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On the Trends in Nuclear Fusion Research

Dr. Bo Lehnert

An Assessment of Laser-Driven Fusion

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A More Optimistic View of Laser Fusion

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ERRATUM

A typographical error in page 39 in the Bostick article in the March 1977 issue of the Journal dropped a zero from a diameter measurement, changing 100 to 10. The sentence should read: "The central channel of this solenoid becomes the plasma nodule with diameter $\sim 100\mu$m."

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Abstract

The present state and future possibilities of nuclear fusion research are reviewed. The complex of included problems is described, as well as various approaches for their solution, based on both magnetic and nonmagnetic confinement schemes.

At the present stage considerable progress has been made in basic plasma physics and fusion reactor technology, bringing theory and experiments closer together. However, none of the approaches and schemes to date will lead for certain to the final solution of the fusion reactor. To concentrate the world's resources on a restricted number of research lines thus is irreconcilable with the present state of knowledge. For fusion research to reach its goal within a reasonable time, a crash program *with emphasis on basic research and new ideas is needed.*

ABOUT THE AUTHOR

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Introduction

To meet the world's future energy demands, we must develop new energy sources in addition to the present conventional ones. In this connection a well-balanced compromise has to be found between solutions that secure energy demands and those that satisfy requirements on safety and environment. Provided that the fusion reactor can be realized with full success *(1-15)*, it is likely to meet such demands to a great extent *(3,6,10,13,16)*.

The *DD-reaction* in pure deuterium represents a practically inexhaustible energy source. The more easily achievable DT-reaction in a mixture of deuterium and tritium represents a fuel resource of at least the same order as that of the fission breeder, and thus is much larger than estimated coal reserves. In addition, fusion reactor systems may open up new possibilities of direct energy conversion and industrial production processes and may be combined with fission reactor systems in a type of hybrid reactor.

Fusion reactors once developed should have advantages in respect to safety and environmental problems: The fuel itself does not produce radioactive "ashes." There is certainly a large flux of energetic neutrons emitted, but a certain freedom exists in the choice of wall material which affects the levels of induced radioactivity. Plutonium does not necessarily have to be bred for the reactor to work as an energy source. Further, a runaway nuclear reaction is impossible in principle, and the reactor can be shut down almost immediately, yielding an after-heat that seems not to require emergency cooling. The biological hazards of the inventory and the "waste" from replaced construction materials are expected to become much smaller than in a fission breeder, especially if vanadium and its alloys are chosen as wall material. Structures based on aluminum and silicon carbide are also of interest in this connection. Finally, it may become possible to use the neutron flux from a fusion reactor for radioactive waste burning.

Given the present limited resources of fusion research, the fission breeder is likely to come into practical operation before the fusion reactor has been realized on full scale. However, this is not a defensible argument for attempts to accelerate fusion research and reactor development by taking shortcuts through basic research and by narrowing the lines of approach. The just-mentioned potentialities of fusion energy are important enough to justify intensified research along broad lines, regardless of the existence of the fission breeder and other possible energy sources.

A Complex of Problems

Keeping in mind the potentialities described above, we shall review the scientific and technical problems of the fusion reactor in the following section. Research and development in this field include a whole complex of problems, ranging all the way from basic scientific concepts to technical and economic questions. These problems can be subdivided broadly into three groups as shown in Table 1:

TABLE 1

THE COMPLEX OF PROBLEMS IN FUSION RESEARCH

PROBLEM GROUPS	INCLUDED PROBLEMS
Problems of plasma physics concerning basic research	Plasma balance and confinement Plasma-neutral gas interaction Instabilities and waves Heating mechanisms Radiation losses Impurities Plasma diagnostics
Problems of reactor technology concerning research and development of laboratory "zero-power" reactors	Magnetic coil systems Systems for plasma generation and startup Systems for plasma heating Refueling and removal of ashes Plasma impurity removal Wall damage, including sputtering, blistering, neutron interaction Cooling systems Power extraction Blankets and radiation shields Tritium breeding Waste handling
Problems of full-scale operation concerning practical use of fusion energy	Technical operation efficiency under full-scale conditions Repair and replacement of construction details Safety and environmental properties Energy price, economical feasibility

TABLE 2 MAGNETIC BOTTLE CONFINEMENT SCHEMES

MAGNETIC					
Magnetic Bottles					
Closed bottles				Open bottles	
Axisymmetric		Nonaxisymmetric		Axisymmetric	
Steady	Pulsed	Steady	Pulsed	Steady	Pulsed
Straight z-pinch TC*	Tokamak TC	Stellarator T	Toroidal theta-pinch aux. field TC	Mirror P	Straight theta pinch PC
Multipoles PI	Toroidal theta-pinch TC	Torsatron T	Pulsed bumpy torus TC	Consecutive Mirrors P	Mirror Compression P
Astron P	Toroidal screw-pinch PTC	Helical heliotron T		Cusp P	
		Bumpy torus		Straight picket fence P	
		Poloidal heliotron PTI		Multigap trap P	

Axisymmetric or nearly axisymmetric magnetic bottles can be based largely on a poloidal field running through planes containing the symmetry axis, on a toroidal field running in circles around the same axis, or on a combination of both field types. The poloidal field becomes mainly responsible for the confinement and for the balance of the pressure forces, whereas the toroidal component merely serves the purpose of an auxiliary field. At a given plasma pressure, therefore, schemes with a dominating toroidal component become subject to the drawback of a large required total field strength.

In respect to neutral gas penetration into a plasma body of the characteristic dimension L_c, there is a critical density, $n_c = 1/\sigma_{cm} L_c$ dividing the range of the ion density, n, into two main regimes *(21)*. For $n \lesssim n_c$ the plasma becomes permeable to penetrating, fast neutral particles; and for $n \gg n_c$ it is impermeable, in the sense that a fully ionized core can be separated from a surrounding wall-near neutral gas blanket by a thin, partially ionized boundary layer. For hydrogen isotopes, $\sigma_{cm} \simeq 3 \times 10^{-19}$ m^2.

Most full-scale quasi-steady or slowly pulsed fusion reactor models discussed up to date operate far inside the impermeable

MAGNETIC		NONMAGNETIC		
Magnetic Bottles		Guided systems	Quasi-steady systems	Inertia systems
Open bottles				
Nonaxisymmetric				
Steady	Pulsed			
Mirror with Ioffe bars P	Polytron	Electrostatic wells	Electrostatic wells	Plasma focus
Toroidal picket fence		Beam systems	Beam systems	Laser compression
Helical multipoles PTI			Microwave confinement	Electron beam compression
Baseball				

*P and T denote poloidal and toroidal *main* field components; C indicates that a plasma current makes an important contribution to the magnetic field; and I indicates internal conductors.

density range. Unless a strong gas pumping is imposed in the wall-near regions and a corresponding flux of matter is injected into the plasma body, a neutral gas blanket should be established near the walls in these models, because of the plasma-wall balance. Such systems differ considerably from most of the permeable plasmas in toroidal and other discharges studied so far in the laboratory, in respect to equilibrium and stability as well as to impurity release and removal.

At this stage several hundred elementary instability modes have been investigated. None of these will become a dangerous threat to the plasma energy balance. In magnetic bottles the magnetohydrodynamic modes are some of the most important ones. Suppression of the violent, flute-type instability becomes a necessary condition which can be satisfied by means of minimum-B or minimum-average-B geometries, and in some cases by other methods as well. Other important instabilities are the ballooning and kink modes, the latter including the Kruskal-Shafronov instability.

In general, it should be noted, not only the field geometry but also the joint effects of the plasma distribution in phase space and the boundary conditions affect the stability properties. Thus, in certain cases, additional restrictions of plasma physics and technology on a confinement scheme may lead even to situations where minimum-average-B stabilization becomes inapplicable, but where other stabilization methods in various types of field structures are still available. One such example is the partially ionized cool and dense diamagnetic boundary layer of an impermeable plasma. Here the combined effects of ion-ion and neutral gas viscosity, pressure, and resistivity can produce strong stabilizing mechanisms, at the same time that minimum-average-B stabilization may become inefficient because of long magnetic connection lengths and a large plasma resistivity *(22)*.

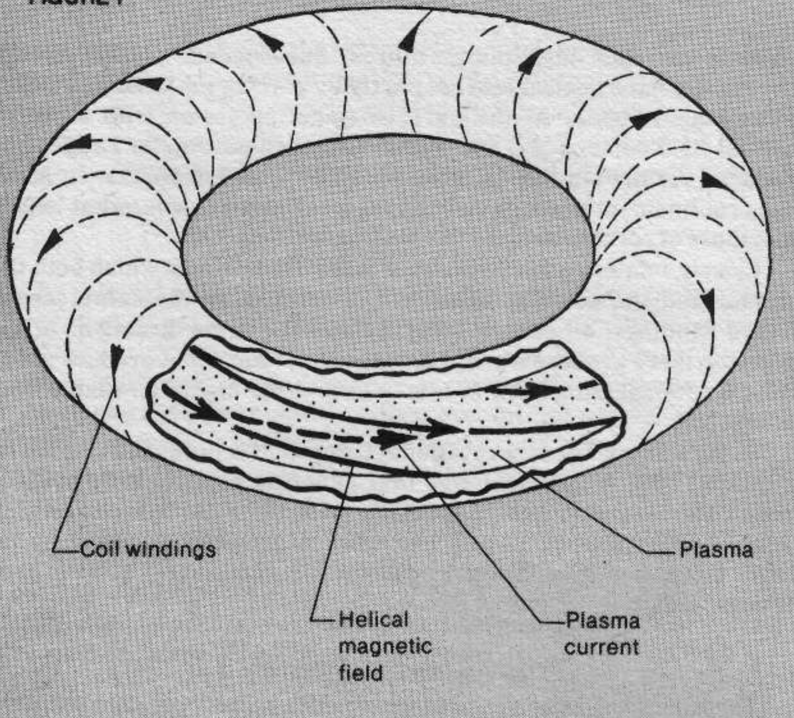
TOKAMAKS

The Tokamak devices are based on a strong toroidal vacuum field, B_t, upon which is superimposed a poloidal component, B_p, originating from an induced toroidal plasma current, J_t. Since $B_p \ll B_t$ because of the Kruskal-Shafronov limit, the field lines have a slightly screw-shaped form as shown in Figure 1. With devices operating in the Soviet Union, the United States, France, England, West Germany, Italy, and Japan, considerable progress has been achieved in attempts to create stable and hot laboratory plasmas with long containment times *(5,8,23-28)* and with superimposed high-frequency heating and beam injection *(24)*. Especially with device T-10 at the Kurchatov Institute in Moscow, temperatures of the order of 10^7 °K have been reached at densities of about $n = 6 \times 10^{18}$ m^{-3} and $n\tau \simeq 3 \times 10^{18}$ s/m^3 using toroidal magnetic field strengths up to about $B_\tau/3.5$ tesla and induced toroidal discharge currents, J_t, up to 400 kA *(23)*. The Princeton group has further succeeded in increasing the plasma temperature and density by about a factor of 3.0 in a toroidal adiabatic compression experiment where the major radius has been decreased by a factor of 2.3 during the compression *(27)*. Finally, encouraging results have been achieved with the Alcator device at MIT where pulses of neutral gas are introduced into the discharge chamber during a shot *(28)*.

Tokamaks are usually subject to the limitations of the Kruskal-Shafranov instability as well as to a magnetic surface splitting limit that sets an additional bound to the poloidal beta value. Further, it is not yet possible for certain to operate Tokamak experiments far inside the impermeable density regime that is of main interest to fusion reactors. Strong so-called disruptive instabilities and temperature pinching are usually observed within the present parameter ranges of operation. These are due to an $m = 1$ internal resistive kink mode, and lead to a breakup of the magnetic surfaces *(23, 25, 26, 28)*.

Several interesting approaches are now being considered to overcome at least part of these difficulties. A number of very large devices are planned — T-20 in the Soviet Union, TCT in the United States, JET in Western Europe, and JT-60 in Japan — in order to obtain a better understanding of the scaling laws and the plasma loss mechanisms as

FIGURE 1



Closed magnetic bottle of Tokamak type.

well as to reach higher discharge currents and temperatures. The possibilities of dynamic and feedback stabilization are also under consideration. Further research has started on various types of strongly noncircular plasma cross-sections, such as in the Finger-Ring Tokamak and the Doublet devices, with the purpose of increasing the ratio between the plasma pressure and the magnetic pressure as well as increasing the available range of toroidal currents within the Kruskal-Shafranov limit. Finally, there are ongoing experiments conducted at Jutphaas with the Ringboog device (*29*), and at MIT with Alcator (*28*), aiming at plasma studies far inside the impermeable density range. Investigations on plasma-neutral gas and plasma-wall interaction are planned also for the Textor device in Jülich and on axisymmetric divertor operation with the Asdex device in Garching.

STELLARATORS AND RELATED DEVICES

In Stellarators the rotational transform and the plasma equilibrium are provided entirely by a vacuum field generated by external helical windings. In addition to being based mainly on a vacuum field, Stellarators differ from axisymmetrically operated Tokamaks in that the field pattern contains asymmetric components.

Earlier Stellarator experiments at rather low densities resulted in Bohm-like losses that were difficult to interpret and that prevented attempts to reach high temperatures. Subsequently, however, improvements have been achieved, partly by placing the windings closer to the plasma body. At the current stage, operation with stronger poloidal field components and within extended parameter ranges has produced results similar to those obtained with Tokamaks. In some cases confinement times have been reported that are somewhat longer than those of corresponding Tokamak operations *(30)*.

Closely related to the Stellarator is the Torsatron in which both the toroidal and the poloidal fields are generated by the same screw-shaped windings, all carrying currents in the same direction. In the Heliotron there are screw-shaped internal windings that produce shear and a rotational transform in a way to provide higher available beta values (*31*).

In the Bumpy Torus, a rotational transform is produced instead by azimuthal field inhomogeneities that generate particle drift motions around the magnetic axis. Especially with the ELMO device at Oak Ridge, high-frequency power has been efficiently absorbed under stable conditions in a toroidal configuration consisting of 24 mirror sections (*32*).

THETA PINCH SYSTEMS

Theta pinches consist of rather rapidly pulsed systems with the magnetic field directed mainly along the axis of the corresponding discharge tube. In the toroidal case additional windings are placed on the discharge tube, when the plasma column is in a compressed state, in order to provide an equilibrium using helical and bumpy fields.

Beta values as high as 0.8 and temperatures even exceeding $10^{7\circ}$ K have been reported by the Los Alamos group for a toroidal sector experiment operated at densities of about 3×10^{22} m^{-3} with a hot plasma lasting for about 8 microseconds. The confinement time was observed to increase with the length of the sector. After closing the device into a torus, and using a helical discharge tube, shock-heating, and feedback stabilization, the confinement time has been increased to about 25 microseconds (*33*). Part of these results was anticipated in Garching with the device Isarl(*34*). There the $m=1$ and $m=2$ modes have been suppressed by placing the external coils close to the plasma body and by using a screw-shaped discharge chamber. In Nagoya, successful theta pinch operation has been realized under stable conditions during 30 microseconds with densities $n\simeq10^{21}$ m^{-3} at temperatures $T\simeq10^{7}$ °K, by placing poloidal guarding rings around the plasma column (*35*).

Studies of straight pinches at Culham Laboratory and other places have obtained clear and important proofs of classical diffusion of a hot plasma across a magnetic field.

TOROIDAL SCREW PINCHES

The toroidal screw pinch is a combination of a z- and theta-pinch, related to the Tokamaks but operated at high beta values and higher compression ratios. Recently beta values up to 16 percent have been obtained under stable conditions for current decay times up to 700 microseconds (*36*). The closely related Belt-Pinch plasma, with an elongated cross-section, becomes grossly stable for about 50 microseconds, as recent experiments in Garching and elsewhere have shown.

RING SYSTEMS

The poloidal field, the essential component providing confinement, is strengthened considerably in the vacuum-field case by introducing electric-ring currents inside the confinement volume. This can be done by means of internal conductors such as those in the multipole systems (of which the Spherator and Levitron are special cases). Minimum-average-B properties can be obtained by certain arrangements of the internal rings, sometimes by combination with a superimposed toroidal field. Another method is that of the Astron where an "E-layer" of relativistic electrons serves the purpose of an internal ring current.

Internal conductor systems are still in an early experimental stage. The confinement properties of the Tokamaks and Stellarators have been simulated in part under well-defined axisymmetric conditions, and in some experiments have led to agreement with neoclassical theory. In the Spherator, containment times on the order of a second have been measured in dilute plasmas at temperatures below 3×10^4 °K consistent with classical theory. At higher temperatures it is also possible that there is agreement with the latter theory, or, there are instabilities; but neutral gas interaction so far has complicated the experimental interpretation (*21*).

In an approach to a fusion device, the internal ring (or rings) has to become accessible by material supports. In cases where relevant support parameter values have been chosen, laboratory experiments with magnetically shielded supports have been successful (*37*). However, the question of whether devices of this type will be feasible in terms of plasma physics and technology on full-reactor scale has not been settled at this stage.

MIRROR TYPE SYSTEMS

The end losses from a magnetic mirror system are reduced by the mirror force that traps particles within the loss cone. However, because of cumulative Coulomb collisions and equivalent scattering processes caused by microinstabilities, there are still serious end losses that threaten the use of mirror systems as fusion reactors. There have been several methods suggested to tackle this problem. First, the outflux of lost particles can be used as driving force in a direct energy-conversion system, thus decreasing the net energy losses (*13, 14*). Second, radio-frequency plugging of the mirror ends reduces the particle losses, at the same time that it introduces a power loss caused by the presence of the corresponding resonator system. Third, putting a large number of consecutive magnetic mirrors along a common axis has been suggested (*38*). By a proper choice of the ratio between the mirror separation distances and the mean free path, the axial diffusion loss out of such a system should become reduced. Fourth, in principle, the end losses can be diminished by using the centrifugal force of rotating plasmas, provided that the velocity limitation effect of such plasmas can be eliminated.

Special geometries and auxiliary fields have been developed for plasma stabilization in mirror systems. For example the so-called Ioffe bars and the Baseball (Yin-Yang) geometry are both minimum-B configurations.

Because of their simplicity and well-defined conditions, mirror systems in any case serve an important purpose in basic plasma physics. With pulsed magnetic field coils, adiabatically compressed plasmas, and neutral injection, the encouraging result of $T_i = 7 \times 10^7$ and $T_e = 8 \times 10^6$ °K at $n = 10^{20}$ m^{-3}, $n\tau = 10^{16}$ s/m^3 and beta values of 0.4, have been achieved recently with the 2XIIB device at Livermore (*39*).

CUSP-TYPE SYSTEMS

The simple cusp represents a magnetohydrodynamically stable minimum-B system. Its main problem is caused by the plasma leakage through the magnetic mirrors that are situated both at the ends and along the rim of its circumference. Part of these losses can be diminished by placing consecutive cusps on a common axis, as in the Picket Fence and Polytron configurations (*40*). In the latter, a toroidal discharge is superimposed, and as a result ions are accelerated because of their large Larmor radius, while electrons remain trapped within the cusps. Another method of loss reduction is based on the fact that the mirror ratio increases at high beta values. A scheme with some similarities to those just mentioned has been proposed by Lavrentev (*41*), who considered a type of plasma confinement in "multigap electromagnetic traps." With this scheme 1 keV ions have been confined for 5 ms at a density $n = 5 \times 10^{18}$ m^{-3} (*42*). Experiments on cusp-type systems have been conducted to date on modest scales, but further important results very well may be achieved with these configurations in the future.

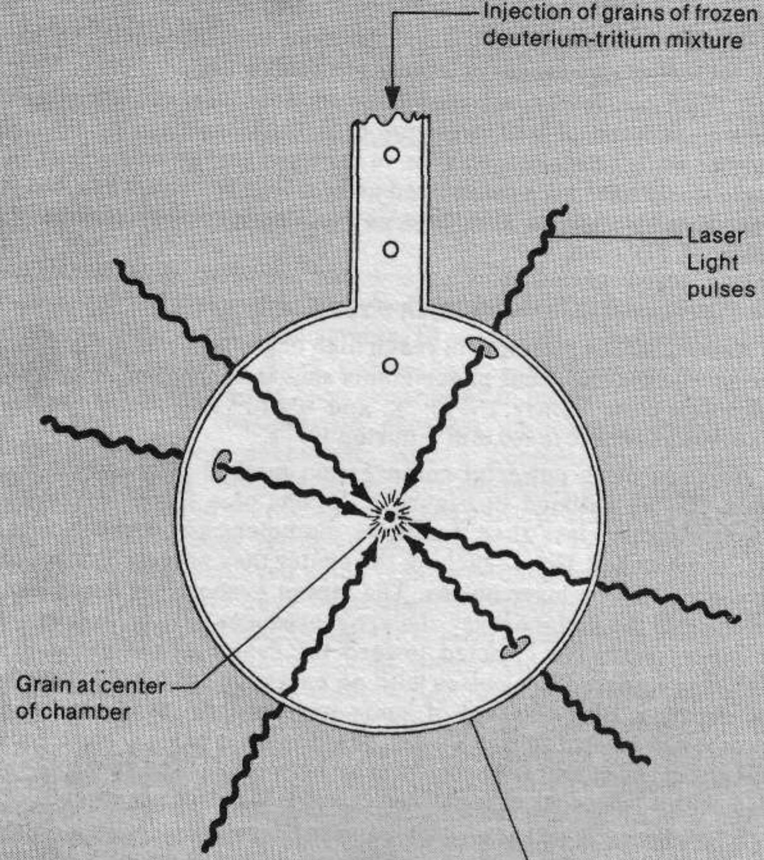
Magnetically Guided Systems

There are approaches in which the magnetic field does not serve the purpose of a bottle for thermal ions and electrons but still has the function of guiding the orbits of at least part of the charged particles.

One example is the strong electrostatic well of an electron cloud trapped in a toroidal or axial magnetic field. The ion Larmor radius in this case is comparable to, or larger than, the size of the cloud, with ions oscillating through the entire well and large mutual velocities within its central regions (*43*).

Another example is the manifold of systems of colliding particle beams in which a magnetic guiding field is used (*11*). The MIGMA

FIGURE 2



Inertia confinement by means of laser pulses.

system should be mentioned, in particular, where an ion beam is made to collide with itself in a magnetic mirror system, and is thus neutralized by a cloud of trapped electrons (*44*).

Nonmagnetic Schemes

In the absence of a magnetic field, confinement can be produced both in quasi-stationary systems and by inertia forces.

QUASI-STATIONARY SYSTEMS

Even in the absence of a magnetic field, under some conditions (when collisions are unimportant), it is possible to create electrostatic walls. In such cases at least one of the particle species has a distribution that is not fully Maxwellian (*11*).

Another possibility is that of colliding charged particle beams (*11*), including approaches of intense ion beams (*45*).

A third possibility is based on a balance between the plasma pressure and the photon pressure of a microwave field, with the chamber walls acting as a cavity. In terms of energy balance, such a system would have been considered unfeasible a few years ago, but the recent development of available cavity Q-values may change this picture (*7, 11*).

INERTIA SYSTEMS

Among earlier attempts to reach high degrees of compression in a plasma, the plasma focus experiments should be mentioned. In these experiments the values $T \simeq 10^7$ °K and $n \simeq 10^{31}$ m^{-3} were obtained within volumes of 10 to 100 mm^{-3} during 10^{-7} s.

An even more powerful compression method is based on laser pulses (*15*) as outlined in Figure 2. Grains of a frozen deuterium-tritium mixture, less than 1 mm in diameter, are dropped into a reaction chamber. Upon reaching the center they become subject to a crossfire of strong laser pulses. The strong evaporation and plasma formation of the surface layer of a grain produces a reaction force and a compression wave directed toward the center of the grain. It is planned to impose laser pulses with an energy of 10^4 to 10^6 Joules per shot during a time interval of some nanoseconds, thus reaching a compression ratio of 10^4 and $T = 10^8$ °K at the grain center. The thermonuclear energy output is expected to be between 10^7 and 10^8 Joules per pulse, with a frequency of repetition of about 10 pulses per second.

Laser fusion is still at an early stage of development and its future possibilities are difficult to survey. Recent experiments at Los Alamos have shown that the surface polishing effect of hot electrons suppresses the asymmetry of the grain implosions well enough to reach compression ratios of about 200 (*46*).

Nevertheless, there are many problems to solve: the efficient use of pulse energies that are several orders of magnitude larger than those available today; the nonlinear reflection properties and instability mechanisms in the plasma and within the grain; the effects of energetic charged particles on the optical system, of induced magnetic fields, and of thermonuclear reactions; the physical and technical problems of symmetry, focusing, nonlinear refraction of laser light in glass, and control of the course of events; as well as the economic requirement of operating the system during 10^{10} microexplosions.

The use of electron beams has been suggested as an alternative to laser compression. This method has the advantage that the required energies are available in present beam sources. At the same time, there are problems to solve, such as those concerning the focusing of the beams on the target and the necessary size and geometry of the grains. Large electron beam devices have been constructed and are now operating at various places, especially in the United States and the Soviet Union.

The Present State

Returning to Table 1, we summarize the present state of nuclear fusion research in the following section.

Problems of Plasma Physics

Considerable progress has been made among basic plasma physics problems in establishing the foundations of plasma theory and connecting theory with experiments. In particular, a great number of plasma instability and wave modes have been thoroughly investigated; Bohm-diffusion has been suppressed in many cases; nonlinear theory has made considerable progress; numerous stabilization methods have been developed; and a considerable number of promising heating methods based, for example, on imposed high-frequency fields, and neutral and charged particle beams, as well as on turbulence, have been tested in the laboratory. Further important results have been achieved using computer calculations; the plasma diagnostic techniques have been greatly extended; and several approaches to the fusion reactor appear to have a fair chance of success.

A number of important basic problems remain to be tackled: the energy-loss mechanisms in complicated field geometries and under nonlinear conditions of instability growth; the effects of plasma impurities in closed magnetic bottles; plasma-neutral gas interaction; and the radiation losses from a magnetized thermonuclear plasma. Before reaching the final goal we will also have to understand how to confine, stabilize, and heat a plasma in a way that avoids *all* undesirable loss mechanisms in one and the *same* confinement system, thus also satisfying *all* other technical requirements.

With special devices such as Tokamaks, Stellarators, theta pinches, magnetic mirrors, and some other systems, considerable progress also has been made toward high plasma temperatures and long confinement times under stable plasma conditions. Nevertheless, in every single experiment performed so far, there must be an increase of one or several orders of magnitude in one or the other of the parameter values T and $n\tau$ before the necessary conditions for a practical, operating fusion reactor can be realized. Thus, the general development of fusion physics and a search for new approaches at this stage should be considered as at least as important as the maximum experimental parameter data obtained using individual devices.

A considerable part of the international fusion research program is now devoted to large experiments with magnetic bottles that have a main toroidal-field component, as well as to a number of large laser-fusion experiments. It is outside the scope of the present review to discuss in detail all these investigations. Attention here will be given only to some features of the magnetic bottles just mentioned. When developed into full reactor scale, such systems face the following problems:

(1) So far only small total beta values have been reached in Tokamak and Stellarators. This leads to a number of drawbacks in terms of plasma physics and reactor technology. First, very large magnetic field energies become necessary at the plasma pressures prevailing in a full-scale system. Second, the existence of a relatively weak poloidal field component results in slightly screw-shaped field lines that in turn lead to long magnetic connection lengths between "bad" and "good" regions. The rotational transform and the magnetic surfaces become sensitive to small disturbances in a way that may affect the symmetry of the magnetic field and the plasma geometry. This makes the confinement vulnerable to a number of instabilities. Third, the large required total magnetic field strength becomes unfavorable with respect to cyclotron radiation losses. Fourth, large magnetomechanical stresses arise on the coil system, at the same time that superconducting windings become necessary for the Ohmic coil losses to be reduced to an acceptably low level.

(2) The complicated diffusion processes and other loss mechanisms which involve "banana" effects and trapped particles are not fully understood at this stage. The losses in Tokamaks and Stellarators are larger than those so far predicted by the theory of a stable and steady state, but they are consistent with anomalous transport processes caused by instabilities (*47, 48*). In addition, violent disruptive instabilities occur under certain conditions (*23, 25*).

(3) Tokamak and Stellarator experiments have not yet been operated far inside the impermeable ion-density range of full-scale reactors.

(4) The problem of steady-state Tokamak operation by means of a "bootstrap" mechanism has not been settled. It is true that if the impurity problem of closed bottles cannot be solved, pulsed operation should in any case become necessary. On the other hand, such operation may reduce the chances of achieving a practical, useful reactor.

(5) The coil windings that generate the toroidal magnetic field introduce complications in the replacement and repair of construction details.

Problems of Reactor Technology and Full-Scale Operation

During recent years, considerable progress has been made in fusion reactor technology; specifically, concerning superconducting coils, material problems including damage by neutron radiation, blanket construction, and model studies of full-scale systems. In particular, the detailed study known as UWMAK at the University of Wisconsin should be mentioned, as well as associated calculations on the afterheat problem and the suggestion of using a graphite curtain to reduce the damage of the first wall (*49*).

Nevertheless, this is just the beginning of a long and laborious road to the final goal; the problems to surmount include the interaction between the plasma and the first wall, sputtering and blistering, refueling and removal of ashes, the cooling system and its working fluids and gases, and, last but not least, repair and replacement of radioactive construction details.

The Future

At the present stage, many of the basic problems of fusion research in fact have been solved, and there is no indication that the remaining problems could not be tackled successfully if sufficient resources were made available. However, it also must be stressed that *none* of the approaches described in this review will lead for certain to the fusion reactor — even if it appears that several schemes have a chance to do so. Consequently, the present concentration of the main activities and resources of the world's fusion research on rather narrow lines and on a few large projects at the expense of basic investigations cannot be reconciled with the corresponding necessary knowledge in fusion physics and technology.

In case none of these large projects is able to keep the promise of being a solution of the reactor problem, fusion research as a whole may end up in a difficult political dilemma. This situation is partly a result of attempts to accelerate fusion research toward its final goal under the present constraint of limited resources. Needless to say, a substantial increase in available funds would cure this situation at once.

Even at the present economic level, however, the efficiency of future research programs could be improved under the following measures:

(1) The large- and intermediate-size experiments with devices of a more conventional Tokamak, Stellarator, pinch, and mirror type, should be reduced in number as far as possible. Unnecessary duplication can be avoided in part by means of improved coordination of projects on the international level. Only a few large devices with strong fields, such as Tokamaks planned for studies of the not-fully-understood scaling laws, should be developed further.

(2) Experimental investigations on modified toroidal devices and other schemes, also within extended parameter ranges, should be encouraged.

(3) More resources can be given to basic research conducted along broad lines and through moderate-size experiments, without affecting the total budget of larger experiments to any greater extent.

(4) A systematic search for more alternatives to the present lines of approach should be continued. *New ideas*, as well as reconsiderations of earlier ideas from new angles, may be critical for future success of fusion research.

In any case, the goal of fusion research ought to be too important to society to undertake half-measures in the form of redistribution of insufficient funds. The real and necessary action to be taken now consists of a *crash program* that strongly features basic research and new ideas along broad lines and that is conducted by all the manpower that is practically available.

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An Assessment of Laser-Driven Fusion

Prepared by
K.A. Brueckner & Assoc., Inc.

This article comprises parts of a report prepared for and published by the Electric Power Research Institute in September 1976 and is reprinted with permission from EPRI. Included are a preface written by the EPRI Laser Fusion Assessment Council, Parts 1 and 2 of the report, and a commentary on a draft of the report by KMS Fusion, Inc. that was published in an appendix to the report.

Parts 1 and 2 were prepared by Keith A. Brueckner, Rolf W. Gross, George R. Hopkins, Siebe Jorna, Ray E. Kidder, Gerald L. Kulcinski, James H. McNally, and William B. Thompson.

Dr. Brueckner, a theoretical physicist and educator, has worked both in the weapons program and as the technical director of KMS Fusion, and has been involved in a broad range of research from basic nuclear structures to plasma physics.

Preface

Background

The search for a way to produce energy by the controlled fusion of light nuclei has, for several decades, centered about the development of a magnetic bottle — a wall-less container of hot plasma. In the past few years, a substantial effort has grown, in the U.S. and elsewhere, to achieve controlled fusion with lasers; this method relies on inertial, rather than magnetic, confinement. What is laser-driven fusion? Is it a viable alternative to magnetic confinement? Will it be achieved in a shorter time? Does it also face serious obstacles? Is it suitable for commercial use as well as for the military applications for which it was originally intended? To provide the utilities with answers to such questions, EPRI formed a Laser Fusion Assessment Council and commissioned K.A. Brueckner and Associates to head a Working Group, a carefully chosen panel of experts, to make a study and evaluation of laser fusion — its current status and future possibilities — from an independent point of view.

The document presented here is the result. Members of the Council feel that the summary of technical progress prepared by the Working Group is the most complete, concise, and accurate statement on laser fusion available. The report also conveys opinions on the interpretation of scientific data and on past and future research policy. These opinions and evaluations are not those of any single individual but are intended to represent the consensus of opinion of the entire Working Group, listed as co-authors of the report.

Members of the Council feel that the report is neutral, showing as little bias one way or the other as is possible in an assessment of this sort. Nonetheless, we recognize that different persons intimately involved with laser fusion or CTR will tend to react differently to what was written. To allow for varying opinions, a draft of the report was sent to members of the scientific community for comment. The comments received are included in the Appendix,* and corrections of a purely factual nature have been made in the text.

Primary Conclusions

A single, definitive conclusion cannot be given because unclassified and classified pellet designs give different results for the gain factor G (ratio of fusion energy to laser energy delivered on target). The report shows that a G value of 31 is required for energy breakeven, and a G of 125 is required for net power production with an overall plant efficiency of 30 percent, even for an optimistic laser efficiency of

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*Only one of the commentaries, the KMS Fusion reply, is included in the Appendix here.

8 percent. Maximum calculated values of G which have been published are in the range 100-200. However, the report points out several experimental results which indicate that achievable G's may be an order of magnitude below this. Consequently, *as of this time*, production of net power with unclassified target designs does not seem likely. Rapid progress is, however, a characteristic of this field; and one cannot conclude that the outlook for unclassified pellets will not improve with further development. The possibility of high values of G with classified pellets does not vitiate the conclusions of this report; most of the problems of physics and engineering discussed are common to both types of pellets.

In 1968, a milestone in CTR was achieved when it was found that "Bohm diffusion," an anomalously fast plasma escape rate, could be overcome in Tokamaks. The observed loss rate, though not classical, was slow enough to allow design of prototype reactors. A conclusion of this report is that laser fusion has not yet reached a comparable milestone. This conclusion is based on the observation that anomalies in laser energy absorption and transport in the pellet are only now beginning to be recognized, and that the understanding and solution of the plasma problems are yet to come. There are many who would disagree with this assessment, and the Council has given an opportunity for divergent opinions to be expressed.... The Council is satisfied, however, that this conclusion of the report was reached only after careful deliberation by the entire list of authors of the report.

Classified Targets

Because of the numerous references in the text to classified targets, leading to a more optimistic prognosis, the Council felt that a classified briefing was needed to supplement the report, which is based entirely on unclassified information. Such a briefing was arranged by ERDA and was attended by several members of this Council. The following statement was written by these members and has been approved by ERDA for release.

> 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 CY '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 tradeoff in laser characteristics. The economic and technological optimization of a reactor may be altered in an 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.

Implications of Classification

Should classified pellet designs be needed for net power production, the utilities would be faced with an unpleasant, but not insurmountable, problem. There is, of course, a possibility that declassification would occur long before the industry is ready to build commercial plants. Failure to declassify could lead to the necessity for the federal government to design, construct, and control the entire laser-fusion core of the plant (even though only the pellets are classified), and for the utilities to purchase thermal power from the government and operate the energy conversion plant. Precedent for this pessimistic scenario can be found in the N reactor at Hanford, which is government-operated, even though only part of the design was classified. The arrangement is workable, but awkward.

It is possible that public utilities would suffer more from classification than private utilities, because of the need for full public disclosure except where federal law supersedes state law. Furthermore, industrial support for classified power plants would be affected by the impossibility of foreign sales. The greatest near-term impact of classification, however, probably lies in the time lag caused by the lack of information on particle and energy output of classified pellets. Reactor design studies cannot be made until at least this information is available.

Electron-Beam Fusion

An alternative inertial confinement scheme employs relativistic electron or ion beams rather than laser light to implode a pellet. Although not explicitly treated in this report, E-beam fusion is a closely related concept having many features and problems in common with laser fusion. The main differences are as follows. E-beam generators do not have the efficiency problem of lasers and are presently capable of delivering comparable power and much more energy per pulse. For reasons of beam focus and energy deposition length, E-beam targets tend to be larger than laser targets. It is difficult to transport an E-beam onto a target far from the source and to achieve repetitive pulsing (presumably, with a foil-less anode). For these reasons, the feasibility of designing a power plant based on E-beam fusion is not clear. However, the possibility of studying implosion phenomena in the near term justifies the current level of support for E-beam fusion, about 10 percent of the ERDA inertial confinement budget.

The Foreign Effort

Besides E-beams, another omission in this report is laser-fusion research in other countries. The only large foreign effort at the present time is in the USSR; smaller but substantial programs exist in West Germany, France, and Japan, with England and Israel just now starting serious participation. Limited information is available from the USSR. It was felt that any additional insight to be gained by reviewing the foreign programs would be minimal.

The Future

This report heavily emphasizes the problems of progress in achieving scientific feasibility, as distinct from the problems of engineering and laser development. This emphasis is merely a reflection of the information currently available. As research proceeds on the solution of the physics problems, it is anticipated that considerations of engineering and laser development will eventually come to the forefront. Laser fusion is a rapidly developing field, and the picture presented here is an instantaneous view as of March 1976. An update of the report in 1978 is advised by the authors. At that time, it is anticipated that considerably more information will be available on reactor designs and efficient laser systems.

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Part I

Introduction And Summary

Introduction

This report, an assessment of the engineering and scientific feasibility of laser-driven fusion, is based on studies and site visits carried out in the period from Nov. 1, 1975 to March 15, 1976. The report is briefly summarized in Part 1, Introduction and Summary. Part 2, Overall Assessment: Engineering and Scientific Feasibility, discusses the general engineering aspects and problems and the overall assessment of the engineering and of the present experimental and theoretical status of laser-driven fusion. Part 3, Engineering Feasibility, and Part 4, Scientific Feasibility, give more detailed discussion of the considerations on which the assessments are based. Part 5, Site Visits, briefly summarizes the principal information obtained in the site visits.* The content of the study is also based on extensive literature collections which have, however, not been specifically referenced in the text.

This document is a survey of a rapidly developing field; the authors therefore recommend an early update (early 1978) of the study.

Summary

Laser-driven fusion, in contrast with the longer-studied and more familiar magnetically confined fusion (CTR), depends on the laser-initiated release of explosively generated fusion energy from an inertially confined fusion pellet. Analyses show that this process can be produced at an interesting level of efficiency only if the pellet is very highly compressed into an optimum condition for ignition and subsequent explosive burning. Although the desired conditions are well known, major uncertainties presently exist in many features of the physical phenomena of laser-interaction, energy transfer, and compression. Present experimental results are far short of a demonstration of scientific feasibility, the compressions achieved being too low by a factor of 100 to 1000. Implosion velocities have been achieved which would be sufficient to produce ignition in large pellets. These velocities have, however, been associated with strong irreversible shock heating, and hence are of little relevance to the conditions of quasi-adiabatic heating essential for high-gain pellets.

Published computations of pellet gain (ratio of fusion-energy output to incident laser energy) have given maximum results of 100-

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*Parts 3 through 5 are not included here.

200. The limit on gain is a result of the restrictions set by the energy required for fuel ignition, the inefficiency of transfer of energy from the laser beam into hydrodynamic compression and heating of the pellet fuel core, and the termination of fuel burn-up at 35-40 percent by depletion and disassembly.

Analysis of reactor efficiency shows that pellet-gain close to the computed maximum is required for driving a pure fusion reactor. The number of unresolved scientific problems now identified, each of which can seriously affect scientific feasibility, raises serious questions concerning eventual demonstration of high pellet gain and hence of engineering feasibility of pure fusion systems. In recognition of this problem, increased attention is being given to classified designs which may have higher gain than those appearing in the literature today. Interest is also increasing in fusion-fission hybrids which give large energy multiplication by fast fission in a fissionable blanket and from the subsequent breeding of fissile fuels. These large multiplications tend to relax the laser and/or pellet gain requirements.

The studies in laser-fusion reactor design considered in this report have assumed high pellet gain and have *not* analyzed the possible effect of pellets of classified design with higher gain and altered output spectra. The designs are therefore quite speculative since the pellet gain as a function of pellet yield may be too low for interest for unclassified pellet designs and is not available for classified designs.

Laser-driven fusion has not yet reached a critical milestone comparable to the 1968 discoveries in the CTR Tokamak program. Two critical measurements which need to be made are: (1) the achievement of high density with quasi-adiabatic compression (100 gm/cm^3 at 3-5 keV); and (2) demonstration of coupling of energy to the D-T fuel of high enough efficiency to allow projected pellet gain of the order of 50-100. These experiments, which would produce considerably less than scientific-breakeven yield, are possible in the next one to two years at several laboratories in the USA and USSR. They are now given high priority in the ERDA program which has in the past given principal emphasis to the design and construction of large laser facilities.

Aside from the question of scientific feasibility, laser-driven and CTR reactors have some very different problems in the nuclear core. A CTR reactor requires large magnetic fields, high plasma purity, possibly divertors to remove wall-produced contaminants (Tokamaks), neutral-beam, RF, or auxiliary heating for ignition, D-T fuel injection, lithium for tritium breeding, heat removal from within the magnetic field, a first-wall subjected to severe radiation damage, and large single unit size for economical plants. The laser-driven reactor has the tritium breeding requirement and a somewhat different (possibly more severe) first-wall problem. The other characteristic features of the CTR reactor are absent but are replaced by the major problems and uncertainties of the laser and laser-beam injection and pellet design, fabrication, and cost. The physics of the laser-driven system allows lower average power units and greater redundancy of critical elements than does the magnetic approach. The laser-driven reactor (laser aside) appears to be a simpler and smaller unit than a CTR reactor, but the technology of the laser system is more *uncertain than any element of the proposed CTR-reactor configurations.*

Laser-driven fusion is presently less well demonstrated as a scientific possibility than is CTR. The appropriate size and characteristics of the laser to drive the implosion cannot yet be specified. However, once the two barriers of scientific feasibility and laser technology are overcome, it is felt that the reactor-associated problems are *relatively easier to solve than in a CTR*. That is, many of the first-wall problems can be solved by increasing the diameter of the vessel which increases the cost at a lower rate than in a CTR but reduces the neutron, charged-particle, and photon flux to levels which can be accommodated with reasonable wall lifetime.

The feasibility of a laser-driven fusion reactor depends principally on the solution of the following problems: (1) the design of pellets (including the possibility of classified designs) which will give high gain (greater than 100) and can be mass produced at a cost of less than roughly 0.02 cents per megajoule yield; (2) development or discovery of lasers at the wavelength allowed by the laser-pellet-interaction physics that can be built at efficiency greater than 5 percent and preferably greater than 10 percent, at acceptable cost (roughly $\$50 \times 10^6$ per megajoule), and acceptable cost per pulse (roughly 1 cent per megajoule pulse).

The only laser technology which presently can be extrapolated with some confidence to reactor scale is CO_2. The problems of laser-pellet interaction (with unclassified targets) at the CO_2 wavelength of 10.6 microns are, however, very severe so that feasibility appears to depend on the success of other pellet designs which cannot be evaluated within the scope of this study. No other laser technology has been sufficiently developed to give even a low confidence level of expansion to reactor scale, pulse rate, and efficiency. The iodine and HF lasers are interesting but far from the level of understanding and performance allowing projection to reactor applications. The problem of laser technology is particularly acute if pellet-interaction physics requires wavelengths considerably less than one micron. In this range, several possibilities exist but only at a primitive level of development and understanding. A major effort in laser research is clearly required, particularly if the attractive features of CO_2 cannot be utilized because of unfavorable pellet-physics.

The other elements of the reactor technology are considered to be soluble with known techniques, and in any case to be far less uncertain and less expensive in solution than the problems just enumerated. In a realistic program for *reactor* development, which has been giving major emphasis to the basic problems, early attention must also be given to the details of first-wall design and protection of the final elements of the optical system, and of pellet injection. These problems, together with many of the other uncertainties affecting design, can be studied in a test-reactor facility which could be built without stringent requirements on laser efficiency and cost. As an example, an iodine or CO_2 laser at 200 kilojoules and designed for a pulse rate of 5/second could (optimistically) give single pulse fusion yields of one megajoule, an average fusion power of 5 megawatts, and 2.5×10^{18} 14 MeV neutrons/second. With a containment vessel of 50-cm radius, the neutron current would be $8\times10^{13}/\text{cm}^2$ sec or 2.5×10^{21} neutrons/cm^2/year. The experimental data on pellet yields should be available by 1978-79 to determine the feasibility of such a test-reactor design. Such a facility could be used to study the combined effects of radiation damage *and* cyclic stress-loading peculiar to the laser-driven reactor first wall. The feasibility of proposed wall designs should be closely examined in the near future to determine the range of workable configurations for later study in a pulsed test facility.

Research on the engineering aspects of reactor design should be concentrated on: (1) those elements of pellet design, computation, yield, and output spectra necessary for reactor design; (2) first-wall and last-mirror design; (3) configuration of the nuclear core, including final laser-beam transport, injection, and isolation; (4) laser technology; (5) the efficiency and reactor configurations for gas production; (6) fusion-fission hybrids; and (7) the design of a laser-assisted fusion engineering research facility (e.g., LAFERF).

Part II

Overall Assessment: Engineering And Scientific Feasibility

Introduction

GENERAL STATEMENT OF THE PROBLEM

Laser-driven fusion, in contrast with the longer-studied and more familiar magnetically confined fusion (CTR), depends on the laser-initiated release of explosively generated fusion energy from an inertially confined fusion pellet. Analyses show that this process can be produced at an interesting level of efficiency only if the pellet is very highly compressed into an optimum condition for ignition and subsequent explosive burning. Although the desired conditions are well known, major uncertainties presently exist in many features of the physical phenomena of laser interaction, energy transfer, and compression. Present experimental results are far short of a demonstration of scientific feasibility, the compressions achieved being too low by a factor of 100 to 1000. Implosion velocities have been achieved which would be sufficient to produce ignition in large pellets. These velocities have, however, been associated with strong irreversible shock heating and hence are of little relevance to the conditions of quasi-adiabatic heating essential for high-gain pellets.

Computations of pellet gain (ratio of fusion-energy output to incident laser energy) have given maximum results of 100-200. The limit on gain is a result of the restrictions set by the energy required for fuel injection, the inefficiency of transfer of energy from the laser beam into hydrodynamic compression and heating of the pellet fuel core, and the termination of fuel burn-up at 35-40 percent by depletion and disassembly. The gain required for reactor application may be easily estimated. If E_{L} is the incident laser energy, G the pellet gain, and η_{FE} the efficiency of conversion of fusion energy to electrical energy, the electrical output per pulse is $\epsilon = E_{\mathrm{L}} G \eta_{\mathrm{FE}}$. If η_{EL} is the efficiency of conversion of electrical energy to laser energy, the ratio of output energy to circulating energy is

$$Q = \frac{\epsilon - E_{\mathrm{L}}/\eta_{\mathrm{EL}}}{E_{\mathrm{L}}/\eta_{\mathrm{EL}}}$$

$$= G\eta_{\mathrm{FE}}\,\eta_{\mathrm{EL}} - 1.$$

The overall plant efficiency is

$$\eta_{\mathrm{net}} = \frac{\epsilon - E_{\mathrm{L}}/\eta_{\mathrm{EL}}}{GE_{\mathrm{L}}}$$

$$= \eta_{\mathrm{FE}}\,\frac{Q}{Q+1}.$$

For an efficiency η_{FE} of 40 percent for conversion of fusion to electrical energy and a laser efficiency of η_{EL} of 8 percent, a gain of 125 is required to give Q of three and η_{net} = 30 percent. For these efficiencies, a gain of 62.5 drops Q to one and η_{net} to 20 percent, and a gain of 31.25 drops Q and η_{net} to zero; i.e., the reactor is only self-sustaining with no output of energy.

The preceding simple analysis shows that pellet gain close to the computed maximum is required for driving a pure fusion reactor. The number of unresolved scientific problems now identified, each of which can seriously affect scientific feasibility, raises serious questions concerning eventual demonstration of high pellet gain and hence of engineering feasibility of pure fusion systems. In recognition of this problem, increased attention is being given to classified designs which may have higher gain. Interest is also increasing in fusion-fission hybrids which give large energy multiplication by fast fission in a fissionable blanket and from the subsequent breeding of fissile fuels.

The studies in laser-fusion reactor design considered in this report have assumed high pellet gain and have *not* analyzed the possible effect of pellets of classified design with higher gain and altered output spectra. The designs are therefore quite speculative since the pellet gain as a function of pellet yield may be too low for interest for unclassified pellet designs and is not available for classified designs.

The pure fusion reactors considered burn D-T in spherically layered pellets containing the D-T fuel. Tritium is bred in a lithium blanket which may or may not circulate to transfer heat to an external heat exchanger. Tritium is removed in a side-loop from the main lithium flow path. The laser beams (usually several) are focused, on a ballistically inserted pellet, through ports in the reactor wall with the last turning mirrors protected from the reactor environment by distance and magnetic shielding. The gas density in the containment vessel is kept at the desired level by pumping to remove pellet debris and possible ablation products from the inner wall of the containment vessel. The laser is well separated from the reactor core; the problem of design of the system therefore can be separated into the essentially independent design problems of the containment vessel, together with the final laser-beam focusing and injection, and of the laser system.

The power level of the reactor is set by the single pulse yield and pulse rate. For 100 megajoules/pulse and 10 pulses/second, the thermal power is 1000 megawatts. No physical constraints prevent operation at considerably lower pulse rates and hence lower powers. The economic operation of the reactor would, however, require an optimized pulse rate and hence determine power output. The average power could also be reduced by the use of lower pellet yields (with pulse rate set at the optimum design point) provided that the pellet gain remains high enough for the desired plant efficiency. Higher powers may be conveniently obtained, if a single containment vessel is operated at the pulse-rate limit which is primarily set by radiation damage containment-vessel pumping rates and average heat load, by the use of multiple containment vessels illuminated in sequence by a single laser. Additional redundancy for purposes of plant reliability can be provided by duplication of the laser system or at least of the more vulnerable components.

COMPARISON WITH CTR

The controlled thermonuclear research (CTR) program of the U.S. was maintained at a relatively low funding level of approximately $30 million per year through the 1960s, with major difficulties in plasma confinement preventing an optimistic prognosis and limiting program development. This situation changed markedly in 1968-69 with the demonstration in the USSR with the Tokamak device of plasma confinement well above the Bohm limit. These results were quickly confirmed and improved upon with the conversion of the C-Stellerator at the Princeton Plasma Laboratory (PPL) to the Tokamak configuration. In the following years (through early 1976) results have continued to improve, particularly with the Alcator (a Tokamak configuration) at MIT and the large Soviet Tokamak T-10. In these experiments $n\tau$ of about 10^{13} sec/cm^3 at an ion temperature of 0.8-1 keV have been achieved. These conditions are about a factor of 10 lower than reactor conditions (both in density and temperature). As a result of the continuing improvement in performance and understanding, the USSR and U.S. are considering much larger Tokamak devices which are engineering-related prototypes to allow more detailed study of the problem of reactor technology as well as to extend the plasma conditions into the reactor range. Significant advances have also been made in the area of the mirror configuration with the 2X-IIB devices at LLL recently achieving ion temperatures of 13 keV and $n\tau$ values of 10^{11} sec/cm ^{3}.

The rapid development of the CTR program since 1968 has been accompanied by a shift of emphasis into the study of engineering problems and of reactor design. These changes resulted primarily from the "scientific milestone" established by the USSR results.

Laser-driven fusion has not yet reached a comparable critical milestone. In terms of maturity, the field has not progressed to the level of the 1968 discoveries in the CTR program. Two critical measurements which need to be made are: (1) the achievement of high density with quasi-adiabatic compression (100 gm/cm^3 at 3-5 keV); and (2) demonstration of coupling of energy to the D-T fuel of high enough efficiency to allow projected pellet gain of the order of 50-100. These experiments, which would produce considerably less than scientific-breakeven yield, are possible in the next one to two years at several laboratories in the USA and USSR. These experiments are now given high priority in the ERDA program which in the past has given principal emphasis to the design and construction of large laser facilities. These experiments have not been possible with existing smaller laser systems. However, new intermediate-sized laser systems such as the Argus system at Livermore should be capable of producing such results. In the meanwhile early experiments on the glass system at KMS, the Janus and Cyclops systems at LLL, and the Pharos system at NRL have all provided key data to allow extrapolation to needed higher density and efficiency implosions. Successful analysis of and experimental results from such implosions should provide the critical impetus for expansion of the program.

Aside from the question of scientific feasibility, laser-driven and CTR reactors have some very different problems in the nuclear core. A CTR reactor requires large magnetic fields, high plasma purity, possible divertors to remove wall-produced contaminants (Tokamaks), neutral-beam, RF, or auxiliary heating for ignition, D-T fuel injection, lithium for tritium breeding, heat removal from within the magnetic field, a first-wall subjected to severe radiation damage, and large single unit size for economical plants. The laser-driven reactor has the tritium breeding requirement and a somewhat different (possibly more severe) first-wall problem. The other characteristic features of the CTR reactor are absent but are replaced by the major problems and uncertainties of the laser and laser-beam injection and pellet design, fabrication, and cost. The physics of the laser-driven system allows lower average power units and greater redundancy of critical elements than does the magnetic approach. The laser-driven reactor (laser aside) appears to be a simpler and smaller unit than a CTR reactor, but the technology of the laser system is more uncertain than any element of the proposed CTR-reactor configurations.

Laser-driven fusion is presently less well demonstrated as a scientific possibility than is CTR. The appropriate size and characteristics of the laser to drive the implosion cannot yet be specified. However, once these two barriers are overcome, it is felt that the reactor-associated problems, excluding the problems of the laser, laser-beam transport and injection, are *relatively* easier to solve than in a CTR. That is, many of the first-wall problems can be solved by increasing the diameter of the vessel which increases the cost at a lower rate than in a CTR but reduces the neutron, charged-particle, and photon flux to levels which can be accommodated with reasonable wall lifetime.

Overall Assessment of Scientific Feasibility

OBJECTIVES

The demonstration of scientific feasibility, which has been defined to be the production of fusion energy equal to laser energy, requires the heating of compressed D-T to the ignition point of about 5 keV, with self-heating of the fuel by energy deposition from the reaction products (primarily α-particle deposition) leading to rapid burning near the peak of the reaction rate. Various calculations of burning under optimized conditions have shown that 0.1 to 0.2 kilojoules of thermal energy in D-T compressed to 1 to 2 thousand grams/cm^3 or to ρR of 0.3 to 0.5 grams/cm^2 are sufficient to obtain the desired yield. For laser-fuel coupling efficiency of 5 percent, the corresponding laser energy required is 2 to 4 kilojoules, which may be somewhat reduced if central ignition and propagating burn can be produced in the fuel by an optimized compression history. The laser power required depends on the pellet design; for a tamping shell containing a cryogenic layer of D-T with an aspect ratio of 30/1, the implosion time with a linearly rising pulse is about one nanosecond and the peak laser power is in the range of 5 to 10 terawatts. No laser has yet operated in this power range. In addition to the laser power requirements, implosions to high density and significant thermonuclear yield will require great precision in laser temporal control and spatial uniformity, uniform pellet irradiation, and very high quality pellets with uniform layers of ablation, tamper, and cryogenic fuel. The diagnosis of pellet implosions requires extensive and expensive instrumentation, large manpower commitments, sophisticated data reduction, and computer-aided analysis and theoretical interpretation. The necessary combination of facilities and techniques, which does not yet exist in the laser-fusion program, is currently under construction at LLL and LASL.

PRESENT STATUS

Early in the program, discussion within the ERDA complex led to decisions that were intended to optimize the construction of the large facilities thought by ERDA to be required for laser-fusion research within the overall dollar constraints of the limited available budget. It was decided that the Nd-glass laser technology was closest to practical exploitation with a large laser system. This technology was to be pursued at LLL and the Shiva facility construction was funded. CO_2 was felt to be the next most promising laser medium and LASL was to pursue CO_2-laser research as a potential candidate for a facility that would be constructed at a later date at Los Alamos. An independent program was started by KMSF in 1969, without government funding. KMSF believed that existing Nd-glass technology could be used as the basis for design and construction of a large Nd-glass laser, and that a peak power of about 30 terawatts would be sufficient to obtain high pellet gains. This large facility, planned for completion in 1972, could however not be funded within the resources available to KMSF. The subsequent KMSF program, directed at the more modest goal of scientific breakeven (pellet gain of unity), was based on a laser requirement considerably less (several terawatts) than estimated by the ERDA laboratories as necessary. At the present time, the construction of the building to house the large LLL system (Shiva) nears completion; it has been concluded by LLL, on the basis of present experiments and continuing calculations, that such a system will in fact be necessary to reach significant thermonuclear burning, although the LLL facility will not allow a full demonstration of scientific feasibility. In the course of developing the Nd-glass technology and the first very large laser facility, LLL has also had the resources to develop advanced instrumentation and computing capability, and hence currently has the leading role in the U.S. laser-fusion program, in an integrated program of laser-fusion research. As other facilities are constructed, other programs can be expected to achieve a balance that is similar to that found in the LLL program, if comparable funding is provided.

A summary of the funding to date in the ERDA program is given in Table 1. In the 14 years since the first government funding of the laser program, $283 million has been invested.

The theoretical and code developments, the experiments on laser-plasma interaction and pellet implosions, and the analysis and interpretation of the experimental results are all in an early stage with considerable disagreement existing among the five laboratories

TABLE 1

SUMMARY OF TOTAL ERDA FUNDING
OF U.S. LASER-FUSION-POWER PROGRAMS

Fiscal Year	Millions of dollars
1963	0.2
1964	1.1
1965	1.3
1966	1.2
1967	1.4
1968	1.3
1969	2.1
1970	3.2
1971	9.4
1972	19.4
1973	34.5
1974	44.3
1975	63.7
1976	79.5
(transition) 1976	20.3
(projected) 1977	98.9

carrying out significant programs directly relevant to laser-driven fusion (LLL, LASL, KMSF, LLE, and NRL). The experience of the past two years (CY 74 and CY 75) has shown that unambiguous interpretation of experiments will not be possible unless integrated measurements are made of the relevant observables. In addition, major effort must be continued on the development of instrumentation. The interpretation of the experiments is also very difficult within the theoretical and code capabilities of the participating laboratories. The already sophisticated code development and analytical capability will require further development as new experimental results are obtained. In all laboratories, the instrumentation and computational needs are recognized. Funding and manpower allocated to these needs have not been adequate. The complexity and cost in capital expenditures as well as in manpower have prevented complete utilization of the necessary diagnostic techniques. This deficiency is least pronounced in LLL but is prominent in the present and planned experiments in the other laboratories.

As a result of the limitations prominent in the present experiments and theoretical and code developments, only partial agreement exists on the most basic aspects of the laser-pellet interaction, such as the plasma absorptivity and the distribution of energy into X-ray emission, ablation, and fast-ion production. The scaling of these processes with laser power, pulse length, and target characteristics is even more poorly known. The more difficult problems have been incompletely studied with the result that interlaboratory comparison is difficult and often inconclusive.

The absorption mechanism is believed to be a combination of inverse bremsstrahlung and resonant absorption, but major disagreement exists on the relative importance of these effects and the experiments are too incomplete to resolve the controversy. Agreement does exist, however, on the probable existence of marked density profile modification by the strong ponderomotive forces near the critical density surface, and some qualitative experimental results have confirmed this theoretical expectation. This effect, while predicted in the simulation codes, has only been incorporated by NRL in the complete hydrodynamic 1- and 2-D codes used for analysis of the experiments.

The energy flow from the laser deposition region into the overdense plasma is of crucial importance since the flow drives the ablation process and hence the implosion. The flow is now believed to be very markedly reduced by a combination of magnetic fields produced by irradiation nonuniformity and reduced electron conductivity resulting from interaction between electrons and ion acoustic waves. The theoretical and computing predictions are, however, only qualitative and determination of the magnitude and origin of the reduced energy flow must be done by experiments. Unfortunately, direct measurement is very difficult and anomalous effects must be inferred by comparison of elaborate and complex calculations with measurements of X-ray spectra, fast ion spectra, and spatial distribution of plasma and temperature obtained from high resolution X-ray and optical imagery. The magnetic fields can also be measured directly by the Faraday rotation of polarized optical probe beams, provided that effects of refraction and possible depolarization by polarization-dependent absorption can be assessed. These should be time-resolved measurements to reduce the ambiguities in the interpretation. No laboratory has yet made the measurements required to resolve the present uncertainties in these phenomena. The best measurements of magnetic fields, although incomplete, are at NRL. The difficulty of interpretation of the present experiments or of future experiments is further increased by the lack of adequate theoretical and code development and of computing capability, with LLL again the only laboratory with facilities and with sufficiently advanced code development in the code LASNEX (which, however, will also be used by LASL). This situation can be improved outside of LLL and LASL if substantial effort and cooperation on theoretical and code development and use is achieved.

Another set of problems of crucial importance in pellet implosions arises from the requirements of uniform pellet illumination at close to normal incidence. Unstable development of magnetic fields which can amplify illumination asymmetry is possibly of major importance. This has not been studied experimentally and present theoretical and computing studies are inconclusive. KMSF has the most nearly uniform illumination at normal incidence in present experiments using large aperture elliptical reflectors. A similar system is, however, planned by LLL for CY 76. The KMSF laser-beam quality in the focal region appears, however, to be poor, although the focal-spot intensity variation is incompletely measured. The temporal variation of beam quality in the focal region has been measured at NRL and LLL where very significant variation has been found. The generally mediocre quality of pellet illumination (except at KMSF) is apparent from the mapping of the laser beam in the plasma which is obtained from the plasma emission of $2\omega_L$ light and from X-ray emission from the pellet surface, although little effort has been made to make quantitative use of this information. Experiments to obtain high compression have been difficult as a result of nonuniform illumination patterns.

All analyses show that temporal pulse shaping is essential for producing high compression in pellet implosions. The only operating pulse-shaping system is at KMSF, although limited pulse form control is possible at the other laboratories by control of pulse width. The pulse-form requirements are markedly affected by pellet design which in turn is controlled by possible instability of high aspect-ratio shells. Experimental study of the variation of compression with pulse form is clearly of major importance but little or no effort has to the present been applied to this problem.

The present experiments on neutron production by implosion of D-T-filled glass shells, while important as a confirmation of limited elements of the implosion process and useful for testing code accuracy, are now generally felt to have little relevance to the ultimate goals of laser-driven fusion. The major problem of shell hydrodynamic stability seems to have been bypassed by working in a regime of strong shell preheat which inhibits shell instability by preventing shell compression and the corresponding very high aspect ratio during the implosion process. The neutron production has been a result of violent shock heating of the D-T filling gas followed by only moderate compression (by a factor of hundreds or less) to a density of the order of one gram/cm^3. These conditions are unfortunately almost at an extreme from the quasi-isentropic compression to a density of 1000 grams/cm^3 required for scientific breakeven or eventually for practical application. The primitive nature of the present neutron-producing experiments is also apparent in the near absence of effort to produce higher compression and less violent early shock heating by the use of temporally shaped laser pulses. In fact, limited attempts at pulse shaping have markedly reduced (or eliminated) neutron production, but the source of this important effect is not known.

The limitation of compression by the theoretically predicted but not as yet observed hydrodynamic instability or by the experimentally observed shell preheat resulting from fast electrons and/or X-rays is now believed to be one of the most serious problems in laser-driven fusion. Compression can be inferred from neutron yield combined with source temperature measurement, using the spectra of reaction products. A more direct determination of compression is possible from the measurement of energy loss by reaction products escaping from the reacting fuel. These measurements do not, however, give any information about symmetry or the origin of possible compression reduction. Much more useful measurements of compression and implosion history can be made by X-ray imaging with high spatial and temporal resolution compared with the computed characteristics of the X-ray image. At present only time-integrated X-ray images with 3-5 micron resolution are available and analysis of these images has given partial confirmation of the theoretical predictions. The improved instrumentation to obtain higher spatial and temporal resolution (axisymmetric reflecting optics X-ray microscopes of high magnification combined with X-ray streak cameras) appears to be technically possible and is planned at LLL and later at LASL. The important quantitative comparison of the measurements with theory, which requires a sophisticated code such as LASNEX, will also be possible at LLL and LASL, but probably not at the other laboratories.

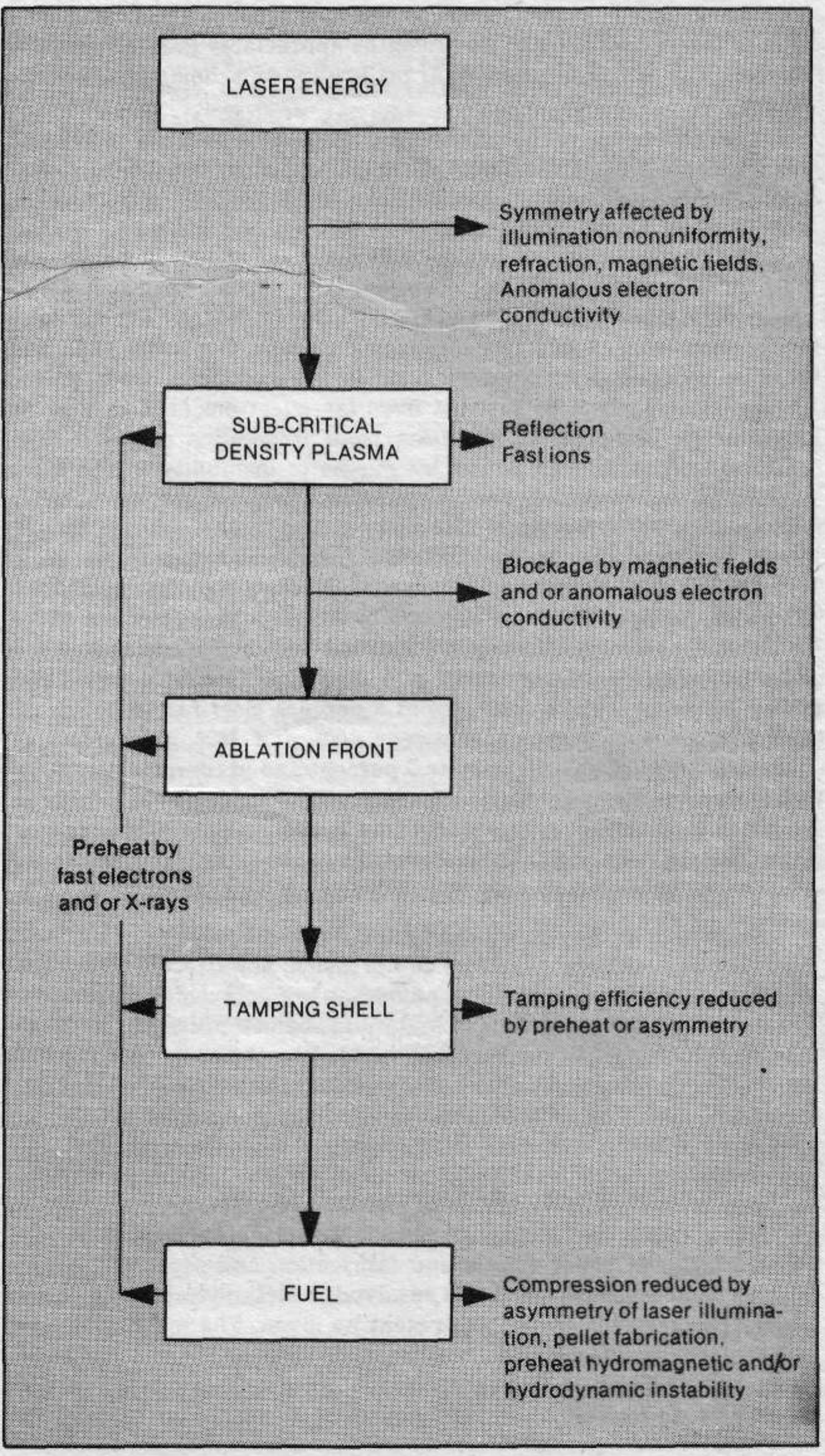
PLANS

Nearly all of the results given in this report were obtained with neodymium-glass lasers with peak power on targets of 0.3-0.5 terawatts. Exceptions were the 1-terawatt Cyclops system of LLL, which was limited to 1-beam illumination and used primarily on classified targets, and the first operation of the KMSF 2-beam double-pass system at about 1.4 terawatts, where results are not yet available. A marked increase in capability is expected in CY 76 at LLL with the dual-beam Argus system (2-4 terawatts with elliptic reflectors), at LLE with 4 beams (4-8 terawatts), and at KMSF with the double-pass system (1.5 to 2 terawatts). Improved instrumentation is planned at all of the laboratories although funding and manpower allocations at LASL and more severely at KMSF, LLE, and NRL will restrict developments.

The many uncertainties now confronting laser-driven fusion, all of which can have major impact on feasibility, can be resolved only by highly instrumented and analyzed experiments. The experiments will require a well-balanced combination of (1) time-integrated and spatially and temporally resolved measurements of the optical and X-ray spectra, of the energy spectrum of the plasma components, and of the characteristics of the nuclear reaction products; (2) computer-aided data gathering and reduction; and (3) theoretical and computer analysis with highly sophisticated 2-D and simulation codes. Laser-fusion research without this capability may be of little value, unless improved interlaboratory cooperation can be developed.

EVALUATION

Laser-driven fusion faces a variety of uncertainties which separately present problems of major significance and together raise serious questions about scientific and hence engineering feasibility. The problems are particularly acute at 10.6μm and may be character-


LASER ENERGY
Symmetry affected by illumination nonuniformity, refraction, magnetic fields. Anomalous electron conductivity
SUB-CRITICAL DENSITY PLASMA
Reflection
Fast ions
Blockage by magnetic fields and or anomalous electron conductivity
ABLATION FRONT
Preheat by fast electrons and or X-rays
TAMPING SHELL
Tamping efficiency reduced by preheat or asymmetry
FUEL
Compression reduced by asymmetry of laser illumination, pellet fabrication, preheat hydromagnetic and/or hydrodynamic instability

istic of lasers operating at wavelengths appreciably greater than one micron. The principal problem areas are shown schematically in the figure.

The efficiency of pellet energy production will be markedly reduced if the laser-fuel coupling efficiency is reduced by reflection of the incident laser energy, energy drain from the corona by fast-ion acceleration, blockage of energy flow into the pellet core by a combination of anomalous resistance and magnetic fields, or by tamper preheat preventing efficient tamping action on the D-T fuel. All of these phenomena have now been seen in experiments and are partially understood from theory and computing studies. The pellet yield will also be reduced if fuel compression is reduced by a nonoptimized compression history, by preheat from fast electrons or X-rays or by asymmetric compression resulting from irradiation or pellet nonuniformity, possibly amplified by magnetic instability in the laser deposition region or by hydrodynamic instability. All of these possible phenomena probably have affected present experiments although direct confirmation has been difficult. Other possible problem areas are a result of inadequate attention to uniformity of pellet irradiation. The consequence of any one or a combination of these effects is given in Table 2, assuming that the D-T fuel has been uniformly heated to 5 keV, for an incident laser energy of 4 kilojoules. This table shows that scientific breakeven is achieved at 5 percent laser-fuel coupling efficiency for a D-T density between 1000 and 2000 grams/cm^3. A decrease of coupling efficiency to 2 percent and of compression to 600 grams/cm^3 reduces the fusion/laser energy ratio to 0.032, while an increase in coupling to 10 percent and a density of 2000 grams/cm^3 increases the ratio to 5.7. Table 2 also shows the critical importance of ρR, the yield dropping off very steeply below $\rho R \cong 0.3$ grams/cm^2.

Since both of the controlling factors in fusion yield, that is, laser-fuel coupling efficiency and fuel compression, are affected by several major uncertainties in the pellet physics, a prediction of fusion yield to better than one-to-two orders of magnitude cannot presently be made. The uncertainties are further compounded by the problems of pellet design and fabrication and by the aspect ratio imposed by stability considerations. The technological problems of pellet fabrication with layers of ablation material, dense tamper, and cryogenic D-T are unsolved. The laser peak power required for breakeven experiments depends on the pellet design, with high aspect-ratio shells containing cryogenic D-T layers giving the lowest power requirement. Since the uncertainties of pellet design and fabrication and especially of implosion stability have not been resolved, a definitive answer to the power requirement cannot at present be given. The most optimistic predictions, made by KMSF, require a peak power of 4 to 8 terawatts, while LLL and LASL believe that the requirement is at least one order-of-magnitude higher. Major technological uncertainties exist in the

TABLE 2

RATIO OF FUSION TO LASER ENERGY; FOR 4 KJ INCIDENT ENERGY, AS A FUNCTION OF FUEL-COUPLING EFFICIENCY ϵ_L AND DENSITY gm/cm^3 : THE PRODUCT ρR (gm/cm^2) IS ALSO GIVEN.

ϵ_L	$\rho = 2000$		$\rho = 1000$		$\rho = 600$		$\rho = 300$	
	Ratio	ρR	Ratio	ρR	Ratio	ρR	Ratio	ρR
10%	5.7	0.88	2.3	0.55	0.80	0.39	0.24	0.25
5%	1.3	0.70	0.6	0.44	0.20	0.31	0.07	0.20
2%	0.42	0.52	0.08	0.32	0.32	0.23	0.02	0.15

design and operation of large optimized laser systems, particularly in the 10-100 terawatt range. Past experience has shown that lasers designed at the limits of the state-of-the-art have encountered delays and been operated usefully for a large number of target shots only after substantial downrating. These uncertainties have a significant impact on the laser system and hence on schedules.

The development of laser-fusion has in several instances been retarded by excessive fragmentation, inadequate communication, and poor cooperation among the laboratories. This problem will become more acute with the increased need for highly sophisticated and extensive instrumentation and with the need for advanced codes and computing facilities for data reduction and analysis. Better definition of the roles of each laboratory within their resources of manpower and facilities and central utilization of the advanced code and computing capabilities of the ERDA laboratories (particularly LLL) should be considered.

For these reasons, we estimate a low probability of early achievement (CY 76 or 77) of significant thermonuclear burn and believe that demonstration of scientific feasibility is doubtful with the planned laser developments (excluding Shiva II at LLL). These estimates are consistent with the present schedules given by LLL and LASL. The number of unresolved scientific problems now identified, each of which can seriously affect scientific feasibility, raises serious questions concerning eventual demonstration of high pellet gain and hence of engineering feasibility of pure fusion systems. The effect of reduced pellet gain is much less serious, however, for hybrid fusion-fission systems which continue to be of major interest.

Our conclusions apply only to the unclassified target designs we have considered; we are unable, within the constraints of this study, to evaluate the impact of classified designs presently being considered by LLL, LASL, and KMSF.

Overall Engineering Assessment

PRESENT STATUS

The design studies at LLL, LASL, Westinghouse, and in West Germany have considered overall reactor configurations and some of the problems of first-wall protection. The analysis of the first wall is incomplete and the feasibility of any proposed design is essentially undemonstrated. The coupled problems of radiation damage and high peak stresses are recognized, but few data are available to support quantitative analysis. The sputtering and erosion of a dry first wall by fast charged particles and by soft X-rays are incompletely understood. The use of a lithium-wetted wall which protects the base structural metal, and from which lithium is vaporized by X-ray and charged-particle heating, is an interesting concept but inadequately analyzed. The flow properties of a layer of adequate thickness (about 1 millimeter) have been only partially analyzed and problems appear to result from relatively high velocity flow which is possibly nonlaminar. Either dry-wall or wetted-wall designs are expected to be markedly affected by details of pellet design, which can change the spectra emitted by the pellet. The use of low-pressure gas in the containment vessel to stop the charged particles and of high-temperature graphite to resist X-ray deposition have been very incompletely considered.

The overall designs are also uncertain because of the lack of definite predictions of probable pellet yields and configurations. These affect containment vessel size and pulse rate. The pellet configuration markedly alters the spectrum of X-rays and charged particles and, for pellets with very high yield, can moderate the neutrons. As an example, in a pellet with a yield of 1000 megajoules, the neutron flux can be appreciably attenuated in the fuel, with additional moderation occurring in the tamper.

The reactor design analyses have usually evaluated a complete reactor which has many features essentially identical with the LMFBR or with the CTR reactors. The costs of a 1000 MW(e) reactor are estimated by LASL and Westinghouse to be comparable to fission plants; i.e., $800 KW(e). The critical elements of the laser-driven system in the nuclear core, pellet fabrication and injection, laser-beam handling, and laser technology have, however, been given only cursory attention. The nuclear core and laser costs are in any case estimated to be only 15-20 percent of the complete reactor costs. This relatively small fraction is in turn dominated by the uncertainties in laser size, pulse rate, frequency, and pulse form, which make laser-system costing very difficult. The LASL and Westinghouse costs (the latter based on LASL-provided component costs) for a CO_2 laser and beam transport are high but still only 5-10 percent of the complete reactor costs. The laser associated uncertainties, however, certainly exceed the other cost uncertainties of the nuclear core.

The remaining problems of the laser-driven system, i.e., pellet fabrication, injection, final mirror survival, have been studied only very incompletely but are not believed to have a significant effect on reactor feasibility or cost.

The fission-fusion hybrid has been studied in concept but none of the engineering problems has been considered in sufficient detail to allow estimates of cost or feasibility.

PROGNOSIS

The feasibility of a laser-driven fusion reactor depends principally on the solution of the following problems: (1) the design of pellets which will give high gain (greater than 100) and can be mass-produced at a cost of less than roughly 0.02 cents per megajoule yield; (2) development or discovery of lasers at the wavelength allowed by the laser-pellet-interaction physics that can be built at efficiency greater than 5 percent and preferably greater than 10 percent, at acceptable cost (roughly \$50 $\times 10^6$ per megajoule) and acceptable cost per pulse (roughly 1 cent per megajoule pulse). The other elements of the reactor technology are considered to be soluble with known techniques, and in any case to be far less uncertain and less expensive in solution than the problems just enumerated. In a realistic program for reactor development, which must give major emphasis to the basic problems, early attention must also be given to the details of first-wall design and protection of the final elements of the optical system, and of pellet injection. These problems, together with many of the other uncertainties affecting design, can be studied in a test-reactor facility which could be built without stringent requirements on laser efficiency and cost. As an example, an iodine or CO_2 laser at 200 kilojoules and designed for a pulse rate of 5 per second could (optimistically) give single pulse fusion yields of one megajoule, an average fusion power of 5 megawatts, and 2.5×10^{18} 14 MeV neutrons/second. The input power to drive the laser would be about 20 megawatts. With a containment vessel of 50 cm radius, the neutron flux would be $8\times10^{13}/\mathrm{cm}^2$ sec and 2.5×10^{21} neutrons/cm^2/year. The experimental data on pellet yields should be available by 1978-79 to determine the feasibility of such a test-reactor design. Radiation damage with 14 MeV neutrons will, however, be available from the CTR program (assumed to be successful) before a laser-driven test facility could operate.

A more limited test program could be considered to study the combined effects of radiation damage and cyclic stress loading peculiar to the laser-driven reactor first wall. The feasibility of proposed wall designs would also be studied in more detail, to determine the range of workable configurations for later study in a pulsed test facility.

RESEARCH RECOMMENDATIONS

(1) Pellet Design It is recommended that the laboratories mutually generate a limited number of pellet-design cases which will span the range of presently foreseeable reactor-pellet configurations.

If possible, at least the output spectra, yields, and gains should be published to enable reactor studies to progress in the most reasonable manner possible. Estimates of individual radioactivity within pellets are also desirable as are pellet-fabrication cost estimates. The nature, amount, and energy content of pellet debris are important in determining the nature of containment-vessel evacuation and pellet debris recovery and processing. The possibility of LiD (tritium initiated) pellets or of other fuel needs clarification.

(2) First-Wall Design Nearly all questions concerning first-wall design are unresolved. The uncertainties arise only in part from the varying assumptions of single-pellet yields, X-ray and charged-particle spectra, and pulse rates. Study of the possible wall configurations is necessary to clarify the advantages and problems of the different proposals. These include liquid lithium-wetted walls, dry refractory-metal walls without and with graphite protection and/or magnetic shielding, and walls protected against charged particles by low gas pressure in the containment vessel.

The wall sputtering by charged-particle bombardment, erosion by soft X-rays or charged particles, radiation blistering by charged particle implantation, and radiation damage by neutrons are all serious problems and a unified study is required. Of particular note is the unique damage rate effects and the high helium content so characteristic of 14 MeV neutrons.

The interaction of high peak stresses characteristic of the laser-driven fusion pulsed operation also is an important area where additional work is essential. The fatigue life of irradiated metals at high temperature and with high helium content needs early investigation in order to recognize any major design limitations which may have to be imposed on laser-fusion reactors.

Of particular importance is early planning for the proper radiation-damage test facilities. Past experience shows that 5-10 years are required to complete such projects when the money becomes available and several years of operation are required before meaningful data can be obtained. Therefore, if laser fusion is to become a reality before the end of the century, test reactors must be built in the 1985 period to provide information for the power reactors in the late 1990s.

(3) Configuration of the Nuclear Core The requirements for pumping, laser-beam inspection, and pellet injection affect the configuration of the containment vessel. The pumping requirement is a strong function of the nature of the first-wall protection and of pulse rate. The energy content of the plasma, the level of radioactivity, and the pellet-debris processing requirements can also markedly affect the configuration of the core.

The protection of the last optical surface, the shielding and isolation of the final sections of the laser-beam transport system, and the safety problems associated with shielding and isolation of the laser may also have important effects on core design and cost. In fact, if quarter wavelength dimensional tolerances are to be maintained, one may have to invent a "new" last optical surface which has an economical lifetime.

The pressure pulses resulting from neutron-energy deposition in the lithium give some design problems. The effect on the lithium flow system, heat exchangers, etc. also needs to be analyzed. These effects are very sensitive to vessel size and coolant choice and need to be more carefully examined.

(4) Laser Technology The CO_2 and iodine lasers are possible candidates for a laser-driven test reactor, for which efficiency is not a critical requirement. The achievable pulse rates, pulse costs, and laser lifetimes need to be determined. These studies would also clarify the longer-term objectives of large-scale reactor use.

The long-term possibility of e-beam pumped large chemical lasers (such as HF) needs further evaluation. These lasers, which offer poor short-term possibilities for research or early development, may be applicable for the relatively long pulses of a large reactor.

Intensive study of other lasers, particularly those operating at wavelengths of $\frac{1}{3} - \frac{1}{2}$ micron, is highly desirable. The physics of pumping, energy storage, and energy extraction does not exclude the possibility of a "brand-x" laser with optimum characteristics.

(5) Gas Production The production of gas or other fuels by direct utilization of neutron energy via charged particle recoils and ionization offers interesting possibilities of good efficiency and reduced capital costs. The presently available knowledge is much too limited, however, to allow evaluation of the possibilities. Studies are required of the achievable efficiencies, the reactor geometry, the need for simultaneous tritium production, and / or simultaneous thermal-electrical conversion, and system costs.

(6) Fusion-Fission Hybrids The principal uncertainties unique to the hybrid are in the fission blanket. The achievable Pu build-up and details of the blanket need further study as well as realistic cost and environmental impact assessments. The pellet gains and laser efficiency required are low, and careful attention should be given to the laser-fusion experiments which may allow early demonstration of hybrid feasibility.

(7) Laser-Assisted Fusion Engineering Reactor Facility (LAFERF) A detailed design should be undertaken while schedules and feasibility are being determined, particularly in comparison with the similar development in CTR. Much more collaboration with the fission and CTR materials scientists is required to develop a cost-effective and pertinent testing program.

Appendix

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April 19, 1976

Professor Francis F. Chen
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Los Angeles, California 90024

Dear Frank:

Subject: KMSF Comments on "Assessment of Laser-Driven Fusion"
KAB and Associates, Inc., March 31, 1976

KMSF greatly appreciates the opportunity to provide comments on the subject report. Since at least the summary portions of the report will be widely read by other than physicists, we believe it important that this assessment provide a balance among the various opinions that might be offered on the feasibility of the laser-fusion process. We stress the word opinion since the report, as it is now drafted, contains many of the authors' opinions. Some of these are labeled as such, but many others are so interwoven with experimental fact or extrapolations of theory that it would be extremely difficult or impossible for a lay reader to distinguish. This deficiency of the report is most prevalent in the discussion of scientific feasibility. The sections on engineering feasibility and special topics or components are better balanced. But, since almost all of the \$262.6 M (Table II-1) spent by ERDA through 1976 plus the \$25 M invested by KMSF before becoming an ERDA contractor has been directed towards scientific feasibility, we feel it a most serious matter that an Assessment, under the imprint of the Electric Power Research Institute, be as objective as possible.

We agree with some of the opinions expressed in the report and disagree with others. The important issue is not whether we agree or not, but rather that all of the opinions be clearly identified.

Our overall opinion regarding the report is that it is unnecessarily and incorrectly discouraging regarding the current status of laser fusion. This tone seems to be derived from the authors' opinion that much of the current experimental work is "of little relevance." We believe the current experiments are extremely relevant and absolutely necessary in the normal progress of research. On a somewhat philosophical basis, we find the authors' statement of relevance to be analogous to telling the Wright brothers that their experiments at Kitty Hawk were irrelevant since they did not immediately demonstrate commercial capability of their vehicle. More directly, as one of the most active experimental organizations in laser fusion, with 850 neutron-producing experiments in the last year, we feel we can very easily demonstrate the relevance of this work.

KMS
industries inc.

Comments on Part 1, Section II — SUMMARY

We agree that the critical measurements necessary to demonstrate achievement of high density and demonstration of strong coupling of energy into the DT fuel are necessary. As far as KMSF is concerned, these experiments have not been given low priority and there has been substantial emphasis on the planning and diagnosis of experiments possible with laser facilities of intermediate size. It is our opinion that we will, in fact, demonstrate the achievement of these critical milestones within the next 18 to 24 months.

The readership of this report may include those to whom it would be an introduction to the status of laser fusion. Such readers should also be advised of the extremely rapid progress that has been made in this field in a relatively short time and with a relatively small investment. The first successful implosions and generation of thermonuclear neutrons was achieved in the KMSF laboratory only two years ago. At that time a different group of distinguished reviewers questioned whether the fusion process could be demonstrated even at the most modest range of thermonuclear neutron yield. All of the early concerns have been laid to rest. It is now our opinion that we and other laboratories know how to proceed from here. There is no question in our mind that the demonstration of both scientific and engineering feasibility as defined by the authors of the current report will be achieved.

We agree with the recommendation that a major effort in laser research in gas lasers other than CO_2 is clearly required. Because we had not yet presented to ERDA our own forward plan at the time of the site visit by the authors, we were not at liberty to discuss our thinking on this point. We have subsequently proposed an aggressive program to conduct high power target-interaction experiments using an iodine gas laser based on a combination of the excellent work of the Max Planck Institute in Garching and the Sandia Laboratories.

We agree with the authors' recommendation that research on the engineering aspects of reactor design should go forward. Certain selected elements of the fields recommended by the report should be promptly started lest they become the limiting problems in a commercial fusion reactor.

Comments on Part 2, Section II —
OVERALL ASSESSMENT OF SCIENTIFIC FEASIBILITY

B. Present Status

Relevance of Current Experiments

It is a fact that the small (50 to 250 micron) targets filled with DT gas and irradiated by sharply rising laser pulses is not the configuration to be used in breakeven experiments. This is well understood and generally agreed upon among all laboratories. This fact does not, in our opinion, make the experiments irrelevant. The experiments are highly relevant to our understanding of the stability of the implosion, of effects and mechanisms of preheating, of energy transport and final tamper conditions. These experiments permit calibration of the codes and thus give increasing confidence that strong thermonuclear burn can be accomplished with the larger targets and the specially shaped pulses required. As pointed out in the report, KMSF has understood the need for precise temporal shaping of the pulse for some time. The unique KMSF "pulse stacker" was designed to accomplish this very function. What has been overlooked in the report, however, is that current limitations on laser power preclude the effective use of such a pulse shape. This limitation is quite well understood. Our forward plan, as well as those of other laboratories we believe, can not pinpoint exactly at what laser power precise control of the pulse shape will become effective to produce the adiabatic compression necessary. We, therefore, take issue with the authors' use of the adjective "primitive" in characterizing current experiments. By connotation the word expresses an opinion which we clearly do not share.

The report found "strong disagreement" among the laboratories in current experiments. We cannot speak for the other laboratories, but it is a fact that the two laboratories carrying out the most significant target-interaction experiments, i.e., LLL and KMSF, are in good agreement including analysis and interpretation of experimental results.

Theory and Simulation

The report makes a subjective judgment and expresses an opinion on the status of the simulation codes by using adjectives such as "sophisticated" and "accurate" to describe the LLL LASNEX Code, while characterizing the KMSF codes as "simplified," "very schematic" and "relatively simple." We agree with the report's opinion of the LASNEX Code. We disagree with the report's opinion of the KMSF codes. LASNEX was originally developed as a theoretical test bed for a number of fusion-related concepts and recently has been calibrated against current experimental data. The KMSF code, on the other hand, was developed solely for the laser-fusion process and in very close association with the experiments. In our code, only those physical models required to interpret the data have been emphasized. Because LASNEX contains more exotic theoretical models does not mean it is more relevant or useful in either understanding the present experiments or in projecting the future course of the program. This opinion of ours is somewhat confirmed by the report (page 4-45 B) which points out that calculations of conditions necessary for scientific breakeven are in good agreement among the laboratories.

KMSF does not plan to duplicate the LASNEX approach. We certainly do not agree that the national laser fusion program should rely on a single central simulation. We will continue to use our 1-D code which now contains a very sophisticated radiation treatment and a model for fast ion production to study phenomena which remain independent of geometrical effects. The 2-D code, which is now routinely used for full implosion calculations of present experiments and hydrodynamic stability studies, will be used to investigate those aspects of the experiments which are sensitive to geometrical effects.

The most rapid progress will be made by those laboratories who have the closest possible interaction between simulation results and experimental data. The combination of this work is called Target-Interaction Experiments and cannot be subdivided. It is our opinion that each major laboratory in laser fusion should have such capability with results carefully reviewed and critiqued by the other laboratories.

Early in this section is a brief history of KMSF. This history is neither accurate nor was it provided by KMSF. In particular, we do not recognize, nor do we understand, "that a peak power of about 30 TW would be sufficient to obtain high pellet gains."

Diagnostics

We do not share the opinion expressed in the report that the only laboratory which may be expected to approach full diagnostic capability is LLL. The experimental work at KMSF has relied on highly sophisticated diagnostics which we feel should not be dismissed as unimportant. The laser fusion program will go forward with these state-of-the-art diagnostics and improvements in the area of good spatial and temporal X-ray resolution are expected to be operational in Fiscal Year 1977. At this time, KMSF is operational with a PDP 11/45 for extensive computerized data acquisition and reduction. The alpha particle, proton and neutron measurements are on-line with Tektronix R7912 transient digitizers interfaced to the PDP 11/45 computer system. KMSF has been noted for being innovative in the approach to diagnostics and many of the concepts originated at KMSF are in use at various other laboratories. A recent example of note is the use of large plastic bubble to both collect fast neutron activated tamper material and to measure target absorbed energy using infrared radiometry.

C. Plans

The power focused on the target by the KMSF laser is quoted incorrectly in this section. The current system is operating at 0.3 to 0.5 TW on target and is planned to be increased, in steps, during 1976-77 to approximately 4 to 5 TW in a two-beam system. This error results from a misquote during the site visit. There were several errors in recording quotes of KMSF personnel during the visit and these have been corrected later in this response.

We fail to understand how the authors of this report can second-guess the Congress of the United States and the management of ERDA with regard to funding and manpower allocations at KMSF. Even at this writing, such allocations have not yet been finally established. In any case, improved instrumentation is the very first priority for future target-interaction experiments at KMSF. We disagree completely that our development will be restricted.

We agree with the recommendation that increased emphasis should be given to the study of the laser-plasma phenomena at shorter wavelength, such as frequency doubling the glass laser output. KMSF is currently the only laboratory that has done high power experiments on spherical targets at $0.53\mu m$ wavelength (12 joules in 240 picoseconds). Further, similar experiments at higher laser power are scheduled within the next two months. KMSF plans substantial high power, short wavelength experiments in Fiscal Year 1977.

D. Evaluation

The report expresses the following opinion: "Laser-driven fusion faces a variety of uncertainties which separately present problems of major significance and together raise serious questions about scientific and hence engineering feasibility." Thomas Edison, Alexander Bell, Robert Goddard, Enrico Fermi and hosts of other individuals and organizations through the years would be amused to learn that because research faces "uncertainties" then it follows that there is a "serious question about feasibility." We agree there are uncertainties. We understand them very well. We have a specific program for their resolution which most certainly will not involve "inadequate attention" to any of the important parameters. It is our opinion that this view is shared by all of the other laboratories.

With regard to power requirements for breakeven discussed in this section, KMSF is, indeed, optimistic but the most important parameter of our forecast has been omitted. We require a peak power of 7 to 8 TW, not 4 to 8 TW, at a wavelength of 0.3 microns, which is not inconsistent with at least one order of magnitude higher at a wavelength of 1.06 microns. Our forward plan for the near term future involves a further modification of this idea whereby we will simultaneously irradiate the pellet at 1.06, 0.53 and 0.3 microns by means of an all-reflective, three wavelength illumination system developed in the present ERDA contract....*

*The final section of the KMS Fusion letter, not reprinted here, lists specific factual corrections to the statements of KMSF personnel quoted in the EPRI report.

A More Optimistic View of Laser Fusion

Editors' Note

To get a third point of view on the Electric Power Research Institute's report on laser fusion, the IJFE editors solicited responses from other scientists who were not in policy-making positions in the major laboratories involved in laser fusion research. The following response was compiled by the editors from an extensive interview with a prominent theorist in laser fusion. It is printed here without attribution because the ideas discussed, although not classified, might be construed under present restrictions as leading uncleared scientists into classified areas.

The EPRI report on laser fusion is contradictory in conception. While nothing it states is incorrect (and its analysis of *specific portions* of the program is accurate and valuable), the attempt to assess the overall status of laser fusion for power production using only conventional concepts is impossible. Unless one includes a number of advanced concepts of energy transformation (other than conventional thermal-electrical cycle), laser fusion cannot be accurately judged.

This internal difficulty in the EPRI report is evident most clearly in the discussion of pellet gain necessary for economical energy production. The report concludes that each pellet must produce 100 times more energy than that used by the laser light that ignited it. This unnecessarily stringent requirement leads to many problems: present unclassified pellet designs cannot achieve these large gains (even within a factor of two or three orders of magnitude); only target designs that are now classified are even conceivable; and, since such designs are classified, one cannot discuss the physics involved, the possible technical problems in fabrication, the materials, or their commercial use.

My estimate of the situation regarding laser fusion is much less pessimistic. There are at least three areas where, with a little speculation, it is possible to see much less stringent requirements on pellet gain. If we can make laser fusion economical with pellet gains of between 1 and 5, then the fundamental problems identified by the EPRI report cease to be a cause for general pessimism. Specifically, conceptual designs and some technical work have been done in the following areas:

(1) Direct conversion of reaction products to electricity or artificial fuel;

(2) Laser pumping with reaction products; and,

(3) Chain reaction pellet configurations.

My point is this: Since any assessment of laser fusion for commercial power production must be largely speculative at this time, restricting the consideration to very limited, conventional technical ideas is unfair; it leads to the generally pessimistic conclusions of the EPRI report.

It is worthwhile to briefly review the three areas listed above because they indicate the sort of advanced concepts that must be considered in an assessment of laser fusion.

Direct Conversion to Electricity or Synthetic Fuels

The general principle here is that the pellet microexplosion produces energy in forms that are immediately usable; changing it to thermal energy, then to steam, and then to electricity may be unnecessary. If the energy of the reaction products can be used directly, there is greater efficiency (and, hence, lower pellet gains). There are three principal reaction products that carry away the energy released in the fusion reaction: the directed motion of the charged particles (the debris from the pellet microexplosion), neutrons, and photons of various energies.

The direct utilization of the motion of the charged particles (direct conversion) is a well-known process for the production of electricity. Magnetohydrodynamic (MHD) generators use this process today, albeit with much lower-temperature plasmas than those in laser-produced plasmas. The unique feature of the laser application of this technique is twofold: the velocity of the charged particles is already ordered — since the explosion sends all the charged debris outward. Thus a magnetic nozzle is not necessary and the magnetic field configuration required to change the energy of motion into electricity is particularly simple. Second, since the voltage generated is directly proportional to the velocity of the debris ($\mathbf{E} = \mathbf{v} \times \mathbf{B}$),the near relativistic velocity of the debris means that extraordinarily high voltages are produced.*

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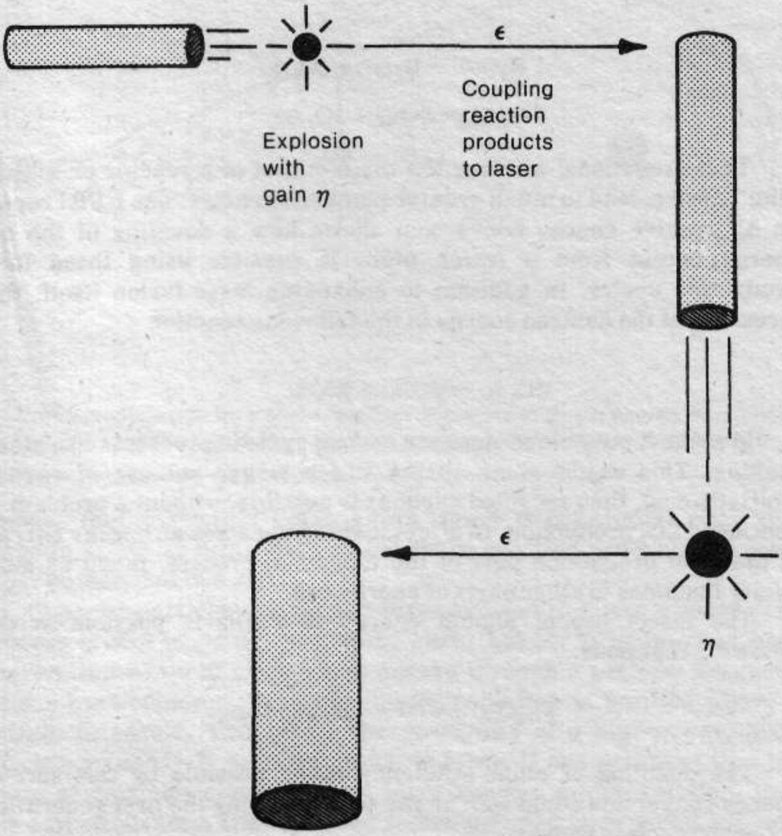
*See George H. Miley's book, *Fusion Energy Conversion*, Hinsdale, Ill. American Nuclear Society, 1976, for more details on this process.

FIGURE 1



Representation of multiple laser pumping with reaction products from pellet microexplosion. The overall gain is $\eta^n \epsilon^{n-1}$ where n is the number of pellets ignited.

Intense fluxes of radiation are the other form of energy release in laser fusion. Since both the neutron or photon energies are high, the processes for converting these to chemical energy in the conventional schemes are difficult and expensive. To stop the neutrons requires materials able to withstand very intense radiation damage, and similar problems occur with high-energy photons. However, EPRI had conducted detailed studies in using this radiation in other ways.*

There are a significant number of industrially important chemical reactions that are catalyzed by fast neutrons, and the most important of these can be used in the creation of synthetic fuels:

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*The studies are reported in *Enhanced Energy Utilization from a Controlled Thermonuclear Fusion Reactor*, September 1976, EPRI-ER-248.

$$H_2O(l) \rightarrow H_2(g) + \tfrac{1}{2}O_2(g)$$

$$CO_2(g) \rightarrow CO(g) + \tfrac{1}{2}O_2(g)$$

These reactions, whether the main output of a reactor or a "topping" device, lead to much greater plant efficiencies. The EPRI report on alternative energy conversion shows how a doubling of the net energy output from a fusion plant is possible using these fuel-production cycles. In addition to enhancing laser fusion itself, this direct use of the neutron energy in the following reaction

$$CO_2(g) \rightarrow CO(g) + \tfrac{1}{2}O_2(g)$$

would make it possible to close the carbon cycle in processes like steelmaking. This would mean that a much larger net use of carbon (initially coal, then recycled carbon) is possible, without a problem of continued CO_2 production. In effect, carbon becomes an energy *carrier* in the steel production part of the industrial process, much as electricity functions in other parts of energy use.

The direct use of photon energy is similarly possible in the following reactions:

$$2H_2O(g) + C(g) \rightarrow CH_4(g) + O_2(g)$$

The doubling of plant efficiency made possible by this sort of energy conversion could well be the key to making the first generation of fusion reactors economical.

Laser Pumping With Reaction Products

Theoretically any pellet gain that exceeds 1, if it could be efficiently coupled to another pellet with the same gain, could result in a large *overall* gain. That is, if the gain of one pellet is η and the efficiency of coupling the energy released by the ignition of this pellet to the ignition of a second is ϵ, then the overall gain of a chain of n such pellets explosions is $\eta^n \epsilon^{(n-1)}$. (See Figure 1.)

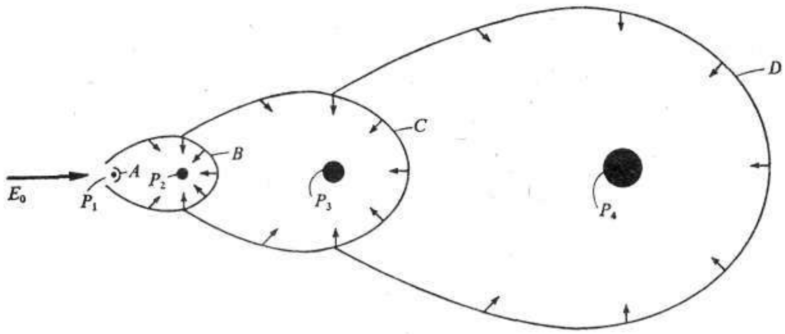
For economical electrical production the most stringent requirement we need is

$$\eta\epsilon^{(n-1)} > 100.$$

This is clearly much easier to achieve than $\eta > 100$.

This is the idea behind the concept of using the reaction products from the pellet explosion to pump a laser that ignites a second pellet. It is also possible, in some more exotic application, to take advantage only of the new types of laser pumping involved, although I won't

FIGURE 2



Conceptual design for a chain reaction of pellets in which energy from one (smaller) pellet is focused on the next (larger) pellet. *(Source: Winterberg)*

discuss that possibility here. Again, there is conceptual work that already exists on using each of the three reaction products for some laser pumping of this sort.

Charged particles have been already used to pump a laser. The process is still in its infancy, to be sure; but the *principle* has been proven. If a beam of electrons is passed through a periodic magnetic field, phase-coherent, monochromatic radiation is emitted above a certain threshold. This offers the possibility of a high-power, high-efficiency (above 20 percent), tunable laser. If one arranges that the beam of particles comes from a laser-induced microexplosion, then one can construct a chain of such fusion-pumped laser-pellet systems whose overall gain will grow geometrically with the number of stages.

In a conceptually similar way, the neutrons from a fusion microexplosion can be used in a process that uses nuclear transitions to generate coherent radiation in the X-ray region. This process has not yet been achieved in the laboratory. Finally, the photons from the laser-induced fusion could themselves be used to pump a conventional laser, the photons from one pellet explosion acting as a flash lamp for the laser that is used to ignite the next pellet.

Chain Reactions of Pellets

The same considerations outlined above also show that a direct coupling of pellet energies could substantially lower the requirements for the gain available in a single pellet. If it is possible to produce some sort of chain reaction of pellet explosions, then any pellet gain that exceeds one can be multiplied very quickly.

There have been a number of schemes proposed for such a coupling of pellets. Basically it is a geometric problem: how does one structure the pellet and its debris so that the energy is concentrated on a second pellet, and so on. Winterberg and Lo Dato have discussed several such proposals.* The simplest such idea is shown schematically in Figure 2. In this type of design, reflecting surfaces focus the energy from the (smaller) pellet onto the next (larger) pellet.

There are a multitude of variations of this basic idea that can be used to greatly reduce the initial energy requirements for the laser implosion and to reduce the requirements for individual pellet gain implied by single-stage construction. The EPRI report explicitly ignores these more adventurous applications of laser fusion energy, and that is the reason for their overall pessimistic assessment of laser fusion.

I want to emphasize that the report's criticism of bureaucratic heavy-handedness in laser fusion research is correct. The division of the governmental administration of laser fusion research between the Laser Office and the Division of Magnetic Fusion Energy is a continual obstruction to research. The sort of infighting that is evident from recent newspaper reports of the pressure within the government to deny funding to KMS Fusion is another aspect of the same problem.

I also agree very strongly with the EPRI assessment of the primitive state of diagnostic ability and basic scientific understanding of the laser-plasma process. It seems very possible that some of the most promising results in laser fusion will come out of the highly nonlinear effects of laser-plasma interaction. The self-focusing instabilities, various density gradient effects, have already made the heating of a pellet easier than expected even two years ago, and these improvements are likely to continue.

Finally, the report's harsh criticism of classification is refreshing. I agree that the problem is much more difficult with classification than without it. It is simply impossible to conduct really creative scientific work under the strictures of military classification. The difficulties of communication of new results, the atmosphere of constriction, and the inability to see the results translated into some application are all very serious impediments to research conducted under the wraps of classification.

There is no point in my going on about my points of agreement with the report. The report defends itself in these areas admirably. However, especially in the case of the layman reading the report, its generally pessimistic tone could be construed as denial that laser fusion is a viable contender as a reactor. This is incorrect, and can be seen to be so once the qualitatively new plasma technologies that fusion makes possible are taken into account. Without some attempt to deal with these qualitative new regimes, any effort at assessment will necessarily despair of the economic viability of laser fusion. We can't *plan* the details of these new developments, but we can and must *plan for* them and *plan on* them.

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* In *J. Plasma Phys.* Vol. 16, p. 81 (1976).