Fusion Energy Sciences Priorities Over the Next 10-20 years

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The Fusion Energy Sciences program is undergoing a transition as the global fusion effort approaches the operation of ITER, and the subsequent pursuit of a demonstration power plant (DEMO). The US fusion program has supported a dominant plasma science emphasis for the last 40 years, due to the extreme complications of pursuing fusion nuclear science simultaneously. However, in the light of numerous engineering/physics issues challenging ITER, the engineering and plasma science areas can no longer sustain a separation (more accurately, a nearly complete separation). In fact, the converging of fusion nuclear science and fusion plasma science is imminent, and a successful program must focus on integrating the elements rather than separating and preserving them.

The US view (experts in fusion nuclear science) has been, and continues to be, that an intermediate fusion nuclear device (between ITER and DEMO, often called a Fusion Nuclear Science Facility or FNSF) is necessary to provide the technical basis to proceed to a DEMO, simply because we have virtually no experimental database on which to design, construct and operate such a device. Following this basic premise back to where we are now, shows that a ramping up of fusion nuclear science research is required. Structural materials have not been demonstrated to survive the fusion nuclear environment to any significant exposure, and a facility capable of providing this environment is necessary to establish the basis for any fusion nuclear confinement experiment. The blankets (combination of structural material, breeder material, insulator material, shield material, coolant) responsible for receiving the nuclear heating and breeding the required tritium have never been constructed or tested even in partial environments (neutrons, temperatures, pressure/stress, magnetic field, chemical/corrosion, hydrogen) they would endure in a fusion device. The divertor and other plasma facing components (including the first wall, launchers/openings, and diagnostics) have not been demonstrated in the appropriate environments or for the durations they would see in a fusion device. A number of other engineering science issues are lacking as well, such as extracting tritium from breeder materials, quantifying the 3D MHD flow of liquid metal breeder materials, establishing continuous plasma exhaust removal, establishing the plasma loading conditions on plasma facing components, and so forth. An element of the fusion program that must be ramped up is that of fusion nuclear science.

The plasma material interface has been recognized for a very long time to be critical to fusion plasmas, although only recently with the design of ITER, has it been taken very seriously as a research thrust in its own right. The plasma durations required of ITER and future devices exceed those presently experienced in plasma experiments by large factors. Simultaneously our knowledge of the scrape-off layer plasma physics, particle evolution and transport from plasma to SOL to walls and back into the SOL and plasma, and physical and chemical material surface behavior under particle and heat loading is quite limited. Even further, our simulation capabilities of these processes are far too immature to predict anything but the simplest trends. A significant thrust in this area needs to become a focus of the fusion plasma science program, including strong integration of the engineering and design science required to develop viable solutions. A very large portion of this science can be done on present pulse length US experiments, with pulse length extension possible by transferring knowledge and experience to KSTAR/EAST. Ultimately, the longest pulse lengths will only likely be accessible on linear plasma devices and the FNSF device itself.

A number of plasma science areas (not exhaustive) require aggressive activities to establish 1) long sustained plasmas at sufficient performance for many current diffusion times, 2) strong plasma shaping and subsequent requirements (passive stabilizer, feedback coils, divertor slot geometries) to achieve this, 3) controlling plasma power and particles simultaneously at sufficient plasma performance (integration), 4) relevant divertor solutions in terms of density, temperature, radiated power fraction, and control/stability, 5) achieving core plasma performance simultaneously with viable edge/SOL/divertor solution, 5) elimination of plasma disruptions, and 6) elimination or mitigation of transients like ELMs. Naturally the SOL plasma physics aspects are common to the PFC and more general plasma science areas. Integration is a new and critical component that emerges from these considerations, and was made very clear in ITER design effort. Again these activities can be performed very efficiently on our present US facilities, with a long term transfer to KSTAR/EAST.

It is well known that most scientific advances result from technology advances, as our diagnostic and control capabilities improve so do our scientific insights. Several areas that serve as support or enabling are necessary to provide the conditions allowing both a fusion plasma and fusion nuclear performance. These include 1) diagnostic development including impacts of nuclear environment and reduction in number and resolution, 2) plasma heating and current drive systems in terms of materials in the nuclear environment, efficiencies from source to plasma, and long term reliability, 3) superconducting magnets, both low and high temperature, and 4) fueling, pumping and particle control. Any of these areas can provide a severe vulnerability to a successful next step fusion nuclear experiment.

The Fusion Nuclear Science Pathways Assessment (FNS-PA), completed in 2011, developed a research activities plan for the next 5-10 years in the areas of 1) fusion material science, 2) PFC and PMI, 3) power extraction and tritium sustainability, 4) safety and environment, and 5) enabling technology. The study used a roll-forward and a roll-back approach, the later by examining power plant study assumptions and turning these into research needs. To a smaller extent, the area of plasma duration and sustainment was addressed to provide focus to the critical physics issues requiring attention for a successful fusion nuclear program. The development of experimental activities is critical to the growth and sustainment of this area. This report is available at http://www.pppl.gov/pub_report//2012/PPPL-4736-abs.html.

The task of identifying priorities for the US fusion energy sciences program over the next decade is not an effort in shuffling, costing, and streamlining, but managing a transition. Parts of this program must grow and parts must shrink. The plasma science program must transition into focused science activities, not an open ended exploration for the next "silver bullet". The US confinement devices (Alcator C-Mod, DIII-D, and NSTX) are tremendous long term investments in people, infrastructure, and scientific discovery. These facilities cannot be operated forever, but from a scientific point of view, at least 5 years, and more likely 10 years, are needed for our program to establish the scientific basis in the areas listed above. These developments can credibly be extended to KSTAR/EAST within the same timeframe, as these Asian devices will become the extension of our confinement program leading to a fusion nuclear facility in the US.

The simultaneous development of theory/simulation capability is absolutely mandatory to move into the future of fusion. Although engineering science has appreciated and relied on computational progress in parallel with its design and experiments for a long time, the plasma science program has continued to provide two separate cultures of experimentalists and theorist/modelers, who rarely cooperate. The parallel development of diagnostic, experiment, theory and simulation in critical topical areas, like those listed above, needs to become the template for fusion plasma and fusion nuclear science in the US. In fact, it is well recognized that our diagnostic capabilities on fusion nuclear devices in the future will diminish, making very clear the need for simulations to augment what measurements can be made, and the need to take advantage of non-nuclear devices (where diagnostic access and capability can be maximized, a closing window of opportunity) to the maximum extend for model validation prior to moving to nuclear devices.

One of the most difficult aspects of long term planning is identifying the goal. Is it a DEMO, is it a FNSF, or is it a 10^{th} of a kind power plant. And after choosing one, you have to define it. There are visions of power plants, which were used in the FNS-PA study, and there are proposals for FNSFs, which were used for reference where parameters were required. Although it is not possible to know these device requirements precisely, or what scientific approaches will succeed between now and then, much of the research over the next 5-10 years is basic and generic, both in plasma and nuclear science. The ReNeW and other studies have outlined the plasma science areas, and the FNS-PA report outlines fusion nuclear science areas. These activities can be addressed without large financial expenditures, by utilizing present infrastructure, upgrading existing facilities, and constructing smaller high effectiveness facilities, and most of all focusing activities. Larger expenditures are likely required beyond the 10 year time frame as the US approaches the design, construction and operation of a FNSF.