Summary Report of the National Spherical Torus Experiment Upgrade (NSTX-U) Review Committee

(Compiled from individual reports by Stephen Eckstrand, DOE Program Manager)

Proposal Title: National Spherical Torus Upgrade Five-Year Plan

University or Organization: Princeton Plasma Physics Laboratory

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Background

On May 9, 2013 Princeton Plasma Physics Laboratory submitted a proposed Five-Year Research Plan for NSTX-U to DOE. The Five-Year Plan includes 5 high level goals:

1. Demonstrate 100% non-inductive sustainment at performance that extrapolates to ≥ $1MW/m^2$ neutron wall loading in FNSF
2. Access reduced collisionality and high-beta combined with ability to vary *q* and rotation to dramatically extend ST physics understanding
3. Develop and understand non-inductive start-up and ramp-up (overdrive) to project to ST-FNSF with small/no solenoid
4. Develop and utilize high-flux-expansion “snowflake” divertor and radiative detachment for mitigating very high heat fluxes
5. Begin to assess high-Z PFCs + liquid Lithium to develop high-duty-factor integrated PMI solutions for next-steps

The proposal is structured around seven topical areas: 1) macroscopic stability, 2) transport and turbulence, 3) boundary physics, 4) materials and plasma facing components, 5) energetic particles, wave heating and current drive, 6) plasma formation and current ramp-up, and plasma sustainment and 7) advanced scenarios and control. Each of these topical areas is composed of 2-4 research thrusts that support one or more of the five high level goals.

The Office of Fusion Energy Sciences organized a peer review panel to review the Five-Year Plan. The charge to the review panel requested an assessment of the proposal based on the following five criteria:

1. The scientific and technical merit of the proposed research
2. The appropriateness of the proposed research plan
3. The competency of the proposed senior research personnel and the adequacy of the proposed research environment and resources
4. The reasonableness of the proposed costs for research and facility operations
5. The performance of the NSTX team during the previous five-year period.

This report summarizes the reviewer panel members’ responses to each of the five elements of the charge. In general, the reviewers’ responses to these five elements were fairly consistent, so representative comments from several reviewers are combined into a single narrative. However, there were some differences of opinion that were not amenable to consolidation, and a summary of these comments is included in a separate section.

This summary report also contains specific comments from individual reviewers on each of the seven technical areas of the proposed NSTX-U research program. Again representative comments on each technical area are combined into a single narrative.

1. Assess the scientific and technical merit of the ongoing and planned research.

- *Does the proposed research effectively address important issues in plasma and fusion energy science and technology at the forefront of the field (e.g., those issues described in the FESAC reports Research Needs for Magnetic Fusion Energy (2010) and Priorities of the Magnetic Fusion Energy Science Program (2013)?*

The scientific and technical merit of the proposed NSTX-U research program is very high. The proposed research targets several of the most urgent and significant issues in spherical torus (ST) physics, including

- Fully non-inductive startup, ramp-up, and sustained operation,
- Confinement and transport at low collisionality,
- 3D field effects for ELM and rotation control, and
- Divertor power and particle control and PFC development.

The facility upgrades and diagnostic improvements that are included in the NSTX-U project will significantly enhance capability of the facility. As the most powerful and best diagnosed spherical torus in the world, NSTX-U will be in a good position to address critical issues for extension of the ST concept into the nuclear regime – non-inductive start-up/ramp-up and power handling at large values of power/radius. Furthermore the program is well positioned to explore fundamental physics issues of stability and transport at low aspect ratio.

The most critical need for the advancement of STs to nuclear the nuclear stage is the capability to start up the plasma and ramp to full performance with no inductive solenoid. The NSTX-U team proposes to addresses this need with coaxial helicity injection, or perhaps plasma gun injection, followed by a complicated mix of electron cyclotron heating/current drive, electron Bernstein wave heating/current drive, high harmonic fast wave heating, and neutral beam heating/current drive. The off-axis neutral beam injection (NBI) capability and increased volt-seconds of NSTX-U will enable state-of-the-art experiments in neutral beam current drive and will complement research on other U.S. facilities.

The second critical area for extension of ST into the nuclear regime is the development of techniques to dissipate large loss power in a device with small major radius, while maintaining compatibility with the constraints of plasma purity, low collisionality and pedestal characteristics needed for high performance. These are very challenging requirements and the plan for addressing them is well thought out and deliberately staged. The key elements of the plan are to control density and Z effective through boron or Lithium conditioning early on and later with cryo-pumping, to manage extremely large heat fluxes with flux expansion techniques using a snowflake divertor and radiative divertor detachment. The choice of magnetic geometry and its optimization to reduce heat flux is a critical research topic for all MFE concepts, including conventional aspect-ratio tokamaks and stellarators. Experimental data on this topic will be essential for validating models of the scrape-off layer, which will be of benefit to all concepts.

While NSTX-U research is likely too late to have significant impact on the ITER design, it can have an impact on research planning and operational scenario development. In particular, research areas where predictive physics understanding can inform ITER include (1) rotation and mode locking, (2) disruption prediction, avoidance and mitigation, (3) ELM control using ITER relevant tools such as 3-D coils and pellet injection, (4) high flux expansion divertors and radiative divertors, (5) impurity transport, and (6) energetic particle physics. NSTX-U has an active program to carry out this type of research in coordination with ITPA topical groups and through joint experiments with other facilities worldwide.

The proposed program is broad, covering, in addition to the two critical areas mentioned above, the other scientific areas necessary for continued development of the ST, including MHD stability, transport physics, and energetic particle physics. The proposed tasks in these areas are well-conceived extensions of ongoing work on NSTX rather than fundamentally new research. Although the extensions required in these areas for nuclear applications are probably not as large as those needed in the areas of non-inductive startup and power handling, an integrated solution of all the requirements will be a big challenge. The most likely area for fundamental advances is in the development of liquid Lithium plasma facing component technology.

Overall, the proposed research is also well in-line with ReNeW Thrust 16: “Develop the spherical torus to advance fusion nuclear science”, an item reiterated in the recent MFE Priorities Panel Report (Rosner Panel, 2013).

- *How well does the proposed research compare with that carried out at other U.S. and foreign tokamak facilities, both in terms of merit and originality, and how well does it maintain a U.S. leadership position in key areas of fusion research?*

NSTX-U will be a leading facility in the world fusion program, exploring the unique physics of the low aspect ratio spherical tokamak with a focus on evaluating the ST as a possible approach for a next step device. NSTX-U will have unique capability to study collisionality and fast-ion effects in high-beta plasmas at low aspect ratio. The addition of a second set of neutral beams, configured for off-axis injection, will enable a broad range of experiments seeking fully non-inductive ramp-up and steady state high-beta operation, needed for any tokamak-based FNSF device. The beam improvements will enhance study of current and pressure profile effects and enable state-of-the-art fast-ion experiments and model validation experiments using an enhanced set of EP diagnostics. NSTX-U capabilities and research plans will enable the US program to maintain world-leading standing in the physics of fast-ion stability and transport moving into the ITER era.

The planned research on 3D magnetic field physics and steady-state operation with off-axis current drive and high bootstrap fraction will confirm and complement research being carried out on other US and international tokamak facilities, which also have 3D coils but operate at higher aspect ratio with other means of non-inductive current drive. Variable off-axis current drive will enable direct comparison with results from ASDEX-U, JT-60U, and DIII-D obtained at higher aspect ratio.

The proposed NSTX-U scrape-off-layer/divertor research program will explore important topics related to power and particle control. There is a relatively heavy emphasis on transient wall conditioning and PFC research by the group as compared to most other tokamaks (JET and C-Mod excepting). The addition of a divertor cryo-pump will be an excellent addition to their program.

The capability to stress the divertor components with high heating power using varying geometries makes NSTX-U a world-leading research facility for boundary science, even with successful operation of the superconducting tokamaks in Asia, since those facilities largely focus on long pulse length and materials testing.

- *What is the likelihood that the research will lead to new or fundamental advances in fusion science and technology?*

Overall, the proposed research on the high-priority subjects outlined in the proposal has much in common in with DIII-D, EAST, KSTAR, and the JT-60SA (under construction in Japan) but will be carried out at much lower aspect ratio, extending to values well below those usually considered for future fusion tokamaks (DEMO and even an ST-based FNSF). However, the impact of such a low-A extension on fusion science is unclear, especially if larger aspect ratios (A>2) are envisioned for reactors.

The heavy emphasis on DC helicity injection (or CHI) for plasma startup and initial current ramp-up seems larger than necessary given likely central solenoid capabilities for an FNSF-ST device and likely limited lifetime of CHI insulators in a high neutron fluence device. The plasma guns offer an alternate means of startup which should be given adequate support and experimental time. The V-S capabilities of NSTX-U will enable effective research on non-inductive current ramp-up and sustainment to current levels expected for startup in an FNSF-ST. Research on the transition from startup to ramp-up should be emphasized as compared to initial formation with CHI.

The proposed test of mesh-entrained liquid Lithium is novel and will generate lots of interest from the PMI community, though the technique may not scale to high power density, steady-state operation. The placement of rows of tungsten tiles is of questionable value, but is a known risk to performance in light of similar experiments carried out elsewhere. NSTX-U has unique capabilities for studying the effect of large poloidal flux expansion and new x-point magnetic configurations at high heat flux; planned improvements to divertor SOL plasma diagnostics will be essential in exploiting this unique leading-edge capability for developing scientific understanding.

The research plan for understanding and improving particle control for the ST was not clearly articulated, so it is hard to gauge the expected significance of the proposed research beyond the present operating regimes. The relative roles of PFC material choices, wall conditioning (including baking, boronization, and lithiumization), and pumping in providing density and impurity control for H-mode operation in the ST was not explained well, nor was the plan to resolve these roles. There appear to be many questions still in play and the plans seem more trial and error rather than a systematic study of the role of ELMs, recycling, and impurity sources on particle control for the ST.

The strength of the proposed NSTX-U program lies in its potential to contribute to plasma physics understanding via model validation and development. Access to unique physics regimes, the rich interplay among core/boundary/PMI physics and the good diagnostic set should enable fundamental contributions in a number of key research areas. NSTX-U is an essential facility to maintain US leadership in ST research, and it appears to be well positioned to make fundamental advances in fusion science and technology.

2. Comment on the appropriateness of the proposed research plan.

- *Is the proposed plan adequately developed and likely to lead to scientifically valid conclusions?*

The proposed five-year plan is comprehensive in scope and supported by an extensive set of simulations and data analysis. There is a clear logic for how the research plan is to be carried out. It is focused on the key high priority topics and goals related to ST physics and operations considerations. The timeline for restarting tokamak operations is well thought out and aims to reach 50% higher field and current during the first year of operation.

Overall, the program seems very ambitious for a five-year program, and the goals of the program are somewhat aggressive. In reality, many elements of the plan seem more appropriate for a 10 year program. The challenge of restarting the facility, with new tools, new configuration, new actuators, new diagnostics, etc. will require more time than it is generally allocated. This will be further complicated by expected lower repetition rate and the need to relearn the particle control physics required to achieve the right conditions.

There is a concern about the staging, or perhaps prioritization, of work on non-inductive start-up/ramp-up and power handling. These are potential showstoppers for nuclear applications of the ST concept. Questions of compatibility of heavy Lithium conditioning with high harmonic fast wave heating, and unfavorable previous results with liquid Lithium on LTX are troubling. Solutions may be slow to develop, may require several iterations, and may also require considerable machine operating time. Moreover, the ECH/EBW system, the plasma guns, and the liquid Lithium divertor do not appear until very late in the plan. Even the low power ECH is not available until the last year of the 5-year plan and flowing liquid Lithium does not appear at all in this time frame. If at all possible these program elements should be accelerated and a concerted effort made to obtain full performance discharges without ohmic assist and to assess the suitability of liquid Lithium. Since the experience base with spherical tokamaks is so much smaller than that for conventional aspect ratio tokamaks, a convincing demonstration of solutions to many issues will be needed before a commitment to an ST based nuclear machine can be made.

The plan for using a mix of materials during the next five years may make it challenging to resolve the physics questions and issues associated with determining the appropriate materials for an application like FNSF. Various techniques ranging from boronization to Lithium evaporation and from carbon to all metals walls are proposed in a broad brush fashion. With all these tools it is anticipated that density control eventually will be achieved on NSTX-U; however, a systematic plan to realize this goal was not clearly presented. It is likely that addressing the PMI issues will only be started during the next 5 years, with a much longer term program required to make significant progress. The transition to all metal walls is tentatively scheduled to begin near the end of the five-year period. The implications of this major change on overall programmatic goals remain to be evaluated and could be significant. Due to the breadth of the proposed research program on NSTX-U, significant prioritization will be required for allocating run time and meeting key mission elements.

The MHD stability research plan is well thought out and organized. The importance of MHD stability to achieving the programmatic research goals is appropriately recognized. Plans to evaluate the MHD issues are comprehensive; research challenges and priorities are clearly identified (e.g., neoclassical tearing modes, error field and NTV torque effects, and disruption avoidance). The research on NSTX-U, coupled with work on other tokamaks, should lead to a significantly improved physics basis for ITER operation and for FNSF design.

The plan for turbulence and transport research relies upon improved density control, increased heating power, and higher confinement to reach lower collisionality, without which its relevance to future burning plasma experiments or FNSF may be somewhat limited. The plan for electron transport research will benefit greatly from the high-k microwave scattering and polarimetry diagnostics to assess possible ETG and micro-tearing modes, an assessment which is essential to investigating the underlying physics of electron thermal transport. Research on electron transport in NSTX-U, coupled with similar research on DIII-D (at higher aspect ratio), and could provide new understanding of this key topic, though such coordination/comparison was not discussed.

There appears to be good complementarity between the NSTX-U and MAST-U research programs in addressing important ST science. A very close coordination is viewed as critical, since validation of all important results would be required if the ST version of a FNSF is preferred. Otherwise, the data base required to make such a critical decision would be lacking.

Coupling to PPPL theory is an issue. Historically, the PPPL theory group has not properly served the NSTX project. The new theory head gave indications that he will change the culture that caused this split. However, it seems from his presentation that there is not yet a clear theory program developed that parallels the goals of the NSTX-U project. Rather, most of the examples cited were projects already in place. If the primary goal of NSTX-U is to provide the scientific basis for extrapolation to FNSF, a well-coupled theory/modeling effort is a requirement of the research program.

- *Does the proposed research employ innovative concepts or methods, and are potential problems identified along with appropriate mitigation strategies?*

The research tool set is extensive and innovative. For example, the NSTX-U program plans to significantly extend the use of plasma guns for non-inductive startup, possibly demonstrating an attractive alternative to CHI-based startup. This may produce exciting new results and is an excellent example of incorporating innovation into the research program. Another innovation centers on the large-scale use of Lithium wall conditioning and exploration of liquid-Lithium PFC technologies. Proposed use and testing will occur on a greatly expanded scale over the next 10 years. The challenges for the research were acknowledged, and cryo-pumping and boronization will be explored as an alternate technique for particle control.

The development and extension of the disruption prediction algorithms also represent a significant innovation which will provide important data on disruptivity of high-performance operating regimes without requiring use of large blocks of dedicated time to observe the frequency of disruptions under specific conditions. The NSTX-U approach will have important implications for work on other tokamaks and for design of FNSF and DEMO.

The proposed research on 3D field effects is well aligned with that of other research groups around the world, which is important. The proposed research likely will provide valuable confirmatory results, but may lead to new understanding due to the low aspect ratio and high beta operation. The new 3D field coils will be an important new tool, provided adequate power supply flexibility and capability is available to fully exploit this new system. The innovative state-space approach to RWM control is an important development for stable high performance operation; capabilities here will also benefit significantly from the new 3D coils.

The research plan is adaptive, recognizing potential problems that might arise in important areas. For example, the decision to implement nearly complete high-Z coverage of the first wall is contingent on demonstrating adequate heat flux mitigation techniques.

- *Assess the strengths of the program with respect to manpower development through graduate student training.*

The NSTX program presently supports 33 graduate students and 14 post docs, which is in line with its connection to Princeton University and its budget. The main part of the proposal does not discuss graduate student training and participation, though it appears that there are many opportunities for students spread throughout the program by way of collaborating institutions.

Graduate students affiliated with NSTX collaborators are called out explicitly only in Chapter 11 of the Program Plan “NSTX-U Collaborator Research Plans.” It is unclear how these students are integrated into the program, how many are situated on site, and how many stay in fusion research as post docs and researchers. However, in the presentations, it was noted that half of the PRLs published had students or junior research scientists as primary authors, which is indicative of their successful integration into the NSTX program. The number of post docs also points to opportunities for career development in fusion energy research provided by the NSTX-U program.

NSTX provides an excellent opportunity for student training. With the disappearance of the Alcator C-mod program the role of NSTX in this regard becomes even more important.

3. Evaluate the competency of the proposed senior research personnel and the adequacy of the proposed research environment and resources.

- *How well qualified are the applicant's personnel to carry out the proposed research?*

The NSTX research team is very experienced, consisting of 70 PPPL researchers and 236 non-PPPL researchers with 59 APS Fellows, 3 Presidential Early Career Award winners, 4 DOE Early Career Award winners. While extensive experience is important, it is also encouraging and perhaps even equally important to see a significant number of young researchers, including 17 recent additions to the research staff along with 14 post-docs and 33 students. The NSTX team averaged 52 refereed publications per year during the past 5 year funding period. The five-year total included 17 publications in Physical Review Letters. The NSTX team was also strongly represented through invited presentations at all important national and international conferences. The NSTX team is truly a national team and is fulfilling its role of training future fusion energy research scientists. Now that the U.S. FES program has been reduced to 2 major devices, the role of collaborators should be expanded at the project level.

The team is highly competent to carry out the planned research, as shown not only by their previous work, but also by the clarity and enthusiasm of their presentation of future plans and research goals. It is a world-class research team.

- *Does the proposed work provide for an adequate set of diagnostics, other necessary facility upgrades, interactions with theory and modeling, and collaborations involving a broad group of domestic and international users?*

The upgrade of the NSTX facility will produce a world-leading ST research facility with unique capability for off-axis current drive that can achieve very high power loading (P/S) for divertor research.

The upgrade to the NSTX facility will greatly enhance its ability to explore novel parameter regimes, and thereby to make possible significant advances in tokamak physics.

The existing baseline diagnostic set is very good. The proposed upgrades follow a logical discussion of measurement needs from the research program, both in the base program and those identified as incremental.

The upgrades proposed in the base funding plan, a lower divertor cryo-pump, a 1 MW, 28 GHz ECH system, and off-midplane, non-axisymmetric control coil set are adequate. The proposed additions of flowing liquid Lithium divertor plates and a divertor Thomson scattering diagnostic are desirable. A reassessment of the importance of the flowing Lithium divertor relative to other items covered under base funding is recommended.

Collaborators have been involved in the NSTX research program through key leadership roles in the science teams and were involved in the formulation of this five year plan for NSTX-U. However, these involvements appear for the most part to be on an individual basis (with possibly one major exception). Collaborating groups have come to be an important part of NSTX/NSTX-U programs, and this should continue and be expanded, where appropriate. However, changes in the US domestic program landscape, specifically the reduced number of facilities, require that NSTX-U offers greater opportunities for strong involvement in the research programs, including project leadership, overall governance, long term planning and prioritization of research elements.

The involvement of theory groups appears in the research program of NSTX-U appears to be improving; one illustration of this is the selection of a theory co-chair in each topical area (a good practice). There is a good connection with the PPPL theory group and broader MHD stability and transport communities, and theory analysis support for the energetic particle research is world class. However, the involvement of PPPL theory group in other areas seems to be at the initial stage, and should be improved.

- *Assess the program's governance practices and the performance of the program management team, as well as the support to collaborators provided by PPPL.*

The NSTX-U program structure is divided into an operations branch and a research branch. Collaborators play only a minor role in the facility operation and upgrade tasks, primarily tasked with providing diagnostic development and support. Collaborations involve ~10% of the total NSTX-U budget and the largest collaborations support 2-3 FTE staff scientists. This level may be optimal for university-scale collaborations, but is much less than optimal for long-term healthy national laboratory collaborations.

PPPL provides a broad range of support for on-site collaborators including offices, telecommunication, library, and publication services. Technical support for diagnostics and data acquisition is also provided to collaborating students, post docs, and scientists.

The NSTX-U management team effectively promotes both PPPL and collaborating scientists for invited talks, conference attendance, and fellowships and other awards. The team provides research leadership opportunities to collaborating scientists on a regular basis and assists younger scientists as they compete for funding to work on NSTX-U.

- *How well do the collaborative arrangements achieve the goal of an integrated research team?*

The team is scientifically well integrated. Based upon the presentations, there is a common programmatic vision among all the institutions participating in the NSTX-U program. Experimental planning appears to effectively engage the staff from all institutions. The review panel did not have formal meetings with collaborating scientists to fully explore this topic.

The overall program is clearly managed by PPPL. No formal governing structures or committees involving representatives from the collaborating institutions are in place; all agreements are documented in a "Record of Discussion" document. The NSTX-U Program Advisory Committee, consisting of members from a broad cross section of the U.S. Fusion community, meets twice a year to provide advice to PPPL management on program plans for research and for facility upgrades. However, it does not formally represent the body of collaborators on the NSTX-U project.

The connection between the NSTX-U project and PPPL is clearly different than that of the other two major tokamak experiments in the U.S., which form almost the entirety of the fusion research efforts at General Atomics and MIT. Other PPPL institutional issues may influence long-range priority decisions for NSTX-U as compared to these other devices; the effect of these on the collaborative arrangements is unknown, though it appears that the present PPPL management is quite open about its priorities and interests.

4. Assess the reasonableness of the proposed costs for fusion research and facility operations.

The cost review should be done at a summary type level, examining major items and using projections from ongoing operational experience.

- *Does the technical proposal call for the equipment and components, labor skill mix, and hours set forth in the summary cost information, and are these reasonable to carry out the proposed research?*

The costs for the proposed research and facility improvements are in line with other comparable facilities in the U.S. The labor hours (including mix of skills) are also in line with projects of this size and nature. The costs for major hardware upgrades (e.g. ECH and divertor modifications) and for new diagnostics are also reasonable. There is an appropriate mix between scientists, engineers, and technical staff. Administrative costs also appear to be typical of research group this size.

- *Are the overall proposed costs reasonable? (Please note that the cost details of the proposal will also be reviewed separately by DOE. However, we are interested in hearing your views on this topic.)*

FES provided funding guidance for both a base and incremental budget for the five-year plan. The overall NSTX-U base budget for the Five Year proposal period will support expanded operations and facility capabilities as described in the program plan. The proposed funding represents a significant increase compared to program funding during the first half of the previous Five Year plan. It looks as if the higher total program budgets associated with the upgrade project are being converted to operating budget. Additional funding is likely needed to cover higher costs associated with operating a second neutral beamline, running at higher current and toroidal field (e.g., higher utility and maintenance costs), and a more complex set of PFCs, including installation of a new divertor cryo-pumping system. The costs of some of the future upgrades are based on engineering estimates and/or DIII-D data.

The balance in research between PPPL and collaborators is an area of concern. About a 20% increase in PPPL research budgets is proposed during the five year plan (no prior data was provided, so it’s unknown if this continues a longer term trend), but collaborator budgets remain relatively flat. This appears to be inconsistent with plans to install more diagnostics in key areas while at the same time relying on the collaborators support diagnostic development and graduate students for the program. With the reduction in the number of major facilities in the US fusion program, it is important for collaborators to have an expanding role in the NSTX-U program.

5. Assess the performance of the NSTX research team during the previous five-year period.

- *Were research and diagnostic milestones met?*

During the previous 5 year funding period, the NSTX project has met all its major operational and research milestones except for number of operation weeks in 2011 when the premature magnet failure resulted in completion of only 4.2 of 14 planned run weeks. This failure led to the early initiation of the NSTX upgrade project. None-the-less, the NSTX team managed to publish significant results and, by shifting staff temporarily to other facilities, conduct meaningful experiments. Overall, the early termination and start on the upgrade may turn out to have been a positive event for the program. Excellent scientific productivity has been maintained and the NSTX has met its adjusted research goals.

The high level update presented at the review on the upgrade project indicated that it remains on schedule and on budget and should be ready for supporting physics experiments in early FY15 as planned.

- *Were NSTX research results appropriately disseminated, and are they having an impact on the international fusion effort?*

The NSTX-U team made excellent progress on their previous Five Year Plan, though hampered by the failure of the tokamak's TF-OH center-stack assembly. Publication rates for scientific results remained high for the program, in spite of the shut down. The work of the macro-stability team on resistive wall modes was particularly praiseworthy. The excellent work on fast-ion transport and the effect of fast ions on MHD stability is informing experiments at DIII-D and abroad. The participation of NSTX in the DOE Joint Research Target on scrape-off-layer heat flux width is an exemplary case of the NSTX having an impact on the broader fusion effort, as the data was combined into an international database.

As noted above, the NSTX research output, as measured in terms of refereed publications and conference presentations has been commendable and is clear evidence of the group's productivity. The NSTX team has made a significant contribution to the advancement of the spherical tokamak concept by performing world-leading research for this magnetic configuration.

- *How well did the NSTX team compare theory and experiment to further the FES goal of improving predictive modeling?*

The research team an excellent record of comparing experiment to simulation and developing improved predictive capability in many areas. MHD stability analysis, state-space controllers for RWM stabilization, and energetic particle physics are outstanding examples of strong coupling between theory-simulation and experiment producing new understanding which is broadly applicable to DOE-FES program goals and to ensuring success on ITER.

- *Also, assess the plans for NSTX Upgrade facility operations (at a top level). Are planned operating, maintenance, repair and upgrade schedules appropriate to support the planned research program?*

The ambitious plan for commissioning the new hardware and carrying out a wide range of experiments starting in FY15 is well within the capability of the research and operations teams. Creating a pause in operations between FY16-17 to install the divertor cryo-pump appears to be a good approach to completing that task. Previous experience has shown that making major changes to the PFCs and/or wall conditioning techniques (e.g., cryo-pumping, boronization, and installation of high-Z materials) will likely introduce new operational challenges requiring some adaptation with some impact on the experimental plan. This however, is to be expected and is a natural consequence of the research. The team should plan to document their experience with a view to broad applicability to future devices.

- *Are environment, safety, health and quality assurance matters being given appropriate priority?*

The NSTX team has demonstrated an outstanding safety record that speaks for itself in terms of program priorities, business practices, and expectations (both for individuals and the program as a whole). It is especially noteworthy that NSTX received the State of New Jersey Commissioner of Labor and Workforce Development's 2010 Continued Excellence Award for working 10 consecutive years (2,011,666 hours) without an away from work lost time injury/illness case.

The analysis of the failure of the center-stack components has informed the design of the Upgrade components, increasing confidence that they can meet requirements. The overall research plan for the first year of operation looks to be a reasonable plan to extend the operating space of the tokamak using the increased capability added during the present shutdown.

A Significantly Different Point of View

The proposed NSTX-U research program places undue emphasis on establishing the feasibility of a next-step ST material testing facility (the so-called FNSF). Given that it has become painfully obvious, over the last few years, that we do not yet possess a sufficient understanding of tokamak physics to design and build ITER with any degree of certainty, the idea that NSTX-U will yield sufficient information to allow us to design and build an FNSF is not credible. The US fusion program, as a whole, would be far better served if NSTX-U's limited operating schedule were devoted to conducting research on topics that have direct relevance to conventional tokamak (CT) physics.

Of the five goals of the NSTX-U program, only the second and the fourth have direct relevance to CTs. Achievement of goal 3 (non-inductive start-up and ramp-up), which likely will be extremely difficult, will have absolutely no relevance whatsoever to CT research and will use up the much of the run-time available to NSTX-U.

Also, there seems to be little sense in introducing high-Z PFC's into NSTX-U. Research on such PFC's is already being performed on many tokamaks, and starting it on NSTX-U would be an unnecessary distraction. NSTX-U boundary research should concentrate instead on establishing the feasibility of the snowflake divertor. Of the stated goals, goal 2 and, in particular, goal 4, are by far the most important, and should be given priority over the others.

Three upgrades to NSTX-U are proposed in the base funding case:

1. A lower divertor cryo-pump.
2. A 1MW ECH/EBW system.
3. An improved non-axisymmetric control coil set.

Of these, the lower divertor cryo-pump is, by far, the most important. NSTX currently uses Lithium as an expedient to maintain density control, to control impurities, and to recover quickly from vents. However, this technique is clearly not feasible in ITER (or in other more advanced CT devices). The installation of the cryo-pump will hopefully allow the NSTX team to discontinue their reliance on Lithium, and to adopt more conventional density control techniques.

The second most important upgrade is the improved non-axisymmetric control coil set. This will enable NSTX-U to extend its already superlative research into the physics and control of the resistive wall mode, neoclassical viscosity, and the neoclassical tearing mode.

The proposed ECH/EBW system is primarily needed to achieve non-inductive start-up. This goal is has little scientific or technical merit for CTs. Moreover, ECH/EBW heating systems are notoriously manpower intensive, and it is not clear where the required additional effort would come from. Hence, this upgrade is much less important than the other two

Detailed Comments on Each Topical Area

Macroscopic Stability

In the topical area of macroscopic stability, the NSTX team is in many ways leading the world effort on resistive wall physics, active and passive stability and control. Efforts to compare theory and experiment for linear instability onset are quite impressive since it necessitates the inclusion of non-MHD physics in the fundamental model. A quibble would be that this area could benefit from interaction with the extended MHD community to address nonlinear physics issues. In particular, understanding the interaction of MHD modes, external 3-D coils, rotation physics, (N)TM physics, etc. requires some understanding of their nonlinear interaction. This is an area where the M3D and NIMROD projects could play a fruitful role.

Non-axisymmetric control coils (NCC) will greatly enhance physics studies and control. A more complete set of theory/modeling studies to understand how the added coils can affect various physics studies are encouraged. The addition of NCC has the potential to impact RWM physics, turbulent transport, particle/heat loading, rotation physics, fast ion properties, disruptivity, etc. and may directly be required to address the highest priority goals of NSTX-U.

Disruptions are an enormously important topic for the magnetic confinement community and NSTX-U is in a position to address a number of important aspects of this area. Emphasis on disruption avoidance and mitigation is desired, but NSTX-U can also address some of the basic disruption physics properties as well. Here is an area where working with the extended MHD groups and more broadly the US theory community could also provide some clarity to the proposed research in disruption physics.

Transport and Turbulence

With the proposed upgrades, NSTX-U will provide access to an array of operational parameters using a set of heat/particle/rotation sources. With the emphasis on extremes in plasma shaping, beta and collisionality, NSTX-U will provide a somewhat unique set of results that should provide an enticing challenge to the theory/simulation community. The long-term goal here should be predictive capability particular in plasma regimes of interest to the FNSF prospects.

With the excellent set of diagnostics tools (both equilibrium and fluctuations) and access to multiple advanced simulation codes, the NSTX-U team has an excellent opportunity to advance this primary goal of establishing a predictive capability through verification and validation. The NSTX-U team is addressing the validation issue on a broad front and should set the standard for treating this issue in fusion plasmas. The team should not be satisfied with implementing a conventional qualitative validation approach. While the proposal only use the term “validation”, it is critically important that the NSTX group work to improve validation science by developing quantitative validation metrics that take into account both the limitations of the measurements and the simulation codes. Experiment-model comparisons in magnetic confinement fusion have not yet satisfied the requirements of validation. The widely followed practice of comparing a model result with experiment and declaring agreement “reasonable” or not, does not constitute validation because it is only a qualitative assessment. The primacy hierarchy of the measurements must be properly treated to deal with integration of measurable quantities, as well as sensitivity analysis which will assess how model output is apportioned to different sources of variation. The use of validation metrics for individual measurements must be extended to multiple measurements with provisions for primacy hierarchy and sensitivity.

Fluctuation measurements are primarily limited to density fluctuations (frequency, wavenumber 2-D, spatial distribution, temporal evolution) by interferometry, scattering, BES, and reflectometry. Plans to develop/attempt internal measurement of magnetic fluctuations via Faraday-effect polarimetry will be implemented and represent an important new capability. Whether this is sufficient to validate codes is not clear. No transformative measurements to determine multi-field quantities like the fluctuation-induced fluxes (particle, heat, momentum, etc.) are proposed. Despite limitations, significant progress can be made and this activity is well worth pursuing.

One of the crucial issues for STs generally is the role of electron thermal transport. An emphasis on electron physics is appropriate noting the anticipated high beta, low collisionality physics to be studied. There are a large number of fluctuation diagnostics that are focused primarily on density fluctuations. It would be nice to have measurements of electron temperature fluctuations to more directly address the electron thermal transport problem. Nonetheless, it is clear NSTX-U can make significant progress in this area. More broadly, since NSTX-U will be operating in regimes not obtainable by the conventional tokamak configurations, it can provide information of general scientific interest to the plasma confinement community.

Boundary Physics

Research in the boundary physics topical area is organized around three major thrusts: (1) "Characterize, control, and optimize the H-mode pedestal performance, transport, and stability," (2) "Control divertor heat fluxes with a combination of innovative and proven techniques," and (3) "Compare the sustainability of particle exhaust via Lithium pumping and cryo-pumping, for density, impurity, and Z_{eff} control consistent with integrated scenarios." These thrusts are appropriate as they are the most critical areas in boundary physics for the advancement of NSTX-U's mission and for magnetic fusion in general.

(1) "Characterize, control, and optimize the H-mode pedestal performance, transport, and stability"

The research program is strong in this area, with plans to address a number of key physics questions towards developing a first-principles understanding of the pedestal. The physics team is well qualified and the proposed research will likely lead to fundamental advances. Connections to theory and numerical modeling are strong.

L-H Transition Physics – A rich program of exploration is proposed, making good use of existing and new/upgraded diagnostics (GPI, BES, microwave scattering, reflectometry, polarimetry), the higher power and fields available in NSTX-U, and the control knobs (x-point geometry, Lithium, cryo-pump, 3D fields).

Pedestal Structure and Edge Localized Mode Physics – The research plan is mature in this area, building upon the extensive progress that the world community has been made in recent years on pedestal physics (PBM/EPED framework), recent comparisons of fluctuation measurements in NSTX to turbulence codes and the expertise of researchers at NSTX. Lithium has turned out to be a very interesting control knob, affecting the pedestal in a much more subtle way than originally envisioned, i.e., affecting the microturbulence via profiles changes rather than changing the convected power flows associated with neutral recycling though the boundary. This tool represents a unique opportunity to unfold key physics that controls the pedestal. Other control knobs that will be appropriately exploited are 3D fields and cryo-pumping. The scaling of the pedestal width and its connection with KBM or other physics is a key area that NSTX-U will contribute. NSTX-U will have T-CHERS and P-CHERS diagnostics to measure E_r, although the role of E_r in pedestal formation was not discussed in the plans.

Active control of ELMs and pedestal on NSTX – This is another rich area. The effect of 3D fields on ELM stability will be a particularly interesting area of study. Lithium granule injection, Lithium evaporation and vertical kicks are important tools to explore to affect ELMS, as are exploring regimes that are inherently ELM-free.

(2) "Control divertor heat fluxes with a combination of innovative and proven techniques"
This is a critical area of study for the success of magnetic fusion. The research program is organized around *Edge/SOL physics* and *Divertor physics*. Important physics topics are identified and plans to address them are stated, although some of the specific details of how model-experiment comparisons will be performed need to be developed. The physics team has very good expertise in this area and the proposed research will likely lead to fundamental advances.

Edge/SOL physics – Important topics in this area include: scaling of the heat flux power channel width and the underlying physics, understanding the effect of Lithium conditioning on heat flux width, impurity transport and migration, blob formation and motion, effect of divertor topology, effect of 3D fields, effect of atomic physics. The research program outlines a very strong emphasis of modeling activity: XGC study of Goldston heuristic drift (HD) model, self-consistent modeling of turbulence (BOUT++) coupled to UEDGE of SOLPS, SOLPS & UEDGE for edge transport and PSI modeling, impurity transport with BOUT++ and XGC1. While this modeling activity is important, NSTX-U is primarily an experimental program and the primary focus should be on developing a comprehensive description of boundary plasma phenomena such that physics models can be confronted and tested. Specific tests of models should be identified, such as the plans to measure flow speeds at the outer midplane and in the divertor to test the HD model. With regard to developing a physics-based model of the heat flux power channel width scaling, a systematic measurement of midplane and divertor density and temperature profiles along with divertor heat flux profiles under a variety of conditions would be very useful, but is not explicitly mentioned in the plan. The overall goal in this area is clearly stated: *"An outcome of this research program should be a theoretical model for edge/SOL turbulence which has been validated by NSTX-U data. This model can then be applied to predict the SOL parameters and SOL heat/particle width at the divertor plates of future devices such as ITER or FNSF."* Yet, the specific means to accomplish this goal requires the development of well-designed experiments. It is not likely that an all-encompassing theoretical model for edge/SOL turbulence will be identified. Rather, smaller steps along that pathway need to be made, such as testing model predictions/trends and/or at least identifying the dominant physics that must be included in models. Experiments on NSTX-U can clearly play this role.

Divertor physics – The program plan recognizes the challenge of developing dissipative divertor operation in low collisionality regimes. An appropriate plan is proposed for divertor/SOL transport: understand basic SOL and divertor power balance, capture this in 2D modeling codes, and model SOL turbulence, if warranted, by the experimental data and theory developments. The divertor Thomson system is not included in the baseline budget scenario, yet this tool will likely be required unfold divertor physics on NSTX-U. The principle focus is on developing heat flux mitigation strategies. These involve the use of snowflake divertor topologies (SF) and radiation in both the divertor and mantle regions. The planned program makes good use of prior experience with SF in NSTX and the expertise of the experimental team. Modeling work has been done to provide guidance for producing SF in a double-null configuration and in projecting towards possible reductions in peak heat fluxes. Interaction with planned cryo-pumping system is also being considered. SF will enable interesting physics to be explored: effect on edge stability, confinement, LH thresholds. Appropriate experiment-modeling comparisons are planned. Feedback control of radiative divertor operation is targeted in the five year plan with initial focus on open loop impurity seeding experiments. The program plan makes use of prior experience in NSTX and scoping studies with UEDGE.

(3) "Compare the sustainability of particle exhaust via Lithium pumping and cryo-pumping, for density, impurity, and Z_{eff} control consistent with integrated scenarios"
This is a critical research thrust for NSTX-U operations. An important new tool is the proposed cryo-pumping system. The role that it is expected to play compared to the NSTX experience with Lithium pumping is appropriately addressed. The primary motivation for the cryo-pump is to provide particle removal while not suppressing ELMs – something that the Lithium is not able to do. Initial scoping studies indicate that the cryo-pump should be able to maintain reduced core plasma densities while handing the beam fueling load. Projecting from experience in NSTX, Lithium pumping can be maintained for time constants on the order of 20 seconds and is therefore expected to last for the 5-10 second pulses on NSTX-U. There is a reasonable chance that some combination of Lithium conditioning plus cryo-pump operation will allow NSTX-U to attain its goal of sustaining particle exhaust while not eliminating ELMs. If successful, then the additional benefit of impurity control via ELMs would also follow.

Materials and Plasma Facing Components

Research in the boundary physics topical area is organized around three major thrusts: (1) "Understand Lithium surface-science for long-pulse PFCs", (2) "Unravel the physics of tokamak induced material migration and evolution" and (3) "Establish the science of continuously vapor-shielded plasma-facing components." PPPL scientists and collaborators are recognized experts in Lithium delivery systems for wall conditioning and have demonstrated the utility of Lithium for improving plasma performance. However, as discussed in the five year plan, the underlying surface science has not been studied in sufficient detail until recently. The five year plan puts appropriate emphasis on making such fundamental measurements to advance the science of Lithium-plasma-PFC interactions.

(1) "Understand Lithium surface-science for long-pulse PFCs"
The primary goals are to understand the roles of boron, oxygen and carbon on affecting coating performance and to assess the performance of using Lithium on high-Z substrates. The proposed methods to accomplish this are reasonable and straightforward. The plan involves a comparison of discharge performance as wall conditions are systematically changed from one campaign to the next: boronization campaign followed by controlled Li introduction; no boronization followed by identical Li introduction; surface interactions of Li on high-Z studied in divertor region. The new Material Analysis Particle Probe (MAPP) is a key component of this research, providing in-situ analysis of surface chemistry. The Surface Science and Technology Laboratory and LTX experiment at PPPL are also important assets.

There is a new understanding that vacuum conditions and contaminants largely determine the efficacy of Lithium as a hydrogen getter. Near-surface chemistry conditions controls plasma-surface interaction; oxidation occurs rapidly under typical vacuum conditions. Thus, fully flowing Lithium, better vacuum conditions, and/or complete removal of carbon contaminants may be required to realize low recycling (R < 0.5) conditions with Lithium. This realization is largely driving the NSTX-U plan to transition towards Lithium on high-Z substrate surfaces and to pursue a possible Li vapor-shielding concept.

(2) "Unravel the physics of tokamak induced material migration and evolution"
The plan calls for an upgraded set of material erosion/redeposition diagnostics to be installed in NSTX-U, with specific locations determined by modeling. Wall erosion would be monitored over a range of plasma conditions. Material transport would be measured as a function of divertor conditions and compared to simulation. Through collaboration with FOM-DIFFER, it is proposed that erosion/redeposition experiments would be performed on Magnum-PSI with Lithium and boron coatings to assess understanding of net erosion yields. Models would be constructed to try to compute material erosion and transport in NSTX-U such that it can be compared to measurements. This plan is appropriate and could lead to new understandings. However, with regard to erosion, redeposition and global transport, it is a bit optimistic to think that meaningful model/measurement comparisons can be made, given the free parameters that are including in models. For example, cross-field plasma transport and parallel plasma flow patters are not yet modeled properly. While divertor probes can be used to specify plasma profiles there, midplane plasma conditions in the SOL are not measured in NSTX-U. Thus the plasma fluid models are poorly constrained. With regard to accessing erosion of the main-chamber wall, direct measurement of the charge exchange neutral fluxes would be most beneficial but is not included in the base plan.

(3) "Establish the science of continuously vapor-shielded plasma-facing components."
The five year plan identifies Lithium vapor-shielding as a mechanism that might be exploited to protect high-Z substrates operating at high temperatures. Experiments are proposed for Magnum-PSI to test these ideas, including an assessment of coating lifetimes, with an eye towards performing similar tests on NSTX-U. With the installation of high-Z tiles on NSTX-U, initial assessments of vapor shielding could be performed, using planned diagnostics (Langmuir probes, IR, surface TCs, bolometry). Under a full funding scenario, a flowing Lithium divertor module would be developed.

These plans for investigating Lithium vapor-shielding as a potential tool for mitigating PMI are appropriate and have the potential to make fundamental contributions to this science area. The research team is well qualified and the facilities are adequate to carry this research forward.

Energetic Particles

Energetic particle research is a strength of the NSTX-U program as the spherical torus is a device that is rich in energetic particle physics and the NSTX-U research team has vast experience in this area. With NSTX-U, PPPL will likely have the control expertise to demonstrate Alfvén Eigenmode (AE) control. The actuators include plasma shape, fueling, NBI and high harmonic fast waves (HHFW) for density and current density profile control, and NCC fields to vary plasma and mode rotation. In collaboration with theory, it would be valuable to develop a predictive capability for the stability of AEs and then include this capability into the control system to learn how to navigate high performance regimes and avoid excessive fast ion loss due to AEs. The 2nd NBI line will open up new physics in AE stability area as it will likely drive even more energetic particle modes and increase fast ion redistribution and losses, making AE stability and dynamics even more critical for NSTX-U than for NSTX. The tangential geometry of second NBI also broadens the energetic particle profile, allowing greater profile control. Additional control of the rotation profile with the NCC can also help control energetic particle modes.

Research in this area should focus on ST-specific issues such as examining the effects of these modes on fast-ion transport, and neutral beam driven rotation and current profiles. Consequently a modest effort and allocation of resources is appropriate. Impacts of this research on ITER will likely be minimal, yet this contribution would be welcome and could bring additional insights in some unexpected ways.

Wave Heating and Current Drive

RF, both HHFW and ECW/EBW (electron cyclotron wave/electron Bernstein wave), heating and current drive are important for non-inductive plasma current start-up and ramp-up, particularly for the intermediate stage to bridge the gap from CHI start-up to NBI + bootstrap (BS) ramp-up.

Experience on NSTX showed incompatibility of high power HHFW with heavy Lithium conditioning, but significant improvement was achieved with reduced edge density and higher toroidal field. Better HHFW current drive efficiency is expected on NSTX-U based on these results.

Importance of power loss in the SOL plasma is recognized, and physics understanding is being developed. Validation of upgraded codes will improve predictive capability, not only for NSTX-U but also for ITER and FNSF.

Given the essential need for non-inductive startup for FNSF-ST, acquisition of a 28 GHz gyrotron to provide capability for heating CHI plasmas to allow better absorption of HHFW, is important to the long-term program. Allocation of appropriate resources will be important.

Many important issues remain to be addressed. The NSTX-U team recognizes the needs for research and development and has incorporated these activities in the plan. As tools, RF heating and current drive will make significantly contributions to the high-level goals. However, there remains some skepticism that all those deliverables (e.g. elucidating the limiting issues and validating codes) can be achieved in that time frame. Non-linear physics and edge effects have been a major roadblock for efficient RF uses for many years, and it is unlikely that such significant progress can be accomplished in 3-4 years, with the proposed level of effort, including manpower (both for operations and physics studies) and proposed dedicated diagnostics.

Plasma Formation and Current Ramp-up

Non-inductive startup is a crucial issue for FNSF applications of the ST. Efforts on NSTX-U are centered on the use CHI for this goal with a smaller effort on the plasma gun approach. Integration going from non-inductive startup to high-performance sustained plasmas is a high level goal of NSTX-U. If achieved, this is a substantial step towards realizing a fusion mission for the ST.

The detailed physics of how CHI works is not a settled topic. This is an area where the research can benefit from close collaborations with the extended MHD projects. Beginning efforts are underway with NIMROD. Comparisons between NSTX-U and MHD simulations are crucial to this physics mission. It is clear that this must be done in order to scale the NSTX-U results to fusion applications.

The role of the plasma guns was not discussed at length. It is not clear how much emphasis this will be given in the upcoming NSTX-U research plan. Close collaboration with Pegasus colleagues is important to establishing the viability of the gun approach to non-inductive plasma startup. Noting the potential uncertainties with CHI, it is important for the ST program to develop alternative strategies for non-inductive startup.

The proposed plan is to heat the CHI start-up plasma by ECH before using HHFW to ramp up the plasma current further. Some experimental time should be invested in direct coupling of HHFW to CHI start-up plasma before 28 GHz ECH becomes available.
Start-up by outboard PF induction and ECH (this was tried briefly on NSTX, but with insufficient ECH power) and further ramp-up by HHFW is another start-up scenario worth trying.

Plasma Sustainment: Advanced Scenarios and Control

The upgrade project for NSTX-U is scheduled to be completed by the end of FY14, with plasma operations to begin in early FY15. It will be quite important to optimize the re-start plan in order to get back to scientific productivity as early as possible. Plans for commissioning the new systems provided by the upgrade are well developed and appropriate. However, at this juncture, they cover mostly the hardware plans (power supplies, coils, beams, etc). It is difficult to judge how much time (and run time) will need to be devoted to relearn how the tokamak plasmas "behave". One of the key aspects of that relearning curve is particle (fuel and impurity) control.

Nevertheless, the addition of fine-tuned capability for current and rotation profile control in NSTX-U should provide greater flexibility for developing high performance ST scenarios, and NSTX-U is expected to make major contributions in advanced scenario development.

Proposed Upgrades

The plans to install a divertor cryo-pump, a 28 GHz ECH system, and off-midplane non-axisymmetric control coils in base funding case are adequate.

The proposed additions of the flowing liquid Lithium divertor and divertor Thomson scattering diagnostic are desirable. Reassessment of the importance of the flowing Lithium divertor relative to other items covered under base funding is recommended.