Five-to-Seven Year ST Research Priorities for Options of High-Gain Burning Plasma, Fusion Nuclear Science, and Plasma Material Interface

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Five-to-Seven Year ST Research Priorities for Options of High-Gain Burning Plasma, Fusion Nuclear Science, and Plasma Material Interface

1. Assignment

This assignment is to produce a 5-page report addressing the high priority ST research needs for the next ~5 years, accounting for the strengths of the world ST Programs in present and planned research capabilities.

2. ST Mission/Facility Options for ITER Era

In the interest of enhancing the opportunities for ST research to contribute strongly to the Fusion Energy Sciences Program, the STCC agreed to consider three important options of ST research mission/facilities for the ITER Era: High-Gain Burning Plasma (BP), Fusion Nuclear Science (FNS), and Plasma Material Interface (PMI).

Since the ReNeW focused on the research required to enable design of a DEMO but specifically did not consider what facilities are required to address these needs, the STCC identified these mission/facility options (BP, FNS, PMI) to address the importance of subsets of these scientific issues. The purpose of this white paper therefore is to lay out the research that must be done to be able to design and build each of these facilities. The research priorities for the next ~5 years and the available research tools are identified.

The STCC further agreed that the key research elements identified in the ReNeW Thrust 16 Report [1] will be used to guide the discussion and selection of high priority research needs for each of the following missions.

2.1. High-Gain Burning Plasma Mission

To assess the viability of achieving high fusion gain in the compact ST configuration under the unique plasma conditions of high normalized pressure and potentially strong drive for fusion alpha driven Alfvénic instabilities, with application to increased fusion performance in the higher fusion power stages of the nuclear fusion science facility and to provide the physics basis for an ST-based fusion power reactor.

2.2. Fusion Nuclear Science Mission

To enable the investigation of synergistic effects for time scales of interest to fusion plasma material interactions and power extraction in an integrated fusion nuclear environment – encountering four phases of matter and across the nuclear, atomic, nano, meso, and macroscopic scales.

2.3. Plasma Material Interface Mission

To qualify candidate wall PFCs and divertors in a long-pulse, DD facility with edge conditions, power loads, and other non-nuclear operating factors approaching those expected in a fusion nuclear device (e.g. a fusion nuclear science facility or DEMO), in order to reduce design uncertainties.

Comment [SAS1]: Tile says 5-7, this says ~ 5. Why not just say "5 years", or "5-7 years". Let's be consistent.

Comment [SAS2]: Mission / Facility – this is a bit confusing. Do we mean facilities, or missions? At the end of ReNeW, many were strongly opposed to stating that a new "device" was needed. Does facility = device here?

It seems that the committee is ok with "mission". Are we ok with "facility"?

Comment [SAS3]: See comment 2 above.

Comment [SAS4]: We should give these some sense of scale in cost. FNS > BP > PMI in cost.

Comment [SAS5]: See comment 2 above.

Comment [SAS6]: (i) this specifically says "facilities", (ii) are we comfortable in saying this? This is a strong statement – have we really done the research needed to make this statement for a facility? Sounds more like we need a conceptual design. Perhaps say "lay out the research in the most simple terms…", or something like that.

Comment [SAS7]: Make this consistent with whatever the title becomes.

Comment [SAS8]: Yes – this is what we agreed, and it's clearly stated.

Comment [SAS9]: I've said before, and it was discussed in email and at the last CC, that this could be strengthened. However, this didn't seem to be accepted in the email exchange, so I'll drop the effort. However, I am an protagonist for FNS, so I think this is unfortunate.

Comment [SAS10]: (i) Doesn't long-pulse DD mean a nuclear facility due to activation?
(ii) how long, compared to FNS?

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3. ST Attractiveness for These Missions

The ST configuration, using the working assumptions and parameters (see, Appendix W for FNS), compared to the higher aspect ratio Tokamak, has the following anticipated advantages:

1) Very high experimentally verified stability beta limits in ST allow prospective disruption-free plasma operation, which is absolutely required by the FNS (10^3–10^6 s continuous plasmas) PMI ($\leq 10^3$ s) missions, over a wide parameter space sufficiently far away from these limits. For shorter pulses required for the BP mission (($\leq 10^1$–10^2 s), closer approach to these limits would also become available.
2) Experimentally verified high ion and adequate electron energy confinement so far to provide high confidence to extend to JET-level plasma confinement conditions required for the FNS and PMI missions. This would also ease the designs for the BP mission, which would require super-ITER level plasma confinement conditions.
3) Assuming successful R&D for the single-turn toroidal field center leg, and for start-up and ramp-up of plasma to full current with little or no central induction, compact designs with $R_0 \sim 1+$ m would become adequate for the FNS mission, allowing modest fusion power requirements while providing substantial fusion neutron fluxes.
4) This would in turn lead to small size divertors and PFCs with increased heat fluxes that improve the cost effectiveness of the PMI mission.
5) This would further simplify the engineering configuration to allow extensive component modularization and remote handling. These are required to achieve adequate duty factors for component testing R&D as part of a future energy development program.
6) These in turn would enable flexible staging of the FNS research program by allowing major component replacement on a more regular basis, starting from D-D to D-T stages with increasing driven burn plasma conditions to enter into FNS research beginning with substantial fusion neutron fluxes.
7) If a closer approach to the stability beta limits is also achieved, the ST plasmas of similar sizes can be optimized for burning plasma research at high gain (Q~20–30).

Multi-turn, jointed toroidal field coils with a substantial solenoid are allowed for the PMI and BP missions in the presence of limited neutron fluences. The first two advantages remain in these cases.

[*MP: Verbatim from file "Menard-BP-priorities-v2-draft1"*]

Assuming adequate thermal and energetic particle confinement, and stability in steady-state, the ST could provide a reduced size, complexity, and cost path to a burning plasma and power reactor. The ST could also provide access to a unique burning plasma regime relevant to both low-A and high-A tokamak reactors – namely the combination of: high normalized pressure, strong self-organization through large self-generated current and self-heating, and potentially strong Alfvénic instabilities and fast-ion transport driven primarily by the fusion alpha population.

Comment [SAS11]: During the last CC, several people suggested dropping this section. I believe stating attractiveness is ok, but at present this section is unrealistically optimistic, and could end up hurting the cause. I would drop it unless we can restate more realistically.

Comment [SAS12]: This is really not true. The ST target equilibrium parameters that we are talking about (even the lowest beta, due to low li) face significant challenges to have disruption-free operation. We'll get strong criticism from AT and stellarator proponents. We should say something positive that we can support – more realistic.

Comment [SAS13]: Untrue if this means turning down beta to operate disruption-free. Far too optimistic, and no need to be.

Comment [SAS14]: Again untrue – not verified at the level needed for the FNS and BP missions. E.g. ReNeW states that a key reason for next step upgrades such as MAST-U and NSTX-U will be to determine the scaling of electron confinement to an FNS-level device.

Comment [SAS15]: This statement is stated more weakly – more realistically. But, in fact, based on recent PEGASUS gun results, I think we can argue that progress to start-up is more optimistic than was stated in the ReNeW document.

Comment [SAS16]: Substantial neutron fluxes require significant stability and confinement performance (columns II and III of Appendix W). This is ok, but let's ensure that the physics required to reach these levels is requested by this document, and not be unrealistic. Fluence for Column I might not be competitive with what ITER could do.

Comment [SAS17]: I suggest this paragraph is very good – not overstated, yet showing ST strengths.

4. High Priority Research Needs and Tools

4.1. BP

[*MP: Verbatim from file "Menard-BP-priorities-v2-draft1"*]

The research priorities for BP below are organized by the elements of ReNeW Thrust 16.

1. **Plasma current formation and ramp-up**
   The shorter pulse duration and reduced neutron damage to the center post of BPST should enable use of solenoid for inductive ramp-up to 8-10MA. High Q~10 may require I_P=12-17MA, so achieving flux savings from non-solenoidal start-up is high priority.
2. **Innovative magnetic geometries and first wall solutions**
   Mitigation requirements for high heat, particle, and neutron flux are comparable to FNS facility but at reduced pulse duration (few 10^2s), thus heat flux reduction via magnetic flux expansion/extension is high priority. For high density/f_{BS} scenarios, heat flux mitigation utilizing divertor radiation is potentially useful and high priority. Control of the plasma density and impurity content is a high priority enabling capability.
3. **Test the understanding of ST confinement, stability at fusion-relevant parameters**
   Q_{DT} ~ 2-10 requires moderate-high HH98~1.3-1.7. The underlying modes/scalings for electron transport are not well understood, and could make high HH difficult to achieve, so e-transport is a high priority research area. Means of improving/increasing confinement may be required to achieve a compact device size with minimized auxiliary power. A low recycling wall is a leading candidate tool for confinement improvement and is high priority. Drive for fast-ion modes may be strong since W_α / W_{TOT} = 5-20% and v_α / $v_{Alfvén}$ = 5-8, so fast-ion transport predictive capability is high priority.
4. **Active and passive control to enable long-pulse disruption-free operation at low l_i**
   Baseline operation (Q~2-3) has β_N ~ 4, $l_i \leq 0.6$ near no-wall limit and higher performance scenarios (Q~3-10) operate closer to the ideal-wall limit β_N ~ 5-6, $l_i \leq 0.4$. Thus, active (and passive) control of resonant field amplification and RWM instability is high priority.
5. **Tools to maintain the current and control the plasma profiles**
   For low-medium density, NBI is likely sufficient to control profiles while sustaining I_P, so this tool development would be low-medium priority, but for high density/f_{BS} scenarios, NBI may be insufficient for profile control, and core fueling is a leading candidate tool for density/pressure profile control and is high priority.
6. **Develop normally-conducting radiation-tolerant magnets**
   Provided shielding required for TF and OH is not unacceptably thick, this is low priority.
7. **Extend the ST to near-burning plasma conditions**
   The BP mission/goal is to extend the ST well beyond "near-burning plasma conditions".

4.2. FNS

The following research needs are of high priority during the next ~5 years (see Appendix X for more detail). The available and planned research tools in the U.S. and world ST programs to fully or substantially address these research needs are also identified.

- **Start-up:** Minimal or non-solenoid formation of plasma with ~keV-level electron temperatures. EBW + Helicity Injection + outboard vertical field are strong candidates at present, based on recent data. Planned research on *ST devices 1, 2, etc.* has an opportunity to prove the scientific principle for FNS.
- **Ramp-up:** Ramp-up further to ST proof of principle level current while maintaining ~keV electron temperatures and substantial densities, by adding NBI and EBW to the above are strong candidates at present. Planned research on *ST devices x, y, etc.* and DIII-D has an opportunity to prove the scientific principle for FNS.

Comment [SAS18]: This is an efficient format that requires no explanation of the elements – very compact – just refer to ReNeW Thrust 16 if more detail is needed.

Comment [SAS19]: Very strong (which is good to back up what some perceive as a "weak" area).

Comment [SAS20]: May need rotation control as well for stability, and perhaps confinement control, unless research in the next 5 -10 years demonstrates clearly how to avoid rotation profiles leading to reduced stability.

Comment [SAS21]: No specific mention of J profile? Bootstrap current? How to sustain current?

Comment [SAS22]: Not certain of the meaning here.

OVERALL, I suggest this section be used as a template for the rest of the document. It's brief, not overstated, and clear.

Deleted: P

Comment [SAS23]: Does this include plasma guns? If so, make it more specific?

Comment [SAS24]: In this case, because of PEGASUS gun success, I might be convinced that planned research on some near-term device might prove the scientific principle. But just to say this without knowing more about the plans for this next step is cavalier. "has an opportunity" might be sufficiently weak.

Comment [SAS25]: Is this meaningful here?

Comment [SAS26]: Again, this might be too cavalier / optimistic.

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- **Divertor and PFC:** Verify eXtended or eXpanded-SOL Divertor (XXD) performance at proof of principle level plasma current for Hot-Ion H-Mode (HIHM) plasmas. Planned research on *ST devices a, b, etc*, has a chance to test some of these approaches in ~5 years at the proof of principle level.
- **Stability Control:** Determine feasibility and requirements of passive control for sustained operation free of plasma induced disruptions with $\beta_N << \beta_{nwl}$, $\beta_N < \beta_{nwl}$, and eventually $\beta_N \sim 1.3\ \beta_{nwl}$ for HIHM plasmas. Experiments on *ST devices r, s, etc., and tokamak DIII-D, EAST, etc.* can be adjusted to resolve this issue in the next ~5 years.
- **Stability Control:** Determine the threshold in normalized resonant field errors (possibly $< 10^{-4\text{-}5}$), below which passive control can ensure sustained operation free of plasma induced disruptions in the presence of adequate plasma rotation. The needed research can be carried out in ST and tokamak where and when such field errors are reduced toward the ~10^{-4} level.
- **Maintain Current and Profiles:** Apply modern and ITER plasma simulation tools to the above FNS plasma operation scenarios to determine potentially new requirements for very long pulse plasma operations ($10^3 \rightarrow 10^6$ s). This can be carried out fully during the next ~5 years.

[*MP: Disposition of this paragraph awaits promised suggestions by Dick and JP.*]
[Divertor and PFC: Liquid Metal Surface is a poorly explored but potentially high benefit/impact research area for FNS application independent of aspect ratio. The following priorities are therefore suggested:

- Develop a high heat flux liquid metal surface research program and carry them out, using toroidal device as well as separate test stands, to determine the scientific uncertainty and the potential benefit/impact for application to the FNS mission.
- Verify, as a first research goal, the capability of liquid metal surface not to cause larger than normal impurity influx.]

Other research elements identified in the Thrust 16 report are of lower near term ST research priorities in support of the ST FNS mission. These include: electron turbulence and transport; energetic particle instabilities; active mode control; neutral beam injection system; plasma waves; particle control; and core fueling. These elements share a strong common basis with the relative mature Tokamak physics understanding.

4.3. PMI

[*MP: This is the old draft by Dick, a discussion at the 4th CC led to some actions. This version will be replaced by his new draft.*]
The long-term vision for an ST-based PMI program is to develop first wall and divertor solutions suitable for use in a DEMO device, which will feature very long pulses, hot walls, and high neutron wall loading, along with strong plasma-material interactions. The near-term, 5-10 year mission of such a program should address critical knowledge gaps in the science and engineering of materials (both solid and liquid) interfaced with plasma. Extensive experimental and theoretical/computational studies are also necessary to identify material response behavior, and its scaling with environmental parameters likely to be encountered in a burning plasma device.

Comment [SAS27]: What does this mean? 1 MA? If so, is that good enough for extrapolation to 3.5 – 10 MA in FNSF?

Comment [SAS28]: This needs to be defined, and what, if any relevance it has for FNSF.

Comment [SAS29]: Define HIHM

Comment [SAS30]: Not true. ST research can make progress, and might solve the problem for shorter pulse, but DIII-D/KSTAR/EAST can't access the high betaN and low li ST target. SO, the long-pulse element of the research for FNSF will not be solved.

Comment [SAS31]: (i) This is not the correct question to ask from the standpoint of BOTH error field level, AND "adequate" rotation. Stability is not determined simply by level of rotation (shown and published in recent ST work).

You should not to make the physics statement of need here – simply rely on the ReNeW document (as was done in the BP section), and address how the concerns defined in ReNeW will be addressed to support FNSF.

Comment [SAS32]: This might imply that the problem is solved after this effort is made (not true). Also, there is experimental work that can be done – including elements of NSTX-U program.

Comment [SAS33]: Can't we already speak to the planned programs on LTX and NSTX? Even the LLD in NSTX should provide important input toward the goal.

Comment [SAS34]: Completely disagree on all points. Even just for the NBI system, do we have a solution for 10^6 second pulses? ReNeW didn't think so. I could fill the better part of a page stating needs of FNSF with regard to these areas.

I can do that on request – but won't do that unless I know it won't be ignored.

Within 5-10 years, we should:

1. Develop a range of plasma test stands to evaluate both solid and liquid PFCs, at power loadings in the 10 – 20 MW/m^2 range, transient heat loads in the 1-5 MJ/s range, particle fluxes in the range of 10^{22} m^{-2}s^{-1}, at near steady-state conditions. One or more test stands capable of handling neutron - irradiated (activated) materials are needed. In addition, integrated test stands that supply either neutron or neutron surrogate sources *coupled* to plasma test stands able to conduct synergistic beam-material interaction studies should be developed.
2. Develop magnetic field (vacuum) test stands with ST-appropriate magnetic field magnitudes and components to test flowing liquid metal wall solutions, at both high and low Hartmann number.
3. Validate and verify theoretical and computational models for solid and liquid materials, in close collaboration with the test stand programs.
4. Evaluate both low recycling (lithium) and high recycling liquid metals as PFCs, in LTX and NSTX, for the purpose of selecting between the two options.
5. Begin an ST-based PFC/PMI evaluation program for the candidate solutions developed in (1) and (2), consistent with modeling (3). Requirements here eventually include hot wall capability (to 500 – 600 °C for solids and high recycling liquids, up to 400 °C for lithium), high divertor flux (heat and particles), extended pulse operation, and capability to evaluate both high-Z solids, innovative solids (i.e. high Z/lowZ alloys), and high or low recycling liquids. Divertor and wall in-situ sample probe diagnostics will be critical to link test stand materials experiments with edge/materials simulation codes. As above, the capability for handling irradiated samples is desirable. In the ~10 year time frame, a promising approach would be adoption of a significant PMI/PFC mission for NSTX-U, with appropriate further upgrades of the device.

We emphasize that this is a development program, with multiple lines of investigation, because there is no clearly viable wall solution for a DEMO at this time. In the same 5-10 year time scale, innovative divertor geometries will be investigated on NSTX, MAST, and their upgrades, in order to further refine the requirements for wall and divertor materials.

5. Summary and the Planned ~6-10 Year Assessment

The STCC has identified high priority ST research needs from among the research elements listed in the ReNeW Thrust 16 Report [1]:

1) **Start-up and Ramp-up** for the FNS mission,
2) **Divertor and PFC** for the FNS and PMI missions
3) **Confinement Stability** for the BP and PMI missions
4) **Stability Control** for the BP and FNS missions
5) **Maintain Current and Profiles** for the FNS mission

A summary of the key results of this white paper in a table format is provided in Appendix Y.

Comment [SAS35]: The goals are clearly stated, but I think you need to define the plasma needs, if any, more clearly.

I still highly suggest using Jon's approach in the BP section as a common format for all sections.

Comment [SAS36]: This begs the question for the general reader of why we don't just to the research on NSTX and MAST.

Comment [SAS37]: I don't think we really have room for this. I suggest focusing on the 5, or the 5-7 year timescale as originally charged, and stick with doing a good job on that charge.

More specifically – drop the 6-10 year section and use the space to better address the 5 year charge.

Comment [SAS38]: ??? Who established the mapping here of missions to elements?

Also, there are seven ReNeW Thrust 16 elements.

Comment [SAS39]: No Appendix Y.

In a next assignment, the STCC is to carry out an assessment of the high priority research needs for the succeeding ~6-10 years to establish the scientific basis for the BP, FNS, and PMI mission/facility options, including further upgrades of ST experiments required to do so. The information in this white paper serves as starting point for the next white paper.

Comment [SAS40]: Not needed, and doesn't give needed information, so delete.

References:

[1] ReNeW Thrust 16 Report.

[*MP: If 4.1 and 4.3 can fit into ~1.5 pages, the technical content of this white paper would be just within 5 pages! Of course, we encourage inclusion of appendices including minority views.*]

Comment [SAS41]: Minority view? What's the majority view?

Perhaps we can take a vote on the following, and see if we can get a majority on the following:

1) Maintain the present format of Section 1 and 2 (PAGE 1)

2) Everyone adopt Jon's format of the BP mission section, and give ~ 1.25 pages for each mission (PAGES 2 – 4.75)

3) Use the remaining 0.25 pages for a concluding paragraph.

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Appendix W. Working Assumptions and Parameters Required by the FNS Mission [1,2,3,4], and Research Issues with Regard to ReNeW Thrust 16 Research Elements [5]:

Stages*	**I**	**II**	**III**
Fuel	D-D → D-T	D-T	D-T
Pressure, $\beta_T B^2$ (%T^2)	18	86	133
Outboard fusion neutron W_L (MW/m^2)	0.01 → 0.25	1.0	2.0
Plasma current I_p (MA)	3.4	8.2	10.1
Safety factor q_{Cyl}	9.2	3.7	3.0
Toroidal beta β_T (%)	5	18	28
Normal beta β_N (MA/Tm)	1.9	3.8	5.9
Avg. T_i (keV)	5.4	10.3	13.3
Avg. T_e (keV)	3.1	6.8	8.1
Research questions organized by ReNeW Thrust-16 research elements			
1: Startup and ramp-up			
1a: Startup (formation)	- Can CHI, EBW, CHI+EBW startup large toroidal current with high Te?		
1b: Ramp-up	- Can CHI, EBW, NBI, CHI+EBW+NBI ramp up to full current with high Te and density?		
2: Divertor and PFC			
2a: Configuration	- Can extended or expanded SOL divertor be made to reduce peak heat flux to levels that permit very long pulse operations, even with uncertain SOL thickness?		
2b: Liquid metal surface	- Can high impurity influx be prevented? - What research will be required to provide long pulse high heat flux data?		
3: Confinement stability			
3a: Confinement	- Will $\tau_{Ei} \sim 0.7\ \tau_{Neo,i}$; $\tau_{Ee} \sim 0.7\ \tau_{ITER\text{-}H}$ remain sufficiently correct? - Can HIHM be maintained, even if τ_{Ee} improves as $\nu^* \rightarrow 10^{-3}$?		
3b: Stability (energetic particles)	Will sub-Alfvenic beam and some super-Alfvenic α cause unacceptable effects on fast ion confinement and J_{NB} profile?		
4: Stability control			
4a: Active	Will $\beta_N << \beta_{nwl}$ require active control?	Will $\beta_N < \beta_{nwl}$ require active control?	Will $\beta_N \sim 1.3\beta_{nwl}$ require active control?
4b: Passive	- Does disruption-free plasma operation require only passive control?		
4c: Resonant field error B_{error}/B_T	- Can B_{error} be made sufficiently small to avoid the need for active stability control for Stage I, II, or III?		
5: Maintain current and profiles			
5a: Energetic particle beam (co-E_{NBI}, kV)	Can 100-kV PINB be made continuous?	Can 240-kV NINB be made continuous?	Can 300-kV NINB be made continuous?
5b: Plasma wave	- Can EBW be applied to maintain $q_{min} > 2$ or 3 and avoid NTM?		

Comment [SAS42]: Criticism may cone that this is too low – can ITER address this?

Comment [SAS43]: More interesting levels of wall loading (and associated fluence). So, define the needed physics to realistically support this – comprehensive and with low enough risk to convince people in the community.

Comment [SAS44]: Just do this organization in the main body text – once. This is what was "agreed to", as stated on the first page.

Comment [SAS45]: Note sure what asking all these questions on this page adds?

The table is good, but why not just delete all the questions?

Comment [SAS46]: All issues like this are in ReNeW Theme 5 chapter.

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5c: Particle control	- Can extended or expanded SOL divertor + cryo-pump be adequate?
5d: Core fueling	- Can high-field side pellet provide adequate fueling?
5e: Continuous burn time	- Can the plasma be maintained continuously for 10^3 s, and in steps progressively for 10^6 s?

*using A_{95}=1.5 case used in 2008 publications with R_0 = 1.2 m, B_T = 2.18T, f_{BS} ~ 0.5; A=1.7 and 1.35 designs, including D-T operation with the JET level plasma pressure, are also calculated during 2009 showing similar range of possibilities, based on the same set of systems analysis models and assumptions [3,4]. **$\beta_{nwl} = \beta_{no\text{-}wall\ limit}$.

References

[1] FEC08, FT, P3-14
[2] TOFE08, Oral 23
[3] PPCF **47** (2005) B263
[4] Neumeyer et al, 2005.
[5] ReNeW Thrust 16, 2009.

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Appendix X. Information supporting the near term (next ~5 years) ST research priorities to enable the ST FNS mission

The scientific Uncertainties (U) in the research outcome, and the potential Benefits/Impacts (BI) of the Uncertainties to the ST FNS design and performance are estimated using the approximate degrees of H=3, M=2, and L=1, vetted by the discussions so far within STCC, using recent published information [1,2,3,4]. ST research Priority (P) with regard to an FNS ST is very roughly proportional to the product of U and BI (P = U*BI). The research questions already identified in the Thrust 16 report [5] of the magnetic fusion ReNeW are used, with necessary additions (in blue) to account for more recently identified high leverage research questions.

Stage	I	II	III		U= Uncertainties	BI= Benefit/ Impact	Priority = U*BI
Relevant research questions organized by ReNeW Thrust-16 research elements				**Research needed to understand the physics basis and adequately project to FNS conditions**			
1: Startup and ramp-up							
1a: Startup (formation)	Can CHI, EBW, CHI+EBW startup large toroidal current with high Te (~1keV)?			• Form ~0.5MA high Te plasma	M	H	6
1b: Ramp-up	Can CHI, EBW, NBI, CHI+EBW+NBI ramp up to full current with high Te (~1keV) and density (~10^{13}/cm^3)?			• Ramp up to ~1MA at high Te and density	M	H	6
2: Divertor and PFC							
2a: Configuration	Can XXD* be made to reduce peak heat flux to levels that permit very long pulse operations, even with uncertain SOL thickness?			• Produce and test XXD at ~1MA in HIHM • Determine common database and understanding with Tokamak	M	H	6
2b: Liquid metal surface	What research will be required to provide long pulse high heat flux data using liquid metal surfaces? Can high impurity influx be prevented?			• Develop high heat flux liquid metal surface research program to determine risk and benefit • Verify the capability of liquid metal surface to eliminate impurity influx	H	Key research missing	Key research missing
3: Confinement stability							

Comment [SAS47]: About the tables in Appendix X:

1) Where did these "U" and "BI" factors come from, post-ReNeW?

2) I don't think it's correct to say that these numbers reflect discussion so far with the STCC. When did we have this discussion during this charge?

3) The questions are the same as in Appendix W. I suggested deleting them from Appendix W.

4) About the questions – I've already commented on certain physics that (i) I don't believe is true – like under "4c", (ii) that I think you need a bit more physics solutions as per ReNeW to confidently meet the FNSF requirements.

I'm willing to give specific comments on what to fill in, following a format like Jon's BP section, but I'm not willing to take the time unless there's a good chance that they won't be completely ignored.

5) Regarding U*BI, how do the product of letters under "U" (like "M") and "BI", like "H" multiply to get a number? Why not just use numbers throughout?

6) I don't suggest that these numbers be used to represent the STCC's position under this charge until we actually take the time and get everyone's input and compile the numbers.

7) With all of the above said, I think a table like this is useful, but I think there's some work to truly make it consistent with the panel's position.

3a: Confinement	Will $\tau_{Ei} \sim 0.7\ \tau_{Neo,i}$; $\tau_{Ee} \sim 0.7\ \tau_{ITER\text{-}H}$ remain sufficiently correct? Can HIHM be maintained, even if τ_{Ee} improves as $\nu^* \rightarrow 10^{-3}$?	• Review database on HIHM from Tokamak and ST to establish working τ_{Ee} scaling • Investigate electron turbulence, transport, confinement in ST for $\nu^* \rightarrow 10^{-3}$ in HIHM	M	M	4
3b: Stability (energetic particles)	Will sub-Alfvenic beam and some super-Alfvenic α cause unacceptable effects on fast ion confinement and J_{NB} profile?	• Update ITER energetic particle database with ST data for HIHM	M	M	4
4: Stability control					
4a: Active	Will $\beta_N \ll \beta_{nwl}$ require active control? / Will $\beta_N < \beta_{nwl}$ require active control? / Will $\beta_N \sim 1.3\beta_{nwl}$ require active control?	• Complete active stability control research on ST and Tokamak for $\beta_N \ll \beta_{nwl}$, $\beta_N < \beta_{nwl}$, and $\beta_N \sim 1.3\ \beta_{nwl}$ for HIHM plasmas	M	M	4
4b: Passive	Does disruption-free plasma operation require only passive control for $\beta_N \ll \beta_{nwl}$, $\beta_N < \beta_{nwl}$, and $\beta_N \sim 1.3\ \beta_{nwl}$?	• Update ITER database with ST data on low disruptivity conditions removed from known stability limits for HIHM plasmas	M	H	6
4c: Resonant field error B_{error}/B_T	Can B_{error} be made sufficiently small to avoid the need for active stability control for $\beta_N \ll \beta_{nwl}$, $\beta_N < \beta_{nwl}$, and $\beta_N \sim 1.3\ \beta_{nwl}$?	• Review Tokamak and ST database on plasma conditions in the presence of very low resonant error fields (possibly $<10^{-4\text{-}5}$) • Create and study plasma conditions in the presence of very low resonant error fields (possibly $<10^{-4\text{-}5}$)	M	H	6
5: Maintain current and profiles					

5a: Energetic particle beam (co-E_{NBI}, kV)	Can 100-kV PINB be made continuous?	Can 240-kV NINB be made continuous ?	Can 300-kV NINB be made continuous ?	• Develop engineering science basis for steady state positive ion neutral beam for ~100 kV, similar to KSTAR, EAST NBI concepts. • Develop engineering science basis for steady state negative ion neutral beam for ~300 kV, scaled down from ITER prototype.	M	M	4
5b: Plasma wave	Can EBW be applied to maintain $q_{min} > 2$ or 3 and avoid NTM?			• Test EBW H&CD at ~ MW level in HIHM plasmas	M	M	4
5c: Particle control	Can extended or expanded SOL divertor + cryo-pump be adequate?			• Scale down ITER cryo-pump prototype	L	H	3
5d: Core fueling	Can high-field side pellet provide adequate fueling?			• Scale down ITER pellet injector prototype	L	H	3
5e: Continuous burn time (s)	Can the plasma be maintained continuously for 10^3 s, and in steps progressively for 10^6 s?			• Apply modern simulation capabilities such as SWIM, GTLF, TLF23, TSC, etc. to refine estimates of FNS ST plasma projections	M	H	6

*XXD = eXtended or eXpanded-SOL Divertor

References

[1] FEC08, FT, P3-14
[2] TOFE08, Oral 23
[3] PPCF **47** (2005) B263
[4] Neumeyer et al, 2005.
[5] ReNeW Thrust 16, 2009.