

Panel Discussion

Technology Research and Development

**Gregory M. Haas,¹ Charles C. Baker,² Mohamed A. Abdou,³
and Raymond F. Beuligmann⁴**

REMARKS BY GREGORY M. HAAS

These are my personal thoughts and don't represent office policy. I want to talk about the impacts of the changing directions on setting priorities. That's an issue we're faced with right now. We're going to have to write something in the next few months that represents more detail for this new direction. I succinctly list in Table I what the old priorities were and what the new priorities are. Previously, we had specific milestones in the plan. We characterized the program as tokamak-dominated. Large central station electric was more or less the focus. The thrust was U.S. leadership of the world fusion energy program.

Based on the new program plan, the parameters are a broad scientific and technology knowledge base, an attractive plasma configuration to be determined, uncertainty as to what constitutes attractive fusion options to be determined in the future, and increased collaboration. The thing I want to focus on now is the issue of attractive fusion options.

In Table II I argue that setting long-term technology priorities—and by that I mean very long-term—requires some boundary conditions set by the DOE Office of Fusion Energy (OFE). One might take as a reasonable assumption that first generation fusion reactors will be DT devices. That's not necessarily a given. We've heard talk about the possibility of advanced fuels. First generation fusion devices will probably be modest beta devices, 10–20%. (There are

many who would argue whether 10–20% is modest.) First generation fusion reactors will require multi-megawatts of auxiliary power for heating, current drive, and stabilization. That may be the case, and it may not; but these are based on the perspectives today about what appears to be reasonable. The one point I would make, however, is, that a desirable attractive reactor option probably will be decided by people who are either very young today or who aren't even born. We can't decide what's attractive because attractive may have a different meaning in the future.

We could use this set of assumptions to determine the long-term technology priorities based upon a perceived first generation fusion reactor. I want to take two simple cases to demonstrate the impacts on the technology R&D program of recognizing that we have limited resources and that we will have to set priorities. We can't possibly do it all.

Let me suggest two examples (Table III) which we've heard about. One of them is beta. Suppose I assumed that I have a modest beta device. It's probably going to require 8 to 10 tesla superconducting magnets, and I would need an R&D program on superconducting magnets. On the other hand, I could assume that I have a high beta device, greater than 50%. It seems reasonable if you get up above that, that you could probably get by with copper magnets. If that were the goal of my program, I might not have to put resources into superconducting magnets. Another option is the fuel. Do I assume tritium or do I assume deuterium? If it's a tritium-based system, I will need a tritium breeding blanket and I will have to spend resources developing that. If it's not, then I do not have to consider the tritium breeding in the energy conversion system.

What I then tried to do in thinking about this, was to look at the long-term program. The near-term

¹U.S. Dept. of Energy, Office of Fusion Energy, Office of Energy Research, ER-533, Germantown, Washington, D.C. 20545.

²Argonne National Laboratories, 9700 S. Cass Ave., Bldg. 205, Argonne, Illinois 60439.

³UCLA, School of Engineering & Applied Sciences, 6288 Boelter Hall, 406 Hilgard Ave., Los Angeles, California 90024.

⁴General Dynamics, Convair Division, MZ 92-1070, P.O. Box 85377, San Diego, California 92138.

Table I. Changing Directions in Magnetic Fusion Energy

Previous directions	New directions
Specific fusion reactor milestones	→ Broad scientific and technology knowledge base
Tokamak dominated	→ Attractive plasma configuration to be determined
Large central electric power station	→ Uncertainty in what constitutes an attractive fusion energy option—to be determined in future
U.S. leadership of world fusion energy program	→ Increased international collaboration
Opportunities for innovation on technology priorities	

Table II. Setting Long-Term Technology Priorities Requires OFE Boundary Conditions

Reasonable assumptions appear to be
● First generation fusion reactors will be DT devices
● First generation fusion reactors will probably be modest beta devices (10–20%)
● First generation fusion reactors will require multi mws of auxiliary power (heating, current drive, stabilization, end plugging, etc.)
● Long-term technology priorities could be set based upon perceived first generation fusion reactor assumptions

is not so bad, but if you're trying to find the long-term technology program, there are a lot of issues. I tried to classify the issues into two general categories (Table IV). Those which probably are not going to be very sensitive, or almost independent, of the kind of confinement device we end up with, and those which are probably going to be device-dependent.

It seems obvious that you're going to have to deal with the plasma-wall interaction. I don't really care what the device is. It's going to have to have high heat flux, and we're going to have to deal with high heat flux components. I put auxiliary power systems in the class of something we will need, although there are those who believe you might be able to have an ohmic ignition device, so maybe even that isn't an absolute necessity.

We'll have to consider safety in order to win public acceptance no matter what the device is. We will have to convert the energy that comes out of the reactor into a different form, and we'll certainly have to fuel it in some way. So these technologies can be addressed in a generic form, almost irrespective of what the boundary condition is, whether it's a high beta or low beta device. Beta might impact on these, but they're pretty generic.

Table III. Two Examples of Boundary Condition Impacts on Long-Term Technology Development

Item	Boundary condition	Impacts
I. Beta	Modest (10–20%)	Probably require 8–10t superconducting magnets
	High (> 50%)	Probably get by with CU magnets
II. Fuel	Tritium	Require tritium breeding blanket
	Deuterium	No breeding required in energy conversion system

Table IV. Long-Term Technology Development Appears to Fall into Two Priority Classes

I. Class of fusion technologies that will have to be addressed irrespective boundary conditions
● Plasma-wall interactions
● High heat flux components
● Auxiliary power systems (10s mws)
● Safety
● Energy conversion system
● Plasma fueling
II. Class of fusion technologies that are boundary condition dependent
● High energy neutron materials damage
● Tritium breeding
● Low activation materials
● Tritium handling
● Superconducting magnets > 8t
● Negative ion beams
● RF sources > 200 kw, > 60ghz
● Pellet injectors > 2 km/sec
● Special remote maintenance equipment

On the other hand, high energy neutron material damage may be much more dependent on whether you use DT or DD, or maybe even speculate about advanced fuels. Tritium breeding is clearly dependent. Low activation materials may be more generic. If you went to sufficiently advanced fuels, it may not be as much of an issue.

Clearly the budgets for technology will never support the knowledge database in all of the areas being developed simultaneously. Somehow there has to be some prioritization on the boundary conditions to make the determination of where you put your resources. The bottom line of all this is that selection of the boundary conditions will be critical in setting the priorities in the long term. On the other hand, new boundary conditions will produce change in the technology priorities and opportunities for innovation. So, we could look at this as a period of turbulence but, also, as a period of opportunity. As an example of that, if we're not required to build a reactor device in the near term, then we don't have to

deliver on the materials in the near term. We may be able to go off and talk about inventing new materials. We may have much longer, so there'll be real opportunities for innovation there. However, we have to define at least some boundary conditions in order to decide where to put our resources in order to try to pursue that innovation.

REMARKS BY CHARLES C. BAKER

I would like to make a few observations about the relationships between science, technology, and the engineering aspects of the fusion program. More specifically, I would like to comment on the perception of science, technology, and engineering as we, in the fusion community, tend to present it to the outside world.

It seems to me that there are some notions that emerge through our literature, rhetoric, and the draft plan that we're all here to talk about. First of all, there is the notion that fusion science is identical with plasma physics. Secondly, there is a notion that technology's primary function is to support plasma physics. Third, there is the notion that fusion engineering refers to the mundane, standard things you do when you have a straightforward application of existing knowledge. I think all these notations are wrong! I think they also do us a disservice. I don't even think they help our plasma physics colleagues. And we have a habit of spreading these notions to the outside world. While there is undoubtedly a lot of relatively standard engineering that we have to do, much of what we do now and have done in the past in fusion engineering is true research by anyone's definition of "research." While much of our fusion technology requirements must be set by the needs of our near-term confinement experiments, there is an awful lot of technology research that should and can stand on its own merits. Last but not least, there is an awful lot of science in fusion besides plasma physics. I think you all probably appreciate that blanket technology involves materials science, thermodynamics, chemistry, atomic physics, nuclear physics, etc. It is science, but not plasma physics.

What does all this say? First of all, we should broaden the way we use the term "scientific feasibility" as applied to fusion's goals. It is not just the problem of confining the plasma, which we all know is very difficult. Confinement is still an issue and will be for some time, but scientific feasibility also in-

cludes the issue of how we are going to *use* fusion energy. It is a feasibility problem because we do not know how to do it today, and we cannot be assured that we can do it. It is a scientific feasibility issue because a great deal of science is involved. In fact, it is more science than engineering. I would also argue that research on fusion technology is required now because of its fundamental importance to fusion feasibility. It stands on its own merits, not just as a support for plasma confinement experiments. Fusion technology research should be carried out in concert with plasma physics research.

Others have identified some new confinement concepts, many of which would increase a fusion reactor's power density. What good does it do to develop a high power density concept, in terms of plasma power density, if we cannot find a way to use it in a practical energy system? Fusion technology requires just as much innovation as does plasma physics. However, we are at different stages of development in fusion physics and technology. Others are able to talk about a lot of these ideas for new concepts in plasma physics, because they have had two to three decades of basic plasma physics studies where they have developed the data, knowledge, and most importantly, the understanding to develop new ideas. For many of the issues we deal with in the technology area, we do not even have the basic data and understanding to develop new technology concepts.

The last point I would make is that the magnetic fusion policy plan should not attempt to draw such a sharp distinction between science and technology. The fact of the matter is that plasma physics has a lot of technology and engineering in it, and fusion technology has a lot of science in it. Those really need to be melded together in a common set of goals.

REMARKS BY MOHAMED A. ABDOU

My remarks will focus on the role of fusion technology in the fusion program plan, specifically the questions of how and what to do. My remarks are based on results from recent studies. Particularly, there is a new study called FINESSE, in which we are looking at technical and programmatic issues. I will start by relating some particular conclusions that have been important to programmatic implications.

First, the fusion environment experienced by nuclear components is unique. There are many inter-

active effects caused by the simultaneous presence of neutrons, magnetic field transients, tritium, vacuum, bulk heating and stresses, and by the multiple functions that many of these nuclear components have to put forth. These multiple interactions lead to new phenomena, either new phenomena or substantial modification in the characteristics of old phenomena. These new phenomena result in critical issues for fusion. These are both feasibility and attractiveness issues, namely, safety and economics. Resolving these issues will require new knowledge, and the new knowledge can be acquired only through new experiments, theories, and models. Therefore, we conclude that fusion technology deserves an enhanced effort on its own merit because it is critical to fusion and is important to science and technology outside fusion.

Another set of conclusions have programmatic implications. We conclude, for example, that neutrons are necessary for many experiments. There are some experiments that don't use neutrons, but there are many experiments that require neutrons. Therefore, in the 1985 to 1995 timeframe we can, and we should, use new and existing small-scale test stands, point neutron sources, and fission reactors. But in the mid-to-late 1990s there is a critical need for a Fusion Engineering Research Facility (FERF). From a strictly technology requirements' viewpoint, this Fusion Engineering Research Facility should have these characteristics: 20 megawatts of fusion DT power, quasi steady state; 10 square meters of experimental area, and a fluence in the range of 2 to 10 square meters.

It is very important to understand clearly what the requirements of the technology in this facility are. We don't need 200 megawatts. We don't need 500 megawatts. We need only 20 megawatts. Fifty megawatts would be fine, but beyond 50 is not needed. Yet this has certain important implications on the strategy of the program, and let me very briefly mention just one. We find that the strategy of combining physics and technology in a single device that has a fusion power greater than 100 megawatts leads to high risk and high cost. An example of this would be the NET European device. This device combines high risk and high cost. You go above 100 megawatts, and you need to produce your own tritium, and you need your own breeding blanket without prior fusion testing.

What does this mean in terms of what the program plan should be?

In order to answer any question about what to do in the program plan, one has to agree on an immediate goal for the program (e.g., a clearly stated goal for the year 2000).

If I understand correctly, it has been said that our nearest term goal should be to produce enough quantitative information to permit the nation to quantitatively judge the potential of fusion. Upon what will the quantitative judgment of fusion be based? On a conceptual product definition, but more importantly, on the hard database to support that conceptualization. The key is the hard database. How do we get the database? The program plan right now does address the plasma physics reasonably well (see Fig. 1). It provides us with a path to get the plasma knowledge we need for the product definition. We have TFTR. We are building MFTF-B. We have some experiments in alternate concepts, and then we are going to build an ignition, longer burn experiment. This is going to develop the plasma knowledge we need for the product definition, but it is not going to give us all the knowledge that is needed to judge fusion as an energy source. In order for the program plan to be balanced, you need the other half of the fusion program, and that is technology.

The plasma goals have been eloquently stated to be understanding the plasmas and improving reactor concepts. The technology program can have similar goals: understanding the fusion engineering sciences, learning the materials' engineering limits in the fusion environment, and also improving reactor concepts.

What do we do in this technology path? First of all, we need to start now doing small-scale experiments, point neutron sources, and fission reactors. In the very near term, over the next five-to-seven year period, we need to build a partially integrated test facility. This is a facility that would have all the features of the fusion environment except the neutrons. We will learn a lot in terms of fusion technology, and there are some similarities between our goals and the physics' goals.

After that, in the mid-to-late 1990s, we need to plan for a Fusion Engineering Research Facility (FERF). The goal of this facility will be to address the issues and conduct the engineering research experiments concerning fuel self-sufficiency and energy extraction and use. Instead of demonstrating that we can burn fuel and produce energy, we will demonstrate here that we can use this energy. There are important distinctions between the goals of FERG,

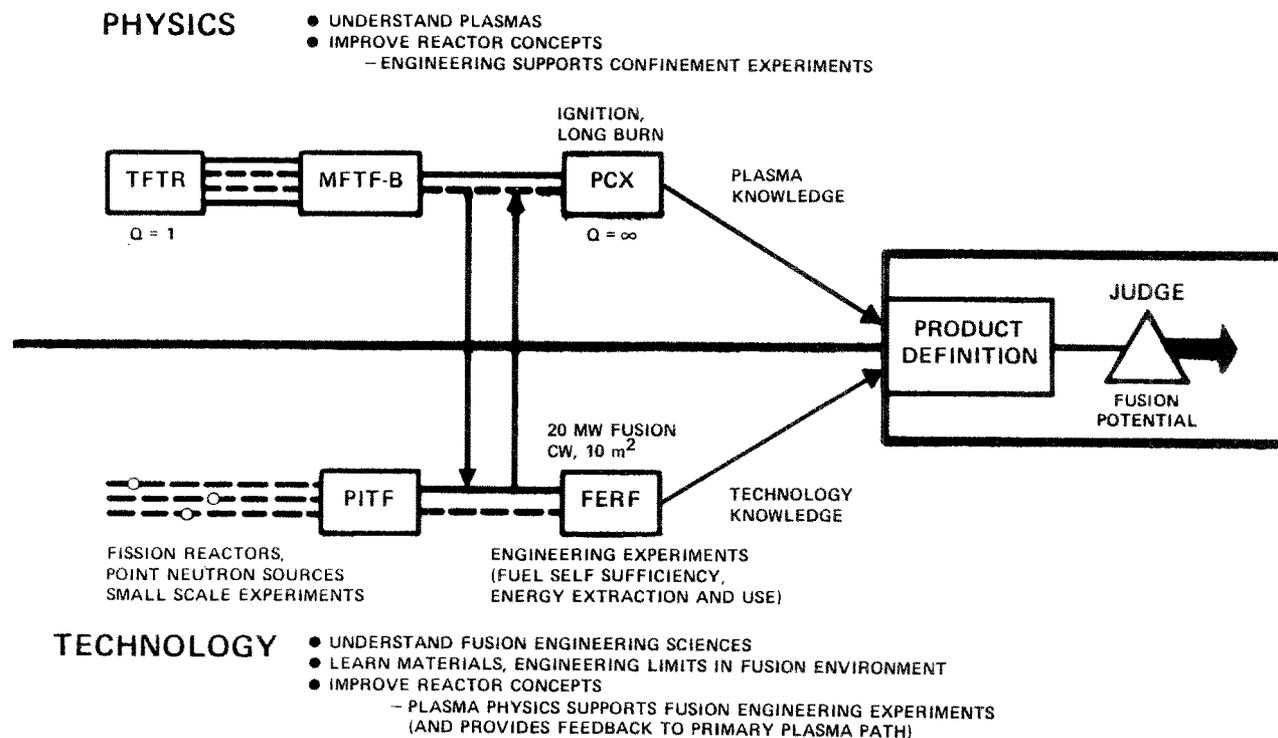


Fig. 1

the Fusion Engineering Research Facility, and an ignition tokamak. You would get, say, 300 or 400 megawatts for an ignition tokamak. For the technology facility, all that you need is 20 megawatts of fusion power over 10 square meters. This will deliver the technology knowledge that we need. Combined with the plasma knowledge, it will get us to a very good point in the year 2000 or shortly after.

There are many issues in enhancing fusion technology. I will address a couple of them. The primary programmatic issue is cost. In the U.S., clearly we would need supplemental funding. How do we get the supplemental funding? I believe that if we succeed in making the notion that fusion technology must proceed on its own merit, because of the importance of technology to fusion and science, we have a good chance.

We have succeeded in expanding the program based on the exciting things we do in physics and we should continue to do that, but we also need to capitalize on the exciting things that we can do in technology. International cooperation happens to be a particularly cost-effective and viable area for future nuclear technology, because nuclear facilities tend to be user facilities. Therefore, several countries can

share costs and benefits without necessarily agreeing on a common path. The key technical question is whether there is a credible and inexpensive option for a Fusion Energy Research Facility. The requirements from the technological standpoint are low-power and high-power density. The plasma is only a neutron producer. Therefore, the physics mode in the Fusion Energy Research Facility does not necessarily have to be on the primary path of confinement.

I believe that the goals of a Fusion Engineering Research Facility can by themselves capture the imagination of the nation. We are going to use a significant amount of fusion power with respectable power density. Their use is equally exciting. This would be the most intense neutron source ever made by man, with a unique fusion environment that provides a unique opportunity to bring knowledge from important fusion engineering research experiments.

REMARKS BY RAYMOND F. BEULIGMANN

Most of you are aware of the concerns that the last three or four months have brought on the in-

Table V. Technology Research and Development in the Program Plan (an Industrial Perspective)

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1. We develop technology to achieve a capability on a product that can earn a profitable share in a future market.
 2. The emphasis on science in this program plan with vague goals, no milestones, and international collaboration will not attract industrial R&D dollars to fusion technology.
 3. Multiple applications will be needed to justify industrial investment in fusion related technology.
 4. Will DOE maintain key U.S. industrial capability as an element of international collaboration programs.
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dustry. We are faced with two issues: one, a reduced budget—we have seen some of the near-term programs disappear; and two, a new emphasis on science which creates an image of the program that has certainly gotten our attention.

The goals of industry are shown in Table V. We highlight the point called “Future.” We in industry are quite aware of technology and we develop technology to achieve a capability on a product or products that can earn a profitable share in a future market. The issue now with fusion is how far out is that future market, and is it viable at this point for us to participate while in it is ongoing, while it is growing. In the past it appeared to be viable, and we could sell it. It is more difficult now to sell it to management.

The emphasis on science has vague goals. When is it and what is it that we are supposed to do in industry, as we try to support the laboratories and universities and attempt to be ready with a final product? What about this idea of international collaboration? What does that mean to us, particularly if we are going to put R&D dollars in fusion technology?

It helps to have multiple applications. I think the deep involvement that TRW has had is one example that multiple applications can make investment dollars in fusion affordable and viable, because there is more than one application for the technology.

We ourselves at General Dynamics, being in the superconducting magnet business, have seen the ben-

efits not just of magnets for fusion, but also for isotope separation, possibly for MHD if it ever comes back, and these days maybe even in the accelerator business. Without that, the application for fusion alone would be quite bleak.

Finally, what will be the U.S. DOE plan in maintaining this key capability? We think we represent a certain amount of corporate skill and knowledge that DOE has invested in us. We recognize that as a responsibility. We are trying to figure out how we get to the future timeframe when fusion does come back and is a more viable business venture.

We have the problem now of balancing this potential, this prospect as outlined here, maintaining this corporate knowledge and being a viable industry ready to compete in the future. For example: personal objectives. The younger people like to be associated—and I am sure the national labs do too—with an ongoing, very dynamic program. That feeling is not any different in industry. I am not going to be there many years from now. I am at the age that I think I will see fusion machines from a rocking chair. We are trying to scale our planning so that our younger chief engineers will be the leaders at that time, but trying to keep their imagination tied to a program like this at a time when it is going toward science is a difficult job. We have to keep working on that. That is an industry consideration.

Another concern is that of investment viability. This means personnel, as well as dollars. The first law of economics is not what it costs you, it is what you *give up* when you spend it. Right now, the aerospace industry is booming, so there are great demands not only on dollars but for personnel to work on other programs that are there and worth millions and billions of dollars. We may disagree that they are viable and may think they are going to go away and that they are pies in the sky as well—but those are the other opportunities that we must also consider.

I don't think I offer any great solutions for the role of technology in the R&D plan, but I wanted to give you our point of view from industry.