October 8, 2014

Dr. Christopher Keane
Acting FESAC Chair for Strategic Planning Panel Report Discussion
Prof. Mark Koepke
Chair, Strategic Planning Sub-Panel

Dear Dr. Keane and Prof. Koepke:

We are writing to the FESAC panel to comment on the draft report "Strategic Planning: Priorities Assessment and Budget Scenarios", and request that FESAC address several deficiencies in this report before transferring it to DOE/FES. We are taking this approach to comment on this report because, given the late release of the report to the public, there was no time to offer constructive comments at the recent FESAC meeting.

Generally, we welcome a new strategic vision to include research on relevant fusion nuclear science issues within the FES program, such as materials under intense neutron fluxes and the interactions of hot confined plasmas with adjacent solid material structures. An increase in emphasis in these areas will necessarily require changes in allocations of scarce research funding, and that in turn requires difficult choices among program elements. Indeed, such changes to the program have been advocated by some U.S. fusion researchers, and suggestions for an evolution of the program while maintaining critical strengths in the U.S. program have been offered. None of the signees to this letter advocates maintenance of the status quo in the fusion research portfolio, and all welcome dialog and planning to advance the program in new directions. However, we are deeply concerned that the elements of the strategic plan as described in this report have major flaws and unsubstantiated foundations. Furthermore, arbitrary or abrupt changes can lead to a degradation of the program if not justified or managed well. In that context, we note the draft report has several glaring deficiencies that could undermine the support of the research community for the proposed strategic directions as a whole. The points of most concern to us that need to be addressed by FESAC are given herein.

• The underlying strategic vision that guides this report is flawed
This report unnecessarily narrows the fusion science research program to a few initiatives. The proposed programmatic emphasis is focused on preparing for the operation of two future facilities that will be producing significant fusion-relevant plasmas more than 10 years from now. The third part of the proposed program vision consists of an extremely narrow call for workforce development aimed at these future facilities as "Generation ITER-FNSF". None of this defines the program as a science-issue-oriented research enterprise that has pressing scientific issues and opportunities.

A troubling feature of the strategic plan is the wholesale orientation of the research program on preparing for an undefined Fusion Nuclear Science Facility in the near future. While some members of the fusion community believe FNSF is a logical next step facility, there is not yet technical or scientific consensus on what the design or even mission for such a facility is. The need and/or importance for such a major step can only be judged in the context of an overall strategic roadmap to fusion energy, which has not been discussed in this or other recent FESAC planning processes. Indeed, many of our international partners do not include an FNSF-like step in their fusion energy development plans, nor do their plans depend on the U.S. pursuing that

step. The cost for such a facility makes it unlikely that the U.S. would pursue FNSF on its own, so international collaboration would be essential and is simply assumed to exist. An extended planning and study exercise is needed in the U.S. technical community to define and motivate any such major step, and no such discussion has taken place as yet. We clearly will not be able to advocate for this step, if needed, unless there is wide technical consensus and enthusiasm for it, both here and abroad.

A major element of the proposed strategy is the development of fusion energy technologies, accompanied by a significant reduction in fusion and plasma science research under any realistic budgets. While an increased emphasis on fusion technology development can be expected along any path to fusion energy at some point, there is no demonstration in this report that progress in fusion and plasma science is sufficiently mature in the context of fusion program objectives to warrant this reduction. New fusion energy technologies include topics such as test blanket module development, tritium fuel cycle tests, etc. Such energy technologies are required to realize fusion power production. However, fusion research is currently located in the DOE Office of Science, and historically such energy technology development has been associated with a commitment to a fusion energy development program. To our knowledge, no such change in policy has been made by the government, and hence it is hard to believe a redirection with an emphasis on fusion energy development will result in maintaining, much less increasing, support for fusion research in the U.S. Here again, the strategic plan appears to make an assumption that is poorly justified. Even the most optimistic funding scenario considered by the subpanel appears to fall well short of what is required to pursue a viable fusion energy development program.

Finally, there are repeated claims to "leadership" in specific areas of fusion research and development, with no accompanying discussion of the content or value of such leadership. The two major initiatives called out as Tier 1, the transient events and PMI studies, are topics of extensive experimental and theoretical investigations by fusion research groups around the world. It is hard to claim leadership in these areas without an in-depth discussion of the particular physics issues that can be resolved exclusively by the U.S. community. Such analyses are not presented in the draft report. It is incumbent on FESAC to more specifically define how such leadership is measured and achieved if it is to be a defining focus of the program.

• The program is presented too much as a facility-oriented development plan

A wide range of white papers and presentations were submitted to this panel by the research community on relatively short notice. These offered challenging and scientifically interesting topics and initiatives to guide and motivate evolution of the research portfolio in fusion sciences, but there is very little reflection of that scientific vitality of the program in this report. The proposed plan starts with an assertion that the fusion program should condense to support participation in ITER and preparation for a large new DT facility in the U.S. The repeated references to those two facilities as the focus of the U.S. fusion program, without justification or broader references to the wide range of compelling scientific issues and challenges inherent in the fusion quest, reinforces the old bias from outside communities that this program is simply an empirical machine-building enterprise. This does a disservice to the fusion science research community, which has worked assiduously over the past decades to be more relevant to the mission of the DOE Office of Science and follow the best scientific practices of the research communities supported by the Office of Science.

• There are no scientific cases made for the choices made in this report
Almost all programmatic choices are presented as simple management decisions to fit the desired new initiatives into a tight budget envelope. In contrast, clear compelling scientific reasons for such decisions are missing. The critical issues to be addressed need to be enunciated, and the reasons for the particular choices must be clearly justified. Simply stating that the program should support ITER and move to a large new FNSF facility does not, in itself, make programmatic choices obvious. There are many assertions of discussions by the subpanel on reaching the conclusions described, but no layout of the scientific reasons to support those conclusions. As such, the conclusions carry little weight but that of declared management direction.

• There is a lack of competition and rigorous peer review for the few major new facilities or programs advocated in the strategic plan
Over the past few decades, FES has done an admirable job in developing a culture of and processes for intellectual competition and peer review to identify research initiatives worthy of funding in times of scarce resources. This conforms to the practices of the Office of Science as a premier sponsor of physical science research, and assures Congress and the Administration that judgments of funding merit are as unbiased and free of conflicts of interest as much as possible. This approach has both improved the science focus of the fusion energy sciences program, and helped improve the standing of fusion and plasma sciences with other STEM communities. Indeed, FESAC itself just received a briefing from the Associate Director for Biological and Environmental Research, which again confirmed the benefits of following these practices.

This Draft Report repeatedly emphasizes the need for community discussion and peer review for some areas of the program. However, in the case of the three recommended major initiatives (the linear high heat flux facility, the spallation-source-based neutron irradiation facility, and the FNSF itself) the report simply declares these initiatives should be pursued in specific facilities, implying no need for competition of ideas and peer review. This contrasts with all past practices that led to significant new facilities in the fusion program. Decisions based on ad hoc 10-minute presentations to this FESAC subpanel should not substitute for in-depth competition and review of proposed new facilities.

To garner support for these new initiatives and identify the best options for fulfilling the goals of such initiatives, FESAC should instead identify the scientific issues and missions for such initiatives and FES should then follow with an open competition for proposals to address the identified issues. Such an approach will result in a sounder decision on these initiatives and significantly reduce any appearance of conflicts of interest in the choice of what initiatives are ultimately funded.

• The stewardship of plasma physics as a respected component of the U.S. physical science research portfolio is seriously undermined
Following repeated FESAC reviews and several National Academies reviews, FES has been encouraged to lead the stewardship of plasma physics in the Federal complex. In the past, FES has attempted to do so, even in the face of limited resources. However, the proposed strategic plan is explicit in its weak support of basic plasma science. It identifies the already modest

Discovery Plasma Science program as a donor for funds to support other initiatives, under any of the more likely funding scenarios. The principal recommendation for DPS defers support for new directions in plasma science to unspecified collaborations with other agencies, with no evidence that growth in such partnerships are in fact welcome or fundable. This recommendation is therefore unsubstantiated, and portrays a willingness to leave plasma science without strong stewardship.

A much better approach would be for FESAC to make a clarion call for eliminating the chronic lack of modest funding of plasma physics in the Federal portfolio by recognizing plasma physics as a fundamental physical science in its own right. The discussion offered in the present report can only encourage suggestions of moving plasma science stewardship in the U.S. to more welcoming sponsors, to the detriment of the fusion community and FES.

• RECOMMENDATIONS:

- FESAC should undertake or require a rewriting of this report to more clearly make the scientific case for recommendations made in the report, and should orient the presentation of these topics to enunciate the deeper scientific issues being addressed.

- The overall 10-year plan needs to be framed as challenging and exciting scientific investigations to resolve specific issues and test relevant theories related to advancing fusion and plasma sciences. It should reflect the wide range of issues that need to be addressed for fusion energy.

- It is premature to select specific facilities for the highest priority initiatives identified by the subpanel. This report should be modified to identify the mission and scientific goals of any new initiatives, and encourage open solicitation and peer-reviewed competition to invite innovative and exciting solutions for those initiatives.

- Plasma science should not be a donor program under any budget scenario. A robust case for funding increases to support plasma science as a physical science in its own right, without depleting fusion science sources, needs to be made.

Thank you for your attention, and we look forward to seeing this report evolve into a plan the research community can enthusiastically support.

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