Perspectives on a Constrained Fusion Ten-Year Fusion Program
(Comments on FESAC charge #2)

S. Prager, R. Goldston, J. Menard, H. Neilson, R. Wilson, M. Zarnstorff

Devising a US domestic research program that plays a key role worldwide on a budget budget that is flat for the next ten years at the FY 12 level is challenging. If that level were sustained indefinitely, the US would never arrive at fusion energy, nor even construct in the next decade a new research facility of a scale equal to that of the five superconducting facilities operating or under construction abroad. It will take careful planning to optimize progress and avoid slippage of the US to second tier status worldwide. This white paper describes an approach to constructing a program at this constrained budget level. Below we describe criteria for selection of research activities, examples of such activities, workforce issues, implications for the US fusion roadmap, and a sample program budget breakdown. This white paper is accompanied by other submittals from PPPL that make the case for particular opportunities in plasma facing components, stellarators, next-step planning activities, spherical tokamaks, and fusion simulation.

I Criteria for Research Activities

We recommend that two criteria be applied to selecting research priorities under such a constrained budget.

1. *Focus on world-leading research on important topics for fusion energy.*

The scientific issues to be resolved for commercial fusion energy require basic research and innovative solutions to numerous scientific challenges. The US should focus on original, important, selected, exciting activities for which it can lead or be at the world forefront. Only such activities should be incorporated within the DOE Office of Science. Work that has breakthrough potential should be emphasized. The US should avoid work that is incremental, or similar to, work that is done elsewhere but at a larger level. The one exception is research to maintain a core capability that is needed for the potential to breakout into a fusion development program, in accord with the second criterion below.

2. Include new research activities that are linked to ITER and fit within a US fusion roadmap to a demonstration power plant.

It is imperative that the US program be constructed so that it can contribute to ITER operation and benefit fully from ITER results. In addition, the program needs to maintain and develop the ability to break out into a fusion development program when the opportunity arises.

The program activities should be selected from the three topic categories that span the fusion challenge: confinement (high performance, steady- state, burning plasma physics), the plasma-material interface (including plasma facing components), and harnessing fusion power (surviving neutron fluxes, producing tritium and heat). The distribution of effort among the three categories should reflect the research opportunities according to the above two criteria.

II Examples of Research Activities for US Leadership within the Next Decade

Below we list example activities for which the US can possibly be at the world forefront in selected areas. The activities range in annual cost from about $10M to about $50M, and can form building blocks from which a program can be devised. The list is organized according to the three large scientific areas described above. We have also added a fourth category – planning for the US fusion future. The list is by no means exhaustive and not in priority order. It is meant to demonstrate an approach to forging an innovative program on a severely constrained budget. The list focuses on tools, assuming that the scientific justification is clear.

1. Plasma Confinement (high performance, steady-state, burning plasmas).
Fusion simulation program: Large-scale integrated simulation that couples different phenomena and regions of a fusion energy system is ripe for new advances, necessary for a full understanding of the system, and key to optimizing ITER operation. The Fusion Simulation Program, which has completed a two-year planning study, represents an example of how a coordinated national program could be at the world forefront in fusion computation. While many nations have programs in large-scale fusion computation, a program such as the FSP would be at the forefront and provide the US with a crucial tool in ITER operations.

Magnet development: Development of magnets capable of higher magnetic field (such as high Tc, superconductors, HTS), could alter the optimal scenarios for tokamaks and stellarators significantly, possibly leading to either smaller reactors or more reliable operation at lower values of beta. Development of HTS magnets is also motivated by the growing shortages and expense of helium. This is an area of historical strength in the US.

Tokamak experiment: Tokamak experiments in the US could prove critical if there is a focus on special features, such as novel divertors, novel plasma facing components, establishment of the physics feasibility of an FNSF (Fusion Nuclear Science Facility), or unique contributions to ITER. The most likely route to world-leading capabilities in about 5 – 10 years would be through upgrades of existing facilities.

Stellarator research: The stellarator could prove to be the most attractive route to fusion power due to its potential for steady-state, disruption-free, high gain plasma confinement. It is an essential complement to the worldwide tokamak research program. As a result of unique designs accessible to US experimentation, the US could be at the world forefront in stellarator research with a medium-scale experiment, complemented by theory and smaller experiments.

Exploratory fusion concepts: Exploratory confinement concepts, if proven feasible, offer potentially large advantages such as tritium-suppression through advanced fuels, compactness, and weak magnetic field. The scientific status, potential, next steps, and physics value of different ideas should be re-evaluated.

International collaboration on superconducting facilities: The long-pulse confinement facilities in Asia and the EU provide the US with opportunity for access critical physics associated with steady-state plasmas. Such collaboration should dominantly be carried out mainly with a "scientific balance of trade," with a nearly equal flow of scientific resources in both directions.

2. The plasma-material interface
New divertor geometries: New divertor geometries offer the possibility of significantly reducing heat flux to divertor plates and thereby reducing the first wall materials challenge. The super-X and snowflake divertor configurations were invented in the US, and the US can be at the lead in testing such configurations in confinement facilities.

Liquid metal first walls: Liquid metals are a potentially breakthrough solution to first wall materials challenge since they are self-regenerating against erosion, suffer no neutron damage, and have controllable heat removal. In addition. liquid lithium, as an absorbing material, provides improvement to plasma confinement. These advantages can be realized only if the substantial research challenges of implementation can be met. The US presently is at the world forefront in this area. With a program that includes fundamental materials science of liquid metals, test stand studies of liquid metal behavior in fusion relevant conditions, and testing in confinement facilities, the US can be the world leader.

Tungsten development: The development of tungsten can proceed through studies in test stands (described in the recent FESAC report on materials) and confinement facilities, and through the development of new tungsten alloys. Assessment of research programs elsewhere is needed to determine whether the US can be at the world forefront with realistic investments.

3. Harnessing fusion power
Modified accelerator-based neutron sources: With modifications to either SNS at ORNL or LANSCE at LANL fusion-relevant neutrons could be available for materials testing which is unavailable elsewhere.

Blanket studies: The US has been a leader in developing the dual coolant lead lithium (DCLL) blanket concept. With a focus on the DCLL concept, perhaps with testing in an ITER test blanket module, the US can acquire leadership in a blanket sub-area. An assessment of research programs elsewhere is necessary to determine potential for US competitiveness. This is also an area in which the US should maintain expertise so as to preserve capability to breakout into an energy development program.

4. Scoping studies for activities after 2020
Scoping/conceptual design studies: A national "next step options" activity should be established to scope various next large facility steps for the US, most notable an FNSF, but also significant but smaller steps such as a toroidal facility for plasma-material interface studies and a stellarator.

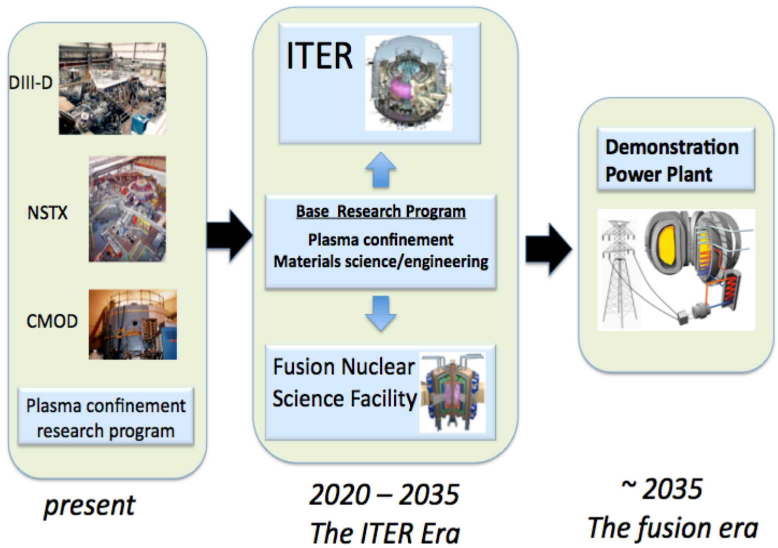
Roadmapping and socioeconomic studies: The fusion program should include an ongoing look-ahead study or a roadmap to a DEMO. Coupled to the above scoping studies for facilities roadmapping exercises should assess the technical risks and costs of various paths. Socioeconomic studies are also essential to understand fusion's role in the evolving larger energy and environmental context.

III Workforce issues

It is critical to maintain and evolve the US fusion workforce to sustain its excellence. As projects terminate and evolve, the changes should be implemented in a way that minimizes disruptions to the workforce. The core capabilities to retain and grow in the workforce should reflect the areas where the US can play a leadership role. Thus, as a top priority we should maintain our worldclass workforce in fusion plasma physics. We should also build expertise in materials and technology in selected areas where we can have significant impact.

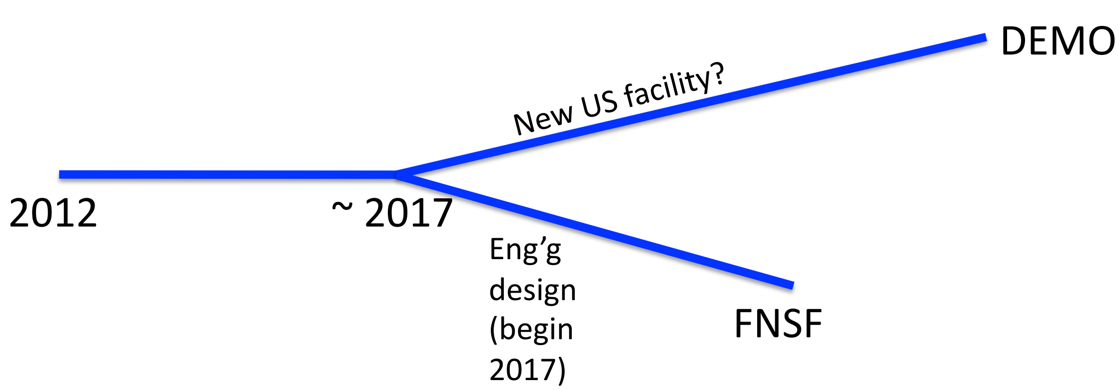
IV Effect of plan on US fusion roadmap

An FNSF is envisioned as a key element in the US roadmap to fusion, as an essential step to develop components and the integrated operation of a fusion energy system exposed to large neutron fluence. In the US a roadmap has been informally discussed in which an FNSF is operated roughly in parallel with ITER, leading to DEMO operation around 2035, as shown in the figure below. This is a much more aggressive program than that at present.



With a roughly flat budget for ten years the above roadmap is not feasible. The engineering design of an FNSF will require several hundred million dollars and is likely too expensive to accomplish in the next ten years on a flat budget. If the engineering design started in ten years (~ 2022), then an FNSF could be operational in about 2032. If FNSF results are needed for DEMO, that implies DEMO operation towards 2050. Such a date for DEMO operation is late. In that case, the US should consider going straight to DEMO, with the FNSF mission being accomplished in the first phase of DEMO, complemented by a strong and prior R&D program in fusion nuclear science. Such a plan is similar to that of other ITER parties. Part of the above roadmapping activities should be to assess the risks and tradeoffs of the FNSF-to-DEMO path vs the path without FNSF.

A possibly attractive path for the near-term would be to proceed for the next five years with research preparations for both FNFS and DEMO, developing the physics basis for the plasma configuration (AT, ST, stellarator) and the materials basis. In five years, a decision can be made to either proceed toward FNSF (possibly with international collaboration) if the US funding situation changes dramatically or proceed straight to DEMO. If the funding situation improves (and the fusion community should work hard to make the case for fusion), the engineering design of FNSF can proceed. If not, then the program can plan for the path to DEMO. This approach is illustrated below.



V A sample program

Given the above examples from section II, a sample OFES MFE research program can be devised, and is shown below. This is only meant as an indicator of a program that can fulfill the criteria of section I on an approximately flat budget. It is a program to which the US can evolve over about the next 8 years. The current three major facilities are performing at the world forefront and playing key programmatic roles in the fusion program. The sample budget below includes two confinement facilities (AT, ST, or stellarator). A smooth transition from the current facilities over the next 5 – 8 years to new facilities or upgraded in the case of tokamaks) is assumed.

Plasma confinement	$170M
Theory/fusion simulation program	
Magnet development	
~ 2 confinement facilities (AT, ST, stellarator)	
exploratory fusion concepts	
international collaboration	
The plasma-material interface	$40M
New divertor geometries	
Liquid metals	
Plasma test stands	
Harnessing fusion power	$30M
Modified accelerator neutron source	
Blanket studies (e.g., DCLL)	
Scoping studies	
Scoping/conceptual design for next steps	$15M
Roadmapping studies	
Socioeconomic studies	
Other (Program office, SBIR.....)	$20M
TOTAL	$275M

The budget slightly exceeds the requirements of charge #2, indicating how stringent is that requirement. At these budget levels, the US program cannot address all topics and must depend on collaboration with international partners for some knowledge and access to some needed facilities. If the FY 13 President's budget were to prevail (about a $40M - $50M reduction) one can see that the cut is of a magnitude equal to major components of the program – such as the entire plasma-material interface program, the theory/computation program, a major confinement facility, or magnet development plus exploratory concepts.