The Scientific Prototype, a proposed next step for the American MFE program

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For my entire career I have been very interested in both magnetic and inertial fusion. I have a reasonable knowledge base of both. However it has been nearly 30 years since I have been supported by MFE, and I have no desire to be supported by this organization in the future. I feel this situation gives me the freedom to speak in a relatively knowledgeable, but far less inhibited way than many of your other respondents.

I have been on record, for more than 15 years [1-9] as proposing that the MFE base program should be reoriented around a tokamak which I have called "The Scientific Prototype". While I have proposed this in the context of fission suppressed hybrid fusion, or more briefly fusion breeding, which I still strongly believe is the best way for fusion to go, it would be just as valuable for pure fusion, which I am sure is FESAC's main interest. This is not some new small device to add onto the base program, but it would be a unified, focused project which would replace the entire base program. I believe it would enormously advance fusion energy science. While there would be many participants, I see the natural leader as being the Princeton Plasma Physics Laboratory. However the scientific prototype is very much a nuclear facility, so it is unlikely that it could be too near the Princeton campus; also they would need a partner with nuclear expertise. General Atomics has experience in both arenas.

I will describe "The Scientific Prototype" shortly, but first let me begin by arguing that FESAC really has very little choice other than this. so to start, I will list other various possibilities, and argue why they are not viable options.

No to a burning plasma or ignition device: I have seen several proposals for this such as FIRE and IGNITOR. Each is a small device which sacrifices nearly every other vital parameter for ignition alone. For instance they are very high field devices, which are not in the mainstream of fusion research. In fact FIRE would only last for a 3000 shot lifetime. It is unlikely that either would blaze a path which fusion could follow. To my mind, they are stunts. In addition, we will hopefully soon have information on burning plasmas from ITER. Surely nothing we in the USA build in the next decade or so will tell us more. Furthermore, we will soon learn much more about ignited plasmas from NIF, assuming LLNL ever gets it to work. There is nothing we can do with the resources in prospect, and others will beat us to it, so let's forget about burning plasmas or ignition.

No to stellarators: Two American stellarator programs have already been cancelled. There is already a good bit of information on stellarators, and to my mind it is not very encouraging. The Japanese have both a large stellarator and a large tokamak. On the vital parameter of the triple fusion product, the tokamak's is 40 times larger than the stellarator's. In addition, the Germans are building yet a larger one. Realistically, we cannot beat them. If there is to be any success in the stellarator program, it will come from the Germans and Japanese. It might make sense to hedge our bet and invest a little in one of these programs, especially if they invest some in the scientific prototype. But regarding an American stellarator program, let's forget it.

No to an enhanced ST: First of all, the ST is immature compared to tokamaks. It has only contained about 250 kilo joules, as compared to about 10 megajoules in a tokamak. But mostly I believe the center post is a show stopper. The magnets certainly cannot be superconducting, so they will be an enormous power drain, probably more than any fusion power. In addition, how can one cool them and shield them in the presence of the large fusion neutron flux? It may be that an ST plasma can give reasonable fusion power on a pulsed basis, but there is no way it will extrapolate to a power plant. At best it is a stunt. Let's forget ST's.

No to a waste burning fusion reactor: The University of Texas has proposed a waste burner based on an ST. If waste burning is the goal, perhaps one can live with the power drain of copper magnets. However the ST is a relatively immature concept as compared with tokamaks. They are proposing to have a plasma, which we do not understand very well, and which contains many hundreds of mega joules in plasma, poloidal and toroidal field energy, an increase of about three orders of magnitude in a single step, just a thin wall away from a ton or so of plutonium and worse stuff. But mre important, what would a major disruption do? This has hardly been studied in ST's. Don't forget that 600 mega joules melts about a ton of copper. This is far too dangerous to attempt. In any case, fast neutron reactors like the Integral Fast Reactor, developed by ANL, has already proven it can safely burn all the actinide wastes of a light water reactor. Fusion may be going way out on a limb to solve a problem which may well have already been solved. In any case, UT seems to be partnering with Culham Laboratory as they upgrade their MAST ST. Let's wish them luck, but let's not put our own resources into it.

No to various other magnetic devices, centirfugal, levitated dipoles, etc: They are hopelessly behind tokamaks and have no prospect of catching up to them in the lifetime of anyone reading this. Let's forget about them.

No to mirrors and beam devices. The DT 90 degree scattering cross section is about 2 orders of magnitude greater than the fusion cross section. Anything based on a non Maxwellian distribution will become Maxwellian and lose confinement long before it fuses.

No to electrostatic devices: Any fusion reactor will have a size huge compared to the Debye length. The plasma will pick its own electric field and any attempt to influence it from the outside will be futile. It is rather like attempting to control a beta equals one million plasma with magnetic fields.

So what is left? First, the tokamak has had enormous success, but still has difficult hurdles to overcome. We have made an enormous bet on tokamaks over the last few decades, and I feel we have little choice but to continue to dance with the lady we came in with. Can we now just tell our sponsors that sorry, unfortunately we made a big mistake, give us a few of billion more and we will try the next thing. Of course not! Furthermore our sponsors have gotten their money's worth from thetokamak program.

Second, I firmly support a strong, well funded American MFE program as being vital to both the country and the world. But the current base program is centered on two rather small tokamaks, D3D and Alcator, and the spherical tokamak at PPPL, which I have argued is at best a stunt. Europe and Asia are now well ahead of us. The base program should not be preserved for its own sake if a better option is available. I believe the scientific prototype is such an option.

The idea of The Scientific Prototype is to build a tokamak of about the size of TFTR, JET or JT-60, but one which runs steady state or at high duty cycle in a DT plasma and breeds its own tritium. Without tritium self sufficiency, no fusion scheme, pure or hybrid makes any sense. TFTR and JET have already produced 20 MJ of neutron power in a one second pulse. Let's see if we can do this in a steady state. To do so, we would run in a plasma regime we are already reasonably familiar with; let's lower our plasma physics buckets where we are. In other words, let’s attack every problem of fusion *except* burning plasma and ignition.

I am no expert on breeding blankets, but from the diagrams I have seen it looks like a breeding blanket would have to be about a meter and a half in depth. This makes sense where the mean free path of a 14 MeV neutron in lithium is about 15 cm. Then consider TFTR with a minor radius of about 80 cm and a major radius of about 2.5 meters. Add enough room to put in a 1.5 meter blanket and we have a tokamak of a major radius of about 4 meters, a minor radius of about 1 meter and a magnetic field of about 5-5.5 Teslas. With all we have learned from TFTR, JET and JT-60 in the last 15 years, I believe it is reasonable to assume the scientific prototype will produce about 40 MW of steady state neutron power if driven by 40 MW of neutral beam power, or about 250kW/m^2 of wall loading.

I believe the best blanket would be a flowing liquid, which self anneals, and where the tritium could be separated out continuously. (Do not forget that any tritium left in a solid blanket for a year loses about 8% due to its natural decay.) A molten salt like FLiBe looks to me like good choice. The beryllium is a neutron multiplier, and of course the lithium would breed the tritium. As I said, I have little experience in blankets or fusion materials and would leave this to people more knowledgeable than I am. But I reiterate that breeding tritium and demonstrating tritium self sufficiency would be a crucial part of the scientific prototype.

While the tokamaks have made remarkable advances, they are hardly out of the woods. There are several crucial plasma physics problems to confront. First there is the issue of disruptions. Where a disruption might not be too damaging in the scientific prototype (contained energy of a few 10’s of MJ), it should be possible to see if we can eliminate or control them (i.e. detect and guide to a soft landing). This is of course absolutely essential for ITER which contains many hundreds of MJ. While disruptions are less frequent as the beta and current decrease, they are never eliminated completely. JET has published statistics on disruptions [10], and has shown that even for low current, low beta plasmas, the disruptivity is about 10^-2/ sec, in other words, a disruption every two minutes. Clearly this is intolerable in a fusion plasma. The steady state scientific prototype would be an ideal vehicle to do research on eliminating disruptions, research which would be crucial for ITER.

Secondly the well publicized results from JET and TFTR of Q~1/2 are for hot ion plasmas which terminate early and abnormally. For steady neutron production, which lasts as long as the pulse, the Q ~ 0.1. Thus the scientific prototype will have to either enhance the Q in the steady plasmas, or get the hot ion mode to run steady state.

Also the scientific prototype would have to operate in steady state (or perhaps pulsed at high duty factor). This means that the current would have to be driven almost entirely by bootstrap, rf and beams injected from the outside. The scientific prototype would be an ideal way to investigate this. ITER might well claim success in its goals without steady state current, but it could never develop into a reactor without it.

Finally, the scientific prototype would operate in an intense neutron flux environment. All diagnostics and materials facing the plasma would have to be properly shielded. As a simple example, this requirement seems to argue that the heating would have to be either ECRH or neutral beams. They alone have sufficient standoff.

Thus tokamaks certainly have plasma physics problems to solve, but have fewer than any other configuration. My assumption is that the scientific prototype will solve these problems.

Estimating the cost of the scientific prototype is far beyond my capability. However if we figure a $350M base program for 15 years, this would allow a cost of over $5B. This likely would be sufficient.

As far as future tasks, an obvious one to me would be to put some thorium into the flowing blanket and breed some 233U for existing reactors, as I have long advocated. The 40MW of neutron power would produce about 200-400 MW of 233U nuclear fuel. For the first time, fusion would produce something the world could actually use. The disruptivity of the higher current, higher pressure plasmas (necessary for pure fusion) is nearly two orders of magnitude larger than that of the lower pressure, lower current plasmas which are fine for fusion breeding [10]. Clearly pure fusion plasmas in a tokamak will be much more difficult to control. The tokamak program may well ultimately be forced to embrace breeding [7]. But this is an argument for another time; let’s first build the scientific prototype.

Nobody is ahead of us at this time and the United States, with its hundred nuclear reactors and vast experience in nuclear science should be able to do the scientific prototype at least as well, or better than any other country or group of countries. The project is both necessary and achievable. While we would manage the project, we would of course welcome international collaborators and expertise; some might even be willing to help with the funding. But to summarize, let's turn conventional wisdom on its head. Instead of sacrificing every figure of merit to achieve ignition or a burning plasma, lets sacrifice the burning plasma and achieve a significant advance of every other figure of merit necessary for fusion.

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