A reinvigorated US fusion energy program

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Reflecting, in part, discussions with T. Carter, R. Fonck, M. Haynes, D. Maurer, D. Meade,G. Navratil, G. Tynan, D. Whyte

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The committee charge is large and expansive

- To advance the US to world leadership in fusion research
- To develop fusion energy as a priority for the US
- Projecting both near- and long-term

This is a crucial and timely opportunity for the US fusion research (and for the field of plasma physics)

Fusion research at mid-life identity crisis

Several paradoxes

- Technical
- Timeline
- US standing

Technical paradox

Remarkable progress

- Tokamak performance, fusion plasma physics, fusion engineering
- Constructing ITER - a reactor-scale, burning plasma experiment

But,

- More scientific and engineering challenges lie ahead,
- Challenges of steady-state, disruptions, high performance are persistent

ITER embodies the strengths and challenges of tokamak confinement (Q = 10, but for 8 minutes),

EU DEMO embodies current knowledge base -> large, pulsed

Timescale paradox

- Now there is a societal desire to achieve fusion urgently

But,

- The time to fusion power is paced by facility construction times (fusion cannot be proven/developed at small scale, then scaled up)

US leadership paradox

The US workforce is at the world forefront (although narrowing)

But,

The US research program is not

(more later)

A redefined US program can alleviate all 3 paradoxes by

- Resolving technical hurdles, emphasizing innovation for improvements
- Accelerating the time to fusion landmarks and fusion power
- Bringing the US to the world forefront

The interim charge – assess current status of US research– is substantive and important

Substantive: Deserves a frank and accurate assessment of the health and stature of the US program

Important: Defines the landscape from which a new strategic plan will be launched

Not been previously articulated "officially"

US has been a world leader in nearly all areas of fusion research

- Built upon major investments in 1980 – 2000
  (TFTR, DIII-D, NSTX, CMOD, coupled to strong base program)
- One metric is the US share of international research prizes
  Nuclear Fusion prize – 8 out of 11 to work led by US scientists
  Alfven Prize, EPS – US scientists won 8 out of 17 years, mainly for fusion research

This metric is a lagging indicator of investments built up over decades

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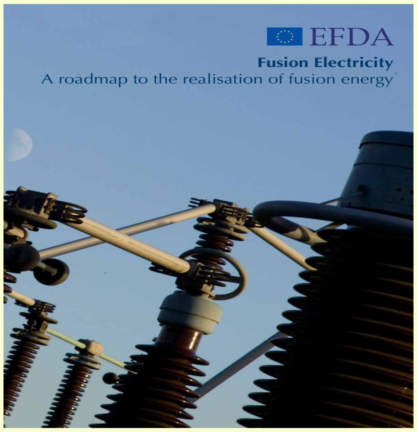
US is losing research leadership and vitality for 2 coupled reasons

- No current strategic plan for fusion energy
  absence of DOE-sanctioned community process,
  no consideration of slip in ITER timeline (20 yrs to DT operations)
- Lack of major investment in new US research facilities and capabilities
  20 year gap in major investments (~$ 1B) in US facilities,
  recent closure of CMOD (2016), closure of TFTR (1997) leaves only DIII-D (1986) and NSTX-U (1999/2019)

This contrasts sharply with our international partners

Our ITER partners have roadmaps to DEMO

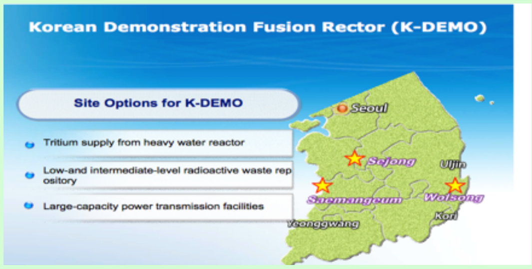
European roadmap



EFDA

Fusion Electricity

A roadmap to the realisation of fusion energy

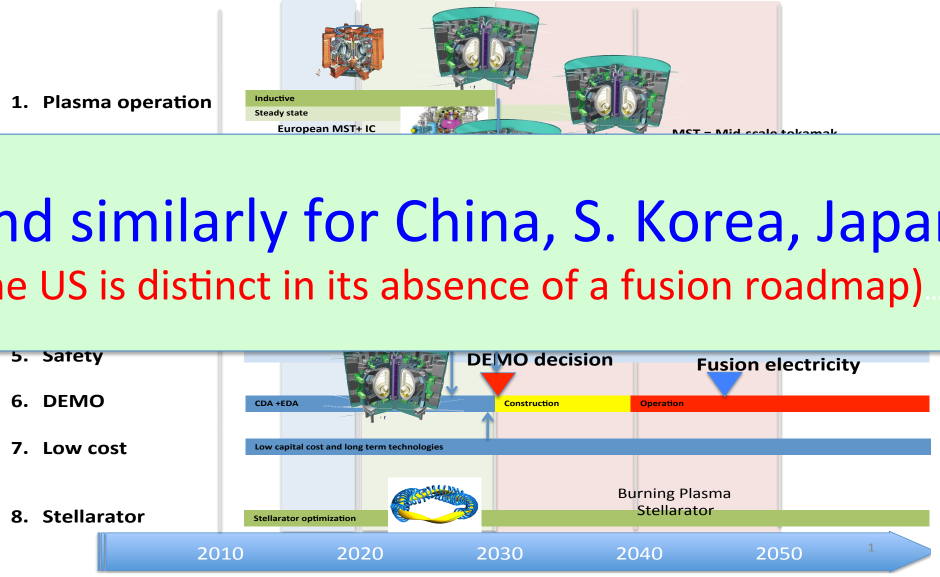


Korean Demonstration Fusion Rector (K-DEMO)

Site Options for K-DEMO

- Tritium supply from heavy water reactor
- Low-and intermediate-level radioactive waste rep ository
- Large-capacity power transmission facilities

EU roadmap in a nutshell



And similarly for China, S. Korea, Japan

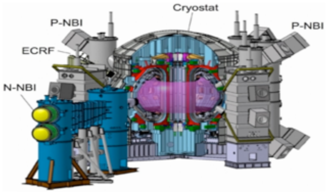
(the US is distinct in its absence of a fusion roadmap)……

Scientific facilities are increasing across the world

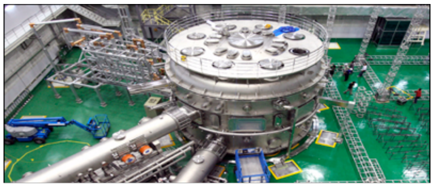
New major facilities
In Asia

China: superconducting tokamak (2006)

Japan: superconducting tokamak (2019)



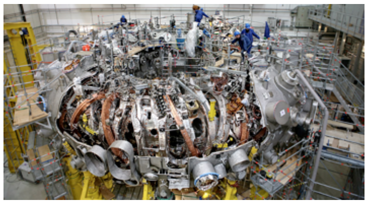
Korea: superconducting tokamak (2008)



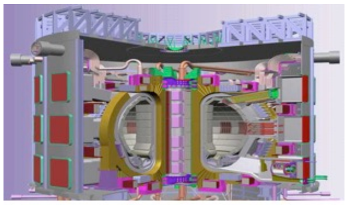
Scientific facilities are increasing across the world

In the European Union

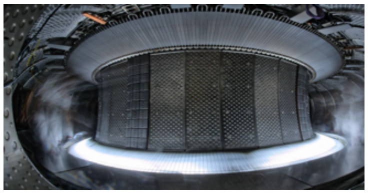
Germany: superconducting stellarator (2016)



France: ITER



France: SC
tokamak (2016)



The world has entered the era of superconducting facilities (steady-state)

A leading indicator

Collaboration on facilities abroad

- An important, valuable opportunity for the US (and host labs)
- But, international collaboration is not a substitute for US leadership.

All fusion research sectors not sufficiently healthy

Universities:
A critical situation. Prospects for faculty positions weak, despite scientific interest. Due to decline in research projects and gov't long-term commitment. (see UFA report, D. Mauer talk)

National labs:
Similar causes led to few national labs with MFE as priority

Industry:
Little involvement in DOE program (except GA), but new phenomenon of fusion startup companies

But the stage is set for the US to transition to a dynamic, leading fusion program

- We have a world-leading workforce
- With many innovative and important ideas ready to pursue

A new fusion strategy

Vision:

Employ new scientific advances for an improved fusion power system and an accelerated path to fusion energy,

building upon the existing knowledge base and international activities

A new fusion strategy

Vision:

Employ ***new scientific advances*** *for an* ***improved*** *fusion power system and an accelerated path to fusion energy,*

building upon *the existing knowledge base and international activities*

Note: the following strategy is described programmatically; the underlying fundamental science is implicit, but not articulated

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Three premises for strategy

1. Pursue ideas to improve the fusion vision and accelerate its pace
   New opportunities have emerged within physics and engineering for improvements and solutions to remaining challenges,
   Allow for higher risk tolerance

2. Builds on and leverages the US and world tokamak/ITER program
   Accommodates tension between staying with proven performance vs exploring new ideas

3. Incorporates a US DT program (and facility) as an integrating goal
   With a mission that complements ITER, and includes advances since the ITER design and those that accrue through #1

Features of above strategy

- A distinct US approach that complements, and benefits from, the world program;
  departs from the dominant ITER -> DEMO -> Reactor framework
  (both too risky and leads to too large a reactor)
- Requires a substantially restructured, reinvigorated, US program
- Would restore the US to a world leadership position in fusion

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Examples of major new research elements suitable to a new strategy

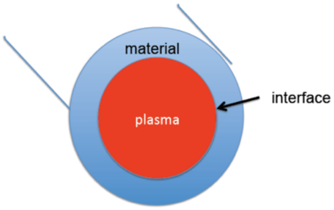
Program mission elements

1. Create fusion plasma core

   high gain, steady-state, disruption-free, compact

2. Tame the plasma-wall Interface

   survivable materials, plasma control



3. Harness fusion power

   tritium breeding, structural materials, energy conversion, environmental attractiveness

4. Establish an integrated fusion power concept that is commercially attractive

   economic, reliable, high availability

In each area,

- There are “big ideas,” many pioneered in the US, not being explored significantly, that can have large impact
- Most ideas have “moderate cost” next steps and “substantial cost” integrating/demonstration steps;

  the strategy is relatively robust to funding level
- Many ideas, no one “silver bullet” for fusion

Create the fusion plasma core

1. Realize a smaller, steady-state tokamak vision
   - High temperature/high field SC magnets
   - Low aspect ratio tokamak
   - Improved operating regimes
2. Develop new 3D magnetic configurations for intrinsic steady-state
   - Optimize stellarator for high performance
   - Aim toward DT facility
3. Establish a program for high risk, high payoff exploratory concepts
   - With an evaluation structure with metrics

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*A new activity,
potentially transformative*

Pushing fusion to smaller size

fusion power $\sim$ $(\text{pressure})^2$

$\sim$ $\beta^2 B^4$ where $\beta = p/B^2$

$\sim$ $(\kappa\beta_N B)^4$ for fixed bootstrap fraction

$(\beta_N = \beta/(I/aB)$,κ = elongation)

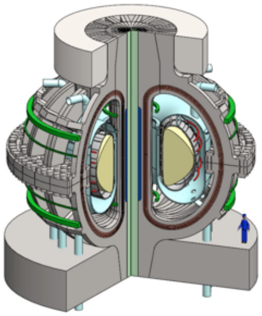
Increase β,κ by pushing physics (spherical tokamak)

Increase B by pushing technology (e.g, high Tc superconductors)

“Compact” fusion pilot plant/FNSF

Generates net electricity,
breeds tritium, high neutron fluence

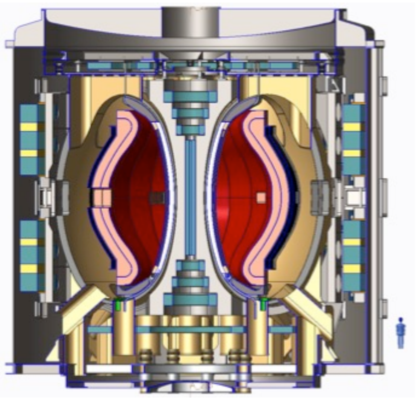
ARC design (MIT)



Major radius = 3 m

High T_c SC for high field and/or smaller magnets

ST design (PPPL)



Volume within cryostat about 1/3 that of ITER

Advancing the High Magnetic Field Approach

- High T_c magnet R/D for fusion (3 – 5 years)
  - Prototype coil tests (incl. for 3D systems)
  - Develop demountable joints
  - (Moderate cost)

- Enables a compact confinement experiment
  - *e.g.,* divertor test tokamak, DT experiment
  - (Substantial cost)

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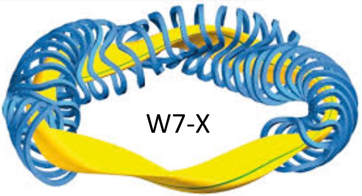
Super-optimizing plasma confinement (stellarators)



3D magnetic field: no symmetry
huge magnetic design space

Merges most advanced physics and computation

Can vary: symmetries (helical, toroidal), no symmetry, magnetic shear, curvature, aspect ratio

Optimizes: thermal confinement, energetic particle confinement, MHD stability, turbulent transport.......

Result: Steady-state, disruption-free	Challenges:
Well-confined	Experimental validation
High fusion energy gain	Find optimum

Advancing the stellarator

- Begin a national optimization/design study now
  Focused on design that complements international effort
  (e.g., quasisymmetry, incorporate new ideas such as turbulent transport optimization, magnet simplification, energetic particle confinement, strong flow.....)
- Construct a mid-scale facility to test these ideas
  ~ moderate cost
- Advance toward a world-leadership-scale experiment
  design could be similar or complementary to mid-scale facility, (substantial cost)

All new and potentially transformative

2. Tame the plasma-wall interface

- Develop liquid metal boundaries
- Explore and deploy new advanced magnetic divertor topologies
- Linear test stands – materials exposed to plasma flux
- Explore additive manufacturing for advanced solid walls

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2. Tame the plasma-wall interface

- Develop liquid metal boundaries

    New and potentially transformative
- Explore and deploy new advanced magnetic divertor topologies

    Existing program, but many additional opportunities
- Linear test stands – materials exposed to plasma flux

    New program, moving beyond existing facilities
- Explore additive manufacturing for advanced solid walls

    Largely unexplored for fusion

Liquid metals for the plasma-facing material

Self-regenerating,

No erosion, neutron damage, heat overload (if moving)

Highly absorbing (if lithium): no influx to plasma, improves confinement,

Many science and engineering challenges to establish feasibility

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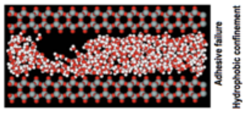
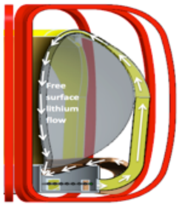
Developing Liquid Metal Boundaries

- Broad R&D program: next 5 – 7 years
  - Surface science studies
  - Linear test stands
  - Liquid flow tests
  - Studies in existing toroidal facilities
  - (Moderate cost)





- Integrated fusion facility with LM boundary
  - Integrated viability of liquid surfaces,
  - Engineering design/construction begin in ~ 5 years,
  - Possibly two stages – divertor, full wall
  - (Substantial cost)

3. Harness fusion power

- Materials and blanket research
  - compact neutron source
  - innovative blankets (immersion, DCLL)
  - (moderate cost)

- **US DT facility**: Initiate now a community planning process for possible missions, complementing ITER:
  - Fusion nuclear science facility (FNSF)
  - High gain burning plasma science (Q > 10)
  - Pilot plant (net electricity)
  - (very substantial cost)

US DT Facility

Next 3-5 years:

- Identify mission
  (FNSF, high gain burning plasma science, net electricity)
- Develop optimal path(s) to a DT experimental program
  (*e.g.*, AT, ST, high field, optimized stellarator……)

In 3-5 years:

- Review status and decide mission/timing of DT facility

Increasing opportunities in computation and theory

- Computation (at all scales, up to exascale), in concert with theory, underlies all fusion topics
- Tremendous opportunities are strongly in line with DOE/ national emphasis on large-scale computation

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With or Without the US in ITER

- Above strategic vision and goals are unchanged ⇒ the strategy is relatively robust
- If US not in ITER: affects timing and mission of US DT program, decreases emphasis on ITER prep research

Although if the US pulls out, an earthquake could result with unpredictable consequences

This strategy would be a 4th shift for fusion research

70s/80s:	Surge in tokamak research (facilities) Tokamak performance/understanding exploded
mid-90s:	Restructuring: shift to science foundations ~~CIT/BPX~~ ~~TFTR~~ ~~TPX~~ & US out of ITER, Succeeded in reinvigorating fusion science
mid-2000s:	Presidential ITER initiative and coordinated world effort ITER moves forward as focus of world program but, US domestic capabilities wane relative to world
2018:	Establish an aggressive program, exploiting new innovative ideas, to carry the US to fusion energy: Re-assert US leadership

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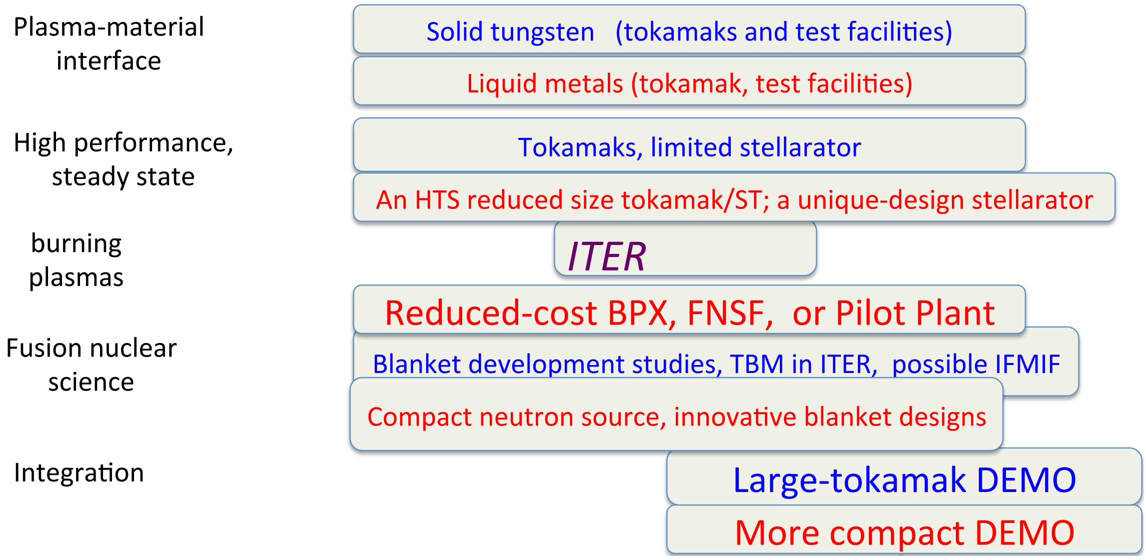
Typical fusion plan in other nations (simplified)

Plasma-material interface	Solid tungsten (tokamaks and test facilities)
High performance, steady state	Tokamaks, limited stellarator
burning plasmas	*ITER*
Fusion nuclear science	Blanket development studies, TBM in ITER, possible IFMIF
Integration	Large-tokamak DEMO

A sample new US program in the international context



Plasma-material interface

Solid tungsten (tokamaks and test facilities)

Liquid metals (tokamak, test facilities)

High performance, steady state

Tokamaks, limited stellarator

An HTS reduced size tokamak/ST; a unique-design stellarator

burning plasmas

ITER

Reduced-cost BPX, FNSF, or Pilot Plant

Fusion nuclear science

Blanket development studies, TBM in ITER, possible IFMIF

Compact neutron source, innovative blanket designs

Integration

Large-tokamak DEMO

More compact DEMO

The above can be described via challenges in fundamental science

- A burning plasma is itself a major physics accomplishment – a new complex, physical system
- The strategy is driven by major questions in
  plasma physics (turbulence, self-organization, breaking of magnetic topology, symmetry effects…..),
  material science (under extreme conditions)
- The connections to other areas of science is large and specific
  (astrophysics, complex systems, accelerator physics…..)

How should fusion R/D be organized within DOE?

- The Office of Science (FES) must remain a key sponsor, given the remaining fundamental challenges in plasma physics and material sciences
- The Office of Science cannot tackle all the critical challenges in fusion engineering and technology
- Thus, a possible approach is to sponsor fusion energy research in both OS and an applied energy office, coordinated from above

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Closing comments

- A frank assessment of the health of the current program is important, and sets the stage from which to grow a new strategy
- A new strategy emphasizing innovations for an improved fusion system concept and accelerated path

  Can be embarked upon with moderate new funds,
  Forms the basis for aggressive roadmap to fusion power,
  Builds constructively upon the current program,
  Re-establishes the US as a world leader,
  Is scientifically exciting