.

4. Study, over the next 5 to 10 years, all the important generic technological problems inherent in the development of fusion power. Engineering experience under realistic conditions representative of a fusion reactor environment must be pursued as soon as feasible. Pilot-plant-scale experiments, which should be kept at a minimum size but could still be quite large, will be required eventually to provide the necessary test beds for further development. To demonstrate the practical value of fusion and encourage industrial participation, fusion systems with useful outputs should be developed in the smallest possible size at the earliest possible date. The move to pilot-plant-scale experiments, for purposes of scaling eventually to commercial size reactors, should not be attempted, however, until a greater level of understanding is reached in the areas of confinement plasma physics and materials properties. Emphasis, for the time being, must continue to be on developing improved confinement schemes.

5. Because of the importance of meeting needs in the energy field, pursue applications of fusion energy for fission and chemical fuel production and fission waste disposal in sufficient depth to make possible a meaningful comparison with other options. Fusion physics concepts that hold unique potentials must be investigated adequately to assure a fair evaluation of fusion in these areas.

6. Finally, recognize the cost of the various program elements, the need for continuing expensive physics and engineering experiments, followed by even more expensive pilot-plant-scale experiments, all leading up to the eventual design and construction of one or more demonstration plants. Some preliminary DOE estimates indicate that cumulative costs over the next 20 to 25 years, arriving at a single demonstration unit, will be on the order of $15 billion in terms of constant dollars, and it's just possible this figure is on the low side. One might be able to accelerate the pace of development somewhat with even larger expenditures of money. This would permit a wider and more rapid exploration of physics and engineering options, and could open up paths to fusion systems that better meet the user's requirements and hence accelerate the time scale to commercialization. Such an action would help avoid

a premature decision to concentrate on an uneconomic concept due to inadequate funds to explore other options.

When all this is said, we realize that the fusion effort is still in relatively early stages of development; ultimate success in terms of a viable commercial entity cannot be predicted with certainty; and it will surely take time, money, and the dedication of skilled workers to make progress in removing the uncertainties. Because of its reliance on virtually unlimited and cheap fuel and its relative safety, a strong program of fusion power development deserves the full support of the federal government.

A number of crucial scientific and engineering questions remain to be answered, including: How high a β can be achieved in closed magnetic confinement schemes? What ultimately governs anomalous diffusion rates which in turn govern $n\tau$ in a device? Can effective end-stoppering be achieved for mirrors? Can efficient and suitably matched drivers for inertial confinement schemes be developed? How high a magnetic field strength can be produced by superconducting coils? What will be the lifetime of first walls? Can low-activation material be found to stand up to the fusion environment?

The fusion program should be guided by continued systems studies and evaluations that incorporate new physics, materials, and engineering data and respond to feedback from the utility industry. As a result of the early system designs made over the past few years, and the extensive planning efforts at ERDA that in part have been inspired by these studies, the fusion effort has come under a certain amount of criticism. Some of this has come from electric utility spokesmen and should be taken seriously, in view of the fact that the utilities are the principal potential customers of fusion reactors.

The early systems studies enabled the users to point out objectionable features that should be avoided in future fusion designs, such as excessively large size (installed unit capacity) by current standards, low power densities (fundamentally the same fault), questionable reliability, cold start requirements (e.g. 1500 MWe startup power required for a particular 840 MWe mirror unit), increased cooling requirements resulting from large circulating power, energy storage for cyclic operation, remote maintenance in the presence of high background radiation inherent to fusion reactors, additional complexity as witnessed in several early designs, imprecise knowledge of reactor control and safety handling, and so forth.

Workers in the field are quite sensitive to all of these issues. Continual conceptual fusion reactor designs will be required in varying degrees of depth to test the significance of new ideas, data and concepts against realistic requirements. However, we feel that 1985 is perhaps the earliest date by which enough new scientific and engineering understanding will have been reached to enable anything resembling a realistic preliminary reactor design. These reactor designs should be considerably more detailed and include many features left out of the current generation of reactor studies. Towards the end of this century, it is conceivable that a demonstration reactor might be constructed and operated, although it is likely that such an early model will be far from optimal.

It is clearly somewhat meaningless at this stage to speculate on the earliest date by which fusion will begin to have a significant economic impact (say, the production of 1 quad equivalent of energy per year).

The conclusion we are prompted to reach at this point is that with gradually improved scientific understanding and technological advance, achievable in a vigorously supported program, fusion, with its multiple approaches and techniques will appear continuously more attractive as an ultimate long-range solution to the energy problem.

SUMMARY

1. It seems highly probable that scientific feasibility for fusion will be demonstrated for magnetic confinement, and perhaps for inertial confinement, within roughly the next 5 years.
2. Early generation conceptual designs based on conservative physics and materials properties have led to reactor systems that do not appear to be commercially attractive. There are obvious paths to follow, leading many to believe that the shortcomings of the above *are* soluble, and most of us share this optimism.
3. A critical step is to learn what plasma performance will be obtained in the reactor physics regime, so we must forge ahead with scaled-up, mainline approaches. In parallel, the technology program has to be pushed hard.
4. There is considerable room for improvement, in terms of evolution of present confinement concepts, development of new ones, and development of superior materials; an R&D atmosphere that stimulates innovation is highly desirable. Many options are permitted by the physics, and those that appear promising should be explored.
5. If the fusion program is continued at a high enough level of funding, we would expect that progress achieved by 1985 or thereabouts would permit a realistic appraisal of the prospects for embarking on a commercial demonstration project.
6. It is plausible, though one can give no guarantee, that a successful demonstration in a prototype commercial fusion reactor could be achieved within the next 20 to 25 years for an estimated 15 to 20 billion dollars (exclusive of inflation). These numbers are highly speculative and could be either high or low.

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Rose, D., and M. Clark. 1961. Plasmas and Controlled Fusion, Cambridge, Mass.: MIT Press.

For an introduction to plasma physics:

Artsimovich, L. 1964. Controlled Thermonuclear Reactions. New York: Gordon & Breach.
Krall, N., and A. Trivelpiece. 1973. Principles of Plasma Physics. New York: McGraw-Hill.

For a survey on fusion prospects:

Brueckner, K., J. Yonas, H. Furth, and F. Ribe. 1974. Physics and the Energy Problem. New York: American Institute of Physics.

For a qualitative discussion of Tokamaks:

Furth, H. P. 1975. Tokamak Research. Nuclear Fusion 15:487-534.
McDonnell Douglas Astronautics Company/University of Wisconsin. February 1976. Major Features of D-T Tokamak Fusion Reactor Systems. Palo Alto: Electric Power Research Institute. EPRI 472-1.

For a detailed discussion of mirrors:

Baldwin, D. April 1977. End-loss Processes from Mirror Images. Reviews of Modern Physics 49 (No. 2):317-339.

For a review of laser fusion:

Brueckner, K., and S. Jorna. April 1974. Laser-driven Fusion. Reviews of Modern Physics 46 (No. 2):325-367.
K. A. Brueckner and Associates, Inc. September 1976. Assessment of Laser-driven Fusion. Palo Alto: Electric Power Research Institute. EPRI ER-203.

A quantitative survey of recent fusion research is best found in the proceedings of the International Atomic Energy Agency biannual Plasma Physics and Controlled Fusion meetings. For example, the proceedings of the 1974 Tokyo and 1976 Berchtesgaden meetings have been published by IAEA in Vienna. Summaries were published in Nuclear Fusion 16:1047.

On reactor technology and conceptual design:

Bloom, E. E., F. W. Wiffen, P. J. Maziasa, and J. O. Stiegler. October 1976. Temperature and Fluence Limits for A-Type 316 Stainless Steel CTR Firsh Wall. Nuclear Technology 31 (No. 1): 115-122.

General Atomic Company. August 1976. Applications of Low Atomic Number Ceramic Materials to Fusion Reactor First Wall. Palo Alto: Electric Power Research Institute. EPRI ER-216.

These papers report results of a series of neutron irradiation experiments conducted on annealed and 20 percent cold-worked 316 stainless steel in a high flux mixed-spectrum fission reactor to simulate controlled thermonuclear research (CTR) first-wall displacement per atom (dpa) and helium production. Using previously suggested uniform strain in a uniaxial tensile test, estimates of temperature and fluence limits for this alloy are made.

The large amounts of helium produced by irradiation in the mixed-spectrum fission reactor caused significantly more swelling than occurred in fast reactor irradiations (low helium generation rates). Cold working effectively suppressed swelling up to 550-600 degrees C. Using a criterion of 10 percent swelling and limited data on the fluence dependence of swelling, a first-wall life of 16.5 megawatt years per square meter (at 530 degrees C) for 20 percent cold-worked 316 stainless steel is estimated.

Embrittlement may be the property that limits first-wall life. At 350 degrees C, acceptable ductility was retained in the cold-worked steel to very high damage levels: 49 dpa, 3320 atomic parts per million (appm) He. It appears that the 0.5 percent uniform strain criterion will not be limiting. At higher temperatures, however, this is not the situation. At 650 degrees C, the uniform and total plastic strain were zero in samples irradiated to 61 dpa and 4140 appm He. At 575 degrees C, 0.5 percent uniform strain was retained in the cold-worked material to relatively high damage levels; however, the fractures were intergranular. The creep-rupture life at 550 degrees C and 45,000 psi was reduced by 50,000 compared to the unirradiated property. Generally greater embrittlement in the solution-annealed material suggests that cold-worked material would be preferred for CTR first-wall structures.

The intergranular tensile fractures and marked reduction in ductility and rupture life suggest that stress will have to be maintained at very low levels to prevent fracture. The loss of ductility indicates reductions in fatigue life that must be investigated.

Carlson, G. A., and R. W. Moir. 1977. Mirror Machine Reactors. Pp. 555-574 in G. L. Kulcinski and N. M. Burleigh eds. Proceedings of Second ANS Topical Meeting on the Technology of Controlled Nuclear Fusion, Richland, Wash., September 21-23, 1976. Washington, D.C.: Government Printing Office. CONF-760935-P2.

Recent mirror reactor conceptual design studies are described. Considered in detail is the design of "standard" Yin-Yang fusion power reactors with classical and enhanced confinement. It is shown that to be economically competitive with estimates for other future energy sources, mirror reactors require a considerable increase in Q, or major design simplifications, or preferably both. These improvements may require a departure from the "standard" configuration. Two attractive possibilities, both of which would use much of the same physics and technology as the "standard" mirror, are the field-reversed mirror and the end-stoppered mirror.

Clarke, J. F. June 1976. High Beta Flux-Conserving Tokamaks. Oak Ridge: Oak Ridge National Laboratory. ORNL/TM-5429.

In any magnetically confined fusion device, there is a premium on operation at the highest possible beta because the fusion power output at a fixed magnetic field depends on the square of the beta. Since much of the capital cost of a magnetically confined fusion reactor is associated with the production of magnetic fields, high beta operation is a necessary ingredient in the formulation of a low capital cost system. With regard to Tokamaks, there is a widely held conception that the attainable beta is limited by equilibrium constraints. This has led to the design of a number of low beta Tokamak reactor systems, and has thereby imposed severe constraints on the economic viability of these systems. It is the purpose of this memo to show that this widely used beta limit on Tokamaks is highly dependent on the method of achieving the high beta equilibrium and that a class of systems exists which is not subject to any equilibrium beta limit at all. In these systems the ultimate limitation on beta must be found from magneto-hydrodynamics (MHD) stability theory, not from equilibrium considerations.

Conn, R. W., and G. L. Kulcinski. 1974. Technological Implciations for Tokamak Fusion Reactors of the UWMAK-I Conceptual Design. Pp. 56-60, in G. R. Hopkins, ed., Proceedings of the First Topical Meeting of the Technology of Controlled Nuclear Fusion. Springfield, Va.: NTIS. CONF-740402.

Kulcinski, G. L., and R. W. Conn. 1974. The Conceptual Design of a Tokamak Fusion Power Reactor, UWMAK. Pp. 38-55, Ibid.

These two articles provide an in-depth analysis of one of the first self-consistent Tokamak reactor designs. While many improvements have been made since this reactor was designed, it does show how conceptual designs can be used to identify unforeseen problems, to provide a quantitative basis for future technology experimental programs, and to lay the groundwork for economic parameter studies. The plasma physics, neutronics materials, responses, radiation damage, heat transfer, tritium handling, magnet design, power cycle, plant layout, and economics have been summarized in these articles while the details can be found in a University of Wisconsin report UWFDM-68, Vol. 1 and 2, 1974.

Conn, R. W., C. L. Kulcinski, and C. W. Maynard. 1976. A Conceptual Design of a Helium Cooled, Solid Breeder, Tokamak Fusion Reactor System. Nuclear Engineering and Design 39:5-44.

This article summarizes a much more detailed design report (UWFDM-112, 1975) on a 5000-MW Tokamak Power Reactor Design. The object of this study was to uncover the technological problems associated with helium-cooled Tokamaks which use solid breeder materials. In-depth analyses of the plasma physics problems revealed the divertor collection plates to be a particularly difficult problem. Theuse of Be as a neutron multiplier, necessary for the attainment of breeding ratios > 1 in this stainless-steel structure blanket, also was identified as a limiting constraint to fusion power. In-depth analyses of the neutron leakage up divertor slots, magnet design, tritium extraction, and load leveling were also conducted.

Davis, J. W., and G. L. Kulcinski. 1976. Major Features of D-T Tokamak Fusion Reactor Systems. Nuclear Fusion 16 (No. 2):355.

This article is an up-to-date (mid-1976) list of the proposed physics, material, coolant, tritium, neutronic, radiation damage, magnet power cycle, and economic parameters of various Tokamak reactor designs. Four classes of reactors were studied; near-term experimental devices (TT-60, TFTR, T-20, JET); three mid-term EPR's (ANL, ORNL, GA); two mid-term Russian hybrid reactors; and 10 long range power reactors. Tabular lists of the proposed operating parameters are given but the reader is cautioned that this listing only represents a snapshot in time and the design parameters may change in the future.

General Atomic Company. September 1975. Fusion Reactor Studies: Potential of Low-Z Materials for the First Wall. Palo Alto: Electric Power Research Institute. EPRI 115-2.

McDonnell Douglas Astronautics Co., University of Wisconsin. February 1976. Major Features of D-T Tokamak Fusion Reactor Systems. Palo Alto: Electric Power Research Institute. EPRI 472-1.

Steiner, D., and A. P. Fraas. September-October 1972. Preliminary Observations on the Radiological Implications of Fusion Power. Nuclear Safety 13 (No. 5):353-362.

The radiological implications of fusion power are considered with reference to a conceptual fusion reactor based on the deuterium-tritium fuel cycle. This analysis leads to the following observations: (1) The engineered features necessary to limit biological impact in the event of an accident may have to satisfy less stringent requirements in fusion-reactor design than in fission-reactor design. (2) During normal operation, tritium will present the primary source of radioactivity in effluents associated with fusion power. The monitoring of tritium in effluents will be required only at the reactor site since the fuel-reprocessing system of a fusion reactor is an integral part of the reactor. Economic containment of tritium must be a major objective of fusion-reactor technology. (3) Long-lived radioisotopes will be produced in the structural components of fusion reactors. If niobium is employed as the structural material, disposal schemes similar to those currently proposed for fission-reactor wastes may be required. If vanadium is employed, recycling of the structural material appears possible. (4) Although afterheat removal will be quantitatively less of a problem with fusion power than with fission power, it must be considered in engineering design of fusion reactors.

Kulcinski, G. L. 1977. Materials Problems and Possible Solutions for Near Term Tokamak Fusion Reactors. Pp. 449-484. H. Knoepfel, ed. Proceedings of International School: Tokamak Reactors for Breakeven--A Critical Study of the Near Term Fusion Reactor Programme, Erice, Sicily, October, 1976. Oxford: Pergamon Press.

This paper examines the potential materials problems for the next round of Tokamak reactors (TFTR, JET, and T-20) which will be the first to burn tritium. The analysis is carried on to the three Experimental Power Reactor Designs in the U.S. proposed by ORNL, ANL, and General Atomics. A brief analysis of the problems for the Demonstration Power Reactors is also given. After an introduction to the mechanisms of radiation damage, the report concludes that there are no major neutron damage problems in the near term reactors. On the other hand, the higher wall loadings and temperatures in the EPR will cause problems for the stainless steel structures in designs which operate over 500°C. Excessive neutron leakage to superconducting magnets is also identified as a serious problem in some designs. The obvious conclusion from the DPR studies is that there is no known material that will last the lifetime of the reactor. The final section of the report addresses how one might go about testing materials to find those suitable for maximum lifetime in a fusion reactor. See also UWFDM-186, University of Wisconsin Report - 1976.

Kulcinski, G. L. 1976. Radiation Damage: The Second Most Serious Obstacle to Commercialization of Fusion Power. Pp. I-17 to I-72 in J. S. Watson and F. W. Wiffen, eds., Radiation Effects and Tritium Technology for Fusion Reactors, Volume I. Springfield, Va.: NTIS. CONF-750989.

The uniquesness of radiation damage associated with 14 MeV neutrons is discussed in relation to total displacements per atom (dpa), dpa rate, gas production rate, gas-to-dpa ratio, and solid transmutation products. Comparisons are made with both light water and fast reactors to illustrate that it will be very difficult to use the latter facilities to provide information about high power fusion reactors. The one exception to this statement pertains to 316 SS in thermal reactors where the proper helium gas generation rate is achieved. Examination of the displacement and transmutation damage with respect to the dimensional, mechanical and physical properties of metals reveals that there is very little if any pertinent experimental data available. Providing this data will require a massive and time consuming test program that could spread over a decade or more. Considering the shear number of radiation damage problems and their magnitude leads one to believe that their solution will be a major barrier to the commercialization of fusion power, second only to those problems associated with plasma physics.

Kulcinski, G. L., and R. W. Conn. 1975. A Possible Scenario for Commercial Tokamak Power Reactors Based on the Results of the UWMAK-I and II Conceptual Design Studies. Madison: University of Wisconsin. UWFDM-130.

After a detailed review of the features of two large scale Tokamak designs, a possible approach to a commercial power plant is outlined. It departed from the proposed USAEC plan at that time in that it moved the EPR's from 1985 to the 1990's and suggested that two facilities be inserted in the 1985 time frame. One device would be aimed at expanding the knowledge of plasma physics scaling laws by using a large hydrogen plasma with 1980 state-of-the-art fusion technology. The other device would use TFTR state-of-the-art plasma physics and will expand the fusion technology, especially in the area of irradiation of materials. The proposed scenario would aim at a Demonstration Power Plant in the 2000-2010 time fram.

Steiner, D. 1971. Neutron Irradiation Effects and Tritium Inventories Associated with Alternate Fuel Cycles for Fusion Reactors. Nuclear Fusion 11:305.

In this note the D-T, D-D, and D-^{3}He fuel cycles are compared on the basis of neutron irradiation effects and tritium inventories. It is concluded that:

1. The effects of neutron-induced damage within the blanket structural material will be comparable for each cycle.
2. The requirements for remote maintenance, radioactive-waste management, and emergency cooling will be similar for each cycle.
3. Tritium containment and management will be required for each cycle.

Steiner, D. 1975. The Technological Requirements for Power by Fusion. Nuclear Science and Engineering 58:107-165.

In this paper the major technological requirements for fusion power, as implied by current conceptual designs of fusion power plants, are elucidated and assessed. As the point of departure, the four fusion reactor concepts that have been most thoroughly considered in these designs studies are described: they are the mirror, the theta-pinch, the Tokamak, and the laser-pellet concepts. The required technology is discussed relative to three principal areas of concern: (a) the power balance, that is, the unique power-handling requirements associated with the production of electrical power by fusion; (b) reactor design, focusing primarily on the requirements imposed by a tritium-based fuel cycle, thermal-hydraulic considerations, and magnet systems; and (c) materials considerations, including surface erosion, radiation effects, materials compatibility, and neutron-induced activation. The major conclusions of the paper are summarized in a final section where it is noted that research and development programs have been initiated to satisfy the technological requirements associated with the realization of commercial fusion power.

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Electric Power Research Institute. October 1976. Tritium Inventory Considerations in Fusion Reactors - Topical Report. Palo Alto: Electric Power Research Institute. EPRI ER-278.

General Atomic Company. December 1976. Experimental Fusion Power Reactor Conceptual Design Study - Final Report. Palo Alto: Electric Power Research Institute. EPRI ER-289.

Los Alamos Scientific Laboratory. September 1976. Conceptual Engineering Design of a 1-GJ Fast Discharging Homopolar Machine for the Reference Theta-Pinch Fusion Reactor. Palo Alto: Electric Power Research Institute. EPRI ER-246.

Mathematical Sciences Northwest, Inc. February 1976. A Feasibility Study of a Linear Laser Heated Solenoid Fusion Reactor. Palo Alto: Electric Power Research Institute. EPRI ER-171.

Miley, George. 1976. Fusion Energy Conversion. La Grange Park, Illinois: American Nuclear Society.

For a discussion of alternate fuel cycles see:

McNally, J. Rand, Jr. April 1972. Prospects for Alternate Fusion Fuel Cycles at High Temperatures. Oak Ridge: Oak Ridge National Laboratory. ORNL TM-3783.

For a discussion of alternate end uses for fusion see:

Electric Power Research Institute. March 1977. The EPRI Asilomar Papers: On the Possibility of Advanced Fuel Fusion Reactors, Fusion-Fission Hybrid Breeders, Small Fusion Power Reactors. Palo Alto: Electric Power Research Institute. EPRI SR/ER-378.

Fusion Systems Corporation. September 1976. Enhanced Energy Utilization from a Controlled Thermonuclear Fusion Reactor. Palo Alto: Electric Power Research Institute. EPRI ER-248.

Miley, G. (ed.) September 1975. Conference Proceedings: Effects of Cyclotron Emission of the Power Balance in Fusion Systems. Palo Alto: Electric Power Research Institute. EPRI SR-16.

Appendix

FUSION-FISSION HYBRID AND ACCELERATOR BREEDING CONCEPTS

Introduction

The neutrons emitted in the more plausible fusion reactions (D-T and D-D) can be used to cause fissions and to create fertile isotopes (U^{233} or P^{239}) in a blanket surrounding the fusion device. The fissile material can in turn be fissioned *in situ* or utilized to fuel nonbreeder fission reactors. The result is in effect to multiply the energy release per fusion reaction by as much as two orders of magnitude. It has been argued that, since it may always be relatively expensive to produce fusion reactions, there will be a permanent and strong economic incentive to add fission blankets to fusion power plants for the purpose of increasing the energy output.

In recent years the rationale for fusion-fission systems has been expanded by the idea that since fusion systems with low energy gain appear closer at hand from the scientific viewpoint than high-gain systems, the fusion-fission concept can provide the added system energy gain that might justify the early commercial introduction of fusion devices. A counterclaim is that the pacing problems of fusion are of an engineering rather than a scientific nature so that the added engineering complexity of the fusion-fission devices would delay rather than hasten the commercial advent of fusion. Specifically, a fundamental obstacle to the commercialization of fusion reactors is the well-known difficulty of achieving acceptable capacity factors in complex plants which are also radioactive. The addition of a highly radioactive fission breeding blanket of necessarily inconvenient geometry thus appears to be diametrically opposed to the requirements for early commercialization. While the coupling of the breeding blanket with the high-technology fusion device is intrinsically less strong in the case of laser fusion than in the case of magnetic fusion, it is not entirely clear that the necessary isolation of the blanket is plausible even in the former case.

The accelerator breeding concept is similar to fusion-fission concepts in that fissile isotopes produced in the target by the accelerator beam can in principle make possible a system energy gain significantly above unity. More specifically, a beam of energetic protons, deuterons, or tritons (H^3) in the 500- to 1000-MeV range, when made to impinge on a target of suitable composition and structure, can release a substantial number of neutrons by spallation and fast-fission processes. Thus it is believed that one or more fissile atoms can be produced for every 20 MeV of beam energy. The fissile atoms can then be burned *in situ* or used to fuel a nonbreeding fission reactor, thereby resulting in an ultimate release in the range of 100 to 1000 MeV per 20 MeV of beam energy. It hence appears plausible that enough energy gain can be achieved to more than make up for the 30 to 40 percent efficiency of converting heat to electricity and the 50 percent efficiency of converting bus-bar electricity to accelerator beam power.

In both fusion-fission hybrids and accelerator breeders, the ratio of *in situ* burning of fissile material to burning in separate fission reactors is an adjustable system design parameter. By increasing the neutron multiplication of the blanket or target, and by increasing the residence time of the blanket or target fertile elements, it is possible to increase both the total generation of fissile material and the fraction that is burned *in situ*. Thus in one limit the fusion-fission hybrid could be considered solely as a source of fissile material with zero, or even negative, production of electrical power. In the opposite limit, power production could be the primary objective with no transfer of fissile material to fission reactors.

Another important class of system design parameters for fusion-fission hybrids and accelerator breeders relates to the neutron economy of the part of the system in which the fissile material is burned. As is familiar from the analysis of near-breeder fission reactor concepts, the ratio of total power generated to fissile material input can be varied over a large range. (This ratio becomes infinite when the breeding ratio reaches unity.) In general, increasing the efficiency of fissile material utilization entails penalties such as greater engineering complexity, reduced power density, or more frequent reprocessing. If the fissile material can be obtained at low cost (e.g., from inexpensive natural uranium) the economic justification for accepting such penalties is marginal. On the other hand, since fusion-fission hybrids and accelerator breeders are unlikely to produce fissile material at low cost, it would appear consistent to assume that if and when such devices are commercially deployed, the economic incentive for improved neutron economy will be considerably greater than it is at present.

Design Concepts

Extensive parameter studies based on preliminary conceptual designs have been made for both fusion-fission and accelerator breeders. The former studies suffer from the lack of many crucial design parameters, to be determined from plasma physics and materials research programs. In addition, none of the conceptual designs have been exposed to critical evaluation by industrial organization experienced in the actual construction of fission reactors. The fission blanket concepts for fusion-fission hybrids are often based on concepts that have not yet been successfully commercialized in fission reactors (e.g., liquid metal cooling, fused salts, high-temperature gas cooling, etc.). While such concepts may ultimately prove successful in the nuclear industry, their advanced nature would appear to preclude the early deployment of fusion-fission hybrids. Furthermore the philosophical advantages of fusion-fission hybrids over fission breeders or near breeders do not appear crucial in a practical sense so that the advances in nuclear technology required for the commercialization of fusion-fission hybrids would be likely to benefit competing pure fission concepts to a comparable extent. (For example, the problems in commercialization of the light water breeder reactor relate to the need for inconveniently large pressure vessels, internal structural complexity, and poor power distributions. Such problems would tend to be exacerbated rather than relieved in fusion-fission hybrids.)

In the case of accelerator breeders, the accelerator concepts are relatively firm because existing linear accelerators have already been operated in the pulsed mode at the necessary peak beam power (several hundred megawatts). Though the extension to steady-state operation is believed not to introduce fundamentally new physics problems, the necessity of raising the average power level by orders of magnitude introduces severe problems associated with heat removal, avoidance or control of beam spill, and maintenance. A better perspective on the difficulty of overcoming these problems will presumably be obtained from construction and operation of the proposed Fusion Materials Irradiation Test Facility (FMIT). (This device will consist of a high-power linear accelerator that provides a beam of energetic deuterons and a target of flowing liquid lithium in which the deuterons are converted to neutrons and protons by nuclear stripping reactions. The neutrons will then be utilized to irradiate test specimens.)

The target problem for accelerator breeding is generally recognized as being of far greater engineering difficulty than the accelerator itself. A composite structure consisting of a liquid metal primary target (presumably located within the vacuum chamber) and one or more secondary targets may be necessary. The total heat release in the accelerator breeder target would be comparable to that in a medium-sized fission reactor. Although control rod systems would not be required, other characteristics (e.g., peak heat flux, nonuniformity of heat deposition, radiation damage of materials, etc., etc.) could be far more severe than for typical fission reactors. Thus the time required for developing and commercially deploying accelerator breeders is likely to be comparable to that required for developing and deploying a new type of fission reactor. Furthermore, as in the case with new reactor types, ultimate commercial success is strongly dependent on the capacity factor actually achieved in the field. This aspect of performance is difficult to predict in advance. There is, however, considerable evidence from both fossil and fission power plant experience that large size and technical complexity tend to markedly lower capacity factor.

The number of different conceptual designs of fusion-fission hybrid and accelerator breeder concepts that have been proposed and studied at least superficially is large indeed. In the fusion-fission hybrid case each fusion concept (e.g., laser fusion, electron and ion beam fusion, Tokamaks, magnetic mirrors, theta pinches, laser heated solenoids, etc., etc.) is a conceivable candidate. In addition, different investigators have selected their own preferred fission reactor concept as the basis for blanket design. Accelerator breeder concepts are generally based on the use of linear accelerators, but differnt ions (e.g., protons, deuterons, or tritons) can be utilized. The choice of the nucleus to be accelerated makes some difference in accelerator design and also in the structure and composition of the target. In the case of deuterons or tritons, a primary target consisting of liquid lithium can be used to strip off the neutrons; these neutrons then impinge on a secondary target (e.g. uranium) that maximizes neutron multiplication by spallation, evaporation, and fast-fission nuclear reactions. The lower energy neutrons resulting from this cascade then enter a tertiary target that incorporates a lattice structure for maximizing the production of fissile material or fission energy release. $Thorium^{232}$ or U^{238} can be utilized as the fertile material, and some designs incorporate both materials. (The U^{238} is advantageous for increasing neutron multiplication by fast neutron fission processes in parts of the target where the neutron energy is high. Thus it might be incorporated in the design even if U^{233} rather than Pu^{239} is the desired fissile product.)

It is generally felt that the required accelerator design would be somewhat simplified if protons, rather than heavier ions, were used. This would entail a significant reduction in fissile yield per unit of beam energy unless a uranium primary target could be used.* A liquid lead primary target within the accelerator vacuum system is a more conservative alternative to the uranium target concepts that have been considered.

The linear accelerators proposed in accelerator breeder concepts are typically designed for a beam power of several hundred megawatts and beam energy of 500 to 1000 MeV. The total length is several thousand feet. It is hoped that much of the accelerator structure will have low enough radioactivity to allow hands-on maintenance. Automatic beam control would rapidly turn off the beam to avoid physical damage or activation of the structure if beam spill occurs.

A plant based on an accelerator with 300 megawatts of average beam power might have a fissile material production rate of about 3000 grams per day. This would be sufficient to fuel three or four light water reactors of current design, assuming that fuel reprocessing is eventually allowed. Perhaps as many as a dozen advanced near-breeder reactors could be fueled by the same plant. On the other hand, if such reactors turn out to be commercially feasible, the exsiting sources of natural uranium would presumably serve the nuclear industry well into the 21st century without the need for accelerator breeders. (Fuel reprocessing would, however, be required to justify the use of advanced reactors.)

*A solid primary target necessitates a metal window to separate the target coolant from the accelerator vacuum. It is not assured that a window with adequate strength and radiation resistance is feasible.

Some thought has been given to fuel cycles that do not require reprocessing. For example fertile material elements might be first loaded into the target of an accelerator breeder and then, after the concentration of fissile material has risen to a few percent, transferred to a fission reactor for use in the throwaway cycle. Whether or not such concepts could be licensed by NRC is problematical. Also, the elmination of reprocessing tends to reduce the amount of energy that can be obtained from the fissile material.

Extensive details of the numerous fusion-fission hybrid and accelerator breeder concepts are available from the documents listed in the References.

Economic Considerations

For reasons indicated previously, the economics of fusion-fission hybrids and accelerator breeders cannot be reliably projected at the present time. Over and above the major remaining technological and design uncertainties, the capacity factor of the ultimate plants has crucial leverage on the economics; this factor will unfortunately not be known until one or more generations of commercial plants have been deployed.

In the interest of obtaining a feel for the order of magnitude of different components in the cost of fusion-fission hybrids and accelerator breeders, Table 1 has been prepared under the set of arbitrary assumptions listed in the accompanying notes. The estimates of MeV of energy associated with the net production of one atom of fissile material are also subject to substantial uncertainty. While the cost estimates are of little quantitative significance, they are at least consistent with the general belief that accelerator breeders might produce fissile material in the cost range of $100 to $300 per gram; fusion-fission hybrids, if they were technologically feasible, could conceivably produce fissile material at about half this cost.

The "low exposure" cases in Table 1 refer to situations in which no fissile material is deliberately added to the blanket or target. In the "high exposure" cases, enough fissile material is allowed to accumulate in the fertile elements before unloading to double the net production of fissile material. This has the effect of reducing the contribution of the accelerator or fusion device to the fissile product cost, but increasing the contribution of the target or blanket cost. In the absence of an optimized lattice structure the fertile material exposure would be in the range of 40,000 MW-days per ton, corresponding to a peak fissile content of about 8 percent, or an average of 4 percent.

One can of course go much further in the direction of increasing blanket or target multiplication, in which case one begins to approach the breeder reactor regime (i.e., the total heat generation per atom of fissile product rises from the 50 to 100 MeV range toward the 1000 to 2000 MeV range characteristic of the LMFBR). Plant revenue from sale of electricity then greatly exceeds the revenue from sale of fissile material. The credit for generation of electricity exceeds the fixed and opearating cost associated with the blanket or target, this fact reflects the assumed equivalence of the blanket or target to a fission reactor with no yellowcake or separative work costs.

Since about 2 grams of fissile material (uranium^{235}) can be obtained by isotopic separation froma pound of yellowcake, a non-power-producing fusion-fission hybrid or accelerator breeder will be economically justified only if yellowcake cost rises to several hundred dollars per pound. Power-producing fusion-fission hybrids would have to compete with breeder reactors. This seems unlikely because of the great engineering complexity of the former concepts.

Proliferation Considerations

Present concerns over nuclear arms proliferation has highlighted the question of whether or not fusion-fission hybrids or accelerator breeders offer any advantages with respect to minimizing the risk of such proliferation. The picture is clouded because of strenuous differences of opinion about the scenarios deemed to constitute the greatest risk one or two decades from now. In addition key information (particularly with respect to the proliferation of fusion weapons) will probably remain classified for the foreseeable future, so that the basis for official policy will not be subject to independent review.

In the first instance, fusion-fission hybrids and accelerator breeders are copious neutron producers and thus in themselves provide avenues for proliferation. For example, if the technologies of fusion-fission devices or high-power linear accelerators turn out to be sufficiently tractable for commercial use, then coupling such a device to a slightly subcritical lattic of natural uranium and light water would presumably provide a tempting avenue for generating fissile isotopes and thermonuclear materials. (The latter would already be available in conjunction with the fusion device fuel cycles).

On the favorable side, it is argued that fusion-fission hybrids or accelerator breeders would provide flexibility in selecting fuel cycles alleged to minimize the proliferation temptation because of the composition of the fuel or because of reduced need for fuel reprocessing. But even granting the controversial claim that commercial nuclear reactors or reprocessing facilities would necessarily constitute uniquely tempting avenues to proliferation, it is by no means clear that fission reactor systems do not possess the flexibility needed to deploy whatever fuel cycle is ultimately judged to constitute a minimum proliferation threat.

The Electronuclear Fuel and Power Producer (EFPP), a particular accelerator-driven system, is now being evaluated by ERDA as a means of eliminating the need for reprocessing. In one mode of operation the accelerator would drive a subcritical fission lattice having a neutron multiplication of about ten. The aim is to achieve a system power recirculation factor well under 50 percent and a fertile atom burnup of 10 to 15 percent. A fuel element exposure in excess of 100,000 megawatt days thermal per metric ton (MWD(th)/MT) would be required (10 to 12 year residence time) as compared to the 30,000 MWD(th)/MT objective in commercial light water reactors. Assuming that this fuel element performance can be reliably achieved, a throwaway fuel cycle for fast breeders might also be possible. It would, however, be necessary to provide fissile material from another source to make up the initial fuel loading. Fusion-fission hybrids or accelerator breeders would thus obviate the need to acquire the initial fissile material by enriching natural uranium or by purchasing plutonium from an off-shore supplier.

TABLE 1 Conjectural Cost of Fissile Material Production by Fusion Hybrid and Spallation Schemes

	Break-Even Fusion		Spallation Breeding	
	Low Exposure Blanket	High Exposure Blanket	Low Exposure Target	High Exposure Target
Electrical Input:				
MeV/net fissile atom	7	3.5	40	20
$/net gram of fissile product	$ 31.20	$ 15.60	$178.00	$ 89.00
Plant Cost Allocated to Blanket or Target (Based on heat removal requirement):				
MeV of heat/net fissile atom	50	100	113	160
$net gram of fissile product	$ 43.30	$ 86.70	$ 98.00	$138.80
Plant Cost Allocated to Fusion Device or Accelerator and RF Power Supply:				
Capital cost of equipment (3000 g/day plant)	$300 Million	$150 Million	$300 Million	$150 Million
Fixed and operating costs of above per net gram of fissile product	$ 91.30	$ 45.60	$ 91.30	$ 45.60
Gross Cost/Net Gram of Fissile Product	$165.80	$147.90	$367.30	$273.40
Credit for Electrical Power Generation	($ 53.30)	($106.50)	($120.30)	($170.40)
Net Cost/gram of Fissile Product	$112.50	$ 41.40	$247.00	$103.00

Note: The following arbitrary assumptions were made in the calculations for Table 1.

Plant Capacity Factor: 60 percent
Fixed Charge Rate: 15 percent per annum
Operating Cost (including fabrication reprocessing, etc.): 5 percent of plant costs per annum
Total Plant Cost Allocated to the blanket or target (including heat removal): \$200/kW thermal
Net Thermal Efficiency of Electrical Generation: 33 percent
Total Plant Cost Allocated to Electrical Power Generation: \$300/kW electric
Capital Cost of 300 Megawatt RF Power Supply and Accelerator (3000 gms/day production with low exposure blanket): $\$300 \times 10^6$
Capital Cost of Fusion Neutron Source (3000 gms/day production with low exposure blanket): $\$300 \times 10^6$
Cost of Electricity to Operate Accelerator or fusion device (consistent with previous assumptions): 39 mills/kWHe
Net Credit for Addition of Conversion equipment to produce electricity for plant use or sale: 28 mills/kWHe

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