Comments on community participation and design studies for a U.S. FNSF

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The U.S. fusion energy sciences program presently aspires to lead the development of a Fusion Nuclear Science Program (FNSP) and Facility (FNSF). A working definition of the FNSF mission is to: "Provide the nuclear environment prototypical of reactor to develop, test, and understand fusion materials and components needed for fusion energy development". Such a FNS program could be world-leading capability and transformational for materials and plasma science provided it is implemented soon enough to make a meaningful contribution in the overall world program. The U.S. certainly could play a strong and unique role in world program – but is the U.S. really prepared for this? Given the funding constraints implicit in the FESAC charge, and the present (potentially downward) trajectory in the non-ITER-project portion of the U.S. fusion program, the answer is: "probably not". In particular, on flat (or reduced) funding, significant physics, technology, design R&D would not be carried out to level sufficient for viable FNSP/FNSF design starting in ~10 years and operation by ~2030. Being able to address this readiness question requires a clear definition of FNSP and FNSF goals and parameters, and this does not yet exist, or is at least not widely accepted in the U.S. community.

Addressing the question of U.S. readiness and goals/parameters for an FNSP/FNSF more carefully would be very useful activity for U.S. fusion community, and such activities should be strengthened and broadened. Such activities can be complimentary to ITER and with the understanding that ITER is and should be very high priority in U.S. research program. Indeed, ITER has a world-wide research program (much of it pro-bono through ITPA) dedicated to achieving the ITER mission, and the U.S. is a very strong contributor to ITER and should remain so.

The ReNeW workshop of 2009 focused on gaps from the present capabilities to DEMO, and an FNSP/FNSF was and is proposed as means a to narrow/close many gaps to DEMO. However, there are also many gaps from the present to an FNSP/FNSF. It is also noteworthy that an FNSF would likely cost at least as much as the presently planned U.S. contribution to ITER. Given the importance of defining the vision for the future U.S. program, the ReNeW activities should be built upon and followed up with a ReNeW-2 or Snowmass-like activity in which the U.S. community focuses on the goals, needs, and priorities for a U.S. next-step emphasizing primarily the mission of an FNSP and FNSF. As part of this exercise, the community should also consider the viability of a FNSP/FNSF program under various funding scenarios including the present and anticipated future flat funding. Alternative and potentially less expensive U.S. leadership opportunities should also be explored - including for example very-long-pulse PMI facilities, a U.S. PoP-class quasi-symmetric stellarator, and other ideas where the U.S. can have a leadership role. In addition, given the advanced operational scenarios with fusion gain Q = 1-5 that would likely be required of an FNSF, the U.S. Burning Plasma Organization could also expand beyond ITER to incorporate FNSP/FNSF research needs and support.

There is no question that developing the basis for FNSP/FNSF is an exciting, necessary, extremely challenging research enterprise. It is noteworthy that the ITER physics basis development is still ongoing in key areas including disruptions, ELM control, divertor detachment, and the development of heating and current drive actuators to name a few. Relative to the ITER inductive scenario, the physics basis for a steady-state nuclear FNSF remains to be developed. FNSF will ultimately require steady-state (~10^6s) scenarios with plasma performance sufficient to provide > 1MW/m^2 neutron wall loading (see for example numerous studies and papers led by M. Abdou). The necessary FNSF-equivalent plasma performance and power and particle exhaust handling have only been accessed transiently in present devices. Further, FNSF would ultimately be fully a nuclear device, and most of the long-pulse actuators, diagnostics, components (NBI, RF, PFCs) are being developed outside of the U.S. Only modest U.S. efforts on FNSF maintainability, structural materials, first-wall components, remote handling, and blankets, are being carried forward.

As just one example to put things in perspective, smaller fusion programs such as India have ITER TBM programs, while the U.S. does not. Thus, it is unclear who will develop, design, or fabricate the materials and components the U.S. would aim to test in an FNSF. Perhaps one possibility for the U.S. is collaboration or partnership with a strong fusion technology and engineering collaborator.

It is clear that an enhancement of design activities and focused R&D is needed to enable development of a U.S. FNSF. To have a viable FNSP/FNSF program, conceptual design and engineering analysis need to be strengthened. Not only do physics/scenario requirements drive facility parameters and design, but facility design also strongly influences achievable plasma performance through the choice of heating and current-drive actuators, stability control systems, and achievable confinement to name a few. The choice of materials also strongly influences the physics and performance of the plasma core, for example high-Z PFCs can reduce retention but lead to core impurity accumulation, and ferritic steel in the blankets could impact the magnetic topology, lead to fast ion loss, etc. There are also many important questions that only design activities can address – examples include:

- Can a single facility support a staged approach, i.e. perform needed PMI research, transition to a fusion nuclear science mission, followed by component testing and/or even a pilot plant mission?
- If a stage approach is not viable or cost effective, how many facilities are really needed for the fusion development path?
- Which ITER physics and technology can be leveraged for FNSF?
- Could stellarators offer attractive (steady-state, disruption-free) alternative approaches to FNSF?

Simply put, it costs money and time to even approximately estimate of how much an FNSP and FNSF would really cost, and this would be money well spent if the U.S. is to pursue the FNSP/FNSF path.

Lastly, in addition to strong support for ITER, the U.S. tokamak facilities should be explicitly charged with a goal of developing scalable integrated scenarios for FNSF that extrapolate to achievement of the FNSF performance requirements. Such efforts would leverages key U.S. strengths, namely: an experienced and highly productive workforce, advanced diagnostics, world-leading plasma control capabilities, and strong efforts in simulation and model validation. These scenario development activities would be highly synergistic with the development of operating scenarios for partial-inductive ITER operation, ITER advanced tokamak (AT) operation, and longer-term for a tokamak or ST Demo. Such FNSF scenario development research would also be a potential leadership area for the U.S., and aspects of these scenarios could be followed up with long-pulse testing on the EAST, KSTAR, and JT-60SA superconducting facilities with eventual application to FNSF.