The PPPL Perspective on Ten Year Planning in Magnetic Fusion

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This white paper describes the framework through which PPPL constructs initiatives to contribute to the US program in magnetic fusion energy and the proposed major initiatives. This framework and the logic for choosing initiatives might also be useful for the panel's task for the whole US program. Below we describe the framework, the selection of initiatives, and an outlook for PPPL moving forward within budget constraints.

This paper is motivated, in part, by three questions posed by the chair of the strategic planning committee to PPPL: (1) Describe initiatives over the next 5, 10, 15 years that would enhance PPPL's leadership in ensuring ITER-experiment success, (2) Describe initiatives over the next 5, 10, 15 years that would inform FNSF configuration decision (AT, ST, stellarator), (3) If/when NSTX-U were to close, say in 5 years, 10 years, and 15 years, what would PPPL like to be involved in?

This paper is a general response to these questions, but cast in a way to inform the decision process for the US, not just for PPPL. This paper focuses on magnetic fusion. PPPL does extensive work and planning in discovery plasma science, separate from magnetic fusion, that is discussed elsewhere.

I Framework for Selecting Initiatives

Prior to selecting initiatives, we define a ten-year mission, strategic objectives to accomplish the mission, and criteria to select initiatives. Our process for selecting initiatives then proceeds through the following hierarchy:

Ten-year mission

Prepare for a breakout to an energy development program

In ten years we are ready to enter a program that includes fusion plasma science, engineering and technology, all proceeding at an accelerated pace

Strategic objectives

ITER preparation: Position the US to play a leading role
Steady- state, high performance: Establish the scientific basis
Plasma-material interface: Produce solutions suitable for entry into an energy development program
Structural materials and technology (fusion nuclear science): Produce solutions suitable for initial experiments in a fusion nuclear environment

Selection criteria for initiatives

We identify two criteria for selecting initiatives:

Importance to fusion
Opportunity for world leadership (and innovation)

Initiatives must satisfy both selection criteria. They must advance a solution for a critical problem for fusion *and* place the US at the world forefront in that research area. The judgment of placement at the world forefront must be done realistically. The world research and facility base outside the US is strong and expanding. US leadership will require innovation and ingenuity.

Selection of initiatives

Initiatives are then selected that satisfy the above criteria, objectives, and mission. The rest of this paper focuses on initiatives.

This approach can be equally applied to selection of initiatives for the US program. The important details of the enhanced program that would ensue *beyond* ten years (e.g., design choice and mission scope of an FNSF) are beyond the scope of the panel and not needed to answer the current charge.

II Initiatives

We present initiatives selected by PPPL in each of the four strategic objectives. We only introduce major initiatives and themes, with no intention of a comprehensive description of all the activities in the PPPL plan. We begin with preparations for a fusion nuclear science facility, which address the fourth strategic initiative (fusion nuclear science), followed by initiatives in the other three areas (plasma-material interface, steady-state, and ITER preparation). For each initiative we describe the activity and how it satisfies the two criteria of importance to fusion energy and world leadership.

Fusion Nuclear Science

Activity: NSTX-U research will establish the physics feasibility of the spherical tokamak (ST) as a candidate design for an FNSF. NSTX-U will determine whether the ramp-up, sustainment, confinement, and stability (at high beta) are sufficient for an FNSF. NSTX-U begins operation in early 2015. After five years of operation the first detailed results will inform the choice of aspect ratio for an FNSF. The second five year period will evolve to a program focused on establishing the solution for the plasma-material interface for an FNSF (and beyond). The NSTX-U activity is part of an integrated US ST program that includes the Pegasus and LTX experiments, and a coordinated theoretical component.

PPPL also contributes to the scoping of an FNSF through its participation in the national System Studies program, with responsibility for system code and physics analyses as well as leadership of the national team. The current three-year study will examine the range of possible missions and materials choices for an FNSF, establishing quantitative metrics for evaluating future FNSF proposals and identifying pre-FNSF R&D priorities.

Importance: The mission of an FNSF is an essential step toward fusion energy. The high cost of an FNSF is a serious obstacle to its realization. In keeping with its national responsibilities, the System Studies program will establish the criteria for an FNSF independent of any particular magnetic configuration. As a candidate for an FNSF, the spherical tokamak offers a distinct advantage in its potentially reduced size relative to larger aspect ratio approaches, reduced fusion power (for given neutron wall loading) and reduced tritium usage. The consequent reduction in cost that an ST offers could be a huge benefit in launching an FNSF and an accompanying research program. Without NSTX-U we will not have the information to make a design choice for the FNSF; with NSTX-U we will. Thus, its importance is clear. In addition, the unique fusion physics studies of NSTX-U (from non-inductive startup of tokamaks to electromagnetic turbulence to stability at high beta to high heat flux divertor studies) carries great importance beyond the FNSF application.

World leadership: As a facility to prepare for an ST FNSF, NSTX-U is the best in the world. The world ST program consists of NSTX-U, MAST (in England) and many smaller experiments. NSTX-U and MAST are facilities of similar capabilities. The programs on NSTX-U and MAST are highly complementary. NSTX-U focuses on high beta, high bootstrap current operation with snowflake divertor and liquid metal first wall solutions. MAST (and MAST-U) focuses on lower beta, lower bootstrap current, beam-driven current and super-X divertor operation. The EU fusion roadmap does not include an FNSF. Thus, the MAST program is not FNSF-focused in the manner of the NSTX-U program, and NSTX-U is the world leader in this mission without ambiguity.

The Plasma-Material Interface

Activity: Liquid metals provide a first wall boundary that is self-regenerating, non-eroding, capable of exhausting heat to the first wall through flow, and immune to neutron damage. Liquid lithium offers the additional advantage of improvement to confinement, a result of the decreased recycling of cold gas into the plasma. The combination of enabling higher heat flux and reducing the cold gas into the plasma has the potential to improve the performance of compact nuclear facilities ranging from FNSF to a power plant. PPPL aims to determine the feasibility of the liquid metal solution in ten years. This requires a comprehensive study – "from atoms to tokamaks" - that includes fundamental material science of liquid metals, development of liquid metal modules and flow techniques, studies in exploratory tokamaks and deployment in NSTX-U. The research program includes NSTX-U collaborators from multiple institutions and collaborators in liquid metal science and engineering.

Importance: Currently, there are two candidates for the first wall material: solid tungsten and liquid metal. Neither is yet known to work in a reactor. Tungsten is deployed in many tokamaks worldwide. It is unknown whether it will be appropriate for a reactor due to its brittleness, erosion, effect on plasma confinement, and neutron damage. Liquid metals have been much less studied than tungsten. Many issues remain to be addressed, especially the liquid-metal/plasma interaction and the engineering science of a flowing liquid metal wall. The challenge of the plasma-material interface is reflected in the statement in the EU roadmap document that a "reliable solution to the problem of heat exhaust is probably the main challenge towards the realisation of magnetic confinement fusion." Whether this is the main challenge is debatable, but it is inarguable that this is a challenge of major importance – a potential show-stopper. With only two solutions currently conceived, and neither yet established, it is exceedingly important to develop both solutions with equal emphasis worldwide.

World leadership: Around the world there are strong programs to develop solid tungsten, including materials science, testing on plasma test stands with high heat flux, and deployment in tokamaks. By contrast, research in liquid metals is only now emerging. The US has been the leader in this area through early and current work on liquid lithium. Motivated in part by early results in the US, research in liquid metals is beginning to expand. From its current starting position, if a comprehensive program in liquid metals were implemented, the US would be the world leader in an area which, if proven feasible, would be a game changer solution for a potential show stopper. No other nation operates the comprehensive program that we plan.

Steady-State, High Performance

Activity: NSTX-U is aimed to establish a steady-state scenario for the spherical tokamak. In addition, we propose to advance the stellarator as a solution to steady-state confinement. At PPPL the main stellarator activity would be centered on the QUASAR experiment, within a national program consisting of smaller stellarator experiments studying critical issues, theoretical optimization of the stellarator concept, and collaboration on the major stellarators abroad. The QUASAR facility is partially constructed, roughly at the half-complete stage (see Fig 1). Completion would require roughly $105M in construction costs (excluding heating and diagnostic systems). With international partnership (such as expressed by China, discussed below) construction costs for the US could be reduced to about $80M. Operational costs would be about $30M per year, if operated in parallel with NSTX-U. Initial assessments indicate that operating both machines concurrently requires roughly 1.5 times the cost of operating either experiment alone. The large cost reduction results from sharing staff (technical and research, in part) and infrastructure (e.g., power systems).

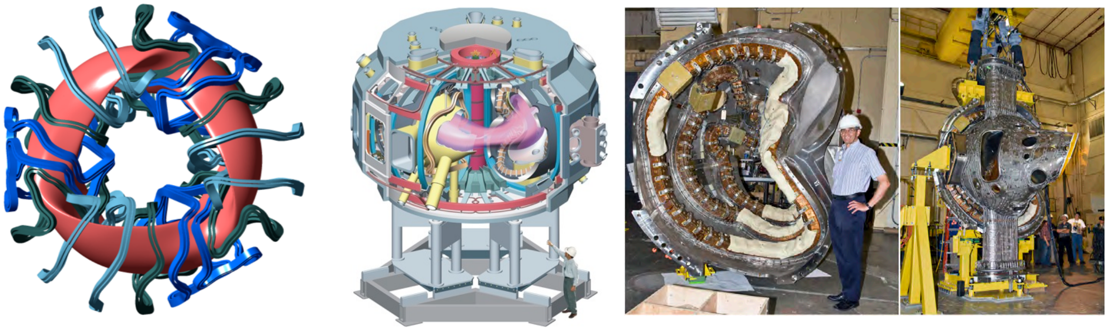

Fig 1: Computation of plasma shape (with magnets in blue), schematic of QUASAR when fully assembled, and photos of constructed components (magnets and vacuum chamber)

Importance: Steady-state operation at high-performance (high beta, disruption-free) is essential for economic fusion energy. There are two advanced solutions: the bootstrap-current-driven tokamak and the stellarator. The two solutions are highly complementary. The stellarator can be viewed as a form of an advanced tokamak – or a 3D tokamak. In particular, quasi-symmetric stellarators are strongly similar to the axisymmetric tokamak, except that driven current in the plasma is not needed.

Tokamak research has made remarkable progress in establishing bootstrap-current driven plasmas. However, the remaining challenge is large – to maintain the self-consistent state in which the pressure is high enough with an appropriate profile shape for stability and bootstrap current drive. A vivid illustration of the challenge is reflected in the ITER specifications. ITER is a power-plant scale facility which is predicted to operate at Q = 10 for about 5 minutes and Q = 5 for 50 minutes. These plasma durations are sufficient for ITER's mission and not a weakness of ITER. However, the Q = 10 operating mode does reflect the current physics data base. Another illustration is the recent EU DEMO study, which produces a conceptual design based exclusively on established physics data. The result is a pulsed tokamak. While it is debatable whether these projections are too conservative, it is unambiguous that achieving a disruption-free steady state is a major challenge. The stellarator offers a steady-state solution, free of disruptions, with high gain (due to the absence of auxiliary current-drive power consumption).

World leadership: NSTX-U is world-leading in establishing the ST steady-state scenario, as described earlier in its relation to FNSF. Stellarators are also an essential research element to provide steady-state plasmas. Quasi-axisymmetric stellarators (the QUASAR design) are an essential research element of the stellarator program for the size reduction that it offers (as well new physics understanding). There are two large stellarators in the world: LHD in Japan and W7-X in Germany (now just beginning engineering commissioning). Only W7-X is an "optimized" stellarator – one whose neoclassical confinement properties are favorable in the reactor regime. However, the W7-X design extrapolates to a very large reactor, with a major radius of 18 m or more. The QUASAR design extrapolates to reactors with major radii of about half that. This size reduction is a necessary path for a stellarator reactor. The QUASAR design also is complementary in physics design to the LHD and W7-X; its topological equivalence to the tokamak is unique.

Thus, despite the smaller size of QUASAR relative to LHD and W7-X, research on QUASAR will be unambiguously at the world forefront because of the uniqueness and importance of its configuration. International support for QUASAR is huge, as evidenced by written statements from the Chinese, German, and Japanese fusion program leaders. Dr. J. Li, director of the Institute for Plasma Physics of the Chinese Academy of Science writes "we would contribute components and staff…..to reduce the U.S. costs for NCSX [now called QUASAR]. Staff can include researchers, engineers, on-site assembly labor, etc." We estimate that this offer of in-kind contributions to QUASAR could reduce the construction cost by about 25%. The motivation for China to partner in QUASAR is simply to acquire the physics results that are critical for fusion. The scientific board of directors (ten members) of the Max-Planck Institute for Plasma Physics in Germany writes "QUASAR would, in our opinion, be the most innovative fusion experiment in the US since many years." Prof. H. Yamada, of Japan's National Institute for Fusion Science writes, "The concept of QUASAR is quite unique and the QUASAR can definitely explore unexplored horizon of fusion plasmas." The strong international support speaks to its potential role in the world fusion program.

The results from QUASAR are important to obtain in the near-term (within a decade). If it proved impossible to begin the next phase of QUASAR construction in about five years, then we would propose that the US seek an international partner to fund the construction and operate the facility abroad. The prospect of giving away such a large and unique US investment is clearly unattractive, but less unattractive than discarding the investment. The US would then have access to the results obtained on QUASAR abroad.

ITER Preparation

Activity: PPPL contributes centrally to ITER physics preparation through its entire research program: NSTX-U, collaborations on other tokamak facilities in the US and abroad, theory, and diagnostic development. NSTX-U contributes in two ways. First, it exploits properties of the ST that provide access to ITER-relevant physics more readily than in conventional aspect ratio tokamaks. Two examples are the study of energetic particle instabilities and electromagnetic turbulence – both of which are more accessible in the ST. Second, NSTX-U contributes in areas for which the aspect ratio is not relevant, such as disruption mitigation and plasma-material interface studies.

PPPL current activities and plans call for collaboration on key tokamak facilities worldwide, integrating those activities across facilities and with NSTX-U. PPPL also aspires to play a key role in a coordinated national program in integrated simulation of the complex fusion system – key to ITER experiments and fusion in general.

World leadership: US world leadership in ITER preparation is challenging due to the impressive arsenal of existing and new tokamak facilities abroad, including JET, ASDEX-U, EAST, KSTAR, West, JT-60SA and others. This set of facilities is strongly focused on ITER preparation and includes an ITER-like wall, possible DT operation (in JET), plasma cross-sections similar in shape to ITER, long pulse, and high performance. US tokamaks (DIII-D and CMod) also are important contributors to ITER preparation. NSTX-U makes unique contributions to ITER. However, it is not the world leader in this area, but contributes at the world forefront within this large group of major experiments.

Integrated simulation is an area for which a coordinated US program can be at the world forefront, building on existing strengths, if the program would begin soon. Otherwise, the opportunity will be overtaken by similar investments abroad.

III Comments on PPPL budget scenarios

One can ask the question what PPPL would propose for its activities under the condition of a flat institutional budget for ten years (this is not a specified constraint in the FESAC panel charge, but we pose it for discussion in analogy to the constraint applied to the whole program in the charge). We answer, very simplistically, through Fig 2 (left side) which treats only the major PPPL initiatives of NSTX-U, QUASAR, and the liquid metal program. The next five years includes initial operation of NSTX-U and a program in liquid metal research for the plasma boundary. We would propose to place a decision point in five years. At that time, NSTX-U progress and prospects would be assessed, along with a similar assessment of the other operating major tokamak facilities in the US (this decision point is also proposed in the white paper submitted by Fonck et al). At the five-year point, we would decide whether to continue NSTX-U research (and seek partners to operate QUASAR abroad) or to begin QUASAR construction (and begin a rampdown of NSTX-U operation). Continuing NSTX-U beyond five years would carry an enhanced focus on novel solutions to the plasma-material interface, deploying liquid metal surfaces and magnetic divertor designs such as the snowflake or x-divertor. NSTX-U would focus on the critical integration of steady-state, high performance plasmas with a plasma boundary solution aimed to be suitable for an FNSF. Alternatively, we could begin the QUASAR program which carries the world-leading contributions described above. On a strictly flat PPPL budget, it is not possible to operate both. The decision would be based on the results obtained from NSTX-U and the many factors derived from the status of the US and world program five years hence. Either decision would force the US to bypass a huge opportunity – full exploitation of NSTX-U or start of QUASAR. To terminate NSTX-U, the newest facility in the US, after only five years of operation (which includes 1.5 years of downtime for modifications), would be to terminate the most advanced spherical tokamak in the world well before it reaches its full potential. To donate QUASAR to another nation (or dispose of the hardware) will stunt the development of the stellarator path for fusion. In either scenario, in five years we would enhance the liquid metal research program to one which is sufficiently comprehensive to enable an assessment of liquid metals for fusion.

The only sensible plan – and a highly scientifically compelling plan - would be to run NSTX-U and QUASAR concurrently, as shown in Fig 2 (right side). This would be of huge physics benefit and highly cost effective by exploiting the available high bay area and shared power infrastructure and staff. PPPL as the lead national laboratory for fusion plasma physics is substantially underutilized. Operating NSTX-U and QUASAR would allow the US to run two front-runner facilities which would attract international users and contribute to a reinvigoration the US fusion program.

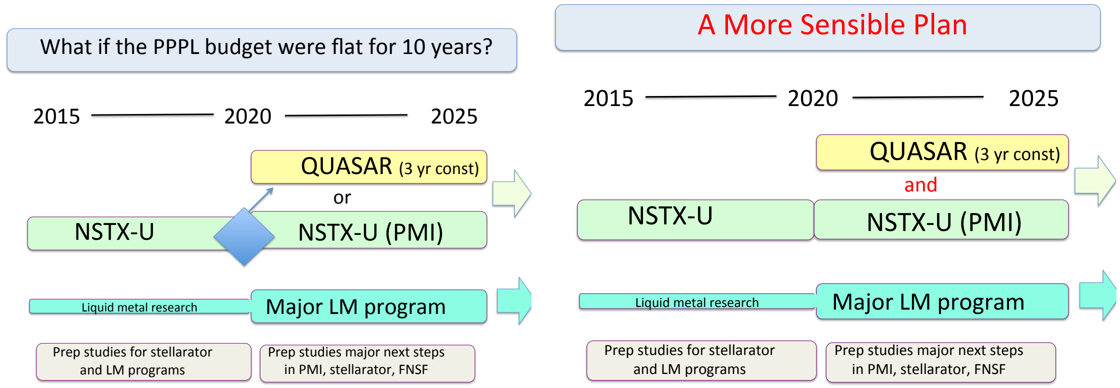

Fig 2: Simplified schematic of PPPL vision of major activities for a constant PPPL budget for ten years and for a more sensible, modestly expanded budget.

IV Summary

The US can contribute at the world forefront over the next decade if we evolve the program to accommodate new activities. This can be accomplished in an orderly manner, maintaining core capabilities and human resources as facilities change. In this white paper we outlined the framework that underlie our choice of proposed PPPL initiatives, and describe initiatives that would be unambiguously at the world forefront in areas critical to fusion energy.