

# White Paper on A New Approach for the Magnetic Fusion Program

Dale M. Meade

## Magnetic Fusion needs a new approach.

Fusion has the potential for producing a long term solution to the world's need for a widely available, environmentally benign energy supply. The present justification for fusion research is based on the assumption that other sources of energy will be unavailable or unacceptable after about 50 years, and therefore fusion will be acceptable even at costs several times that of present energy supplies. Over the past twenty years, the other energy technologies have had their own innovation programs which have improved their product and lowered costs, and this should be expected to continue. Fusion needs a product that can compete directly with other technologies without relying on the other technologies to fail due to concerns about potential environmental deficiencies.

Magnetic fusion has made significant progress in increasing the fusion power produced from 0.1 watts in 1975 to over 10 million watts in 1995, and has also increased correspondingly the degree of scientific understanding. However, during the past decade magnetic fusion has come under increasing criticism that progress has slowed (haven't even been able to ignite a fusion fire) and that the product, still at least 20 years away, is not attractive. The magnetic fusion program needs a plan that addresses these issues in a convincing way. The present magnetic fusion program is dominated by ITER whose goals of "demonstrating scientific and technological feasibility" have lead to an very large and expensive device that will require long time scales for completion (Fig. 1). The notion of a single large scale integrated engineering test reactor with the combined requirements of significant neutron fluence, tritium breeding and the flexibility of studying ignition in advanced tokamak regimes is inconsistent with the present situation in magnetic fusion. Trying to do everything on one facility has made that facility very expensive and has significantly increased the risk and impact of a single point failure that could cripple the world's magnetic fusion program. Now is the time to complete the ITER EDA, and to separate the ITER mission into smaller functional pieces that address the critical technical issues of magnetic fusion. These functional sub-elements would have smaller cost requirements that are fundable with far less technical risk and could be accomplished on a faster time scale. Examples of this general strategy have been described previously by P.-H. Rebut<sup>(1)</sup>(Nov., 1990), various U.S. Fusion Laboratory Directors <sup>(2)</sup> (May, 1995) and by PCAST<sup>(3)</sup> (July, 1995).

**Recommendation 1:** Establish a process to develop a contingency plan to replace the present ITER program with a multi-element burning plasma and fusion technology program. Determine the most critical technical issues to be addressed in a multi-element burning plasma and fusion technology program

The discussion below, buttressing this recommendation, is an example of a new multi-element burning plasma program. Not only is it important that the new program be oriented toward a better, cheaper product on a faster time scale, but that there are real, easily understood deliverables that will occur over the next 5 years to justify the cumulative expenditure in the U. S. fusion program during that period of over one billion dollars.

## Ignition - The Litmus Test for Fusion

One should keep in mind the parallelism between the development of magnetic and inertial fusion. The National Ignition Facility (NIF), the next step in inertial fusion, is a transient core ignition experiment that is not constrained by reactor relevant considerations. The operative definition for ignition in NIF adopted by the Committee for the Review of the Department of Energy's Inertial Confinement Fusion Program (NRC, 1997)<sup>(4)</sup> is that the fusion energy yield exceed the laser energy incident on the target; at this level alpha heating is sufficient to cause a significant ion temperature excursion in the central core. The study of the resulting dynamic phenomena would allow the burning process to be optimized. The first laboratory experiments on ignition in NIF scheduled for ~2005 may make magnetic fusion the alternative approach unless magnetic fusion has made similar progress (Fig. 1). Since there is a finite possibility that the ITER Project will not go forward, the magnetic fusion program needs a contingency plan that will carry forward the most important elements of the ITER physics mission. Two significant

sub-elements of the ITER burning plasma physics mission, fusion plasma physics and core ignition physics, can be identified that are within the capability of modest upgrades at the TFTR or JET facilities. The U.S. fusion program should have a specific objective to develop innovations leading to an advanced ignition experiment that could be carried out within the general capabilities of an existing fusion facility such as the TFTR D-site facility or the JET facility. It is not that ITER has failed and we must go back to square one, or that tokamaks have failed and or that magnetic fusion has failed. Rather, through an improved understanding of the science of fusion plasmas, something better, faster and cheaper has been found (e.g., transport barriers). There are innovative ideas that could dramatically change the outlook for magnetic fusion and these ideas should be developed and tested on fusion-grade plasmas ASAP. The emerging understanding of confinement and the development of techniques to improve confinement offer the possibility of profoundly changing the image of magnetic fusion. A magnetic fusion concept that could ignite in a plasma not much larger than existing D-T compatible fusion devices (TFTR and JET) would be a clear indication that magnetic fusion was on a path to practical applications of fusion power. The goal for the next decade of magnetic fusion research should be limited to the next logical step in the physics of burning plasmas, the study of the dynamics of fusion plasmas with significant core alpha heating ( $P_\alpha > P_{\text{ext}}$ ) for modest pulse lengths (several plasma energy confinement times). This would provide the world magnetic fusion program with a test bed where fundamental physics and innovations could be studied and understood in the "real thing," a reacting fusion plasma. Then, after the physics of a burning plasma is understood sufficiently to facilitate performance optimization the required technology could be specified and developed.

#### Transient Core Ignition: A Near Term Deliverable for Fusion Science.

The major focus of the inertial fusion program for the next decade will be the achievement of transient core ignition, and the study of the resulting burning plasma physics. Present tokamak devices have achieved significant D-T burning with the core plasma parameters ( $T_i$ ,  $\beta$  and  $n_\alpha/n_e$ ) and alpha heating density ( $P_\alpha/m^3$ ) equal or greater than those projected for the ITER reference ignition scenario. However, the core plasma energy transport is about a factor of 5 higher than that needed for the alpha heating to equal the plasma losses in the core. If turbulence can be reduced so that particle-particle collisions are the main limitation on confinement of the energy in a D-T plasma then, even in a tokamak roughly the size of TFTR/JET, the plasma core could be transiently ignited using the same definition and philosophy as that developed for NIF. The enhanced reversed shear (ERS) experiments in deuterium and D-T plasmas on TFTR did demonstrate significant reductions in turbulence and ion energy transport in the plasma core. The ion core transport was reduced well below Chang-Hinton neoclassical calculations to values near more refined calculations of neoclassical transport. Additional JET D-T experiments, now in progress, should provide additional information on the capability of improved confinement modes in D-T. A key requirement, and hence result, of an experiment at the TFTR/JET scale would be to control pressure and current profiles simultaneously in the presence of an alpha heated D-T plasma. Achievement of the theoretical potential of the ERS/Optimized Shear plasma regimes in a D-T plasma should be sufficient to investigate many crucial reacting plasma issues, and would form the basis of a strong and exciting burning plasma physics program.

**Recommendation 2:** Carry out a technical assessment to determine the potential for significant burning plasma experiments in the 1998 - 2005 time frame made possible by modifications to the TFTR/JET facilities. Evaluate possibilities for enhanced collaboration with the JET D-T program.

#### Sustained Plasma Ignition: A Proof of Fusion Experiment

The achievement of core ignition or even significant alpha heating in a TFTR/JET scale device would do much to restore confidence in magnetic fusion, and would provide the impetus to move to the second stage of burning plasmas - sustained plasma ignition. Such a facility is crucial to sustain the development of magnetic fusion and would provide the basis for specifying the technology development needed for a magnetic fusion test reactor. Can you imagine specifying the technology of a coal burning plant without first studying the dynamics of combustion, i.e., studying the real thing? The minimal goals for a sustained ignition experiment might be  $Q \sim 10$  in D-T, pulse length  $\sim 20 \tau_E$  at a construction cost  $< \$1B$ . This is envisioned as a new device at a site with significant site credits. This is within the general range of the IGNITOR device that is being designed, and prototype components are being built with Italian

funding. The 1992 proposal for BPX-AT identified this possibility if high beta ( $\beta_N \sim 3.5$ ) and good confinement ( $H \sim 3.5$ ) could be produced simultaneously in D-T. This device could incorporate transport barrier physics from advanced tokamaks, and advanced physics from experiments on spherical tori. The experimental program could be developed as an initial phase where only moderately advanced plasma performance is required to achieve  $Q \sim 10$  for  $\sim 10$  s, and then as advanced performance was achieved the field could be reduced and the pulse length extended for sustained burn. This facility would be able to investigate the physics of a self-driven plasma and could determine techniques for making advanced tokamak modes compatible with strong alpha heating. Again, the temptation to do everything simultaneously must be resisted.

**Recommendation 3:** Carry out a technical assessment to determine the potential for a <\$1B class sustained plasma ignition experiment in the 2005 - 2015 time frame.

#### Alternate Applications of Fusion Power - A Near Term Fusion Product.

The previous discussion focused on the traditional fusion application of using 14 MeV neutrons to boil water where it is difficult to compete with existing technologies. Significant attention should also be given to developing and evaluating alternate applications of fusion that derive value from energetic neutrons such as: nuclear waste transmutation, destruction of weapons grade plutonium, tritium breeding, isotope production, neutron research, etc. Several years ago, studies showed that nuclear waste can be transmuted using fusion systems with a blanket containing the nuclear waste or that tritium can be produced using a fusion plasma driven by neutral beams. Can magnetic fusion compete in this area? If these alternate applications were useful at fusion powers in the 50 to 100 MW range, only a modest extension of existing physics would be required. This type of application would also serve as a driver for almost all the technologies (long pulse, remote handling, low activation, tritium handling, etc.) needed eventually for fusion power. The determining issue comes back to economics; can fusion compete? If it can, here is a real fusion product that can be delivered in the near term.

**Recommendation 4:** Carry out a technical assessment to determine the potential for alternate applications of fusion, particularly 14 MeV neutrons and the extent to which this type application would drive fusion energy technology.

#### **Summary**

The technical assessments recommended above would provide an important part of the basis for defining the technical elements of a new multi-element burning plasma and fusion technology plan. The achievement of the multi-element burning plasma and fusion technology program such as that described here would: provide visible and easily understood deliverables, allow the study of the science of burning plasmas during the next decade, and address many of the critical issues of the magnetic fusion program with lower technical risk, at lower cost and on a faster time scale than the present ITER project. This would allow for a major decision in  $\sim 2015$  on the further development of fusion, both inertial and magnetic fusion (Fig. 2).

#### References:

1. P.-H. Rebut, M. L. Watkins, D. J. Gambier and D. Boucher, Phys. Fluids B 3 (8), 2209, 1991
2. D. E. Baldwin, R. C. Davidson, G. A. Navratil, M. Porkolab, M. J. Saltmarsh and K. I. Thomassen, "White Paper on Magnetic Fusion Strategies", May 16, 1995
3. Report of the Fusion Review Panel of PCAST, July 1995
4. S. Koonin, et al, "Review of the Department of Energy's Inertial Confinement Fusion Program", National Research Council, 1997. Fig. 1 and page 11.

October 15, 1997. First Edition --February, 1997 Fusion Approach

# Present Approach to Fusion Energy

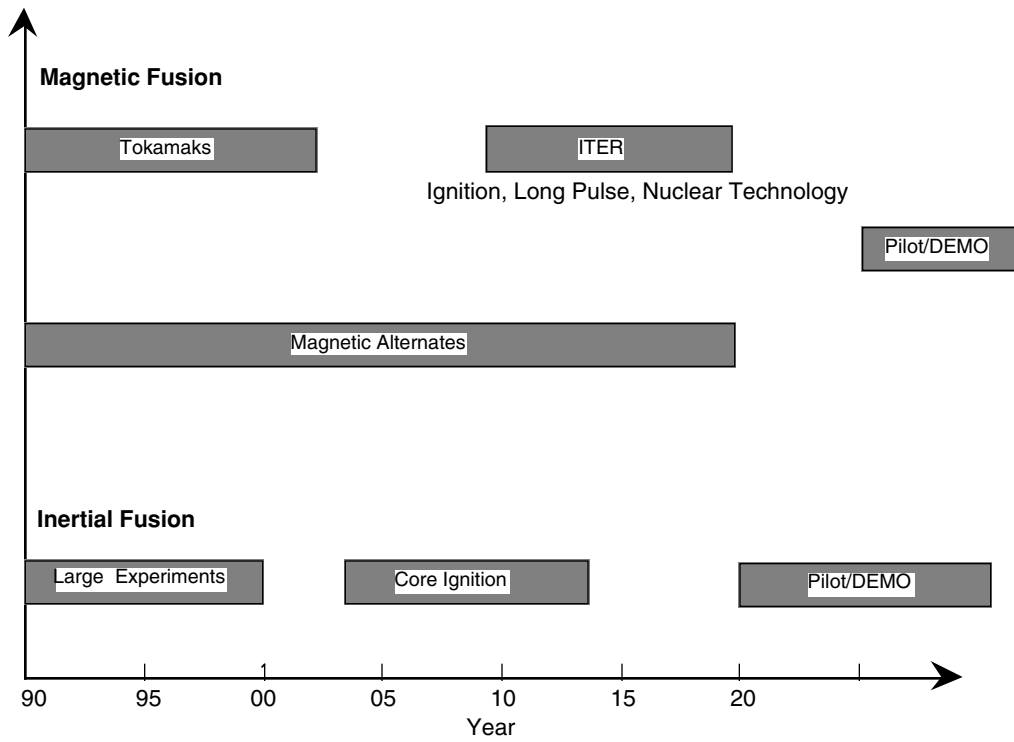


Figure 1.

# Multi-Element Burning Plasma and Fusion Technology Program

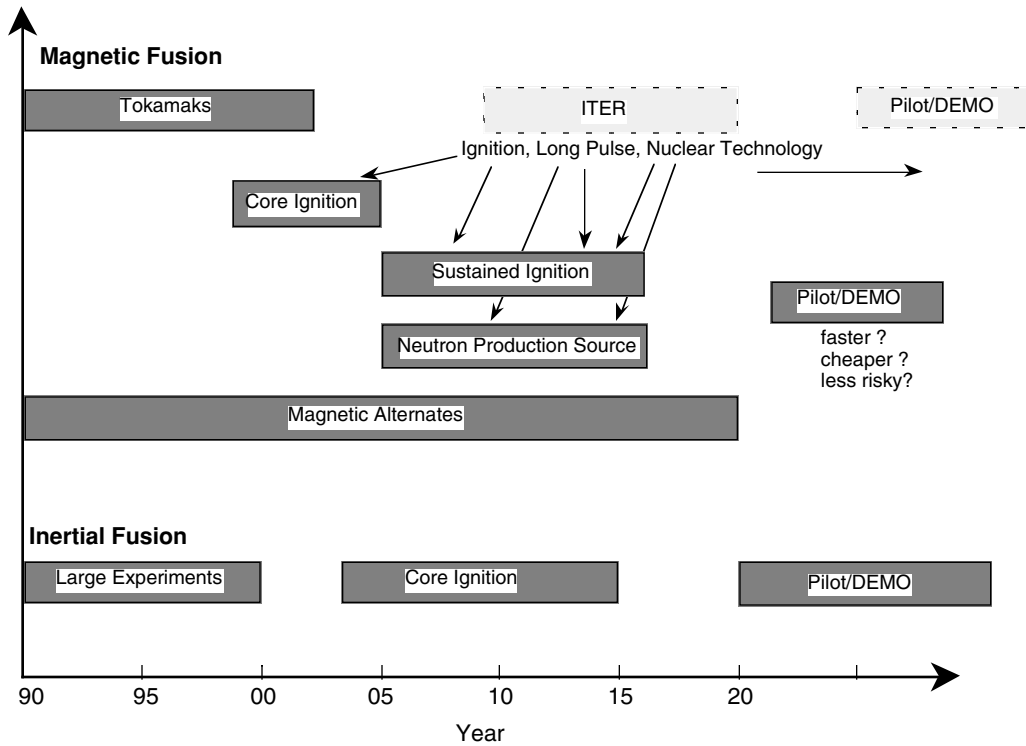


Figure 2.