The Continuing Need for Multi-Configuration Research

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Research that explores and optimizes the magnetic configuration has been and should continue to be a priority in US fusion research. This is a strength of our program, and has led to US leadership on a number of fronts. There are at least two ongoing challenges that cry out for continuing exploration of magnetic configuration space:

(1) *Predictive understanding of the science of magnetically confined plasmas*: due to the cost and complexity of steps toward a fusion reactor, extrapolation beyond the parameter ranges and experimental base attained directly on any development path appears unavoidable. Maximizing the range of understanding through research that adjusts the basic variables of magnetic confinement is much more likely to provide reliable and predictive fusion science than if restricted to the variables that define a tokamak configuration. This is the case even if the ultimate fusion reactor configuration is similar to the present-day tokamak.

(2) *The enormous technological gap between present-day experiments and an economical GW fusion power source*: a fusion power source must operate robustly for millions of seconds without major maintenance and repair to attain the required availability for economic viability. Surely the challenges have not yet been fully articulated. The tradeoffs represented in optimized magnetic configurations offer enormous potential to address specific challenges as they arise, especially armed with predictive understanding that spans magnetic configuration space.

Below are brief comments that illustrate (far from completely) how multi-configuration research helps address these challenges. There are many more examples described in documents such as ReNeW, and through research on configurations that were explicitly excluded from ReNeW.

While the fusion community has yet to embrace fully the rigorous methodologies of Verification and Validation as practiced in fields such as fluid dynamics, it is obvious that theoretical, computational, and experimental capability that spans a wide range of the major variables of magnetic confinement (toroidal field, plasma current, magnetic shear, 2D/3D shaping, aspect ratio, etc) provides a more robust approach for the Validation of fusion plasma science. With the challenge of predictive capability on the table, it is arguably more important to increase rather than decrease the range of configuration space research. With regard to experimental capability, a range of facility scales will likely be optimum, from focused experiments targeting specific processes, to moderate scale toroidal plasmas that exhibit multiple processes linked by complex nonlinear phenomena, to the fusion power production scale like ITER where plasma diagnosis is much more difficult. Could the required maturity in predictive science really be attained with ITER and an FNSF alone? Will anybody trust the path that follows ITER other than through demonstrated predictive science?

The viability of fusion will no doubt be strongly determined in large part by solutions to the plasma-material boundary interface. Beyond processes such as heat removal, this interface must also incorporate the technology to produce, maintain, and control the core plasma. For example, high-power rf antennas for heating and current drive are located at the boundary, close to the plasma for their efficient operation. Antennas have defining constraints that are separate, even disparate from the challenge of heat removal. Will these antennas struggle to maintain consistent operation for millions of seconds adjacent to a 2-3 GW fusion plasma, as required for high availability of the power source? An alternative is inductive heating and current drive, which nearly invisibly crosses smooth boundary surfaces. This could greatly increase the reliability and maintainability of a fusion system, not only in delivering power more simply, but freeing the boundary to be better optimized for challenges such as heat removal. Ohmic ignition is not an option for the configuration space occupied by the tokamak and stellarator, but it is a possibility for the reversed field pinch, which explores the limit of high plasma current and low applied toroidal magnetic field. There are of course potential disadvantages of the reduced external magnetization corner of magnetic configuration space, e.g., susceptibility to magnetic turbulence. The point is to understand and develop the science so that these tradeoffs and options for solving hard problems are predictable.

Continued progress demands investment of resources on all fronts of fusion development. Because of the relative state of maturity, the cost for research on configurations beyond the tokamak is presently modest. But it is important to recognize that well-diagnosed experiments are fundamental to rigorous Validation as outlined in Thrust 6 of ReNeW, and that next steps in development are essential to maintain the viability of alternative approaches to the integration of the many solutions needed for an economic fusion power source. The opportunity for next steps in non-tokamak research must be included in future planning. The trend in the last couple of years, e.g., reductions in the “ICC” portfolio, suggest the opportunity for configuration optimization is slipping away.