Entering the Burning Plasma Frontier

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1. Description

The National Academy of Science' Committee on a Strategic Plan for U.S. Burning Plasma Research was tasked to develop a strategic plan "for a national program of burning plasma science and technology research which includes supporting capabilities and which may include participation in international activities, given the U.S. strategic interest in realizing economical fusion energy in the long term.[1]" The panel's interim report[2] states that "burning plasma research is essential to the development of magnetic fusion energy and contributes to advancements in plasma science, materials science, and the nation's industrial capacity to deliver high-technology components." Further, "any strategy to develop magnetic fusion energy requires study of a burning plasma." Hence, we believe the importance of burning plasma and its place in fusion energy development has already been established, and the main point of this white paper – that *a burning plasma step is the logical and necessary next frontier for the US Fusion Energy Sciences program* - is largely non-controversial.

In the sections below, we will describe the benefits and importance of the burning plasma step, and briefly describe options for providing access to a first burning plasma experiment for US researchers. It should be stated that while the authors may not agree on the *relative likelihood of completion* of the described facilities, there is general agreement that *any of the described possibilities, if brought to fruition, would fulfil the basic need for a burning plasma experiment*.

2. Benefits

Burning plasmas are the critical next step in the development of fusion as an energy source for the future. Successful production of large amounts of fusion power presents both great challenges and great opportunities. A successful burning plasma experiment will (1) begin to establish the feasibility of fusion as a potential energy source; (2) provide the scientific and technological basis for aggressively pursuing next steps on the path to fusion energy; and (3) provide US researchers with opportunities to carry out world-leading scientific research in a heretofore unexplored parameter space.

The US has long held a leadership position in fusion energy science due in large part to a unique emphasis on scientific understanding backed up by developments in physics-based models validated by innovative diagnostic measurements. US progress in burning plasma science, either through continued participation in ITER or a domestic burning plasma experiment, will be essential in maintaining leadership in plasma science and fusion research. In addition, a burning plasma experiment will be an entry point for the US to participate in development of materials and technologies essential for a fusion power plant.

Access to the first burning plasma experiment will provide opportunities to address a set of grand challenges for plasma science and technology, including:

- How will a large population of energetic α particles in the bulk DT plasma behave?
- How will a self-organized plasma develop, in which most of the heating power is provided by the fusion reactions themselves?
- What control schemes and actuators will be effective for controlling that self-organized state and allowing optimization of that state?
- How will materials behave in the burning plasma environment?
- How can we make measurements in this new, harsh, environment?

- How can we control power and particle exhaust, including the effects of plasma physics, divertor geometry, and plasma-facing materials?
- Can we develop the self-consistent, steady-state capable, operating scenarios that will probably be required by the eventual customer (power utilities)?
- Can we develop the necessary blanket technology for tritium breeding (subsequent burning plasma devices will almost certainly have to demonstrate tritium self-sufficiency) and capture of the fusion reactions' energy?
- What materials and technologies will be able to withstand the hostile environment of a fusion reactor?

Scientific or technical leadership requires two things: Acknowledged expertise, and resources to develop that expertise. Although the US FES program does not have the resources to ensure leadership in all areas of fusion development, we are leaders in several areas critical for progress in burning plasmas, including transient control, general plasma control, operating scenarios, energetic particle physics, and integrated modeling, to name a few. There is a common theme in that our position in all of these areas relies on a strong scientific approach that includes sophisticated measurement (a US leadership area in its own right) and model validation. That theme is a recognized hallmark of US fusion research.

Continued progress toward fusion requires that we take a bold burning plasma step. Such a step has several benefits: It pushes progress toward fusion energy, maintains US leadership in at least some areas, and provides exciting new opportunities for discovery science. The alternative is to cede leadership to our international partners, and eventually allow them to become the world's suppliers of fusion energy technology.

3. Current status

The US is currently engaged as a partner in the ITER project, along with China, Europe, India, Japan, Korea, and Russia – a partnership representing over half the population of the planet. The mission of ITER is to demonstrate the technological and scientific feasibility of fusion energy by:

1. Producing 500 MW of fusion power for pulses of 400 s
2. Demonstrating the integrated operation of some of the technologies for a fusion power plant
3. Achieving a deuterium-tritium plasma in which the reaction is partially ($P_{alpha} = 2\ P_{heating}$) sustained through internal heating
4. Operation and optimization of tritium and deuterium plasma fueling, exhaust, processing, and control at large scale.
5. Demonstrating the safety and licensing characteristics of a fusion device

Although the ITER project started slowly, in the three years since Bernard Bigot was named Director General, ITER has developed a resource-loaded schedule and kept to it, achieving all major milestones on time. This schedule has ITER on track to produce its first plasma in 2025, begin its research program in 2028, and operate with the deuterium-tritium (DT) fuel mix needed to produce a true burning plasma in 2035. The US' share of ITER's construction cost is 1/11, but the US Fusion Energy Sciences community will have access to 100% of the scientific and technical output of the project (results from the Test Blanket Module Program are a possible exception as the US withdrew from that several years ago).

As a first-of-a-kind facility, there is a great deal of technical complexity involved in successfully completing and operating ITER. There is a presumption that the design is now sufficiently advanced that there are plans in place to address these complexities; and this has been borne out by the excellent schedule performance demonstrated by the ITER project during the last 2-3 years. In November 2017, ITER reached the 50% mark in construction leading to first plasma. However, the recent funding shortfalls for the US' ITER contribution are threatening the schedule; Director General Bigot recently informed the Energy Subcommittee of the House Science Committee that if the US does not uphold its commitments, ITER might have to announce a delay as early as June 2018, which would have a cascading effect on the entire project and the costs to our partners. It is encouraging that the recently approved FY-2018 appropriation for ITER improves the prospects of forestalling such an outcome.

There is currently no DOE-funded activity within the US to consider alternative approaches to a burning plasma, either in addition to or in place of ITER. Prior to the 2002 Snowmass Summer Study[3] two other alternatives were considered: Ignitor[4] and FIRE[5]. The Snowmass Summer Study judged both ITER and FIRE as viable burning plasma elements in a roadmap leading to practical fusion energy:

> *An international tokamak research program centered around ITER and including these national performance-extension devices has the highest chance of success in exploring burning plasma physics in steady-state. ITER will provide valuable data on integration of power-plant relevant plasma support technologies. Assuming successful outcome (demonstration of high-performance AT burning plasma), an ITER-based development path would lead to the shortest development time to a demonstration power plant.*
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> *A FIRE-based development plan reduces initial facility investment costs and allows optimization of experiments for separable missions. It is a lower risk option, as it requires "smaller" extrapolation in physics and technology basis. Assuming a successful outcome, a FIRE-based development path provides further optimization before integration steps, allowing a more advanced and/or less costly integration step to follow.*

4. Programmatic context

A fusion power plant will need to operate at very high gain, ideally Q=30-∞. The challenge of operating in the dominantly self-heated regime has been documented, in particular within the "Thrust 8" defined by the 2009 Research Needs for Magnetic Fusion Energy Sciences (ReNeW) study[6]. Several present and previous tokamaks have operated at near-unity (0.3-0.65) fusion gain including two (TFTR and JET) that demonstrated significant fusion power for short periods of time with DT fuel and carried out the first "weakly burning" experiments that demonstrated: alpha confinement, alpha heating, alpha slowing down velocity distributions and transport of alpha ash[7]. A first dominantly alpha-heated "burning plasma" device with Q=10 (where 2/3 of the plasma heating power is self-generated by fusion reactions) operating point anticipated for either ITER or FIRE is a large improvement over the current state-of-the-art, giving us an opportunity to start to learn how to control a burning plasma before moving to higher gains with increasing levels of challenge (it should be noted that higher gain is not precluded in either ITER or FIRE).

One might also consider a non-tokamak device for a burning plasma mission, but the most advanced such concept, the stellarator, has additional gaps that must be filled first. In particular, a proof-of-principle level device demonstrating tokamak-like confinement has not been established in the US program, although there have been proposals for such a device. It seems unlikely that a burning plasma stellarator could be operated in the 15-year timeframe.

Subsequent to the first burning plasma, two major missions have been identified that must be addressed prior to a demonstration power plant, presumably in new facilities that could be built during the DT phase of a Q = 10 burning plasma device, a high-gain (Q>30) mission to develop techniques to control a dominantly alpha-driven fusion plasma, and a fusion nuclear science facility to develop materials and technology.

5. Possible 15-year US research agenda

For a large portion of the 15-year timeframe, existing US devices (DIII-D and NSTX-U) will continue to operate, addressing issues that will help to prepare us for operation of a next-step burning plasma device. Present-day tokamaks in the US (DIII-D, C-Mod) and elsewhere (JET, ASDEX-U, JT-60U, EAST, KSTAR,…) have already demonstrated most of the dimensionless parameters anticipated in a first burning plasma device, with the principle remaining core parameter being $1/\rho^*$, the effective size of the plasma, and the caveat that it does not appear possible to simultaneously match both the core and edge parameters of a burning plasma device since the two sets of parameters scale differently. In dimensional terms, however, plasma conditions in present day devices remain quite far away from their anticipated burning plasma descendants. To reach the burning plasma state, simultaneous achievement of high density and high temperature is required, leading to very high energy density plasmas. Transient events in burning plasmas will therefore potentially lead to much larger energy releases, making the need for transient control for machine protection more critical. Work on present day devices is still needed to establish the ability to control transients (disruptions and ELMs), and to couple "reactor-relevant" divertor conditions (which scale differently than core parameters) to a burning-plasma core. Also, many aspects of operating scenarios for a burning plasma environment can and should be developed in these devices in order to minimize risk to the larger, more energetic, burning-plasma devices. To the extent possible, these capabilities are being developed and demonstrated on present-day devices (with the US taking a leadership position, especially in all aspects of transient control), but the remaining scientific gaps can be represented as research opportunities in the burning-plasma tokamak.

During the early operational phases of a burning plasma device (presumably during non-activated commissioning operations), the results of these studies on present-day devices can be applied and tested prior to full-power DT operation. It will likely be desirable to maintain at least one of the present-day devices as a test bed to evaluate new operating scenarios and control techniques in a relatively low-risk environment before deployment in the burning plasma device.

The task given the NAS panel calls out two cases; with and without ITER as part of the US Fusion program. In either case, a burning plasma demonstration at fusion gain $Q \geq 10$ is needed. The two plans address how the US MFE program will access the critical burning plasma knowledge and experience to inform the next step facilities required to proceed toward fusion energy. We may presume that if the US withdraws from the ITER project, it will not have access to the experimental and technical information developed in the ITER program, but that ITER will likely continue as an international project.

Case 1: The US continues as a partner in ITER

ITER will provide the initial burning plasma demonstration starting in 2035 and continue as a critical element of the US Fusion program through the 2040s. The timeline for ITER (Fig. 1), including both a construction schedule[8] and a research plan[9] through the first DT operation, has been established, but it relies on appropriate resource allocations from each of the seven ITER partners.

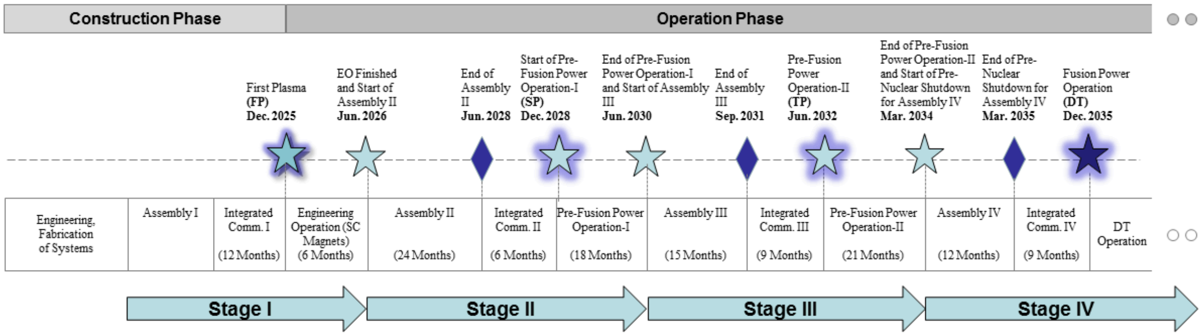

Fig. 1. The ITER schedule and Research Plan envisions first plasma at the end of 2025 and a staged series of research phases culminating in DT operation at the end of 2035.[10]

Although 2035 seems a long way off, ITER will provide opportunities for non-activated (He/H plasmas) fusion science studies at unprecedented scale and parameters as early as 10 years from now. A staged approach was developed by the ITER Organization in collaboration with the communities of the seven ITER partners, starting with first plasma in December 2025, and succeeded by a progressive upgrade of the capabilities of the ITER tokamak and facility interleaved with two periods (Pre Fusion Power Operation-1 in 2028-2030 and Pre Fusion Power Operation-2 in 2032-2034) of system commissioning with plasma and experimental plasma studies in H and He plasmas. Following the end of the second such phase, the last few systems needed for D and D-T operation (primarily the tritium processing capabilities) will be completed, so that ITER will be ready to move on to these experiments starting in 2035.

Although details of how the research collaboration will be organized have yet to be determined (US community input has been prepared[11]), but presumably the final determination will occur in negotiations between the US Domestic Agency and its partners), the US will be a full participant in ITER research and will have access to all of its scientific and technical data (the lone exception being the Test Blanket Module program, in which the US is currently not a participant). As is the case with large present-day tokamak facilities (e.g. DIII-D, NSTX-U, JET, EAST,…), it is to be anticipated that ITER will provide opportunities for a large number of US researchers to actively participate, both on-site and remotely.

While ITER construction continues, and even through the early non-activated operational phases, US research can and should continue to inform ITER in numerous areas that have been strengths of our program, including disruption avoidance and mitigation, ELM control, plasma control, operating scenario development and qualification, stability control, error field identification and compensation, and integrated model validation, to name a few. None of these issues are specific to ITER, but US leadership in these areas has served to elevate our program's stature within the ITER community.

While continuing to operate our own facilities that exploit our strengths, we should also continue to leverage the capabilities of our international partners' facilities with complementary capabilities. In particular, EAST and KSTAR have provided and should continue to provide US researchers with significant opportunities to explore long-pulse operation. The US should also seek roles on JT-60SA, which should have capabilities to push long-pulse operation to higher fusion parameters.

Case 2: The US does not continue as a partner in ITER

In the event that the US withdraws from ITER, the US MFE program can recover the burning plasma science and target US focused goals toward an attractive fusion power plant. Three options appear the most accessible, (1) US designs, constructs and operates its own burning plasma experiment focused on high Q, high bootstrap fraction f_{BS}, and 100% non-inductive current, (2) as part of a fusion nuclear science facility step, the burning plasma step is added to the device's mission as a front end, targeting the same performance of maximal Q, high bootstrap current fraction and 100% non-inductive current, or (3) US pursues a stellarator path in which a first step is a physics proof of principle facility, followed by a burning plasma facility. All options represent a complementary path to ITER and the remaining international parties' programs.

Option 1: The US could pursue a **high field compact burning plasma facility**, most likely with high strength copper coils. A recent example of such a design is FIRE[12], conceptually designed in the early 2000s, and considered an attractive option to pursue burning plasma science in the Snowmass 2002 Report[3] with a lower risk than ITER due to smaller extrapolation in physics and technologies. Power plant relevant technologies are generally not a focus, since the goal is a rapid design, licensing, construction, and operation to establish the scientific plasma basis to continue on a fusion energy development path. Since the US energy market will not entertain technically or economically unattractive power plants, a US burning plasma experiment would specifically develop high fusion gain ($Q > 30$), high bootstrap current fraction ($f_{BS} > 65\%$), and 100% non-inductive current plasmas, all known to contribute to high fusion power density and low recirculating power, and consequently economic attractiveness[13,14,15]. It is critical to understand the nonlinear plasma regime and identify the limits to control for a power plant. The device's high magnetic field at 10 T in the plasma and high density, would also contribute to the potentially attractive high-density plasma operating regime ($n_{Gr} = I_P/\pi a^2$, at small a).

As an example, an increase in linear dimensions of the 2004 FIRE design, by 10%, while maintaining the magnetic field (in the plasma center) at 10 T, an isomorphic transformation that maintains the same coil stresses, increases the plasma fusion gain to >30.[13] This FIRE-like device is very compact and would have a plasma volume comparable to TFTR and JET and only 4.3% that of ITER. The tritium inventory would also be comparable to JET and TFTR resulting in safety regulations, and licensing complexity, much less onerous than ITER.

The timescale for optimizing the design, licensing, and construction is estimated to be <20 years. According to its proponents, the cost of such a Super-FIRE, escalated from the 2004 FIRE design[12], for an expanded mission of high Q, and by inflation leads to ~\$2 B[13]. Physics activities would include all the same topics presently studied for ITER, as well as engineering design and engineering-physics interface design. The potential locations identified in the FIRE study were ORNL and INL, due to strong electrical supplies and established infrastructure for nuclear and large scientific projects.

All of the interim activities mentioned in the previous section on ITER would also be the focus for the US fusion research program preceding a domestic burning plasma experiment.

Option 2: A **fusion nuclear science facility**[16] is anticipated as an intermediate step from a burning plasma to a demonstration power plant (DEMO), in order to establish material and component behaviors in the fusion nuclear environment, tritium self-sufficiency, power plant relevant operating conditions, and a wide range of enabling technologies. This facility requires a burning plasma to achieve its mission. It is natural to **attach a burning plasma program element to the facility at its front-end**, to provide the burning plasma scientific and operating knowledge required. This would have to be designed into the fusion nuclear facility, and would have the same goals of maximizing Q, high f_{BS}, and 100% non-inductive plasma current, even if the fusion nuclear mission did not require all of them simultaneously. The fusion nuclear facility requires considerable R&D as pre-requisite to its construction and operation, and would likely not operate for ~25 years at the soonest. Staging of the burning plasma prior to this could take advantage of the same lifetime fusion components like the vacuum vessel, CS/PF/TF magnets, and cryostat, but use a different fusion core (e.g. shielding). Since the fusion nuclear mission involves ultra-long plasma operation, the coils are superconducting. An advantage of the fusion nuclear facility over most burning plasma facilities is that it is nuclear-ready, pursuing ultra-long plasma pulses (weeks), and remotely maintained, eliminating issues associated with activation. The site's infrastructure would be efficiently utilized. At present the only qualified sites for such a facility are ORNL and INL.

Again, the staged burning plasma / fusion nuclear facility would mobilize the US fusion community broadly as optimized steady state plasma physics, burning plasma physics, and fusion nuclear engineering science must converge to produce the multi-function facility program.

Option 3: The stellarator may offer advantages to the tokamak, and the US can pursue the **quasi-symmetric (QS) stellarator path**, a configuration favored by US scientists[17]. The first step in the program is a physics proof of principle facility that establishes the plasma physics behavior and scientific basis. This is then followed by a burning QS stellarator facility (e.g. TFTR/JET scale) based on the results of the previous one. The first facility requires design and construction taking < 10 years, with ~ 15 years of operation. The following burning plasma facility can be designed in parallel with the later part of physics performance operation, and licensed and constructed in ~ 8-10 years.

The stellarator path would complement the ITER tokamak path and non-QS stellarator path taken by international partners. The experiments would likely cause a stellarator community to grow considerably in the US. The US tokamak community would participate in tokamak development around the world.

6. Research directions beyond the 15-year horizon

At the end of 15 years we are unlikely to have demonstrated a burning plasma under any plausible scenario. However, with assumptions of resource availability and no scientific or technical surprises, we can be positioned to proceed to that goal soon thereafter.

Case 1: The US Continues as a Partner in ITER

Completion of ITER construction activities is scheduled for early 2035 and the transition to experiments in D and DT plasmas is planned for December 2035, with trace tritium experiments likely in early 2036 and a gradual transition to fusion power production over the next 12 – 15 months of experimental studies, leading to an initial demonstration of several hundred megawatts of fusion power production for several tens of seconds. In subsequent experimental campaigns in DT plasmas, planned on a two-yearly cycle, the experimental bases for achieving the principal scientific mission goals of the ITER project are developed: a demonstration of $Q \geq 10$ for burn durations of 300 – 500 s and development of long-pulse, non-inductive scenarios aiming at maintaining $Q \sim 5$ for periods of up to 3000 s.

ITER has been designed with conservative physics assumptions, so that there is high confidence that the single operating point goal of Q=10 and 500MW for hundreds of seconds can be achieved. A range of predictions have been made of ITER's fusion performance, including some that exhibit somewhat higher, and lower, gain than that single operating point. The possibility of applying ITER to a higher gain mission exists, although it requires some combination of optimistic physics and later facility upgrades that to date have not been seriously considered. Along those lines, it should be noted that several operating scenarios that significantly outperform the "conventional H-mode" baseline scenario have already been demonstrated. Such facility improvements would have to be negotiated with our partners at some later time.

Case 2: The US Does Not Continue as a Partner in ITER

Option 1 (DT operations in a high field compact burning plasma facility): In this time frame the US burning plasma experiment would complete its construction and begin operation. After an appropriate shakedown in He/H and DD, the DT experiments targeting high Q, high f_{BS}, and 100% non-inductive plasma current would begin. Apart from exploring and establishing burning plasma physics knowledge and operating experience, the limits and control of the highly nonlinear regime described by $P_{alpha} >> P_{aux}$ and $I_{BS}/I_P > 0.65$ is fundamental to establishing the plasma relevant to power plants. A wide range of plasma physics thrusts would be explored including fast particle confinement, fueling/exhaust and particle control, core plasma and divertor power handling, plasma transport in the burning regime, MHD, and plasma materials interactions and plasma facing components.

Option 2 (DT operations in a fusion nuclear science facility): The burning plasma phase of the fusion nuclear science facility would begin operation with the same goals of maximizing Q, f_{BS}, and 100% non-inductive plasma current operation in order to explore and establish this regime for power plant operation. This program would be focused in order to advance to the fusion nuclear mission in a timely way and would explore the same topics as listed in Option 1. The break-in fusion nuclear mission would begin and establish the technical basis for DEMO and power plants.

Option 3 (ending physics phase and start of DT operations of a quasi-symmetric stellarator burning plasma): The plasma performance phase would end and the burning plasma phase of the QS stellarator would begin. The same burning plasma exploration activities would be pursued as in Option 1, with some differences associated with the stellarator. This would provide a basis for making a tokamak or stellarator fusion nuclear facility.

Longer-term needs in either case

Assuming the first burning plasma device does not achieve the high-gain (Q>30) mission, a new or upgraded high plasma gain burning plasma facility would complete construction and begin operation in this phase. Since the target is high Q, it complements the ITER goal of $Q \sim 10$, providing critical physics knowledge needed by our international colleagues in fusion. Based on the US emphasis on high performance plasmas in order to make an attractive power plant, the configuration would also focus on higher bootstrap current fraction and steady state. This is a necessary step to provide the opportunity to develop control strategies for a challenging scenario where the plasma largely provides its own heating (fusion alphas) and current drive (bootstrap).

At least one additional step will be needed prior to a demonstration power plant (DEMO) in order to explore the complete fusion nuclear environment for fully integrated components (e.g. tritium breeding blanket, divertor, RF launchers) and their materials, demonstrate a closed tritium fuel cycle, demonstrate the ultra-long plasma pulses at high performance required for power plant operation, test the numerous enabling technologies required to support the plasma and subsystems, and numerous other functions. As described in some of the “case 2” scenarios above, such a fusion nuclear facility might absorb the burning plasma step into the front-end of its operating program (this would have to be planned from the outset). Since the fusion nuclear aspects of the facility require an increased research and development program preceding it, this facility may be delayed with respect to DT operation of a Q=10 burning plasma device.

7. Critics’ objections and advocates’ responses

A. We’d be happy with a Q≈1 device now and that will build support so we can build a Q≥10 device later
  - JET and TFTR both demonstrated DT performance below unity, but of similar order of magnitude. JT-60U actually exceeded breakeven conditions, albeit with DD fuel. Demonstrating breakeven with DT fuel would be a first, but is actually only a small step in terms of plasma physics from our present status.
  - At Q=1, only 17% of the heating power is provided by the fusion reactions. Although this is sufficient to begin studies of the behavior of energetic alpha particles in the hot plasma, it is insufficient to begin studies of the challenge of control in an alpha-dominated environment. At Q=10, 2/3 of the heating power is coming from fusion alphas.
  - Since a subsequent step would still be needed to demonstrate Q≈10, building a low-gain device now would only divert resources and delay that step.

B. The tokamak approach is not likely to lead to an attractive fusion power plant… we should return to exploring an array of concepts on a smaller scale.
  - The tokamak has demonstrated performance that appears to extrapolate to a *viable* fusion power plant. The tokamak community is well aware of the need to make it *attractive*, and there are numerous innovative avenues being pursued to accomplish that.
  - Nothing here should preclude having a broader portfolio of concepts, but if that broader portfolio comes at the expense of advancing a “mainline” concept, the stated goal of “economical fusion energy within the next several decades” becomes very unlikely.
  - The numerous fusion nuclear science areas that must be developed for a tokamak, are in fact generic to virtually all configurations that employ the DT fuel cycle. This R&D can and should be enhanced to prepare for the long term.

C. Why shouldn’t the US focus strictly on the scientific aspects of fusion, generally benefiting foreign fusion efforts, and defer any US fusion energy goal indefinitely?
  - This is not a technical question… we can certainly choose to allow others to do the research and development, and ultimately reap its benefits. This probably positions the US as an eventual customer for fusion energy systems produced by other countries.
  - National pride is not the only reason to lead – scientific and technical leadership has put the US in a preeminent position in the world, and the cost of ceding that leadership is real.

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