

Lessons Learned from 40 Years of Fusion Science and Technology Research



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**With much appreciation to the many scientists and engineers
I have worked with over decades!**

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40 Years !!

- I have worked on fusion research for > 40 years
- I led and/or participated in many US and international studies on:
 - conceptual designs of reactors, DEMO, and testing facilities
 - understanding and characterizing technical issues, and identifying experiments and facilities for FNST R&D
 - next step facilities (EPR, TNS, INTOR, ITER CDA/EDA/etc.)
 - detailed designs and analysis and R&D experiments and modelling of blankets, PFCs, and materials and many related areas
- I led studies that proposed and developed mission and designs for VNS/CTF/FNSF.
- I introduced and led studies on blanket testing in INTOR then on ITER.
- Since I was a student, I was fortunate to have served on many influential US and international panels and committees that also greatly influenced fusion.

So, I understand why I am asked to talk about 40 years of fusion research.

But **“lessons learned”** is a complex topic that would take a long time to cover.

I will try to give you a **selective combination of lessons learned, reflections, observations, and some suggestions about the future (cover only MFE, not IFE)**

Outline

- Introduction
- “**Then**” (1970’s) Versus “**Now**” (2019): top level
- **Key Challenges/Issues and Required R&D for which progress over the past decades has been frustratingly slow. But must be confronted in any serious plan to develop fusion:**
 1. Confinement Concepts
 2. High Power Density and High Temp. Operation/Economics
 3. Multiple Effects/Multiple Interactions
 4. RAMI (Reliability/Availability/Maintainability/Inspectability)
 5. Tritium Fuel Cycle and Tritium Self-Sufficiency
 6. External T Supply and Required T Startup Inventory
 7. Construction and operation of a facility in which the fusion nuclear components inside the vacuum vessel can be tested and developed in the true fusion nuclear environment (FNSF, VNS, CTF, or whatever you call it)
- Remarks on the roles of university, industry, and private investment
- Concluding Remarks

“Then” Versus “Now”

Big Picture

- **In the 1970’s** we thought: - we had good designs for fusion reactors, -we understood the issues and what R&D to do, and that we can build a **DEMO in 20 years (i.e. in 1990)**.
- **In 2019:** - we are not sure what a competitive reactor will be, - we are debating the pathway and what R&D is needed, and -we think that we can build a **DEMO in 40 years (~2060)** but many doubt if this is achievable.

TEST Facility: ITER type facility

- **In the 1970’s**, we designed EPR, TNS, etc. (~ same scope as ITER) in the US. We said we would construct it in **the 1980’s**. Europe had NET and Japan had FER.
- INTOR started in 1979 as an international study for project construction in the **1990’s**.
- INTOR was replaced by ITER: CDA in 1987, then EDA in 1992 – the goal was to build and operate ITER by **early 2000’s**.
- **In 2019: ITER** is being constructed with the first DT plasma scheduled **for 2036** – 57 years after the initiation of the design of an international burning plasma device.

Key Lessons Learned

- Pace of fusion development has been painfully slow
- Time scale for fusion development is difficult to predict
- Fusion development spans human generations

Disappointments

- The reasons for the painful reality that “the time to fusion is 40 years away, and expanding” are not only scientific/technological challenges and insufficient funding, but also inadequate leadership in funding agencies and the community, inability to modify or change strategies that do not work, community fragmentation due to institutional and technical discipline self-interest, and more!!
- Many of the issues we identified in the 1970’s are still persistent today.
- Very important R&D identified in the 1970’s and 1980’s has not been done yet!!
- Cannot see in my lifetime whether the products of my work will ever succeed.

What to Advise Current and Future Generations

As I approach retirement, I feel a sense of obligation to give more time to special tasks:

- Continue to educate and train new generation of researchers. Also make them aware of the vast literature generated in many studies over 40 years.
- Convey what I learned about the most critical issues, and the most critical R&D yet to be done.
- Teach young fusion scientists how to confront challenges. Be unflinchingly honest about the results of the science.

I hope that this presentation contributes toward this goal.

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Confinement Concepts

- **In the 1970's:** three confinement concepts (theta-pinch, mirrors, and tokamaks) were competing. By late 1970's, only tokamaks and mirrors.
- Some other “innovative confinement concepts” (e.g. FRC, Spheromak) have been pursued since the 1970's with a small budget up and down until now.
- Stellarator was considered from the 1950's and nearly abandoned, but gained momentum the past decade because of some physics success, and construction of W-7.
- **In 2019: Only the “Tokamak”** is considered in plans of all countries **for DEMO.**
 - But many scientists/engineers have concerns about its ultimate suitability for a competitive, maintainable fusion energy system. Stellarator is a back-up but shares many of the “go-no go” issues of tokamaks.

Not a good situation. What do we do now?

- Continue to work with the confinement concept we have and finish ITER
- But aggressively encourage innovative research to discover/invent an attractive fusion confinement concept with much higher potential for commercialization (e.g. simplicity of configuration, better maintainability, more manageable RAMI problems, and higher power density)

The Goal of Fusion R&D has been to Develop Energy Systems for Electric Power Generation

Can current concepts for Fusion Energy Systems be competitive in the marketplace (e.g. compete with fission)?

- Environmental advantages? **YES**
- Safety? **Probably** (if we can control tritium permeation)
- Economics? **Not Sure**
 - * **COE?** (high power density, high temp, cost of materials, RAMI, etc.)
 - * **Initial Capital Cost?**

COE is a function of plasma performance and is substantially affected by FNST issues most of which have not yet been resolved and technologies not yet developed

Need High Power Density/Physics-Technology Partnership

- High-Performance Plasma
- Blanket/FW/divertor Technology Capabilities

Need Low Failure Rate

$$COE = \frac{C \cdot i + \text{replacement cost}}{P_{fusion} \cdot \text{Availability} \cdot M \cdot \eta_{th}} + O \& M$$

$$P_f \sim \beta^2 B^4 \cdot \text{Volume}$$

Energy Multiplication

Need High Temp. Energy Extraction Blanket

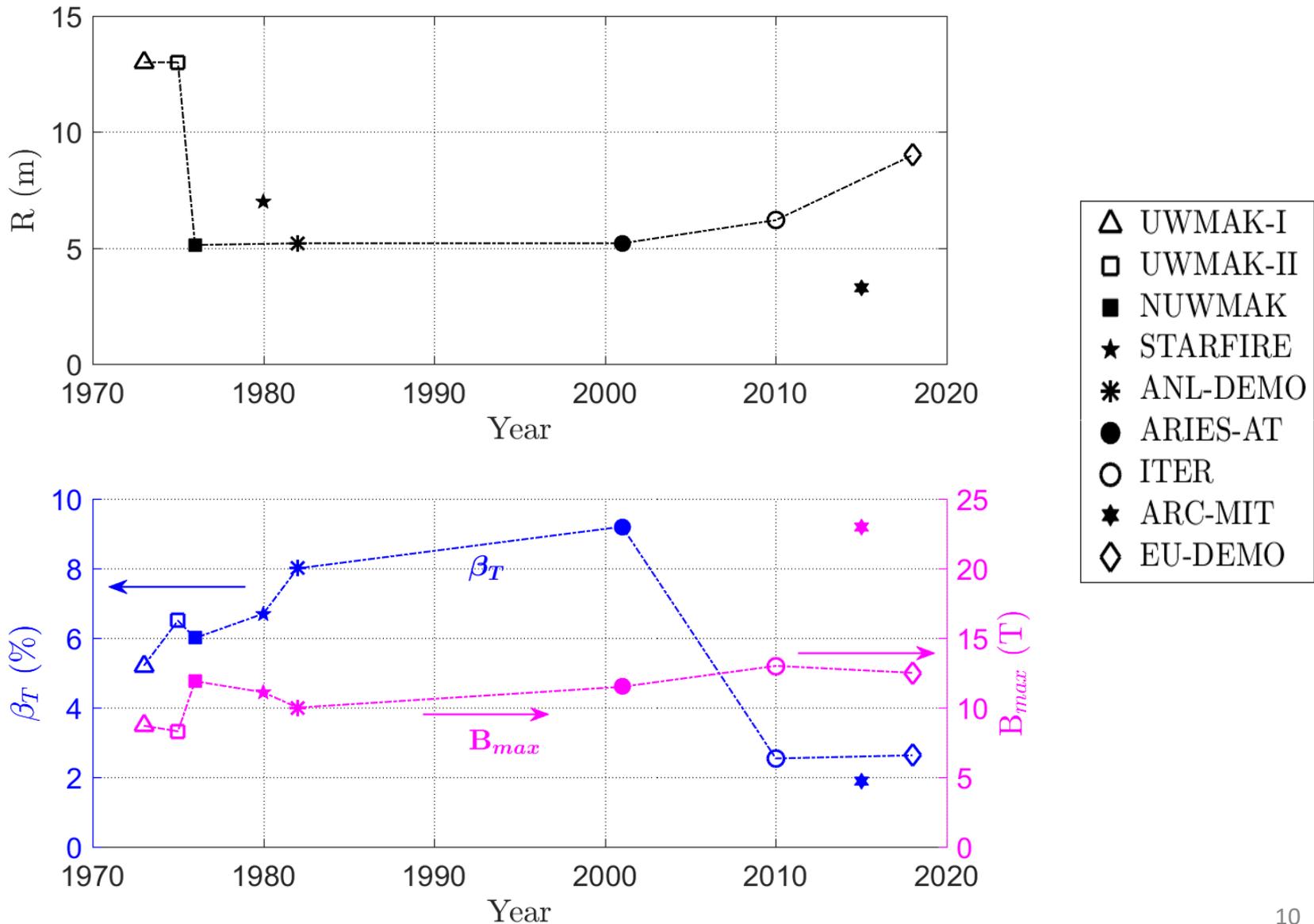
Need High Availability / Simpler Technological and Material Constraints

$$\frac{(1/\text{failure rate})}{1/\text{failure rate} + \text{replacement time}}$$

- **Need Low Failure Rate:**
 - Innovative Chamber Technology
- **Need Short Maintenance Time:**
 - Simple Configuration Confinement
 - Easier to Maintain Chamber Technology

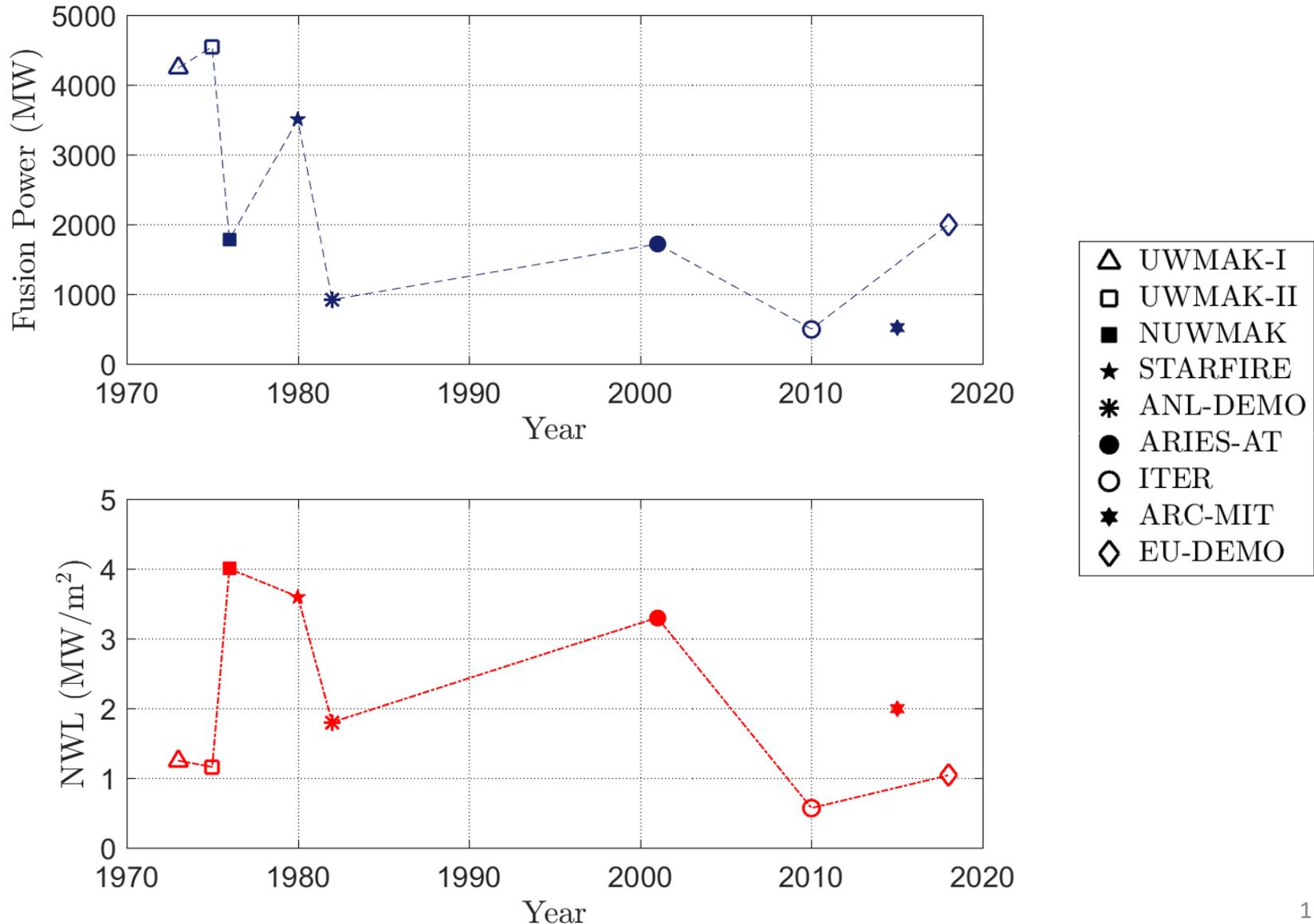
Key Parameters for Reactor and Demo Studies from 1972 to 2019

The rapid trends in the early 1970's of **increasing power density and reducing size** are **being reversed in the 2000's** for alarming reasons



Key Parameters for Reactor and Demo Studies from 1972 to 2019

The rapid trends in the early 1970's of **increasing power density and reducing size** are **being reversed in the 2000's** for alarming reasons (cont'd)



Need for **High Power Density** was realized early.
But after 40 years we do not have a way to achieve it!!

- Need High Power Density to improve potential attractiveness of fusion power compared to other energy sources (e.g., fission)

	PWR	BWR	LMFR	ITER-Type
Average core power density (MW/m ³)	96	56	240	0.4

- The challenges in realizing High Power Density in current fusion concepts are:
 - 1- Difficulty achieving high power density in the plasma (high $\beta^2 B^4$)
 - 2- Limitations on power handling capabilities of Current FW/Blanket/Divertor concepts (high wall load and surface heat flux)
- The APEX Study (1997-2003) made a lot of progress in developing concepts with higher wall load/surface heat flux capability: Liquid Surfaces, Solid Tungsten Wall with 2-phase Li (EVOLVE)
But the highest practical Neutron Wall Load was $< 5 \text{ MW/m}^2$, and Surface Heat flux $< 1 \text{ MW/m}^2$ **Still too low for economic competitiveness?**
- **ITER estimates β of only 2%. EUROfusion DEMO using realistic assumptions has β of $\sim 2\%$, which leads to Neutron Wall Load of only $\sim 1 \text{ MW/m}^2$!!**

**Alarm: We don't have a credible pathway to achieve high power density.
Current pathway is trending toward even lower power density
- unlikely to lead to an economically competitive system.**

Need for **High-Temperature Structural Material** was realized early. *But after 40 years we do not have it!!*

The need for development of structural material with high temperature operational capability was recognized from the very early 1970's. A range of structural materials were evaluated: Steels, PE-16, ferritic steels, V, Nb, TZM, SiC.

Refractory alloys were initially considered attractive because of high temperature operation (~750 C) and resistance to radiation damage. But detailed investigations ruled them out because:

- Refractory materials are expensive: primarily the cost of the heat transport system/piping. High thermal efficiency cannot offset the cost of piping. (Results of UWMAK-III, 1975; Abdou ICFRM 1979)
- Nb and TZM are high activation
- V is low activation but compatible only with Li (embrittlement by interstitial impurities). But development of MHD insulators for V-Li system failed

So, **only steels remained as the primary option for fusion**. Modified stainless steel (PCA) in the late 1970's, early 1980's. Then **ferritic-martensitic steel** (small alloy variation among countries). Limited to ~550 C

2019: Unpleasant surprise: Recent estimate of the cost of EUROFER for FW/Blanket may be \$3 Billion!!!

So, after 40 years, the only viable structural material that fusion has now is limited to < 550 C and is very EXPENSIVE!!

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Fusion Nuclear Environment has multi-component fields with strong gradients, and is Complex & Unique

Neutrons (*flux, spectrum, gradients, pulses*)

- Bulk (volumetric) Heating
- Tritium Production
- Radiation Effects
- Activation and Decay Heat

Heat Sources (*thermal gradients, pulses*)

- Bulk (neutrons)
- Surface (particles, radiation)

Particle/Debris Fluxes (*energy, density, gradients*)

Magnetic Fields (*3-components, gradients*)

- Steady and Time-Varying Field

Mechanical & Electromagnetic Forces

- Normal (*steady, cyclic*) and Off-Normal (*pulsed*)

Combined Loads, Multiple Environmental Effects

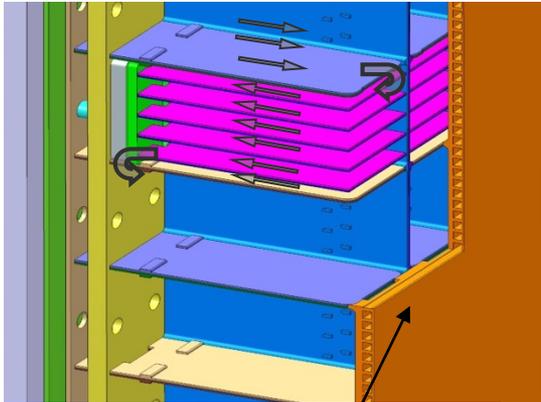
- Thermal-chemical-mechanical-electrical-magnetic-gravitational-nuclear interactions and multiple/synergistic effects
- Interactions among physical elements of components

Multiple functions, materials,
and many interfaces in highly
constrained system

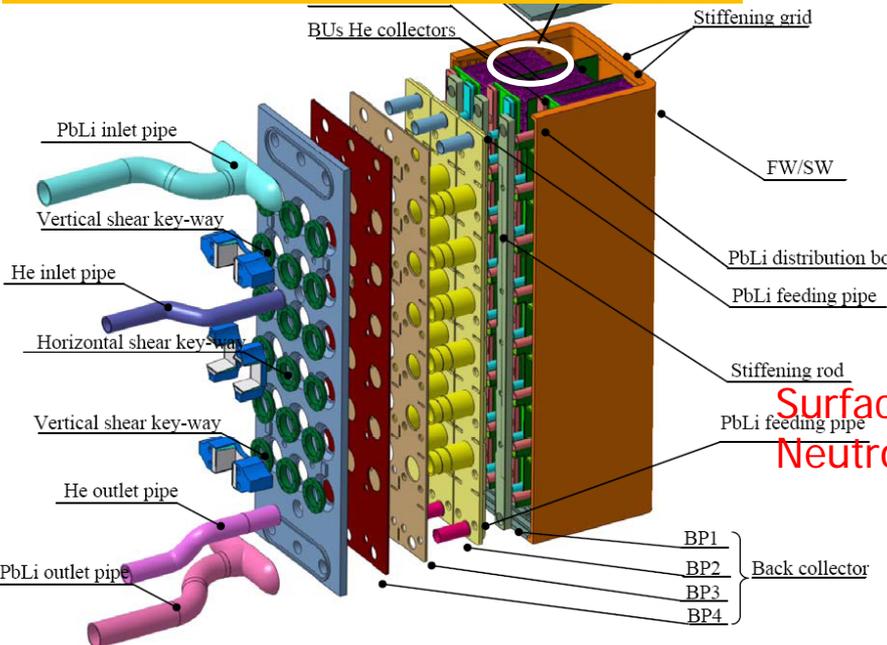
Experiments and modelling on multiple effects/multiple interactions are essential to predict performance and behavior of fusion nuclear components

Blanket/FW systems are complex and have many functional materials, joints, fluids, and interfaces

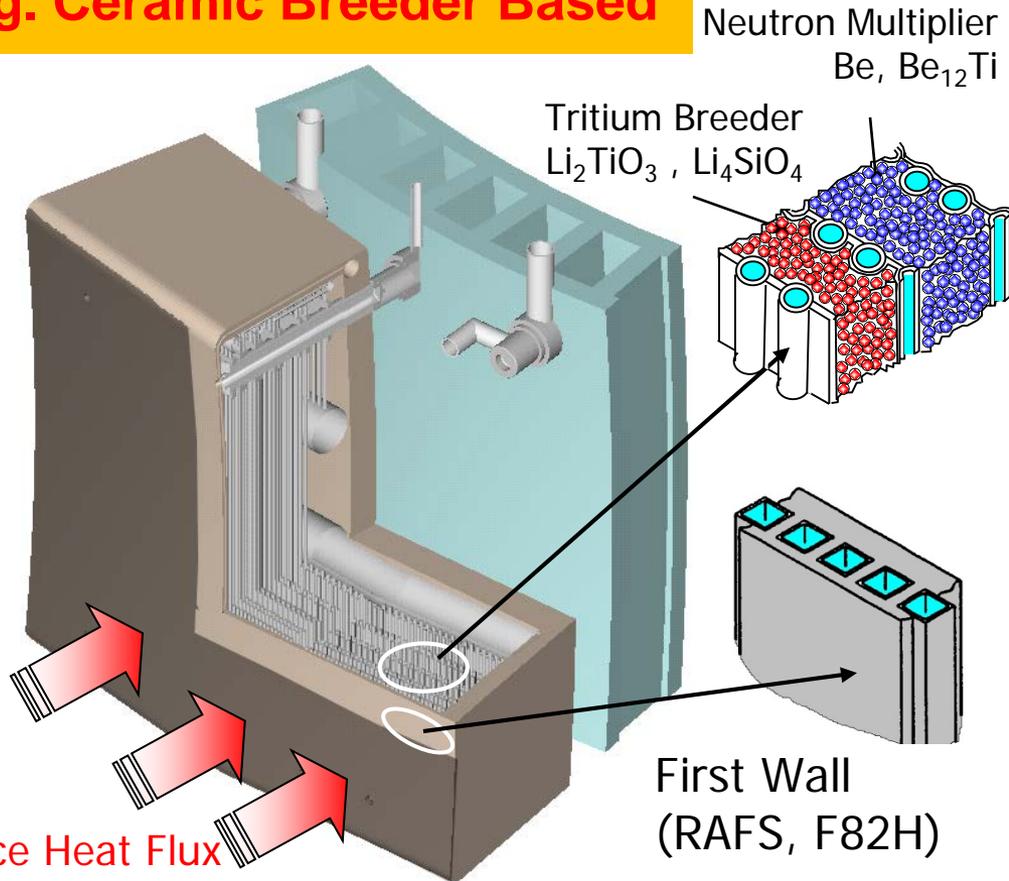
Li, PbLi,
Li-Salt flow



E.g. Liquid Breeder Based



E.g. Ceramic Breeder Based



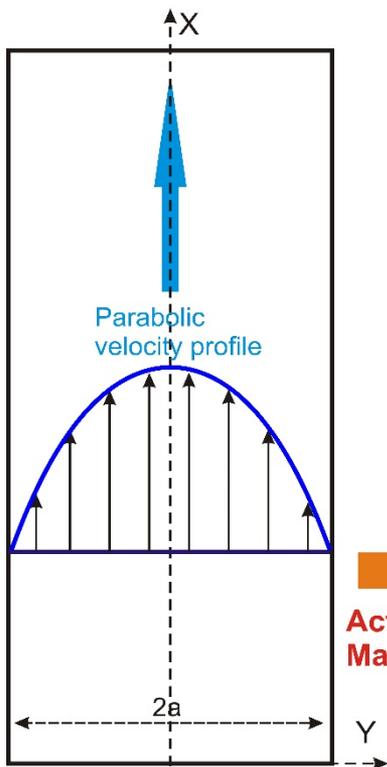
Surface Heat Flux
Neutron Wall Load

Coolants: He, H₂O,
or liquid metal or salt

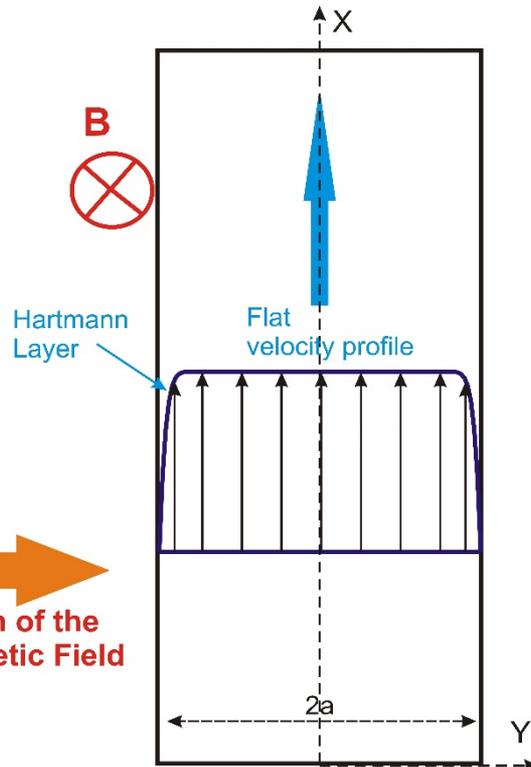
For 30 years fusion researchers studied Liquid Metal MHD Flow Behavior in Blankets as if it were PURELY in the Presence of Magnetic Field (i.e. separate effect). So, the common assumption has been:

Flow is *Laminar*: the flow velocity profile is strongly altered by the action of the Lorentz force leading to flat laminar core with very thin Hartmann and side layers

Laminar Velocity Profile



Purely MHD Velocity Profile



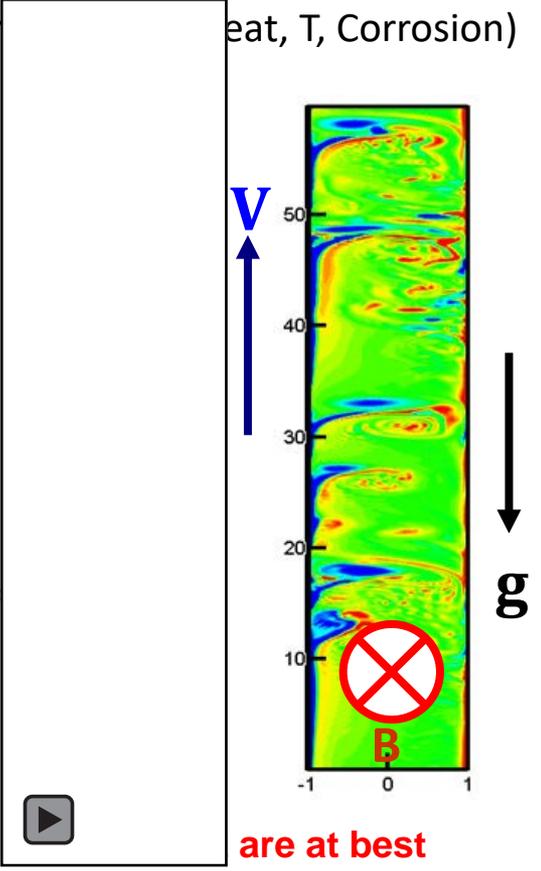
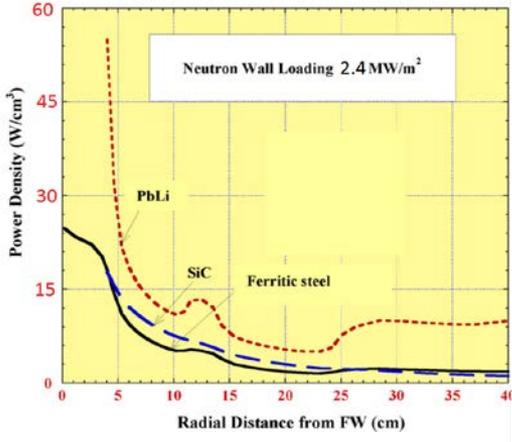
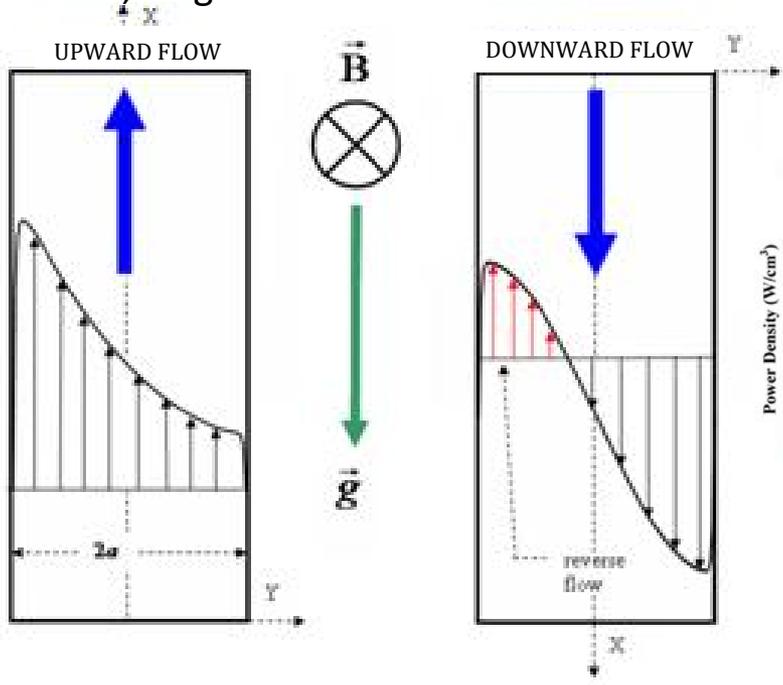
But we just discovered that what we assumed for 30 years is wrong

Discovery: Spatial gradients in nuclear heating & temperature in LM blanket combined with \vec{g} and \vec{B} lead to New Phenomena that fundamentally alter our understanding of the MHD Thermofluid behavior, Tritium Transport/Permeation and Materials Interactions in the blanket in the fusion nuclear environment

Lead to **Buoyant MHD interactions resulting in an unstable “Mixed Convection” flow regime**

Base flow strongly altered leading to velocity gradients, stagnant zones and even “**flow reversal**”

Vorticity Field shows new **instabilities** that affect transport phenomena (heat, T, Corrosion)



This result is from modeling at limited parameters in idealized geometry.

- Predictions from separate effect tests for the integrated fusion nuclear misleading, and quite often simply wrong
- Blankets designed with current knowledge of phenomena and data will not work

are at best

Non-Linear LM MHD Phenomena is difficult to scale from experiment to DEMO (Blanket scaling problem similar to plasma physics!)

DEMO BLANKET: $Ha \sim 10^4$, $Gr \sim 10^{12}$, $Re \sim 10^5$

EXPERIMENT: $Ha \sim 10^3$, $Gr \sim 10^9$, $Re \sim 10^5$

Grand Challenge

Since blankets in DEMO/Power Reactors have very high parameters (e.g. Ha , Gr) that cannot be reached in laboratory, **how do we scale results from experiments to predict Blanket behavior in DEMO?**

- Non-linear phenomena (difficult to scale)
- Higher Ha will suppress turbulence/instabilities
- Higher Gr will enhance buoyancy/instabilities
- **So, what will be the real behavior in the real blanket where both Ha and Gr are high?**

Encouraging recent progress in Multiple Effects/Multiple Interactions R&D

- Very few multiple effects/multiple interactions facilities exist in the world.
- A first-of-a-kind facility, called MaPLE-U, has been completed at UCLA, in partnership with EUROfusion, to study MHD thermofluids multiple-effects, material interactions, and tritium transport & permeation.
- First experiments on mixed convection in MaPLE-U successfully started August 2018. Results show unstable mixed convection with flow reversal -- direct proof of the underlying scientific motivation for this MaPLE-U.

Recent Lesson Learned

- Multiple Effects/Multiple Interaction facilities and experiments are much more complex than those for separate effects
- They require long time, expensive equipment, substantial experiment planning, complex instrumentation all accompanied by intensive 3D modeling effort. This means substantially more resources will be required going forward and funding agencies need to understand this need

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Reliability/Availability/Maintainability/Inspectability (RAMI)

$$\text{Availability} = \frac{\text{MTBF}}{\text{MTBF} + \text{MTTR}}$$

MTBF – Mean time between failures

MTTR – Mean time to repair

- RAMI/Availability is a key factor in COE - economics
- For fusion, RAMI is also a most serious Engineering Feasibility Issue.
- Yet, the world fusion program still has no dedicated RAMI experts, and no serious R&D and no database to realistically estimate what availability can be realized.
 - **Availability has been an assumed number in ALL fusion studies (reactors, DEMO, test facilities, ITER) because we know what we need (75% - 85% for reactors), but no one estimated what can be achieved (except for small individual efforts).**
 - **The IEA International Study on High Volume Plasma-Based Neutron Source (HVPNS) (1994-96) made good effort to predict availability based on extrapolation from fission and aerospace industry and how much testing in the fusion nuclear environment (See Fusion Technology, 29: 1-57 (1996))**
 - **The results of this IEA HVPNS Study were very alarming. They show that RAMI is the Achilles' Heel issue for fusion**

Reliability/Availability/Maintainability/Inspectability (RAMI) is a serious challenge that has major impact on engineering feasibility and economics

Availability required for each component needs to be high								
Component	#	failure rate (1/hr)	MTBF (yrs)	MTTR/type		Fraction Failures Major	Outage Risk	Component Availability
				Major (hrs)	Minor (hrs)			
Toroidal	16	5×10^{-6}	23	10^4	240	0.1	0.098	0.91
Two key parameters:				MTBF – Mean time between failures				
				MTTR – Mean time to repair				
Magnet supplies	4	1×10^{-4}	1.14	72	10	0.1	0.007	0.99
Cryogenics	2	2×10^{-4}	0.57	300	24	0.1	0.022	0.978
Blanket	100	1×10^{-5}	11.4	800	100	0.05	0.135	0.881
Divertor	32	2×10^{-5}	5.7	500	200	0.1	0.147	0.871
Htg/CD	4							0.884
Fueling	1							0.998
Tritium System	1							0.995
Vacuum	3							0.998
Conventional equipment – instrumentation, cooling, turbines, electrical plant							0.05	0.952
TOTAL SYSTEM							0.624	0.615
							(Due to unscheduled maintenances)	

DEMO availability of 50% requires:

- Blanket/Divertor Availability ~ 87%
- Blanket MTBF >11 years
- MTTR < 2 weeks

Extrapolation from other technologies shows that for fusion blankets/divertor, the expected MTBF is as short as ~hours/days, and MTTR ~months.
GRAND Challenge: Huge difference between Required and Expected!!

Fundamental reasons why we have Serious Problems with short MTBF, long MTTR, and very low expected availability in current fusion “confinement” systems

- Location of Blanket/FW/Divertor **inside*** the **vacuum vessel**:
 - **low fault tolerance** → **short MTBF** because many failures (e.g. coolant leak) require immediate shutdown, also no redundancy possible.
 - **long MTTR** because repair & replacement require breaking “vacuum seal” and many connects/disconnects, and many operations in the limited access space of tokamaks, stellerators, and other “toroidal/closed” configurations

** The decision to put the blanket inside the vacuum vessel is necessary to protect the vacuum vessel, which must be robust and cannot be in high radiation/temperature/stress state facing the plasma.*

- Large surface area of the first wall results in high failure rate for a given unit failure rate per unit length of piping, welds, and joints → **short MTBF**

Results show: anticipated MTBF is hours/days (required is years), and MTTR is 3-4 months (required is days), and availability is very low < 5%

Contrast this to fission reactors:

- Can continue operation with ~2% of fuel rods with failures (MTBF ~ years)
- An entire fuel bundle can be replaced in ~ 2 days (MTTR ~ 2 days).
- Fission reactors have been able to achieve 90% availability

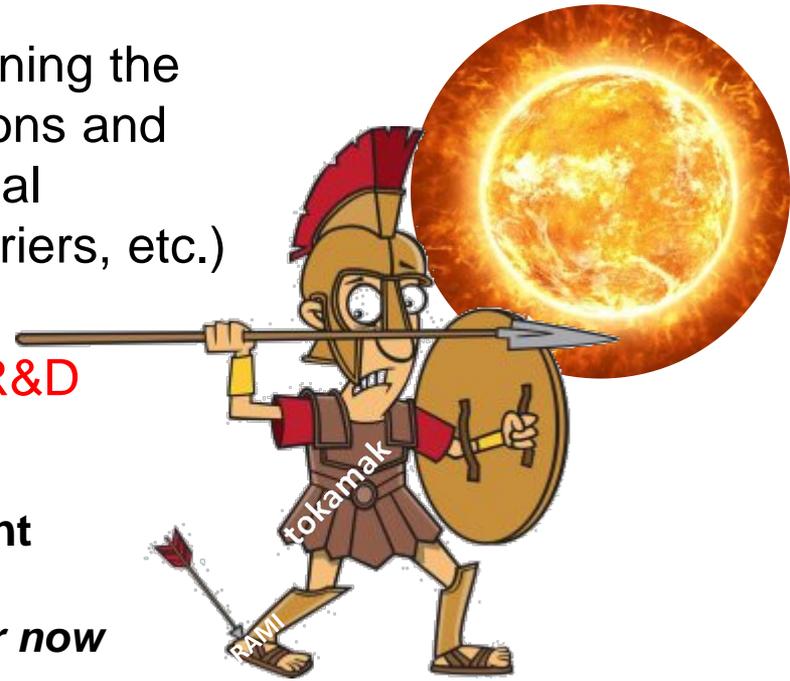
Lessons learned and suggestions for improving the situation with RAMI, the *Achilles' Heel* issue for fusion

- MTBF/MTTR will be the key issue in determining the feasibility of plasma confinement configurations and the feasibility of blanket concepts and material choices (structure, breeder, insulators, T barriers, etc.)
- Performance, Design Margin, Failure Modes/Rates should be the focus of FNST R&D
Not a long dpa life

1. **Setting goals for MTBF/MTTR is more important NOW than dpa goals for lifetime of materials**
RAFS with 10-20 dpa, 100 ppm He is sufficient for now

2. R&D should now focus on:

- Scientific understanding of multiple effects, performance and failures so that functions, requirements & safety margins can be achieved, and designs simplified and improved
- Strive for design simplicity and bring Industry into the design process
- Understand that Reliability Growth takes very long time, Build FNSF early as “experimental” facility that focuses only on the FNST components inside the vacuum vessel. Realistic understanding of MTBF/MTTR can be obtained in such FNSF
- Be prepared for surprises and be ready to change pathway

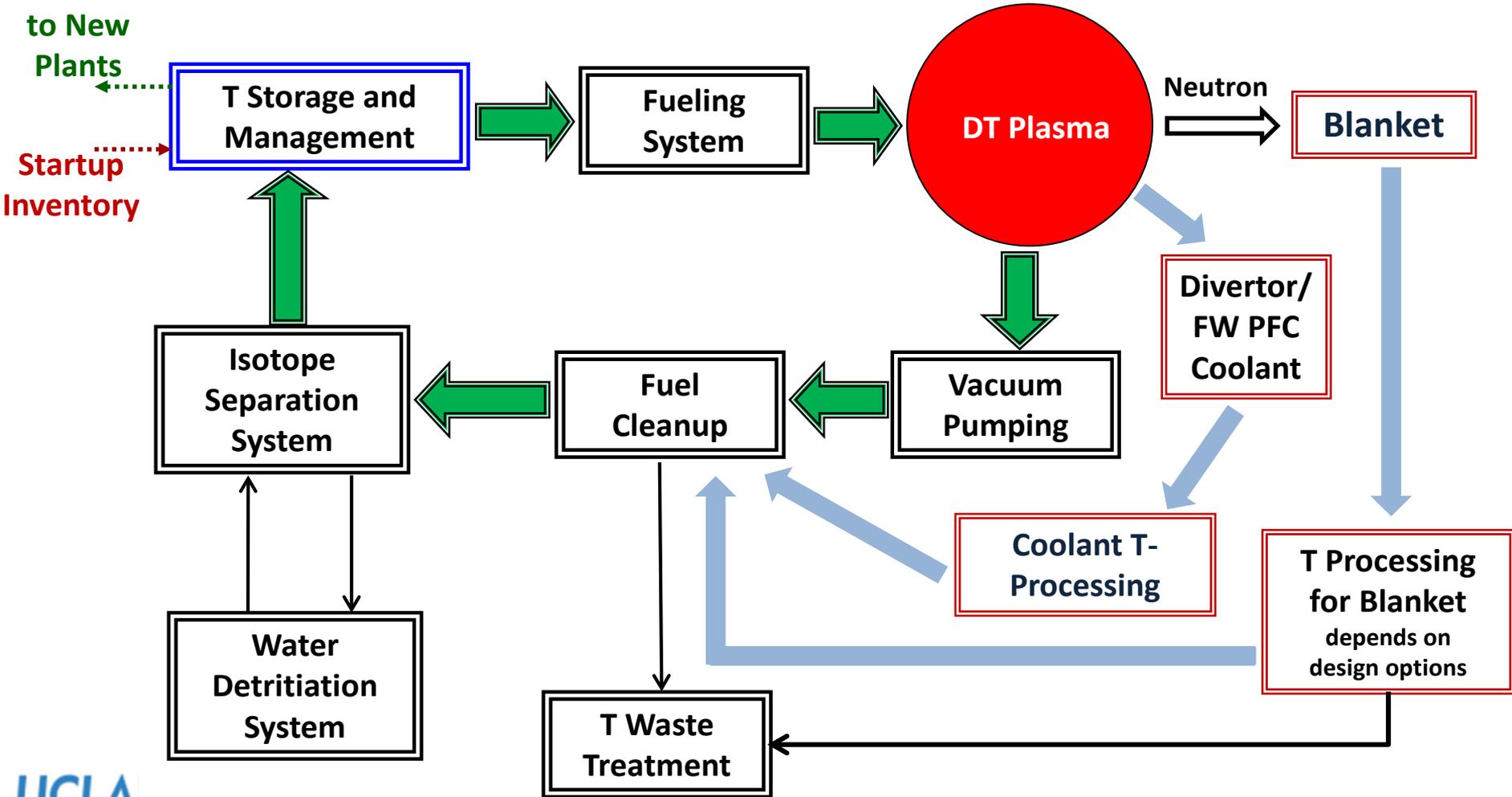


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Tritium Fuel Cycle: Dynamic models developed & advanced since 1986 to calculate time-dependent tritium flow rates and inventories and required TBR

- These models revealed serious issues with the likelihood of attaining tritium self sufficiency in current fusion systems: Very challenging advances in plasma physics and fusion technology are required.
- Since 1986, we have worked with physicists and technologists to perform needed R&D. Progress made.
- But in 2019: more challenging issues and R&D remain



Issues in Achieving Tritium Self-Sufficiency Condition :

Achievable TBR \geq Required TBR

Achievable TBR

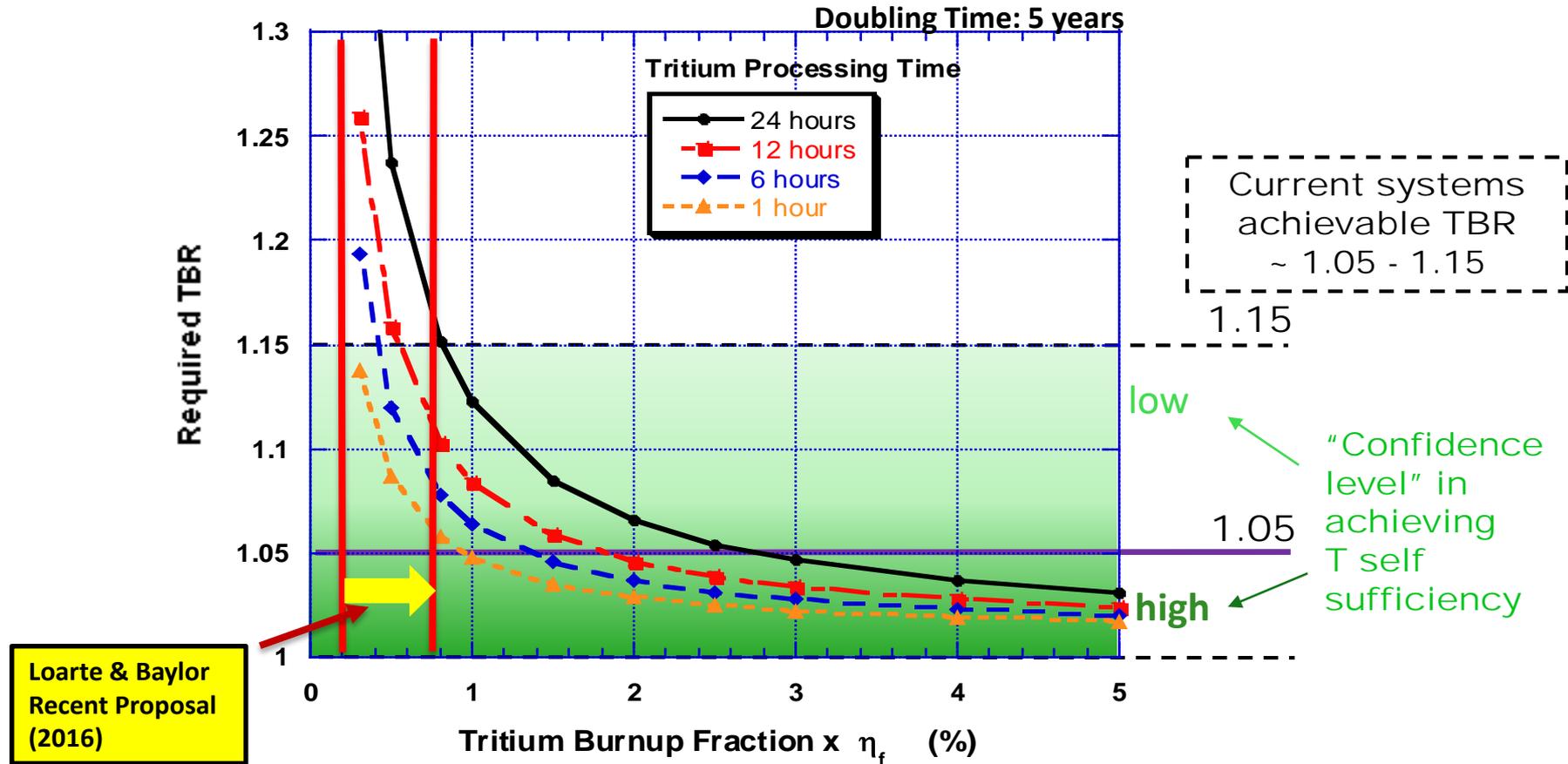
- Maximum achievable TBR with current concepts is 1.05-1.15 (the range is due to uncertainties in calculations and data)
- Strong dependence on “System Definitions” (e.g. amount of structure in FW/Blanket/Divertor, presence of passive coils for plasma stabilization, penetrations)
- Accurate prediction of achievable TBR requires testing of full blanket (or at least a full sector) in plasma-based device (cannot be done with ITER TBM modules)

Required TBR

- Very strong dependence on plasma and technology parameters: e.g. plasma burn fraction, fueling efficiency, tritium processing time, reliability of tritium system, reactor system availability
- With **state of the art** (ITER: $f_b \sim 0.35\%$, $\eta_f < 50\%$), the required TBR is > 1.2
- Recent proposals for improvements in $f_b \eta_f$ are promising but not assured, nor sufficient

There are large uncertainties in achieving T Self-Sufficiency

The required R&D is challenging

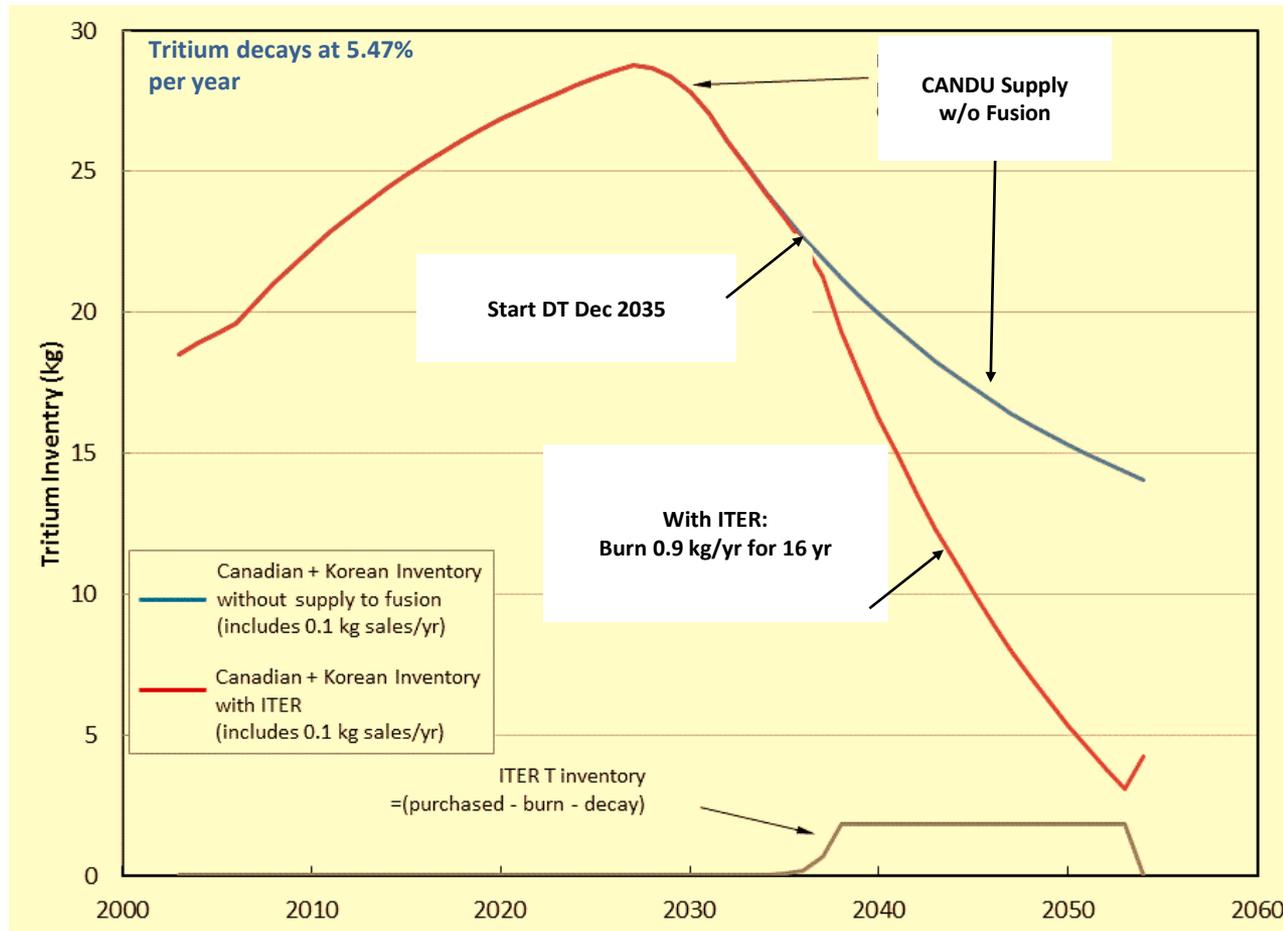


State of the art (ITER: $f_b \sim 0.35\%$, $\eta_f < 50\%$) achieving T self-sufficiency is Unlikely.

To change this to Likely, we must:

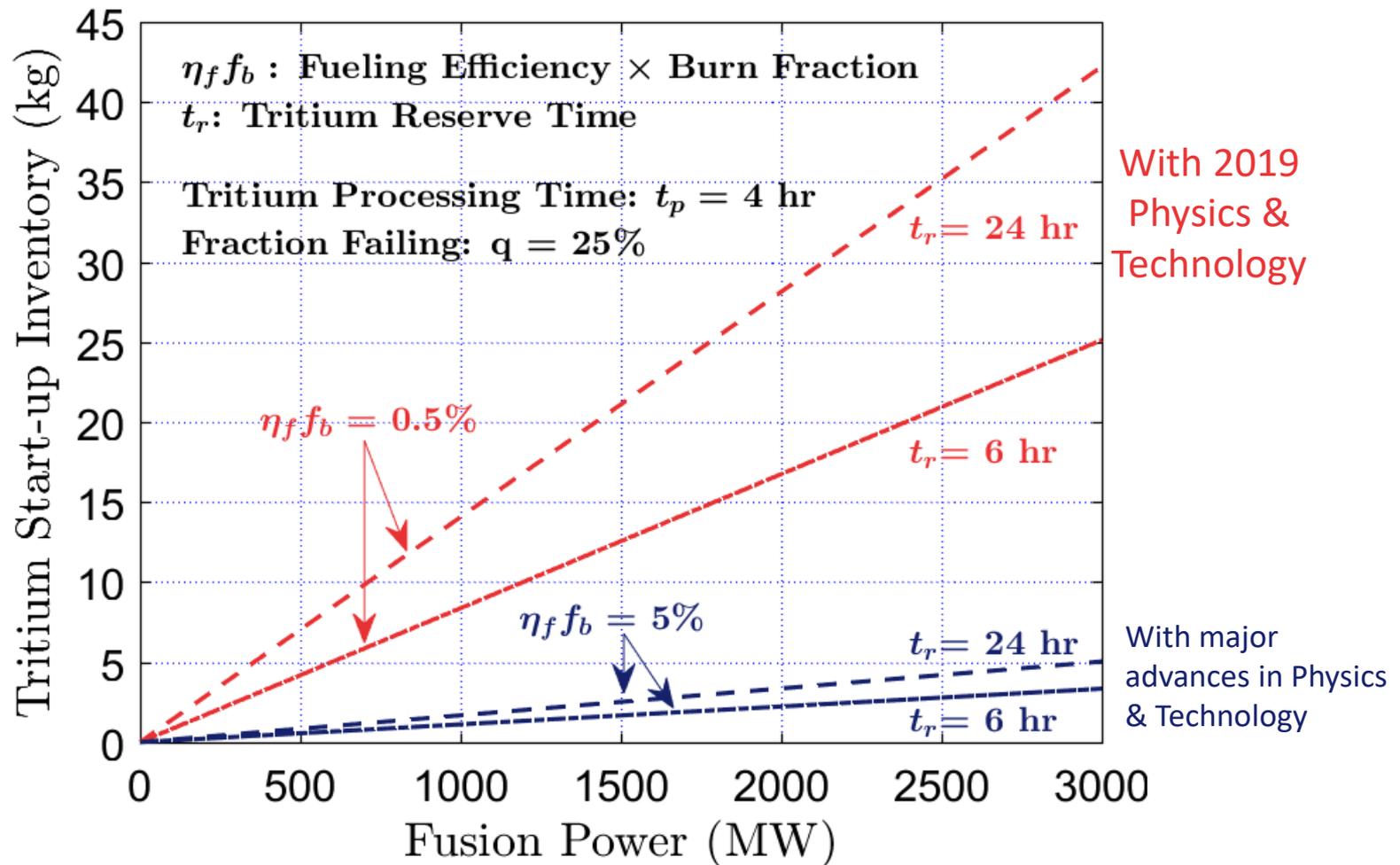
- **Lower Required TBR:** R&D to achieve $f_b \times \eta_f > 5\%$ and $t_p < 6$ hours (how to get there?)
- **Increase Achievable TBR:** Reduce structure and non breeding materials, etc.&

Issue: With ITER DT start in 2036, there will be no tritium left to provide **“Start up” T inventory** for any major DT Fusion facility beyond ITER
The tritium we had at the beginning of ITER design has already decayed!



Required tritium Start-up Inventory depends on many plasma physics and technology parameters.

Also note that it increases with Fusion Power. Plasma-based test facilities with low fusion power need relatively small and obtainable start-up inventory



Lessons learned regarding tritium supply for start up inventory

The world fusion programs **cannot depend** on external non-fusion supply of T to:

1. Provide startup T inventory for 2 or 3 DEMOs plus other facilities such as FNSF and CFETR
2. Provide replacement for any shortfall in satisfying T self-sufficiency in large power fusion devices

Therefore, Fusion Development Pathway must develop a strategy that confronts this problem. Examples of some key elements of such a strategy:

- Every effort must be done to **minimize the Required Startup T Inventory:** e.g. higher burn fraction, higher fueling efficiency, shorter T processing time, and minimization of T inventory in all components
- **Minimize failures in tritium processing systems** and required reserve time
- No DT fusion devices other than ITER can be operated without a full breeding blanket
- Development of breeding blanket technology must be done in low fusion power devices
- **Use FNSF to accumulate excess tritium sufficient to provide the tritium inventory required for startup of DEMO**

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Why FNSF (i.e. VNS, CTF, etc.) was proposed in 1984 with many subsequent studies confirming the need for such a facility

- Laboratory facilities cannot simulate adequately the multiple field fusion nuclear environment. In particular, nuclear heating in a large volume with steep gradients cannot be simulated in laboratory facilities or fission reactors. These can be simulated only in a DT Plasma-based facility (now called FNSF).
- FNSF is a plasma-based facility to learn behavior of Blankets/FW/Divertor in the fusion nuclear environment, learn about multiple/synergistic-effects phenomena, quantify the potential to attain T self-sufficiency, and possibly produce excess tritium to supply the Required Start up inventory for DEMO; and understand failure modes, rates, effects (RAMI).
- The requirements for FNSF were defined in FINESSE (1983-85) and refined in IEA HVPNS (1994-96): 1-2 MW/m² on 10-20 m² test area. Only inside the vacuum vessel (FW/Blanket/divertor modules) need to be prototypical. Plasma can be highly driven, $Q \sim 1-3$. Recommend normal conducting TF coils (to reduce inboard B/S thickness, also increase maintainability e.g. by using demountable coils).
- In the 1980's, we studied if plasma physics and FNST development should be combined into one facility or performed in two separate facilities (one ITER-type facility for burning plasma physics and plasma support technology, and another smaller size FNSF for FNST). The conclusion was DEFINITIVE: Two facilities are faster, less expensive, and more practical than one facility!!!

Launching an initiative to build FNSF soon is good for ITER, good for DEMO, good for fusion

- **In 2019:** The changes in ITER design, ITER TBM and what we are learning about the importance of extensive FNST testing for multiple effects, RAMI, tritium self sufficiency, etc. show that the conclusions of the 1980's and 1990's studies about the need for both ITER and FNSF were far-sighted.
 - Blanket testing in ITER has been sharply reduced from the original program planned on ITER in the 1980's. ITER has a good reason to do this: ITER is focused on burning plasma physics and large-scale plasma effects (e.g. disruptions). So now ITER TBM is useful but does not address the FNST development needs for DEMO

Recommendations

- Build FNSF soon, parallel to ITER, to focus on development of FW/Blankets/PFCs/Materials/RAMI for DEMO. This way we can build the DEMO sooner and let ITER focus on its primary mission.
- Select a version of FNSF that can make it near term (operation parallel to ITER). Make it small volume, low fusion power, with small requirements for external T supply, simplest, most reliable, driven plasma with current physics basis to enable the FNST mission.

Who should lead the effort to build FNSF?

- Not fair to ask the EU. **EU** is contributing its fair share for fusion development by taking the lead on ITER and carrying out a very strong program on DEMO with associated extensive R&D program.
- Taking the lead on FNSF is an excellent opportunity for the **US** to restore a leadership role, enhance contributions to fusion development, and to provide a solution for the rapid erosion of experienced human resource base, and the severe decline in R&D facilities.
- **China** has made an excellent initiative by introducing CFETR and by rapidly expanding R&D facilities and man-power.
 - CFETR plan has two phases: phase-I is FNSF-type mission with low fusion power (~ 100 MW) while phase-II is an upgrade of the same facility with much larger power to serve as DEMO. The device is large, comparable to the size of ITER ($R \sim 6$ m).
FNSF Phase-I in CFETR is important to the world fusion program.
- **JA** and **KO** can also take the lead, or contribute to the R&D for FNSF.

Important Role of Universities and Involvement of Industry

Important role of universities

- Fusion development is long term with many grand challenges: need to attract and train bright, young, brilliant minds to do research in environment conducive to innovation. This is what universities do.
- University programs are absolutely vital to sustain the worldwide effort to develop fusion.
- Recent trends in some countries to move university programs to national labs are ill-thought out. This threatens the future of fusion development. It will make it difficult to attract bright students any longer. There is already lack of engineering skills in fusion.

Early involvement of industry

- In the 70's, 80's, and 90's, industry was supporting fusion development in a variety of roles. (I personally learned a lot from experienced engineers in the aerospace and fission industry.)
- With ITER, industry role is viewed strictly as supplier of parts and components.
- A better approach is needed to bring industry back into fusion R&D, design, and planning. Exploit ability of industry to contribute its experiences to simplify and improve the design, manufacturability, assembly, and maintainability.

Private vs Public Investments in Fusion

- In the 70's, 80's, and 90's there were no private investors interested in fusion.
- In recent years, a number of fusion companies were formed with **private funding** from major donors. Examples of such companies are TAE Technologies, Commonwealth Fusion Systems and General Fusion in North America. Examples of major donors and private entrepreneurs include Bill Gates and Paul Allen from Microsoft, Jeff Bezos/Amazon, Google, and several others).
- These private companies seek to develop fusion on a **SHORT time scale (10 - 15 years)**.
- **This is a very welcome trend for fusion.** Such private funding from major donors is a recognition of the tremendous potential of fusion to advance the welfare of mankind.
- Some fusion scientists argue that these companies are making many unproven assumptions and are following risky approaches. My view is that these privately funded companies are creating R&D environment conducive to innovation in which brilliant minds will make new discoveries and inventions that might lead to a more effective approach to fusion commercialization.
- This trend should be encouraged and private-public partnerships should be pursued.
- **Two privately-funded fusion companies have been invited to this ISFNT-14 to give Special Plenary Keynotes presentations on Friday morning.**

Concluding Remarks

- Pace of fusion development has been too slow.
- Regardless of the reasons for this, the negative effects on the perception of fusion outside the community and the confidence and enthusiasm inside the community are obvious.
- We can not continue to talk about issues we know how to solve and ignore critical go/no-go problems that we don't know yet how to solve.
- It is time for all of us to bring in **ingenuity, experience, determination,** and **honest critical thinking,** and to **ask for a more effective, more agile management and leadership,** to develop a credible strategy for solving them and begin serious implementation at a much faster pace— *than over these past 40 years.*

Thank you!