Journal of Fusion Energy, Vol. 5, No. 2, 1986

Introduction to Panel Discussions

Whither Fusion Research?

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An unnamed former fusion program director retired and felt he needed some time to think and meditate, and so he went off to a secluded monastery in Tibet. When he arrived, the major monk showed him around and told him the rules. The monk's final point was that the man would be allowed to say only two words per year, and they could only be spoken to the major monk on the anniversary of our friend's arrival. Our nameless director understood and set out on his meditation

After a year, our friend appeared before the major monk for his annual two words, which were, "Room cold." The monk nodded acknowledgement and our friend again went about his business.

A year later at the annual gabfest, our friend said, "Food bad." Again, the major monk nodded understanding.

Then the snows came and gave way to the summer and another year passed. On his third anniversary when our former fusion director appeared, he said, "I quit."

The major monk contemplated for a moment and then said, "I'm not surprised, you've been complaining ever since you arrived!"

I am not here today to complain, but my message is that I believe that the magnetic fusion program is ready for a rather dramatic change of course. My reasons are logic-based and not related to the current budgetary crunch.

Part of my message was delivered by Steve Dean for me at the recent Fusion Power Associates meeting. At that time some people commented that I was taking an extreme position to spur thought. To you here today I say that my recommendations are put forth not only to spur thought, but to spur actions that I think are needed now.

Let me start by discussing the Magnetic Fusion Program goal, namely the development of a practical fusion power reactor. My criteria for a desirable fusion reactor are as follows:

1. It must produce power at a competitive price.
2. It must work with high reliability, and maintenance must be easy, fast, and low cost. I suppose these could fit under the first criterion, but I break them out separately for a purpose.
3. Our fusion reactor must be safe and not environmentally insulting.

There is a significant segment of the fusion community that feels that the goal of practical fusion power can only be attained by first developing a detailed understanding of magnetic fusion plasma physics. While a general understanding is clearly a must, I contend that a detailed understanding is not necessarily required in the real world, as long as the understanding is sufficient to meet minimum design and operational requirements. After all, we do not yet fully understand spark-ignited gasoline combustion, and yet there are roughly 200 million automobiles usefully and happily running around the world!

The necessary minimum level of understanding is difficult to pinpoint, and trying to find it leads to some basic conflicts. For instance, the fusion physicist is trained and motivated to develop detailed understanding. On the other hand, the fusion engineer wants a gross understanding but does not need to know it all in order to build a practical, useful system. This problem is particularly significant these days when there is a major push to understand tokamak plasma physics.

In earlier years in fusion research the question was whether anything would ever work, and the fusion problem was fundamentally a physics research problem. The physicists clearly had to do "their thing" because there was nothing for the engineers to really sink their teeth into.

To determine if anything would work, physicists tried a whole raft of concepts. In the late 1960s it was not very clear where we stood, although some posi-

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0164-0313/86/0600-0101$05.00/0

tive physics results were beginning to emerge. I remember a particular standing committee meeting during that time; I think it was at Oak Ridge. In the midst of a heated discussion, Sol Buchsbaum made a particularly notable statement. He said, "If one of these things works, then they'll probably all work."

Time has proven Sol correct: most fusion confinement concepts that we have tried do work to a significant degree, although each has been carried to different levels of experimentation. That is indeed fortunate, because we now have a variety of concepts or combination of concepts to choose from.

Why do I make this point? It is because I believe that we have been blessed with a range of options for magnetic fusion power. Happily, we are not limited to one concept.

Let me now turn to the question of how long one pursues a concept or option. A useful principle has been discussed at recent meetings of the Department of Energy's Energy Research Advisory board. It is the concept of a "window" for research. The term "window" is roughly defined as the period during which an idea or concept is given the funding, staffing, and time to determine its general character and costs. The window closes after a while. It is closed for the near term for solar power towers and synfuels, for example. In my view the window is closed, or closing, on tokamaks. This is an engineering judgment, not a physics judgment. If it is correct, it is an example of how the motivations of physicists and engineers can conflict.

Supporting this position are years of tokamak reactor conceptualization and analysis and heroic efforts to conceive of tokamak improvements, many of which I expect will prove to be successful. However, all of these efforts have not succeeded in removing what I have long believed to be the fatal flaw of the tokamak concept: it is inherently a complex maze of rings and a toroidal chamber inside of other rings.

In my view, this complex geometry will not be acceptable to the utility world, where power plants must be maintained and serviced rapidly at low cost. In that world, simple geometries are essential, particularly in complex new technologies, which will always be difficult to introduce.

My feelings are based on years of observation of the utility industry in general and commercial nuclear power development in particular. My views then are market-driven, meaning they reflect my perception not only of the utility market as it exists today, but how it might look 40–50 years in the future, which is of course difficult to project.

It is extremely important to know your market whenever you develop a new product. For fusion, the utilities are the market, and unfortunately, most people in fusion research have not been particularly closely tied to the utilities.

Interestingly, I was in Los Angeles a week ago sitting in while the Arco Executive Committee reviewed our plans in a new, high–technology area. We have superb, exciting research in this area, and Arco has some activities in the related markets. While our technology is extremely promising, it was quite clear that our market connections were weak. The thrust of the discussion centered around the importance of being in the market to know how it operates, what will succeed, what the real issues are, etc. There was no question that we needed to be better tied to the market in question before we could seriously contemplate moving aggressively on what will be a many-years-to-commercialization project.

The same is true in fusion. Its development must be in part guided and judged based upon market considerations.

It took me years to firm up my feelings on tokamaks, for I like many others, had hopes that people would be able to invent their way out of the "rings and things" problem. But they have not been successful. I do not expect the first fusion reactor to be optimum, or the best. But the first fusion reactor must be good or no one will want it. I am well aware of the famous saying, "The best is the enemy of the good," and I do not expect the first fusion reactor to be the best.

My views on the undesirability of tokamaks are not based on economics, which are still very much in doubt. Indeed there are a number of cost-cutting simplifications under current study. In particular, higher β holds real promise to bring tokamak costs down into the range of interest.

My contention is that competitive power cost is "necessary but not sufficient" and that is why I was explicit in my second criterion for a practical fusion reactor.

If this position regarding tokamak unacceptability is correct, what are the programmatic implications?

1. Alternate concepts merit a much greater effort.

2. The program does not need four major, expensive tokamak experiments.
3. Plans to do an ignition experiment in a multi-hundred million to billion dollar tokamak are subject to question. That is a lot of money to spend on an impractical concept, even though we would like to demonstrate and study an ignited fusion plasma. If tokamak unacceptability is a tough pill to swallow now, think of how much worse it would be in the mid-1990s after spending all that time and money on a tokamak ignition device.
4. Since TFTR is such an expensive machine to operate, it should be reoriented toward demonstrating D–T break even as fast as possible and then turned off. It does not make sense dollarwise or talentwise to operate TFTR for years to get physics that we cannot clearly certify to be relevant to practical fusion power.

There is evidence that some people in the program are more and more coming to recognize these points and beginning to search for better approaches. But from what I see and hear the debate does not appear to be very direct. Change seems to be more motivated by budget pressures than pragmatic consideration of the realities. Indeed, there appears to be a malaise in the fusion community these days that appears to me to go beyond the budget problems. This malaise may be associated with the realization that the tokamak really does not look like an attractive product.

If not tokamak, then what? To this I must answer that I do not know, because there is insufficient experience with any alternate concept to say that is clearly qualifies to be number one.

Two concepts illustrate some of my own thinking on this subject. The two concepts are the reversed field pinch and the spheromak. Research on RFP has gone well in recent years. Mother nature seems to like the RFP plasma configuration and experiments have been very promising. Slow plasma buildup has replaced the old rapid pulsed methods, which would have been unacceptable for practical utility application.

Folks at Los Alamos have been doing RFP reactor studies for a number of years. From their original large-size, tokamak-like concept, they have moved toward something much smaller. Their current reactor concept is roughly TFTR in size—directionally a significant improvement.

Nevertheless, the RFP reactor concept still looks a lot like a tokamak, i.e., rings and a torus inside of rings and rings.

As much as I applaud the progress on RFP physics and reactor design, I must conclude that more is needed conceptually before an RFP could be an interesting reactor. The basic RFP geometry still is not simple and amenable to easy maintenance and repair.

Let us go on the spheromak. Recent results from experiments at Princeton and Los Alamos are very encouraging. Nevertheless, the work is limited in size and plasma parameters, and favorable scaling to larger sized plasmas is needed before the spheromak could be considered a real contender.

I have not seen any reactor designs for a spheromak power system, but I would think that they could be inherently very attractive.

One could envision a spheromak in a right circular cylinder with the primary magnetic fields provided by a pair of simple ring magnet coils with relatively little additional complex magnetic structure. That is basically the kind of simple geometry needed in a practical reactor.

The spheromak has a natural divertor out the ends of the cylinder—that is attractive. Impurities are a problem that must be dealt with and divertors are a good way to manage impurities as long as they do not involve complex structures inside of complex structures.

High β appears possible in spheromaks. And I will bet that much of the physics and technology that we have developed for tokamaks applies one way or another to spheromaks.

Another important and hopeful aspect is that spheromaks appear to come in small sizes. That means that development could move faster than for big systems because the cost and time to build larger, new experiments would be less.

Most important, small-sized commercial fusion reactors at the few hundred megawatt level would be a real plus. A demonstration reactor would thus be relatively inexpensive. Further, commercial systems could be built faster and at lower cost than required for big, 1000 MW concepts. That is desirable from the point of view of utility investment and risk. So spheromaks could prove to be very interesting.

Let me go on to comment on two other major topics that are being extensively discussed these days. I have heard that a number of people feel that the magnetic fusion program should respond to its current budgetary problems by retreating from the goal of creating a practical fusion reactor and become more basic plasma physics oriented. In my view that would be suicidal. There is no way that I believe that you can justify a $350–400 million dollar a year basic plasma physics program, particularly during a period of tight federal budgets. As a taxpayer, I would personally object to that kind of redirection. If the program were to become much more basic and to yield on its goal of practical fusion power, then it should be funded at maybe $100–150 million per year. There are too many other energy R&D needs out there to justify more than that in my view.

One of my very strong messages is that if you retreat to basic plasma physics research, I believe that you will guarantee drastic budget cuts.

Let me next turn to the issue of international collaboration. In the past, fusioneers worldwide have worked together cooperatively to the benefit of all. That is excellent and should be nurtured in the future. Going beyond that level of cooperation is asking for real trouble. First, there is no practical way to assign different magnetic fusion concepts to different countries or groups of countries. Everyone wants to and should work on the front-runners. It is impractical to expect otherwise. The idea of building an ignition device as an international machine has some appeal on the surface. However, if you buy my argument that the tokamak is not attractive from a commercial standpoint, then you do not want to spend the time and effort to build what would be a large, international ignition physics experiment that would have questionable practical application and that would divert people and funds from the real need to develop an attractive reactor concept.

How does this all add up? It says that the practical fusion reactor goal must continue to be the major thrust of the magnetic fusion program. To pursue that goal, there has been, is now, and will continue to be the need for a balanced program of theory, small- and large-scale experiments, engineering, and basic plasma physics.

If the tokamak window is closing or closed for the near-term, then the magnetic fusion program needs major alteration. That is uncomfortable at any time, but particularly so when the budget cutters are looking for any excuse to take away funding. Nevertheless, given the choice of being eroded away while you pursue a "dying horse" or taking the bull by the horns and managing your own destiny, I believe there is no choice.

What would I specifically recommend?

1. Immediately turn off two of the three large tokamaks, i.e., PBX, PLT, and/or Doublet III. Leave one to test new concepts, to study low-β toroidal physics and work the outside possibility that the tokamak could be made beautiful.
2. Take the steps necessary to achieve breakeven in DT in TFTR and plan to turn TFTR off immediately thereafter.
3. Build one or two larger spheromaks to demonstrate scaling in an energetic plasma.
4. Select one or more alternate concepts for serious study at reasonable size. The choice should be largely driven by reactor attractiveness and less by current physics data base.
5. Because of budget pressures, MFTF probably has to be put in mothballs. Further mirror research can be at smaller scale until there is clear justification to go back to MFTF. MFTF represented a heroic gamble to leap ahead in mirrors. But it turned out to be somewhat premature and very expensive.
6. Maintain a vigorous university program. That has always been a strength.
7. Last, but by no means least, fusion engineering must continue at a healthy level. Reactor studies are critical to maintain perspective and to provide guidance to the physicists regarding the most significant problems and opportunities. Industry can make major contributions in fusion engineering and must be given a partnership role to help guide the program effectively.

That is my message. It took me years to come to these conclusions, because I was both hopeful and maybe slow to read the emerging technological realities. I have great hopes for fusion, and so I thought long and hard before speaking out. A draft of this talk went to a fusion friend, who wrote me back the following:

"Your talk will to a greater or lesser degree have two effects:

1. It will make you a lot of enemies.

2. It will do a great deal of damage to the fusion program.

Those two statements are without regard as to whether you are right or wrong. I am not sure those are reasons not to give the talk. Certainly the program is not going anywhere as it is."

I do not want to make enemies of people whose friendship I have long cherished. And I do not mean to damage the fusion program, which I believe is going somewhere that is extremely important to this country and the world.

However, given the choice of being quiet and keeping friends, or speaking out and possibly causing damage, I have chosen to come forth, because to do otherwise would be to shirk a responsibility that I strongly feel.

I am no less dedicated to fusion power now than ever before. Fusion power will work; it will be economical; it can be made attractive, both mechanically and environmentally; and it can power planet earth essentially forever, if need be.

Thank you for your kind attention and good luck. I'm off to Tibet now!