NAS Community Information Paper:

Sustained High Performance Tokamak as the Leading Magnetic Fusion Path to Net Electricity Production

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Context.
The 2017 U.S. fusion community workshop ('Magnetic Fusion Research Strategic Directions') organizers approached the above authors to generate an information paper on perspectives from the community that addresses the 8 questions set out below. In fact, these questions go somewhat below informational, toward development of a strategic plan. It is our view that, the community has not yet converged on a common view due to time constraints. At this stage of the process, separate groups articulated different visions and ideas for the direction of the entire magnetic fusion program. Nevertheless, several common themes arose in the tokamak concept which leads the U.S. program. We have attempted to address the common elements in as representative a way as possible, as well as convey different possibilities raised. In many cases this also requires interpretation as to what the target of the research should be; we have also tried to draw this broadly.

1. **Description of the element and its potential role in U.S. MFE research strategy.**
Here we assumed "Sustained High Performance Tokamak" refers to a number of potential or alternative devices which we address together. This includes sustaining high performance in ITER, fusion nuclear science devices such as an FNSF or CFETR, a possible U.S. net electric D-T device such as ARC, ST Pilot Plant, or Compact-AT (each of which might integrate a nuclear testing mission), or an eventual low cost of electricity 1GWe+ power plant such as ARIES or SlimCS.

To achieve sustained high tokamak performance requires a number of program elements spanning both plasma science and technology. We list these below in no particular priority order except we single out the first element – Participation in ITER – due to the project scale, its construction status, and as it is arguably the most directly relevant to the study of burning plasma physics. This element received both substantial, but not unanimous support at the Madison and Austin meetings from many members of the community (and was supported in papers such as [1-4]). It also encountered some mixed critique that included the timescale over which ITER plasmas will become available and the cost of the project. The ITER project has declared [ref https://www.iter.org/newsline/-/2877] that construction has reached the half-way point toward first plasma operation, with Q=10 D-T operation projected to start at least a decade after that.

(i) **Participation in ITER** is invaluable in order to validate burning plasma physics and tokamak sustainment at larger scale parameters that are closer to future sustained tokamak devices, and to get first-hand experience in the design, construction and operation of a fusion production scale, nuclear regulated facility. Without this the U.S. would have to start from scratch to develop this know-how, and this could hamper effectiveness or require an additional device (such as an intermediate Q~2-5 pulsed tokamak) to develop the knowledge and experience.

In particular, ITER participation provides:

- direct engagement in engineering and design of a major nuclear fusion device at the reactor scale.
- Development of safe modes of operation and licensing from an engineering perspective
- Development of robust operational scenarios with the techniques to ensure control, stability, safe quenching, ELM mitigation, divertor protection, and the study of fully non-inductive regimes at the reactor scale.
- Validation of physics basis for phenomena at the reactor scale (higher field, current, stored energy, pressure, etc.) to assist projection and design of future reactors
- Study of the specific physics regimes of the burning plasma state, role of energetic particles, and self-heating.

The balance of the research and technology development elements listed below had strong support and arguably consensus [5]. Most of these research elements are needed for most potential future device concepts. <u>No particular priority order is asserted in this listing</u>.

(ii) **Understanding how to sustain a high performance plasma core.** To proceed with next steps exceeding ITER performance (such as fully non-inductive, or higher β or fusion performance), requires confident validation of the techniques needed for, and parameters of, the high performance core. This is particularly important if it is a D-T device. This comprises four key themes:

a. **Projecting and maximizing the burning plasma core performance**, which is crucial to determining required scale and auxiliary systems on a future high performance device. This requires work to understand transport, stability, energetic particle physics and pedestal behavior in relevant physics regimes such as $T_e \sim T_i$, coupled ions and electrons, relevant rotation profile and collisionality, β and current profile – most of the key parameters that govern this physics. Normalized plasma gyro-radius, ρ^*, the other relevant plasma parameter, reaches reactor levels in ITER.

b. **Continuous operation avoiding transients** – sustained stability avoiding disruptions and other deleterious events, such as ELMs and large scale MHD instabilities. Means to predict, avoid, and provide robust control of these events are vital to future reactor concepts. Techniques to achieve these either through self-consistent scenario solutions and/or active approaches such as use of 3-D fields, localized current drive, or particle injection facilities must be established in relevant regimes.

c. **Fully non-inductive self-consistent solutions and long pulse operation** must be developed, where the physics and performance of a non-inductive discharge are understood, and compatibility with required heating and current drive sources (and scale, B_T, I_P) is resolved for various future device options. This requires flexibility in research device parameters (β, density, q, shape) and plasma profiles (e.g. current, and pressure). The development of such regimes for long pulse operation (including such long pulse issues as wall compatibility) should also be pursued, including potential upgrades to present U.S. facilities and/or collaborative work with super-conducting facilities abroad.

d. **Compact designs** to produce low cost device solutions. Optimizing the device aspect ratio is one part of this endeavor [6] and is a main focus of the combined research effort of the major tokamak devices in the U.S. today [2,7]. The high field and high β approaches are other aspects of the optimization and potential paths to reducing facility size. Research should explore benefits and trade-offs in these approaches.

(iii) **Reactor-relevant current drive tools** need to be developed that achieve the high levels of efficiency needed for a net electric plant (if that is the U.S. goal), are compatible with the harsh reactor environment, and achieve good coupling to the plasma. Thus systems need to be developed and tested on present tokamaks including the needs for plasma start-up and current ramp-up in compact tokamak designs with a small, or no central transformer coil [4].

(iv) **A projectable & integrable divertor solution** is needed. A sustained device (beyond ITER) requires handling of the hot plasma exhaust 24/7, without erosion of divertor or wall materials, and with compatibility with the high performance core plasma. This requires first an understanding of detached divertor physics be developed (present simulations fail to capture such dense ionizing/recombining plasma behavior) to project required solutions, and second development, or at least validation, of improved divertor concepts. This also includes the call for resilient first wall power handling solutions, such as designs that include flowing liquids (e.g. lithium) or powder.

A critical issue is compatibility with the high performance core; it is easy to solve one problem without the other. Some of this can be explored on present devices. But fully understanding this interaction and the compatibility between core and edge requires simultaneously reaching reactor relevant core physics regimes (characterized by parameters such as collisionality) and divertor configurations (characterized by absolute density and heat fluxes). Exploring this requires performance upgrades to presently available devices, or new devices. A decision on this requires careful analysis as to how much can be gleaned from ITER, from present devices, and what is needed to provide confidence for a D-T device design.

(v) **Resolution of relevant materials.** Understanding what materials can cope with the sustained particle fluxes and nuclear bombardment in a reactor wall and divertor is important. Full development of this is in part the mission for a D-T FNSF / pilot plant device. But one must know what materials to build it from that are sufficient to sustain the mission. Invaluable lessons can be gleaned from test bed facilities, while interactions with relevant next-step tokamak plasma particle fluxes are also important. A program to develop and simulate novel candidate materials is important.

(vi) **High temperature, high field superconductors** are predicted to greatly leverage performance of a future fusion device [8,9]. Further, the possible potential for demountability would enable a device that could be opened up to test successive wall materials and breeding techniques, as part of the research mission of a combined D-T FNSF / pilot plant. For net electricity production, recent studies have shown that the very high current density and field strength potentially achievable using HTS tape and cable technology may lead to more viable compact superconducting magnet net electric tokamak Pilot Plants [7]. While small bore magnets using HTS have been constructed, large bore magnets that can handle the high stresses in a tokamak need to be developed. A program to develop these should be pursued as aggressively as possible.

(vii) **Reactor engineering and breeding concepts.** To prepare for and proceed with a U.S. sustained D-T reactor requires a much larger engineering effort to be initiated, to properly understand the challenges and constraints, and develop design solutions. Particular consideration and research is needed on breeding approaches, where the U.S. is a world leader, but needs to target effort on fusion reactor concepts.

2. **Benefits**; *e.g. end-product attractiveness, timeline improvement, gap closure, risk mitigation, leadership potential, education, etc.*

Progress on the missions discussed above would provide the basis for the U.S. to decide on a major new device to be located in the U.S. and to confront challenges towards sustained fusion energy. There appears to be a strong interest amongst the community for this to be a D-T device [3,4,7,8], through there is some debate over the scope of the mission (from FNSF to net electric pilot plant, combined with FNSF). Some discussion at Austin did not see a high priority for an intermediate Q device (Q ~ 3), whereas other individuals were more enthusiastic for this step.

The knowledge gained from these missions would equip the U.S. to be an expert and sought after partner in international ventures. A significant portion of the work would also prepare the U.S. to lead in the ITER program, resolving control, disruption prediction, avoidance and mitigation, and understanding of how to raise performance and sustain steady state, enabling the U.S. to validate physics and develop knowledge of how to translate ITER’s lessons to the U.S. fusion energy path.

3. Current status of R&D in this area, including readiness

The U.S. has two major tokamak facilities that can address much of the plasma physics and plasma solution development, with suitable upgrades and recovery. These are capable, flexible and sophisticated scientific instruments, with world-leading diagnostics and outstanding scientific teams – they are ready for this mission. This is clearly a cost effective path to resolve these issues. Some aspects need considerable performance extension (particularly the core-edge mission) which might be met by upgrades or motivate a new D-D device (including a divertor test tokamak, such as the ADX design, and/or D IV) – although there has been inadequate community discussion to date to reach consensus on what is needed.

The program implies a developing technology and engineering program that is not presently invested at the necessary level. This is an opportunity to grow the U.S. fusion energy program and re-establish confidence in its abilities, as well as solve critical problems that can accelerate and improve the path to fusion energy. Indeed it is vital to grow this program to re-orient the U.S. fusion science program to also be a fusion energy program, and to develop a coherent U.S. strategy to fusion energy.

4. **Programmatic context**; *what will or could be done outside the U.S. Fusion Program, i.e.., in industry, international fusion, other U.S. agencies, etc., in the next several years*

Some of the technological aspects could be developed by encouragement of, or collaboration with, private industry. There are opportunities for nuclear, engineering and materials issues to outreach to other fields such as high performance fission.

Continued strong engagement of international partners is vital, where they often bring unique and complementary elements such as superconducting long pulse conditions for stability, transport, and non-inductive current drive validation and with unique capabilities to assess wall behavior on materials evolution timescales (e.g. KSTAR, EAST), divertor flexibility (e.g. MAST-U, TCV), tungsten behavior (ASDEX Upgrade, JET) or size scaling (JET, JT-60SA). In addition, China's plans to proceed with CFETR will greatly assist development of nuclear technologies – a key strategic opportunity. Such international collaborative research, when conducted properly, is a very high benefit/cost path to fill key gaps in the U.S. program and to provide critical multi-device data for physics validation. Experience shows though that success in this collaborative approach vitally depends on several key factors: (i) appreciation and understanding of the culture, research, and business environments of the foreign host country, and (ii) the U.S. having something to bring to the table in terms of knowledge and investigative capabilities. Contributing to hardware, including diagnostic systems, has often been a successful engagement approach. US strengths in theory and modeling are also key avenues to enable successful collaboration.. U.S. goals cannot simply be met by attempting to drive a foreign host's program plans or priorities and buying plane tickets.

As argued in item (i), ITER participation provides unique and essential know-how if the U.S. wishes to pursue a reactor path without having to invest resources and additional time in developing these by itself in parallel. As ITER Director General Bigot says, ITER provides 100% of the learning for 10% of the cost.

5. **Possible 15-year U.S. research agenda**

A U.S. research agenda would foresee work on each of the seven missions highlighted in Question 1 proceeding for the next ~10 years or so. This would include ramping up activities particularly on technology elements, and thus building the U.S. program and its capabilities to close out research questions on each. It also implies targeted investment on tokamaks (rather than exhaustively running them without upgrades), or one or more new facilities, to close out remaining issues.

At some point the potential to assemble the developed solutions in each mission into a viable overall concept device will become apparent – a decision point would then be reached to decide to proceed with a particular device, with sufficient knowledge to determine the principle parameters, systems and mission of that device. Engineering design could then commence. This device might be ARC, C-AT, Spherical Tokamak Pilot Plant, or an FNSF as examples. Exact device parameters and mission are an outcome of this 15 year mission and of ongoing U.S. strategic considerations.

To determine these choices, the entire U.S. magnetic fusion community must continue to be engaged in an open, focused dialogue to reach consensus defining the specific paths forward. Researchers that span the spectrum of facilities – from university to national lab – must be engaged in the process to define the U.S. fusion program.

The exact requirements for this decision point depend on the nature of the mission for this new device. This is not yet converged within the community. If a steady state pilot/nuclear testing D-T device is envisaged, this requires stronger progress in the missions. Other ideas have been proposed for more restrained D-T devices (e.g. SPARC or a more restrained FNSF) or even a D-D device. Timescales to get to these decisions are to be determined, but earnest discussion should start now.

6. **Research directions beyond the 15-year horizon**

If the U.S. were to reach the above decision point for a major sustained D-T pilot/nuclear testing device within 15 years, and gains the relevant knowhow expected from ITER, the path beyond 15 years is somewhat clear – design and construction in the 2030s, for operation in the late 2030s or early 2040s. Such a device would operate at similar or lower Q to ITER, and does not need to await ITER D-T operation prior to commencement of design. If a pilot plant, this would likely have a phased mission with initial short pulse performance/net electric tests, followed by long pulse nuclear testing and breeding development with possible wall and breeding change-outs periodically. It is hoped that success in the research mission of this device would at some point spur industry investment to either develop further technology developments or attempt competitive-COE devices. It was mentioned at Austin that private investors want to see magnetic fusion reach for the goal of net electricity production to dedicate significant venture capital to this research [10,11].

With other future facility options (e.g. D-D, medium Q D-T) there might be a longer path, as research in these devices would lead to successor long pulse and/or net electric devices. Several designs for tokamaks at this level already exist, which might enable further progress on the seven research areas/gaps stated in Section 1. All paths benefit from parallel engagement and U.S. leadership on ITER as well as other international tokamak collaborative research activities, to bring the experience gained in ITER and non-U.S. devices back to the U.S. domestic program and fusion energy path.

7. Conclusion

To conclude, what seems clear to these writers (but may not yet be community consensus) is that a variety of possible paths to fusion energy based on the tokamak now have largely common near term (<15 year) research needs due to the relative maturity of tokamak research. This is set out in the seven missions set out in section 1. Maintaining focus and vigorously pursuing these missions will enable a decision on the form of the future D-T device and path. It will also provide continued unique U.S. expertise and leadership in the world fusion program, and the potential to accelerate fusion energy.

8. Critics' objections and advocates' responses.

There is not consensus on answers here, but some examples are given:

"The tokamak will disrupt – build a stellarator": This is a highly important misnomer! "Minor disruptions" or significant thermal quenches have been shown to occur in large stellarators such as LHD at high performance ($\sim \beta = 5\%$). Equally as important is realizing that advanced tokamaks can have very low disruptivity – even with relatively rudimentary systems for disruption avoidance. For example, the JET device over many years of carbon wall operation reduced disruptivity to under 4% without leveraging the technologies now either being used or studied for disruption avoidance. This is already close to the 1 – 2% disruptivity goals that ITER has set for thermal and electromagnetic stresses on the device. An analysis of DIII-D discharges shows disruptivity approaches zero at the q_{95} values proposed for steady state operation [Garofalo FED 2014]. Further disruption prediction, avoidance, and innovative quench methods are now possible and are being pursued with high priority research in the U.S. program. The tokamak is the primary choice of most ITER partners for the magnetic fusion path because it is judged by them to be the most possible and earliest path to fusion energy. Nevertheless, stellarator development is important as it may ultimately offer a more attractive solution.

"Combining net electric into FNSF mission makes it too expensive" – advanced approaches to tokamak design including HTS and/or natural advantages demonstrated by the spherical or advanced tokamak approach enable net electric to be achieved more compactly, making an integrated device more viable and more compelling, thereby removing a generation to fusion energy.

"Q_{DT}~1 or Q_{DT}-eq~1 are PR points" – to some degree this is true, but achieving these does mean your device is likely operating in reactor relevant physics regimes from core to the edge, and thus able to address physics challenges of a D-T reactor.

"this depends too much on HTS – single point of failure" – Untrue. HTS is a relatively recent technological innovation and many tokamak designs aiming for net electrical production do not rely on it. By conducting the set out package of research, we will establish the possibility for all optimizations, and so could modify the path if HTS is found not to be viable. Nevertheless, the advantages that could be had by HTS are highly compelling, and so should be pursued.

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