
**Implications and Lessons
from the 2007 Strategic Planning Activity and
Subsequent Events**

A Personal view

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Input to the 2014 FESAC Planning Panel

Motivation

- The 2007 report on priorities and opportunities is one of the foundational documents for the current activity.
 - As is the case now, that report was intended to inform the development of a strategic plan (sometime, real soon)
 - It is important to talk about the context of that activity and differences between the charges.
 - Moreover, the situation has changed over the last 7 years.
- Obviously this represents my personal views only – I don't speak for 2007 panel nor for the FESAC that ratified it.

Planning: We were only able to begin the process in 2007

- While our work was meant as an important contributor to a strategic plan, it was not, by itself, such a plan.
- We wanted that plan to be timely, broad and bold
 - ***Recommendation 1. A long-term strategic plan should be developed and implemented as soon as possible to begin addressing the gaps identified in this report.***
 - *Such a plan should include metrics to prioritize research areas, scientific milestones to judge the progress, and should identify means to educate and train a new generation of scientists.*
 - ***Recommendation 2. Such a strategic plan should recognize and address all scientific challenges of fusion energy including fusion engineering, materials sciences and plasma physics.***
 - ***Recommendation 3. A long-term strategic plan needs to include bold steps***
 - *The panel encourages the adoption of new initiatives or the construction of new facilities that are vital in filling the gaps identified in this report and that can hold their own in the international arena.*

Planning: How far can you go in 2014?

- The recent mandate from Congress is for a strategic plan for fusion energy. The current charge really focuses on a spending plan. This is unfortunate.
- A true strategic plan:
 - Begins with a well defined mission and vision
 - Defines the principles and metrics that guide decision making
 - Summarizes required program elements and deliverables
 - Identifies risks and risk mitigation strategies
 - Outlines a path to achieve the mission with well-defined decision points
 - Only then iterate on the budget profile.
- What is panel to do given this contradiction?
 - **My suggestion – go as far as you can in this orderly process and reiterate the recommendation for a follow-on, community-based planning effort**
 - **Don't close out options before the strategic planning is complete**

Boundary & Initial Conditions

For The 2007 and 2014 Efforts are Different

- The 2014 charge includes a much broader range of science and technology
- The 2007 charge instructed the panel to “assume success on ITER and related activities.”
 - This required assumptions about the ITER schedule and accomplishments of supporting research. (Burning plasma research is still 20 years away.)
 - It is not clear that these assumptions are appropriate or valid.
- 2014 charge excludes only ITER construction from consideration
 - What about activities that “support” ITER? Assumed “successfully completed” in 2007
 - Status and progress on supporting research for ITER has large overlap requirements for parallel and follow-on activities.
 - Current domestic funding has not been sufficient for any of these goals (ITER, ITER support and R&D for other fusion development activities).

The Plan Must Identify Risks and Develop Strategies to Mitigate Risk

- It is inevitable that any ambitious program will have to assume risks.
 - We have tended to shy away from this discussion, fearing that we will not be allowed to proceed if we acknowledge risks.
 - In fact, no one should allow us to proceed if we haven't thought through the risks
 - Risks come in all flavors - technical, managerial and budgetary
 - Ignoring risk invites failure, but...
- The tension is that we want the program to take bold initiatives, but not to be foolhardy
 - *“a program carried out so slowly and deliberately as to never make a wrong step may carry more risk than one which tries to move more boldly and accepts that it will make some mistakes and follow some blind paths...At some point delay is equivalent to failure as government and industry conclude that no solution will be forthcoming.”*

The Technical Gaps Remain (and have sharpened)

- Many gaps identified and **ALL** must be addressed and closed before we can take the step to Demo and commercialization.
- So how do we set priorities?, **What do we do first?**
- I suggest the following considerations:
 - Are there challenges that require dramatic progress to overcome obstacles to the achievement of practical fusion energy? (“**show-stoppers**”)
 - Are there opportunities for new technologies or new approaches that could change the landscape? (“**game changers**”)
 - When is information required for future critical decisions?
- **Given the need to address the long list of technical gaps, constrained budgets will inevitably lead to a stretched-out schedule.**
 - This is unfortunate but must be faced.

Gaps – Show stoppers and Game changers

- “Show-stoppers” need to be overcome first – there is no point in optimizing a design around a hopeless operating point.
 - Development of edge plasma solutions that meet PMI engineering, materials and fuel cycle (retention) requirements
 - More robust (esp. w.r.t. disruption avoidance and mitigation) approaches to steady-state scenarios and configurations using reactor relevant drivers.
 - Disruptions – avoidance and mitigation - 3D fields/stellarators
 - Nuclear qualified materials and components.
- Given the costs and time scales on our current path, we should look seriously at opportunities for significant improvements. Particularly salient would be
 - Development of magnet technology that opens up high field
 - Development of high-field-launch RF systems
 - Advanced divertor geometries, Liquid metal first wall?

Budgets And Their Impacts

- In 2007, no constraints were imposed.
 - We did not estimate costs of new initiatives and did not try to minimize the total fusion development cost.
 - We recognized the importance of economics for fusion energy, but did not address this directly. The ITER experience suggests that this is a 0th order issue and demonstrates that we must search for lower cost development paths.
 - **In general, we should always look for the cheapest way to achieve particular research goals or to reach particular technical readiness levels.**
- While the program aspires to an FNS step, we currently do not have the prerequisite information in hand - Nor are we likely to obtain it in the next 10 years without some major change in focus **and** a substantial increase in the budget.
 - The 2007 report identified gaps on the path to Demo, but most must be resolved before an FSNF as well
 - Given the assumed constrained budgets, most nuclear issues would likely have to be deferred.

The Fusion Program Has Always Had An International Context

- *“One important set of choices for the U.S. program involves deciding which issues to address through international collaboration and which to take on itself. ”*
- *“U.S. should not shrink from competing where we have the ability to make strong contributions. ”*
 - By far, the largest single element of our program is an international collaboration (ITER) with research to be carried out off-shore.
 - Other significant elements of our program are carried out with strong international collaboration – on experiments, theory and computation. This has been a salient characteristic of our field since 1958 – we have nothing to apologize for in terms of our international engagement.
 - Further, the US will not continue to be an interesting partner without a strong domestic program with compelling and unique domestic facilities.
 - Even further - U.S. competitiveness and leadership come into play - the question is “do we aspire to a future where the U.S. buys fusion energy technology from other countries or a future where the U.S. sells that technology?”

Final Thoughts

- These reports can have a profound and long-lasting impact on the field – your efforts (and the inevitable distractions from your “real” work) are well worth it.
- The program needs strong and bold advocacy
- The program needs strong community leadership - I urge you take an expansive view of your task.
- **Be careful about making recommendations with irreversible consequences before a self-consistent plan emerges.**