Discussion Group 5 Summary of USMFRSD Workshop in Austin, TX
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Early on during the workshop, Discussion Group 5 (DG-5) gravitated towards an aggressive fusion research and development (R&D) path geared towards the design, construction, and operation of a net-electric pilot plant in the mid-2040s. We developed a mission statement that reflects our sentiment, quoted below in bold text,

Our mission is for the US to develop innovative technologies to enable construction of a compact, low capital cost DT pilot plant that demonstrates net electricity and evolves to a nuclear testing mission.

This mission would provide the critical confidence necessary to attract private industry participation.

This mission statement guided the majority of our conversations during the week, and is in contrast to the SA-1 strategic approach. We consider this mission statement as addressing the NAS Charge F2 that there is a U.S. strategic interest in realizing economical fusion energy in the long term. Starting with this mission statement, we developed an outline of a strategic plan backward in time while addressing a multitude of questions put forth during plenary talks, though we did not address all questions proposed by other working groups (WG). Rather, we addressed WG discussion questions organically per their relevance to our mission and approach of developing a strategic plan outline.

The strategic plan outline to be described directly addresses the question posed by WG-2, "Is the US vision of an attractive fusion power plant fundamentally different than other countries'?" Our answer to this question is yes, the U.S. vision is fundamentally different and complementary to other countries' visions. We agree that *compact* fusion research and development is a unique path for the U.S. to pursue as a member of the worldwide research community. The value of this approach is driven by market attractiveness considerations. That is, compact fusion systems will likely have lower capital costs than larger fusion systems, which will allow for lower R&D costs on the pathway towards a net-electric pilot plant. Our judgment is that low capital cost, net-electric facility is an essential stage on the path to commercial fusion energy, to provide the confidence and key data for potentially competitive levelized cost of electricity (LCOE) devices. Drawing from the plenary talks of WG-3, it appears that the U.S. and worldwide energy markets will require reliable baseload, load-following energy sources to enable and complement intermittent renewable energy sources that are rapidly increasing their market shares. However, though the U.S. energy market in recent decades has preferred lower output (< 1 GWe) power plants, it appears that worldwide energy markets may have varying needs. Thus, due to uncertainty of the energy markets commercial fusion systems will encounter mid-century, DG-5 agrees that power outputs of LCOE-optimized commercial power plants following the net-electric pilot plant will vary, and this flexibility should be granted to industry players without assuming an optimal power output at this time.

With the underlying assumption that compact fusion research and development is a unique path for the U.S. to pursue, motivated by lower capital cost R&D devices and a pathway towards market competitiveness, we established a set of guiding principles to help focus our efforts. First, we agree that using innovation to enable a more efficient R&D approach is necessary, as suggested by SA-3. We also agree with the "Gaps" approach to form a strategic plan, described by the plenary talk by WG-1, though we recognize the danger of orthogonal gaps (e.g. device-specific gaps). Overall, we emphasize the need to focus on technical gaps where the U.S. has a comparative advantage, and leverage international collaborations that are geared towards addressing other technical gaps that must be closed to enable a net-electric pilot plant. However, we believe that Q_p (plasma gain) alone is not a useful metric to guide strategic planning. When addressing a question posed in the plenary SA-2 talk "Our objective should be an aggressive program to demonstrate $Q_p > 1$ in approximately a decade" our vote was not conclusive; the result varied depending on whether we were in or out of ITER, and led to DG members wanting to better specify the device with regards to what technical gaps it would be addressing, since it may change their votes. Although fusion gain could be an important component of a new device with a broader scientific scope, our consensus is that Q_p alone should not be the sole metric guiding strategic planning. Instead, we have defined a staged roadmap to a compact DT pilot plant, identifying key gaps that offer opportunities for exciting near-term achievements to improve public perception, garner excitement and trigger investments.

Provided with these guiding principles and mission for developing an outline of a strategic plan, we sought to develop a two-stage plan to achieve our mission, working backwards from a net-electric pilot plant. Starting with Stage 2, this would entail the construction of a net-electric pilot plant beginning in the 2030s, with operation beginning in the 2040s. We envision a multi-phased Stage 2 approach, with the key insight that a compact net electric facility requires approximately the same scale as a nuclear science facility, and thus these two missions can be addressed with a single device. The first phase of Stage 2 is to demonstrate short-pulse (< 10 displacements per atom (DPA) on first-wall materials) net-electric power production in a compact fusion system, focusing on integrated core/edge performance, assessing plasma-material interaction (PMI) performance, demonstrating sufficient tritium breeding, and demonstrating net-electric equivalent first before producing net-electricity. In an attempt to address the scientific implications of pursuing an aggressive net-electric pilot plant timeline that begins construction prior to ITER DT data, we posed the question: "Is burning plasma physics data required prior to designing and constructing a pilot plant?" The majority of the DG (2/3) voted "no". In short, the majority of DG-5 agrees that the design and construction of a pilot plant could technically begin before ITER DT operations, noting that earlier DD ITER results will provide data on the scaling of key physical processes to reactors. The second phase of Stage 2 is pursuing long-pulse discharges that allow for nuclear materials testing at greater than 10 DPA, allow for integrated full fuel cycle blanket tests, and full power plant demonstration. At expected pilot plant neutron wall loadings of ~2-4 MW/m^2 (~2-4 DPA/full-power-month), multi-month operation of the pilot plant will be required for adequate materials testing. In short, this phased approach of Stage 2 enables a net-electric demonstration preceding a fusion nuclear science facility (FNSF) phase of operation due to lesser material concerns at < 10 DPA, and the ability to confidently demonstrate net-electric fusion power production with multi-hour to multi-day pulses.

Provided with the phased Stage 2 approach, we continued developing this strategic plan outline backward in time to Stage 1, which we envision beginning now. Stage 1 pursues a range of innovative, intermediate-scale devices and facilities that can meaningfully address technical gaps required for the realization of Stage 2. This Stage 1 approach is heavily influenced by strategic elements provided by WG-4 and in SA-3. There are seven high-level elements that Stage 1 consists of that both represent opportunities for continued U.S. leadership and the leveraging of international collaborations. These essential elements are (*note, we did not attempt to prioritize these elements due to time constraints*):

1) **HTS technology development** (e.g. large, demountable coils of interest to MFE): HTS technology may be highly levering to the compact fusion devices in both improving reactor performance and reliability, facilitating rapid maintenance cycles, and provide effective nuclear testing environments.

2) **Advanced divertor and plasma facing components (PFCs) development**: Require divertor concept exploration and scaling to reactor conditions (possible facility development/upgrades), and core-edge integration studies at reactor relevant parameters. International collaborations may play a significant role (eg. MAST-U, COMPASS) in this subject area. Lastly, initiatives geared toward the development of liquid metal walls (a U.S. strength) should be expanded.

3) **Steady-state operation (e.g. high performance, disruption-free/transient-free fusion cores):** Continue work on existing tokamaks and perform upgrades of these devices to develop basis for fully non-inductive, stable cores, with suitable performance and transient control.

4) **Ramp-up of fusion nuclear material research**: U.S should consider possible small-scale, near-term facilities such as the accelerator-based (Phoenix Nuclear Labs) proposal, and a possible nuclear test facility (e.g. volumetric neutron source (VNS), gas dynamic trap (GDT)). The U.S. should also collaborate with international initiatives in EU and Japan, and perform integrated blanket design studies.

5) **Reactor design studies targeted to U.S strategic plan**: Address specific engineering aspects of compact fusion systems, as well as the full device systems integration, preliminary licensing scoping, including operational limits.

6) **Continued development of advanced heating, current drive, and fueling methods**: Test innovative techniques in existing devices and conduct design analyses for future reactors, including in a DT neutron environment.

7) **Compact, quasi-symmetric stellarator research and development:** Design and build an intermediate-scale quasi-symmetric stellarator as a potential concept for a future reactor core, and to address various technical gaps of interest.

We did not complete the discussion of the elements of Stage 1, and further community discussions should develop their details beyond the list provided above. In particular, there were some DG-5 members who think that a near-term national leadership-class experiment might be an important intermediate step towards a pilot plant, as discussed on the previous page. Such a device would address multiple gaps in a unified way, possibly culminating in a DT mission. A decision on such a device might be taken in the next 5 years and depend on multiple factors, such as progress on existing machines (DIII-D, NSTX-U, EAST, KSTAR, JET, W7X, etc.). Prospective facilities to address Stage 1 technical gaps could include university-scale experiments, upgrades to existing devices, and new leadership-class facilities. These facilities should be assessed based on their capacity to efficiently and rigorously address technical gaps and maintain US fusion leadership. Pursuit of these missions will provide a basis to grow the domestic program, as well as establish credibility that the U.S. fusion community is ready and capable for the net-electric pilot plant mission.

Furthermore, we emphasized the critical need for theory and predictive, integrated modelling (PIM) across all technical areas and device scales for the successful completion of Stage 2. Representative topic areas are core/edge plasmas, PMI, nuclear testing, tritium fuel cycle, blanket designs, etc… We recognize the need for continued verification and validation (V&V) and uncertainty quantification (UQ) efforts across all configurations and scales. We emphasize the need for a range of different models, from high-fidelity first-principles simulations to reduced-order models. In particular, we should build on the rapid growth in computer power and US strengths in computing (e.g. the Exascale initiative) to develop simulations that make a large leap in our ability to simulate key processes and give a more complete prediction of fusion device performance. Lastly, the U.S. fusion program should leverage advances in machine learning, quantum computing, etc… where appropriate, and these advances should be evaluated on an ongoing basis to assess potential applications for fusion energy R&D.

We fully recognize the value of fusion-relevant alternates both in the mainline program, and beyond. We prefer to refer to fusion-relevant alternates as “concept exploration” (CX) devices. Currently, concept exploration devices are not part of the U.S. fusion program and are confined to non-fusion research activities. DG-5 unanimously agrees that CX research should be reestablished as a meaningful portion of the U.S. fusion program, and there should be a review of the metrics by which we evaluate CX devices, as suggested by SA-4. We recognize the value of disruptive innovation that, by definition, cannot be part of a strategic plan. However, in reestablishing CX research as a strategic element of the U.S. fusion program, this action will create an environment in which disruptive innovation is more likely to occur, which could both improve mainline fusion concepts and potentially lead to more attractive pathways toward economical fusion power. Furthermore, DG-5 agrees that collaboration between Discovery Plasma Science (e.g. non-fusion focused plasma research) and the fusion research programs (e.g. mainline and CX) should be encouraged and rewarded.

Provided with the outline of a strategic plan, and other important strategic elements of the U.S. fusion program such as theory, modelling, and concept exploration research and development, we addressed how this plan would be influenced by a decision for continued U.S. participation in ITER or a U.S. withdrawal. In short, we agree that our mission remains the same in either case; however, a withdrawal from ITER would place a greater responsibility on the U.S. program for closing Stage 1 technical gaps, and would result in riskier extrapolations to Stage 2; ITER is providing important know-how in reactor design that will be more difficult for the U.S to develop outside of ITER. If the U.S. remains a partner in ITER, this continued participation will enable a better understanding of the scaling of fusion reactors, provide critical benchmark data for theory and modelling, and provide an additional set of burning plasma data prior to beginning operations of a net-electric pilot plant. In short, the continued U.S. participation in ITER would be complementary to this aggressive compact, net-electric pilot plant mission. The combined learning from ITER and the pilot plant would provide a pathway towards commercial, LCOE-optimized power plants that may vary in power output depending on energy market demands.

Follow-on activities from this workshop are recommended as there is a need for ongoing, long-term planning. DG-5 believes there should be an alternative community input mechanism to FES (e.g. P5) than is currently available. We would like to continue developing the strategic plan we have outlined to assess the credibility and completeness of Stage 1 elements, and assess the nuclear viability and technical credibility of the phased Stage 2 approach. In the more immediate term, we request feedback from the NAS panel into whether an additional workshop would be helpful for their report. The decision to hold another workshop will also be dependent on other DG summaries, and should be made after input from the NAS panel and other discussion groups.

Consensus statements

For convenience, we have provided a list of consensus statements that were formulated during our discussions, which closely follows the summary written above.

Monday, December 11, 2017

WG 3 - Market attractiveness

- **The US & world energy markets will require reliable baseload, load-following energy sources to complement intermittent renewable energy sources.**

- **Keep both small and large scale reactors (e.g. 200 MWe vs. 1.5 GWe) as potential future market solutions**

- **Public perception/making a splash (high profile success) is important to garner excitement & generate investment**

- **The goal of a first net electric plant in the US should be to minimize capital cost rather than COE**

WG 1 - Principles and Vision, Metrics and Criteria

- **We agree with the "Gaps Approach" (e.g. 2007 FESAC Greenwald Report) for developing and judging strategic plans and strategic approaches**

- **We should focus on developing a credible plan (not automatically constrained by present budget assumptions)**

Tuesday, December 12, 2017

WG 4 - Review and Characterize Key Strategic Elements

- **We agree the summary of strategic elements from Madison Workshop is sufficient.**

WG 2 - Impact of access to ITER

- We agree with the following consensus statements presented at the end of the WG-2 talk:
  - **A burning plasma demonstration is absolutely necessary to move into next steps toward fusion energy**

  - **When ITER is operational, and reaches its projected performance, it can provide critical burning plasma science needed to progress to fusion energy**

  - **To take advantage of ITER's output as a burning plasma experiment (or any BPX) → the domestic fusion program must be revitalized and appropriately positioned to pursue the next steps after a burning plasma demonstration**

SA-2 - Deliver key technical achievements ASAP, then optimize and develop DEMO

- **Q_p alone is not the most useful metric to guide strategic planning**

- **We agree $P_{electric} > 0$ is an important strategic element that should be pursued aggressively.**

- **Compact fusion is unique path for US to pursue (fits in with WG 3 Market Attractiveness)**

- **We should definitely try to develop HTS technology for possible high-field magnet solutions with an aggressive 5-10 year research and development program beginning as soon as possible.**

- **Imperative to do nuclear and materials testing to enable electricity generation**

- **First wall nuclear materials -- (1) materials testing facility needed to qualify materials for licensing, (2) could be part of but distinct also from volumetric neutron source (VNS)**

SA-4 - Develop several alternate concepts to sufficient states to enable comparison; down-select to DEMO

- **An environment that encourages disruptive innovation should be created and fostered by reestablishing concept exploration (CX) as a meaningful fraction of the U.S. fusion budget.**

- **A more systematic review process should be performed of concept exploration devices which includes the development of new metrics to assess the potential impact of these devices.**

- **Collaboration between concept exploration devices and the mainline fusion research programs should be encouraged and rewarded.**

- **Collaboration between Discovery Plasma Science (e.g. non-fusion plasma science) and the fusion research programs (e.g. mainline and CX) should be encouraged and rewarded.**

Wednesday, December 13, 2017

SA-3 - Innovate first

- **The Innovation spreadsheet (figure below) is a reasonable start, though should be developed more to ensure there are no missing elements.**

	Fusion pathways->	Configurations (E. Marmar)		Technology (R. Nygren)		Understanding (F. Ebrahimi)
Fusion Energy Objectives		Optimized stellarator (QH, QA, QO)	Advanced/ Compact/ Spherical tokamak	Plasma Technology	Fusion Nuclear Technology	Theory and Modeling
Improved plasma science	Confinement with Confidence					
	Plasma Transients Controlled					
	Maintain Burning Plasma					
Improved device performance	Higher field, pressure operation					
	Steady state operation					
Materials	Plasma Material Interaction					
	Lower Activation w/ long life					
Sustaining the fuel cycle safely	Safe Self Sufficient Tritium Systems					
	Siting and Operating D/T Facilities					

- **Work backwards from the mission of beginning construction of a net-electric pilot plant in the 2030s, with operation beginning in the 2040s, based on a US vision of "compact" fusion.**

Thursday, December 14, 2017

SA-1 - DEMO now

- **Integrated blanket research, design, tritium issues → motivates net electric pilot plant first before going straight to DEMO.**

- **We were not considering this (now to DEMO) as a useful strategy, need to use innovation to achieve more compact, faster (cheaper) route**

SA-3 - Theory

- **Theory & Modeling (Theoretically Integrated Predictive Modeling) is inherent and critical across all areas (core plasmas, PMI, nuclear testing, tritium cycle, integrated blanket design, …)**

- **We endorse the major themes from the Integrated Simulations 2015 report (Bonoli / Curfman McInnes).**

- **We recognize the value and need for ongoing verification and validation (V&V) activities, and generalizing codes to apply to different sized and types of configurations**