Suggestions Regarding US-International Collaboration on Tokamak Magnetic Fusion Research

Steven A. Sabbagh – Columbia University 12/14/11 v1

Dear Dale and the FESAC International Collaboration Panel,

As per your request, below please find suggestions regarding US-International collaboration on tokamak magnetic fusion research. Most suggestions address your charge questions directly. Others might be considered outside of your charge, but are related and are equally important to how DOE will establish a new level of international collaboration in magnetic fusion research. Please consider them in completing your present task.

For reference, please note that I was a member of Mike Zarnstorff's committee that recently produced the document "International Collaboration Opportunities for the US Fusion Science Program" (July 2011). The charge that defined this report was similar to your present charge. Also, it may be useful to note that I have over 20 years of experience as a US university collaborator located off-campus at a US national lab, and have over 5 years of experience managing a small research effort on the Korean Superconducting Tokamak Advanced Research (KSTAR) device in Daejeon, Korea. I hope that you and your panel will find these suggestions helpful.

Best regards,

Steven A. Sabbagh

Senior Research Scientist
Adjunct Professor of Applied Physics
Columbia University
National Spherical Torus Experiment Run Coordinator

Princeton Office: (609) 243-2645 (LSB EWA 244)
Princeton FAX: (609) 243-2665

Suggestions Regarding US-International Collaboration on Tokamak Magnetic Fusion Research

Steven A. Sabbagh – Columbia University 12/14/11 v1

Suggestions are arranged in the following manner: (A) Research topics that can be addressed on international facilities (focusing on KSTAR in this document), (B) considerations and potential issues with extending the present US domestic collaboration research mode to international collaborations.

(A) Research topics that can be addressed on international facilities (focusing on KSTAR)

Mike Zarnstorff's document "International Collaboration Opportunities for the US Fusion Science Program" (July 2011) defined research topics that can be addressed on international facilities in greater scope and detail than I intend to give here. These are discussed at length and are also concisely summarized (organized by ReNeW thrust) in a table in that document. There is no need to repeat that information, as your panel can read the full report. Mike's document was produced following a similar charge to your present charge, and largely used the results from the ReNeW report (July 2010). Both reports are available on Dale Meade's FIRE website.

I would like to concentrate here on some specific areas mentioned in these documents that match my areas of expertise and interest, providing some further detail. These areas, marked in **bold** in the present document, are related to high beta physics and operation that are important to ITER, DEMO, and for forwarding our domestic fusion science program toward possible future US devices such as FNSF. I aim to be brief. As KSTAR is an existing superconducting device that is targeting high performance plasmas, and operating at high beta, the research mentioned is perhaps most highly associated with it, although EAST (presently operating) and JT-60SA (first plasma expected in 2016) might address this research as well (EAST would need a hardware upgrade, and JT-60SA should be able to address most of these topics perhaps a few years after starting operation).

A final important note is that these topics are being addressed to different extents in shorter pulse US facilities with copper magnetic coils. The US and international machines each offer unique capabilities to service the research. The unique aspects that can be investigated in superconducting devices are related to their capability of examining the physics and operation at greatly extended pulse. To be brief, the topics will basically be listed with a short explanation. These topics have been established as key research areas, and further discussion can be found elsewhere (e.g. the ReNeW 2010 document).

<u>Suggested areas of research</u>:

1) High beta plasma physics and maintenance at long pulse

While many targeted performance parameters have been reached in world tokamaks, such plasmas will need to be sustained for far longer pulse lengths in machines as FNSF, ITER, and DEMO. The superconducting high beta tokamaks will allow physics and control studies of maintaining high performance (e.g. high confinement, stability) plasmas on timescales of thermal equilibration of first wall components. The pulse length will far surpass the plasma current diffusion timescale, and examine profile evolution (or eventually demonstrate little to no evolution) in a long pulse operating mode not yet produced at high beta.

Common to all of the following studies is the unique physics understanding and control ramifications that come from full steady-state conditions, and the exact excursions from this condition due to transient behavior discovered.

(i) Stability physics of beta-limiting modes

(a) NTM onset and re-stabilization conditions under long pulse operation

(b) ELM onset and mitigation conditions under long pulse operation

(c) RWM onset and stabilization conditions under long pulse operation

(Note: especially important as the RWM has been found to be unstable in conditions of relatively high rotation, and disruptions due to the mode having been found to occur at lower than maximum beta due to unfavorable evolution of the plasma rotation profile. Also, sufficiently low plasma internal inductance, li, causes instability at any beta (current-driven kink).

(d) Current-driven kink onset and stabilization conditions under long pulse operation

(e) Cause and characteristics of perturbations taking the discharge away from steady-state profiles, and ramification on mode stability

(f) The role of fast particle profile in stabilizing beta-limiting and neutron-limiting MHD modes over long pule operation

(Applies to NTM, RWM, and *AE modes. Further investigations may show a link to ELM stability)

(ii) Control of beta-limiting modes

(e.g. KSTAR is close to needing this. The machine is already approaching the n = 1 no-wall limit. Normalized beta ~ 2 was reached in 2011. The no-wall limit is 2.5 – albeit at somewhat lower li than presently generated. In 2012, with a planned doubling of NBI power, the device might exceed the no-wall limit, generating RWMs and have significantly more issues with NTMs during H-mode.)

(a) Stability-relevant profile control under long pulse operation (see e.g. rotation control by 3D fields below)

(b) Real-time stability detection for MHD modes to avoid crossing marginal stability boundaries under long pulse operation

(NOTE: this is a multiple input / multiple output problem with profound physics modeling and implications)

(c) Active feedback control of global MHD with real-time alteration of mode spectrum under long pulse operation

(Includes multi-mode MHD analysis, as the mode spectrum can change depending on operating conditions. Key aspects specific to long pulse operation would be included, and emphasized as need, including long-pulse operation of sensors, etc.)

(d) Model-based advanced active feedback control of global MHD with multi-mode and real-time mode spectrum control under long pulse operation

(e.g. state-space control of RWM instabilities - as demonstrated in NSTX)

(e) Integration of mode stability physics and active control appropriate for long pulse operation

2) Three-dimensional physics effects and their use in fusion plasmas

(i) ELM mitigation under long pulse operation

(a high priority ITER request, which would require pages to fully address here)

(ii) Plasma rotation control by non-resonant applied fields under long pulse operation

(e.g. rotation alteration by NTV has many applications. For example, it can be used to avoid rotation profiles that lead to NTM or RWM destabilization. Also, in devices with co-NBI – most favorable for the best energy confinement (e.g. KSTAR), NTV-induced rotation control is a practical way to slow the plasma rotation to ITER-relevant levels. The highest ITER-relevance is a key to the missions of many of the devices being considered for international collaboration. Foreign machines with non-axisymmetric coils suitable for these long-pulse are presently KSTAR, and in the future JT-60SA (perhaps by 2018). EAST presently does not have a stabilizing conducting wall, nor non-axisymmetric coils.

(iii) Investigation of neoclassical toroidal viscosity (NTV) physics by non-resonant applied fields under long pulse operation

(Certain key details of NTV physics, such as the magnitude and scaling of the steady-state offset rotation, differ between present devices. Long pulse operation should allow more definite conclusions to be made on this topic when coupled to work performed on US devices – including dependence on plasma collisionality.)

(iv) Utility of high performance regimes using applied 3D fields under long pulse operation

(Operation such as QH mode in DIII-D is eased by the application of 3D fields. Can such operation be sustained over wall thermal equilibration times required for future devices?)

(B) Considerations and potential issues with extending the present US domestic collaboration research mode to international collaborations

Most points made here address the "research modes" portion of your charge.

Some of the following points might be outside the scope of your charge, but are very important and perhaps are underappreciated, so you might consider them in preparation of your report for DOE.

1) <u>Research solicitations should not be formed topically</u>

I have heard that DOE may organize the international collaboration solicitation by topic, rather than for example, by machine. While originally compelling, I believe this has some critical issues:

(i) Modern magnetic fusion research strongly combines topics

Other than for specific diagnostic collaborations, many research groups now span several topics. For example, stability and boundary physics are becoming more cross-cutting, mostly for ELMs, but now is becoming more important for global mode stability due to wall conditioning effects on confinement and ramifications of long pulse operation. The evolution of wall conditions and effects on profiles is key. Many other examples exist. Such essential cross-cutting may be declined in the review process if the reviews are conducted along strict topical guidelines.

(ii) Disadvantage to smaller proposals

Perhaps more important here is that a solicitation by topic, rather than machine, will hurt smaller proposals. This is less of a problem for domestic collaborations. The main problem is that a small group – for example, a university proposal of a few FTEs, will find it difficult to fully collaborate on one topic over several devices, as it takes a far greater investment in time and cultural understanding to conduct the most effective research program in more than one country. A larger group with many FTEs will be able to handle this and produce a more convincing proposal to conduct research on one topic across several devices. This could eliminate productive international research (historically delivered by small US domestic collaborations) before it could ever get started.

The significant time investment by researchers to learn and understand the culture of a country (including basic language – see #3 and #4 below) – needed for the most successful research - will be minimized if researchers focus on one country and device. Again, this gives larger group proposals, with more people that can each focus single on a device in one country, an advantage over smaller groups if proposals are organized and ranked by topic, rather than device.

2) <u>Certain proposed university program collaboration proposals are at a significant disadvantage vs. national lab proposals</u>

PIs from universities will in many cases have teaching responsibilities, and will not be able to spend extensive time abroad. Researchers at national labs generally have more freedom, which puts teaching professors and researchers at a disadvantage. This should at least be recognized in the proposal process. Other compensations might be considered. It might be expected that experimental and diagnostic proposals would be most affected, while theory and computation proposals may not be highly affected.

This can exacerbate other critical problems, such as the following (#3 below).

3) <u>Additional safety measures need to be implemented to avoid serious potential problems</u>

DOE pays significant attention to safety in general, which applies to US domestic collaborations. However, significant holes would exist for international collaborations unless specific actions are taken by DOE, some at the level of the proposal solicitation.

This is best illustrated by an example. Say that in a small university proposal, the PI cannot remain at the foreign institution. A PI might mistakenly send a researcher with insufficient experience to a foreign institution with a safety culture that is sub-standard. Especially if that researcher is an experimentalist, they could far more easily be hurt, or perhaps tragically killed, which could have major negative ramifications on DOE.

Special safety training and requirements need to be implemented by DOE to keep US researchers as safe as they are in DOE labs. Minimum requirements should include that US grantees understand the basics of the foreign language – one minor, but key example would be training on how to read caution and danger signs in the appropriate foreign language. Further special training should be given in cases where the foreign host has a safety culture that is less safe than DOE’s high standard.

4) <u>Cultural understanding is important for the most successful research</u>

Unlike in US domestic collaborations, even nominal success can be blocked from the most talented researchers by not understanding the appropriate foreign culture. Understanding language is additionally very helpful in understanding a culture. Groups that have skills in these areas – PIs/researchers with significant experience in the country / with the group of interest, and ability in the foreign language, have a significant advantage in producing successful research.

5) <u>Continuity of research is even more essential than for domestic programs</u>

Providing continuity of funding for students and researchers involved in research conducted internationally will be key to attract and maintain students and researchers. Because of this, DOE should consider longer funding profiles for international research – perhaps a 4 year timescale if possible.

6) International collaboration will most likely be more expensive than US domestic collaboration

One of the arguments for international collaboration vs. US domestic collaboration is that it will be less expensive, usually presenting the logic of cost savings by not having to build and operate a device at home. While that aspect is true, the cost per FTE for a foreign collaboration is greater vs. a US collaboration if the device cost is excluded. Having experience in both, just the travel expenses alone drive the cost of the foreign collaboration higher. Special training programs as mentioned above would further increase overhead. But there are far greater costs in productivity, especially if the PIs do not have experience with the foreign group, in the foreign culture, and especially and perhaps most importantly – with individual foreign contacts.

7) US collaborations will most likely have little say in foreign project milestones, which needs to be addressed in research topics requested and highlighted in grant solicitations

This is also a research cost that is not quantified. Most important here though is the realization by groups suggesting research topics for international collaboration that the US will not be able to set the research topics on foreign devices. Committees recommending such topics need to realize both US fusion program needs, and also the constraints imposed by the relevant foreign research facilities set by their own research milestones, which in many cases are different from US research priorities.

When possible, US collaborations should request some level of milestone development, and/or management of milestones for foreign programs.