The critical importance of materials and plasma-wall interactions studies

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The ReNeW study, and subsequent discussions, have focused a spotlight on the substantial gaps in knowledge between the current state of fusion plasma physics understanding and engineering knowledge that are required for design of a successful FNSF or reactor. These gaps range from magnets to core plasma physics. In our minds, and in the reports of successive FESAC panels (including the Greenwald, Zinkle and Meade panels), the gaps are much larger in the areas of materials and plasma wall interactions than in the core physics. Here we focus on the requirements for materials at "zero dpa", i.e. even before response to neutron irradiation is considered. To get us within striking distance of the ability to design an FNSF/reactor, in the context of even this "zero-dpa" case, requires answering many crucial questions:

- Can we reduce the heat loads on PFCs down to low enough levels to be handled steady state? Reactors or FNSF will have up to 5 times the global power density of ITER, yet must accommodate this using thinner structural/HHF materials than ITER at the plasma-facing surfaces, due to tritium breeding ratio (TBR) requirements. Low power density is not an option, since high power density is the single most important factor determining economic attractiveness, and TBR>1 is a must. Failure of heat exhaust components will immediately terminate viability of the device. This calls for two simultaneous urgent efforts: 1) make substantial progress in our understanding and control of the plasma's heat distribution and 2) make integrated progress in PFC material and coolant technology that provides a larger margin for peak heat loading on PFCs.
- What will be the long-pulse (days/months/year) effects of wall plasma equilibration, long-range erosion and material migration on plasma control, performance and PFC lifetime? This timescale leap presents the biggest extrapolation from current experience of any relevant parameter (factor of 100,000 at least).
- Will tungsten, or some alternative, work at the required high particle/head loads simultaneously with the high bulk temperature necessary for reactor thermal efficiency? While high temperature is advantageous for annealing of nuclear damage in materials, lower T retention, and some mechanical properties, we presently have ZERO integrated experience with high bulk temperature materials in confinement devices, Because the material response to temperature is hyper-exponential, effectively we have "tested" all our plasma-facing materials in an irrelevant environment.
- What can be done to make our RF, heating, current drive and control methods, required in reactors, as efficient as possible, while simultaneously having them work in steady state without requiring coupling structures in close proximity to the plasma? The effects of high power RF on the SOL and the core plasma must be compatible with PFC lifetime and core cleanliness. Simultaneously, how can one assure survival of launching structures, which are typically discrete toroidal structures? This area requires close coupling between the core and boundary physics, i.e. the development of steady-state scenarios simultaneously compatible with the current drive and heat load limits in each.
- How do we develop robustly disruption-free tokamak operation, including finding integrated core/boundary solutions which are stable? If these cannot be found then the unacceptable damage to plasma-facing materials will force us to abandon the tokamak towards confinement schemes that separate thermal and magnetic instabilities.

The recent JET results remind us again of the importance of materials choices to tokamak operation, verifying decades of experience in confinement devices that the boundary/material conditions matter greatly to plasma performance. JET core plasma operation, in terms of pedestal characteristics and confinement, are clearly affected by the choice of wall material; the dynamics of disruptions are strongly affected as well. C-Mod and AUG results emphasize the importance of thorough research to understand the related role of RF (both ICRF and LHCD) in affecting the SOL and impurities (sources and transport).

General guiding principles to the panel: As we contemplate the above gaps and the FESAC charge for direction of the future US program we urge you to emphasize the above issues associated with plasma-wall interaction as the highest priority. We should preserve the research into, and tokamak operation with, high-Z materials which, from our present state of knowledge, provide the best chance to meet the various reactor requirements using solid materials. It is imperative overall that we "push" the PFC materials, particularly tungsten, testing to more prototypical reactors conditions of heat loading and operating temperature to truly assess their viability. The community should explore how to make a smooth transition from the current complement of tokamaks and materials research to a better coordinated program which will be even more effective in addressing the issues discussed here.

Recommendations:

a) Near term (under FY12 or FY13 budget scenario):

Materials research: Don't let the US fusion program evolve exclusively into a materials research program as Dr. Synakowski advocates. This would leave the US with far too narrow a knowledge base, making it impossible for us to take advantage of the eventual world development fusion energy. Instead, concentrate this materials research on niche topics (e.g. modeling, high temperature PFC systems) where we can excel and compete with the EU, which currently has much larger dedicated effort (and soon China will as well).

Toroidal systems science: At a minimum, preserve today's research on the tokamak plasma edge, the majority of which, for the most relevant materials and plasma conditions, resides at C-Mod. In parallel, and as funding allows, expand plasma edge research utilizing other existing facilities - e.g. alternative divertor designs, stellerators, liquid metals if possible, even if this requires some de-emphasis of core physics studies.

b) Longer term: Convert the US toroidal research program over to one more directly focused on the large gaps outlined above, while maintaining capabilities in core plasma research. This may require phasing out present confinement devices, to bring on new ones to test and close the above gaps, perhaps using ideas outside of those based on our present (20-years old) reactor boundary concepts.

Determine whether tungsten can fulfill the requirements of a reactor PFC under as close to reactor conditions as possible - steady state, high bulk temperature, high heat/power fluxes. If it does not, then drop it.

Explore/develop solutions (which may mean a dedicated small device) that cut the connection between thermal and magnetic instability – the fundamental issue underlying disruption triggers. This can be addressed through both stellerator and tokamak research.

Focus the US materials research program on niche areas that maximize US talents and capabilities.

Produce realistic reactor studies that examine the integrated issues of alternative boundary solutions and technologies (liquid surfaces, divertor topologies, etc) in order to understand the necessary trade-offs (e.g. tritium breeding ratio) in using these solutions and for guiding experiments/machines to test them.

Dedicate significant machine time, and perhaps new facilities, to the development and understanding of RF current drive (and to a lesser extent, heating) with the levels of efficiency, the core plasma compatibility, and gap to the separatrix, required for reactors.

The focus of current FNSF design studies relies on extrapolation of performance that would be at the edge of stability – leading to a complex device, that would likely suffer significant downtime and low availability. What we need instead is to be focusing on simpler designs, that stay away from operational boundaries but still achieve the economics needed.