# A Perspective on the US Fusion Technology R&D Path Toward Fusion Energy

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This personal perspective on US fusion technology brings together an overview of some recent national US planning activities, notes on past US activities, the strong redirections resulting from the commitment to ITER and the decline in critical areas such as blanket technology using three themes. The first is the evolution of US fusion nuclear science and technology (FNST). We once led the world in FNST, left a signficant legacy that still has impact, but now face severely depleted resources and capabilities. This status is discussed against the current landscape with ITER looming large and Europe and China taking different approaches to the next step beyond ITER. The second theme is current influences on the US progam that bring some optimism for reinvigorating US activity in FNST. Among these are strong engagement by the physics community in meaningful discussions of FNST, new transformative capabilities that could improve our vision of a fusion reactor, investment by private industry, and more apparent readiness by government decision makers to embrace a new version of the pathway for fusion R&D. The third theme is about opportunities. This is a time when the US is reevaluating its role in the world program and refining its view of the path foward in fusion. Redirection within the US program toward some consensus-view of a path forward with a much stronger FNST component is a challenge. The perspective presented includes comments on the culture of the US fusion program, opportunities for transformative technology, and the challenges of rebuilding a US work force in FNST.

Keywords: fusion technology

## 1. Introduction

The challenge of developing fusion includes advancements in fusion nuclear technology. Power exhaust, the tritium fuel cycle and materials are three important areas where much work is needed, particularly where innovations can simplify or improve fusion designs. This paper gives some examples of possible innovations. However, the status of the US program in fusion nuclear science and technology (FNST) is the main theme. The perspective on US fusion technology and pathway toward fusion energy is personal (my opinions). I have worked in FNST for over four decades.

### *1.1 Evolution of US FNST to 2005*

In the pre-2000 development of FNST, the US exerted strong leadership[2] through a combination of experimental research programs and design studies. These studies create a vision of a reactor concept and identify issues that require solutions and included the Blanket Comparison and Selection Study (BCSS) [1], which focused on breeding blankets but did not include liquid surfaces, the ARIES series [2-7], which explored various confinement options and technology solutions, and designs for two ITER-like facilities. The Fusion Engineering Device or FED [8] was to be built in the US. INTOR was an international collaboration that led to ITER. The team studied options that would test various blankets and materials. [9-10] and international groups were refining the R&D goals for FNST. [9-13]

One US legacy is a strong impact on strategic planning in FNST worldwide, as is evident in citations in the major planning documents. Table 1 (next page) notes some of these. Also, many senior fusion researchers in FNST outside the US had all or part of their graduate education in the US.

The brief historical summary here sets the stage for my perspective on US FNST in Section 1.2 where I contrast two snapshots-in-time of the status of US FNST. The first is from ~2005 and the second at the present time.

In the period from the mid-1980's to ~2005, funding for FNST diminished and many experimental activities ceased altogether and some were redirected to support ITER. US-led reactor design studies with significant international participation continued. Following ARIES, the APEX and ALPS studies investigated transformational technology solutions for blankets, first walls and divertors, e.g., liquid metals and molten salts. [14-15]

In 2005, many issues of power exhaust, tritium handling and the fuel cycle, and materials degradation under ion and neutron damage were well recognized. In power exhaust, the important issue of power balance between the first wall and divertor was typically characterized through the radiated power for each, and solutions that could enhance these were offered. The designs included plasma edge modeling of the impurity feedback from the walls and their role in radiative losses. The US fusion development path typically included a Component Test Facility and a high fluence neutron source to study fusion neutron damage to prepare for a DEMO that had reactor features but lower availability.

<u>US FNST Snapshot circa ~2005</u>

- The US reactor design effort remains strong (but is severly diminished by 2010). Its design portfolio, with challenging technology and physics in visionary reactor designs for various confinement concepts, is an influential legacy.
- US FNST had a strong experimental phase through the 1990's plus continuing design studies. Its imprint in defining key R&D issues is also an influential legacy.

Between 2005 and 2010, two impactful US strategic planning activities generated impactful resource documents. This section includes these because much of the activity occurred ealier and the mindset reflected the snapshpot above. The Greenwald Report in 2007 aggregated extensive input from the fusion comunity, organized this information into themes, had extensive sections on fusion technology, and identified gaps and prioirities for research. [16] The Research Needs Workshop(s) or ReNeW followed with a similar scope but different format. [17] *Taming the Plasma Interface* and *Harnessing Fusion Power* were two of

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[2] The US fusion budget with inflation adjusted dollars and that for R&D on FNST were respectively ~4X and ~10X the current allocations. FNST included design studies, experimental facilities for PSI, high heat flux testing and international collaborations on PFC development.

**Table 1. Summary of Activities and Trends**

| | *Activities – US or international (INT)* |
|---|---|
| **1980's** | |
| INT | ▪ New confinement devices with more power.<br>▪ Emerging new concern with PSI.<br>▪ Preference for diverted plasmas in tokamaks.<br>▪ INTOR - influential design study. [9-10]<br>▪ Leaders from the US and from other countries consolidated an information base and highlighted critical issues the fusion nuclear technology. [9-13] |
| US | ▪ Blanket Comparison and Selection Study [1] was an influential design study. |
| **1990's** | |
| INT | ▪ Focus on walls and test blanket modules for ITER.<br>▪ Much effort redirected from DEMO PFCs and blanket concepts to ITER. |
| US | ▪ ARIES studies, 1988-2006 [2-7] one with LM divertor [4]<br>▪ US championed dual coolant Pb-Li blanket.<br>▪ US withdrew from ITER.<br>▪ APEX explored liquid surface wall and blanket technology [14] |
| **2000+** | |
| INT | ▪ EU Power Plant Study [18,19] influenced decisions on program emphasis. |
| US | ▪ APEX continued; ALPS, another design study, focused on PFCs. [14,15]<br>▪ 2007 Greenwald Report was an in-depth assessment of US FNST. [16]<br>▪ ReNeW had extensive input by researchers plus documentation from the earlier studies. [17] |
| **▪ 2010+** | |
| INT | ▪ Much of the work on liquid metals and liquid metal MHD stopped.<br>▪ EU DEMO R&D programs, with others following, redirected emphasis based on an extension of existing technology, e.g., water-cooled PFCs. [18-24]<br>▪ Preference (outside US) for solid breeders and in ITER test blanket modules. [24]<br>▪ Some effort on high temperature blankets and He-cooled refractory PFCs was retained.<br>▪ EU DEMO R&D programs expanded DEMO support; goals were achievable near-term components targeted development to resolve issues.[25-29]<br>▪ China's strong design activity for its CFETR developed baseline then final options, targeted R&D to resolve issues, and seeks funding authorization in next 5 years. [30-34, *many 2018-19 papers, search CFETR*] |
| US | ▪ Several post-ReNeW studies assessed FNST. An extensive national effort in FNST was reformed into a 2012 FESAC study. [35] well-attended workshop on PSI in 2016 had white papers advanced divertors and liquid surfaces. [36]<br>▪ The Burning Plasma Report [37] and one on Enabling Transformational Capabilities [38] used input from national planning meetings and workshops. A roadmap activity identified possible budget-constrained options. [39]<br>▪ The Office of Fusion Energy Sciences (FES) funded a limited study on post-ITER confinement machines, specifically a tokamak fusion Pilot Plant, and CTF's based on an Advanced Tokamak and on a Spherical Torus at institutions that championed these differing approaches. [40-42]<br>▪ Two recent FES workshops reflect targets for redirecting the program. One probed ideas for a US fusion neutron source and the gathered experts in blankets and the tritium fuel cycle to consider product directions to reinvigorate a US program in these areas. FES also initiated a 3-year strategic planning exercise under the auspices of APS that covers all its fusion and plasma science programs. |

ReNeW's overarching themes. Thrusts 10-14, with the titles below, detailed how specific issues might be addressed.

- Decode and advance the science and technology of plasma-surface interactions
- Improve power handling through engineering innovation
- Demonstrate an integrated solution for plasma-material interfaces compatible with an optimized core plasma
- Establish the science and technology for fusion power extraction and tritium sustainability
- Develop the material science and technology needed to harness fusion power

### *1.2 Evolution of US FNST since 2005*

Embedded in the US legacy of designs and derived FNST R&D issues were design features that have since changed. For example, the first walls in the ARIES and other designs received power only by radiation, charge-exchange neutrals and neutron heating and had "conforming wall" solutions. The issue of convected power to the walls was not yet understood. So, these designs ignored the leading edge[3] issue that plagued ITER. Its evolution has forced a recognition of the magnitude of effort in science-based engineering needed for its design as well as the complexity in engineering of the subsystems and the strong interdepence of competing design requirements.

Among the important changes are our much improved understanding of 1) power dissipation in the plasma edge, e.g., convected power to the walls, ELMs and disruptions, 2) issues related to tritium management, the fuel cycle and blanket test modules, and 3) their signficant impacts on what can be accomplished in ITER and the research path for devices that follow ITER. The resulting and more realistic realization of the challenges we face in fusion have changed not only our outlook but fusion's culture overall.

Other changes include: 1) increases in computational power have significantly extended the range for modeling in such topics as damage to materials, plasma edge processes, and liquid metal MHD, 2) research in liquid surface plasma facing components (LSPFCs) has expanded due both (a) improved plasma peformace with Li and (b) the potential for novel wall protection, and 3) the emergence of strong fusion programs with new researchers and new facilities in Asia.

During the time since 2005, important design studies and planning activities worldwide have incorporated the changes noted above. With ITER's progress, and the considerable funding from its partners, the flavor of these planning activities has changed. Government funders have more information for their projections. Fusion has much higher visibility and higher scrutiny, particularly with the cost-escalations for ITER. These considerations have forced fusion programs to develop strategic plans where advances in technology come from more detailed and credible steps in technical progress than offered earlier strategic planning.

The European Power Plant Conceptual Study has been extremely influential. [18,19] Of four options examined, an aggressive design with technology that required leaps in development, and a "roll forward" option that uses water-cooled PFC technology to be proven in ITER. The baseline strategy in the EU roadmap for fusion [20] calls for the roll forward approach. [21-23] The alternative strategies still include liquid metal PFCs. Other major world programs have also been preparing roadmaps. The Chinese program has developed an impressive collection of capabilities for

[3] When portions of misaligned tiles, probes, etc. protrude into the plasma, the toroidal facing sides intercept the very high parallel heat and particle loads, particularly during transients, that can easily melt these leading edges in high power density plasmas.

their Chinese Fusion Engineering Test Reactor (CFETR). The R&D pathways for the EU DEMO and the CFETR, and its follow-on devices, differ in their acceptance of risk for the respective design features. The EU track follows a roll-forward plan to use ITER-proven technology such as a water-cooled first wall and divertor and blankets based on the ITER TBM results, whereas the Chinese CFETR incorporates some more advanced design. SOFT2018 has a good collection of papers on these activities. [30-34]

In US, materials and plasma-materials interations within FNST have preserved some prior capabilities while design studies and blanket technology have eroded severely. Direct funding for work on blankets dimished to near zero over the last decade, and what work was done proceeded in large part via international collaborations, led by UCLA. Part of these changes came from redirection of effort to support ITER. Also, early on, much of the US expertise in blankets focused on the ITER Test Blanket Modules, but this effort was later dropped. What had been a much broader program on PFC development and materials for fusion reactors became focused on the design of the ITER first wall.

The complexion of the US program is changing. A positive trend is a much stronger engagement by physics researchers in strategic planning in discussions on fusion technology, although the motivation comes in part from the struggles of dealing with a US domestic program denuded of much of its diversity in physics by the higher priority for ITER and severly depleted in non-ITER related technology. The trend in the rest of the world away from a one-stop high-tech DEMO as a credible path toward fusion energy also prompted rethinking in the US program. A US Test Facility, a Fusion Nuclear Science Facility (FNSF) or an alternative path was explored in two ways. First, design concepts for FNSF's based on an Advanced Tokamak or Sherical Torus and an alternative path with options for a fusion tokamak Pilot Plant (an old idea [43]) were developed by three institutions that championed confinement concepts. [40-42] Second, the Fusion Nuclear Science Pathways Assessment or FNSPA was a strategic planning exercise that subseqently morphed into the FESAC Materials Science and Technology Research Study. [35]

The summary above sets the stage for the next snapshot.

*<u>US FNST Snapshot circa ~2018</u>*

- We continue to struggle with a US domestic program denuded of much of its diversity in physics by the higher priority for ITER and severly depleted in non-ITER related technology. Parts of a fragmented US reactor design activity remain in tact for separate missions within their institutions, e.g. PPPL support for Wendelstein 7X and CFETR and PPPL and ORNL support for NSTX-U.
- Strong disparities between the US and major programs in the rest of the world in the maintenance of capabilties in FNST, the US view of a DEMO, and the pathway forward in fusion led to a US program that was out of step with the world.
- The strong history of innovation in the US program has been a strength. This has justified some research directions by which progress might leap forward. Some of this innovation is now coming through investment by private industry.
- Significant increases in funding that could enable a more technology-rich program have not been forthcoming. The program requires a rethinking of its resources, its role in the world program, and what is compelling about fusion in the context of US priorities for its federal spending.

## 2. *Influences on US FNST*

During the last three decades, two strong influences on the US mindset in our approach to fusion technology have been 1) an overall mission of science rather than energy and 2) a US DEMO defined as a machine that enabled private industry to build a prototype reactor.

### *2.1. The Science Mission*

Fusion as science differs from fusion as an energy program. Realization of fusion power is a strong widespread personal motivation among US fusion researchers. However, we joined a program in the US Department of Energy but one administered under the Office of Science.

The early years (mid-1970s to mid-1980s) saw a strong growth of FNST programs and experimental facilities. Ed Kintner was an early Associate Director of Fusion Energy. His background was in fission fuels for submarines.

In contrast, this millenium's first decade saw a narrower science focus in the program's oversight as revealed in the language of budget authorizations and the wranglings of the US administrations and congress to reach this language. So, we straddled the fence between applied science and engineering and technology. We spoke of science-based engineering needed to enhance science outcomes.

### *2.2. A US DEMO*

A reactor-like device with sufficient robust technical solutions to enable private industry to invest in a proto-type fusion reactor has persisted as the US view from early planning, such as that for the Fusion Engineering Device [8]. Many FNST challenges, such as neutron damage to materials, were already recognized. However, the solution and issues related to power exhaust were less well developed. For example, the advantages of diverted plasmas were not well established and designs with limiters and bundle divertors were still being explored.

As existing confinement experiments added power, the role of the plasma edge, generation and feedback of impurities from the walls, and issues related to core power density became more prominent. A figure-of-merit of cost of electricity, which scales with plant size, favors high power density. However, this makes the solutions, which have high power loads on the PFCs, much more challenging. This classic issue for fusion, and other energy systems, persists and can only be resolved by developing robust designs with confirmed and acceptable materials performance.

Section 1.2 described how the challenge of developing suitable integrated FNST in-vessel subsystems became better articulated in the ITER era. Interest grew in first in the US and then worldwide in a CTF. However, the earlier US mindset of a high-tech DEMO persisted. This definition diverged from those in the rest of the world, where a DEMO was the post-ITER step in whatever progression of devices was needed to realize fusion energy.

The US view noted above persisted in part to maintain two parts of a consistent story. First, the number of devices that needed to be funded by the government did not escalate. Second, the technical agenda for FNST had a science base that was valuable and served the dual purpose of supporting ITER and the science-based understanding needed to lay out a post-ITER development path.

## 3. *US FNST – Challenges and Opportunities*

Fusion has its technical challenges. Our understanding of fusion reactor systems has progressed, but we do not yet have the solutions we need. What it will take to deliver robust FNST is continuing aggressive development promoted, one hopes, by effective world-wide cooperation.

ITER has been a wake-up call. Its design and nuclear systems may not be optimal, the project has coalesced into a buildable design with continuing support by its partners. We will learn how a burning plasma distributes its energy.[4]

We are far enough along to be confident that a fusion reactor can be built and produce electricity. The fundamental issue is whether a workable and sufficiently attractive solution can be realized in the not-too-distance future.

***We do not have it right yet. We can do better.***

We pass this baton to the new generation of fusion leaders.

The tokamak has been the proven workhorse in developing fusion this far, and innovative ideas for diverted plasmas and superconducting magnets may lead to new solutions as might other confinement concepts (spherical torus, stellerator, etc.) which have some advantages in confinement but also technical challenges, primarily in how to integrate more complex coils designs with the plumbing needed for the PFC-blanket systems.

Next, three aspects of rebuilding the US FNST are highlighted in relation to the earlier discussion: 1) Ongoing strategic planning; 2) An alignment of stakeholders to support a reinvigorated US FNST program; 3) US innovation and opportunities; and 4) some concerns about rebuilding a broad program and a suitable work force.

### *3.1 FNST – Strategic Planning*

The US must now re-evaluate its role and strategy for FNST, and this is underway. We have some remaining strengths but a diminished capability overall, and must overlay this position on a much stronger world program.

The US National Academy of Sciences (NAS) used input from the fusion community to develop a recently released assessment and recommendations on a strategic plan for burning plasma research.[37] The recommendations include doubling the program's budget, significantly increasing the fraction of the budget for fusion technology, and a strategy of preparing for a small fusion pilot plant (DEMO) preceded by a H/D-D/D device to confirm a solution for power exhaust. This is supportive and encouraging. However, past assessments of the US program also have recommended increased R&D on fusion technology and greater funding.

A study by the Fusion Energy Sciences Advisory Committee (FESAC) identified Transformative Enabling Capabilities (TECs). [38] This gathered experts from fusion along with outside experts in areas such as Additive Manufacturing (AM) and High Field High Temperature Superconductors (HFHTSC) and posed the question: How might these advances radically alter the development path for fusion? The next section gives some examples.

FESAC has started a prioritization study. The first step of soliciting community input is underway using a Community Planning Process (CPP) [44] organized by the American Physical Society (APS). APS-CPP has helped other Office of Science programs achieve consensuses on their goals and priorities. In the author's opinion, the objective of consensus makes this process more powerful than past fusion planning activities.

The process provides opportunity for proposing initiatives, and indeed all program elements including existing activities and facilities are to propose initiatives. Various Expert Groups focused on subtopics within fusion science and technology will assist the Program Committee[5] (PC) in evaluating the initiatives. Individuals or groups participate by proposing one or more initiatives. Each initiative places them in an Advocacy Group. Community members may also ask to join an Expert Group. Comments online, participation in webinars, and discussions at workshops are ways the community can give feedback.

Our program seems poised for new FNST initiatives, but these will await both increased funding and recommendations from the ongoing strategic planning. Two recent (2019) FES workshops kicked of processes to use US strengths and capability in activities that enhance the US as a strategic partner in world FNST activities. One explored the potential benefits of a near term US fusion neutron source. The second gathered experts in the tritium fuel cycle to explore productive future R&D directions for an ORNL-led program that will supplant the old blanket program.

### *3.2 Rebuilding FNST Expertise – Positive Signs*

We have faced a long period where funding for US FNST has been stagnant with minor ups and downs. Now some more general positive signs are appearing.

Within the Department of Energy and the Office of Science (SC), there appears to be a more positive attitude toward investment in fusion technology beyond that for ITER. We passed through a time where the federal management of fusion had interim acting staff in several leadership positions. This hampered decision making. Now we hear supportive comments. In remarks to FESAC, Paul Dabbar, Undersecretary for Science Policy, noted "*a strong opinion that this is both a discovery science and an applied energy* [science], *and there's no reason to say it's either-or:...*" [45] In questions, he said support for technology facilities as enabling capability is an accepted part of SC programs.

Another encouraging sign is greater readiness than in the past by senior physics leaders to accept that a much stronger near-term FNST R&D program is necessary. This is part of a needed cultural change in fusion. The extensive discussions in two well-attended national strategic planning meetings confirmed this readiness. These meetings provided community input to the NAS Burning Plasma Study.

Strengthening FNST in a physics-dominated program is difficult for a couple of reasons. First, the change in emphasis implies redirection of funding that may threaten some parts of the physics program. Second, a deeper understanding of the technology issues along with the physics requires significant extra work. A person seeking this perspective must go beyond their own discipline to appreciate the depth of science-based engineering in FNST and the complexity and interrelationships of the subsystems needed to make a fusion reactor work.

### *3.3 Rebuilding FNST Expertise - opportunities*

Innovation has long been a strength in the US fusion program. The continuing birth and refinement of new ideas in the US as well as outside, e.g. in liquid metal surfaces and divertor concepts, is one source for optimism about fusion. The first three examples below of innovative technology

[4] Confinement experiments supply external energy. Doing this helps control the plasma. In a burning plasma, fusion produced α particles energize the rest of the plasma. Understanding the control of this burning plasma is the primary reason we are building ITER.

[5] The author is on the CPP Program Committee or PC, in the Magnet and Technologies Expert Group in the Fusion Materials and Technologies Program Subcommittee, and leads the Enabling Technologies Cross Cut activity.

each could have big impact on what a fusion reactor would look like. Several together might revolutionize our vision of fusion. The last example (heat pipes) is of smaller scope but shows how we might adapt older technologies.

*High temperature high field superconductors (HTHFSCs) and demountable SC joints*

The primary benefits from HTHFSCs is that the higher fields can enhance the performance and feasibility of fusion reactor designs and potentially reduce the time and cost of research needed to realize power generation. The higher fields enable more compact burning plasma experiments. Designs with the high-energy gain and high power density are thought to be more economically attractive for commercialization.

Operating temperatures higher than conventional low temperature SCs eases the requirements for cooling and insulating the magnets. The higher temperature operation also raises the prospect of using demountable joints for these coils. This change would be truly transformational for construction and maintenance. In most current reactor designs for toriodal systems, the in-vessel systems are segmented so they can be withdrawn between through ports between the SC coils. The ARC and SPARC conceptual designs incorporate these features. [46-49]

*New materials and materials architectures*

New materials, materials designs and processes, e.g., "advanced manufacturing" (AM+), may have the potential for radically redesigned components that can survive the harsh fusion environment and improve the reactor's performance. High entropy alloys and new cermets are classes of new refractory materials that have not yet been explored much in fusion. [38,50,51]

Additive manufacturing (AM) and AM+ enable novel features that include complex geometries, for example incorporating complex cooling passages, and transitional structures that replace welded of brazed joints. Materials or constituents with hard-to-machine refractory metals are of interest for PFCs. The hope is to exploit local control of the material microsuctures to mitigate degradation as the materials evolve due to neutron and ion damage and operation at high temperatures and under thermal stresses. Also, rapid design-build-test iteration cycles offer potential to shorten development times.

*Liquid surface PFCs (LSPFCs)*

Lithium (Li) in the plasma edge can enhanced the performance of plasmas. [51] Fast flowing and slow flowing are two basic classes of liquid metal PFCs. Russians developed the nearly static Capillary Pore System (CPS) after exploring fast flow systems and the issues resulting from the MHD-controlled flow. [52-53]

The hope is that LSPFCs will be an attractive alternative to handle high steady-state and transient plasma heat and particle loads in a fusion power plant, or at least a valuable technology that can advance fusion along it R&D path. Liquid surfaces continually replenish the plasma facing surface and eliminate concerns regarding the erosion and cracking of solid materials that are being damaged by ions and neutrals. The material requirements for liquid surface PFCs differ from those for helium or water cooling, due in part to the corrosive nature of the liquids. [53]

In addition, certain liquids, e.g. lithium, can strongly improve plasma confinement and lead to smaller, more economical reactor designs. There are however, several important knowledge gaps in these systems, including managing the tritium fuel retention, maintaining clean surfaces for reliable flow, counteracting mass ejection forces, determining operating temperature windows, and demonstrating helium ash exhaust.

*Radiating refractory heat pipes (RRHPs)*

A heat pipe (HP) transports heat from its evaporator and rejects the heat at its condenser. HPs have been used for many years in many applications. The jacket materials and range of liquids inside enable a wide range of operating temperatures and uses. Lithium (Li) provides the highest performance with operating temperatures in the range of 1000-1400° C which requires refractory metals.

The exploration of HPs for fusion has been incomplete. The author and collaborators exposed a high-performance HP to a hydrogen plasma and magnetic field as a first step toward a future proof-of-principle test in a long-pulse tokamak. [54] In one of many possible configurations, the HP condenser radiates to heat sink that surrounds but is not mechanically attached to the condenser. This "pull away" concept may offer advantages in remote maintenance.

### 3.4 Rebuilding FNST Expertise - Funding

The magnitude of a broad fusion program is a strategic challenge and significant obstacle. Even before the step of a burning plasma, demonstrating confinement with some advanced technology requires a plasma experiment with long pulses, high power density, and therefore the plumbing for active heat removal and fairly high cost. A broad program would also have technology facilities to develop and test materials and FNST subsystem mock-ups.

Fusion has evolved into big science. Funding big science is a worldwide concern. Steven Weinberg among others has spoken eloquently on this issue. [56] Moreover, the US overlays this concern with the additional issues of (1) a strong anti-science mindset public opinion and (2) the political mechanics of the annual authorizations for federal spending with an increasingly rancorous and dysfunctional governing body.

Let's assume we've identified The Path Forward (TPF) that is the most cost-effective way to accomplish this. The TPF budget is many multiples of the current US fusion budget. So, the answer is money with a continuing resolve to realize fusion. In the US, we do not have the resolve. So, our program must understand how the world will collectively rally the world path forward (WPF) and identify roles that build on US strengths to make us a critical and accepted partner. Prerequisites are that we correctly project a reasonable WPF and can identify good critical US roles.

We see some aspects of the TPF of this abroad where sufficient resolve leads to investment in capabilities, most notably in China, but also with forward-thinking investments in Europe and Japan, noted below, and elsewhere. Space is not available here to describe these but the recent SOFT conference [57] has many papers.

### 3.5 Rebuilding FNST Expertise – Work Force

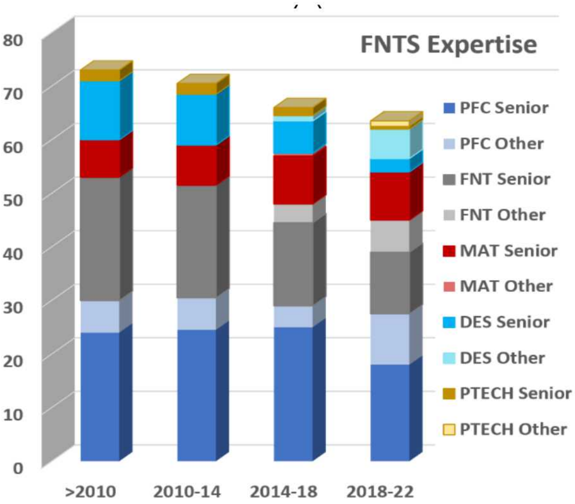
This section characterizes the demographics of US FNST capabilities to set a basis for what the program will need to rebuild this expertise. The figures show the categories of expertise below with the work force divided between senior staff and newer researchers and their populations in 4-year time increments. Senior staff means those with a work experience of 4 years minimum. This plus their graduate education covers a period of 8-10 years.

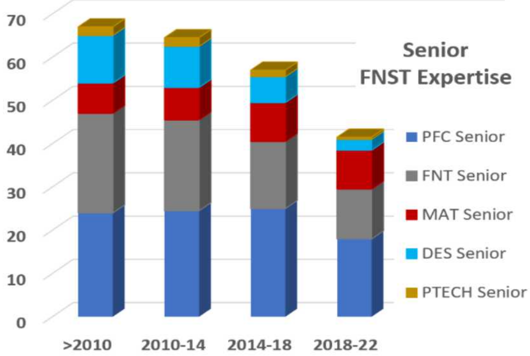
- Plasma Facing Components and Plasma Materials Interactions (PFC), PFC development and testing plus experimenters and analysts in the evolution of plasma affected surfaces plus modelers concerned primarily with defining the source terms for plasma impurities

- Plasma Technologists (PTech), developers of launchers, fueling systems, gas puffing, etc. but excluding magnets
- Materials (MAT), primarily structural materials and radiation damage effects
- Fusion Nuclear Technology (FNT), analysts and experimenters in neutronics, thermal-hydraulics, etc. including a few managers directly involved in FNST
- Designers (DES), design study teams and CAD modelers who develop machine designs and integrate the subsystems.

The table in Figure 1 is a tally of FNSF staff at US labs, universities and industry, including, for example, IFE experts working on the LIFE concept. The charts show the total staff working during the periods indicated[6] as well as separate charts for senior researchers and those with less less expertise. The tallies for 2018-22 are based in the authors best guess about retirements and new hires.

| >2010 | 2010-14 | 2014-18 | 2018-22 | |
|---|---|---|---|---|
| 0.0 | 0.0 | 0.0 | 1.0 | PTECH Other |
| 2.0 | 2.0 | 1.5 | 0.5 | PTECH Senior |
| 0.0 | 0.0 | 1.0 | 5.5 | DES Other |
| 11.0 | 9.5 | 6.0 | 2.5 | DES Other |
| 0.0 | 0.0 | 0.5 | 0.0 | MAT Other |
| 7.0 | 7.5 | 9.0 | 9.0 | MAT Senior |
| 0.0 | 0.0 | 3.5 | 6.0 | FNT Other |
| 23.0 | 21.0 | 15.5 | 11.5 | FNT Senior |
| 6.0 | 6.0 | 4.0 | 9.5 | PFC Other |
| 24.0 | 24.5 | 25.0 | 18.0 | PFC Senior |
| 73.0 | 70.5 | 66.0 | 63.5 | total |
| 6.0 | 6.0 | 9.0 | 22.0 | total junior |
| 67.0 | 64.5 | 57.0 | 41.5 | total senior |



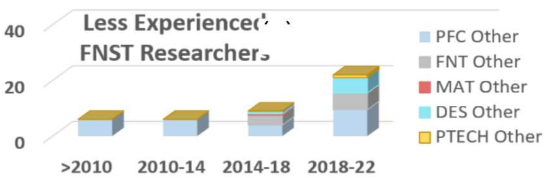




Figure 1. US FNST 2010-2022 demographic history. Table lists data. Charts show (a) total number of experts, (b) senior researchers and (c) those with less less expertise.

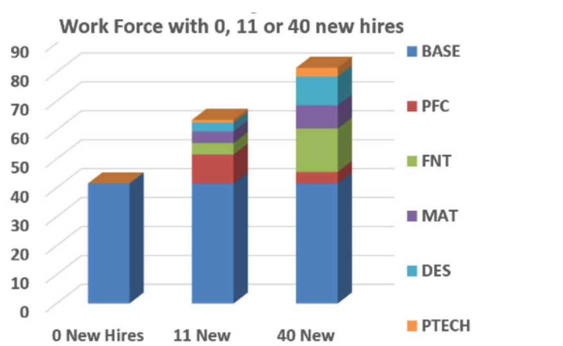



Figure 2. FNST 2020 work force with 0, 11 or 40 new hires.

The main points are as follows.

1. The FNST work force is mostly senior researchers. Many are near retirement.
2. PFC and FNT experts make up ~⅔ of this expertise.
3. The number of senior researchers declined from 67 to 57 in recent years, ~16% of the median of 62.5. The projected decline in the next 4 years is a drop from 57 to 40, a further drop of ~25% leaving ⅔ of the senior researchers.

Figure 2 portrays the 2020 US FNST work force under three scenarios. The base (41.5, left tower) is the post-2018 work force, based on assumed retirements, etc., and with funding conditions that permit no new hires. The middle scenario (11 new hires) would fill positions the author estimates are likely to be vacated at various institutions. The third scenario (40 new hires) brings the work force back to the 2010-14 level.

However, in the third case, half the people are new. To ramp up the program to a much stronger FNST effort would require even more experts. Where would these people come from, and how rapidly could they master the needed expertise?

## 4. *Concluding Personal Remarks*

Realization of fusion energy is a world goal. While the US program remains in flux, the ongoing strategic planning plus positive signs in the administration of the program and within the fusion community are heartening.

Let us assume a richer US FNST program ahead. This is both a challenge and a time of opportunity, particularly for young researchers.

[6] In each period, staff that retired or leave the program are tallied at 50% FTE. Those that moved within the program are tallied at 50% in their old institution and 50% in their new one.

One challenge noted above is how the US might ramp up a stronger FNST effort. The people needed are specialists. Due to the complex nature of the fusion in-vessel systems and tritium fuel cycle and the multi-disciplinary knowledge base needed, the minimum time to achieve this expertise is probably 4-5 years in grad school plus a post-doc of 2-3 years in FNST plus 3-5 yr work in FNST. The total is 11-13 years – over a decade! For new hires as post-docs, the learning time is still 6-8 years, plus the additional experience needed.

*A coordinated US process with universities, labs and industry would help. The process would identify good candidates, counsel them on the skip sets needed and coordinate their placement.*

I hope new scientists and engineers joining the fusion program will find exciting opportunities. Let me offer you a personal perspective on working in the program. Fusion has two main attractions for me.

The first is fusion as a valuable alternative energy source. Generation of electricity is the accepted goal for fusion power. However, I doubt we know this future yet and other uses are also possible.

Changes are rapid and the future speeding toward us could bury us in garbage, particularly in the high-consuming US. Fusion's best end use may be to burn garbage as well as generate power. My friend Prof. Satoshi Konishi is studying fusion in a carbon credit economy. [24]

Second, I like my work space in the broad sense. I enjoy the people in fusion, their diversity of skills, and the cross-cutting technology needed to make fusion work.

There is a derogatory saying about some engineers with 40 years of experience: *Yes, but it's one year of experience repeated 40 times.* That's not me.

I'm a generalist with limited depth in many areas who has enjoyed adding many differing experiences in fusion technology. My early work was on radiation damage to materials. Fascinated by the breadth of fusion's technical challenges, I moved progressively into fusion materials and design, plasma-materials interactions, development of plasma facing components.

As a non-physicist in this physics-dominated program, I am a second-class citizen, but have persisted. My lack of understanding of plasma physics has sometimes been (appropriately) humiliating. I keep asking questions, and the breadth of my experience is valued.

My interest in and concern about the integration of fusion subsystems grew from my experiences in several areas. Design studies identify features of fusion subsystems and development issues. Another part of my insights comes from hands-on experience in testing PFCs and in working to get experiments approved (interface requirements) and hardware installed with the teams who operate TEXTOR at FZK-Juelich, Tore Supra at CEA-Cadarache (also the ITER site), TFTR and NSTX at the Princeton Plasma Physics Lab, and DIII-D at General Atomics.

I offer this word of advice to young leaders. Find the people with the existing knowledge base. (Some may be retired.) Find mentors. Reach outside your field and try to understand fusion's issues. Speaking as one of the older generation of leaders, we hope that we can transfer our legacy of knowledge and insight as the US program rebounds and finds its way as a valued partner in the world.

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