October 22, 2001

Dr. Robert J. Goldston, Director
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Subject: Report of the 11th NSTX Program Advisory Committee Meeting – October 2001

Dear Rob:

The NSTX Program Advisory Committee (PAC) met at the Princeton Plasma Physics Laboratory on 4–5 October 2001 (agenda attached). Our activities at this eleventh meeting of the PAC focused on three areas in response to your charge to the committee (copy attached): (1) To comment on the report of the ST Theory Development Panel; (2) to comment on the FY 2002—2003 NSTX research plan; and (3) to advise you on the recently completed assessment of next step ST options. In addition, we received a status report on action items from our last meeting.

The PAC commends the NSTX team on the many exciting results they have achieved since our last meeting including: The achievement of 25% β_T; The routine use of HHFW for heating together with initial HHFW current drive studies; Characterization of β-limiting and pulse-length-limiting MHD activity; Excellent progress on transport studies — including ion temperature and rotation profiles; Further exploration of CHI current initiation and sustainment; and initial studies of boundary physics. Of particular interest are the hypothesis that the apparent ion heat pinch results from neutral beam induced MHD heating, studies of H-mode access requirements, and expansion of the NSTX operating space..

Charge #1 — The ST Theory Panel Report

Our first charge was to *"review the results of the ST Theory Panel's efforts and give advice on its preliminary recommendations."* We begin by commending both the NSTX program management for taking the initiative to set up the ST Theory Development Panel, and also the members of this panel (and especially its chairman) for their willingness to serve on it and to write an ST theory white paper on a fast-track time schedule. We divide our comments on the theory panel report into theory issues and programmatic issues.

KEY THEORY ISSUES:

1. Prioritization. The first charge to PAC-11 specifically asks whether the "high-priority opportunities for new ST theory [have been] appropriately identified."

We note that the charge to the Theory Development Panel apparently did not ask for prioritization. We also note that, nonetheless, the list of key issues implicitly signals that some degree of priority has been accorded to the issues that are highlighted within each topical area, by the mere fact of their having been listed. However, it assigns priorities neither among the topical areas nor among the issues in a given topical area. We suggest that it would be helpful for the Panel to do the latter, at least to some extent, if possible.

2. Generic versus specific. The list does not clearly identify which of the issues are generic theory issues and which are specific to ST parameter regimes. The full version of the Panel's report will presumably address this.

The Panel's list is categorized by topical areas, which is eminently reasonable. Their further subdividing each area into programmatic issues, theory challenges, and experimental mysteries is also helpful. However, another organizing principle would be in terms of generic physics issues. These latter categories would cut across the topical area categories. We suggest that the Panel might use both sets of categories and arrange the key ST issues in a matrix. Indeed when the PAC asked about holes in the NSTX theory program, it was interesting that the response was largely given in terms of generic physics issues: viz., large gyroradius, rotation, helicity injection and reconnection, electron transport, and self-consistency (i.e., nonlinear physics). Generic issues have appeal to the broader magnetic fusion science community. In particular, university groups work on generic theory and then apply it to specific problems, like STs. Also, categorization in terms of generic issues would dovetail with corresponding SciDAC efforts.

3. Existing versus new theory tools. The list does not clearly identify whether the respective issues will require (1) access to existing, well-established codes, (2) modification of theories and codes for ST-specific parameter regimes and geometry, or (3) development of new codes and theory for future needs. We recommend that the Panel report do so.

4. "Mysteries". At least one PAC member felt that some of the experimental issues characterized as "mysteries" are really not. We recommend that such input be provided to the ST Theory Panel. Similarly we recommend that the Panel encourage input from scientists in the general MFE community.

The list seems to focus on current needs for theoretical analysis. We suggest that the list also indicate where totally new ideas might be needed, either now or in the future.

KEY PROGRAMMATIC ISSUES RELATED TO ST THEORY SUPPORT:

1. Personnel. Considering that NSTX is a national experiment (one of the Big Three) and the largest project at PPPL, the PAC is concerned that the theory effort specifically devoted to ST

physics is below critical mass. PPPL supports only two theory and two modeling FTEs for NSTX work. Moreover, these FTEs have been broadly spread over approximately 20 people. We were told that many scientists contribute about 10% of their effort. Hence the ratio of "FTEs to noses" is a small quantity (about on the order of a typical small parameter for a theory expansion). We concur with the recent decision to fund larger chunks of fewer people at PPPL. NSTX might also encourage OFES theory managers to increase the priority for ST theory funding and identify areas of greatest need.

2. Integration. The fractionation of effort mentioned previously can lead to many separate pieces of work that are not combined into a solid whole. We applaud the recent anointing of one person (J. Manickam) to serve as ST theory coordinator. Although he had already held that title, the position now will be expanded to include the duty of encouraging and coordinating ST theory efforts on a national scale, as well as proactively guiding the ST theory work locally at PPPL. This is a large responsibility, and we recommend that a proportionately increased fraction of his time be devoted to this task. Also, we request that the ST theory coordinator present a report at the next PAC meeting.

3. Student involvement. The ST Theory Panel presentation mentioned the need to discuss deep physics issues in order to stimulate students to work on ST problems. We agree with this point. We further observe that some of the code developmental tasks, which can be on one- to two-year time scales, might well be entrusted to students, particularly those who seek to build up computational skills.

4. SciDAC linkage. It may be possible for STs in general and the NSTX in particular to leverage off SciDAC initiatives on microturbulence, macrostability, reconnection, and wave-particle interactions. We recommend that the ST theory coordinator seek to generate interest by these groups in applications to the key ST issues as listed in the White Paper report.

EXAMPLES OF SUCCESS. The PAC suggests that the White Paper, in addition to highlighting key outstanding ST physics issues, cite several instances of successful theoretical-computational-experimental interactions. Recent examples might be: (1) neoclassical tearing modes, (2) electron Bernstein waves, and (3) compressional Alfvén eigenmodes. Of course it should not be forgotten that the spherical torus concept itself was the result of MHD predictions.

UNIFIED ST RESULTS MEETING. In presenting a preliminary summary of the work of the ST Theory Panel, its chairman mentioned the idea to have the NSTX Results Meeting expanded to include all ST experiments in the US, possibly along with invited talks from abroad. The PAC appreciates the rationale for this idea. At the same time it advises care in not placing unreasonable expectations on small ST groups at such a meeting and also in not supplanting the existing international ST conference. A unified showing at the APS/DPP meeting is a related option.

MARKETING THE PRODUCT. The PAC is confident that the ST Theory Panel will produce an excellent White Paper. Equally important, however, will be (1) how to report this report, (2) where to publish it, and (3) how to promote its conclusions. One suggestion is that the Panel chairman—or, perhaps even better, the ST theory coordinator—be provided a mandate to

promote it, e.g., through a special talk at the Sherwood Meeting, a talk at OFES, a talk at the annual Budget Planning Meeting, etc. Having the theory coordinator organize an ST theory workshop (US-JA-EU) is also a suggestion.

Anticipating that the ST White Paper will be successful, the PAC would encourage that it be updated in a couple of years, presumably by a new panel.

Charge # 2 — FY 2002-2003 NSTX RESEARCH PROGRAM

Our second charge related to the FY 2002-2003 NSTX research program, asking **"***Which among the new opportunities identified should receive emphasis during the upcoming campaign in achieving the FY-2002 research milestones? Should the balance and emphasis among the FY 2002-2003 research milestones be adjusted as a result, while maintaining a proper balance between the inductive and the non-inductive research maintained?***"**

The NSTX Project Team has outlined a broad research program that generally continues the research priorities established for the FY2001 experimental campaign. The PAC generally agrees with the priorities outlined for FY2002, which places most emphasis on HHFW experiments. We are glad to see the restoration of time devoted to boundary physics research. We note that the research plan contains enough contingency days to allow shifting priority to any of the other four research topics should results warrant, however we continue our concern that this plan has even fewer run days than last year's.

The members of the PAC are pleased that the research team increased the time spent on HHFW experiments in FY01 and recognize the progress made in developing HHFW as a reliable research tool, as evidenced by rf-generated H-mode and high-T_e operation.

The PAC recognizes that a number of trade-offs exist between pushing forward with experiments on key research topics such as CHI or HHFW-CD, and increasing hardware and diagnostic capabilities to obtain more scientifically valuable results. We believe that the present program properly balances these tensions, but we are concerned that the current drive experiments may be of limited value until the MSE current profile diagnostic comes on line.

Extending performance beyond the no-wall • limits is a critical research goal for the ST. We are pleased that the project has continued to pursue its plan to first characterize and then to apply active control to limiting MHD modes. We support the attention now being given to correcting the observed PF5 error fields.

The PAC encourages the NSTX group to carry out more routine and more complete analysis for the routine ST shot (e.g., to routinely carry out data analysis to at least the TRANS level). We also support the plans for beginning scaling experiments comparing NSTX confinement and H-mode thresholds with DIII-D and other tokamaks, but suggest this activity should first be focused on working with the MAST team to develop an appropriate ST transport and confinement database.

We agree with the plan to increase time for CHI experiments and explore coupling with RF and OH drive, but feel that the project should more clearly define the physics and diagnostic requirements needed for developing this non-inductive startup tool. In parallel, the group should work with the theory community to apply or develop predictive models for CHI. We are pleased

to see work beginning on alternative non-inductive current drive techniques such as EBW, and support continued effort in this area.

The NSTX team needs to continue their efforts to understand the limits on NSTX performance; i.e., define the modes or events that limit H-mode operation, beta, and confinement. Such an understanding may help in setting program priorities. We further encourage the group to focus more of the operations on the high performance regimes already obtained to better illustrate the advantages of the ST concept.

Charge # 3 — Next Step ST Options

At our 10th meeting, the PAC invited the NSTX Team to take 6 months to review its strategy relative to a burning plasma experiment, presenting the result of this review at the next PAC meeting. The NSTX team has completed an initial assessment of the options of the next step ST devices and the PAC was charged to review this on-going assessment and advise on its direction and emphasis. In particular, we have been asked *"Is the present scope of assessment of next step ST options appropriate? Are there additional options or modifications of the present options, which should be given emphasis?"*

The NSTX team has done a good job in responding to the request from the PAC that it review its long term strategy relative to a Burning Plasma experiment. The result has taken the form of a first cut at defining a long-term vision of the ST program. The NSTX team's basic approach was to use the ARIES-ST design point as a logical vision of the long-term goal to drive large ST experiments. Based on that, they presented a single design point, NSST (for Next Step ST), which attempts to reach the conditions relevant to ARIES-ST under the constraints of available site credits at the PPPL site.

In considering the response of the NSTX group to this charge, the committee evolved some questions and findings, which in turn suggest some action items for the NSTX group and all interested parties of the ST community.

FINDINGS/QUESTIONS AND ACTION ITEMS

1. Is this a reasonable vision to guide large-scale ST research? The committee finds that this is a reasonable vision of the successful development of the ST approach to fusion energy, and as such provides a good model to influence the present and future directions of the NSTX and ST programs. Positioning NSST as an intermediate point between the present ST programs, especially NSTX and MAST, and the long-term goal embodied by ARIES-ST is a good first estimate of an attractive ST next-step. However, this is an initial thrust in this direction, and a considerable amount of new information from the present program, such as information on confinement and transport properties, H-mode thresholds, MHD and fast-particle interactions, etc., will be required to move this design forward to a more concrete plan.

Pointing the mainline ST program in a direction aligned with the ARIES-ST reactor concept is, of course, quite programmatically directed towards fusion energy development. It is also very

important that the ST community continue working to identify and explore broader scientific interests in ST physics, both those relevant to fusion science and those with more general scientific interest. Examples of the latter may include unity-beta plasmas and unique MHD behavior such as the compressional Alfvén modes which may be of particular interest to the astrophysics community.

The committee urges the NSTX group to promote engagement of the rest of the ST community in expanding this into a national vision for the ST research line.

2. Is the present set of constraints used to define this next step option for ST's appropriate? The committee agrees with the NSTX group that, given the present level of maturity of understanding of ST physics, the appropriate next step should most likely be at the Performance Extension level of development. This represents a substantial and reasonably aggressively large step from where the present ST program is and where it can reasonably be expected to be after successful execution of present experimental programs in this field. The committee supports the effort to push up to the limits of site credits at PPPL in developing an initial concept of an NSST. It is encouraging to note that, at first consideration, a significant and relevant experiment may fit within these constraints. The committee also notes the need for the national ST community to keep a constant watch on developments in the international community so that any eventual design under consideration fits appropriately into a world ST program.

3. Do any considerations of possible NSST options suggest changes in the direction of the NSTX program? At such an early stage in consideration of NSST options, it is premature to call for significant changes in the NSTX program. Nevertheless, focusing the NSTX program within the context of this longer range vision for ST fusion reactor development does provide a conceptual framework in which various aspects and priorities can be evaluated. The committee notes that one obvious area of concern or interest that arises is whether NSST considerations may motivate the pursuit of a long-pulse startup experimental phase in the NSTX program. It is not clear that a relevant or compelling experiment can be pursued at the NSTX level of capability, but it is worth considering. Hence, the committee recommends the following

Action Item: *The NSTX group should pursue a modest-scale examination of the needs for and implications of pursuing a slow-start (ARIES-ST relevant) experiment phase.*

Given the vision of the long-term goals of the ST research and development program, there is confusion about the plans and philosophy behind present and future plans for centerstack upgrades in NSTX. There is a present need to pursue noninductive current drive experiments on NSTX, but it may be that a modest centerstack upgrade will be useful or even necessary to fulfill the present program through FY04 or FY06. Some advantages that are noted are increased volt-seconds to allow better single-swing operation and/or increases in the magnitude or pulse length of the toroidal field system. Given the discussion of these issues by the NSTX group in past PAC meetings, and the evolving focus on the long-term program vision, the committee requests an additional

Action Item: *The NSTX program should further define the mission and hardware options and plans for NSTX, to be consistent with this evolving ST vision.*

4. Does the immature state of knowledge about confinement in STs make this discussion moot for now? This question was posed in the course of committee discussions, especially because the issue of confinement properties and some predictive capability for projecting the confinement properties of future ST's appears critical to considerations of NSST concepts and mission. The consensus of the committee is that just-developing information on ST confinement is not a reason to defer discussions of NSST, but rather provides added incentive to the ST program for confinement studies. Since there is some concern that standard tokamak confinement scaling relations may not be relevant to ST parameters, there is considerable interest in collecting, even if in a preliminary fashion, as much information on ST confinement behavior as available from the present generation of experiments. This effort, in combination with tests of models based on specific transport theories, should provide a picture of confinement properties of ST's which can be used for consideration of future directions along this line. To that end, the committee requests the following

Action Item: *Confinement results from NSTX and MAST, as well as from smaller devices (e.g., Globus, START, Pegasus, etc.) as appropriate, should be combined to form a preliminary ST confinement database. While this database and any resultant scaling may be ST-specific for now, it would be useful to compare these results on a uniform basis to existing large-tokamak scaling models. In addition, the NSTX group should continue its efforts to work with the theory community to come up with appropriate theory-based models which can be evaluated in present in future devices.*

5. Given these initial considerations for NSST, what should the ST community do about Snowmass? Positioning NSST at the Performance Extension level indicates that it would not directly compete with any initiative for a Burning Plasma experiment in the near term. Nevertheless, discussion of such a large endeavor necessarily is coupled to interests in Burning Plasma experiments and the overall direction of the U.S. fusion science program. Even the conceptual NSST discussed here contains DT capability and as such will address some issues relevant to alpha particle physics in the ST geometry.

In addition, there exist suggestions that the appropriate next step in the U.S. fusion program should be a larger ST. The ST line has been promoted as a more efficient and economical means of achieving fusion plasma conditions, and this naturally gives rise to discussions of waiting for further development along this line to decide on the next major step in the U.S. program beyond Performance Extension facilities.

Within this context, it is important for the fusion program that the NSTX community participate extensively in the upcoming Snowmass meeting next summer. The NSTX group should coordinate with the ICC session convenors to optimize the input from the ST community on its potential to contribute to understanding Burning Plasmas and/or how a tokamak BP experiment might help expedite development of a future reactor prototype based on the ST approach.

The committee recommends that the NSTX/NSST groups should approach the Snowmass meeting with a goal of describing the vision for future development of the ST concept. This

should include a basic concept and operating point for a ST-next-step. Basic cost and schedule estimates should also be developed and presented to the community discussion. There should be a discussion on how a conceptual plan for NSST fits into an overall community strategy for pursuing a Burning Plasma experiment. Finally, the argument for waiting for the ST line to develop before moving forward to a BP experiment should be presented, including any pros and cons as appropriate.

The committee understands that this requested effort is necessarily based on a quite preliminary concept, and that there are no dedicated resources to support this preparation for Snowmass. In addition, much of the relevant physics have yet to be tested in NSTX and MAST. Hence, this work will have to be done on a best-effort basis. It is not our intent to raise unrealistic expectations on the level of detailed work which can be done. Nevertheless, addressing a vision for future ST development and putting it in the context of a program with a Burning Plasma experiment would both provide a valuable service to the entire fusion community, and help refine and promote the directions of large ST research.

In closing, we express our congratulation to the NSTX National Research Team for their very successful completion of the ’01 experimental campaign.

Sincerely yours,

William M. Nevins, Chairman
for the NSTX Program
Advisory Committee

National Spherical Torus Experiment
Program Advisory Committee
11th Meeting

Agenda

Princeton Plasma Physics Laboratory
Conference Room LSB-318
October 4-5, 2001

Thursday, October 4, 2001

8:30	Coffee & Donuts	
9:00	PAC Executive Session	
9:30	Goldston	Welcome and Charge to the PAC
9:35	Priester	Comments from DOE
9:40	Nevins	Agenda
I	ST Theory Development	
9:45	Callen	Preliminary Summary of Panel Report
(10:30	Coffee Break)	
11:30	Peng	Closure of PAC-10 Action Items
12:00	Lunch	
II.	NSTX Results and New Opportunities	
1:00	Synakowski	Summary of NSTX 2001 Research Results
3:00	Coffee Break	
3:15	Maingi	New Research Opportunities and Proposed Emphasis for 2002
4:45	PAC Caucus	
5:40	Adjourn	
6:30	PAC Party	

Friday, October 5, 2001

8:30	Coffee & Donuts	
III.	Next Step ST Discussion	
9:00	Ono	Status Report of Next Step ST Assessment
10:50	Coffee Break	
11:00	PAC Caucus	
12:00	Lunch	
1:00	PAC Caucus	
2:00	Nevins	Briefing for PPPL Director
3:00	Adjourn	

Charge to the Eleventh NSTX Program Advisory Committee Meeting, October 4-5, 2001

An ST Theory Panel has been established recently to engage the Theory leadership of the fusion community in identifying new opportunities for advancing magnetic fusion plasma theory, that have the potential for accelerating the progress of ST as well as magnetic fusion research in the near future. The Panel will have participated in the NSTX Results Review and met on September 21, 2001. I therefore ask The PAC to review the results of this Panel's efforts and advise me on its preliminary recommendations:

1) Are the key issues for theory of low-aspect-ratio and high-beta confinement system adequately addressed, and the high-priority opportunities for new ST theory appropriately identified?

The NSTX Team produced new and exciting results on a broad number of physics topics since the last PAC meeting (February 8-9, 2001). These results indicate new opportunities for important scientific progress during FY 2002. The first NSTX Results Review will have been carried out during September 19-20, 2001 to ascertain this progress and clarify the implications. I therefore ask the PAC to review and advise me on the potential benefits and impact of these results on the FY-2002 NSTX research program:

2) Which among the new opportunities identified should receive emphasis during the upcoming campaign in achieving the FY-2002 research milestones? Should the balance and emphasis among the FY 2002-2003 research milestones be adjusted as a result, while maintaining a proper balance between the inductive and the non-inductive research maintained?

Following its tenth meeting, the PAC "invited the NSTX Team to take 6 months to review its strategy relative to a burning plasma experiment, presenting the result of this review at the next PAC meeting." The Team began an initial assessment of the options of the next step ST devices as function of the potential outcome of the ST research in the upcoming years, and will be prepared to report on the status of this assessment. I therefore ask the PAC to review this on-going assessment and advise me on its direction and emphasis:

3) Is the present scope of assessment of next step ST options appropriate? Are there additional options or modifications of the present options, which should be given emphasis?