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An Alternate Development Path for Magnetic Fusion

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Mid-century requirements for carbon free energy are daunting. Perhaps fusion could play a vital role. One of rather few possible solutions for sustainable development might be the fission fusion hybrid coupled with transmutation of the long lived actinide wastes. This paper suggests such an alternate development path for fusion, one that could lead to the production of multi-terawatts of carbon free power by 2050.

KEY WORDS: Fusion energy, fission-fusion hybrid.

I. INTRODUCTION

The American fusion community is now seriously making the case for a scale up of the program with either FIRE [1] or participation in ITER [2,3]. It is now thought that one or the other could lead to a reasonable development path for fusion. The purpose of this article is to sketch out another possible development path, the fission fusion hybrid. This could provide a development path in competition with these two, or else a fall back position in case neither FIRE nor ITER sells.

In the case of FIRE, it may not sell because most other parameters are sacrificed for ignition and ignition alone. For instance its magnetic field is 100–120 kG. This is rather far out of the mainstream of other reactor concepts. In both the old and new ITER a field of around 55 kG is used [2,3]. The original ARIES-RS design [4], exploiting advanced tokamak operating regimes, proposed a field of about 80 kG, but this has since been revised downward in ARIES-AT [5] to the usual value of about 55 kG. Also, probably because of the high field, there is no provision for heating FIRE with neutral beams, almost certainly the principal driver for a commercial reactor. Thus FIRE will teach us very little of what we need to know to operate a commercial reactor. Furthermore, there will only be 3,000 shots in FIRE.

ITER is more in line with what are viewed as commercial reactor parameters, but it is very expensive and it takes a worldwide collaboration to build it. It started as a 20 year project for $20B capital and operating costs, and would generate about 1.5 GW of fusion power. We refer to this as the old ITER [2]. In 1998 America pulled out. It was then scaled down with Europe, Japan and Russia planning to go ahead with a half-sized version, about $10B capital and operating costs, and generating about 400 MW of fusion power. We refer to this as the new ITER [3]. However, empirical evidence shows that in fusion, these collaborations are difficult to set up and sustain. It has been 18 years since ITER (starting out as INTOR) was first proposed. In not much more time than this, TFTR was designed, built, operated and decommissioned. Furthermore, regarding fusion, the ultimate goal is to build power plants. This cannot be viable as a world wide effort, because in the end, each country will build its own power plants.

Thus this author is drawn to the conclusion that it is reasonable and proper to put forth an alternate development path, the fission fusion hybrid. Certainly he does not use rose colored glasses to envision a large

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scale up of nuclear energy as a panacea; it must itself clear many technical and societal hurdles. However equally, where global warming is almost sure to become more and more important, and the requirements for carbon free energy will increase, it is simply not responsible to neglect it either. Recently van der Zwann [6] has done a balanced analysis of the opportunities and pitfalls of a tenfold expansion of nuclear power (to 3 terawatts).

In making the case for the hybrid, let us consider what power requirements are likely to be by mid cenury. Recent studies [7] have indicated that considering world development, population increase, improvements in energy efficiency, and global warming, the world will need 10–30 TWs of carbon free power by 2050. Another paper has looked into options for achieving this [8]. The startling thing is that there are not very many ways to do this; all of the possibilities evaluated require radical departures from what is done today. We have less than 50 years to make these changes in the power production and distribution infrastructure. These changes are far beyond what has occurred in this sector in the last 50 years. As the co-author of Ref. 8 advocating the fission fusion hybrid [9,10], I sketch out here, in more detail than was possible there, a development path that could lead fusion to play a crucial role in economically generating multi terawatts of carbon free power, in an environmentally sound way, by 2050.

II. WHEN HAS FUSION BEEN PROMISED? WHEN IS IT NEEDED?

Here we consider an alternate strategy for magnetic fusion, the return to fission fusion. To motivate this, let's see how the time scale for the implementation of fusion has evolved. As an adolescent, around 1955, I remember reading a *Life* magazine article predicting fusion powered rocket ships in about 30 years. Now jump to 1990 when Secretary of Energy Watkins commissioned the Fusion Policy Advisory Committee (FPAC) to report on the implementation of fusion power. They concluded that a commercial reactor could be built in 50 years, by 2040, assuming large fusion budgets and also assuming that the whole world cooperates. [To see this go to www.doe.gov → Sources and Production → fusion → FESAC → Report on Criteria, Goals and Metrics, Oct 1999.] We have already lost 13 years from this schedule. Since 1996 fusion had been recast as a science project. Also, as far as I could see in 2002, nothing on the DOE Web site discussed implementation of fusion in the economy. Hence, when cast as a science project, one could only conclude that the DOE was thinking of an economic benefit from fusion occurring at best late in the century, perhaps not even until 2100. More recently, however, there has been talk of a single fusion power reactor going on the grid in 35 years. It is as if no time were lost since 1990! In fact it will now be on line sooner than under Admiral Watkins' original schedule even with the smaller new ITER (if the international collaboration route is chosen) and lower budgets. It would still be much later in the century before fusion could have an economic impact. Hence, before fusion can payoff even a small part, of what was invested in it, we expect it to be supported by the government for about 100–150 years, that is through about 5 or 6 generations of sponsors. I believe it is absolutely essential for the fusion project to deliver much sooner.

Specifically the fusion project should aim toward meeting mid-century energy requirements. A simple fact is that by mid-century, the world will have ten billion people; all of them will demand a middle class life style. As a community we should absolutely reject the alternative, namely, that the majority of these people are condemned to live in poverty. A simple, canonical number taken from Ref [7] is that by 2050, the world will need an additional 10–30 terawatts of carbon free power. Thus under the current plan, fusion is absolutely unable to make an impact on the crucial mid-century energy requirements. Below we give estimates of various world energy resources in terawatt years [11]:

Source	Energy (twyrs)
Fossil	7500
Coal	5000
Oil	1250
Gas	1250
Mined uranium	60–300

Clearly for large amounts of carbon free power, not only is it likely that nuclear power will be required, it is also likely that either breeding of nuclear fuel, or uranium from sea water, or more likely both, will be required as well. Ref 6 points this out as well. Since the mined uranium estimate is the energy content of the ^{235}U, and breeding makes available the energy content of the ^{238}U (or ^{232}Th for the thorium cycle), it multiplies the available energy by more than a factor of 100.

III. SOME FUNDAMENTALS

Nuclear fuel can be bred via either fission or fusion. There are two cycles, breeding ^{239}Pu from ^{238}U, or breeding ^{233}U from ^{232}Th. The energy resource from the thorium cycle is about twice that from the uranium cycle.

To eliminate the proliferation hazards of the raw fuel (that is the possibility of chemical separation of nuclear weapons material), we consider only the breeding of ^{233}U which can be mixed with ^{238}U in a subcritical mixture. A fission breeder has the advantage that the technology is here and now. However it has strong disadvantages as well. A fission breeder can typically supply itself and a single other burner. Also it must operate for a long time before sufficient fuel is bred. If the fission breeder route were aggressively pursued starting now, it is not clear that sufficient fuel could be bred to satisfy mid century energy requirements.

A fusion breeder is certainly not here and now, but it does have significant advantages if it could be made to work. First, there is almost no doubling time consideration. Second, each fusion breeder supplies roughly 10 burners. Thus there are many fewer fusion breeders than there would be fission breeders. Since any breeder (whether fission or fusion) is a proliferation hazard, it would have to be secured, with fences, guards, and the like. However, in a fission fusion economy, only about 10% of the reactors would have to be so secured, instead of half in a fission breeder economy. Thus for a thorium fusion breeder, only thorium enters, only a sub-critical mixture of ^{233}U and ^{238}U leaves. The reason a fusion breeder has this advantage is that each 14 MeV fusion neutron, in a well designed blanket, breeds about one triton and one ^{233}U. However, when the ^{233}U is burned in a conventional burner, it releases about 200 MeV. So instead of using the kinetic energy of the neutron to boil water, one uses its potential energy to breed 10–15 times more fuel. Thus by going to fission fusion, we increase the fusion Q by about a factor of 15. In other words, fission is energy rich and neutron poor, while fusion is energy poor and neutron rich, a perfect marriage. Hence, the fusion and fission burner communities (if not the fission breeder community) are natural allies, not natural competitors.

IV. AN ALTERNATE DEVELOPMENT PATH FOR FUSION

Given this, can fusion contribute to mid century energy requirements? It seems to me that it can. TFTR and JET have each generated 10^{19} neutrons in DT plasmas with 40 MW of beam injection in a one second pulse. The Q is about 0.5. However, these machines have no average power capability. The essential next step, in this development path, is to dip our plasma physics buckets where we are, that is build another TFTR (now with Q ~ 1), but with average power capability in a DT plasma. (Actually it would probably have to be somewhat larger than TFTR so as to make room for the blanket.) Let's call this a scientific prototype. Now 40 MW of beam power generates 40 MW of neutrons. These 40 MW of neutrons then generate 600 MW of ^{233}U. This could be used as nuclear fuel. While many plasma physics problems would have to be successfully confronted and solved in building such a device, equally important would be the nuclear engineering. It would have to be located at a nuclear facility, preferably one with a great deal of water and power, perhaps Oak Ridge or Savannah River. Furthermore, when the experimental period is over, the machine will be able to do something for a sponsor. It could generate 40 MW of 14 MeV neutrons. These can be used for a number of applications. Since generating sufficient power for mid-century is the main motivation for the program, I feel convinced that the best application is the breeding of ^{233}U. However perhaps other applications then would garner more support, transmuting nuclear waste from previous energy or weapons programs, for instance. In any case, with the scientific prototype, fusion would demonstrate a capability to actually do something useful and difficult, even if not yet economically relevant.

Let's estimate very roughly how much such a program would cost. CIT/BPX was proposed at \$1.6B in 1991, and TPS was proposed at \$0.75B in 1996. Since there is no requirement for ignition in the scientific prototype, the physics regime is less stressing than BPX, so we estimate the cost for the plasma physics as at least \$1B. However, there is also the nuclear engineering and materials research. If this costs the same as the device itself, the total cost is about \$2–2.5B over about 15 years, or about \$150M/yr. Let's say congress authorizes this program, but in a worst case scenario, tells us to take it out of our \$250M/yr budget. We could (and should) do this. There would still be \$100M/yr for other fusion research, perhaps 5 or 6 advanced concepts of small to mid size. Of course these would now include not only plasma science, but also nuclear engineering. For instance one of them might involve liquid or flowing liners.

Where might this lead? If we think of a TFTR sized Q ~ 1 tokamak as a scientific prototype, it is natural to consider the next larger, ITER sized Q ~ 10 tokamak, but operated as a breeder, as a commercial prototype. However, instead of operating as a world wide consortium, it would operate within the national program. As we will see, its large cost would now be more than offset by economically producing a vital commodity. We consider the old ITER [2] which has a lower cost per neutron than the new ITER. Also it makes more conservative assumptions than ARIES RS or AT. Taking these estimates, we envision 150 MW of beam producing 1.5 GW of neutron power. However, in a breeding blanket, this produces about 24 GW of ^{233}U, enough to power 8 conventional nuclear reactors of 3 GW (1 GW electric power). Let us estimate the fuel cost. The ITER estimate is that the total capital and operating cost will be about $20B over about 20 years. We assume that the cost of the blanket to breed ^{233}U plus T is about the same as the cost of the tritium blanked included in the ITER estimate. This translates into a fuel cost of about a penny and a half per kilowatt hour as the fuel cost for a nuclear power plant. Even if we have significantly underestimated the cost in these admittedly very crude estimates, it could still be economical. For instance the cost of gasoline or oil, at a dollar a gallon, is just under three cents per kilowatt hour. Since these are occasionally used as fuels for power stations, this is a cost that power plant operators are apparently willing to pay for their fuel.

Let's see how this plan might affect mid-century energy needs. We assume the lifetime of such tokamak experiments is about 15 years. Thus starting right away in 2003, the scientific prototype takes us to 2018. If this is successful, the commercial prototype takes us to 2033. If this is successful, the world builds several hundred or a thousand such fusion breeders to breed several terawatts to ten terawatts of carbon free fuel by mid-century. There is a real chance that the fission fusion hybrid can go a long way toward economically satisfying carbon free world energy requirements by mid-century. Even its most optimistic proponents do not see how fusion alone can do this.

The above development path seems to me like a much more sensible use of limited fusion resources than a small ignition experiment far off the main development path, or the endless delay and indecision of a worldwide consortium. This is doubly true where the Italians may do an ignition experiment anyway. In conjunction with an American breeder experiment, its impact could be greatly magnified. For instance their experience would surely help greatly as we move from the scientific to the commercial prototype. However, we Americans should concentrate on cracking the really tough nut in fusion research, namely figuring out a way to satisfy mid-century energy requirements.

Of course for the world to accept nuclear power on this scale, it would have to be convinced that both the proliferation and waste problems were well in hand. Using the thorium cycle solves the proliferation problem, at least for the raw fuel. The waste disposal problem is much more difficult. If we need one Yucca Mountain for America's hundred nuclear power plants, then by mid-century the world will need some 30 to 100 Yucca Mountains for the proposed 3–10 TW of total nuclear power. However, as we see from today's headlines, getting even one Yucca Mountain is difficult enough. Yucca Mountain will almost surely be a temporary repository, but the world's people will never accept the idea that we have created a plutonium or ^{235}U mine which will last for millions of years. It seems as if transmuting at least the long lived radioactive wastes *must* be a part of the world's energy plan. While initial evaluations of transmuting the waste were rather negative [12], this author feels it is way premature to accept this as a final assessment. There are many possible ways of transmuting using fission reactors, fusion reactors or accelerators. Even if transmutation doubles the final fuel cost, it could still be cheaper than gasoline or oil, and this is before the cost of sequestration is added on to those fuel costs. While transmutation is not our problem as fusion scientists (unless a fusion reactor is used for the transmutation), we should take a great interest in it and be strong advocates for it.

The problem of waste disposal illustrates the great advantage of a pure fusion economy over a fission fusion economy. *In fact, one of the strongest arguments for a fission fusion economy is that it can be a stepping stone to a pure fusion economy.* With the experience of developing a fission fusion economy, a pure fusion economy may develop in the future. However, this is not our decision to make. The people to decide this are at least 50–100 years from even being born! The development of a fission fusion economy in the next 50 years may be the best way we can help these future generations switch to a pure fusion economy.

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