# Delivering key technical achievements, then DEMO

A report by the Working Group for Strategic Approach 2 (SA-2) for the
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# 1. Introduction

This report presents ideas for potential paths forward for the U.S. Magnetic Fusion Energy Program based on strategic approaches that address the following charge:

> *"Deliver key technical achievements (e.g., Qplasma > 1, Pelec > 0) as soon as possible, then optimize concept and develop technology for DEMO*"*
>
> **DEMO is not a specific device or concept but rather simply a marker for the U.S. having the capabilities in place to move forward with the practical demonstration of fusion energy. Example DEMO characteristics could include tritium self- sufficiency, net electricity production, and high availability.*

Since there is not community consensus on a single best approach, we identify several possibilities for key technical achievements and outline the broad research objectives needed to achieve each goal. The proposals are to:

1. Achieve Q>1 as soon as possible
2. Achieve Q>30 as soon as possible
3. Achieve a closed tritium breeding cycle
4. Achieve net electric power and pursue a nuclear science mission (pilot plant)

Proposed achievements #1, #2, and #4 are presented here as separate strategic approaches, though they need not be independent of one another. The U.S. fusion program could create a strategic plan that integrates several of the proposed technical achievements in a staged approach, for example using a flexible device that can pivot its mission through a sequence of upgrades. The emphasis in the proposed strategic approaches is to create a path that enables novel achievements on a relatively short timescale to enable the U.S. to achieve fusion energy in the next several decades. A presentation summarizing the proposed achievements can be found here: USMFR SA-2 Presentation - Austin, TX.

# 2. Achieve Q>1 as soon as possible

*written by Nathan Howard*

The world fusion program has demonstrated no progress (in the form of triple product) since the JET DT campaign (1997). The lack of an easily digestible achievement (by congress & the public) fundamentally limits our ability to inject additional money into the program and advance development of MFE. The lack of such an achievement combined with ITER's schedule slippage (first plasma slipped from 2016 to 2025 – in approximately a 12 year period) has stagnated the US fusion program and put US fusion in a dangerous position where we are tied to the fate of ITER. A flat US fusion budget has led to an infighting for resources which prevents the development of a community consensus and in turn hinders our ability to advocate congress for more resources to advance MFE. The demonstration of Q>1 is an easily understood achievement that has the ability to fundamentally change the narrative that "fusion doesn't work". By changing the narrative, the field will be able to attract increased public and congressional support that will ultimately translate to higher budgets and the ability for the US fusion community to explore parallel research paths – a necessary requirement to speed up the development of fusion energy. Although fusion experiments in the 90's were able to approach plasma breakeven, it is important to note that TFTR and JET did not achieve a demonstration of Q=1, and perhaps more importantly, both machines promised to achieve beyond plasma breakeven and failed. Independent of the ITER decision, the US needs to build a Q>1 device as soon as possible, to demonstrate clear progress towards fusion energy development and regain credibility for our field.

### 2.1. Description of Strategic Approach

In this strategic approach, we propose a path centered on the goal of achieving a demonstration of Q>1 as soon as possible. With an emphasis on speed, construction of a high field tokamak provides the most realistic option. Essentially, one is able to build off of the well-established physics basis of the FIRE device but shrink the overall scope and size of the device to achieve a low plasma gain (Q of approximately 2-3). Using the empirical H98 confinement scaling it can be shown that a mid-sized (R = 1.5 – 2.0) tokamak operated with an on-axis field exceeding 10T should be able to demonstrate a low value of plasma gain, creating a "Q of a few" device. The moderate size of this device would place it firmly within the parameter space of existing tokamak facilities (DIII-D, KSTAR, JET, JT60-SA). As a result, an accelerated timeline for device design/construction is feasible and estimates of cost are likely more reliable. Such a compact device, operated at a high field will push into reactor relevant values of divertor heat flux. This is both an advantage and disadvantage as it allows for the study of reactor relevant heat fluxes to divertor targets but would also require an advanced divertor solution to be incorporated into the design to allow for reliable for operation. The use of a compact device presents a tremendous advantage from the standpoint of nuclear licensing. A small device can operate with very small amount of on-site tritium (the same approached used for TFTR operation), which allows it to avoid a full nuclear license which would inevitability delay the project. Although it is anticipated that the development of coils based on High Temperature Superconductors (HTS) will likely represent a game changer in the development of fusion, at this time, such coils have not been demonstrated and reports from the NAS community workshop at Austin, suggest that this development may be up to 7 years away. As a result, the proposed development of the "Q of a few" facility proceeds using established, cryogenically cooled copper coil technology which was demonstrated at similar fields on the Alcator C-Mod device (at a smaller physical scale) – with a planned upgraded to HTS as the technology becomes available. A simplistic estimate of the total device cost of such a device can be obtained using an established relationship between the overall mass of the tokamak core, (coils, vessel, cryostat) and the cost. The estimated cost per ton is approximately 1M USD/ton, which leads to an estimate of 500M – 1B USD for development of this "Q of a few" device. We note that these estimates are well below the cost of ITER and could be possible within the present funding for US Fusion Energy Sciences when spread over approximately 5-10 years for design & construction. The mission of this approach would be the demonstration of Q>1 on a decade timescale.

### 2.2. Timing, and Decision Points

An aggressive US program, focused on development of a “Q of a few” device within a decade is outlined in Figure 1. This approach features two phases of operation for the “Q of a few” device. In Phase 1, approximately 2-3 years would be dedicated to the design of the device, followed by 5 years of construction and 2-3 years of D-D and D-T plasma operations for demonstration of plasma breakeven in a compact, high field, copper coil based device. If this path were initiated today, demonstration of plasma breakeven would occur in the 2026 - 2028 timeframe, well before D-T operation of the ITER (scheduled 2035) and would occur in a device a fraction of the cost (~1/50th) and size. The proposed timeline to design, construct, and operate a D-T tokamak in a decade has been criticized as being unrealistic. However, it should be noted that there are multiple existence proofs for such a timeline and scope (JET and TFTR). TFTR was designed and constructed in approximately 7 years and it took only 11 years from the design phase to supershot operation. Therefore, an aggressive timeline is achievable if the political and community will exist. Design, construction, and operation of the first phase of the “Q of a few” device would occur in parallel with elements of the existing US fusion program, both to maintain US expertise during the construction period and to continue existing research paths which support the development of fusion energy as a whole.

*Figure 1. Timeline of “Q of a few” device.*

After demonstration of plasma breakeven in the “Q of a few” copper device, one could envision a transition to Phase 2 of the project. This phase would an upgrade the device by removing the existing copper coils and replacing them with demountable HTS coils, likely increasing the overall field & performance – ultimately pushing towards a device capable of closer to net energy gain ($Q_{eng}$ approaching 1). Although not all the components would be transferable from the copper to HTS based device, if planned in advance, a large amount of the site infrastructure could likely be reused during the Phase 2 upgrade. The start of Phase 2, represents a critical decision point. Prior to device upgrades, the fusion community should decide what crucial physics and technology questions must be answered to inform the eventual design of a DEMO or pilot plant. Possible directions include steady state tokamak development, an FSNF-like mission that involves development of tritium breeding and nuclear materials, or PMI and divertor research. Paths which are not addressed by the upgraded "Q of a few" device should be pursued in parallel at this time to reduce the time until development of a pilot plant is feasible. A careful survey of the world and domestic US programs should be used to determine the most impactful path. A proposed upgrade to the "Q of a few" device should be used to address open issues, all while leveraging the existing components of the original device. The hope would be that the progress made in Phase 1 and Phase 2 of the strategic plan would enable reliable design of a pilot plant or DEMO reactor.

## 2.3. The Risks and Benefits

There are clear risks and benefits associated with the strategic approach presented here. Disadvantages of this approach are 1.) it is based on a tokamak design which carries with it all the concerns with disruptions, nuclear materials, divertors, and runaways. However, no other confinement concept can yet achieve tokamak performance so it is still seen as the frontrunner for a viable fusion reactor. 2.) demonstration of "Q of a few" likely requires a divertor solution that can handle the associated high Q_|| 3.) Phase 2 of the proposed approach is dependent on the development of HTS magnets on approximately a 10 year timeline. Although the Phase 1 of the "Q of a few" device is copper magnet based, Phase 2 suggests an upgrade to HTS coils. These coils have yet to be demonstrated and therefore there is risk associated with this part of the proposed approach.

However, there are clear physics and political benefits associated with the "Q of a few" development as soon as possible. The proposed tokamak would operate in a regime of parameter space that is beyond any current device and begins to approach that of a burning plasma. The values of rho* and Q|| sit between current day devices and ITER. This presents a opportunities to study interesting new core, pedestal, scrape off layer, and divertor physics while reducing risk by taking an intermediate step in plasma physics parameters towards an eventual DEMO-like reactor. The design and construction of such a fusion device would serve to reinvigorate the US program, which has not built a new tokamak in over 20 years, all while mitigating the risk associated with waiting for ITER completion, which has been plagued by cost and timeline overruns.

## 2.4. Strategic Elements

Development of the "Q of a few" device as part of the US fusion program would naturally encompass a number of the strategic elements identified during recent community planning exercises. Although Phase 1 of the proposed device would not achieve true burning plasma conditions (Q = 5), operation of this device would approach burning plasma conditions providing an intermediate step between current burning plasma physics knowledge and that which will achieved by ITER around 2036. Phase 1 and 2 of this device will serve as the ideal testbed for divertor/PMI research as it will generate conditions which approach those envisioned for future fusion devices. Currently, no divertor solution has been demonstrated that can withstand reactor level heat flux levels, particularly in a compact device. The "Q of a few" device should be viewed as an opportunity to test emerging and leading divertor concepts in a pulsed device with relatively low duty cycle. Advances in theory/computation will be leveraged to assist with the design and operation of this device. The "Q of a few device" will operate in a unique parameter space from a core, pedestal, and edge perspective, providing a unique opportunity to test the predictive capabilities of leading models in a new device configuration and to optimize aspects of this configuration during the design phase.

In Phase 2 of the the proposed path to fusion, the "Q of a few" device could be upgraded to address any of several strategic elements. Depending on the chosen research path, this device could be used to study steady state research, tritium breeding and nuclear materials, or further research into reactor-relevant divertor configurations.

### 2.5. Impact of ITER and International Context

The approach presented here is robust to any decision on US participation in ITER. If the US is not an ITER partner, development of a "Q of few" device provides the ability for the US to study breakeven plasma independent of the world community. Alternatively, with the US as an ITER partner, the development of a compact, high field "Q of a few" device completement ITER participation. ITER will achieve its burning plasma conditions through the application of moderately high magnetic fields and a large device size, effectively leveraging the strong scaling of energy confinement with major radius (~R^2). In contrast, the high field fusion path seeks to develop more compact devices by leveraging what in effect is the Ip scaling of energy confinement (proportional to Ip). Given a minimum value of q95 which can be reliably operated, increasing magnetic field allows for operation at higher Ip and therefore achieves higher confinement. These approaches are identical in that they are both based on existing plasma physics knowledge. However, they are fundamentally different in that the high field path could lead to smaller device sizes would could be developed at lower cost – leading to what could ultimately be a more attractive DEMO. The "Q of a few" device would be unique and produce the world's highest performing plasmas well before ITER (ITER DT planned 2036). As a result, the US would likely attract significant interest from the world fusion community – fostering and strengthening international collaborations.

### 2.6. Conclusions

The strategic plan proposed in this paper lays out a potential path for the US fusion program which features a high field, compact tokamak as its centerpiece. The pursuit of a "Q of a few" device as soon as possible has clear benefits to the US program which are attainable at relatively low cost and on short timescales. Development of this facility could change the narrative of fusion through a clear demonstration of fusion's feasibility, ultimately allowing for a more rapid development of fusion energy and putting the US at the forefront of worldwide fusion research.

# 3. Achieve Q>30 as soon as possible

*written by Dale Meade*

## 3.1. Description of Strategic Approach

Attaining and exploring a burning plasma core suitable for an Attractive Fusion Power Plant is a critical milestone on the path to attractive magnetic fusion energy. Fusion power plant studies [1, 2, 3] have shown that Q > 30 is required for economical fusion energy. This is a highly non-linear regime, where alpha heating and subsequent transport, define the pressure profile. In addition, a self-consistent high-bootstrap current fraction is required for steady-state operation of a tokamak fusion power plant, presenting additional nonlinear coupling within the plasma. External control of this self-organized high-gain burning plasma state becomes less practical as the fusion gain is increased. The existence of a stable Attractive Fusion Core (AFC) state is a major scientific issue for the practicality of an attractive magnetic fusion power plant. The broader scientific community has understood for years that the high gain (Q > 30) milestone must be accomplished in the path to attractive fusion energy. For example, John Holdren, President's Science Advisor (2010-2015) has stated that "until we can understand and master the physics of **a burning plasma—a plasma that is generating enough fusion energy to sustain its temperature and density**—we will not know whether fusion can ever be managed as a practical energy source" [4].

There are several strategic possibilities to accomplish the Attractive Fusion Core Mission:

1. An early achievement at the smallest size, lowest cost and program risk that focuses on the exploration of the Q > 30 non-linear burning plasma regime and employs existing technologies to the extent possible, and only employing extensions of existing technologies (e.g. PMI/Divertor) where required to access the non-linear burning plasma regime.

2. A mid-term achievement of very long pulse sustainment of Q > 30 at larger size to accommodate the shielding required to protect high temperature superconducting coils (HTS) from sustained thermal loads and neutron damage, but without a tritium breeding blanket. This approach would allow sustained high performance at Q > 30 and would employ nuclear technologies only to meet the Q > 30 Mission thereby avoiding the even larger size and complexity that a breeding blanket would require. This mission would constrained by the tritium site inventory and associated safety regulations that would limit the pulse duration, repetition rate and total neutron fluence. This facility would require a PFC/divertor with fusion relevant PFC materials capable of handling DEMO scale power densities, but would not require neutron resistant structural materials.

3. A longer term achievement Q > 30 on a demonstration power plant (DEMO) scale facility based on high- field high temperature superconducting (HTS) coils. This would be the largest size, highest cost facility and carrying with it the increased risk of trying to

control non-linear Q > 30 plasmas with reactor scale plasma energies for the first time. This is not the recommended strategy for developing a new technology.

This White Paper will focus on the fastest path with lowest risk and cost to an Attractive Fusion Core Experiment (AFCX) capable of attaining and exploring a Q > 30 plasma by choosing the high-density high-field approach, and making a slight modification to the FIRE-2004 Design that was vetted at Snowmass 2002 and carried through to a successful Physics Validation Review by DOE in 2004. Increasing the linear dimensions for FIRE-2004 by 10% (isomorphic transformation) maintains the same magnetic stresses in the magnetic coils. As an example, the projected H-Mode performance with fixed profiles increases to Q > 30 using ITER98(y, 2) scaling with H98 ≤ 1, $n/n_{GW} \approx 0.7$ and $\beta_N \approx 1.7$. This AFCX approach is not a replacement for ITER, it is a separate complementary mission of attaining Q > 30 that is unattainable by ITER using the same design criteria and the existing ITER engineering limits. AFCX would be of the same physical scale as prior DT experiments, TFTR and JET, that were constructed in ~ 8 years. This approach was technically ready in 1989, and would take advantage of prior design activities, CIT (1986-1989), BPX (1990-1991) and FIRE (1998-2005). During the intervening 13 years there have been advances in physics, engineering and manufacturing can be used to further optimize performance and reduce construction costs.

* Based on official cost estimates of US (9.1%) [11] and EU (45.3%) [12] shares in ITER.

The AFCX design would:

- Incorporate advanced tokamak features (e.g.. shaping, Hi-B CD, etc) into the basic design
- Incorporate advanced divertor and PFCs to deal with reactor relevant power densities
- Exploit benefits of high plasma density: stabilize Alfvén eigenmodes, enhance radiation of exhaust plasma energy, mitigation of disruption runaway avalanche, etc
- Exploit innovations in design, construction, manufacturing, and materials to reduce cost and improve performance.

### 3.2. Benefits

This strategic element would allow the U. S. magnetic fusion program to retire the critical Q >30 milestone risk as early as possible at the lowest cost, and would provide the justification for proceeding with the construction of integrated fusion nuclear projects (FNSF, DEMO, Pilot Plant, and commercial power plants). This activity would re-establish the US as a world leader in magnetic fusion research, and would provide a driving focus for strengthening and sustaining the U.S. scientific, engineering and industrial fusion infrastructure for decades. This approach would allow the large integrated nuclear technology facilities built after ITER to be designed with confidence to reach Q values required for an attractive fusion power plant, instead of having the additional requirement of being designed to be an advanced burning plasma experiment. The risk of cost increases and schedule delays is reduced since comparable scale copper coil DT facilities with comparable tritium site inventories have already been constructed (TFTR, JET). This approach is consistent with the technical assessment at Snowmass 2002 -- *"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."* [5]

Previously this high-density high-field approach was considered at a dead-end after the high-gain burn demonstration due to lack of a high field superconductor to carry the design forward to a power plant. **However, advances over the past decade in high-field high-temperature superconductors now make the high-density high-field path feasible all the way to an attractive Fusion Power Plant.** [6]

### 3.3. Strategic Elements

A successful Q > 30 technical achievement would provide research advancing the following strategic elements discussed during the U. S. Fusion Community process. The evaluation of contributions based on the AFCX approach is illustrated in Table 1 with 10 being required for a power plant.

**Burning Plasmas** – Previous burning plasma experiments on TFTR (1993-1997) and JET have produced over 10 MW of fusion power and explored the physics of the weakly burning regime (Q ~ 1). Important results included: isotope effect on energy confinement, confirmation of expectations for: alpha particle confinement, alpha heating of electrons, collisional transfer of alpha energy to the electrons, excitation of alpha-driven TAE modes and alpha ash transport. The achievement of Q > 30 would essentially retire all the tokamak burning plasma technical risk, particularly the risk associated with attaining and controlling a tokamak fusion power plant burning plasma. There would be some risk remaining regarding the long time sustainment of a

Q > 30 plasma in a tokamak that would remain as a major objective for DEMO.

**HTS Development** – This achievement does not require HTS coils, but would have the impact of stimulating the urgent development of high field HTS coils for implementation on large integrated fusion facilities of the DEMO scale that would follow.

**Stellarators** – While stellarators do not require a bootstrap current for plasma sustainment, the dynamics of a burning plasma modifying the plasma pressure profile in accordance with local plasma energy transport thereby creating bootstrap currents is of great importance to address the issue of Q > 30 performance in a stellarator.

**Configuration Research** – Many of the systems under study in Configuration Research depend on a large degree of self organization, and therefore must also address the issue of whether a stable high Q state is possible.

**Theory/Computation** – A significant theory/computation effort should be initiated to model the performance of self-organized high fusion gain plasmas. This capability would be essential in analyzing high-gain experiments and in refining the high-gain experimental program.

**Plasma-materials and divertor** – The most attractive approach to high gain involves a high density plasma operating with fusion power plant scale divertor and exhaust power densities for moderate pulse lengths and low duty cycle. This is a significant challenge for plasma facing components and the divertor. This is a high priority area no matter what future path is chosen for magnetic fusion. A significant focused effort should be immediately launched in this area. The Q > 30 initiative would provide a very convincing test of PMI/divertor capabilities on a fusion plasma with the exception of long time scale material migration and neutron damage effects on the properties of plasma facing and divertor materials.

**Fusion nuclear materials** – The Q > 30 initiative would not require neutron resistant structural materials. However, the damage to coil insulation would provide a limit to the production of fusion energy which in FIRE was 6.5 TJ of fusion energy resulting in a total of 30,000 pulses including ≈ 3000 full power full pulse length pulses. This area needs to be revisited for recent advances in materials and for the increased fusion power and longer pulses in AFCX.

**Tritium fuel cycle** – The tritium site inventory requirements for a compact high field Q > 30 initiative are ≤ 30 gmT similar to TFTR (≤ 5 gmT) and JET(≤ 90 gmT) and much less than the ≈2,000 gmT site inventory for ITER. No significant development beyond TFTR/JET is required.

**Sustained high performance tokamak** – The strategy would be to do the Q > 30 achievement at modest pulse length first and then use that information as input for the design of a Sustained High Performance (Q > 30) tokamak, such as a compact high-field (HTS) DEMO.

### 3.4. Impact of ITER

The ITER project was initiated in 2006 with the goal of producing Q ≈ 10 at fusion power levels of 500 MW. Important physics goals include: determination of plasma energy confinement at large scale and exploring alpha particle physics when the alpha heating power is ~ twice the external heating power. At these conditions, the plasma pressure profile is not modified significantly by the self-heating and 33% of the plasma heating power is available to control the plasma behavior. Under the most optimistic conditions, it is expected that ITER could begin DT operation in 2036. [10]

The Q > 30 initiative is envisioned as an effort in parallel to ITER with a complementary mission that is aimed at a different operating regime – high-field, high-density with strong non-linear self-organization. There would be a commonality of needs for some DT compatible systems such as: auxiliary heating, PFC/divertor, diagnostics and plasma control actuators.

### 3.5. International Context

The major international programs will extend previous weakly burning DT plasma experiments with Q ~ 1 in JET (2019) to determine plasma performance projections for ITER-like plasma facing components (Be first wall and W divertor targets). Beginning in 2020, sustained Advanced Tokamak non-burning DD plasmas will be explored at large size (R = 3m, B = 2.25T) in JT60-SA, and at smaller size in EAST and KSTAR for longer plasma durations. This would be followed, beginning in early 2037, by DT experiments with Q ≈ 10 and fusion powers of 500 MW at near reactor size (R = 6.2m, B=5.3T) in ITER. There is currently no facility in international fusion program plans capable of exploring Q > 30 burning plasma physics prior to the construction of DEMO.

### 3.6. Decision Points

The AFCX Q > 30 achievement is put forward as part of a **technical issues driven** Multi-Machine Development Plan [5]. The major elements are:

1. Non-Burning Plasma Confinement - existing Tokamaks + JT60SA (2020), Theory and Modeling of high-gain non-linear burning plasmas (new initiative)
2. Non-Burning PMI/Divertor - existing tokamak upgrades, linear, and focused DTT
3. Burning Plasmas- JET(Q~1, 2020), ITER (Q ≈ 10, 2038) and AFCX(Q ≥ 30, 2038)
4. Materials Development - fission and spallation neutron sources, IFMIF (2028)
5. Integrated Nuclear Test Facilities – Fusion Nuclear Science Facility (FNSF) for Closing the Fuel Cycle and Neutron Testing of components in a fusion environment.
6. Fusion Demonstration Scale Nuclear Facilities – DEMO, Pilot Plant, Net-Electric Demo

*Input Links:* For AFCX, the present confinement physics (similar to ITER) is sufficient for initial design. Results from JT60-SA, EAST, KSTAR on AT modes will be used to refine design for current drive systems during facility construction. There is a major link to PMI/Divertor research that is needed to update the prior FIRE divertor design, and to decide on the appropriate material for the first wall. The present Be first wall (like ITER) is not suitable for a DEMO; should this be changed to a DEMO-relevant material? Theory and computer models will be used to predict behavior and develop operating regimes for high-gain non-linear plasmas.

*Output Links:* The most critical output is data on attaining Q≥ 30 and controlling the non-linear high-gain burning plasma state. In addition, the data on the performance of the PMI/divertor at high power densities will be critical for the design of a DEMO leading to attractive fusion power. Note an FNSF would be needed in parallel with the Q> 30 initiative to support a U. S. style DEMO design.

### 3.7. Timing

1. Immediate actions:
    - Update/assess projections of plasma performance and engineering design of AFCX.
    - Update and assess possible revisions to FIRE PFC/Divertor Design
    - Initiate a more focused Divertor Test Program with fusion relevant materials ASAP on existing facilities. Utilize international facilities with fusion relevant PFC materials

    ***Complete in 2 years***
2. Complete design, begin staged construction (similar to ITER).
    ***Complete in 4 years***
3. Complete Stage 1 construction and begin initial non-DT operations
    ***Complete in 7 years***
4. Complete Stage 2 construction, system and plasma commissioning and ready to begin DT
    ***Complete in 5 years***

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# 4. Vision for a U.S. Compact Net Electric Fusion Pilot Plant Incorporating a Nuclear Science Mission

*written by R.J. Buttery, summarizing discussions with many colleagues**

## 4.1. Description of Strategic Approach

The U.S. should pursue an aggressive science and technology program to enable a decision to commence engineering design and construction of a compact net electricity generating fusion pilot plant in the United States of America. Such a device would prove the viability of fusion energy, showing that the loop can be closed on net electricity generation between fusion performance, energy capture, and plant consumption. The device would incorporate nuclear science, materials and breeding development missions through a phased approach, with re-licensing and upgrades as key techniques were evaluated. Coupled with the progress and insights obtained from ITER, this would provide the confidence and lay the foundations for the private sector to build successor competitive cost of electricity (COE) fusion power plants to herald in a new era of fusion energy.

A decision to proceed with this facility requires (or is highly leveraged by) research progress on seven enabling missions, in order to resolve key techniques and parameters for this facility, and not least, the basis to enable the device to be built on a compact scale. They are to resolve:

- A successful ITER engagement.
- A high performance, stable, fully non-inductive fusion core.
- An erosion free divertor solution.
- Validation of suitable materials for the high heat and nuclear fluence environment.
- Efficient current drive technologies.
- High temperature demountable superconductors.
- Reactor engineering exploration and design, including breeding concepts.

Research on each should be commenced immediately. These missions are highly needed by almost all paths to fusion energy and would provide the U.S. with key intellectual property. While progress on each is vital to enable a reactor decision, some trade-off exists in the degree of success needed for each mission, as the solution to each issue may either place or alleviate constraints on the rest. Once sufficient progress has been made, this will naturally enable one or other confinement concept as the basis for the net electric facility. Put simply, a viable design solution will become apparent. Thus, the choice of confinement concept and parameters of the net electric facility will be a result of progress made.

*This two stage plan represents a tractable, potentially rising, U.S. domestic fusion research program, with an accelerating energy focus. Importantly, it does not set the specific facility and investments choices, but rather provides a framework and goals to make such funding decisions.*

### 4.2. The Need and Mission for a Compact Net Electric Nuclear Science Pilot Plant

The world program is increasingly focused on fusion energy, with concepts for large scale power plants developed by most of the ITER partners, based on the tokamak approach (SlimCS, Japan [Tobita 2009], EU-DEMO/FPP [Zohm 2010, Lee 2015], K-DEMO, Korea [Kim 2015, Kang 2016], ARIES-ACT [Kessel 2015]), set out in Table 1. These concepts are focused on the ultimate potential of the tokamak to provide a competitive COE at the GW level. However, the first net electric reactor is itself unlikely to be COE competitive, due to its inevitably experimental nature, both in terms of its construction and its performance. For example, EU power plant studies are predicated on a tenth of a kind 1.5GWe device to reach acceptable COE [Lee 2015]. Further, due to their large size, these GW scale devices are likely to have high capital cost. Applying the Meade scaling 'law of cost per ton (or per volume) of tokamak, one arises at ~$100Bn for facilities such as EU-DEMO or K-DEMO. Securing funding for such a large first of a kind device, without establishing viability at the smaller scale, and without the prospect of low COE, is unlikely, especially for U.S. investors. Moreover, learning the lessons at this large scale is likely to be highly costly and time consuming. A better strategy is to use a more compact and lower capital cost device to prove out the fusion technologies and energy potential, before the leap to large scale/low COE. It is important that this compact device show that the loop to net electricity can be closed, as this otherwise adds to the mission and uncertainty of a successor device (effectively requiring another generation of devices before competitive COE fusion energy is reached). It would thus be a pilot plant.

In addition to establishing net electric viability, a major nuclear science mission must be pursued to develop techniques for breeding and at least start to qualify materials for the long pulse high neutron fluence/high dpa conditions of a continuously operating power plant. This is set out in various proposals for nuclear science facilities [Blanchard 2015, Chan 2010, Garofalo 2014, Menard 2016], which typically require devices on the 2-5m radius scale. From the above arguments, a key point emerges that if the net electric mission can be achieved at similar scale to the nuclear science mission through a compact approach, then it is logical to combine the net electric mission with the nuclear science mission in a phased program on a single device. Such a device would commence with basic short pulse performance evaluation of the plasma and various systems. An early goal might be a $P_{net\text{-}equivalent}$ demonstration, to validate that the systems and plasma operate with required performance, even before the installation of electricity generation systems. Subsequently, materials behavior and breeding technologies would be assessed, with possible change outs of components to optimize solutions. Through further facility development and relicensing, this would progress to long pulse assessments (10's of dpa) and net electricity demonstration. The goal would be to sufficiently establish the feasibility to either progress directly with an economic fusion power plant, or better still, provide the confidence for the private sector to engage in the final steps, where development of validated materials or other IP may provide incentivizing opportunities.

*Table 1: Summary of tokamak power plant parameters.*

| | ARIES-AT [N06] | ARIES-ACT1 [K15] | SlimCS [T09] | ARIES-ACT2 [K15] | K-DEMO [KANG16] | EU-DEMO [Z10] | ARC [S15] |
|---|---|---|---|---|---|---|---|
| R (m) | 5.2 | 6.25 | 5.5 | 9.75 | 6.8 | 7.85 | 3.3 |
| a(m) | 1.25 | 1.56 | 2.1 | 2.44 | 2.1 | 2.5 | 1.1 |
| $B_T$ (T) | 5.6 | 6 | 6 | 8.75 | 7.4 | 5.6 | 9.2 |
| $I_P$ (MA) | 13 | 11 | 16.7 | 14 | 17 | 14 | 7.8 |
| $b_N$ | 5.4 | 5.6 | 4.3 | 2.6 | 3.1 | 3.5 | 2.6 |
| $f_{BS}$ | 0.91 | 0.91 | 0.75 | 0.77 | 0.77 | 0.62 | 0.63 |
| $P_{AUX}$ (MW) | 35 | 43 | 60-100 | 106 | 120 | 115 | 39 |
| $P_{FUS}$ (MW) | 1719 | 1800 | 2950 | 2600 | 2870 | 1960 | 525 |
| $P_{EL}$ (MWe) | 1000 | 1000 | 1000 | 1000 | 400-700 | 300-500 | 200 |
| $N_W$(MW/m$^2$) | 3.2 | 2.45 | 3 | 1.5 | | 1.2 | 2.5 |

To enable this approach, the need arises to adapt the power plant concept to the smaller pilot plant scale. However, presently available technologies preclude this, as identified by the extensive European studies for an EU-DEMO, where a conservative approach based largely on "what we know now" leads to a large scale 8m radius device [Zohm 2010, 2016], even with a more modest net electric goal (500 MWe). Thus, a more advanced approach is needed to make a fusion reactor tractable on a more compact and capital-cost-affordable scale. This can then enable the net electric mission to be combined with a nuclear science mission in a single step to fusion energy. As we discuss below, this requires enabling research to make this more advanced approach possible.

### 4.3. Key Strategic Elements that Enable a Compact Net Electric Nuclear Science Pilot Plant

Innovative approaches to compact net electric facilities have been proposed. The original ARIES concept, updated in ARIES-ACT1 [Kessel 2015], and SlimCS [Tobita 2009] identified relatively compact devices (~6m scale) that generate 1GWe based on the advanced tokamak concept operating at high $b_N$ (~5). This is still large and expensive for a first proof of principle device. Thus, studies for the proposed ARC device [Sorbom 2015] explored a lower net electric power of 200MWe, exploiting the high field potential of high temperature superconductors (HTS), rather than high $b_N$, and benefiting from an assumed confinement enhancement factor $H_{98pby2}$~1.8

at 9T and 500MW gross fusion power. More recently, physics based integrated predictive transport/pedestal/current drive/equilibrium analysis based on the high $b_N$ advanced tokamak has found fully non-inductive solutions at the 4m scale with predicted confinement $H_{98pby2}$~1.3-1.5 and 200MWe at 6-7T from ~600-800 MW fusion power. These are discussed in a white paper to the NAS panel on a strategic plan for U.S. burning plasma research in [Buttery 2018]. Similarly, a spherical tokamak approach [Menard 2016] projected a 3m scale device with HTS could generate net electricity with a required $H_{98pby2}$~1.4-1.8 and 560MW of fusion power.

A key difference of the more compact approach is that net electric performance becomes more sensitive to achieving key plant and plasma performance metrics (as discussed in [Buttery 2018]). Large fusion reactors such as ARIES-ACT1/ACT2, SlimCS, EU-DEMO or K-DEMO generate large amounts of gross fusion power (~2-3GW level), requiring a relatively small fraction to be recycled to heating and current drive to sustain the plasma, typically 40-120MW (Table 1). More compact devices, such as those mentioned above, actually require similar amounts of heating and current drive power, but from a much lower gross fusion power (~500-700MW). They are also more reliant on good thermal energy confinement to ensure the plasma can largely self-heat and confine energy to maintain the required operating point, rather than then rely on expensive auxiliary heating. This makes the net electricity extractable more sensitive to deviations in fusion performance, confinement, auxiliary current drive needs and efficiencies.

Each of the compact approaches discussed above deploys key techniques to raise device performance, such as HTS to raise toroidal field, strong shaping or high density and high $b_N$ to raise fusion power, or high $b_P$ and bootstrap fraction to reduce auxiliary current drive needs. They also rely more heavily on good thermodynamic and current drive efficiency. And they may pose greater engineering challenges to handle the large fields and forces involved at the compact scale. ***These therefore set important research missions*** to establish if the challenging parameters required can be met to enable a more efficient compact device. In addition, ***progress is also required on a number of further issues*** for any future continuously-operating fusion reactor, such as choice of materials for first wall, a robust non-eroding divertor solution that permits a high performance core, elimination of transients, and other engineering and design aspects, as summarized in section 4.1.

***Thus, one foresees an early research program*** *(set out below)* ***to provide the conceptual plasma and technology solutions to enable a decision to proceed with a compact net electric fusion reactor engineering design.*** One can see this as investing effort in the near term, with some years of research with D-D facilities, test beds and other developments, to establish viability of a more attractive integrated concept, instead of immediately starting on a D-T device in a two-facility sequence for the nuclear mission, and then net electric. This makes sense as ***the U.S. is not yet ready to commit to a major new nuclear facility***, and this enabling research, which is substantive, can ***help establish U.S. fusion program credibility*** as well as ***concept viability***. Further, it might also be argued that pursuit of a combined nuclear science / net electric mission in a single facility ***may be more compelling to the funding agency than either the longer timescale two facility path*** or moving directly to a large scale high COE DEMO as the EU proposes.

It is important to note that this approach does not pre-decide the confinement concept: one or several can be explored in parallel (high field, AT, ST, stellarator…); once one or more approach leads to a viable integrated solution for a net electric facility, a decision can be taken to proceed with an engineering design. *This approach would provide a distinctive path in the world program, building on collaborative activities abroad, but developing unique and valuable techniques, with the potential of a faster path to fusion energy that places the U.S. and the forefront of the world program.*

## 4.4. Impact of ITER & the Near-Term U.S. Mission

The decision to design and construct a compact net electric nuclear science facility requires a number of critical research tasks to be completed. It should be noted that although this analysis is focused on the concept of a compact net electric/nuclear science phased facility, these elements are largely common to a range of long pulse D-T fusion devices. Development of expertise and solutions on any of these missions will be highly leveraging to any of these potential U.S. paths.

### 4.4.1. Successful ITER engagement

***U.S. participation and leadership in ITER*** offers far more than that which can be gained by simply reading ITER's papers. Direct participation brings with it big facility construction and operation experience that the U.S. has lost. This brings insights in three key areas:

(i) Firstly, the ITER construction is providing key insights and new technical solutions in reactor design to which the U.S. is gaining direct access. These are aspects where the publications only provide a partial record; direct participation in ongoing implementation of design solutions and systems integration provides unique insights into reactor scale solutions and manufacturing approaches. The U.S. gets 100% of the research (that it participates in) for 9% of the cost.

(ii) Secondly, early D-D ITER results will provide valuable data on the scaling of phenomena to reactor scale (radius, current, field) that will help finalize U.S. reactor parameter choices. Behavior of transport and confinement, stability, pedestal, ELM mitigation and energetic particle are areas where the US has leading scientific capabilities, and this work will validate U.S. simulation tools (if engaged) as well as improve overall insight from ITER.

(iii) Finally, partnership and leadership in ITER provides the experience to manage construction and operation of a large nuclear facility.

***Without ITER participation***, a U.S. strategy to a compact net electric facility is possible but more difficult, due to loss of nuclear tokamak experience and reduced international collaboration. This would likely require much more time and additional facilities or testbeds to prepare U.S. expertise. It should be noted, that because the compact pilot plant envisaged for the U.S. will operate at similar Q to ITER (~10-20), it does not need to await D-T results from ITER but can exploit the same physics basis used to develop ITER's operating scenario (plus advances discussed below). Nevertheless, ITER D-T data and higher performance operation will be valuable in combining with data from the U.S. pilot plant to project to larger scale competitive-COE power plants.

### 4.4.2. A high performance, stable, fully non-inductive fusion core.

The specification of a pilot plant requires confident projection of its fusion performance and energy confinement to determine device scale, required subsystems and key operational insights. The techniques required to resolve viable steady state operation must be discovered. This includes achieving robust stability, including to edge transient events such as ELMs, and a configuration that can be sustained entirely non-inductively. These are aspects that have not yet been established in reactor-relevant physics regimes in the present generation of devices; a projectable understanding is mandatory in order to resolve what to build and on what scale.

This requires adapting present facilities to explore the relevant physics regimes and techniques to:

- Find fully non-inductive solutions through use of auxiliary current drive and high bootstrap fraction plasmas, understanding how to achieve consistency between plasma configurations and heating and current drive systems, sufficient performance, and identifying limits from MHD and energetic particle driven instabilities.
- Project to burning plasma relevant parameters, particularly $T_e{\sim}T_i$, low rotation and collisionality – key parameters that influence turbulent transport, stability, pedestal and ELM suppression.
- Resolve transients with robust control of ELMs and macroscopic stability, developing effective off-normal fault response systems and more effective disruption mitigators

The above elements focus on the tokamak. A stellarator may alleviate some of these challenges (such as instabilities and disruptions), but requires additional research, which should be pursued to determine if it can provide the required performance and confinement (and on what scale), so that it might provide an option for the pilot plant configuration, should sufficient progress be made in time. A quasi-symmetric stellarator provides a promising and distinctive option for the U.S. to pursue here.

### 4.4.3. An erosion free divertor solution compatible with the fusion core.

Continuous operation requires a plasma exhaust solution that can operate without erosion; hence. it must be detached. Techniques to achieve detached divertor conditions that are compatible with the high performance core must be developed. In particular, this must be demonstrated with high power exhaust, without adversely impacting a high performance pedestal solution, and without excessive erosion or influxes of impurities. The interaction between pedestal and divertor is key here, and the use of radiative mantle techniques to mitigate divertor heat loads, compatible with high performance fusion cores, must also be studied.

Key techniques to address these issues are facilities with sufficient flexibility in divertor magnetic geometry and closure structures, and suitable performance cores. While MAST-U can explore innovative geometries, new or upgraded higher power capabilities are needed to find consistent core-edge divertor solutions. For the stellarator, options exist to exploit LHD to explore more of this challenge. A liquid lithium divertor provides a high risk but potentially transformative approach to this problem too.

**4.4.4. Validation of suitable materials for the high heat and nuclear fluence environment.**

Materials pose a particular challenge to withstand the nuclear environment, particular on the first wall and divertor. Bombardment by neutrons can damage material structure, greatly exacerbated by generation of helium. While materials research would be a mission of the pilot plant, it is important to find strong candidates and to develop a thriving and advanced materials research program to facilitate this mission. High Z materials like tungsten remain frontrunners; these pose challenges in power handling and impurity sourcing. Innovative alloys and medium Z materials (such as ceramic SiC) should also be explored. Therefore, nuclear test bed facilities are needed to explore promising candidates, with a volume neutron source (for example, a gas dynamic trap) being highly desirable to address these issues. Exposure of irradiated samples under suitable plasma exhaust conditions may also be needed. On the stellarator path, solutions to enable stellarator configurations and divertor with sufficient blanket thicknesses must be developed.

**4.4.5. Efficient and reactor relevant current drive technologies.**

Reactor studies show current drive efficiency to be a critical enabling parameter for a compact net-electric device. RF driven sources have the potential to reach sufficient efficiencies (as opposed to neutral beams, where the large ports also decrease breeding volumes), but coupling to the plasma becomes a key issue. Plasma-interaction with antennae, which must be placed near to the fusing plasma, also becomes a concern. High field Lower Hybrid Current Drive (LHCD) and ultrahigh harmonics ‘helicon’ fast wave with a traveling wave antenna have recently been proposed. These highly efficient techniques offer potential solutions that must be tested for effective coupling. The former also places the antenna in a region of decreased exposure to plasma fluxes. Top launch ECH, which avoids the antenna issue almost entirely is also predicted to be more efficient than conventional ECCD. These techniques need to be tested for exposure to plasma, coupling, and effective current drive in a tokamak.

For a spherical tokamak (without or with limited solenoid) the issue of non-inductive start up must be addressed, using a combination of novel techniques, such as local helicity injection, and RF solutions.

**4.4.6. High temperature demountable superconductors.**

Simulations of pilot plant concepts show that their performance and thus compactness are highly increased s $B^3$-$B^4$ by raising the magnetic field. High temperature superconductors (HTS) thus offer the potential to greatly improve device cost, though some simulations [Buttery NAS AT paper] indicate possible solutions with conventional superconductors. However, perhaps more significantly for a facility with a nuclear testing and breeding mission, HTS may also offer the potential to make demountable coils, which would enable the opening up and rapid change out of wall and breeding components. Thus, large bore demountable HTS technology should be pursued as a greatly levering priority. Proposals for this are well documented in the papers and talks from MIT.

**4.4.7. Reactor engineering exploration and design.**

In order to develop readiness for a pilot plant construction decision and subsequent rapid progress, the U.S. needs to rekindle and expand efforts on reactor engineering and design. Focus must be given to individual technical solutions, such as breeding approaches, mechanical analysis, change out scheme, and also to overall systems design and integration of key components. Many other white papers go into more depth on these issues.

### 4.5. International Context

See 4.2.

### 4.6. Decision Points

The critical decision point is the decision to commit to a particular device and start its engineering design. This decision point requires progress on the seven enabling missions described in section 4.4, to put in place the requisite elements of a conceptual, but well simulated, design point in terms of main parameters, systems choices, and techniques required.

### 4.7. Timing

The timing of the major decision point for the long-term objective to proceed with a pilot plant will become apparent based on attaining required progress in missions in section 4.4. It is anticipated that this is of order ten years. Short term objectives are to initiate suitable research projects on each of these lines, and then (medium term) achieve significant progress in developing solutions.

### 4.8. Conclusions

The U.S. should focus its fusion research program on the path to a compact net electric nuclear science pilot plant facility, to be built in the United States of America, to prove net electric power generation and to test breeding and nuclear materials. To achieve this, the U.S. should engage in the 7 parallel 'enabling research' missions set out here, developing capabilities and building confidence, to provide the knowledge to take a decision to proceed on this device, and specify its systems and parameters. This research will establish unique U.S. intellectual property, and pilot plant will provide the confidence for the private sector to take over with successor commercial fusion power plants, to commence the fusion era.

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