A U.S. STRATEGIC PLAN FOR TIMELY FUSION ENERGY DEVELOPMENT

M.R. Wade*

General Atomics, San Diego, CA, USA

WHITE PAPER

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* With input from C. Greenfield, H. Guo, D. Hill, J. Menard, C. Petty, W. Solomon, and T. Taylor and revised based on comments by P. Ferguson, R. Hawyrluk, D. Whyte, and M. Zarnstorff

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A U.S. Strategic Plan for Timely Fusion Energy Development

Executive Summary

A strategic plan is outlined that focuses U.S. R&D on specific research paths that converge in ~ 2040, alongside Q=10 demonstration in ITER to provide the technical basis for beginning design of a cost-attractive pilot plant and/or DEMO device. The focus of the strategic plan is on developing the basis for high power density solutions that offer the potential of cost-attractive fusion systems in the future. The research paths discussed in this strategic plan are not new to the fusion program; however, the strategic approach outlined here develops these paths in a new way so as to achieve convergence of the paths in the 2040 time frame. The strategic plan outlines a path that could deliver three world-class U.S. facilities in the 2035-2040 time frame that simultaneously a) enable resolution of critical issues for fusion development; b) provide compelling scientific opportunities for US researchers to carry out cutting edge research, c) deliver US leadership in key areas that will significantly impact the direction of fusion energy development worldwide and d) enables a pathway for a cost-attractive US pilot plant or DEMO device in ~ 2040.

Background

To date, fusion research globally has had the common purpose of demonstrating the feasibility of fusion energy, which is now being aggressively pursued in the ITER project [1]. However, moving forward beyond ITER, the U.S. fusion program will likely find itself in a unique position among its world partners. Due to limitations on availability of fossil fuels, renewable capabilities, and public acceptance of nuclear fission reactors, other countries are aggressively pursuing fusion energy as a near-term (2-3 decade) source of baseline energy supply. This pursuit would be made more urgent if policy makers choose to aggressively address potential climate-change issues through reductions in fossil fuel usage. This focus on energy supply has naturally led to proposed R&D programs in those countries that utilize existing or high-confidence approaches to develop practical fusion energy as soon as possible. Proposed next-step facilities in many countries (e.g., EU-DEMO[2], CFETR[3], K-DEMO[4]) embody these assumptions and typically are relatively large (major radius ~ 7-10 m) tokamak devices. In contrast, the U.S. has a competitive energy market with ample supplies of fossil fuels (natural gas, oil, coal) and a rapidly expanding renewable energy sector (solar, wind), as well as legacy hydroelectric and fission reactors. In this environment, fusion systems will need to display distinct advantages compared to existing methods to break into the energy market. This difference in positioning presents the U.S. with an opportunity to focus on specific new capabilities that could significantly improve the prospects of fusion energy through an R&D emphasis on the development of a compelling physics and technology basis for cost-attractive fusion systems going forward. Pursuing such an approach also should inform the potential of cost-attractive steps beyond ITER that could rapidly advance the fusion program from its current feasibility stage (ITER) to practical demonstrations (pilot plant or DEMO). A potential strategic plan that leverages U.S. capabilities to thrive in this space is outlined below.

Overview of Strategic Plan

The most distinctive feature of this strategic plan is the convergence of the R&D activities in the 2040 time frame so that the U.S. is positioned to move aggressively with a cost-attractive followup to ITER that fully demonstrates the technological capability to produce net electricity from fusion energy. A key attribute of this plan is the focus on developing a cost-attractive pathway to such a demonstration and by extension, developing cost-attractive fusion energy systems for the future. Ensuring the success of ITER in achieving (and exceeding) its technical objective is central to this plan. Alongside this effort, the U.S. would take the lead role in:

- Developing high power density, integrated core-edge plasma scenarios capable of steady-state operation
- Fabrication of high-critical-temperature superconductors for fusion purposes
- Development and qualification of materials for fusion energy systems including both plasma facing and structural materials

Very importantly, this R&D program would establish demonstrative U.S. leadership in each of these areas, thereby positioning the U.S. with strategic advantages in the worldwide development of fusion energy in the future.

The foundation of any U.S. strategic plan going forward is the production, evaluation, and exploitation of the burning plasma regime. This regime is characterized by plasmas that are dominantly self-heated by the alpha particles generated by the fusion process itself (i.e., burning plasmas), potentially leading to non-linear interactions of a range of effects. The U.S. is presently a partner in the ITER project, which starts operations in 2025 and will enter its burning plasma phase in 2035. Alongside the burning plasma effort, a critical aspect of any U.S. plan should be to put into place the technical know-how of other elements required for fusion energy to capitalize on the public enthusiasm that Q=10 operation in ITER should generate. Given the comments above with regard to U.S positioning, the program plan in the 2020-2040 timeframe should be focused on delivering key capabilities that significantly improve the cost attractiveness of future fusion systems.

Each of these development areas would be enabled by unique and/or world-class facilities that would provide US researchers will highly capable platforms for carrying out cutting edge research. These facilities include:

- **ITER** to explore and exploit the burning plasma regime
- **High Power Density Tokamak (HPDT) facility** to test the limits of core and edge performance in reactor-relevant conditions
- **Volumetric Neutron Source** for large-sample materials and components exposure
- **Material Test Facility** capable of high heat flux over an extended range of conditions
- **Magnet Test Facilit**y capable of very high magnetic fields (> 15 T)
- Proof-of-principle **quasi-symmetric stellarator**

Each of these facilities build significantly on previous, ongoing, and/or planned R&D efforts in the US, which should provide a well-informed basis for the design, construction, and operation of these new facilities.

As noted in numerous community reports and systems studies, a further essential requirement for cost-attractive fusion systems is the ability to efficiently breed tritium and extract high quality heat. While it is recognized that this blanket technology is critical to the success of fusion, this strategic plan relies heavily on leveraging worldwide investments in this area, which will likely be an important R&D element in fast-track approaches to DEMO development in other nations.

All together, successful implementation of this plan should enable development of the required basis for essential features of a cost-attractive fusion system for the future, namely (along with the facilities that will enable this development):

- High power density, high performance, steady-state, burning plasma operation (existing facilities, ITER, High Power Density Tokamak, Magnet Test Facility)
- Solutions for controlled dissipation of very high heat flux solutions (existing facilities, ITER, High Power Density Tokamak, Material test facility)
- Materials that can maintain requisite properties under high heat flux and high fluence 14-MeV neutron spectrum (ITER, Material test facility, Volumetric Neutron Source)
- Robust solutions and/or increased margin to potentially damaging transients (existing facilities, ITER, High Power Density Tokamak, Magnet Test Facility)

An overview of the strategic plan is shown in Figure 1 (with ITER), providing both the high priority technical objectives with the foreseen facilities necessary to support these objectives. Key technical objectives are also given at the end of the document for each R&D area. It is recommended that the reader reference these figures and the associated objectives while reading the dialogue below for better appreciation of the priorities, timing, and linkages of the various program elements.

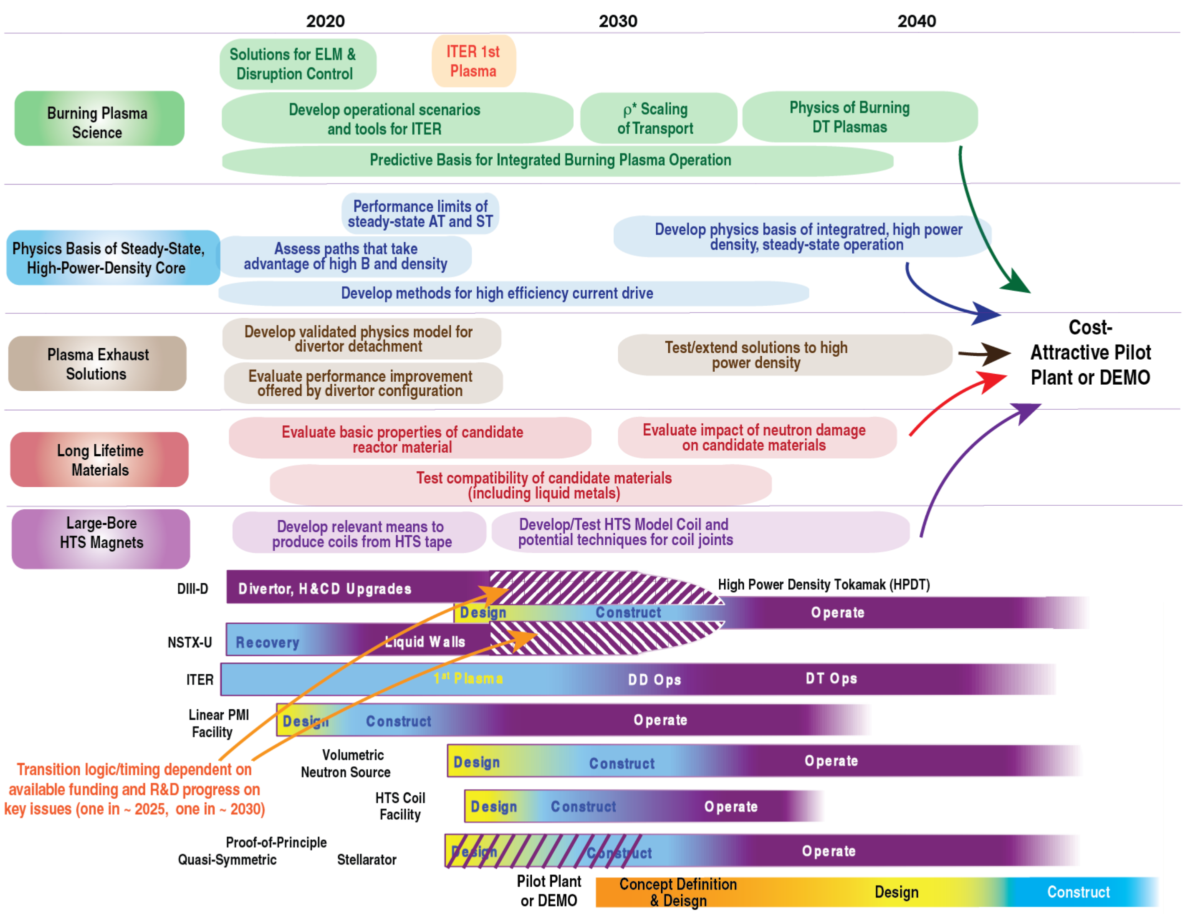

Figure 1: Strategic plan assuming U.S. continued participation in ITER

Key Objectives of the Plan

This plan was developed with five key objectives in mind:

- Establish U.S. leadership in critical physics and technological areas with the goal of strategically positioning the U.S. with critical expertise that will be needed by other nations in their pursuit of fusion energy
- Deliver world-class research platforms in a timely manner to enable excellent science leading to key knowledge & breakthroughs
- Utilize the most cost-attractive approaches to establish this leadership so as to enable a broader set of pursuits
- Broaden the constituency base to enable strengthened technical and political support by promoting pathways that broaden the required scientific disciplines and institutional engagement
- Provide a compelling 2040 goal for program direction and resourcing so that the destination and associated technical objectives are clear

Plan Considerations

Foundational to this strategic plan is the continued role of theory, simulation, and computation in motivating innovative approaches to improving the prospects of fusion energy. This includes theoretical work in the traditional area of plasma physics and new areas such as materials science. These efforts should benefit tremendously from the availability of exascale computing capabilities to tackle complex problems and high-capacity computing for scoping studies, machine learning, and data analysis. Utilizing these tools, validation of important physics models should be a strong emphasis of the R&D program supporting this plan.

The strategic plan outlined here focuses on the development path through the tokamak line, primarily due to the performance obtained and the maturity level of tokamak physics relative to other configurations at the moment as well as the heavy investment worldwide in tokamak development, including ITER. However, it is recognized that there would be significant value in developing at a secondary pathway to sufficient maturity that the performance capabilities could be evaluated on a comparative basis. Recommendations on potential R&D for such a secondary pathway are included in this plan. It should be noted that many of the elements outlined here should be, in principle, agnostic to the specific configuration, though the specific implementation and timeline with non-tokamak configurations would likely be delayed relative to the tokamak approach.

This plan also presumes that ITER goes forward successfully as planned. The anticipated changes required to this plan if U.S. participation in ITER were terminated are elucidated later in the document. International collaboration should remain a key aspect of the U.S. program to ensure that the U.S. can capitalize on the significant investment and intellectual resources provided by other nations. In this regard, the synergy between the world program and this U.S. strategic plan is a key consideration. In the discussion below, the foreseen research program and focus for the key elements outlined above is provided together with the areas in which the U.S. can benefit from the significant work being done worldwide.

It should be noted that this strategic plan has been developed without significant consideration of the implications on the funding required to support the plan. However, since each of the program elements put forth by this plan simultaneously addresses key issues and provides world-class research opportunities, this plan's convergence of these elements in an approximately 20-year time frame to deliver the technical basis for first-generation fusion power-producing system should significantly bolster the argument for increased US fusion funding. The exact staging of the elements and which ones to emphasize should include evaluation by a national team of experts, especially in the event that such a funding increase did not materialize,.

Burning Plasma Science

The U.S. fusion program has been a key contributor to the physics basis for ITER design and operation. Results from C-Mod, DIII-D, and NSTX have either motivated or confirmed design choices for plasma configuration, operating scenarios, ELM control, disruption avoidance and mitigation, test blanket modules, plasma control, etc. In addition, predictive models developed in the U.S. (and validated worldwide) are now at the heart of simulations that are projecting the operational envelope for ITER when it begins to operate. Looking forward, participation in ITER provides the most timely and cost-effective opportunity for the U.S. to gain entry into the science of the burning plasma regime. This will enable the U.S. to develop first-hand knowledge of key physics issues (e.g., ρ^* scaling, alpha particle confinement/stability, plasma exhaust, integrated scenarios, burn stability) and just as importantly, how to operate a burning-plasma-class facility.

In the 2020-2035 period, the U.S. program through its domestic facilities (sometimes in collaboration with international partners) will continue playing a key role in providing solutions to critical ITER needs. For example, the U.S. is a world leader in the control of transient behavior such as ELMs and disruptions both experimentally and theoretically. Additional capabilities coming online in the next few years should enable continued leadership in these areas. In addition, the U.S. has world-leading capabilities that can be exploited in developing scenarios that deliver the potential of $Q > 10$ operation or achieving $Q = 10$ at lower plasma current in ITER. The high priority placed on well diagnosed plasmas in the U.S. coupled with continued U.S. leadership in developing physics-based models will provide the U.S. with unique capabilities not only to improve the understanding of reactor-grade plasmas but also to use this understanding for improving the performance of fusion systems beyond ITER (see [5] for more detailed information).

Early in this period, DIII-D and NSTX-U are well suited for ensuring continued U.S. impact on ITER as well as training the U.S. workforce in preparation for ITER operations*. *As ITER comes online, the U.S. research program will shift to using its domestic facilities to provide solutions to immediate ITER needs (i.e., responding to unforeseen situations in ITER), developing a physics understanding of the burning plasma regime, exploiting ITER for higher performance, and better informing next-step prospects for fusion energy. As other facility elements of this plan begin to develop, the role of domestic scientific support could potentially be taken on by other facilities called out in the plan *(see Steady-state and Power Exhaust sections below).* A key attribute of this focus on ITER success is maintaining strong technical connections between the U.S. program and ITER to ensure awareness of emerging questions/challenges. Continued involvement in the International Tokamak Physics Activity (ITPA) and future international activities coordinating burning plasma research should remain a U.S. priority.

Steady-state, high-confinement, high power density operation

The U.S. has long been a world leader in the development of high performance, steady-state-capable scenarios. The focus on higher performing systems (which are inherently higher energy density) has always been a hallmark of the U.S. program. While new superconducting tokamaks (KSTAR, EAST, and in the near future JT-60SA) will have a natural advantage in extending these scenarios to true steady-state, the planned capabilities of DIII-D and NSTX-U will remain world-leading in pushing the envelope of performance and in establishing the feasibility of high performance, fully non-inductive scenarios. In addition, recent advances in the U.S program, motivated by theoretical studies, point to new research elements that not only offer the U.S. distinct leadership opportunities but also potentially lead to more cost-attractive fusion systems, a central theme of this strategic plan. For example, recent experiments, motivated by theoretical studies, demonstrated record performance on C-mod (world record pedestal pressure) and DIII-D (Q_DT_eq ~ 0.4) through optimization of the pedestal performance. Separate experiments have demonstrated high-performance steady-state scenarios with no ELMs and the sustainment of high bootstrap fraction scenarios with internal transport barriers at near zero rotation. Further, the impact of reduced aspect ratio and enhanced boundary shaping on access to regimes of enhanced pedestal confinement and stability will be investigated utilizing NSTX-U operation at higher plasma current and toroidal field. Separately, theoretical studies have elucidated the distinct advantages of high-field-side-launched RF in obtaining much higher current drive efficiencies, which if realized could lead to significantly more efficient fusion systems. These are just a few examples of recent research that offers the potential to significantly improve the efficiency of future tokamak systems.

The U.S. program in 2020-2035 should focus on these potential breakthroughs through a dedicated program aimed at quantifying the benefits of such ideas. ***Early in this period, planned upgrades to DIII-D and NSTX-U should provide sufficient capability to test the basic aspects of the underlying ideas and establish the feasibility of steady-state tokamak operation (see [6] for details).*** However, either substantial upgrades to these facilities and/or a new facility will likely be required to extend these results to burning-plasma-relevant conditions where the self-consistency of transport, stability, and current drive can be assessed.

Fundamentally, moving towards burning plasma conditions will require increased values of normalized size $1/\rho_* \propto aB/\mathrm{T}^{1/2}$ to more closely approach reactor ρ_* values while maintaining achieved levels of collisionality $\nu_* \propto \mathrm{qna}/\mathrm{T}^2$ and normalized pressure $\beta \propto \mathrm{nT}/\mathrm{B}^2$. Under these assumptions (following Ref. [7]), larger field and larger size is favored since $1/\rho_* \propto B^{2/3}a^{5/6}$ with the loss power associated with a given ρ_* scaling as $\mathrm{P}_{loss} \propto \rho_*^{-5/2+\alpha} a^{-3/4}$ where α represents the scaling of transport with ρ_*. In this context, α is related to the process that governs heat transport: α = 1 is called gyro Bohm-like, α = 0 is called Bohm-like, α = −1/2 is called Goldston-like, and α = −1 is called stochastic-like. Figure 2 depicts P_{loss} and P_{alpha} versus $1/\rho_*$ for a representative case with gyro-Bohm-like

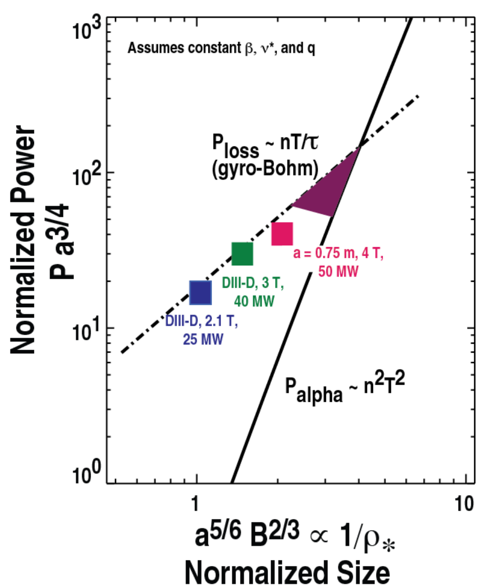

Figure 2

transport ($\alpha = 1$) assuming $\beta_N{\sim}4$ and collisionality levels consistent with burning plasma operation. In this figure, the crossing point of P_{loss} (dashed line) and P_{alpha} (solid line) yields the ignition point for conditions consistent with the above assumptions. The region just below this crossover point (denoted in magenta) is therefore the desired region of high gain (P_{alpha}/P_{loss}) operation. Note that lower values of β_N would act to move the P_{loss} lower and the P_{alpha} curve to the right, yielding an ignition (or equivalent high Q) point to higher normalized size $1/\rho_*$. This illustrates the importance of obtaining high β_N in reducing the size of fusion reactor systems. High β_N is also advantageous in increasing the bootstrap current fraction, thereby reducing the current drive (and related power) requirements.

A key issue in assessing the high β_N approach is the transport scaling as the normalized size is increased. The symbols in Figure 1 depict potential operating points for a progression of facility capabilities that could map out a gyro-Bohm transport path to a compact, high gain device: DIII-D sized device with B_T = 2.1 T, P_{input} = 25 MW (blue); DIII-D sized device with B_T = 3 T, P_{input} = 40 MW (green); and an a= 0.75 m, B_T = 4 T, P_{input} = 50 MW device (red). This figure suggests that a potential compelling path is to map out this pathway through a series of facility upgrades that enable successive assessment of this approach. Such an approach would also enable assessments of the current drive and active stability control requirements as the reactor regime is approached.

Note that detailed pedestal models predict that the highest performance (and hence highest power density) will be achieved with optimal shaping of the plasma (including aspect ratio, triangularity, and elongation). DIII-D and NSTX-U are well positioned to provide key information on the choice of the required parameters. This approach would also enable exploration of the physics benefit of high field operation, particularly the impact on plasma confinement, which is synergistic with the high temperature superconductor (HTS) R&D outlined below. In fact, practical experience could be gained with smaller HTS TF magnets if the magnet R&D delivers such a capability on the necessary timeline.

This plan is agnostic towards whether the proposed capability improvements outlined here require an entirely new facility. It is conceivable that the most cost-effective approach for delivering the highest performance facility depicted in Figure 2 is a significant upgrade of DIII-D or NSTX-U. Note that such an upgrade could be extensive. This plan is also agnostic towards whether this device should be steady-state capable and on the choice of aspect ratio. Such determinations should be debated and discussed through a coordinated community process that includes consideration of multiple mission elements (see Power Exhaust section below). The resulting upgrades or new facility should be designed, fabricated, and operated by a national team.

Large-bore high-temperature superconducting coils

Recent advances in the technology of high-critical-temperature superconductors (HTS) offer several unique benefits for potentially increasing performance limits of fusion systems including very high current density (smaller radial build needed for coil), operation at much higher magnetic field (increased physics margin or smaller machine size), and jointed coils (improved maintainability). Small-bore magnets with extremely high magnetic field (> 40 T) have been produced and operated. However, large-bore magnets from HTS tapes are much less developed.

Because the highest performing HTS conductors are presently made in tape form (rather than as strand), the extensive technology base including the testing and qualification of conductor developed by ITER to produce magnets from strands of Nb_3SN or NbTi may not be applicable to the production of HTS magnets. Therefore, extensive R&D will be likely be necessary to either develop HTS strands that can readily take advantage of the ITER cable-in-conduit-conductor (CICC) approach or develop new techniques that fabricate robust magnets from HTS tapes.

In order for the U.S. program to be in a position to utilize HTS magnet technology on the timeline articulated in this strategic plan, a development program should begin as soon as practical. Early stages of this effort should focus on assessing relevant means to produce coils from the HTS conductor and assessment of any performance degradation or other issues as the bore size is increased. These tests should conclude early enough that a HTS model coil could be developed and tested by 2035. For this activity, ***a program of similar scope as the development of the ITER Central Solenoid Model Coil will likely be required*** to develop the technology to a sufficient state of maturity that it could be confidently engineered into a future facility. The confidence in incorporating HTS magnets into a pilot plant or DEMO design would be further improved if practical experience could be gained through use of HTS magnets in the facility called out in the Steady-state and Power Exhaust sections of this plan. It should be noted that while HTS magnets likely offer advantages to their low temperature counterparts for devices with modest net electricity targets (< 250 MW), the benefit for 1-GW class devices is less clear. In this regard, systems studies (similar to previous ARIES and ACT studies) that quantify the anticipated benefit of HTS magnets on the cost of electricity in future power plants is recommended as a parallel activity in this area.

Power exhaust solutions for high-power density fusion systems

A key enabling element of any US strategic plan must be the development of power exhaust solutions and materials that can handle the very high heat and neutron flux intrinsic to these systems (materials discussed in next section). The U.S. program is reasonably well positioned at present to develop power exhaust solutions. DIII-D and NSTX-U have world-leading diagnostic sets and the ability to vary divertor conditions over a wide range. Targeted diagnostic upgrades (e.g., improved resolution bolometry, electron density and temperature) and/or additions (e.g., neutral density and ion temperature measurements) will further improve this positioning. Both facilities also have the capability to make modest divertor configuration changes to test emerging ideas. This research will be complemented by research worldwide on high-Z metallic walls (ASDEX-Upgrade, JET), long-pulse divertor operation (EAST, JT-60SA, KSTAR, WEST), and alternate divertor configurations (MAST, TCV). The developed solutions will have a natural testing ground in ITER where projected heat fluxes approach those anticipated in future fusion systems.

A key aspect of projecting these solutions to devices beyond ITER is the ability to confidently predict the boundary's ability to dissipate the very high heat fluxes anticipated in those devices. Unfortunately, projecting boundary solutions for future devices is quite uncertain due to the inability of edge modeling codes to accurately predict behavior in present-day devices as detached divertor conditions are approached. In this regard, a key aspect of any plan going forward is to reduce the predictive uncertainties through a ***science-driven, model validation approach*** to elucidate key features of the boundary solution and should include identification of phenomena that are not captured properly by measurement or by simulation. A second key aspect of this plan is the assessment of a range of ideas to improve the heat flux handling capability of the divertor through innovative configurations (e.g., snowflake, Super-X, Small-Angle Slot) and/or material choices (e.g., liquid metal walls). Hence, ***early in the 2020-2035 period, DIII-D and NSTX-U will focus a significant portion of their research program on improving the physics basis (and confidence) in the predictive capabilities of the divertor modeling suite of codes. New diagnostics are likely required to enable the necessary progress (see [8] for details). While each of these aspects can be developed and tested in existing facilities, confidence in such solutions for future devices may require substantially increased capabilities to explore of heat fluxes at the levels expected in fusion power systems.***

It should be noted that ITER will offer a compelling opportunity to assess boundary solutions at heat flux levels significantly higher than presently available devices and therefore should be an integral part of this plan. In addition, possible medium to longer-term upgrades to NSTX-U and/or DIII-D could include transitioning to medium or high-Z plasma facing components and tests of both static and flowing liquid metal divertors and/or walls.

If new capabilities are deemed necessary for addressing very high power densities, the device path outlined in the steady-state, high-performance section above potentially has many of the features needed in such a device. An example of the capabilities of this is shown in Figure 3 in which the anticipated heat flux is compared for a range of current and future devices. The heat flux in this figure is characterized as the anticipated perpendicular heat flux taking into account the core radiated power $q_\perp = P_{heat}\left(1 - f_{rad,core}\right)/2\pi R\Delta\lambda_q$. The challenge for projected power exhaust solutions from present-day devices (green points) to future devices (yellow points) is obvious from this plot with factors of 2-4 increases in power exhaust capabilities needed. Note that many of the future-device data points already contain an assumption of 70-85% core radiation in order to reduce $q_\perp$ to assumed allowable levels. For reference, if $f_{rad,core}$ were assumed to be 30% in these cases, the perpendicular heat flux would be significantly higher as denoted by the shaded region at the top of Figure 3.

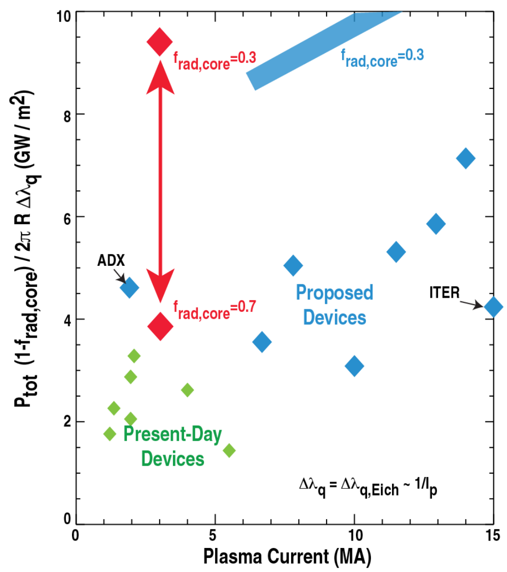

Figure 3

The expected range in $q_\perp$ for the last device called out in the succession of devices in Figure 2 is shown as the red data points in Figure 3. At $f_{rad,core}$ = 30%, $q_\perp$ would be more than a factor of four higher than is possible in present-day devices. Even at $f_{rad,core}$ = 70%, $q_\perp$ would approach the levels expected in future devices.

This available variability in $f_{rad,core}$ along with sufficient headroom above the L-H power threshold would provide an opportunity to develop simultaneous core and divertor radiative solutions for future high power density fusion systems. An additional key consideration for developing the physics basis for integrated core-edge solutions is the ability to increase the plasma density while maintaining low collisionality. Under the assumption of constant β and safety factor q, one finds that the density scales as $n \propto \nu_*^{1/2} B/a^{1/2}$. Hence, for the core-edge integration mission, higher field and smaller size is favored.

Such capability would provide access to significantly higher power density plasmas than are currently available anywhere in the world and approach those to be achieved on ITER, but in a smaller D-D device (potentially upgradeable to D-T) that has the virtue of better hands-on access for ease of diagnostic and other hardware upgrades and maintenance.

Materials that deliver high performance and long lifetime

Since facility availability is a key metric in the economics of a future fusion system, the lifetime of materials are a critical factor in reactor design and operation. Rapid material degradation due to plasma erosion or neutron bombardment could severely limit the benefit that could be gained from a high-power core/boundary solution. In this regard, the presence of 14 MeV neutrons and their deleterious effects are very specific to the fusion environment and therefore data quantifying such effects is sparse. For this area, two branches of research are envisioned. First, the effect of long-term exposure of plasma facing materials to divertor-like conditions must be understood, leading to the development and qualification of materials that meet the stringent demands of a fusion system. Second, these plasma facing materials as well as structural materials that can maintain their required properties (e.g, heat transfer, tensile strength, …) under high neutron fluence must be developed, tested, and qualified. The development of these materials requires a better scientific understanding of the processes that modify the material properties. This understanding requires continued improvement in theory, numerical simulations, and experimental capabilities. The importance of exposing materials to relevant neutron spectra and fluences is evidenced by the American Nuclear Society recently selecting this as a Nuclear Grand Challenge [9].

During the 2020-2035 period, the U.S. program will develop two new facilities to address these issues. ***Early in this period, the U.S. would bring online a material test facility that has the capability to expose materials to relevant heat fluxes over the range of plasma conditions expected at the material surface in future fusion systems***. Such a facility (similar to the proposed MPEX facility [10]) would provide a unique test bed for new materials including low-Z composites and liquid metals. Once the characteristics of promising materials have been quantified, they could then be installed in existing confinement facilities to test the capabilities in the tokamak environment. ***Later in this period, the U.S. would develop and build a volumetric neutron source (VNS) for tests of modest-scale-sample materials and components in a 14-MeV neutron environment.*** This would complement efforts worldwide that are focused on small-sample exposure of materials (e.g., IFMIF). Delivering the necessary capabilities in a cost-attractive manner should be a key factor in the choice of concept. A facility that combines a gas-dynamic trap with HTS magnets may offer a cost-effective means for this role [11]. A VNS would truly be a world-leading scientific instrument, enabling U.S. and international researchers to explore effects not possible at any other facility. Synergies with other programs within DOE, NASA, and NNSA are also envisioned as a means to expand the user base of this facility.

Other Considerations In The Plan

Blanket systems that breed tritium and extract high quality heat

A very important component of any efficient fusion energy system will be the ability to efficiently breed tritium and convert the fusion energy to high quality heat for electricity production. In particular, achieving sufficient tritium breeding and extraction efficiency is absolutely critical to the success of fusion given the very limited availability of tritium. As noted above, the worldwide fusion program has developed plans that move rapidly towards the demonstration of fusion energy. Hence, it is anticipated that there will significant technical development of blanket systems through the ITER Test Blanket Module program and further dedicated research worldwide. ***The U.S. should invest a sufficient amount in blanket R&D to ensure that we are capable of leveraging this international investment while addressing specific issues that high power density solutions will benefit from and/or additionally require.*** Specific examples include heat generation and removal at very high-power density and potential new forms of blanket solutions and those that are predicted to achieve very high thermal efficiency (e.g., helium-cooled PbLi).

Secondary Pathways

As noted above, this plan is specifically based on utilizing tokamak capabilities due to the maturity of the development of the tokamak relative to other configurations and the extensive investment presently in tokamak development worldwide. However, it is recognized that there are significant challenges for the tokamak that still remain a concern or need to be resolved (e.g., current sustainment, plasma disruptions) and that compelling secondary pathways should be developed to reduce the implied risk that these challenges pose to the overall plan. For this reason, ***this strategic plan envisions that, provided adequate funding availability, the U.S. will develop at least one configuration to sufficient maturity to adequately inform an evaluation of the performance capabilities of such a configuration(s). Such investment should tailor the research program to favor those configurations with the most mature physics basis.*** At present, it is our view that the next most mature configuration is the quasi-symmetric stellarator, based on the emerging theoretical basis for quasi-symmetric configurations in combination with recent results from LHD and W7-X. In this regard, the U.S. (possibly in partnership with other countries) should develop a proof-of-principle-scale quasi-symmetric facility capable of assessing theoretical predictions of turbulence-driven ion thermal transport and energetic particle confinement. The timeline shown in Figure 1 would enable evaluation of this configuration alongside research seeking to establish the viability of steady-state tokamak operation. This would allow a comparative study of the potential of the two approaches for future fusion development. In addition, continued collaborations on LHD and W7-X are a cost effective means to remain involved in and aware of the latest developments that advance the maturity of the stellarator line.

Research in less mature and therefore more speculative configurations should continue to be a feature of the US program. Such investment should be modest but is required in order to continue encouraging out-of-the-box thinking on fusion systems of the future. The choice of which configuration(s) to pursue, as well as the appropriate research focus of those facilities, should be determined by a national team of experts.

As the technical basis of each of non-tokamak pathways matures, it is likely that potential "show-stopping" issues will become evident. Resolution of these issues will necessarily become the focus of the R&D program and will likely introduce some (possibly significant) time delay in developing the basis for a cost-attractive pilot plant or DEMO.

The "Without ITER" plan

It is difficult to develop a 'no-ITER' strategic plan due to a range of uncertainties associated with the timing of the withdrawal, the knock-on effect on the U.S. base program, and the international response to such an event. In the absence of ITER participation, the U.S. needs an alternative means of access to the burning plasma regime to support fusion energy development. While other devices worldwide may provide opportunities for U.S. participation in burning plasmas (e.g., CFETR), the plan outlined above may need to be modified to accommodate a potential U.S. burning plasma experiment. This could entail expanding the mission scope of the facility outlined in the Steady-State and Power Exhaust sections to include a D-T phase to enable burning plasma research. It is anticipated that the development of such a facility will take approximately 15 years from program initiation (i.e., concept definition phase) to D-T operation and may take longer depending on budget availability. Any plan that incorporates a U.S. burning plasma device will likely have a knock-on effect on the number of facilities that the research program can support during its construction and will likely result in schedule delay relative to the "With ITER" plan in achieving the necessary tasks required for design/construction of a pilot plant or DEMO facility. These features are apparent in Figure 4, which defines a potential strategic plan assuming the U.S. is not a participant in ITER. ***Note that a feature of the overall strategic plan outlined here is that the program logic does not change markedly with or without access to ITER with the primary impact being the timeliness of executing the required elements of the program.***

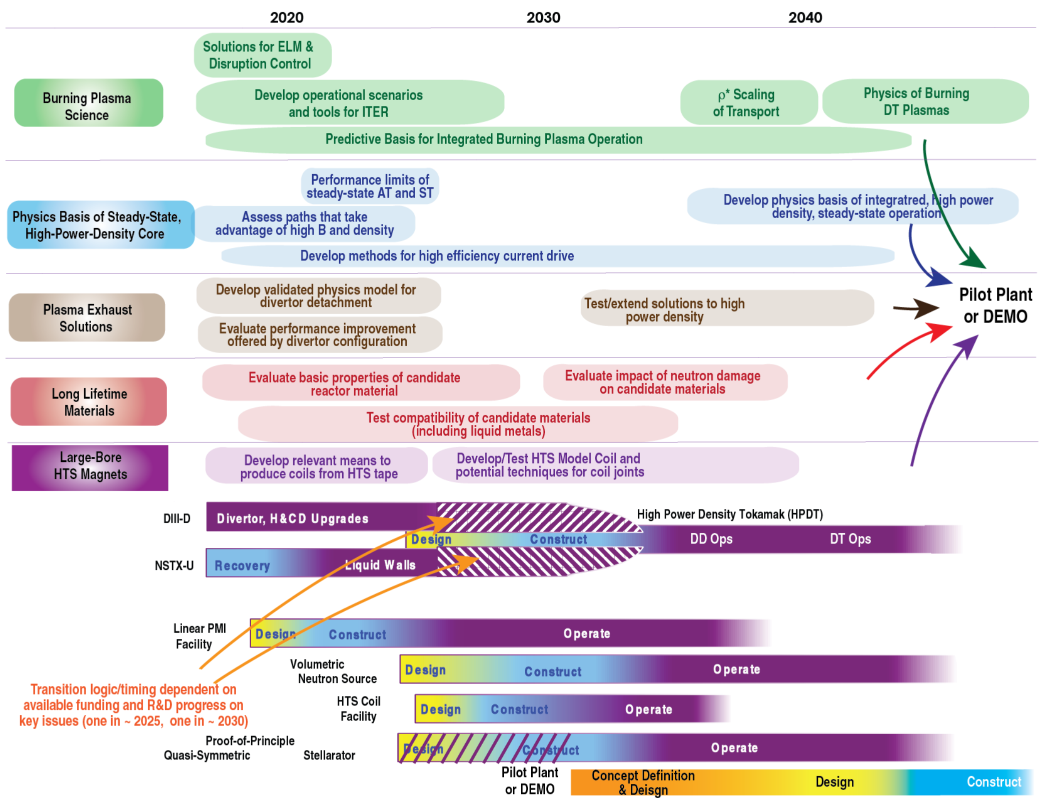

Figure 4: Strategic plan assuming U.S. is not a participant in ITER. Primary difference from the 'with ITER' plan is the replacement of ITER by a D-T phase for the High Power Density Tokamak and as associated delay in other elements.

Technical Objectives and Milestones

Objectives

Establishing confidence in our ability to predict the behavior of plasma, materials, and blanket systems in the fusion reactor regime will be critical in establishing credibility that a cost-attractive pilot plant or DEMO will succeed in its mission. In this regard, a key objective of any U.S. strategic plan should be developing, testing, and validating physics and engineering models that have the largest impact of overall system performance. Alongside this knowledge development, there are several technical objectives that will simultaneously serve to prioritize resource allocation and generate regular enthusiasm-generating events necessary to motivate the funding needed for successful execution of the plan. These milestones are listed below.

Year 1-5 Milestones

- Burning Plasma Science
  - Deliver on U.S. in-kind contributions to ITER for first plasma
  - Develop robust disruption mitigation and avoidance systems for ITER
  - Provide multiple solutions for ELM control on ITER
- HTS Magnets
  - Establish practical means for fabricating fusion-relevant cable and large-bore coils from HTS conductor
- Physics of High-Performance, Steady-State
  - Demonstrate fully non-inductive, "in principle" steady-state tokamak operation on DIII-D and NSTX-U
  - Establish physics basis for stationary, optimized pedestal performance
  - Establish viability of new heating and current drive systems for high field device
  - Initiate community process to identify the appropriate next-step approach (i.e., upgrades or new facility) for achieving high power density and high divertor heat flux capabilities
- Plasma Exhaust Solutions
  - Establish high confidence in SOL/divertor predictive models highly dissipative conditions
  - Identify most promising divertor configuration for high heat flux operation
  - Initiate concept definition and conceptual design of next-step approach combining high power density and high divertor heat flux capabilities
- Materials Development
  - Begin operation of a plasma facing material test facility

Year 5-10 Milestones

- Burning Plasma Science
  - ITER First Plasma
  - Establish physics basis for maximizing ITER performance utilizing existing facilities

- HTS Magnets
  - Fabricate HTS model coil and begin testing
- Steady-state High Performance & Power Exhaust Solutions
  - If upgrade path is chosen, implement and exploit facility upgrades
  - If a new facility path is chosen, finalize design of new facility combining high power density and high divertor heat flux capabilities
- Materials Development
  - Identify most promising plasma facing material including liquid metals
  - Complete conceptual design of Volumetric Neutron Source

Year 10-15 Milestones

- Burning Plasma Science
  - Begin ITER DD operations; assess ρ^* scaling of transport and power exhaust solutions
- HTS Magnets
  - Qualify large-bore HTS magnet at relevant magnetic field levels (e.g., model coil at ~ 20 T)
- Steady-state High Performance & Power Exhaust Solutions
  - If new facility path chosen, finish construction/begin operation of facility combining high power density and high divertor heat flux capabilities; initial assessment of high power density core solutions and high heat flux dissipation solutions
- Materials Development
  - Construct Volumetric Neutron Source and begin operation
- Future: Begin conceptual design of cost-attractive pilot plant or DEMO

Year 15-20 Milestones

- Burning Plasma Science
  - Begin ITER DT operations
  - Demonstrate Q=10 operation and evaluate physics of burning plasmas
- Steady-state High Performance & Power Exhaust Solutions
  - Demonstrate core-edge-integrated, high-power density operation
- Materials Development
  - Identify and characterize the most attractive plasma facing and structural materials from a neutron handling perspective through dedicated exposures
- Future: Finalize design of cost-attractive pilot plant or DEMO

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