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Critical Issues and Required Facilities

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I will discuss fusion nuclear technology issues and development needs, including approaches to solving these problems.

The major elements of fusion reactors are engineering components (such as magnets and plasma heating components) and the nuclear components. Anything that is not a magnet or a plasma heating component is a nuclear component. A more useful appreciation of the importance of nuclear components, however, comes from looking at the components that are affected by the nuclear environment. These are the blanket and shield, which are strictly nuclear components, but also things like plasma-interactive and high heat flux components, which are plasma support systems. These include the first wall, impurity control, rf antenna and so forth. These are affected critically by the nuclear environment. Tritium and vacuum systems, instrumentation and control, magnets, remote maintenance, even the heat transport and the power conversion are also affected by the nuclear environment.

Many unresolved fusion issues are in the nuclear technology. These unresolved fusion nuclear technology issues relate to feasibility which is, of course, the ultimate acceptance test for the technology community; the economic potential, which is the ultimate acceptance test for the utility industry; and safety and environment, which are the ultimate tests for public acceptance. Resolving these issues appears to be relatively costly, primarily because we require neutrons in the test environment, and requires a long lead time. I would like to make a distinction between long lead time and long-term development. Quite often you hear about nuclear technology being a long-term development and therefore it should be put off. I would argue that it is long lead time, which means you start now; you don't put it off. Therefore, I believe that the U.S. and other international fusion programs must seek successful and timely resolution of the fusion nuclear issues. We can talk about what we mean by successful and what we mean by timely.

Before I do this, let me tell you a little bit about a new study that is called FINESSE. The objective of this study is to investigate the technical and the programmatic issues in the development of fusion nuclear components. It is a study that started in November 1983 and is being carried out by a number of U.S. organizations: UCLA, Argonne, EG&G, HEDL, McDonnell-Douglas, and TRW. The study receives important support from Livermore and Princeton and is coordinated with other DOE and EPRI programs in that area. It has participation by the fusion community through an advisory committee (15 senior members of the community) and a number of workshops. An important characteristic of FINESSE is international participation. We have a number of key experts residing at UCLA, participating in the study, that come from Germany, Japan, and Canada. It is indeed reassuring that these international organizations find fusion nuclear technology issues to be important enough that they sent some of their key experts at their own expense to participate in the study. I believe that it is really important because all the world's programs face the same issues of fusion nuclear technology development. Furthermore, nuclear technology development for fusion appears to be a very promising area for international cooperation.

In FINESSE, there are several major tasks. The first is identification of issues and the required nuclear tests. The second is quantifying the test requirements. This is one of the most important tasks in FINESSE. What we are trying to do is quantify the requirements of the test conditions. Why do we need 2 or 3 megawatts per square meter of wall load instead of one? How better is the information that you get from the testing as a function of the wall loading? How high a fluence do we need? What is the minimum size test

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module? There are a number of key issues in engineering scaling that we won't have time to explain here, but they are very difficult.

We are also examining the need for neutrons and integrated testing. The outcome of this task is to develop figures of merit with which to judge the usefulness of information obtained from testing facilities as a function of their testing capabilities. A third task is evaluation of experience from other technologies, particularly fission and aerospace. Task four is surveying and evaluation of neutron-producing test facilities. Here we are concerned with both nonfusion devices and fusion devices. What we hope to do here is to get the cost as a function of the test capabilities.

When we finish all of this work, then we'll have sufficient information to do a comparative evaluation of test facilities and the various scenarios for fusion nuclear technology development. One of these scenarios can go like this. You have TFCX that will resolve most or all of the physics issues and can do something in the engineering. You have complementary facilities such as FMIT and fission reactors. The question is: What is needed to solve the major feasibility issues in fusion nuclear technology. It can range from doing nothing, "these are sufficient," to "we need a new fusion device dedicated to fusion nuclear testing." At the end we hope to develop recommendations on fusion nuclear technology strategy.

As I mentioned, one of the most important things we are doing in FINESSE is the characterization of the fusion nuclear issues and the testing needs. I cannot go through the details here. I'll only give you a few examples.

A good example is the blanket, starting with solid breeder blankets. It is now clear that there is a tritium breeding issue there. We cannot obtain sufficient tritium breeding without using beryllium, which is resource limited. The tritium inventory in solid breeders might prove to be unacceptably high. There is a collection of issues that collectively, or partially, might render solid breeders infeasible. Another issue is the form of the tritium in the purge stream which might lead to an unacceptably high tritium permeation rate. The conclusion was that it is difficult today to predict that any solid breeder blanket concept would ultimately prove feasible.

Now, if you don't like that, you go to liquid metals. They have their own set of critical feasibility issues related to corrosion, MHD, safety, hydraulic, and tritium breeding. Again, the conclusion is that it is very difficult to predict, without doing a lot more work and testing, that any of the liquid metal blankets would prove ultimately feasible. Now, the problem is that we underestimate what we need to do to solve the fusion nuclear issues. We have examined these problems in considerable technical detail in order to find possible solutions. Take tritium recovery in solid breeders as an example. What are the requirements in terms of the need for neutrons and the test parameters? We'll use an example here: radiation effects on solid breeder tritium diffusivity and solubility might lead to unacceptably high tritium inventory. In order to do a meaningful test to resolve this problem, you need the neutrons because they produce a specific action and they also produce the damage in the material that leads to high tritium inventory. These are the key test parameters that have to be looked at in terms of quantifying the test requirements: the fluence, the temperature, the impurity, neutron spectrum and flux. Take another example, the solid breeder/structure interface; the thermal conductance. We need a solution so we can predict the thermal conductance and also control it. If we don't develop a solution in combination with other things, it might render solid breeders infeasible.

In order to do testing to resolve an issue like this, you need the neutrons to produce the damage. If you have dimension changes in the solid breeder and the structure, both would affect that interface and the thermal conductance at that interface, so you can't do one without the other. The parameters that are important for the simulation are temperature, stress, fluence, geometry, and impurities.

Let me talk about what types of tests one would need to do in order to address these issues. A normal classification is to talk about basic tests, specimen tests, separate effects tests (in which you simulate only one element of the environment), multiple effects tests (in which you try to simulate two or more environmental elements), integrated tests (in which you try to simulate all or most of the environmental elements), and finally the component tests (in which you test the component as part of an integrated system).

The most critical issue in doing these tests is the need for neutrons because, if you don't want neutrons, you can think of a modest cost facility that costs something no more than 30 or 40 (perhaps 50) million dollars to do all the testing we want. It's really when you talk about the neutrons that the cost increases rapidly. You start to think about hundreds of millions to maybe a billion dollars plus. So it's important to address the need for neutrons; however, first one has to understand what neutrons do. They are a source of heat and they also induce changes in material properties commonly referred to as radiation effects and material damage. They also produce a specific reaction to serve a particular function, (e.g., tritium breeding). Now we find that in many of these tests, neutrons are necessary and you can talk about that in terms of two broad reasons. First, the neutrons represent an ingredient in the fusion nuclear environment that is the most harsh, and produces the largest effects and changes in the component, leading to numerous critical feasibility issues. Despite all we know about neutrons, neutron effects remain poorly understood today. There are no substitutes for neutrons. You worry about the correctness of simulation. Even if you are willing to give up the correctness of the simulation, there is the question of cost in order to produce bulk heating by resistive heaters and so forth. These are very expensive. For radiation effects, there is no substitute for the neutrons. Neutrons are important not only for blanket testing (I don't want you to get that idea since I talked only about the blanket). They are also important for testing other components. For shielding, of course, the neutrons are mandatory for any useful test about radiation transport and streaming tests. For impurity control and exhaust, the neutron environment at these limiter/divertor plates is as harsh as that of the first wall. That's not the number-one issue for these components; now, erosion is, but we hope to solve the erosion problem so that we can extrapolate to a reactor. Once we are able to develop a solution like this, the nuclear issues will be the most important issues. Even if we don't solve the erosion problem, and find another way, like rapid replacement, then the radioactive erosion product transport becomes a key issue. Similarly, in auxiliary heating, you look at antennas, waveguides, and so forth, and find the neutron environment is just as harsh as that of the first wall. There are many issues there that we have not resolved yet. For superconducting magnets, the nuclear effects are a key driver on any design and also on instrumentation and control.

Now that we talked about the needs for testing and the need for neutrons, where do we get these neutrons? That is a key issue in the program now. There are only three types of neutron producing facilities that we know about. These are accelerator-based point sources, fission reactors, and fusion reactors. Let's look at what each one offers.

Point neutron sources are necessary and useful for a specific purpose such as radiation effects on capsules, particularly high fluence tests. They are important for neutron tests on tritium breeding and shielding. However, they are not suitable for multiple effects integrated tests because of limitations on volume. Fission reactors offer larger volumes than point neutron sources, yet still the volume is limited. They are suitable for doing capsule tests, of course, as well as subelement tests, such as tritium recovery. We are getting very useful information out of fission reactors today. They are being used and we need to continue to use them. There's really no alternative. The important thing is that fission reactors don't appear to be a substitute for fusion testing because of limitations in volume, some limitations in simulating the environmental elements and even some limitations on simulating the environmental parameters, such as power density, space and time dependence, and so forth. Of course, there are also the well-known spectral differences between fusion and fission neutrons.

This leaves us with fusion facilities. The first question is: Are they needed? I don't want to answer that yet. The only thing I can say is we haven't yet found an alternative to them. So until we find an alternative, I will assume they are needed. The reason they are needed as it appears today is, firstly, the volume and surface area of the test element. Some of these key tests require a large volume, say a cubic meter or something. It's only in a fusion test device that you can obtain that kind of test under the right environmental conditions. Then there is the question of the total volume and surface area of the test matrix. If you look at how many test elements you want to do, what area, what kind of flux, and so forth, you conclude that we need a uniform volumetric steady neutron source with a neutron yield, a minimum, of 2×10^{18} neutrons per second and perhaps as high as a few times 10^{19}. It's only in a fusion reactor that you can get that kind of neutron yield. Of course, in a fusion device you can simulate all the environmental elements, neutrons, electromagnetics, plasma, particles, tritium, and vacuum—and you can get the correct neutron spectrum.

However, there are limitations and problems with using fusion devices as test facilities related to cost, risks, and limitations of the simulation. The most important problem here is that the cost is relatively high on a single capital investment basis. However, if you look at what it is that you want to do, all the tests you want to do for the key issues, you'll find that really on a per-neutron basis these fusion test facilities are more economical than any other option. There is also a question of risk. These test devices must deliver the performance they are designed for. There is the plasma performance. Even if we say the plasma in a device dedicated to nuclear testing does not have to extrapolate to a reactor, we want to drive it and so forth, it is still not clear whether the database is sufficient to convince us that we can get the plasma performance we need. Of course there are risks associated with the engineering components —that comes primarily in areas of reliability and availability. For a test device such as this to be useful, you would need an availability that might be as high as 20 or 25%. We haven't arrived at an exact number yet; this is what we are doing in FINESSE. Even if you do that, it requires a lot of development; however, this development is not wasted because it would be a forcing function for component development, something that we need to do in the program. Finally, there are potentially serious limitations on simulating the environmental parameters in a fusion testing device. The problem comes from the following: in order to keep the cost of a fusion testing device down, we are forced to use a scaled-down condition like one megawatt per square meter wall load instead of five megawatts per square meter. If you do this, if you take a module from a commercial reactor or a demo that "looks like" that and you put it in this testing device you get practically zero information out of it. So what's the alternative? You have to redesign it. We are working on designing a testing module that would "act like" these demo or commercial reactor modules. But we are encountering many difficulties, and there are also complex issues with engineering scaling that I won't have time to talk about here that are being addressed.

As I mentioned before, one of the things that is being done in FINESSE is to evaluate the experience from the fission and aerospace industries. The first draft of the fission experience report was just completed. It was done by two groups at Argonne and UCLA. It was done by a group of people who are experienced in fission, but they know very little about fusion. The thing that is very striking about this report is that almost every paragraph or every point has a parallel in fusion. It's not an exact parallel, but I think there's a lot to learn from their experience. I will mention a few examples of the points made in that report. The first point is that major technological developments require extremely long lead times. If you look at the fast breeder, it's taken about 40 years after the proof of principle. You look back at the Clementine and the EBR-1 and note that these were constructed in the 1940s and, after the proof of principle, there have been 40 years of mostly engineering development and they are not quite there yet.

As you know, fission is experiencing difficulties and these difficulties are attributed, first, to economics, which is the ultimate industry acceptance test. Part of the problem in the breeder program now, is that the economic potential is being questioned. There are questions about public acceptance; it doesn't matter what the technical community thinks is safe and sound. It is really what the public perception is that counts. Regulations have also proved to be a lot more difficult and cumbersome than initially anticipated. They also point out in this report that confirmation of a new sophisticated technology requires one of two options. Option A is a series of demonstration prototype plants of progressively larger size or, alternatively, option B is a vigorous component development and testing program in conjunction with selectively fewer demonstration plants. Indeed, in the course of the fast breeder program, they had to move into fewer demonstration plants and do a lot more extensive component testing.

Another point made is that test facilities have played an indispensable role in the development of the breeder program. They built a lot of new test facilities, a lot of new special purpose facilities in addition to the use of existing facilities. Fission always has had the advantage that in-pile testing facilities were always available. Another point mentioned is that premature expansion of some of the early technology beyond its limits resulted in costly failures and delays. These delays were caused by using components that had not been thoroughly tested. When a single important component failed, an entire facility was shut down. Such a delay resulted in further movement in the breeder program toward more extensive testing of components prior to system integration. In the handout, there are more examples like this.

What I would like to do now is share with you some thoughts that I have on overall fusion development and, in particular, on how engineering fits into that overall program, To start with, I believe that clearly defined intermediate and ultimate goals for fusion are fundamental to defining a program direction and development of strategy. There is no single unique best strategy. If the strategy is to develop the physics or develop engineering only, or whatever the goal is, then you can think of a strategy that fits in there. So it's important to define the goal. The best strategy is the one that achieves the goal with the minimum time and cost. So what are the goals for the fusion program now? The ultimate goal has always been the commercial power reactor, and we all agree on that. For the intermediate goal, a goal for something like the year 2000, you can think of many goals. I just offer a suggestion here that a goal that sounds, to me at least, to be both attractive and realistic is to develop a sufficiently credible database to permit the nation to quantitatively judge the potential of fusion. So by the year 2000, we want a database to permit the nation to quantitatively judge the potential of fusion. Well, if you don't agree with this goal, then the following is not going to make sense. If you do, then you might find some of these things to be worthwhile to think about.

How is the nation going to judge the potential of fusion? First, what is it that we judge? What does the program have to deliver to the nation in order for the nation to make a judgment? You can think that what we will deliver is something like a conceptual definition of what the end product might look like; a conceptual definition for a commercial reactor, for example, a STARFIRE or MARS type of reactor. In the year 2000, this is going to be after many decades of development in fusion, so that the database that we deliver to go with this is going to be very important. So what are the judgment criteria? If you have a panel of people who are trying to judge the potential of fusion, what are the criteria they are going to use? They might use the following: feasibility and potential attractiveness of the end product. Feasibility in the year 2000 is going to be judged by how hard and convincing the data are that we have from realistic testing and experience to show that the major feasibility issues are resolved. We don't have to build a power reactor by the year 2000, but we have to show that at least the major feasibility issues are resolved. Next, the potential attractiveness of the end product. How is that going to be judged? First, they would look at how credible this end product is, as extrapolated from the database. In other words, in the year 2000, if we have a MARS type or STARFIRE type of end product, the extrapolation from the database is going to be judged from that. Then, if that's acceptable or credible, the potential economic attractiveness and potential safety and the environmental attractiveness would be the judgment criteria. You don't have to prove that you are economical, but you have to prove that at least you have a good chance of being a reasonably economic competitor.

If we do that, then we can think of a strategy. What I would like to point out is what I think we should focus on in order to achieve these goals. We would need to focus on both the major feasibility issues and the attractiveness issue; not just one area of these. The major feasibility issues in fusion now are in both physics and technology; they are not one without the other, they are in both. Therefore, we would need to solve the major feasibility issues by doing realistic tests. This includes single, multiple effects tests and integrated tests. On the attractiveness issues, we need to learn the physics and the engineering limits from realistic tests. If, somehow, between now and year 2000 we select a few areas in which there are really key drivers and learn the physics and the engineering limit, then we would know how to optimize a reactor and how to find a better end product. You can go through examples of these and define them. So these would be important areas for focus. We need to encourage innovative ideas. We need to focus some of the studies on R&D issues related to the cost of energy. We have always done that, but I think some more work in the area of a smaller size unit and lower capital cost, would be very useful here to address one of the issues that's becoming very important in the program. Also, some ideas that result in improved safety would be very useful.

Another important requirement in a program like this is to strive to maintain a balance on R&D among the major feasibility issues in physics and technology. The program will not be better off in the year 2000 if we solve to perfection only two or three major feasibility issues. We have a number of major feasibility issues in physics and engineering, and I think we would be in a very good position if we try to solve as many of them as we can, so the balance would be very important.

How do we get the data base in physics and engineering? We would get the plasma physics information from small devices, plus a major device such as TFCX. One needs to think very carefully what the mission of TFCX is and how you do it with a minimum cost. For plasma interactive components, many of the problems can be solved within physics devices; however, many of the feasibility, and the important issues for these components, will have to be solved also in engineering devices. In engineering, we have superconducting magnets, auxiliary heating, and the nuclear components. The superconducting magnets can be in either a major physics device or in a major engineering testing device. The auxiliary heating, again, requires both the physics and the engineering devices to be solved. Finally, the nuclear components can be solved only with engineering devices. Here we would need separate and multiple effects tests.

The most critical time is the next decade—now until the year 1995. In order to even develop a test module that you can test in a fusion testing device, you need to do a lot of separate and multiple effects tests prior to getting it to that point. We have some in progress but we need additional important smaller-scale facilities and tests in a neutron producing, nonfusion facility.

Some of the key feasibility issues in nuclear technology cannot be solved without doing the integral tests. Therefore, some significant tests and integrated test for nuclear components have to start sometime in the mid 90s in order to be in reasonable shape by the year 2000 or the year 2005. The question of building a fusion testing facility that would be dedicated to nuclear testing is a very important question, and it would need to be addressed more thoroughly than in this context.

Since the nuclear technology is part of fusion engineering development and, therefore, fits to that overall strategy I talked about, let me repeat again that fusion nuclear technology involves many unresolved issues as they relate to feasibility, economics, and safety. Therefore, a stronger program on fusion nuclear technology is a necessary element. It's not sufficient, but it is a necessary element of the national–international fusion program. There are three important characteristics of fusion nuclear technology development that we have to be aware of. First, it requires a long lead time. This means starting now. We should also have some realism in that time schedule. Nuclear technology development appears to be relatively expensive, which means we have to do a lot of careful planning and we should also explore the international cooperation option in this area. Finally, it appears to be complex. There are many complex issues, which means we must understand the issues and testing needs, quantify the test requirements, and work more in developing the engineering scaling relationships.