Two Comments on the NAS Interim Report on a Strategy Plan for U.S. Burning Plasma Research

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From 1975 until my retirement in 1999, I was the head of the Naval Research Laboratory's laser fusion program. Although I am an expert in laser fusion, I have no equivalent expertise in magnetic fusion. My comments here should be viewed in that context.

1. Your committee was asked to assess the importance of burning plasma research. I urge you to analyze the relevance of ITER burning plasma science to the other attractive toroidal approaches in magnetic confinement. Its applicability appears to me to be minimal.

2. Your committee was asked to assess how "unanticipated events or innovations may necessitate mid-course re-directions," I urge you to consider how the fusion energy community should deal with the disruptive political, administrative, and budgetary problems that would be associated with any redirection of the current fusion energy program. It seems unlikely that the current fusion energy program could accept innovative approaches if it required budgetary changes.

The Limited Usefulness of ITER. I agree of course that an experimental study of a burning plasma is critical to any evaluation of magnetic fusion. The behavior of plasmas in a magnetic field is sensitive to the ratio of electron temperature to ion temperature, the spatial distribution of the temperatures, the velocity spectra, and probably also to the way that energy is transferred between species. These physics parameters will all change with burning plasma. Perhaps the ITER scientists will be lucky, and the burning plasma will not be as sensitive to disruptions or edge modes, and perhaps it will have much lower transport across magnetic field lines. Or it could go the other way. The community does indeed need to experimentally study burning plasmas.

However I am concerned about the limited usefulness and flexibility of ITER. The physics of a burning plasma should also be sensitive to the details of the magnetic field geometry and any current flows. As an example, the fusion community is exploring stellarators that are not subject to the same MHD instabilities or the same density limits, and do not have the disruptions that occur in tokamaks. Let us assume that the stellarator research in Germany finds a clear overall superiority of non-burning stellarators to non-burning tokamaks. The Germans would then have to build a burning-plasma stellarator to study the unique physics. The engineering learned in building ITER would be helpful, but not the science from the plasma.

The fusion community also has an interest in tokamaks with lower aspect-ratio than ITER. If it turns out that lower aspect-ratio tokamaks are inherently superior to the design used on ITER, then is there any way of knowing whether the science results from ITER will be relevant? Wouldn't they have to build another burning plasma device? A similar question can be raised for the higher-density Alcator concept.

ITER will be fully relevant only if it is on the direct path to a DEMO and a commercial power plant. Because of the very large physical size of ITER, its very high cost and long construction time, its inflexibility, and need for iterations (*that they do not admit to*), it seems doubtful that it is the proper direction for the magnetic fusion program. If I am wrong and ITER science results are generally applicable to all toroidal designs, or if ITER is on the optimum path to fusion energy, then the U.S. program should clearly keep its support for ITER as its highest priority. However if ITER burning plasma results are not generally applicable, then perhaps the U.S. program should withdraw and develop its own smaller and cheaper burning plasma approach. I recommend that these options be reviewed and evaluated.

Innovation in the Fusion Energy Program. My second basic concern is whether the fusion program is currently capable of taking advantage of "unanticipated events or innovations" if they involve significant shifts in budgets from one laboratory to another, without any total increase in funding. There are instances in the past when the magnetic fusion energy program has responded to changing knowledge with modifications to existing facilities, and even with cancellations of some fusion approaches. However there are other instances where the community has been unwilling to accept new ideas. It is easier to innovate or change direction when there is enough funding for those changes. For one example of failure to accept change, where I was directly involved, see the ***footnote*** on the next page.

Summary. Why should the U.S. support ITER if it is inflexible and costly, and some scientists do not think it is necessarily the best path to fusion energy? Should the U.S. seek instead its own burning plasma approach that has better long-term prospects for commercial power? Smaller in size, with lower capital costs, a shorter construction time, and with the potential for necessary iterations.? And how should the U.S. fusion program be restructured to make it more receptive to radical innovation (maybe even including laser fusion) when total funding is limited?

Footnote:

At NRL we were pursuing an alternate laser fusion concept. While Livermore Lab was studying asymmetric laser illumination of complex fusion targets, at NRL we were studying symmetric direct-illumination of purely-spherical fusion targets. While Rochester Lab was using a Nd:glass laser, we showed that there were several inherent physics advantages to a KrF gas laser. By the mid 1990s we had extensive experimental and computational studies suggesting that direct illumination with a KrF laser could have high enough energy gains for a commercially attractive fusion reactor. At the same time, ***all*** the rest of the laser fusion community was supporting the construction of the NIF laser. I tried, and failed, to convince everyone that NIF would be a disastrous failure. I then received guidance from DOE to expect a cancellation of the NRL program in a few years, because the NRL program did not sufficiently support the NIF.

I then visited the DOE magnetic fusion energy program office, and asked them to formally evaluate the NRL program for possible transfer from the nuclear weapons program to the fusion energy program. The DOE office refused to even consider my request. Their written response was that the NRL program was not sufficiently advanced to justify a scientific review. However PPPL then organized a two-day conference to let us make our case. We were successful, and the various laboratory leaders agreed that we had a good story. However they said that I should stay with weapons program funding because there were no extra funds available in fusion energy to support the NRL laser fusion program. If the worst of cases occurred, and NRL program funding was indeed cancelled, then they promised that they would ask DOE magnetic fusion office to scrape up minimal funding to keep the NRL program alive. Note that this happened in the mid 1990s, long before the current terrible squeeze in fusion energy funding.

I asked the fusion energy leadership if they would accept the NRL program if Congress provided extra funding. They explained that they were seeking their own increase in funding, which they needed, and would not give this extra funding to NRL. They were also adamant that they did not want Congress determining how the DOE fusion energy program divided its money. So then I asked if NRL could have 50% of any increase in fusion energy funding in the next fiscal year. I would then do my best to assist them in their request for increased funding. They said yes! I was then invited to join a small group of scientific leaders in magnetic fusion who met frequently and informally to frankly discuss fusion energy problems. Eventually they set up a series of informational meetings with OSTP and various congressional staffers. The evening before these scheduled meetings I was informed that the lab leaders had changed their minds. Perhaps I would not get the full 50% of any increase; perhaps it would be 20%. It would depend on another scientific review. I responded that did not see how a science review could determine budget levels. I then told them that since they had reneged on our deal, I would not oppose them, but I would remain silent and provide no help. We all went to the meetings the next day, but there was no increase in fusion energy funding for the next fiscal year.