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October 7, 2014

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Attention: Dr. Christopher Keane: Acting FESAC Chair for Strategic Planning Panel Report Discussion; Fusion Energy Sciences Advisory Committee and 2014 Strategic Planning Panel

Critique of FESAC Strategic Planning Sub-Panel Draft Report
We are writing to express our deep concern with the recent draft FESAC sub-panel report on strategic planning. While we can agree with many of the identified priority areas, in our view the present draft fails to offer a strategic plan, contains inconsistent recommendations and fails to take account of the extensive input of the fusion community.

In the following sections, we present specific criticisms regarding 1) the alignment of the plan with its stated goals, 2) the recommended approach to the plasma-material interface challenge and 3) its lack of innovative initiatives to improve fusion concepts.

1. **Alignment of Strategy and Goals:** The draft report adopts a 10 year goal - to be ready for a start on an FNSF in 10 years - but does not provide a roadmap for getting there; nor does it assess whether that goal is achievable. As a result, the program elements favored by the report do not constitute a coherent plan that would lead us to the goal, within the constraints of the given budget scenarios. Instead the report suggests a set of budget choices uninformed by a consistent broader strategy. This is a major flaw. Choices on where to put resources need to be consistent with a realistic roadmap. In our view, this flaw led in fact, to incorrect choices and priorities. A useful plan to achieve the proposed goal would place high priority on the near-term R&D required to start the FNSF design by the end of this 10 year period, but the report recommends initiatives whose results will not be essential for decades (for example long-term neutron fluence effects on structural materials). A roadmap is also essential in order to assess the resources needed to achieve the goal. The recent FESAC priorities panel report [1] judged that budget levels significantly higher than the guidance would be required. Thus the path endorsed by this report would almost certainly not achieve its aspiration while failing to take advantage of opportunities that are in reach. Consequently it is strategically imbalanced.

2. **Addressing the PMI Challenge:** With respect to the plasma materials interaction challenge, which the panel endorsed as critical, the report ignores the numerous white paper submissions and testimony by the community which emphasize research on integrated toroidal confinement experiments that can best simulate the divertor and boundary plasma conditions needed [2]. Instead it recommends development of linear-plasma material test stand(s) which would not be unique in the world and would not be capable of addressing this issue in a decisive way. The real

challenge is to develop a configuration and operational scenario, compatible with realistic engineering constraints and a high performance plasma core. The correct metrics for relevance in such experiments are ITER-level power flux density and plasma pressure along with reactor-like divertor geometry and materials. It recommends that the US program "undertake a technical assessment with community experts to ascertain which existing facility (or facilities) could most effectively address the key boundary physics issues". In making this recommendation the panel acknowledges the fact that it is not technically qualified to make a judgment in this area. We strongly support carrying out this assessment of facility effectiveness -- by an expert peer group. The assessment should be open to all proposals and carried out without prejudice. Inconsistently with this review recommendation, and without justification, the draft report goes on to recommend the linear plasma device initiative, which would be a "single-effect" experiment and would not address the main challenge of integrated physics. In making these and other recommendations concerning facilities, the report offers programmatic choices in this area that are technically unjustified and potentially damaging to the US research portfolio; it therefore does not provide appropriate guidance towards a solution to this critical challenge.

3. **Improving Fusion Concepts:** The report does not support, in a serious way, innovations that could improve fusion concepts and make them more attractive as a power source. For example, by ignoring research into high-field fusion magnets that would exploit emerging high-temperature superconductor technology, the report misses perhaps the best opportunity for major cost savings in next-step fusion facilities and reactors. The current path, using conventional superconducting magnets, as in ITER, leads to large unit size, high costs and very long development times. The recent emergence of high temperature superconductors as a forward-looking magnet option offers the possibility of game changing technology for future high-magnetic-field fusion reactor concepts. The option for higher fields can only be available for next step designs (including an FNSF) if R&D not advocated by the report is pursued. Its omission would effectively eliminate perhaps the best option we have for reducing the cost of the next step and a future reactor [3]. By shortchanging research on RF current drive and stellarators, the draft report sidelines the U.S. in the vital area of plasma steady state sustainment. For the tokamak to be useful for component testing or as a practical steady-state energy source, advanced operation with reactor-relevant current drive will be required. Exciting new ideas have emerged for efficient, reactor-compatible RF current drive systems, as outlined in the white papers [4]. However, domestic contributions in current drive, under the report's recommendations, would focus on technologies (such as neutral beam current drive) that are unlikely to be reactor-relevant or reactor-compatible, and are thus essentially irrelevant [5]. The draft report also fails to advocate any significant experiments in the U.S. on stellarators. The stellarator, while not as advanced in performance as the tokamak, is a plausible alternate with advantages for producing a steady-state fusion plasma and for avoiding the transient events that are identified as a high priority [6].

We are conscious of how difficult the task was with which the panel was charged. However, its draft report proposes research and facility priorities without technical justification or a broader strategic focus and does not support innovations in areas required to make fusion energy feasible and attractive. If followed, the draft report would effectively cede leadership in most important areas to other countries while ending up with a fusion reactor concept that is economically unattractive in U.S. terms. We therefore urge FESAC to reject this report and to engage more strongly with the community to formulate an exciting and effective plan for the nation's fusion energy research.

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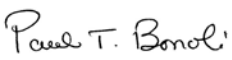
This statement reflects the personal views of the undersigned, not MIT as an institution.



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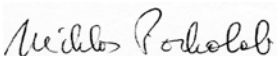

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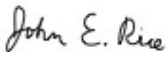

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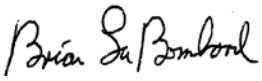

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