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Back to the Future:
The Historical, Scientific, Naval, and Environmental Case for Fission Fusion

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It is proposed that a return to fission fusion, and especially the development of the thorium cycle could be a means to revitalize magnetic fusion research. This work analyzes recent history, attempts to find the reason magnetic fusion research is in the shape it is in, and argues that an embrace of the hybrid could improve its prospects. Then it analyzes recent Tokamak results, concluding that a research Tokamak reactor, which could generate significant amounts of nuclear fuel could be built now. Finally it discusses both whether the Navy could be involved, and the environmental issues.

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Back to the Future: The Historical, Scientific, Naval, and Environmental Case for Fission Fusion

I. Introduction

Magnetic fusion unquestionably has the potential for evolving into an inexhaustible energy source, and furthermore, one which would produce virtually no chemical pollution, greenhouse gases, or nuclear waste. As such, one might surmise that research in this area would be very popular with lawmakers. Unfortunately this does not appear to be the case. This paper argues that the reason is that the project is too expensive, and promises no real benefit until very far in the future. An alternate strategy for magnetic fusion is proposed, one which emphasizes fission fusion.

The nuclear industry is certainly in the doldrums now, with no new orders for reactors and the price of mined uranium still fairly low. Furthermore gasoline is selling in the United States today for about a dollar a gallon, nearly the lowest price, in inflation adjusted dollars, in American history. Nevertheless, the basic thesis of this paper is that the nuclear industry will come back, probably in this country, and almost certainly in the world [1]. Additionally, there is growing concern that in as little as 10-15 years a petroleum shortage will hit, this one being the real one [2].

The nuclear industry is now tied to the ^{235}U and ultimately the ^{239}Pu cycle. For the safety of the planet, another option is greatly preferable. This option is the use of ^{233}U bred from thorium [3,4]. The time to investigate this cycle is now, so that when the nuclear industry receives orders for new plants, this option will be evaluated and available. Furthermore, the most prolific source of ^{233}U is from a fusion reactor.

This paper therefore proposes a revitalized fusion program, to be accomplished mostly by constructing a fission fusion reactor. The size would be comparable to TFTR or JET, but now, to be run steady state or at high duty cycle, and at high neutron flux. Furthermore, it strongly advocates the tokamak approach for such a program for the near term, because it is by far the most advanced confinement scheme as far as the plasma physics is concerned. Also, research on the fission fusion blanket would necessarily be as important as research on the plasma. In such a fission fusion research program, a double purpose would be accomplished. First, progress will be made on a much safer nuclear cycle, one which does not build up plutonium, but rather could build it down. Second, fusion research will be greatly enhanced. Furthermore, these can both can be accomplished reasonably soon, in a decade or so.

This paper discusses the political environment fusion finds itself in and the scientific justification for such a program. Finally it very briefly discusses what role the Navy might play, and environmental issues. This author was involved in the NRL magnetic fusion modeling program from the mid 70's to the early 80's, but has been involved in other areas of plasma physics since then. He hopes his perspective will be fresh, and his experience, not too badly out of date.

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II. The Historical Case

Frequently in the 80's and early 90's, panel after panel have studied the magnetic fusion program and have proposed healthy increases in funding so that a commercial fusion plant could be built at some time in the distant future. For instance the *FEAC Report on Program Strategy for US Magnetic Energy Fusion Research*, dated September 23, 1992 talked principally of a 5% real growth per year. For this we would get a demonstration fusion reactor in 2025, assuming all the major nations of the world cooperate. The *Statement by the Energy Policy of IEEE on the FY 1993 Department of Energy Request for Fusion* also spoke of a 5% real growth per year. In this case, the demo in 2025 would be followed by a commercial plant in 2040, again assuming all the major nations of the world cooperate. However each year, what we have seen, is more like a 5-10% decrease in the magnetic fusion budget. The cumulative effect is shown in Figure 1, where the magnetic fusion budget, in 1997 dollars is plotted as a function of year [5].

More recently, with the large drop in 1996 , a new FEAC report, dated January 27, 1996, *A Restructured Fusion Energy Sciences Program* was commissioned, and it concluded that in its new, more impoverished state, the US fusion program should emphasize fusion science, and cooperation with the international tokamak project, ITER, as much as possible. It is tempting to think that this most recent whack will be the last, and that things are bound to improve. This is probably not so; this author contends that if the fusion community continues business as usual, the graph in Figure 1 will continue to fall until it hits cement. In fact, it is the contention of this paper that for at least the last decade, virtually all of the government advocacy done by the fusion science community has been harmful. The problem, as this author perceives it, is that the time scale for magnetic fusion development is very long compared to election cycles, political careers, recessions, wars, etc., and that in a democracy such as ours, lawmakers will not be able to maintain interest in a project such as fusion, with no immediately pressing need, and no payoff until so far into the future. The fusion community may not like this, but it is a simple fact of life. The graph of Figure 1 was compiled when Congress and the Presidency were controlled by Democrats and Republicans in just about every possible permutation. If our elected leaders are so consistently sending this message, who are we to say they are wrong?

Over the years, there have been a number of proposed fixes, all of which are counter productive in the opinion of the author. These are to internationalize the program, to become more politically active in advocating it, and, more recently, to find some different, intermediate milestone which might be salable. Each will be briefly discussed.

In 1985, General Secretary Gorbachev proposed an international fusion project, which evolved into ITER, to be built by the then Soviet Union, the United States, Europe and Japan. The total construction cost is now proposed at over $10 Billion with at least a

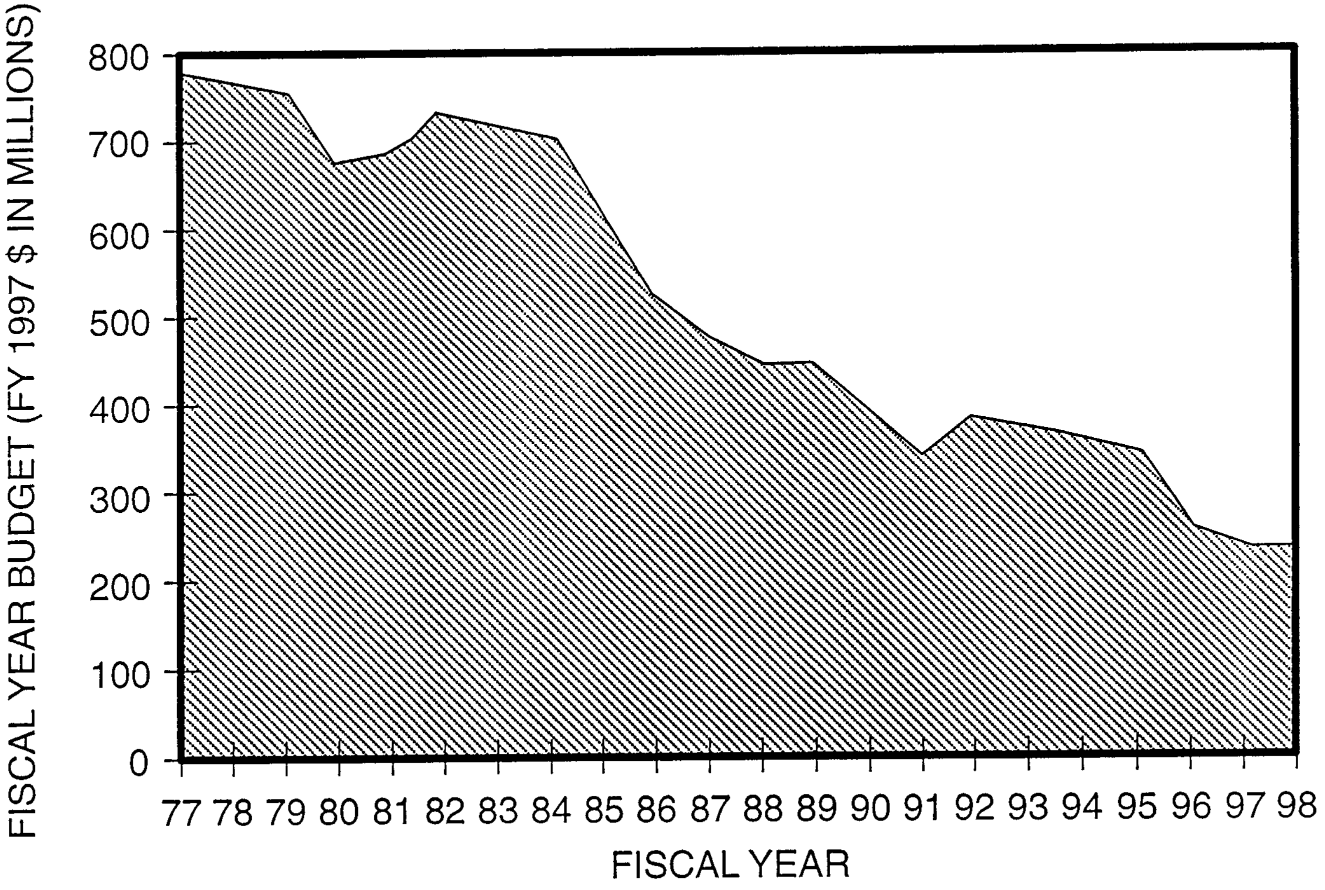

Fig. 1 – U.S. Magnetic Fusion Budget History

$500 million dollar per year operating cost. To approve this project, not only must the American Congress agree, which we have seen is difficult, all of our foreign partners must agree as well. This introduces an even larger element of instability into the system. Any one can at least delay, and possibly even disrupt the project. An example is JET, a very successful tokamak project. However at the outset, it was delayed for years and years as the European partners squabbled over where to build it. ITER multiplies these difficulties by a large factor. There have certainly been very successful international projects such as CERN, but this is a one of a kind facility exploring the very borders of physics. It has generated many Nobel prizes. ITER on the other hand is a power plant. This is no big deal. Every one of the partners has hundreds of them. Therefore this author has no hesitation in going out on a limb and asserting that *ITER will never be built.*

We are often told to be more politically astute with our congressmen and senators in promoting fusion. This author certainly does not advocate political naiveté; the recent gathering of many scientific societies, representing millions of scientists, to inform congress of the important role of federal science support in strengthening the American economy, appears to have been successful and important. However fusion is different. Putting aside for now the moral and ethical issues of participants in a single government sponsored science project actively lobbying the government to support that project, this approach simply will not work. People who lobby the government (all of whom honestly believe that by helping them, Congress helps the nation) bring to the table real blocks of votes and campaign contributions which we could never match. Our *only* weapons are our credibility and scientific reputation. To put these aside, or even to give the appearance of doing so, so we can compete with the labor unions, industrialists, farmers, AARP, the NRA, etc. on their own turf, for government money and/or favor is the height of folly.

Finally, there is now an effort to find an intermediate milestone for fusion research, so as to give our sponsors something useful in a more reasonable time. There has recently been at least one study of spinoffs [6],(using some particular algorithm to evaluate each), ranging from pollution abatement to remote sensing to medical applications to lithography. In a sense, this paper, advocating fission fusion is a search for a spin off. It would certainly be wonderful if these other spinoffs did exist, but it is unlikely that they do. The problem is that fusion has been a well funded, well publicized program for decades now. If it had another application, we probably would have known about it long ago. Furthermore, if after decades of promising an inexhaustible energy supply, we suddenly started selling say the 'medical tokamak', we would be accused of bait and switch big time. No, for better or worse, magnetic fusion is almost certainly tied to energy supply.

The contention of this paper is that the salvation of the magnetic fusion project will be found in going back to fission fusion. This will allow the fusion project to produce energy (nuclear fuel) in a demonstration project relatively quickly. Also it will allow early research on a much safer nuclear cycle. In doing so it will still have to confront and solve innumerable important research issues in both plasma and nuclear science and engineering. Fission fusion was studied rather extensively in the late 70's and early 80's, but almost nothing seems to have been done on the project since then. Also, there does not seem to have been a single article on the topic in *Comments on Plasma Physics and Controlled Fusion.* A very convincing one of these articles is the *Physics Today* article by Hans Bethe [7]. Very simply the case made by Bethe is the following. A D-T fusion reactor which may have Q of order, or even less than unity is surrounded by a blanket of ^{238}U or ^{232}Th. Fourteen MeV Neutrons from a fusion plasma slow down in the blanket, generating a total of perhaps 2-4 slow neutrons. One of these is used to breed the tritium from the lithium, and the rest are available to transmute the blanket material to either ^{239}Pu or ^{233}U. At this point, it sounds like a breeder reactor, but as Bethe points out, it has one very large advantage over a breeder. This is that a fission fusion reactor can supply many more satellite reactors than a breeder. Depending on the type of reactor and blanket, Bethe's article tabulates the number of reactors a fission fusion or breeder reactor can supply. For instance a hybrid with a thorium blanket could provide fuel for 5 light water reactors or 16 advanced reactors. This is in contrast to a breeder which provides for 0.7 of the former or 2.7 of the latter.

This is a tremendous advantage to such a system, as Bethe points out. Since there will be relatively few fission fusion plants (FFP's) compared to the total number of power plants, these can be run by the government in highly secured facilities. Fuel would leave and go to power plants which would be run in the normal way. Also, when introducing a new technology such as fusion, it would necessarily be less reliable and its down time would be greater. Where the FFP's are not the primary energy producers, the entire system could tolerate this much more easily than if all plants were fusion plants.

There is another very great advantage to such an economy. While fusion plants have been touted as being environmentally benign, it is important to realize that in a fusion economy, with fusion plants widespread, any rouge nation (or even power plant owner or operator) could very easily include ^{238}U in the blanket, rapidly breed plutonium, and produce atomic bombs. Perhaps it is better, for the first century or so, to have few fusion plants, and to have them behind a fence. There exist real proliferation dangers to a fusion economy, which have thus far received very little attention.

In 1985, the National Academy of Sciences reviewed fission fusion [8]. Their conclusions were somewhat different from Bethe's. First and foremost, they tied their recommendations to the perceived economics of uranium fuel prices. At this time, mined and enriched uranium is cheaper than it could be produced from FFP's, and it saw no compelling reason to proceed with fission fusion. However since uranium ore is in limited supply, it could foresee situations in which the economics might ultimately favor FFP's as supplies of uranium diminish. Since the energy content of natural uranium without breeding is less than that available from coal, it is by no means an inexhaustible energy supply. The NAS report gave various estimates for this time ranging from early to late in the next century. It pointed out that very little technology for fission fusion is different from that for fusion, and fission fusion could effectively ride fusion's coattails. It did not recommend any separate program in fission fusion, but stated that its potential should be carefully monitored. Very surprisingly, it recommended against the ^{232}Th-^{233}U cycle, saying that the reprocessing would be too expensive. Other authors (whom we will discuss shortly) do not agree. The NAS report seems to almost completely ignore the dangers of proliferation and a plutonium economy. From the author's point of view, this is almost amazing; a plutonium economy is a tremendous potential danger to the world and should be avoided if at all possible.

The ^{232}Th-^{233}U cycle has tremendous advantages in this respect. Bethe and others[3,4] have all recognized this fact. Furthermore, the ^{232}Th-^{233}U cycle depends mostly on thorium supply, not uranium. References 3,4 and 7 estimate that there is about as much thorium as uranium. Also, the thorium cycle necessarily involves breeding. By using this cycle, only thorium enters the plant, and only a subcritical mixture of ^{233}U and ^{238}U leaves. All of the material with bomb making potential (pure ^{233}U in this case) would exist only in the heavily secured facility. When this fuel mixture is used in conventional reactors, it would generate small quantities of ^{239}Pu. However these would be mixed into a highly radioactive waste and reprocessing would be difficult. Furthermore, the plutonium in the fuel could additionally be spiked with, or the nuclear reactor itself could generate small amounts of ^{238}Pu, ^{240}Pu or ^{241}Pu to make diversion to weapons grade plutonium very difficult without isotope separation. In this way fuel produced in a FFP is quite safe, and it could be exported, possibly even to small countries we did not entirely trust. It seems clear that to analyze the $^{238}\mathrm{U} \rightarrow {}^{239}\mathrm{Pu}$ vs the $^{232}\mathrm{Th} \rightarrow {}^{233}\mathrm{U}$ in *only* economic terms misses a very, very important issue. This is particularly so since in any scenario, fuel costs are a relatively small portion of the cost of delivered nuclear power.

It is now natural to ask what has changed since the early 80's to alter the case for fission fusion. There are a number of things, some of which make the case more compelling, some less. However on balance, in this observer's opinion, the case was much more compelling in the 70's and 80's than most people (including myself) realized then, and it is still more compelling now. First of all, there is Figure 1. The NAS argument that fission fusion should ride fusion's coattails is obviously moot. There are no coattails to ride any more. Another aspect to Figure 1 is that Bethe and others in discussing fission fusion in the 70's assumed that fusion machines would achieve Q=1 in the 80's and this obviously did not happen.

Another change is that the nuclear industry, which was weak and unpopular in the 70's is virtually in disarray today. No new reactors have been ordered, and at least one, Shoreham in Long Island, has been decommissioned as it was completed. Endorsing fission fusion would obviously mean getting into bed with the nuclear industry, if not for a marriage, then for much more than a casual date. But can one add weakness to weakness and get strength? This author's contention is that the nuclear industry will and must come back. An entire issue of IEEE Spectrum [1] makes this point. It discusses advances in nuclear technology such as new reactor designs that are passively safe. Furthermore, no matter what we do, the rest of the world will develop nuclear power. A recent article in the *Washington Post* [9] told about the Chinese developing nuclear power on a large scale. Whether we develop nuclear power in this country or not, there is a big export market out there for somebody; why not us? Also, by participating in the export market, this country will have a much greater voice in making nuclear power plants as safe and diversion resistant as possible.

Another thing which could bring the nuclear industry back is concern over global warming and green house gases. While nuclear power plants have their own particular waste difficulties, which we will discuss briefly shortly, their competition, fossil fuel plants are far from pollution free. The green house gases which they emit, and which nuclear plants do not, are an important concern, and most likely will be taken even more seriously in the future. If Congress ratifies the Kyoto Treaty, the United States will be obligated to reduce CO_2 emissions by a very considerable amount. Furthermore, most knowledgeable authorities consider it unlikely that dilute natural energy, the sun, wind, and tides will ever be very important in the nation's power budget. This work contends that the nuclear industry will and ought to exist. Furthermore, a possible alliance with it may be the best hope to both develop a safer nuclear fuel cycle and enhance fusion research.

Despite the disadvantages and dangers of the fast breeder fission reactor, it is one option for an inexhaustible energy supply. Several nations, including France and Japan, have had long programs to develop the breeder. Both of these programs have ended in failure and have been abandoned for now [1]. This then could be a particularly opportune time for the initiation of a rather large and substantial program in this country on breeding nuclear fuel via fission fusion.

Of course the overwhelming world historical event since the early 80's has been the end of the cold war. All of a sudden, there is lots of nuclear fuel, in the world's bombs, which nobody knows what to do with. It is very easy to argue that we do not need more. However, if the decision were made to use the nuclear material in reactors, there is really not that much of it. If one assumes 10% of the energy in a one megaton bomb is in the ^{235}U or ^{239}Pu fission trigger, this will power a 3 GW power plant (producing 1 GW of electric power) for about 3 days. The world's 10,000 bombs would run 100 such power plants for about a year. To be sure, this is a very significant amount of nuclear fuel. However, if a decision were made to use it in power plants, and at the same time a decision were made to start a crash program on fission fusion, the bomb fuel would long since have been used up before the FFP produced its first gram of nuclear material.

Another rather astounding and very recent turn of events is President Clinton's announcement of a balanced budget in FY 1999. It is tempting to think that fusion is now out of the woods, particularly since part of the surplus is to go towards funding scientific research. However this is unlikely to be the case, in part for the reason already discussed. Furthermore, the priorities for the scientific research have been announced in the *Washington Post* [10]. Fusion was not among them.

Thus recent events have altered to some extent the arguments for and against fission fusion that were made in the 70's and early 80's. While some events weigh towards fission fusion, and some against, this author sees the overwhelming tendency of recent events as one that now favors the development of fission fusion, and especially the development of the thorium cycle . However, perhaps the most important events are the discoveries of new, advanced operating modes in tokamaks. There are 3 large tokamaks operating now, TFTR in Princeton (unfortunately retired in FY 1997), JET in England, and JT 60-U in Japan. Here large means having the ability to inject about 40 MW of beam power. Also there are 2 smaller tokamaks, DIII-D in General Atomics and ASDEX-U in Germany, which can inject 20 MW of beam power. All have given *very* impressive results recently. These will be discussed in more detail in the next section.

What appears to be a possible enhanced magnetic fusion program could be proposed. It would build not a bigger tokamak, but one perhaps the size of JT 60 U. It would run on DT, either steady state or at high duty factor, and which utilized a thorium blanket. In addition to research on advanced operating modes in tokamaks, thorium blanket science and development, an important milestone would be to produce enriched uranium for actual use in nuclear reactors. The Q of the reactor, including the energy content of the ^{233}U produced would probably be greater than unity, but even if not, it would be producing a valuable product as well as valuable research on a very safe and inexhaustible energy supply.

Estimating the cost of such a program is far beyond the scope of this article, but one can do some zero order analysis. The cost would almost certainly be more than what could be accommodated in the current fusion program, but it would be a small fraction of the cost of ITER. Furthermore, this country would do the work itself and would not rely on international partners. To get some idea of the cost, there have been two proposed tokamaks over the last decade. The burning plasma experiment (BPX) was budgeted at \$1.6 Billion in FY 1991 and its goal was to study ignition physics in a very high magnetic field. Then the Tokamak Physics Experiment (TPX) was budgeted at \$740 million in FY 1996 and its goal was to study steady state behavior of tokamaks using superconducting toroidal and poloidal field coils [5]. Any tokamak running at high duty factor almost certainly has to use superconducting toroidal field coils to minimize power input. For instance the toriodal field coils on TFTR dissipate hundreds of megawatts. Thus we focus on a tokamak like TPX. It would certainly be more expensive because it would be running at high duty factor in a high neutron flux. Every wall and diagnostic facing the plasma would have to be aggressively cooled and/or shielded. However, it would probably cost less than BPX because there would be no requirement for ignition. Nevertheless, neutron flux issues will have to be faced at some point in a successful fusion program anyway. This proposal is to face them sooner rather than later, in a smaller rather than larger sized facility, and to produce a useful product along the way.

The above addressed the cost of the tokamak alone, which is only part of the total. There would also need to be research and development on the blanket, and most important, ^{233}U, which could be chemically reprocessed, could not be allowed to just build up in say Princeton or San Diego. Reprocessing and mixing with ^{238}U would also have to be an important part of the program. It seems likely that the tokamak would have to be built at some existing national nuclear lab such as Los Alamos or Oak Ridge, or else at some national nuclear facility such as Hanford or Savannah River. Furthermore, because of a high energy γ in the decay chain, the ^{233}U has to be handled remotely. The proposed research program would not be cheap, but it would face problems that must be faced at some point in the fusion program regardless. It would also be contributing to our nation's energy budget on a much more rapid time scale, and in a way that could be much more easily integrated into existing power grids, than a commercial tokamak reactor which follows ITER by many years

This tokamak would be leading the way to an economy of a few fission fusion reactors supporting many nuclear power plants. It would not be as ideal an economy as pure fusion. However, in the unforseeable future, people might want to, and be able to convert from a fission fusion economy to a pure fusion economy. Unquestionably, this is a decision for the people who live during this time, people who are at least fifty or a hundred years from even being born! The fact that this option would be preserved is a very important advantage to the proposed program. It may be that this is the best way for today's fusion community to contribute to a pure fusion economy a century or so in the future. Finally as Hans Bethe said [7], "It seems important to me to have an achievable goal in the not too distant future in order to encourage continued work, and continued progress, toward the large goal, in this case pure fusion."

II. The Scientific Case

A. The Tokamak

The tokamak has certainly been the most successful fusion device world wide for decades now. However, they have been built to such size that they can no longer be sustained by the reduced U.S. magnetic fusion budget. Accordingly, there is now an emphasis in the U.S. fusion project to go to other confinement schemes, to do more with less. In this author's opinion, this is a calamity for the fusion project. Tokamaks were selected 30 years ago because they offered the most optimum means to confine a plasma. There were many alternate schemes then, and none could come even close to doing what tokamaks could. It is still true today, except that tokamaks have progressed even further. There is now a world wide infrastructure supporting tokamak confinement, an infrastructure consisting of thousands of people working together for decades. No other confinement scheme has, or in the foreseeable future, will have anything close to this. This author will gladly bet anyone that if the U.S. fusion projects drops tokamaks in favor of some other confinement scheme, say stellarators or RFP's, in 15 years, these will not be where TFTR is today. This is particularly true because these other possible confinement systems will not only have to get over technical hurdles, but also political ones, which will not get easier in the coming decades. As larger and larger budgets are proposed for say stellarators, Congress will cut it off just as they are doing today with tokamaks. To reiterate, this author strongly feels that the U.S. fusion program has a future not only by going to fission fusion and the development of the thorium cycle, but also by sticking with the tokamak approach at least for the next decade or so. Surely it is only the tokamak that can produce reasonable amounts of nuclear fuel on this time scale.

To proceed, we review briefly where tokamaks are and were, and discuss the advanced operating modes that have been discovered in the last few years. A very rough schematic is shown in Figs. (2A-C) where the history of the tokamak project is sketched out. Shown are plots of figures of merit as a function of time for tokamaks of the mid-70's, ATC [11], ORMAK type B (ORMB) [12], TFR [13], and ST [14]; the mid-80's TFTR [15], Alcator C (ALCC) [16], Doublet 3 (DOUB3) [17], PLT [18], JET [19], T10 [20] and ASDEX [21]; and the mid-90's, DIII-D [22], JET [23], TFTR [24], and JT60-U [25]. The figures of merit are (A) triple fusion product $n(0)T_i(0)\tau_e$ in m^{-3}keVsec, input power in megawatts (B), and total DT fusion neutron production rate in neutrons per second. The latter was obtained either from the actual rate quoted in the references, the DD neutron rate extrapolated by the authors of the references, the DD reaction rate multiplied by 200 [26] if the reference did not give the extrapolation, or an approximate calculation from profiles and known reaction rates. So far, only TFTR and JET have produced DT plasmas. For all of the graphs shown there are uncertainties because the published data may have been incomplete, but these are probably no greater than the widths of the letters shown (perhaps a factor of 2). The shaded regions approximately bound the parameters as a function of year.

Three things are very clear from Figure 2. First of all the tokamak project has made tremendous progress in the last 20 years, second, the problems seem to be getting harder, and third, the neutrons produced are already at a very significant level. For instance JET is producing something like 10^{19} neutrons per second, which corresponds to a neutron power of about 20 MW. As we will see, if this reactor could be run steady state and all neutrons were captured in the blanket, it could generate enough ^{233}U to power a nuclear reactor of 100 MW or so; perhaps the nuclear reactor of a submarine or naval ship. The tokamaks have gotten these recent results by running in various advanced regimes, which we will now briefly discuss.

Before discussing particular tokamaks, we review some general aspects. An often cited scaling law for the confinement time of tokamaks is the so called ITER89-P law [27],

$$\tau^{\text{ITER89-P}}(\text{sec}) = 0.048 M^{0.5} I^{0.85}(\text{MA})\, R^{1.2}(\text{m})\, a^{0.3}(\text{m})\, k^{0.5}\, n^{0.1}(\text{m}^{-3})\, B^{0.2}(\text{T})\, P^{-0.5}(\text{MW}) \quad (1)$$

where M is the isotopic mass number, I is the current, R is the major radius, a is the minor radius, k is the elongation, n is the electron density, B is the magnetic field, and P is the heating power. One of the most startling tokamak results of the 80's was the discovery of the H mode, originally in ASDEX [28]. As neutral beam power increases, (originally only a divertor tokamak, but ultimately in any tokamak and in stellarators as well), the equilibrium bifurcates and the confinement time abruptly doubles. This is the H (high confinement) mode, the original low confinement mode was the L mode. Generally the

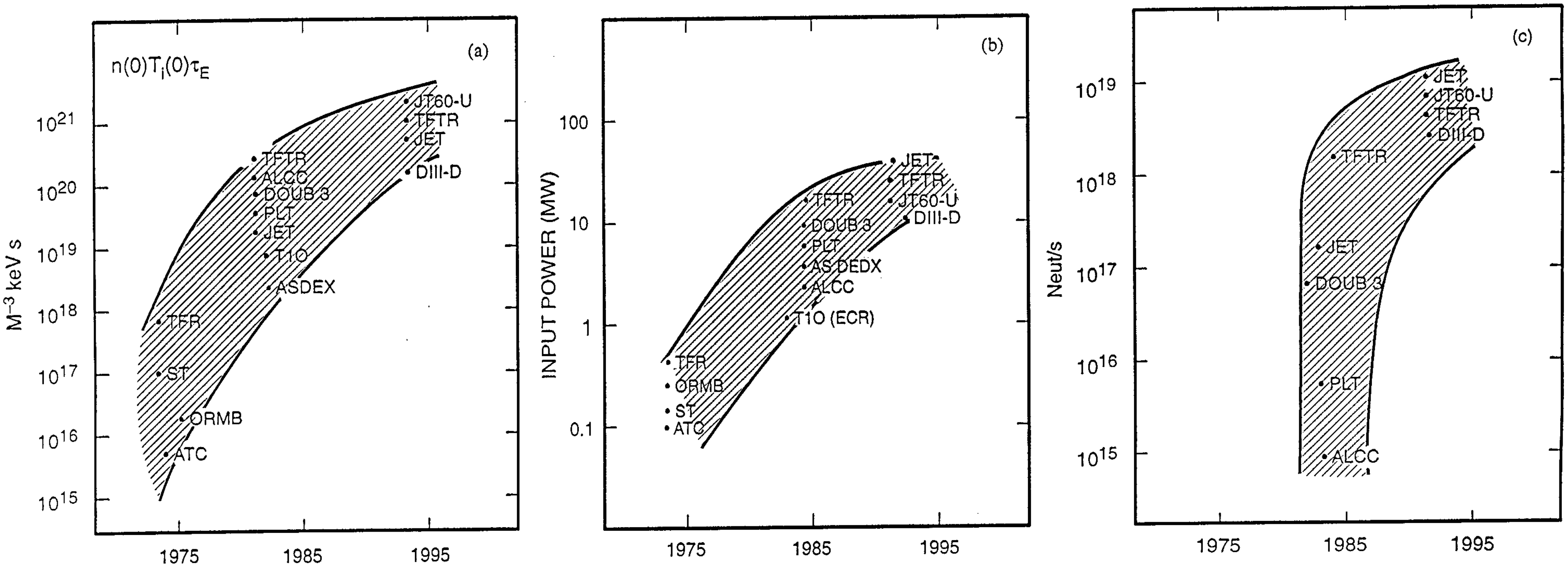

Fig. 2 – Figures of merit for recent tokamaks: (a) triple fusion product, (b) input power, and (c) D-T neutron production rate.

mode is characterized by a factor H which is the ratio of confinement time to that predicted in Eq.(1). H is typically about 2. In some recent very high mode studies in DIII-D, the H factor occasionally gets as high as 4 [29]. It is now reasonably well established that plasma edge is responsible for this transition [30]. At some point large radial electric fields are set up at the edge. These fields have both gradient and curvature, both of which may be important. This electric field causes a differential rotation of the edge plasma, which presumably damps out the edge fluctuations. As a result, H modes are usually characterized by sharp gradients at the edge, and broader profiles inside the plasma. This H mode then degrades in one of a number of ways. Because of the enhanced confinement, impurities may build up in the center and cause radiative collapse. On the other hand as edge gradients build up, they may destabilize edge localized modes (ELM's), which may either abruptly disrupt the plasma back to the L mode, or else may build up gently and limit further confinement. In this latter state (grassy ELM's), the H mode can be in a nearly steady state.

Another important advance is the more recent understanding of beta limits in tokamaks [31]. Troyon calculated the beta limits under ideal MHD, but if a profile was unstable, he would attempt to vary it some to stabilize it. Generally he could do this with ballooning modes, but not with free boundary modes. He found that the beta limit is given in terms of a parameter called the normalized β given by

$$\beta_N(\%) = \beta_T(\%)\ a(m)\ B(T)/I(MA) \qquad (2)$$

Stability of n=1 modes generally limits β_N to about 3 if there is no wall stabilization, and to values which may be as large as 5 if there is a nearby conducting wall. Much recent tokamak data, at the highest beta is consistent with the Troyon condition. It is often used in designing tokamaks with a particular beta limit.

Now let us review some additional tokamak data. All of the large tokamaks have produced very impressive results recently. We discuss all, but focus perhaps a bit more on JT60-U. One recent advance here is the development of negative ion sources and accelerators. With these, the JT60-U program has injected 2.5 MW of 350 keV neutrals into the plasma, with development on line to produce 10 MW of 500 keV neutrals. These high energy neutrals are particularly effective at current drive. Shown in Fig 3A is a sketch of the various components of the plasma current as the high energy neutrals are injected [25]. During the beam pulse, all of the current is either beam generated or is bootstrap current. This capability is very important for either steady state or high duty cycle operation of a tokamak.

JT60-U, along with JET and DIII-D can shape the plasma cross section, and all have found that triangularity is essential in increasing the energy content of the plasma. Apparently the reason is that the added shear increases edge stability, so that the pressure at the edge of the plasma can be greater. Shown in Fig 3B is an approximate sketch of the edge temperature (electron or ion) as a function of triangularity parameter δ [32]. This experiment also showed that one very effective way to heat the electrons at high current is to find a way to stabilize internal modes which give rise to the sawtooth oscillations. In the case of Ref.[32], this was done with ion cyclotron heating. Shown in Fig 3C is a plot of electron temperature as a function of time during a period of sawtooth free operation. While this may not be the most important result, it is particularly interesting to this author because the NRL program suggested in the 70's that stabilizing the sawtooth oscillation was likely to be the most effective method of electron heating [33] (although Ref. 33 did not propose a stabilization mechanism).

One thing that is very clear on reading recent tokamak results is that the problem of disruption has not been solved yet. Just about all of the papers cited mentioned disruptions as a limiting factor. What this means in practice, is that the maximum results claimed often are those in plasmas which disrupt. In planning a steady state or high duty cycle tokamak, where frequent disruptions could not be tolerated, it is often best to take the greatest claimed result and back off a bit. An example is in Fig 3D from JET [23]. The top graph is a plot of neutron rate as a function of time which achieves a DT Q of unity. It is in an H mode plasma during the period of no ELM's. However, the plasma ultimately disrupts. Also shown in Fig 3D is a plot of neutron rate for a different shot where the plasma is in an H mode, but limited by low amplitude ELM's. The plasma is in nearly steady state, and generates a DT Q of about 0.7 for as long as the discharge persists. JET has in fact demonstrated H mode plasma, limited by grassy ELM's, which are steady for 20 seconds. In this case, 75% of the input power is radiated away by low Z impurities seeded in the outer region of the plasma. This radiation buffer is important to limit the power dissipated on the divertor plates.

A very important advanced operating mode in tokamaks is the hot ion, or supershot regime, first discovered in TFTR [15,34,35]. This mode has two principal qualities. First, the high neutral beam power is deposited principally in the center, and second, the recycling is reduced by aggressive limiter conditioning. Then the central plasma is both heated, and to a large extent fueled by the beam. The energy is very well confined there, with confinement time typically 2 or 3 times that given by Eq. (1). The density and temperature profiles are very peaked. Figure 4 shows radial profiles of density and ion temperature in two different shots in TFTR [36]. The only difference between the two is the limiter conditioning. Hot ion modes almost invariably give the best fusion performance in D-T plasmas.

Typically supershots are plagued by disruptions, and the MHD behavior is rather complicated. Even though $q(0)<1$, and most theories predict $m=n=1$ modes in the center, these are rarely seen. Often the disruption seems to follow from low mode island formation in the center. The outer part of this new equilibrium is unstable to ballooning modes and these provide the coup de grace.

Another advanced operating mode is the reversed shear mode. It is interesting that the advantages of this operating regime were first predicted theoretically [37]. Here, the rotational transform q has a maximum at the center and decreases out to some radius, at which point it increases. The plasma current is then largely in a shell rather than

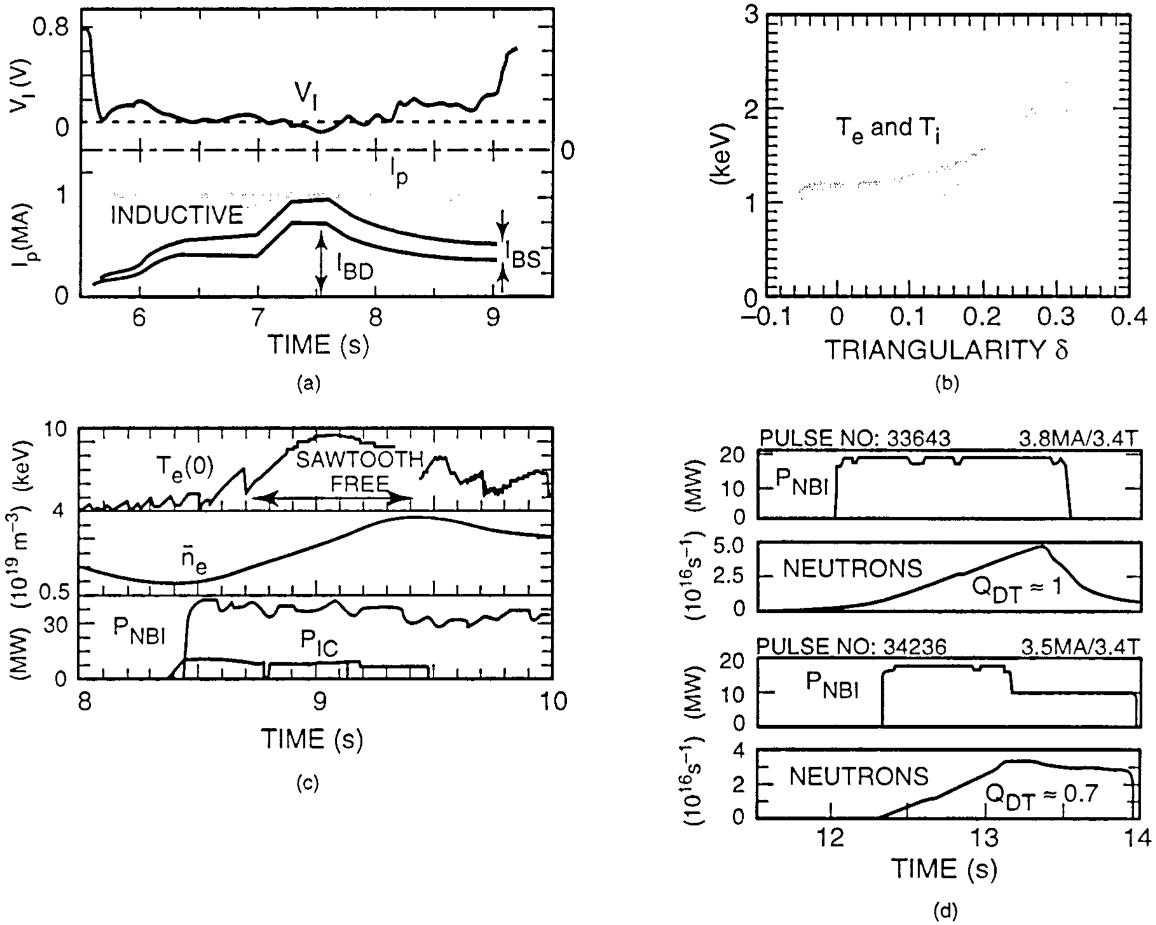

Fig. 3 – (a) Loop voltage, and inductive, beam driven and bootstrap current for JT60-U, (b) edge electron or i on temperature for JT60-U as a function of triangularity, (c) electron heating in a sawtooth free plasma due to neutral beams and ICRH in JT60-U, and (d) neutron rate as a function of beam power for disrupting and nondisrupting plasmas in JET.

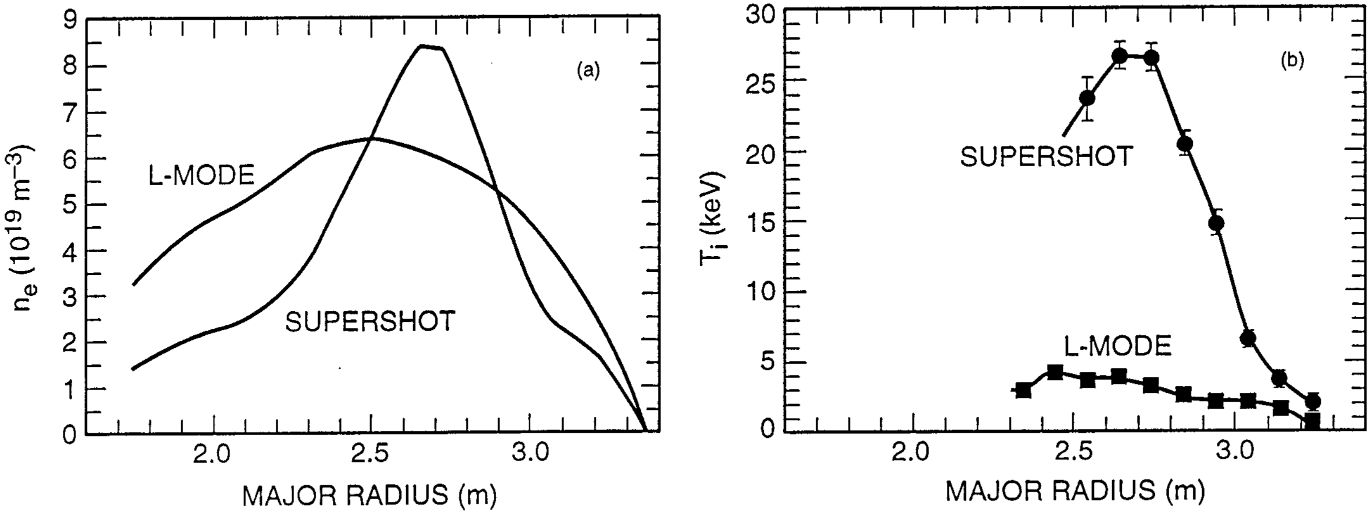

Fig. 4 – Comparison of L-mode ($\tau_E = 0.06$ s) and a hot ion mode supershot ($\tau_E = 0.18$ s) for $P_{NB} = 22$ MW, $I = 1.4\ \mu A$, $B = 4.7$ T. Except for the limiter conditioning, the parameters of the two shots are the same.

having a maximum at the center. These reverse shear states often have enhanced confinement properties, and these in turn are generated by the plasma setting up an inhibited transport region. Two recent advances have greatly aided research in reverse shear states. The first is the development of the motional Stark effect (MSE) diagnostic which directly measures the poloidal field and therefore the q profile. The second is a reliable set up scheme where the plasma center is heated by the beams before the current profile is complete. This hot center keeps the current out, and as the remainder of the current diffuses in, it remains in the outer region.

Shown in Fig 5A are radial plots of electron and ion temperature and q in a reverse shear shot on JT60-U [25]. The regions of sharp temperature gradient shown are also regions of very low transport, the inhibited transport region. This is near the minimum of q. It is now reasonably well established that along with this inhibited transport, there is also a velocity shear in the toriodal and/or poloidal plasma velocity. Virtually every author recognizes this, but most are not willing to assign a cause and effect relation at this time. It also appears that it is this shear in the rotation frequency that stabilizes the double tearing modes that one usually associates with minima in q. Some very interesting data from TFTR [38] is shown in Fig 5B, in what they call the enhanced reverse shear mode. The shear in rotation velocity is converted into a damping rate, and this damping rate is compared to the growth rate of various micro instabilities, the trapped electron mode and the ion temperature gradient mode in this case. It is apparent that when the shear rate gets larger than the growth rate, transport is inhibited, and fluctuations actually die out.

One difficulty of the reverse shear mode in the JT60-U experiments is that these invariably end in disruption after some time. The DIII-D group has done some interesting research on this [39], and in their experiments, reverse shear states with L mode edges often disrupt. However, if an H mode transition is triggered, the profiles become broad and usually there is no disruption. The ideal and resistive MHD stability of these states has been investigated. Shown in Fig 5C is a plot of the stability boundary in a two dimensional space whose horizontal axis is central pressure divided by the average pressure and whose vertical axis is β_N [40]. Also shown are various L mode (dots) and H mode (crosses). The L modes are much more likely to be in an unstable state and disrupt.

Hopefully this very brief summary conveys an appreciation for advances in tokamak physics, both over the last few decades, and recently. The question is how best to exploit these in the U.S. fusion program. As already discussed, this author's case is that the best thing to do is to build a tokamak like say JT60-U, but to run it at steady state or high duty cycle. The proposed facility would have superconducting toroidal field coils, a divertor with triangulation, and high energy ion beam injection from a negative ion accelerator for both heating and effective current drive. A tokamak rather like this has already been proposed in the U.S. fusion program, TPX [41,42]. This was to be a steady state tokamak. It was to run in a reverse shear mode, in part because the reverse shear profile is consistent with a high fractional bootstrap current. TPX also had

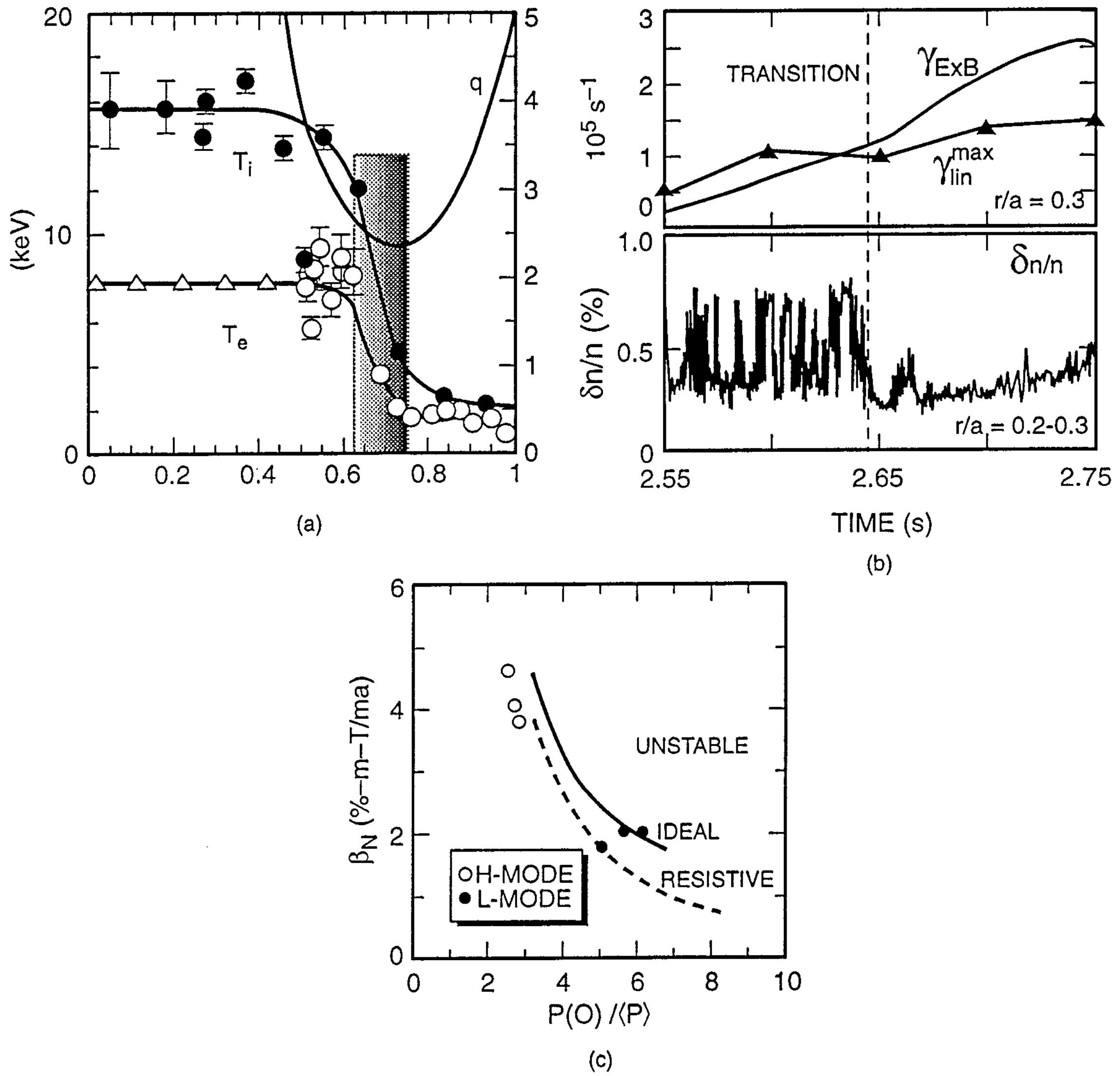

Fig. 5 – Reversed shear states in tokamak plasmas: (a) the temperature and q profile in JT60-U showing region of inhibited transport, (b) the growth rate for DTEM and ITG instability and rotational shear rate vs time for TFTR. Also shown are measured density fluctuations vs time, and (c) various states of disrupting and nondisrupting plasmas for DIII-D for L mode (high P(O)/⟨P⟩) discharges at H mode (low P(O)/⟨P⟩) discharges shown against MHD and resistive stability boundary.

superconducting poloidal field coils and was designed to run with a close fitting wall so that normalized beta values of 5 could be obtained.

If a tokamak like TPX is to be built for breeding ^{233}U, as well as research, it is not clear that the close fitting wall will be consistent with constraints imposed by the breeding blanket. It might be preferable to operate without wall stabilization and with a lower β_N. High neutron rates have already been produced in tokamaks with β_N of 2 or 3. Furthermore, on perusing Refs. 41 and 42, it is clear that the steady state nature relies on many speculative assumptions and is a very large extrapolation from the longest tokamak pulse to date, perhaps 20 seconds. Also, many of the advanced modes require time dependent control of one sort or another. It might be a more conservative approach to run pulsed at high duty factor, say 50% rather than steady state. Then the poloidal coils might not have to be superconducting. These coils produce smaller fields, so would dissipate less power (than copper toroidal field coils), and the currents in them could be more easily programmed in time for control of the plasma. It is worth noting that a tokamak of about this size with superconducting toroidal field coils and normal poloidal coils, TORE SUPRA [43], has been operating for a while now in France. In any case, an important goal of the program would be the production of nuclear fuel, specifically ^{233}U mixed in with ^{238}U in a subcritical mixture. P. Rebut, formerly head of JET is also now seriously proposing fission fusion [44], although on a much larger scale than what is proposed here.

Let us close with a brief additional word on alternate confinement systems. As tokamaks are scaled up to for instance ITER size [45], it is clear that they get enormously large. The world is unlikely to use them for very many power plants; ultimately an alternate concept will be essential. There are certain alternate concepts, such as the spherical tokamak (ST) which might in fact be better for a fission fusion reactor [46], if they live up to their promise.

If one accepts the necessity for fission fusion as proposed here, a legitimate issue is whether we are better off doing the research now on for instance an ST and then build a research FFP based on it. The author feels that the answer is no. Spherical tokamaks have to first do research and development to get to where say TFTR is now. This will probably take 10 years, and it may fail. Then another ST must be built to run at high duty factor, an additional 10 years for a total of 20 to start producing nuclear fuel. However, if we wish to influence the nuclear fuel cycle before many new plants are ordered, shouldn't we start producing and researching the fuel before that? Also, does the fusion program, in view of Fig. (1), have the luxury of this kind of time? Clearly the author feels it does not. The great advantage of tokamaks is that by exploiting a bird in the hand, it jumps right to the second stage and cuts off 10 years. If this accomplishment captures the imagination of the country and impacts the nuclear fuel cycle, there will be plenty of time to develop more optimum confinement systems. However, as Abraham Lincoln said, "Don't change horses in the middle of a stream."

B. The Blanket

A vital part of any such research program is the fusion blanket. There has been a good deal of study of this both in the science and technology itself [47-49], and the possibility of a commercial sized fission fusion power plant based on a tokamak [50,51]. The philosophy here is different in that a small sized tokamak research reactor rather than a commercial reactor is emphasized. The key advantage to a fission fusion system is that the energy given up in a fission event is very large, typically about 200 MeV. If a 14 MeV fusion neutron produces ζ ^{233}U atoms in the blanket, and the blanket captures a fraction of the neutrons f, let us define the fission fusion Q in terms of the pure fusion Q a

$$Q(\text{fisfus}) = [200/14]\ \zeta\ f\ Q(\text{fus}) \tag{3}$$

Of course, for either fission fusion, or pure fusion, the energy budget is less favorable because of various inefficiencies. However fission fusion does have the potential of raising the Q by about an order of magnitude, and this could be very significant. Let us consider the possibility of building a research reactor the size of JET, but run steady state with superconducting coils. If we assume $f=0.75$ (to leave room for diagnostics) and $\zeta=2$, the fission fusion Q is about 20 times the Q of the fusion reactor alone. We assume that with all of the experience acquired, one could now build a such tokamak with $Q=1$. The 20 MW time average input beam power (assuming say 40 MW at 50% duty cycle) would produce enough ^{233}U to run a 400 MW nuclear reactor. This is larger than the reactor on any naval ship; it would give an opportunity to do further development on the thorium fuel cycle.

Another consideration is the tritium breeding. If a large part of the nation's power is to come from fission fusion, and tritium is not bred separately, the reactor must breed enough tritium to keep itself going. This adds an additional constraint to the system. In most of the published blanket designs, ζ is maximized, but is constrained by the need to keep the number of tritium atoms produced per fusion neutron, λ, just slightly greater than one. This means that to breed enough tritium, f must be just about unity. The question is whether one desires to breed tritium in an initial research reactor, or use some other source of it, perhaps decommissioned nuclear weapons, or tritium purchase from Russia, or a separate breeder to be built (the United States today has no operating reactor to breed tritium). Running an initial research reactor without tritium breeding would certainly simplify the operation and the reprocessing, and would also make ζ larger as we will see. Thus running a first tokamak fission fusion reactor without tritium breeding might be an attractive option for an initial project. (A research FFP like JET, producing 10^{19} neut/s running cw would require about 1 kg of tritium per year.)

The next question is how much fissile material and tritium is generated by each fusion neutron for a particular blanket design. This is rather complicated, depending on cross sections for various nuclear processes at various energies. The fusion neutron in the blanket produces other neutrons by a variety of nuclear processes including fission and nuclear multiplication from a single element (for instance $n + {}^{238}\text{U} \rightarrow 2n + {}^{237}\text{U}$). There are a variety of materials which can be added to the blanket to increase the multiplication of neutrons. The material particularly emphasized in Refs. [47 and 48] is beryllium. Finally, there is the reaction of ultimate interest for a thorium blanket,

$$n + {}^{232}\text{Th} \rightarrow {}^{233}\text{Th} \rightarrow (\beta \text{ decay, half life 22 min)) } {}^{233}\text{Pa} \rightarrow (\beta, \text{ 25 days) } {}^{233}\text{U}$$

The competition of all these reactions determines what finally is generated by the single neutron, and all its progeny, as they all slow down to zero energy and are absorbed. These are calculated by Monte Carlo simulations, and no further details will be given here, only results. The first four rows of Table 1 show values of ζ and λ taken from Ref. [47] for a variety of different infinite, homogeneous blankets. Also shown is the energy absorbed in the blanket for each fusion neutron. This energy would be fed through a heat exchanger to produce electricity to run the reactor or for other customers. There are other more complicated blanket designs including a two zone blanket, where the neutron enters the first zone, where it mostly multiplies, and then proceeds to the second zone where it mostly generates ^{233}U. These calculations do not account for the structural material mixed in. Another calculation including this, the 'engineered blanket', which accounts for various different regions and structural materials, is shown in the last row of Table 1. The goal is to maximize ζ, or ζ and λ if tritium is to be bred. Clearly, there is a significant price to pay for the tritium breeding, especially in the engineered blanket.

There are two approaches to the fusion blanket. In the first, one designs the blanket to produce as much fission power as possible so as to maximize power plant production. Since only neutrons above about 1 MeV give rise to fission in ^{232}Th, the thorium blanket is placed right in the fast neutron flux. As ^{233}U builds up in the blanket, it begins to burn and ultimately can give more power than the fusion reactions. Furthermore, the energy directly deposited in the blanket by the neutron (the last column in Table 1) is used and one would like to maximize it. A fissioning blanket is certainly one reasonable approach for a hybrid. The disadvantage is that the fast fusion blanket brings in all of the complexities of a fission plant, in addition to those of a fusion plant, which has its own particular requirements, and may even have a disrupting tokamak plasma just a thin wall away from the nuclear reactor. One authority called it "an accident waiting to happen" [52].

The other approach is to minimize the fission reactions in the blanket so that the fusion plant generates almost exclusively fuel to be used at other off site power plants. The goal of the fission suppressed blanket is to maximize ζ, or ζ and λ if tritium is to be bred, but *minimize* the energy deposited in the blanket, while nevertheless using it in a heat exchanger. (In the engineered blanket, this energy alone doubles the fusion Q) This author prefers the fission suppressed fusion blanket. It is almost surely a much safer approach, the fusion plant produces fuel, which is burned in other power plants set up to safely do that and only that. There are basically two approaches to the fission suppressed blanket, each of which relies on a flowing blanket. The thorium may itself be a liquid, usually a liquid salt, or else it may be in the form of pebbles carried along with the flow of a different fluid. First, the fission suppression may rely only on the flow. The slow neutrons create ^{233}U in the blanket, but before these can build up and react, they are

TABLE 1

PRODUCED PER 14 MEV NEUTRON

BLANKET	ζ	λ	E(MeV)
^{232}Th (homogeneous)	2.5	0	50
Natural Li (7.5% ^{6}Li)	0	1.9	16
^{232}Th + 16% ^{6}Li	1.3	1.1	49
^{9}Be + 5% ^{232}Th	2.7	0	30
^{232}Th and Li (engineered)	0.73	1.1	35

removed from the flow. Secondly, a moderator which multiplies the neutrons and softens the neutron spectrum can be added. This then effectively prevents fission of the Th as the ^{233}U builds up, since fast fission of Th requires neutrons with energy above 1 MeV. The moderator favored in Refs.[47 and 48] is beryllium, but it is pointed out there that there are other possibilities as well.

References [47 and 48] argue that the reprocessing is not necessarily very expensive. If a molten thorium salt is used in the blanket, the removal of ^{233}U can apparently be done by fluorination and little development work would be needed. The key is keeping the concentration of the uranium low. If this is done, the radioactive decay products would have concentration still lower, and would not necessarily have to be removed. However, if the decay products did have to be removed, additional development would be required. If the pebbles in a flowing system are used, it could be possible not to reprocess at all. Once the ^{233}U built up to some appropriate level in the pebbles, the pebbles themselves could just be used as fuel in nuclear reactors. Presumably they could also be powdered and mixed with ^{238}U powder and used as fuel as well. However initial calculations show that there would be a performance penalty associated with this option. This author does not have very much experience in nuclear science, but it does seem clear that there are numerous options for fission suppressed blankets. All blanket concepts require some development and have technical risks associated with them. However, the technical risks associated with the blanket appear to be less than those associated with the plasma.

III. The Naval Case

One ordinarily does not think of the Navy as an organization which would support the development of magnetic fusion. However, there is at least some consideration within ONR [53,54] to define ship propulsion by fusion as one of the *ONR Grand Challenges to Science and Technology*. In fact, a small project on this has already been funded by ONR for at least a year [55]. A careful examination of Figure 3 of Ref. 55 does indeed show clearly the Naval motivation. Unfortunately, fusion will not be powering Naval ships in the 21st (or probably even the 31st) century. For the foreseeable future, there is simply no fusion scheme which makes any sense for direct naval propulsion.

However there is a way the Navy could be a player. There are now many ships powered by nuclear fission reactors. For example Seawolf Class Submarines are powered by 40 MW nuclear reactors, Nimitz class carriers are powered by 200 MW nuclear reactors, and Virginia Class guided missile cruisers are powered by 50 MW nuclear reactors [56]. In fact, the nuclear reactors were developed first for the Navy, and this expertise then fed into the civilian economy.

The civilian economy may be run entirely on fossil fuel or entirely by fusion, but there will *always* be a nuclear navy. The very intriguing question is whether the Navy could be a customer for a nuclear fuel which is a ^{233}U-^{238}U mixture. Actually the Navy is very willing to use ^{233}U. In the 60's and 70's, the Navy developed a light water breeder to breed ^{233}U [57,58] for ship propulsion. This program was in fact very successful, but it was not continued, and the reactor core was finally discharged in about 1980. Today Naval reactors use ^{235}U, which was found to be somewhat less expensive than bred ^{233}U. However, as we have seen, nuclear fuel for a Naval reactor could be generated by a fission fusion tokamak the size of say JET (situated on land, of course) if it were to run cw. The Navy could be a first customer, as well as a beacon to guide the civilian economy toward both a safer nuclear fuel cycle, and ultimately toward fusion.

Furthermore, there is an important research role the Navy could play. The Navy is now the lead service in the *Vacuum Electronics Initiative*, the project developing advanced power tubes for the military. The project's headquarters is in the Electronics Science and Technology Division at NRL, and other divisions at NRL also have significant experience in this area. One such microwave tube currently under development is a high power 94 GHz gyroklystron for a radar. This is roughly the frequency required for ECRH in a tokamak plasma. It seems clear that profile control will be important in a steady state tokamak, and ECRH could be an important tool in achieving this. The cw power unit for such a tokamak, appears to be about 5 MW, nearly 3 orders of magnitude larger than the radar tube, and at least one order of magnitude greater than conventional gyrotrons. However, the Navy has very significant talent and experience which could be useful in developing this tube. Furthermore, many plasma physicists are themselves quite experienced in microwave tube development; the two fields are closely related. In a different area, the innovative quasi-neutral particle simulation techniques [59] developed in NRL's *Plasma Processing Accelerated Research Initiative* could find application in simulation of the tokamak divertor scrape off region, or of micro-instabilities in the interior plasma.

IV. The Environmental Case

These days, one cannot simply advocate nuclear power and be unaware of the environmental issues involved. The build up of spent nuclear fuel, as well as the residue from government weapons development presents the world with a very difficult challenge. An entire issue of *Physics Today* [60] was devoted to this problem. Right now, American policy is to let the residues build up on site (i.e. bury our heads in the sand). When receiving his Fermi award, Richard Garwin blasted this do nothing policy, especially as regards the build up of plutonium [61]. Since plutonium and its decay products are potential bomb making material for more than *seven hundred million years*, the issue is not only political, scientific, and environmental, many people would think it has religious aspects as well. How ever well we dispose of plutonium, does our species have a right to create a plutonium (or ^{235}U) mine, something that God never put on this planet?

Nevertheless, we should certainly do better than we are doing today. There are two nuclear disposal sites, WIPP in New Mexico for low level waste, and Yucca Mountain in Nevada for high level wastes. The latter, particularly, is running into political problems. What we would like to do with nuclear waste is treat it and forget it. However, a new paradigm has been proposed [62], one where one does not forget nuclear waste, but remains open to the possibility of treating it far into the future. In this sense, North argues that Yucca mountain should not be closed off for all time, but rather material stored there should be accessible for future treatment as innovations develop.

Furthermore, the concept of permanent solution to the problem via transmutation of the wastes should not be dismissed. There are several options involving either reactors or accelerators. The accelerator based transmutation is particularly intriguing because it uses extrapolations of existing accelerator technology coupled to a subcritical reactor [63]. For plasma physicists, this option is very interesting, because as with microwave tubes, accelerators (and their microwave tube drivers) have a great deal in common with plasma physics. These proposed transmutation options have been reviewed by the National Academy of Sciences, and their review was rather negative [64]. The costs and development times would be very high. It is certainly no substitute for Yucca Mountain and geological disposal for say the next 20 years. However, as mentioned, the relevant time scales are much greater than twenty years. The author feel that this option should be continued to be examined vigorously.

V. Conclusions

So what is the fusion community to propose and argue for? This paper provides the answers according to one observer. First, since ITER will never be built, the United States should pull out of the project and use its fusion resources domestically. Secondly, it should propose the building of a tokamak like JET, JT60-U or TPX, to be run at steady state or at high duty factor, to produce nuclear fuel, and especially to produce a ^{233}U-^{238}U mixture. Third, it should try to get the Navy involved as a customer and a junior partner, and fourth, it should encourage the responsible disposal of nuclear waste. Virtually all of these scientific and technical problems involve, or might involve plasma physics or its closely neighboring fields.

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