STATUS OF INTERNATIONAL FUSION DEVELOPMENT

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ABSTRACT

Based on an historical perspective, recent progress in fusion research is summarized and some factors bearing on the prospects for fusion power are discussed.

INTRODUCTION

The immediate goal of fusion research may seem deceptively modest: it is to construct a machine that produces more fusion energy than the energy it takes to drive the reaction. The crucial issue that blocked progress for decades is the formidable problem of achieving adequate thermal insulation at thermonuclear temperatures. It must be remembered that the goal of energy breakeven-sometimes called scientific feasibility-is only the first step on the road to constructing a practical fusion reactor. This single step has challenged thousands of scientists and engineers for 40 years.

Today the international effort employs some 5000 scientists and engineers in all the major industrial nations and world-wide annual expenditures are about $2 Billion per year. The reason that governments have been willing to back the effort at such great cost and over four decades is the enormity of the pay off for success, an inexhaustible and universally available fuel that can be burnt with a potentially benign environmental impact. The international effort is still essentially a research program, and research programs are historically much less expensive than development programs. So it is easy to understand why the development of fusion power has been called the greatest scientific and technological challenge ever undertaken by mankind.

This year (1987) marks a major milestone in the development of fusion power. It is expected that for the first time conditions will be reached which correspond to energy breakeven. But these are only "conditions". Experiments which include the tritium necessary for actual burning and energy breakeven are not scheduled until the period 1990-1992. Nevertheless the fusion community is now confident that the scientific feasibility of fusion power will have been demonstrated by 1990.

1. HISTORICAL PERSPECTIVE - BEFORE 1980

1.1 Basic Principles

The basic ideas for a fusion reactor originated in classified nuclear weapons research during the 1940's which had shown that the fusion fuel most easily ignited is a 50:50 mixture of the hydrogen isotopes, deuterium (D) and tritium (T). The reaction is:

$$D + T \rightarrow {}^{4}He + n + 17.6 \text{ MeV}$$

which produces inert helium ash and the neutron needed to breed more tritium fuel.

$$^{6}Li + n \rightarrow {}^{4}He + T + 4.8 \text{ MeV}$$
$$^{7}Li + n \rightarrow {}^{4}He + T + n - 2.47 \text{ MeV}$$

The net result of this fuel cycle is that the reactor actually burns deuterium and lithium (Li) to produce helium ash. Lithium is about as abundant as uranium - 238, not as plentiful as essentially inexhaustible deuterium, but more than ample for such a first-generation fuel cycle.

It was also recognized from the beginning that the fusion fuel next easiest to ignite is pure deuterium. This is certainly the preferred fuel from the point of view both of ease in handling and elimination of the need to breed tritium from lithium. However, the reaction cross-section for pure deuterium is over 100 times smaller than that for D-T so that the more demanding technology corresponds to a second generation of advanced reactors. For the first generation D-T reactors Canada's leading expertise in tritium technology (see following paper on the Canadian Fusion Fuels Technology Project) can be expected to find important applications.

The two basic requirements that must be met in order to obtain net power from burning D-T are known as the Lawson criteria and were published in 1957.

$$T \sim 10 \text{ KeV } (10^{8}\ {}^{\circ}K)$$
$$n\,\tau > 10^{14} \text{ cm}^{-3}\text{s}$$

The temperature, T, of the D-T nuclei must be greater than about 10 KeV in order to overcome their electrostatic repulsion and allow the nuclei to get close enough to fuse. The product of the density, n, of D-T nuclei (number per cubic centimeter) and the energy confinement (i.e. cooling) time (in seconds) must be greater than about 10^{14} cm $^{-3}$s in order for the power output from fusion reactions to exceed the power lost through thermal radiation and thermal insulation leakage. If the Lawson criteria are satisfied, then Q, the ratio of fusion power output to the plasma thermal power loss, will exceed unity and there is a net power gain. The condition Q=1 is known as scientific breakeven.

It is important to note that Q=1 is not sufficient for ignition of the D-T fuel. 80% of the fusion power is emitted in fast (14 MeV) neutrons which are lost from the plasma. Only the 3.5 MeV ^{4}He ash nuclei are available for sustaining the plasma temperature, so that the fusion power must exceed the power losses by a facor of 5 or more (depending on how well the ^{4}He energy is confined) before sustained burning or ignition can occur ($Q=\infty$).

The struggle to demonstrate the scientific feasibility of fusion power has been owerwhelmingly the struggle to achieve an adequate confinement parameter $n\tau$. There are three ways to confine a plasma. Gravity is effective for the sun and stars but inadequate for a reactor. Magnetic fields are effective for low density plasmas such that the pressures involved do not exceed strengths of materials. No confinement other than that by the plasmas own inertia is effective if the density can be driven high enough with laser or particle beams.

1.2 Magnetic Confinement

Magnetic confinement fusion (MCF) was the first technique pursued experimentally beginning about 1950. The plasma is confined because the ions and electrons gyrate around the magnetic field lines and cannot cross them unless some other force perturbs their orbits. The gyrations of the ions and electrons lead to a net plasma diamagnetic current, J, which interacts with the magnetic field, B, to give a confining force that balances the outward force due to the plasma pressure gradient, ∇p.

$$\underline{J} \times \underline{B} = \nabla p.$$

The magnetic field can also be viewed as exerting an equivalent pressure of $B^2/2\mu_0$. Then the pressure balance between the plasma and the magnetic field can be written

$$p = \beta \; B^2/2\mu_0$$

where β is the efficiency factor and is a maximum of 1 only if there is no magnetic field inside the plasma. (B is defined here to be the magnetic field external to the plasma.) To be economic a reactor would need a $\beta \sim$ 5-10% because at lower values the magnetic field stored energy and the stresses on the structure becomes too large.

During the 50's, experiments were carried out on a wide variety of magnetic field configurations. These included fast, high current pinches, steady state stellarators and mirrors, and slow diffuse toroidal pinches (Zeta and Tokamaks). The science of plasma dynamics was rudimentary and, although some basic principles were elucidated during the 50's, the experimental results were depressing. The approach taken had been intuitive, inventive and empirical. The quality of plasma confinement achieved was totally inadequate. By the early 1960's, the initial enthusiastic optimism had given way to a clear recognition that a better scientific understanding was imperative.

During the 60's, a seemingly endless series of instabilities were unearthed and studied for their effect on degrading confinement. The suspicion grew that the inherent instability of a plasma confined in a magnetic field might lead inevitably to unacceptable loss rates. Finally, this depressing state of affairs was broken in 1968 by reports from the Kurchatov Institute in Moscow that they had succeeded in attaining temperatures exceeding 1 KeV and confinement times of several milliseconds (along with thermonuclear neutron emission) on a tokamak device called T3. The results were verified by a laser diagnostic team from the UK that went to Moscow in 1969. This Russian breakthrough laid the foundation for this year's demonstration of scientific breakeven conditions.

During the 1970's the results for T3 were confirmed and extended on many different machines around the world. The data was assembled that made the design and construction of scientific test reactors feasible by the end of the decade. But perhaps even more important, during the 1970's the theoretical description of the dynamics of thermonuclear plasmas advanced to the point where analysis and predictions of most, but not all, the observed phenomena became possible. For example, the plasma loses energy to the reactor walls by ion and electron thermal conduction across the confining magnetic field. The ion contribution is well predicted by the so-called neo-classical collision theory, but the electron contribution is anomalously large by a factor of 10-100. Although the magnitude and scaling behaviour of this anomalous loss is known, there is still no adequate explanation for what causes it. Another important development during the 70's was the convergence of the physics. Earlier each type of machine had its own special theory. But by the end of the decade the theory was becoming sufficiently broad to apply to more than one type of machine.

An obvious conclusion from the history of magnetic fusion research is that confinement physics is exceedingly complex. A wide range of magnetic geometries have been explored but improvements have been evolutionary not revolutionary. Progress has been and continues to be steady but the historical record provides no encouragement for breakthroughs that could provide reactors tomorrow.

1.3 Inertial Confinement

Inertial confinement fusion (ICF) is the second, main-line technique being pursued to demonstrate fusion power. The basic idea, again originating from nuclear weapons research, is to create a nuclear micro-explosion by compressing and heating a small solid pellet of D-T fuel to the ignition temperature so rapidly that the pellet will burn before it can disassemble under its own internal pressure. Because of the enormous energy densities required, high-power lasers or charged particle beams are the only reasonable candidates to drive the compression and heating. Compression of the solid D-T fuel by a factor of 10^2 - 10^3 must be achieved in order to make ICF practical. The driver energy deposition requirement is an impractical 10^9 - 10^6 joules at solid D-T density, but is reduced to 10^3 - 10^6 joules by such a compression. The reason goes back to the Lawson criteria discussed earlier. The pellet disassembles in a time τ determined by the radius, r, and the speed of sound, v_s, at thermonuclear temperatures.

$$\tau \sim r/v_s$$

Thus the condition on the Lawson confinement parameter, $n\tau$, becomes a condition on nr. It follows that the energy, E, needed to heat the pellet decreases as the density squared.

$$E \propto nT \cdot \frac{4}{3} \pi r^3 \propto (nr)^3 T/n^2$$

Although compression reduces the required E to a practical range, it must be remembered that the efficiency with which the driver converts electrical energy to beam energy and the efficiency with which this beam energy is converted to compression and heating of the pellet are also crucial factors for constructing a practical reactor. Extensive numerical calculations indicate that driver energies of 1-10 negajoules, driver efficiencies of 5-10%, and pellet gains (fusion energy out/energy into pellet) of 100-1000 are required, and these appear to be feasible.

Inertial confinement fusion research was started about 1960 in nuclear weapons laboratories and was classified. The first incomplete public discussion was by the USSR in 1963, but the basic ideas remained classified until 1972. Even today certain aspects bearing in particular on non-sperically symmetric targets remain classified because of their connection to weapons concepts. The 1960's were a period of intense optimism similar to that for MCF in the 1950's but little experimental progress was made. During the 1970's several large experiments came on line and progress was rapid culminating by 1980 in the 10kJ Shiva neodymium-glass laser at Livermore which achieved thermonuclear temperatures of 10 keV, pellet gains close to 1%, and $n\tau$ values of about 2×10^{12} $cm^{-3}s$. However, at the same time, some serious problems appeared. Hot electrons generated by instabilities were preheating the pellet and thwarting the attainment of ultrahigh densities; assymetries in the implosion compression were degrading the central singularity of the imploding shock needed for efficient ignition. Nevertheless ICF benefitted from the major advances in the theory of plasma dynamics arising from the MCF research effort and ways to eliminate these problems seemed possible. The electron beam drivers were re-oriented to ion beam drivers, and long-wavelength laser drivers, to short-wavelength drivers. These latter drivers deposit their energy more effectively in the outer high-temperature ablating layer of the pellet where it is needed.

1.4 Basis For The Canadian Program

In the period up to 1980 the road to demonstrating the scientific feasibility of fusion power had been reasonably well charted. During this time Canadian involvement had been marginal, limited primarily to four small groups at universities and another at NRC. Their work had been academically oriented and focused on a few narrow areas of fusion research, most notably the physics of laser - plasma interactions. The most significant Canadian contribution had been to provide expatriate scientists to work in foreign laboratories. This situation started to change in 1978 when NRC announced the establishment of a national program of fusion research and development but it was not until 1981 that the first major project was actually approved.

The NRC fusion research program plan had to cope with a number of difficult factors. The government would not agree to fund a research program as in the other major industrial nations. Indeed Canadian research overall was being funded at about one-half the level of that in Canada's major industrial trading partners. The fusion program plan successfully dealt with this difficulty by proposing that the effort be divided into three major projects and that each project be equally cost-shared with a provincial or industrial partner. This the government found acceptable, and the result is that the Canadian program is unique in the world in being only 50% funded by the national government.

A second major difficulty was that fusion science is notably complex and broad in scope. The prospects were very dim for funding a comprehensive fusion program competitive with that in the USA, USSR, Japan and Europe. To cope with this, the NRC fusion program plan proposed only a modest long term goal - to develop the capability eventually to manufacture sub-systems and components for foreign fusion power reactors. As a result, the proposed program focused on three narrow specialties, one in each of the three broad areas of fusion research: magnetic confinement technology, inertial confinement technology, and non-confinement (materials/engineering) technology. This would enable Canada to attain "scientific awareness" and would promote "industrial preparedness", but it also allowed the government to reject even an implied commitment to a CANDU-scale development effort.

A third difficulty was that, after 30 years of research, fusion science was well-established abroad but almost non-existant in Canada. Thus, in order to ensure that Canadian work would make an early and significant contribution sufficient to justify access to foreign developments, the NRC fusion program plan proposed that each of the required three specialized projects be based on technologies where Canada had special indigenous expertise gleaned from work in other areas. This strategy was implemented by identifying Hydro Quebec's expertise in high-power electrotechnology as a base for specialization in magnetic confinement, Ontario Hydro's expertise in technology of the fusion fuel, tritium, as a base for specialization in fusion materials and engineering, and the NRC/university expertise in high-power gas lasers and their interactions with plasmas as a base for specialization in inertial confinement. Because of the support of Hydro Quebec and Ontario Hydro, the first two elements of the plan were successfully put in place. Because no partner willing to equally co-fund the ICF specialization has materialized, the third essential element remains in abeyance.

2. RECENT PROGRESS - SINCE 1980

2.1 Establishment of The Canadian Program

The magnetic confinement specialization was approved by the federal government in 1981 subject to equal co-funding by Hydro-Quebec. The project consists of constructing and operating a modest tokamak called the Tokamak de Varennes in the high-power laboratory of Hydro-Quebec. The specialized aspect of this machine lies in its capability to investigate long pulse discharge effects and give data needed by foreign programs for the design of the next generation Engineering Test Reactors. The project is more fully described in the following paper by R.A. Bolton. The Tokamak de Varennes is not a reactor and will never burn deuterium or tritium, but completion of its construction early in 1987 at a total cost of $46 million marked a milestone for the Canadian program in providing the first major piece of fusion research hardware. Canadian scientists are now in a position to make significant albeit modest contributions to magnetic confinement physics and technology and to access foreign scientific and technical achievements.

The specialization in materials and engineering for fusion was approved by the federal government in 1982 subject to equal co-funding by Ontario Hydro and the Ontario Government. Called the Canadian Fusion Fuels Technology Project, it focuses on the development and application of tritium technology for the fusion test reactors which are both in operation and being planned in foreign programs. Based on the world-class Canadian expertise in tritium technology and remote handling arising primarily from the development of the CANDU system, the first 5-year plan of this project, completed in March 1987 at a total direct cost of \$16.5 Million, established Canada as a leading international partner for developing fusion fuel systems. The development and achievements of this project are more fully described in a following paper by D.P. Dautovich. The prior CANDU investment of over \$1 Billion in tritium-related technology was a major factor in the projects success at attracting international interest, requests for collaboration, and willingness to co-fund collaborative projects.

Funding for the specialization in inertial confinement has still not been approved by the federal government despite an intense effort on the part of NRC, although funding for university scientists has been somewhat enhanced through NSERC grants and funding for the small NRC group has been maintained and augmented by equipment loans from the U.S.A. This situation is unfortunate in having prevented exploitation of the fact that Canada has leading scientists in high-power gas lasers and their interactions with plasmas. High-power, pulsed, gas laser technology actually originated in Canada with the invention of the TEA CO_2 laser shortly before the declassification of the basic inertial confinement concept in 1972. However, efforts described in a following paper by A.A. Offenburger are still being made to implement this third essential element of the original NRC fusion research program plan.

Following major government cutbacks to research and development in 1984 - in which however fusion funding was largely preserved - it was decided to transfer responsibility for the Canadian fusion program to Atomic Energy of Canada as of April 1987.

2.2 Test Reactors

While Canada was establishing its modest fusion program, the major fusion programs in other countries were building scientific test reactors. These machines with their first plasma dates are TFTR (1982) in the USA, JET (1983) in the European Community, JT-60 (1985) in Japan and T-15 (1988) in the USSR. They are all designed to reach energy breakeven, Q=1, plasma conditions and (except for T-15) are expected to do so in 1987. However, actual energy breakeven requires operating with tritium and this is not scheduled for TFTR until 1990 and for JET until 1992. JT-60 is not intended for D-T operation but rather, with its divertor for controlling plasma impurities and its excellent auxiliary heating capability, it is focused on exploring the limits of plasma confinement physics. The Soviet T-15 is still not in operation. It is the only one of the four with superconducting NbSn coils, a generally agreed necessity for a practical tokamak reactor but a daunting technological challenge to implement. Each of these machines has linear dimensions about 3 times larger than the Tokamak de Varennes and as a result cost 3^3 times as much, or about \$500 Million, to construct. The Canadian program has undertaken several collaborative tasks with TFTR and JET including the posting of several scientists and engineers working on tritium, diagnostic, remote maintenance, and advanced engineering problems.

Although these scientific test reactors should be able to demonstrate scientific feasibility, i.e. energy breakeven, it is unlikely that they will attain ignition such that the plasma burns of its own accord heated by the 3.5 MeV ^4He fusion ash. To complete the data base needed for constructing an engineering test reactor, the U.S. is preparing to construct a Compact Ignition Tokamak, CIT, designed to study the physics of an ignited plasma. The goal is an ignited D-T plasma burning for about ten energy confinement times. Canada has had some involvement in the preliminary work because of its tritium expertise.

Planning is underway for the next generation of engineering test reactors (ETR). Initial activity centered on the INTOR project proposed by the Soviet Union in 1977 and carried out under the auspices of the International Atomic Energy Agency (IAEA). The INTOR project envisaged joint construction by the USSR, US, EC, and Japan of a \$4 Billion engineering test reactor capable of resolving most of the engineering issues for design of an eventual commercial fusion power demonstration reactor. The project progressed from joint definition of the scientific and engineering data base requirements to pre-conceptual design based on each nation's own concepts, before activity was reduced to a marginal level in the early 1980's due to east-west political problems. An attempt was made with some success, starting in 1982 and under the auspices of the Group of Eight, Economic Summit, to promote joint cooperation and planning in the West. The resulting consultation structure served as a base for developing a common western response to the Reagan-Gorbachev Initiative (RGI) of 1985. The RGI was essentially an effort to revitalize the moribund INTOR project by taking another step towards a common ETR. The proposed, first goal is to define by 1990 a single common technical design in a joint effort under the auspices of the IAEA. Negotiations to implement this are underway. Europe is well advanced since starting its own design effort (NET) in 1983, as is Japan (FER). Canada did not participate in INTOR since it had no fusion program at the time, but it has been active in the RGI discussions and has been invited to participate.

Inertial confinement fusion devices differ markedly from MCF reactors in that at least in principle the "reactor" part is simply a cavity that can be well separated from the complex driver. ICF "test reactors" are better described as driver facilities. Both the US and USSR have large laser and large accelerator driver facilities; Japan has large laser drivers (Gekko (neodymium-glass) and Lekko (CO_2 gas) lasers at the Institute for Laser Engineering in Osaka.) The largest laser facility is the Nova neodymium-glass laser at the Lawrence Livermore National Laboratory in the US, which can now deliver about 25 kJ of 0.35 μm frequency-tripled radiation. It should eventually be able to produce 50-80 kJ in 1.5-3 ns pulses but it is still doubtful that this will be sufficient for energy breakeven (fusion yield out equal to laser energy in). Part of the uncertainty is due to the classification of possible high-gain target pellets; part is due to the stringent requirements that must be met to maintain sufficient implosion symmetry and to avoid pre-heating the cold D-T fuel during compression. Driver energies of 5-10 MJ are probably required for practical high-yield micro-explosions of 400-1000 MJ. The PBFA II light ion accelerator at Sandia National Laboratories in the US is currently the only driver potentially capable of delivering more than 1 MJ. It is undergoing initial tests and should eventually deliver 1-2 MJ on target in 10 ns.

2.3 Scientific Status

The simplest global indicator of achievement in magnetic confinement fusion is the so-called Lawson diagram (see Figure 1) in which plasma ion temperature is plotted against the confinement parameter $n\tau$. The line marked $Q_{NM}=1$ represents the threshold for energy breakeven in a non-Maxwellian (i.e. non-thermal)D-T plasma having an excess of high-energy ions dueto auxiliary heating by energetic deuterium ortritium beams. More difficult to attain is the energy breakeven threshold in a thermal-equilibrium, Maxwellian plasma shown as the line marked $Q_M=1$. The eventual goal for a reactor is achievement of parameters exceeding the $Q=\infty$ threshold contour for D-T ignition where the burning becomes self- sustaining.

In 1955 the confinement parameter was 1000 times too small and the ion temperature 100 times too small to plot in Figure 1. By 1965 the values were only 10 times too small to plot, and ten years later in 1975 values in the lower left hand corner on the figure had finally been achieved. By 1980 improvements in temperature and confinement parameter by about a factor of 10 had been achieved but these advances were on different machines and could not be combined. Combining further improvements in both ion temperature and confinement parameter was the goal of the large scientific test reactors, and by the end of 1986 their operational development had progressed to the points marked in Figure 1. (Results for only TFTR and JET are available because JT-60 is only in the commissioning phase and T-15 is not yet on line.)

In a plasma undisturbed by auxiliary heating, TFTR reached a record $n\tau$ of $1.5 \times 10^{14} cm^{-3}$sec, much more than that needed for energy breakeven, and only a factor of two below that needed for full plasma ignition. But this was achieved at an ion temperature of only 1.2 keV, about 5 times too small for energy breakeven. Under different conditions of low plasma density and intense auxiliary heating, reactor-like, ion temperatures of 20 keV were achieved. But the $n\tau$ was only $1 \times 10^{13} cm^{-3}$sec, about half that needed for non-thermal, energy breakeven. These conditions correspond to a Q of 0.2 had it been a D-T plasma.

The results for JET are similar. For ohmic discharges undisturbed by auxiliary heating $n\tau$ is high ($3 \times 10^{13} cm^{-3}$sec) but the ion temperature is low (3 keV), while for the hottest plasmas (10 keV) with neutral beam heating the $n\tau$ drops to an inadequate $1 \times 10^{13} cm^{-3}$sec giving an effective Q, if it were a D-T plasma, of 0.15. These results show that heating the plasma with either RF power or neutral beams, while successful in raising the temperature, is simultaneously enhancing the thermal energy transport losses and degrading the confinement. From Figure 1 it can be seen that heating effectively drives the confinement time down inversely proportional to temperature. This is not a direct temperature effect. The heating process, using either RF or neutral particle beams, drives up the intensity of microscopic fluctuations, and it is these microscopic fluctuations which are responsible for the cross-field diffusive transport losses. Although the details are still not fully understood, there is sufficient qualitative understanding to support the expectation that experimental progress in 1987 should allow adequate ion temperatures and confinement parameters to be reached simultaneously.

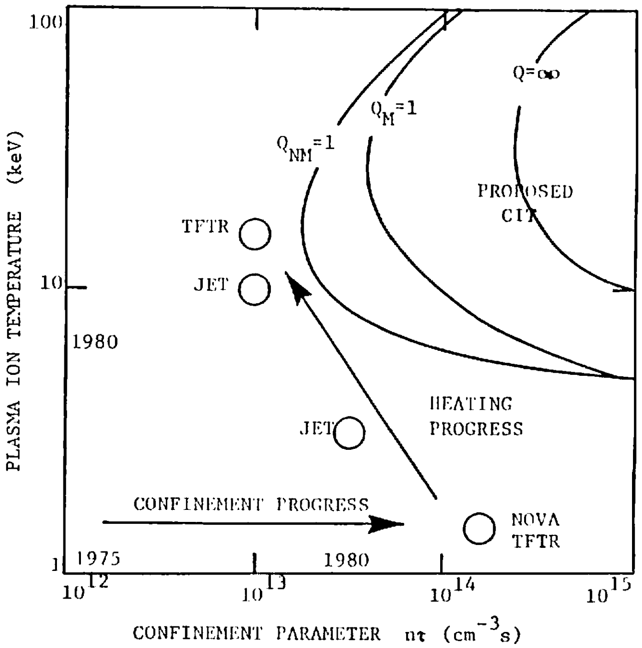

FIGURE 1. STATUS OF LEADING FUSION SCIENTIFIC TEST REACTORS AT THE END OF 1986 ON A PLOT OF ION TEMPERATURE AGAINST THE LAWSON CONFINEMENT PARAMETER $n\tau$

In inertial confinement fusion, progress has also been impressive. In 1986 the first indirectly (x-ray) driven implosions on Nova using only 18 kJ of 0.35 μm radiation gave a (calculated, equivalent) $n\tau$ of $1.5 - 3 \times 10^{14} cm^{-3}$s at an ion temperature of 1.5 - 1.7 keV. When the full 50-80 kJ Nova capability is reached, substantial improvement in target performance is expected. The direct-drive experiments on Nova undertaken to develop neutron diagnostics and using simple D-T filled glass microshells as targets have given maximum ICF fusion yields of 30J corresponding to a rather modest Q of 0.0018. The maximum equivalent $n\tau$ achieved in ICF is thus comparable to that for MCF, but the maximum Q achieved is considerably lower. Also just as for MCF, there is now reasonably good agreement between ICF theory and experiment. However, the diagnostics of micro-explosions is very difficult; more accurate diagnostic techniques now under development for measuring density and temperature are still needed to verify the agreement between theory and experiment.

3. PROSPECTS

Timing for the first commercial exploitation of fusion power and its subsequent market penetration depends on so many complicated factors that it would be foolish to predict dates with any pretense at confidence. If the financial and political support for foreign programs is maintained at levels and extended at rates of the past decade (so that the development pace is unchanged), and if an historical extrapolation of the scientific and technical surprises and advances during the last decade holds, then the scientific and engineering data base necessary for constructing a demonstration power reactor will be available about the year 2000. Again, with the same assumptions, a demonstration power reactor would be operating by about 2015. Finally, assuming the same market penetration rate as that achieved by fission power reactors implies that fusion power would have a significant commercial impact by the middle of the next century. Thus the conventional wisdom is that a fusion power data base is about 10 years away and a demonstration power unit, 20-25 years away. The uncertainty in these projections is related to a number of unresolved crucial issues whose examination can perhaps give a better insight into the prospects for fusion power. These issues can be grouped in several areas: scientific and technical, environmental and safety, economic, and political.

3.1 Scientific and Technical

The crucial scientific issue in MCF is the physical limit on the reduction of cross-field transport losses imposed by the very nature of magnetic confinement (i.e. density and thermal gradients). Using only current experimental results and understanding, commercial reactor reference designs are marginally economic and certainly cannot exploit the attractive features of the advanced D-D fuel cycle. Most transport losses are understood qualitatively and many, quantitatively. For example, radiative and ion thermal conductivity losses can be predicted quantitatively in parameter regimes of interest for Tokamaks, but sawtooth oscillations, disruptions, and electron thermal conductivity can only be predicted qualitatively. An experimentally-verified theory capable of quantitative transport loss predictions is needed before designers can be confident of minimizing losses and understanding the scientific limits on performance. Scientific understanding is essential to define the prospects of fusion power, but by its very nature it cannot be rigidly scheduled, and today it is only marginally adequate.

Technical issues are at an earlier stage of definition. One of the biggest concerns centres on the feasibility of constructing a practical, long-lived first wall in the hostile fusion environment. The neutron, atomic, and radiation fluxes are intense and much more data on their effects is still needed. Other technical issues concern the very large, high-field superconducting magnets, the blanket for breeding tritium and the fueling and exhaust systems. All these issues have been addressed in a preliminary way, but they will be the focus of intense effort in the 1990's. Attainable first-wall lifetimes is probably the most crucial technical question for the longer term economic prospects of MCF.

For ICF the most crucial scientific question is the minimum driver energy needed to ignite a practical, high-gain target. Steady, step-by-step progress in the experimental results, constrained by instabilities in the compression/heating process and the availability of improved drivers, has led to increases in the estimates of minimum required driver energy by about a factor of ten every six years since the late 1960's. Current indications are that driver energies in the range 5-10 megajoules may be required which would place very severe demands especially on laser driver technology. The results of recent innovations such as induced spatial incoherence (ISI), which could reduce driver energy requirements by suppressing non-uniformities in the implosion, or krypton-fluoride laser pulse compression, which could reduce driver costs, should help clarify the practical prospects for laser fusion. For accelerator drivers, beam transport issues are the most crucial and are still at an early stage of exploration. Compared to MCF, few ICF reactor reference designs have been explored so that technical issues are less defined. However it has been argued that stresses due to the thermal cycling intrinsic to ICF could be the major technical issue.

3.2 Economics

The international effort has concentrated on making fusion work, not on making it cheap, which only reflects the fact that fusion is still in the research/engineering phase. However, there have been about eight major conceptual reactor design studies for D-T tokamaks and a dozen for other systems, which have been costed and provide reasonable projections for fusion power costs. The results of a recent European NET study are a good indicator of the economic prospects of fusion power. Using only the present understanding of plasma physics and technology, and assuming a first-of-a-kind 1200 MW (electric) tokamak reactor, the generating cost of electricity was calculated to be 2-3 times that of today's thermal fission and coal stations. This, of course, is based on very pessimistic assumptions. Series production alone might close the gap, especially in view of the fact that the present very high cost of large superconducting magnets is due principally to the fact that the current, miniscule market is for fusion research. More important though are the major cost reductions possible by advances in confinement physics. Increasing the plasma β by a factor of three, would reduce the generating cost of electricity by one third. The economic prospects for competitive magnetic fusion power costs are at least encouraging. There have been some arguments against economic feasibility based on (a) low power density (b) long energy payback times, and (c) complexity, but they do not stand up under close scrutiny even though they cannot absolutely be refuted without pointing to an operating unit. The argument that costs are too high for low-power-density devices characteristic of fusion is contradicted by the flat cost/power-density relations for fission reactors. The argument that the energy payback time is too long is contradicted by the fact that, when fuel manufacturing and processing is included, the energy payback time for fusion is significantly less than that for equivalent fission systems. The argument that fusion is too complex to be reliable cannot be refuted quantitatively without an operating system, but seems doubtful when one notes that increased complexity of aircraft has not led to a decrease in reliability.

3.3 Environment and Safety

Fusion has been praised as a clean, safe source of power. While this is essentially true in the sense that fusion has considerable advantages over fission and fossil-fueled generating plants, fusion is neither perfectly safe nor perfectly clean. It is only a lot better.

A D-T fusion reactor would contain radioactive tritium which decays with a half-life of 12 years by emitting an electron. Tritium is excreted from the human body with a biological half-life of about 10 days and does not concentrate in food chains. A 1200 MW reactor plasma would contain a miniscule 1 gram of tritium although the total plant might contain as much as 3 kilograms including that in seperate, bunkered, fuel store rooms. Maximum credible accidents might release about 200 grams. A comparable accident (50 grams) has already occured at a nuclear weapons facility (Savannah River Plant). The result was a maximum contamination dose on the plant boundary less than 1% of the natural, background-radioactivity, annual dose. This is very small. Studies of potential catastrophic fusion reactor accidents conclude that major equipment failures, fire and radioactive releases could be dangerous for plant personnel but would not disrupt society in the immediate vicinity.

The situation under normal plant operating conditions is equally advantageous. A study for the European Community published in November 1986 concludes that the maximum dose to the local public would be much less than the normal, natural variation in background radiation from place to place. Also there would be none of the biotoxic chemical emissions characteristic of fossil fuels. The most important feature, though, may be a fusion reactor's intrinsic safety; no matter what fails or goes wrong a fusion reactor is self-quenching. Confinement research has amply demonstrated that only near-optimal operating conditions are capable of sustaining the burning plasma. Neither fusion nor any other source of power can be perfectly clean and perfectly safe but the prospects are that fusion will indeed be a safe, clean source of power.

3.4 Political

Three fusion-related factors which particularly influence political views on the next decade of fusion development are that an ETR will cost about $4 Billion, will take 10 years to construct and commission, and will only produce a fusion science and engineering data base. The cost is large, even for the research budget of a major industrial power like the U.S. The time scale is long compared to government election cycles but appropriate for serious initiatives to improve international relations. The goal of a data base is compatible with national self-interest in that it minimizes jeopardy to a nation's potential commercial advantage. There are other non-fusion-related factors, ranging from trade frictions to USSR détente initiatives, which may also have a favourable influence on accelerating the development of fusion power through world-wide joint action to construct an ETR.

From the Canadian point of view, enhanced international collaboration would be particularly beneficial. Government reticence has limited federal funding on a per capita basis to 20-30% of that in other countries, so that accepting the risk of relying on international collaboration has been essential. If other countries adopted a similar approach, the return on investment for Canada would be greatly enhanced. The major European nations have already accepted a supranational approach in consolidating their national efforts under a common Euratom program. The Japanese have proved their willingness to collaborate by concluding agreements with the US at the $100 Million level. US/EC/Japan/Canada cooperation under several International Energy Agency agreements has been very successful, and Canada has already negotiated separate bilateral agreements with the US, EC and Japan. The fact that negotiations for world-wide collaboration on an ETR are progressing well at the preparatory technical level and also the initiatives at the political level of Mitterand in the Economic Summit of 1982, and Reagan-Gorbachev in Geneva in 1985, suggest that the climate is favourable for enhancing the pace of fusion power development through international collaboration. The fact that Canada has been invited to participate in these ETR discussions reflects the success of the NRC fusion program plan and is an important step in keeping open the opportunity for Canadian industry to access the international fusion science and engineering data base.

The ETR project is only one example of the opening market for fusion-related equipment and services identified by Canada's fusion research effort. Whether it or a similar project such as NET proceeds, because of the projects described in the three following papers, Canada is well placed to realize industrial benefits. Hydro Quebec and Ontario Hydro deserve much of the credit since it was their support which launched the original NRC fusion program.