Key Priorities of the U.S. MFE Program in Fusion Science

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There exists a great opportunity for the U.S. to lead ITER research in several strategic areas where there is clear U.S. strength, but achieving this requires focus and commitment on our part. Fields in which the U.S. has scientific leadership, both theoretically and experimentally, include transport and turbulence, energetic particles and nonlinear MHD; additionally, the U.S. is rapidly rising in the area of non-axisymmetric or "3-D" magnetic fields. We should use these areas of U.S. scientific leadership to select research thrusts for prioritization that have a payoff in fundamental understanding and are amenable to rigorous validation. For example, the U.S. fusion energy program can elect to focus on certain thrusts from the *Research Needs for Magnetic Fusion Energy Sciences* report, namely

> Thrust 3: Understand the Role of Alpha Particles in Burning Plasmas
> Thrust 4: Qualify operational scenarios and the supporting physics basis for ITER
> Thrust 6: Develop predictive models for fusion plasmas, supported by theory and challenged with experimental measurement

The U.S. has unique capabilities in the areas of transport, energetic particles, nonlinear MHD and 3-D fields that are critical to ITER's success but also support the long term research needed to move us forward on the fusion energy development path. Strengthening our research in these fields should be an important part of the risk management plan for ITER, in which the U.S. has developed the expertise and modeling capability to overcome any physics obstacle that may prevent ITER from achieving its fusion performance goals. Each of these areas provides a good mix of short term and long term research possibilities and would engage all segments of the U.S. fusion community, *i.e.*, the students, postdocs, staff scientists and professors who work on theory/simulation, modeling, diagnostics and experiments.

The primary and essential strength of the U.S. fusion energy program that has led to its persistent leadership position is the close coupling of flexible, well-diagnosed experiments with detailed, quantitative theoretical and computational studies. Specifically the development and experimental ***validation*** of theoretical models, and their computational implementations, has led to a strong predictive capability that has been extensively used to guide experiments on existing devices, and to plan and optimize future devices such as ITER, FNSF and DEMO. Several decades ago our advances were primarily empirical, such as the discovery of H-mode in tokamaks, while more recent advances, such as development of high performance, low torque ELM-free operation in Quiescent H-mode, were theoretically predicted before being observed. However, substantial gaps in our understanding remain, and further scientific advances are required not only in the hot plasma core but also in the edge region of the confined plasmas and in the open field line region and plasma-material interface. These scientific advances will improve our capability to predict and optimize the full plasma system and open possible avenues to dramatically improve fusion performance in ways which could have substantial impact on reactor design and cost.

The following essential components are key to maintaining and enhancing U.S. scientific leadership in fusion:

1. Flexible devices that are capable of exploring a broad parameter space in key dimensionless parameters, in both the core and the edge, as well as in plasma shape.

2. Extensive diagnostic systems that measure all essential profiles, with good spatial and temporal resolution, to allow definitive, quantitative validation studies.
3. Sufficient experimental time to collect detailed information for regimes critical to ITER, FNSF, *etc.*, but also to allow exploration of qualitatively new regimes (not just incrementalism).
4. A strong theory and computation program, with dedicated resources for validation, emphasis on key gaps in understanding, and close coupling to experiment, while maintaining sufficient freedom to explore basic theory and innovative ideas.

In the presence of budgetary constraints emphasis should be placed on exploiting the U.S. scientific strength: the world's most flexible, best diagnosed facilities and a strong theory and computational program closely coupled to those facilities. *The key missing resource presently is sufficient experimental run time* (by a factor of 2) to take full advantage of our excellent facilities. Research areas should be prioritized in terms of U.S. scientific leadership (*e.g.* electromagnetic gyrokinetics, energetic particle physics, nonlinear MHD, pedestal physics and the effect of 3-D magnetic fields) while recognizing the importance of advancing towards the MFE goal (*e.g.* edge physics, disruption physics, integration). Resources should be made available for extensive validation studies. A focused effort similar to the planned Fusion Simulation Program could be highly useful, but it must emphasize validated physics results. The clear goal must be to develop the scientific understanding required to predict and optimize the performance of future devices, including devices such as ITER where many characteristics are already fixed, and future devices such as FNSF and DEMO, where there is an enormous range of high-dimensional parameter space available for extensive optimization and cost reduction.

Towards the end of the next 10 year period, it is essential that the U.S. find resources to both operate ancillary facilities to ITER to ensure its success and to begin building a new facility that can reproduce DEMO-relevant conditions, particularly at the plasma edge and material interface. Ongoing advances in the next 2-3 years should allow continual reductions in the cost and size of such a device, while maintaining reactor relevant characteristics. Better understanding and characterization of materials is clearly important, but unlikely to be sufficient. An optimal solution to the fusion plasma material interface problem will require a detailed predictive understanding of the plasma characteristics at the interface and robust techniques to modify them, together with innovative approaches, such as novel divertor geometries, and/or combinations of sacrificial low-Z materials with structural high-Z components well isolated from the core plasma.

International collaboration offers significant opportunities, including model validation for long pulses in a variety different configurations, but it is important to note that new international devices are typically more limited in their range of parameters and will have limited diagnostic systems (particularly in the near term). There is no guarantee that local priorities will align with the U.S. desire to experimentally validate physically comprehensive models. Furthermore, the extensive engagement between theorists and experimentalists necessary for definitive model validation is significantly more difficult to achieve for U.S. theorists when the experiments are overseas, particularly in cases where the full set of data is not easily available. Most importantly, the incentive for international groups to collaborate strongly with the U.S. is based on our leadership in key areas, and that maintaining strong U.S. scientific efforts as discussed here is necessary to maintaining this leadership. If these U.S. efforts are not maintained, the value of U.S. partnerships to international groups will steadily diminish, and U.S. researchers will lose the ability to take full advantage of collaboration with international devices including ITER.