The Road Not Taken* --- Yet

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MIT Plasma Science & Fusion Center

NAS Committee
Strategic Plan for U.S. Burning Plasma Research
Austin, TX
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psfc.mit.edu

** with sincere apologies to R. Frost*

1) *long-term strategic plans for national program of burning plasma science?*

Presently

- Insufficient diversification in approaches
- Stagnated innovation in science/technology → incrementalism
- US program is missing strategic opportunitites for leadership

Could be

- Leveraging present thought-leadership in US
- Program that stands in contrast to “locked-step” international scene
    - Higher risk tolerance
    - Innovative
    - US-defined economic targets

2) the potential impact on this plan if the U.S. is, or is not, a partner in the ITER project?

ITER decision	Pros	Challenges
In	Leveraged investment	Out-leveraged by $$
	Known technology	40-year old tech at burn
	Known timeline	> 20 years, slow!
	Partnered	Not leading
Out	Take lead	Lost trust /w partners
	Forced to innovate	Unknown timeline + tech
	Free to innovate	Tolerate higher risk

2) the potential impact on this plan if the U.S. is, or is not, a partner in the ITER project?

ITER decision	Pros	Challenges
Not ours to make!	Leveraged investment	Out-leveraged
	Known technology	40-year old tech at burn
	Known timeline	> 20 years, too slow
	Partnered	Not leading
	Take lead	Lost trust /w partners
	Forced to innovate	Unknown timeline + tech
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Plan must be robust to decision → US fusion energy goals

(3) the vision and plan for MIT's Plasma Science and Fusion Center in the pursuit of fusion energy science
(4) any strategic elements that might strengthen or accelerate U.S. research in burning plasma science given that economical fusion energy within the next several decades is a U.S. strategic interest

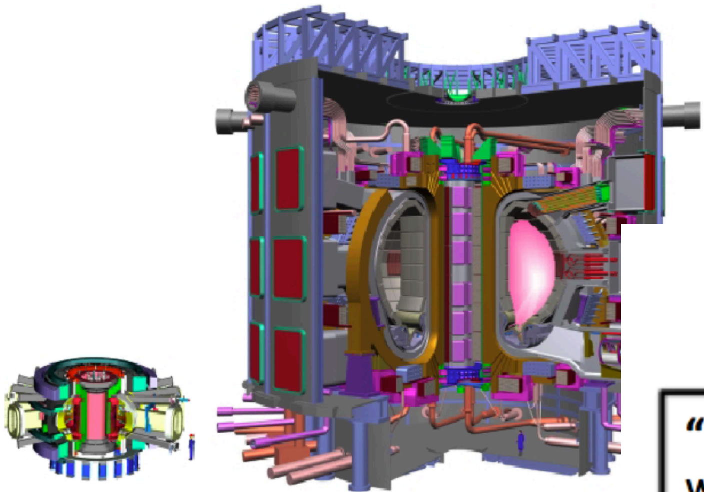
•Reinvigorated US leadership, leveraging a newly available science and technology toolkit →

•Enabling cost-effective diversity and innovation in fusion and plasma sciences →

•Accelerated path towards US-led vision of economically competetive fusion energy that considers

- Climate change timeline; deep decarbonization needed by 2050
- A rapidly evolving energy marketplace
- Making fusion a part of the conversation in this new energy landscape

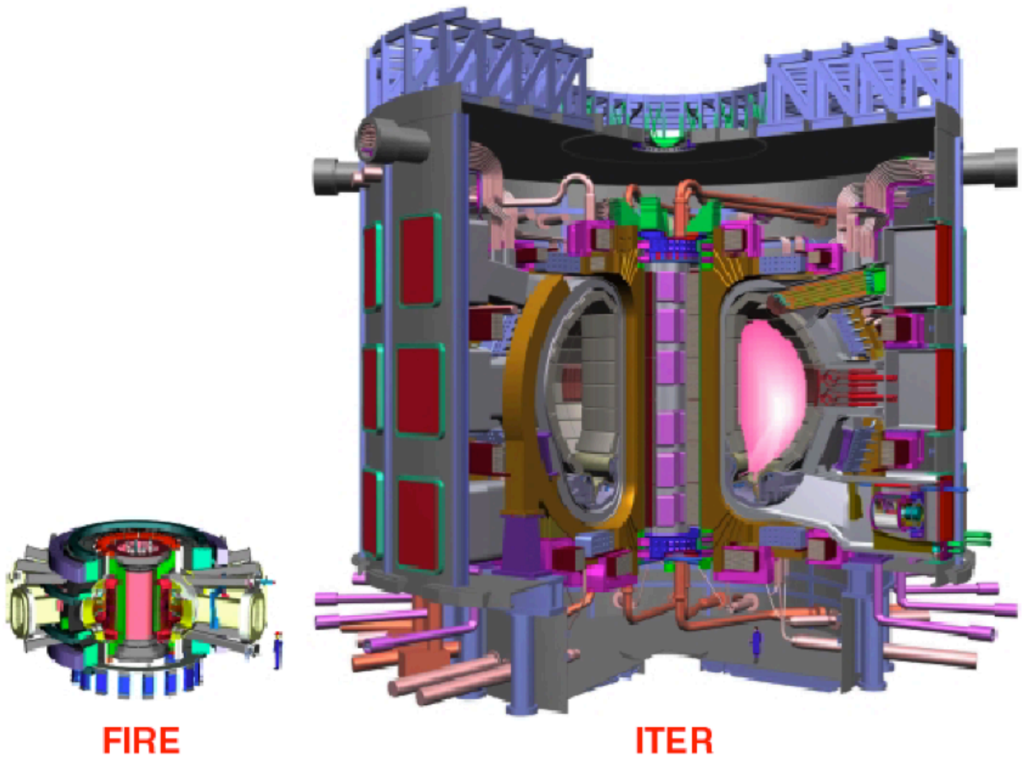
Reminder: NAS sanctioned more than a single pathway to burning plasmas



["Burning Plasma, Bringing a Star to Earth", National Research Council, 2003]

"**In the case of failure to proceed with ITER**, the world community would naturally reassess and look for an alternative approach to a burning plasma experiment that most likely would become an international collaboration. All potential participants would want a role in the choice of parameters and the final design of such an experiment. **The FIRE concept represents one possible contingency that could be revisited in this context**. Depending on the circumstances, partners would need to reassess the optimal path for the development of a burning plasma experiment. **Because a burning plasma experiment is a key step on the unavoidable scientific critical path toward fusion energy, any delays in realizing such an experiment—such as a failure in ITER negotiations**—will necessarily delay the domestic program's ability to address and understand fusion science questions necessary for practical fusion power. "

ITER and FIRE addressed same physics, with same assumptions; vast difference in scale due to single technology design choice



Cryogenic Copper

Nb-Sn Superconductor

$$p_{th}\tau_E \sim R^{2.7} B^{5.5}$$

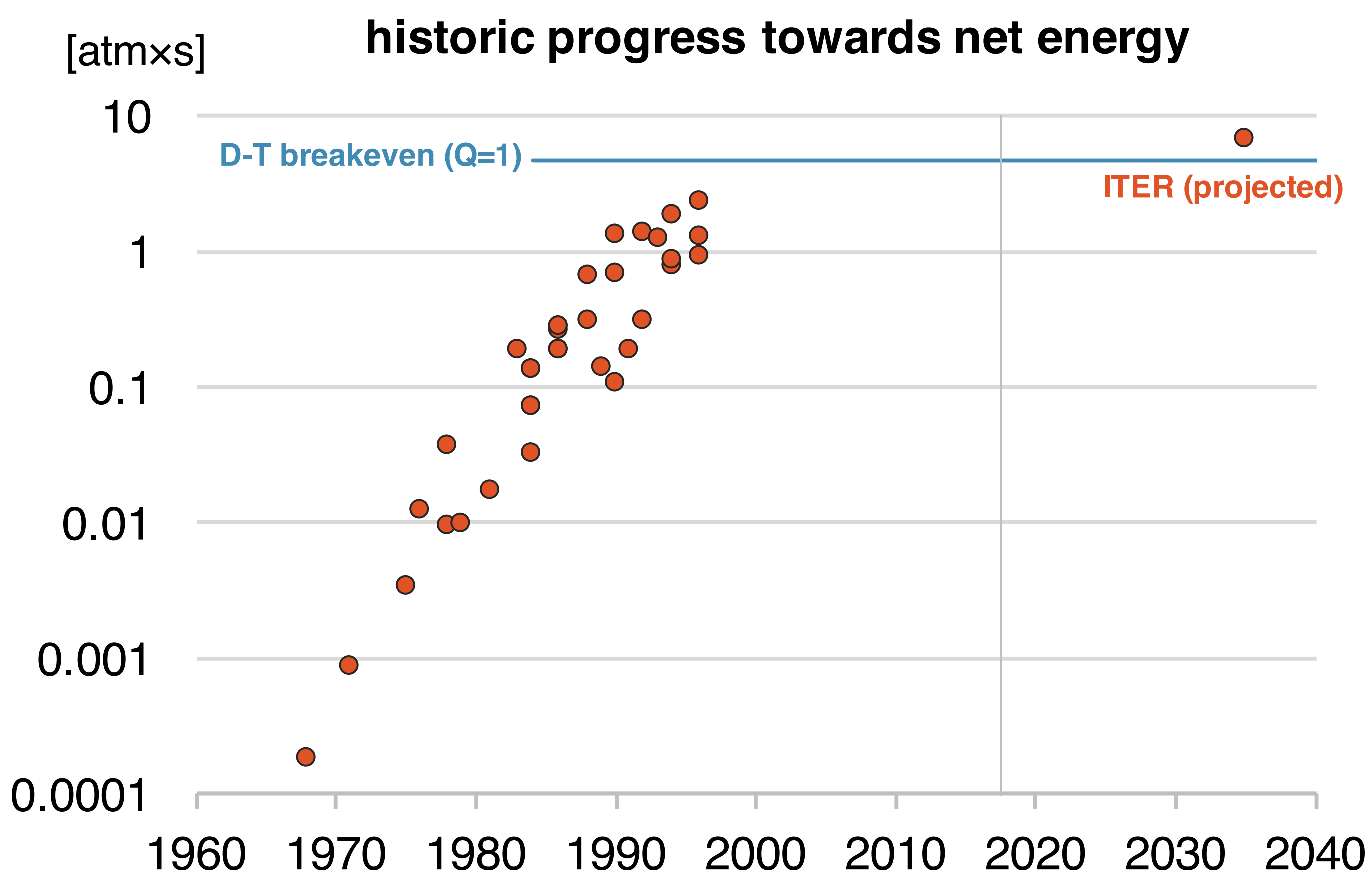
Volume ~ R^3~ $1/B^5$

	FIRE	ITER
B (T)	10	5.3
R (m)	2.14	6.2
Q	10	10
τ / τ_{CR}	> 1	> 1
V_p (m^3)	**30**	**800**

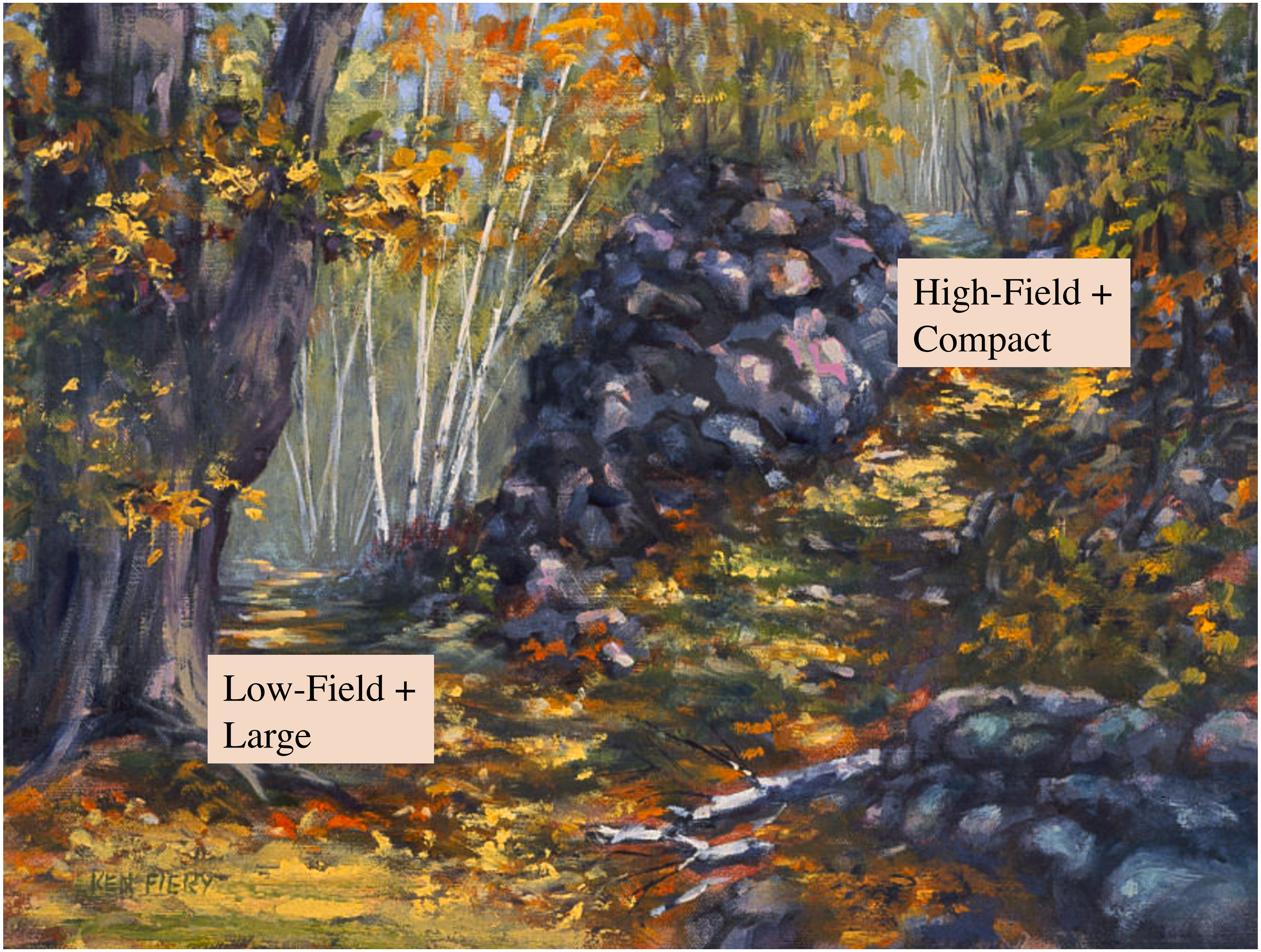
→ 25x

Choosing ONLY the low-field, large volume path now appears to have severe strategic disadvantages

- **40-year gap!**
- **A single experiment in the entire world!**
  - ➢ **Fusion is too important to rely on a single project/concept**
- **Technology & science probably evolve in 20-30 years?**



"Two roads diverged in a yellow wood"



How did we get here?
He who ignores history..

Technical vs programmatic vs political constraints

Perception of where you are going – for your sponsor "perception is reality"

Certain scale ($$, time) necessarily lead to deep politicization of decisions

Was your 2003 version correct? YES
High-B field path is viable and attractive

Issue	Scaling
Power density	B^4
Confinement (generic)	$R^2 B^2$
Confinement (tokamak)	$R^{2.7} B^{3.5}$ (H_{98}) $R^{3.1} B^{2.1}$ (Petty)
Confinement (stellarator)	$R^{2.8} B^{2.1}$
Gain	$R^{2\text{-}3.1}$ $B^{4\text{-}5.5}$
Stable pedestal/I-mode	$\sim \beta_N B^2$

Issue	Scaling
Density (tokamak)	$R^{-1} B^1$
Density (stellarator)	$\beta B^{2.5}$ (burning)
Heat exhaust: min. f_Z	$R^{1.3} B^{0.9}$
Heat exhaust: $q_{//}$	B^{-1} (burning)
Runaway e- amp.	$\exp(R^{0.28} / B^{0.3})$
Synchrotron: runaways	B^2
Synchrotron:thermal	$\sim B^{1.5}$
TAE	$n \sim B$, $v_A \sim B$

In fact they were more correct than they could have known!

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Stable pedestal/I-… 2010	2016

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Oh boy, here we go, HTS will save us all, yada, yada
Send lots of money to MIT..
NO

- Debate is NOT:
  - ITER vs. not ITER
  - tokamak vs. stellarator
  - HTS vs NbSn, etc.
- The debate is about slow & incremental vs. rapid & innovative

The hard truths of innovation

- Difficult/impossible to incorporate into large, decadal+ projects
- Often end up demonstrating (ISS) or abandoning (Human Genome Project) obsolete technologies
- Require stomach for risk

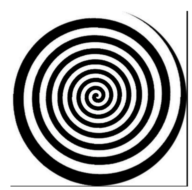
"Two roads diverged in a yellow wood"

The science death spiral of having a single mega-project "road"

Single device must satisfy all constituents

Increased scope
More conservatism

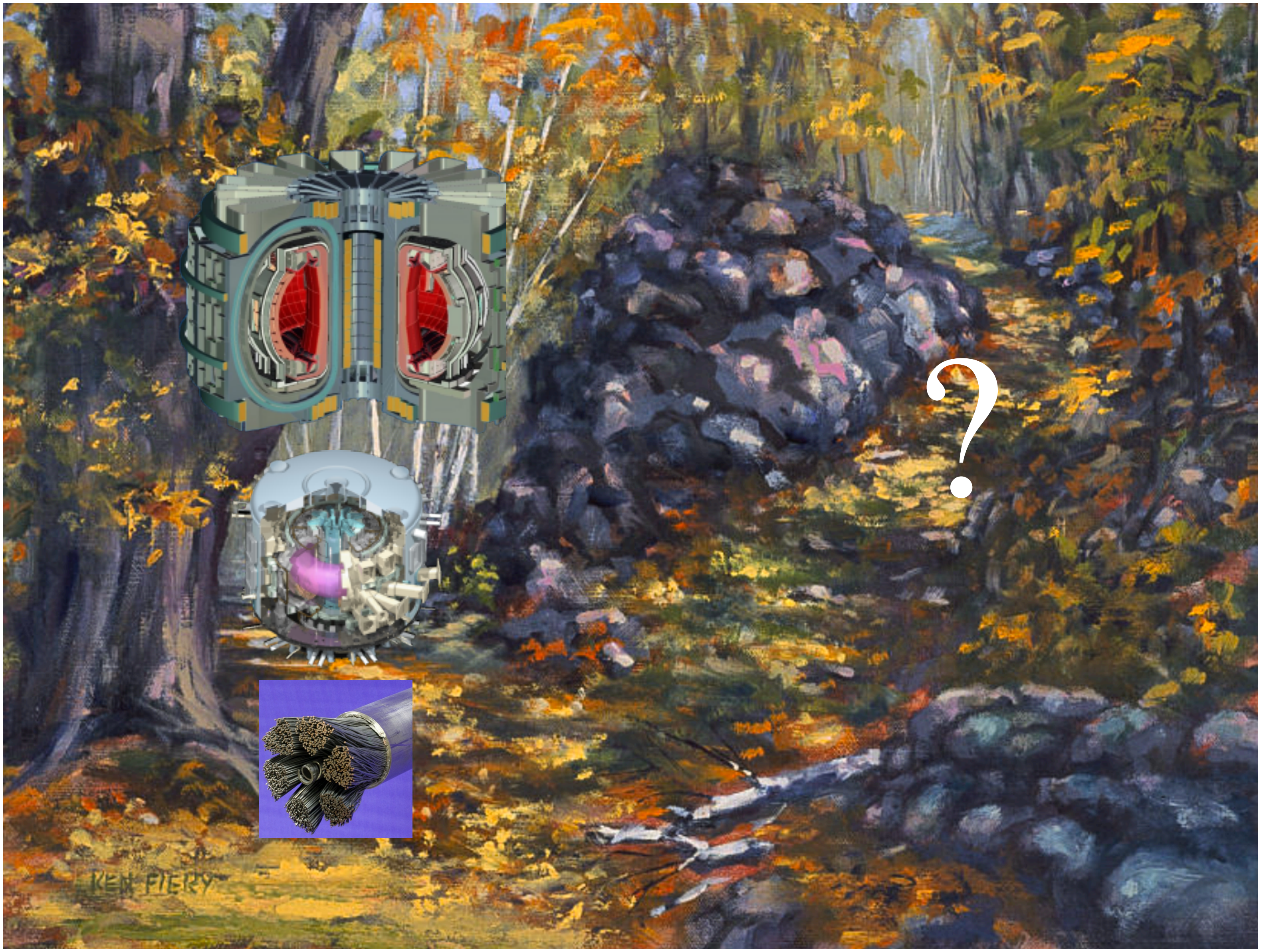
Innovation cannot be adapted



Takes longer
Costs more

"Two roads diverged in a yellow wood And sorry I could not travel both"

"The one less travelled by"



The virtuous circle of the high-field compact "road"

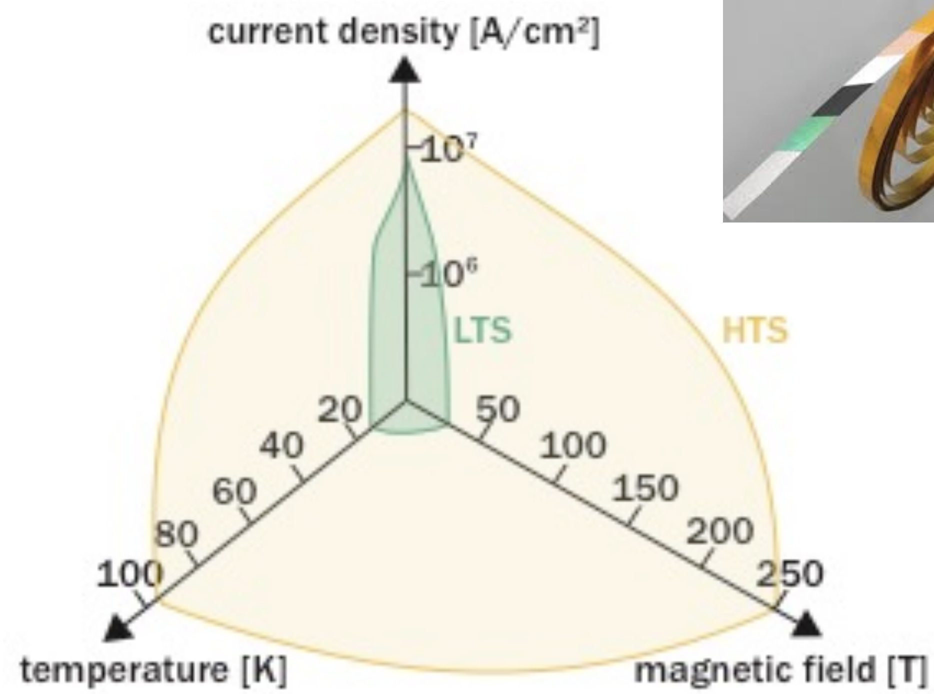
Smaller devices to
access new science
Focussed constituents

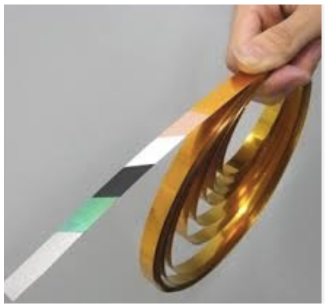
Faster
Cheaper

Science & technology
Innovation leveraged

Diverse missions
Diverse approaches

High-Temperature (HTS) superconductors is an example of leveraging revolutionary technology developed outside fusion





Commerically Available tapes

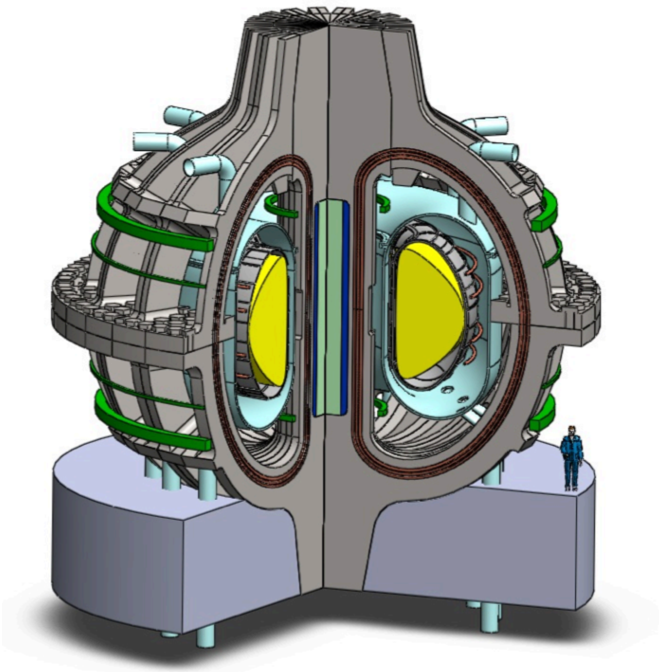
$$\frac{J_c}{J_{c,0}} = \left(\frac{B}{B_0}\right)^{-\alpha}$$

	J_{c0}	B_0	α
Nb-Ti	10^3	5	3
Nb_3-Sn	10^3	10	3
REBCO	$2.5x10^3$	5	0.6

T~4 K, $B>B_0$

High-Temperature (HTS) superconductors is an example of leveraging revolutionary technology developed outside fusion

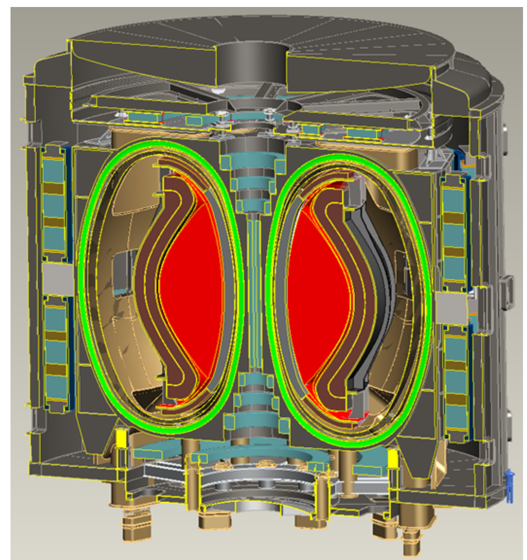
ARC
Q~13, 500 MW
R~3.2 m



B_{max}~23T
B_0 ~ 9.2 T

B. Sorbom et al FED 2015

Pilot
ST

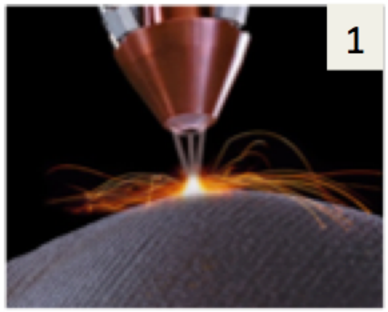


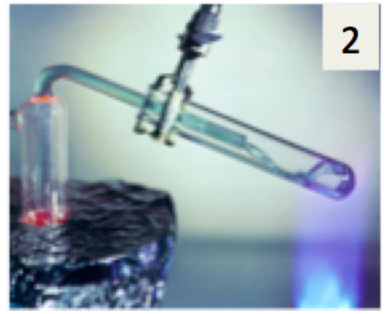
B_{max}~19 T
B_0 ~ 6 T

*Menard et al Nucl. Fusion **56** 2017*

$$p_{th}\tau_E \sim R^{2.7} B^{5.5}$$

Many news technologies, developed outside fusion, should be leveraged to improve our fusion science and energy product. And these are often highly synergistic





Innovation	Description	Bottom line impact
3D printing (1)	Components with internal efficient cooling, precise alignment	Uprated heat removal limits, simplified assembly
Liquid blankets (2)	Liquid Tritium breeding and heat extraction in tanks	Simpler build, higher reliability and availability, leverage fission
Demountable jointed magnets (3)	Ability to take the machine apart to service internals	Higher availability, multi-mission plants
Machine learning	Predictions of plasma performance and stability	Optimized performance, avoid damaging disruptions
Enhanced confinement modes	Better plasmas than H-mode in the same device, e.g. I-mode	Higher plasma performance overall, smaller devices
Supercomputer simulations	Predictive understanding of plasmas for optimization of core (c.f. Bonoli et al)	Plasma optimization for higher performance

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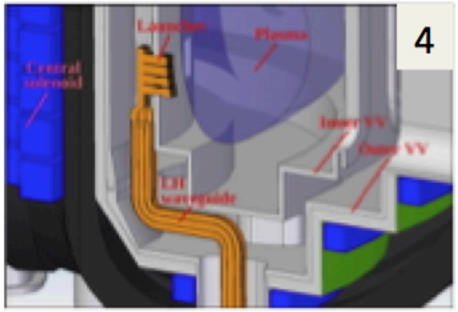
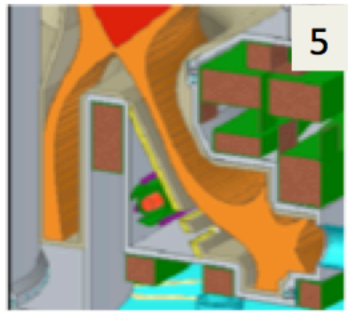
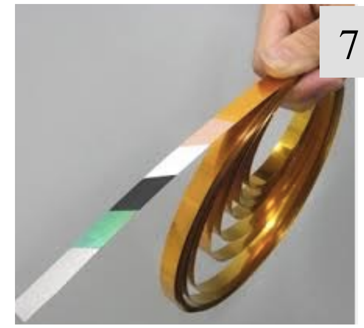






Innovation	Description	Bottom line impact
Inside launch antennas (4)	Advanced antennas for current drive and heating	Lower recirculating power, longer life internals
Long-leg divertors (5)	New magnetic geometries for better plasma heat exhaust	Uprated performance and longer life, higher power density
Higher temp cryogens	Higher operating temperatures and more efficient cooling	Lower recirculating power, smaller devices
Nuclear shielding materials (6)	Optimized to reduce required thickness in key areas	Smaller devices or longer lifetimes
HTS superconductors (7)	Access higher field, operated at higher temperatures, larger SC margin	Smaller devices for plasma and fusion science

Other US-centric innovations we are not pursuing in earnest

- Quasi-axisymmetric stellarators → recovering tokamak confinement

- Modular SC coil design for stellarators
  - jointed coils?
  - PPPL-design of "straight coils" for outer section

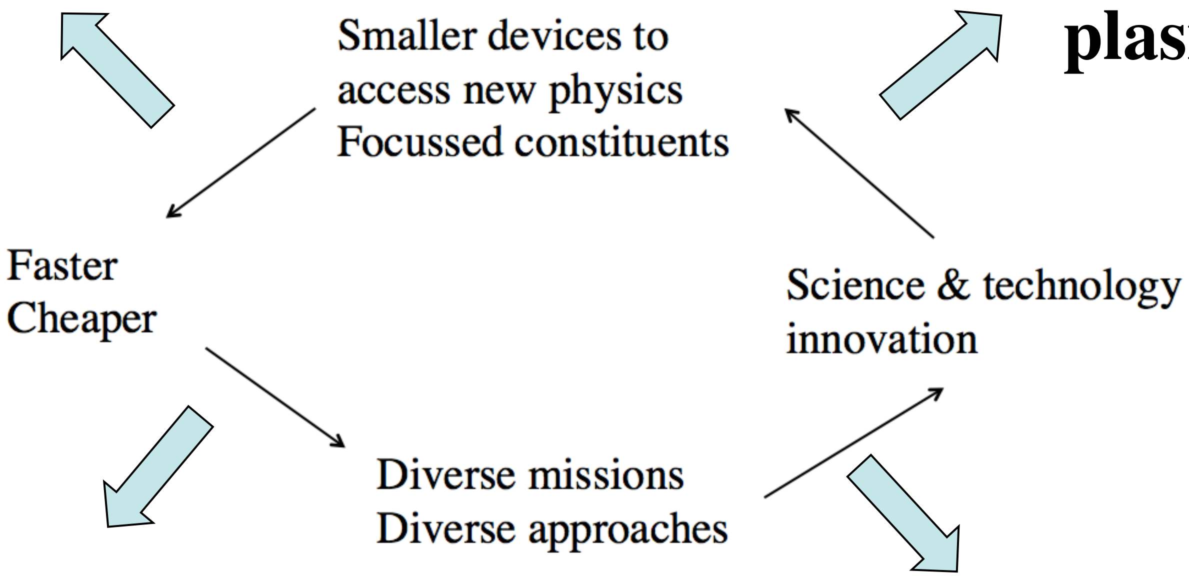
- Liquid plasma-facing surfaces (c.f. Boundary Workshop)

- GDT neutron source

The virtuous circle of the high-field compact "road" leads to increased US strategic opportunities

Configuration optimization
Stellarators, ST, Advanced divertors, 3D printing, liquid blankets, etc.

Multiple paths to burning plasma



US-based economic target for fusion

Impact/connections outside fusion (NMR, HEP, etc.)

My recommendations

- Leadership in developing fusion energy as a commercially competetive energy source is in our national interest
  - And this is unlikely if we stay on our present path, only lock-stepped to a much larger, risk-adverse international effort

- There *must* be an innovation-driven, fast-moving US fusion program regardless of the ITER decision
  - And we have the talent, resources and overall US creativity/ innovation to carry this out.
  - We must act soon with several modest, but “needle-moving” initiatives
  - Re-establish our credibility to deliver exciting science
  - Inject enthuasiasm across full spectrum of our community
  - And with sponsors

"I shall be telling this with a sigh, Somewhere ages and ages hence: Two road diverged in a wood, and I --- I took the one less traveled by, and that has made all the difference"

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Thank you

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