Some Suggested Actions to Enable U.S. Magnetic Fusion Program Leadership - Comments to the FESAC Subcommittee on MFE Priorities

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Preamble: Below are some suggested actions not only to DOE, but to the U.S. Magnetic Fusion Science community in general, to enable U.S. leadership in our hard-fought endeavors to make magnetic fusion a practical reality given the difficult fiscal constraints of the present time.

My thoughts are shaped by the following experience:
- U.S. DOE Research Needs Workshop activity (ReNeW) – Thrust 16 (ST) Panel Leader (2009)
- U.S. DOE International Collaborations Coordinating Committee Member (2008-11)
- International Tokamak Physics Activity (ITPA) MHD Stability Group U.S. Representative; leader of an ITPA MHD Stability joint experiment (2008-)
- 5+ years experience on a modern international superconducting tokamak (KSTAR)
- 20+ years experience as an on-site collaborator at major US fusion facilities (primarily PPPL), serving numerous leaderships roles throughout the years.

Suggested Actions: (in summary)

1) Focus discussions of sustaining a vital U.S. program to meet ReNeW future device goals
   (i) Appreciate that the U.S. Magnetic Fusion Program is critically lean and transition planning is needed
   (ii) Consider the impact of U.S. support of ITER
   (iii) Beware of radical changes and improve planning efficiency

2) Maintain, and where possible expand our areas of leadership using present facilities
   (i) Stay focused on community-stated research gaps near-term, and plan for longer-term
   (ii) Promote existing major U.S. experiments as the key practical way to remain vital
      - (NSTX-U is a logical top choice)
   (iii) Embrace international collaboration
   (iv) Justify the need for a new generation of U.S. fusion scientists

3) Define a practical plan that advances magnetic fusion and execute when opportunity arises
   (i) Prepare as a community now for a future short-term opportunity
   (ii) Approach changes to the program carefully to gain consensus
   (iii) Plan for manpower continuity during significant changes
   (iv) Encourage actions that efficiently address the defined research gaps

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Below please find a longer discussion of these points.

1) Focus discussions of sustaining a vital U.S. program to meet ReNeW future device goals

(i) Appreciate that the U.S. Magnetic Fusion Program is critically lean and transition planning is needed

Goals have been defined in the ReNeW (2009) document that show many ways for the U.S to close the research gaps needed for magnetic fusion. U.S. leadership in the world community will greatly improve if our program can produce a world-leading facility (e.g. a fusion nuclear science facility (FNSF)) as part of meeting these goals. Achieving this will most likely include a natural transition over many years from our present major devices to this new device. Succeeding in this path requires careful planning by DOE and the fusion community to create this new research paradigm.

This planning is especially important at present, since the U.S. Magnetic Fusion Program is critically lean. Our three major experimental facilities are each at least 3 times oversubscribed each year with proposed experiments, and one is planned to be terminated next year. We must make changes carefully that preserve people, and our productive major devices,

This does *not* mean to preserve the status quo. In a highly constraint budget, it means that careful transition planning is needed when making changes to keep the program vital. It is counter-productive to everyone to point fingers at other devices/institutions and call for device "shootouts". Instead, we need to be careful on how to best preserve manpower while we make continued changes to advance our program toward ReNeW goals.

(ii) Consider the impact of U.S. support of ITER

Dr. Brinkman's charge letter makes a statement that I expect is somewhat confusing to the committee, and the community in general. Specifically, consider the statement:

"Please assume that the ITER project is ongoing, will be until the end of this decade, and is supported separately from the rest of the program:"

If ITER were indeed supported separately from the rest of the program, there probably would have been no discussion of the termination of C-Mod, and no major cause of concern by FESAC in early 2012. The present charge to FESAC may not have been made. Extrapolating a re-routing of U.S. base program funds to ITER at the rate needed to support U.S. commitments to ITER in the next several years quickly consumes the entire U.S. domestic base program in just a few years. This point has been made publically by many individuals, and drives present concerns and discussions.

As recent actions indicate that U.S. support of ITER will indeed have significant effect on the domestic fusion base program, it's logical that the committee and the community consider the impact that U.S. support of ITER will have on the U.S. domestic base program.

(iii) Beware of radical changes in lean times, and improve planning efficiency

It's interesting to discuss new, and sometimes radical thinking regarding directions for the U.S. fusion program. But with our program in a "critically lean" state, radical suggestions that fit within budget constraints typically undermine U.S. leadership. Suggestions that promote U.S. leadership based on new device construction are typically beyond budget constraints. Therefore, a prudent approach is to sustain our successful U.S. programs, trusting our extensive review processes that have helped define and enable our present research successes.

On a related note, our program spends a significant amount of effort on reviews (time that takes away from research), and we should respect the cost of those efforts by avoiding near-duplications of reviews. Recent examples come to mind – the ReNeW process (2009) followed the FESAC TAP in close order – close enough that many still refer to the FESAC TAP rather than the ReNeW report. The International Research Priorities panel (chaired by Dale Meade) was charged to write a report a few weeks after a significant report was written by an international research committee (chaired by Mike Zarnstorff) after many months of effort.

2) <u>Maintain, and where possible expand our areas of leadership under significant constraints</u>

(i) Stay focused on community-stated research gaps near-term, and plan for longer term

Our present major devices are productive, have programs that are highly influential in the world program, and can continue to be influential for many years with upgrades. During this time, we need to plan as a community a significant next-step device in the U.S. This planning, including community consensus and practical issues related to maintaining manpower resources will take several years. This should take us into the ITER construction roll-off period, when an opportunity for constructing a next-step U.S. device can arise.

(ii) Promote existing major U.S. experiments as the key practical way to remain vital
- (NSTX-U is a logical top choice)

How do we then maintain, and even expand leadership in certain areas of magnetic fusion given that our near-term budget is significantly constrained? First, consider that both time and budget requirements work strongly against us. Dropping existing major devices to pursue smaller, and/or high-risk devices undermines U.S. leadership, as any device of sufficient size to create a collisionless plasma will take years to decide upon, and perhaps just as many years to build. This route eliminates U.S. leadership for many years and can put program on jeopardy. This statement above applies to stellarators as well as tokamaks.

This argues:

A) That we need to start from existing devices, most favorably with a significant upgrade, to maintain U.S. leadership over the next ~10 years

B) We need to define, as a community, a new, next-step fusion device (best to be consistent with ReNeW physics and technology goals). Superior choices would include devices that serve both physics and technology roles, that had at least one milestone that would positively capture wide attention (e.g. $Q_{eng} > 1$), and that would serve as a step toward DEMO.

Each of the three major facilities could argue for paths forward along these lines. While not downplaying the importance of DIII-D and C-Mod, I suggest that the leader of the three based on the statements above is NSTX-U:

- NSTX is the world-leading ST. It is one instance that the U.S. can clearly state world-leadership
- The NSTX device is already in a major upgrade (NSTX-U). This is a huge credit in time – years of reviews are successfully passed, and the device is almost one year into the hardware portion of the upgrade
- As DIII-D and C-Mod, NSTX has provided (and continues to provide) key physics insights needed for future devices that would not have been discovered otherwise.
- NSTX-U will continue to serve ITER needs
- The NSTX-U program offers significant new physics understanding in a next-step operating regime for the ST, while providing unique leverage of this operating space to challenge and re-define understanding from more conventional tokamaks. (See the white paper by Jon Menard, et al. for some further detail. Additional review of the literature, and the full NSTX-U program plans provides further information). The tests of ST confinement physics, stability and control physics at lower collisionality, the aim for a significant amount of full non-inductive operation and related stability and disruption avoidance studies, and innovative techniques for high edge heat and particle flux handling are just some examples of key ReNeW goals that will be addressed.

Many other scenarios are possible. We could, for example, accept a downsizing of our program to university-scale devices supporting the education of students as the primary goal. But is it reasonable to define a country at this level of effort as a world leader? Without world-leading elements in the U.S. program, both the need for students, the students' desire to enter our field, and the students' competitiveness will decrease.

(iii) Embrace international collaboration

As suggested by DOE, we should further embrace the opportunities that international collaboration offer – most notably – modern long-pulse/steady-state superconducting devices.

There are advantages:
- It leverages modern, world-class superconducting facilities that are not available in the U.S.
- It provides a true research symbiosis. It's not business as usual, and that can be very good
- Young, energetic scientists are interested and willing to collaborate

and also difficulties:
- It's far more demanding than U.S. national collaboration, especially for young families
- It's more expensive than U.S. national collaboration
- It risks losing our scientists that decide to move abroad
- It requires collaborative interaction with a program managed by the foreign host, and driven by the host country's goals
- Success requires interest and respect for the host nation's history, culture, language, etc.

An important characteristic of international collaborative research is that it be strongly anchored to a U.S. based program to maintain U.S. expertise in our domestic program, and to provide a direct conduit for the research conducted abroad to immediately benefit the U.S. program.

While it may not be desired by all, and is not easy, international collaboration provides significant opportunities for research and the education of capable students and post-doctoral researchers. The most vocal opponents of international collaboration many times have little or no experience with it. Many negative comments are often just misinformed. Here are two examples in related to experiences regarding KSTAR:

- An individual recently questioned how H-mode studies could be conducting on KSTAR without auxiliary heating, unaware that the machine demonstrated NBI heated H-mode more than a year ago, and will be doubling NBI power in 2012.
- Many have the impression that plasmas in the modern superconducting devices will not be interesting/competitive compared to plasmas in U.S. devices. As one counter-example, KSTAR performance extrapolates to reach the n = 1 ideal no-wall limit with the doubling of NBI power in 2012 – a significant device milestone for stability studies.

A practical manifestation of the importance of magnetic fusion results is the granting of APS invited talks, especially the highly competitive post-deadline slots. A young POSTECH (Korea) researcher working on KSTAR was granted such a presentation (which contained compelling research based on analysis using an advanced diagnostic) at the APS DPP 2011 meeting.

With DOE supporting international collaboration, especially on modern superconducting devices, it is somewhat puzzling why DOE funding is not higher than it is at present. Research funding (excluding stellarators) is presently being re-competed at the $6M level. This represents an incremental increase of about $2M.

(iv) Justify the need for a new generation of U.S. fusion scientists

Much has been said about the need to train a new generation of U.S. fusion scientists – including input written by students. However, the need to train students goes hand-in-hand with a vital U.S. domestic program. Without a strong domestic program, students will find better training abroad before long. There will also be less motivation for U.S. students to pursue a career in fusion plasma physics if the U.S. program is limited to university-scale experiments. A strong U.S. fusion program, with a coordinated U.S. research effort on international facilities, provides the best justification to train the next generation of U.S. fusion scientists.

3) <u>Define a practical plan that advances magnetic fusion and execute when opportunity arises</u>

(i) Prepare as a community now for a future short-term opportunity

We know well that funding opportunities that would support a next-step fusion device in the U.S. are very rare. The next opportunity might be the period following the peak of ITER construction

funding. In contrast, it takes several years of planning and consensus building to be prepared to propose a major new U.S. facility. Therefore, we must prepare as a community now to be fully prepared when the rare opportunity arises. We will miss the opportunity without such advanced planning, and it might only come once in a generation.

(ii) Approach changes to the program carefully to gain consensus

Major directional changes in the US magnetic fusion program (e.g. closing of a major facility) should be done with plans that attempt to preserve manpower resources when these resources are critically low. Aiming at a future direction that has community consensus is difficult, but is most probable if concrete plans to preserve manpower are clearly stated.

(iii) Plan for manpower continuity during significant changes

Comments in early 2012 from FESAC indicated that this was not the approach taken regarding C-Mod. People will generally not give sound, unbiased advice about our program if their jobs are at stake. The committee should answer DOE's charge suggesting paths that would attempt to preserve manpower.

(iv) Encourage actions that efficiently address the defined research gaps

The 2009 ReNeW process defined the gaps in understanding and technological capabilities required for the advancement of magnetic fusion. It is best that we move on and do the research, and minimize distractions from this work. DOE should help scientists succeed in this by reducing impediments and helping to ensure that research FTEs be used on research and its management.

Dr. Richard Buttery recently made the observation at a USBPO session of FESAC white paper presentations that many presentations/discussions were highly negative, stressful, and didn't focus on solutions of how we should conduct the work that the ReNeW process has defined. Programs on DIII-D, NSTX, and C-Mod are doing this. They are reviewed with high frequency – processes that themselves consume significant FTEs of research manpower. Let's have confidence in this process, stay focused on the research, and let's ask DOE to find ways to help us do this with the highest efficiency.