BACK TO THE FUTURE: THE HISTORICAL, SCIENTIFIC, NAVAL, AND ENVIRONMENTAL CASE FOR FISSION FUSION

FUSION REACTORS

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A return to fission fusion, and especially the development of the thorium cycle, is proposed as a means to revitalize magnetic fusion research. Recent history is analyzed, causes are sought for the current state of fusion research, and possibilities for how its prospects can be improved are examined. Recent tokamak results are also analyzed, and the conclusion is reached that a research tokamak reactor could now be built that could generate significant amounts of nuclear fuel. Finally, possible Naval involvement and environmental issues are discussed.

I. INTRODUCTION

Can the magnetic fusion program now declare victory and start producing energy? This paper asserts that by embracing the fission fusion hybrid, it may be able to do just that. Accordingly, it proposes a new fusion program reoriented toward this goal.

Magnetic fusion unquestionably has the potential for evolving into an inexhaustible energy source and furthermore, one that 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 for about a dollar a gallon in the United States today, nearly the lowest price, in inflation-adjusted dollars, in American history. Nevertheless, the basic thesis of this paper is that the nuclear industry will probably come back in the United States, and almost certainly in the world.[1] Additionally, there is growing concern that in as little as 10 to 15 yr 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. However, many knowledgeable experts[3,4] have pointed out the dangers of a plutonium economy. An alternate nuclear cycle could greatly reduce these dangers. 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 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 the Tokamak Fusion Test Reactor (TFTR) or Joint European Torus (JET), but it would be run steady state or at high duty cycle, and at high neutron flux. In fact, research and development on maximizing the duty cycle in a D-T plasma would necessarily be as important as other research milestones. Furthermore, this paper 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 certainly 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 decrease it. Second, fusion research will be greatly enhanced. Furthermore, these can both can be accomplished reasonably soon, within a decade or so.

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This paper discusses the historical and 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, as well as environmental issues. Although various cases are presented in support of the fusion research program, the author does not attempt to make an economic case and indeed believes that such a case would be premature. However, at some point in the next decade or two, a fission fusion research program would have to transition into the real world and compete economically with other energy options. Perhaps it would win, or win a niche for itself. This author was involved in the Naval Research Laboratory (NRL) magnetic fusion modeling program from the mid 1970s to the early 1980s, 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.

II. THE HISTORICAL CASE

Frequently in the 1980s and early 1990s, panel after panel studied the magnetic fusion program and proposed healthy increases in funding so that a commercial fusion plant could be built at some time in the distant future. For instance, Ref. 5 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. Reference 6 also spoke of a 5% real growth per year. In this case, the demonstration reactor in 2025 would be followed by a commercial plant in 2040, again assuming all the major nations of the world cooperate. What we have seen each year, however, is more like a 5 to 10% decrease in the magnetic fusion budget. The cumulative effect is shown in Fig. 1, where the magnetic fusion budget in 1997 dollars is plotted as a function of year.[7]

More recently, with the large drop in funding in 1996, a new Fusion Energy Advisory Committee (FEAC) report was commissioned,[8] and it concluded that in its new, more impoverished state, the U.S. fusion program should emphasize fusion science and cooperation with the international tokamak project, the International Thermonuclear Experimental Reactor (ITER), as much as possible. It is tempting to think that this most recent decrease will be the last and that things are bound to improve, but this author disagrees. Now that the fusion program has been recast as a fusion science project, it is easy to envision future large cuts. One could ultimately foresee the project as being reduced to perhaps four or five university-scale advanced concepts, each funded at $4 to 5 million/yr.

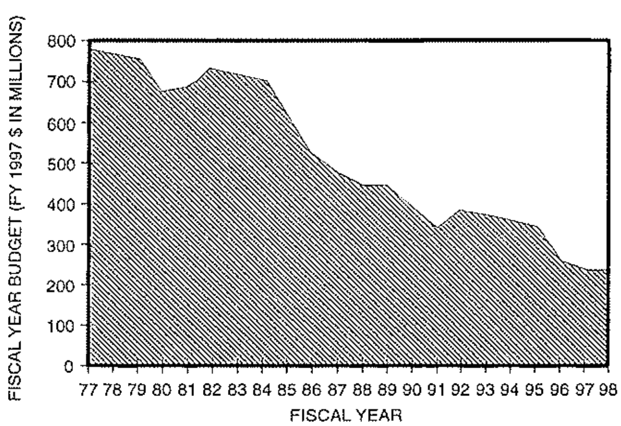

Fig. 1. The U.S. magnetic fusion budget history.

The problem as this author perceives it is that the timescale 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. This is but a simple fact of life. The graph of Fig. 1 covers two decades, during which 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 counterproductive, 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 that might be salable. A brief discussion of each follows.

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 an operating cost of at least $500 million/yr. To approve this project, not only must the U.S. Congress agree, which we have seen is difficult, but all of our foreign partners must agree as well. This introduces an even larger element of instability into the system. Any one of the partners 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 that has generated many Nobel prizes. ITER, on the other hand, is a power plant, and a very expensive one at that. This is not usually a fitting subject for a vast international collaboration. It is not surprising that the project is dead. Fusion research is probably best not done on a world scale.

We are often told to be more politically astute in promoting fusion with our congressmen and senators. This author certainly does not advocate ignoring politics; 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 that 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, the American Association of Retired Persons, the National Rifle Association, 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[9] (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 spinoff. 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 certainly be accused of "bait and switch." 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 may 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 1970s and early 1980s, but almost nothing seems to have been done on the project since then. A very convincing article on the subject is Ref. 10, in which Bethe makes the following case[a]: 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 to 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, which is that a fission fusion reactor can supply fuel to many more satellite reactors than a breeder. Bethe's article tabulates the number of reactors a fission fusion or breeder reactor can supply, depending on the type of reactor and blanket. For instance, a hybrid with a thorium blanket could provide fuel for five light water reactors or 16 advanced reactors. This is in contrast to a breeder, which provides fuel for 0.7 of the former or 2.7 of the latter.

This reflects the fact that fusion is neutron rich and energy poor, while fission is energy rich and neutron poor. In this sense at least, it is a perfect marriage. This is a tremendous advantage to such a system, as Bethe points out. Since there will be relatively few fission fusion plants (FFPs) compared to the total number of power plants, these can be run, or closely monitored by, the government in highly secured facilities. Fuel would be transported 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 downtime would be greater. Where the FFPs are not the primary energy producers, the entire system could tolerate this much more easily than if all plants were fusion plants.

The temper of the times is certainly one of minimizing government involvement in energy production. Unfortunately, with any nuclear option, including fusion, this will not be possible. If the fuel is ^{235}U mixed with ^{238}U in a subcritical mixture, the government will be intimately involved in the isotope separation to make sure no ^{235}U is clandestinely diverted. If the fuel is ^{239}Pu, there will be an intrusive government presence at every power plant to prevent diversion. We have just discussed the necessary government involvement in breeding ^{233}U.

Now let us consider fusion. 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 rogue nation (or even power plant owner or operator) could very easily include ^{238}U in the blanket (especially in a liquid or flowing blanket), rapidly breed plutonium, and produce atomic bombs. To guard against this, there will necessarily be an intrusive government presence at every fusion plant. A fusion economy would present real proliferation dangers, which have thus far received very little attention.

In 1985, the National Academy of Sciences (NAS) reviewed fission fusion.[11] 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 would be if produced from FFPs, and the NAS saw no compelling reason to proceed with fission fusion. Since the energy content of

[a]More recently, in a private communication from July of 1993, Bethe has argued against fission fusion, saying that with coming nuclear disarmament, the world "is awash in nuclear materials" and that he supports the mainline fusion effort.

mined natural uranium without breeding is less than that available from coal, it is by no means an inexhaustible energy supply. Therefore, the NAS could foresee a time in which the economics might ultimately favor FFPs as supplies of uranium diminish. 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. As we have pointed out, numerous respected experts[3,4,10] regard a plutonium economy as a tremendous potential danger to the world, one to be avoided if at all possible. Plutonium may be a bargain, but is it a bargain with the devil?

The ^{232}Th-^{233}U cycle has important 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 10 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 an 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 $^{238}\text{U} \rightarrow {}^{239}\text{Pu}$ versus the $^{232}\text{Th} \rightarrow {}^{233}\text{U}$ in *only* economic terms misses a very, very important issue. This is particularly true because 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 1980s 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 1970s and 1980s than most people (including myself) realized then, and it is still more compelling now. First of all, there is Fig. 1. The NAS argument that fission fusion should ride fusion's coattails is obviously obsolete. There are no coattails to ride anymore. Another aspect to Fig. 1 is that Bethe and others who discussed fission fusion in the 1970s assumed that fusion machines would achieve $Q = 1$ in the 1980s, and this obviously did not happen.

Another change is that the nuclear industry, which was weak and unpopular in the 1970s, 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 joining with the nuclear industry. But would we do this? Would they have us? Are fission and fusion more naturally allies or competitors? Furthermore, can one add weakness to weakness and get strength? This author's contention is that the nuclear industry will and must come back and teaming with it will help both fission and fusion. He sees fission and fusion as allies, not competitors. An entire issue of *IEEE Spectrum*[1] claims that over the next decade, the nuclear industry will come back. 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*[12] 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 that could bring the nuclear industry back is concern over global warming and greenhouse gases. While nuclear power plants have their own particular waste difficulties, discussed later in this paper, their competition, fossil fuel plants, are far from pollution free. The greenhouse gases 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 or a modified version of it, 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 develop a safer nuclear fuel cycle, enhance fusion research, and reduce global warming.

Despite the disadvantages and dangers of the fast breeder fission reactor, it is one option for an inexhaustible energy supply. France and Japan, among other nations, had long-term programs to develop the breeder. Both of these programs 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 1980s was the end of the Cold War.

Suddenly, there is a great deal of nuclear fuel, in the world's bombs, that 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 this nuclear material in reactors, there is really not that much of it. If one assumes that 10% of the energy in a 1-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 ~3 days. The world's 10 000 bombs would run 100 such power plants for ~1 yr. 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 be used up long before the FFP produced its first gram of nuclear material.

Another rather astounding and very recent turn of events is the U.S. government's balanced budget in FY 1998 and surpluses for the foreseeable future. It is tempting to think that fusion is now out of the woods, particularly since part of the surplus is to go toward funding scientific research. However, this is unlikely to be the case, in part for the reason already discussed. Furthermore, fusion was not among the priorities announced for scientific research.[13]

Thus, recent events have altered to some extent the arguments for and against fission fusion that were made in the 1970s and early 1980s. While some events argue for fission fusion and some against, this author sees the overwhelming tendency of recent events as favoring 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 have been three large tokamaks: TFTR at Princeton (unfortunately retired in FY 1997), JET in England, and JT-60-U in Japan (both of which are still operational). (Here, "large" means having the ability to inject ~40 MW of beam power.) Also, there are two smaller tokamaks: DIII-D at 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 Sec. III.

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 D-T, either steady state or at high duty factor, and have a thorium blanket. In addition to research on advanced operating modes in tokamaks, high duty factor operation, and thorium blanket science and development, an important goal 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 paper, 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 probably be much less than the world's potential investment in 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.[7] Any tokamak running at high duty factor almost certainly has to use superconducting toroidal field coils to minimize power input. For instance, the toroidal 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 and it would therefore be in a much less stressing physics regime. Nevertheless, issues regarding high duty cycle and neutron flux 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 facility, and to produce a useful product along the way.

The foregoing 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 laboratory 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 gamma particle 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 timescale, and in a way that could be much more easily integrated into existing power grids, than a commercial tokamak reactor that 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 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 Bethe[10] said, "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."

III. THE SCIENTIFIC CASE

III.A. The Tokamak

The tokamak has certainly been the most successful fusion device worldwide for decades. However, tokamaks 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 yr ago because they offered the optimum means to confine a plasma. There were many alternate schemes then, and none could come even close to doing what tokamaks could. This is still true today, except that tokamaks have progressed even further. There is now a worldwide infrastructure supporting tokamak confinement, an infrastructure consisting of thousands of people who have worked together for decades. No other confinement scheme has, or will have in the foreseeable future, anything close to this. This author will gladly bet anyone that if the U.S. magnetic fusion project drops tokamaks in favor of some other confinement scheme, say, stellarators or reversed-field pinch reactors, in 15 yr, these will not be where TFTR is today. Let us define this milestone as $\eta Q \gtrsim 0.25$ (η is the efficiency of the driver), 10^{19} neutrons per shot and a reasonably clear technical approach to steady state or high duty factor operation. Note that other possible confinement systems will have to get over not only 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 off funding just as they are doing today with tokamaks. It is also notable that no American alternate magnetic confinement scheme today can achieve what tokamaks achieved 15 yr ago. 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 only the tokamak can produce reasonable amounts of nuclear fuel on this timescale.

This section reviews briefly where tokamaks are and were and discusses the advanced operating modes that have been discovered in the last few years. A very rough schematic is shown in Figs. 2a, 2b, and 2c, 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-1970s: Advanced Toroidal Facility[14] (ATC), ORMAK type B (ORMB) (Ref. 15), Tokamak Fontenay-aux-Roses[16] (TFR), and ST (Ref. 17); the mid-1980s: TFTR (Ref. 18), Alcator C (ALCC) (Ref. 19), Doublet 3 (DOUB3) (Ref. 20), Princeton Large Torus[21] (PLT), JET (Ref. 22), T10 (Ref. 23), and Axially Symmetric Divertor Experiment[24] (ASDEX); and the mid-1990s: DIII-D (Ref. 25), JET (Ref. 26), TFTR (Ref. 27), and JT-60-U (Ref. 28). The figures of merit are (a) triple fusion product $n(0)T_i(0)\tau_e$ in keV·s/m^3, (b) input power in megawatts, and (c) total D-T fusion neutron production rate in neutrons per second. The latter was obtained either from the actual rate quoted in the references, the

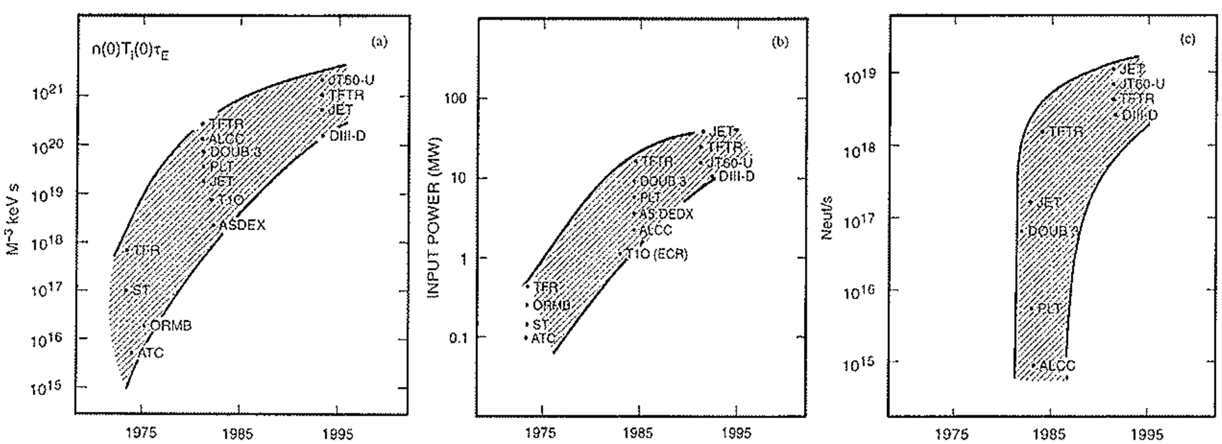

Fig. 2. Figures of merit for recent tokamaks: (a) triple fusion product, (b) input power, and (c) D-T neutron production rate.

D-D neutron rate extrapolated by the authors of the references, the D-D reaction rate multiplied by 200 if the reference did not give the extrapolation,[29] or an approximate calculation from profiles and known reaction rates. So far, only TFTR and JET have produced D-T plasmas. For all 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 Fig. 2. First, the tokamak project has made tremendous progress in the last 20 yr; 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} n/s, which corresponds to a neutron power of ~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, 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,[30]

$$\begin{aligned}\tau^{\mathrm{ITER89\text{-}P}}\ (\mathrm{s}) &= 0.048 M^{0.5} I^{0.85}\ (\mathrm{MA})\ R^{1.2}\ (\mathrm{m}) \\ &\quad \times a^{0.3}\ (\mathrm{m})\ k^{0.5} n^{0.1}\ (\mathrm{m}^{-3}) \\ &\quad \times B^{0.2}\ (\mathrm{T})\ P^{-0.5}\ (\mathrm{MW})\ , \end{aligned} \tag{1}$$

where

M = isotopic mass number

I = current

R = major radius

a = minor radius

k = elongation

n = electron density

B = magnetic field

P = heating power.

One of the most startling tokamak results of the 1980s was the discovery of the H mode, originally in ASDEX (Ref. 31). As neutral beam power increases (originally only in 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 mode is characterized by a factor H, which is the ratio of confinement time to that predicted in Eq. (1), with H typically ~2. In some recent very high mode studies in DIII-D, the H factor occasionally gets as high as 4 (Ref. 32). It is now reasonably well established that plasma edge is responsible for this transition (Ref. 33). 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 (ELMs), which may either abruptly disrupt the plasma back to the L mode or build up gently and limit further confinement. In this latter state (grassy ELMs), the H mode can be in a nearly steady state.

Another important advance is the more recent understanding of beta limits in tokamaks.[34] Troyon calculated the beta limits under ideal magnetohydrodynamics (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

$$\beta_N\ (\%) = \beta_T\ (\%)\ a\ (\mathrm{m})\ B\ (\mathrm{T})/I\ (\mathrm{MA})\ . \tag{2}$$

The stability of $n = 1$ modes generally limits β_N to ~3 if there is no wall stabilization and to values that 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 of these but focus perhaps a bit more on JT-60-U. One recent advance is the development of negative-ion sources and accelerators. With these, the JT-60-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.[28] 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.

JT-60-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

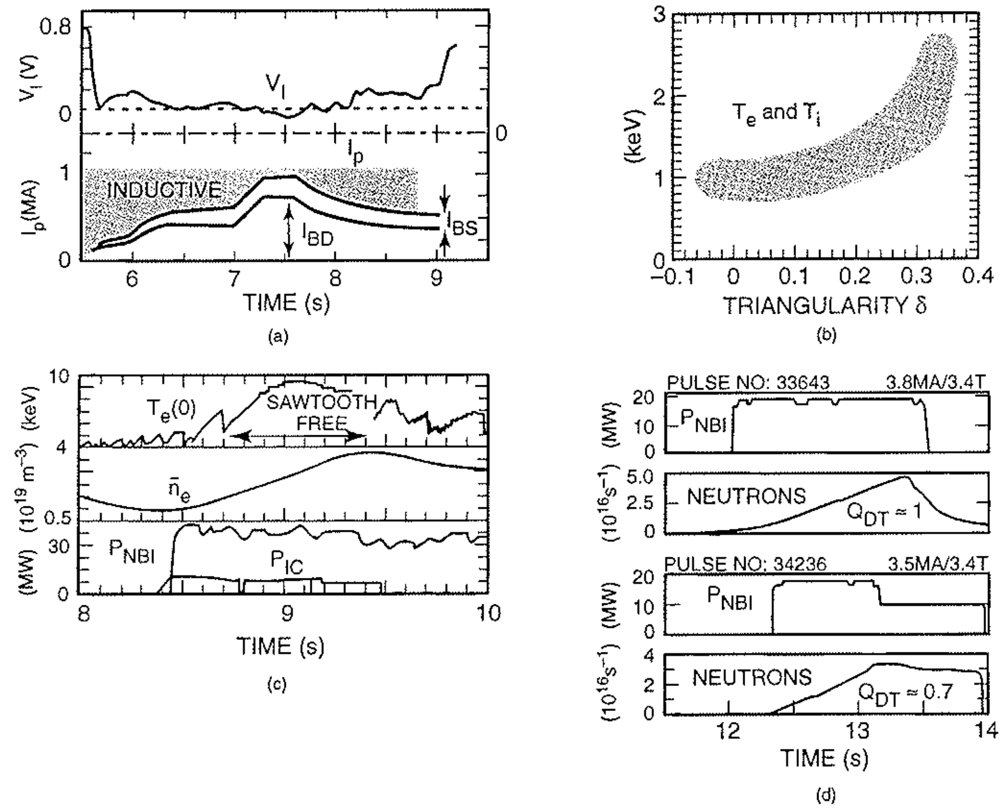

Fig. 3. (a) Loop voltage and inductive, beam-driven, and bootstrap current for JT-60-U, (b) edge electron or ion temperature for JT-60-U as a function of triangularity, (c) electron heating in a sawtooth-free plasma due to neutral beams and ion cyclotron resonance heating in JT-60-U, and (d) neutron rate as a function of beam power for disrupting and nondisrupting plasmas in JET.

the edge temperature (electron or ion) as a function of triangularity parameter δ (Ref. 35). 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. 35, 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, as reported in Ref. 36, the NRL program suggested in the 1970s that stabilizing the sawtooth oscillation was likely to be the most effective method of electron heating (although Ref. 36 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 that 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 (Ref. 26). The top graphs are plots of beam power and neutron rate as a function of time for a plasma that achieves a D-T Q of unity. It is in an H-mode plasma during the period of no ELMs. However, the plasma ultimately disrupts. Also shown in Fig. 3d are plots of beam power and neutron rate for a different shot where the plasma is in an H mode but is limited by low-amplitude ELMs. The plasma is in nearly steady state and generates a D-T Q of $\sim$0.7 for as long as the discharge persists. JET has in fact demonstrated H-mode plasmas, limited by grassy ELMs, that are steady for 20 s. 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.[18,37,38] 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

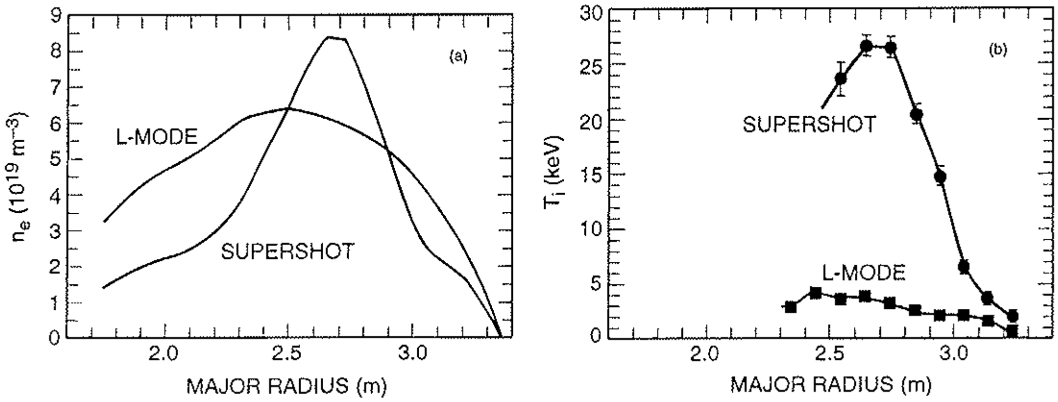

Fig. 4. Comparison of L-mode ($\tau_E = 0.06$) and a hot ion supershot ($\tau_E = 0.18$ s) for $P_{NB} = 22$ MW, $I = 1.4$ μA, $B = 4.7$ T. Except for the limiter conditioning, the parameters of the two shots are the same.

plasma is both heated and, to a large extent, fueled by the beam. The energy is very well confined there, with confinement time typically two or three 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 (Ref. 39). 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 grâce.

Another advanced operating mode is the reverse-shear mode. It is interesting that the advantages of this operating regime were first predicted theoretically.[40] 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 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 diagnostic, which directly measures the poloidal field and therefore the q profile. The second is a reliable setup 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 JT-60-U (Ref. 28). 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 toroidal 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[41] from TFTR are 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 microinstabilities, in this case the trapped-electron mode and the ion temperature gradient mode. 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 JT-60-U experiments is that these invariably end in disruption after some time. The DIII-D group has done some interesting research on this,[42] 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 (Ref. 43). Also shown are various L modes (dots) and H modes (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 advances 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 JT-60-U but to

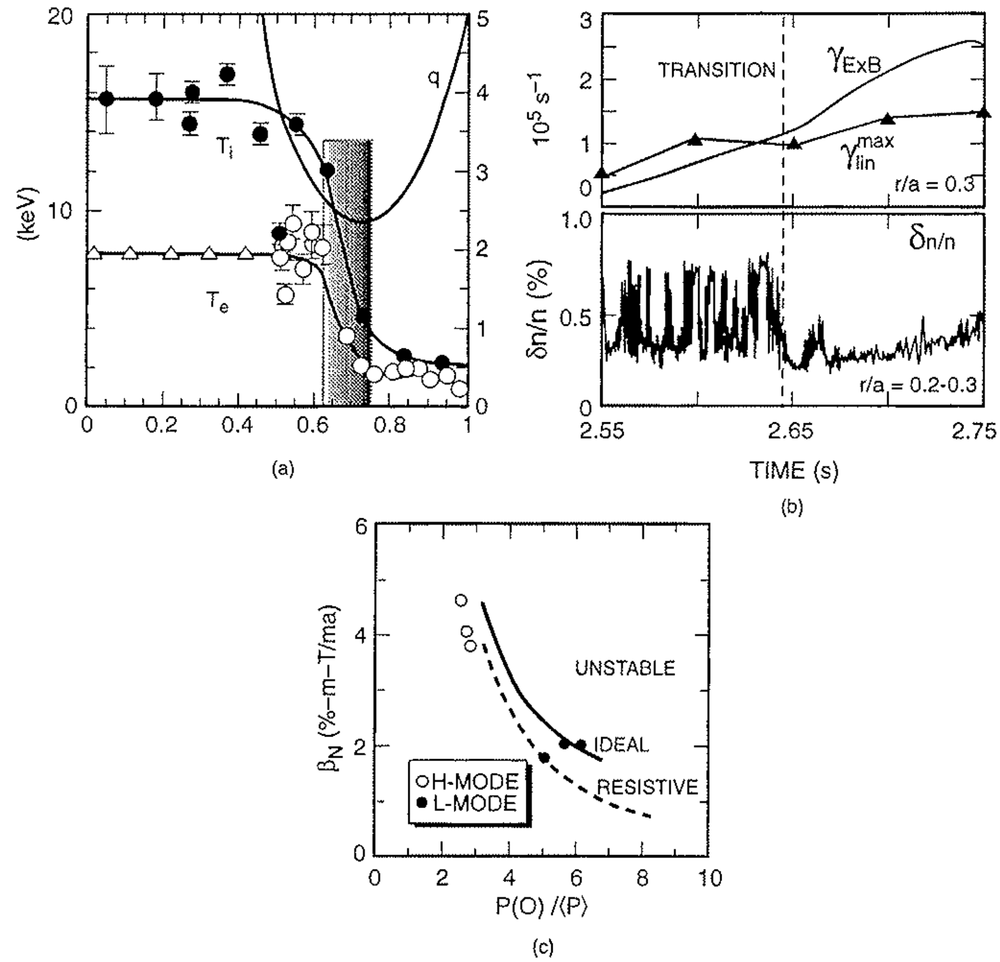

Fig. 5. Reverse-shear states in tokamak plasmas: (a) the temperature and q profile in JT-60-U showing region of inhibited transport, (b) the growth rate for DTEM and ITG instability and rotational shear rate as a function of time for TFTR. Also shown are measured density fluctuations as a function of time and (c) various states of disrupting and nondisrupting plasmas for DIII-D for L-mode (high $P(O)/\langle P\rangle$ discharges at H-mode (low $P(O)/\langle P\rangle$ discharges shown against MHD and resistive stability boundary.

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, TPX (Refs. 44 and 45), has already been proposed in the U.S. fusion program. 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 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 for 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 a β_N of 2 or 3. Furthermore, on perusing Refs. 44 and 45, 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 s. 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 they 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, has been operating for a while now in France.[46] 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,[47] although on a much larger scale than what is proposed here.

Will a tokamak itself ever evolve or progress to an economical fusion or fission fusion plant? Twenty-five years ago, informed opinion dismissed this possibility because of the inherent pulsed nature of the current drive. However, a quarter of a century of research has conclusively demonstrated continuous-wave (cw) or long-pulse operation with beam, microwave, or bootstrap current drive. Future research on, for instance, α channeling[48] could further enhance the reactivity. However, scaling with currently known laws to ITER size, one is led to very large but rather marginal machines for pure fusion.[49] 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,[50] 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 building 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 yr, and it may fail. Then another ST must be built to run at high duty factor, which might take an additional 10 yr, for a total of 20 yr before it could 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 building on the tokamak program is that by exploiting a bird in the hand, it jumps right to the second stage and cuts off 10 yr. 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. Furthermore, lower-cost research on alternate concepts could proceed while the tokamak research FFP was being built and operated. Abraham Lincoln said it best: "Don't change horses in the middle of a stream."

III.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[51–53] and the possibility of a commercial-sized fission fusion power plant based on a tokamak.[54,55] 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 ~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 as

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

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 more than 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 ~20 times the Q of the fusion reactor alone. We assume that with all of the experience acquired, one could now build such a 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, 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 purchased from Russia, or from a separate breeder yet 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} n/s and running cw, would require ~1 kg of tritium per year.) However, since tritium breeding is such an important part of the overall picture, producing fuel without breeding tritium could not constitute a convincing energy program for very long.

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, $n + {}^{238}U \rightarrow 2n + {}^{237}U$). There are a variety of materials that can be added to the blanket to increase the multiplication of neutrons. The material particularly emphasized in Refs. 51 and 52 is beryllium. Finally, there is the reaction of ultimate interest for a thorium blanket,

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

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 I show values of ζ and λ taken from Ref. 51 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. One calculation that does include this, the "engineered blanket," which accounts for various different regions and structural materials, is shown in the last row of Table I. 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 ~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 I) 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."[56]

TABLE I

Production 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

The other approach is to minimize the fission reactions in the blanket so that the fusion plant almost exclusively generates 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, which is almost surely a much safer approach: The fusion plant produces fuel that is burned in other power plants, which are 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 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 thorium as the ^{233}U builds up, since fast fission of thorium requires neutrons with energy >1 MeV. The moderator favored in Refs. 51 and 52 is beryllium, but it is pointed out there that there are other possibilities as well.

References 51 and 52 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 a still lower concentration 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.

IV. THE NAVAL CASE

One ordinarily does not think of the Navy as an organization that would support the development of magnetic fusion. However, there is at least some consideration within the Office of Naval Research[57,58] (ONR) 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.[59] A careful examination of Fig. 3 of Ref. 59 does indeed show clearly the Naval motivation. Unfortunately, onboard 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 that 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.[60] 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 that is a ^{233}U-^{238}U mixture. Actually, the Navy is very willing to use ^{233}U. In the 1960s and 1970s, the Navy developed a light water breeder to breed ^{233}U for ship propulsion.[61,62] 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. Furthermore, the Navy is a small enough energy consumer (compared to the civilian economy) that it might be able to use tritium from decommissioned bombs or other sources and avoid initially the necessity of breeding. Hence, while the Navy almost certainly will not take the lead in or pay for a large fission fusion program, it could be a first customer, as well as a beacon to guide the civilian economy toward a safer nuclear fuel cycle and ultimately toward fusion.

Finally, 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 electron cyclotron resonance heating (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 ~5 MW, nearly 3 orders of magnitude larger than the radar tube and at least 1 order of magnitude greater than conventional gyrotrons. However, the Navy has very significant talent and experience that 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[63] developed in NRL's Plasma Processing Accelerated Research Initiative could find application in simulation of the tokamak divertor scrape-off region or simulation of microinstabilities in the interior plasma.

V. THE ENVIRONMENTAL CASE

These days, one cannot simply advocate nuclear power and be unaware of the environmental issues involved. The buildup of spent nuclear fuel and the residue from government weapons development present the world with a very difficult challenge. An entire issue of *Physics Today*[64] was devoted to this problem. Right now, American policy is to let the residues build up on site and wait until some risk-free, universally agreed upon solution emerges. When receiving his Fermi award, R. Garwin[65] blasted this do-nothing policy, especially as regards the buildup of plutonium. 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 but many people would think it has religious aspects as well. However 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, the Waste Isolation Pilot Plant in New Mexico for low-level waste, and Yucca Mountain in Nevada for high-level waste. The latter, particularly, is running into political problems. What we would like to do with nuclear waste is treat it and forget it. However, North[66] proposes a new paradigm, one that 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 a 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.[67] 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 NAS, and their review was rather negative.[68] 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 yr. However, as mentioned, the relevant timescales are much greater than 20 yr. The author feels that this option should be continued to be examined vigorously.

VI. CONCLUSIONS

So what is the American fusion community to propose and argue for? This paper provides one observer's answers. First, since ITER clearly will never be built, the United States should pull out of the project, not join in other large follow-on international fusion projects, and use its fusion resources domestically. Secondly, it should propose the building of a tokamak like JET, JT-60-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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