

R&D challenges in FNST for which we need a credible strategy and implementation plan NOW

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with input from N. B. Morley, A. Ying, S. Smolentsev and others

This Talk:

- Will not list detailed R&D items. We have done this many times.
- The focus is on the critical go/no-go problems for which **HOW** and **WHERE** to perform the R&D is a challenge, yet there is not a credible strategy being adopted, communicated, nor pursued

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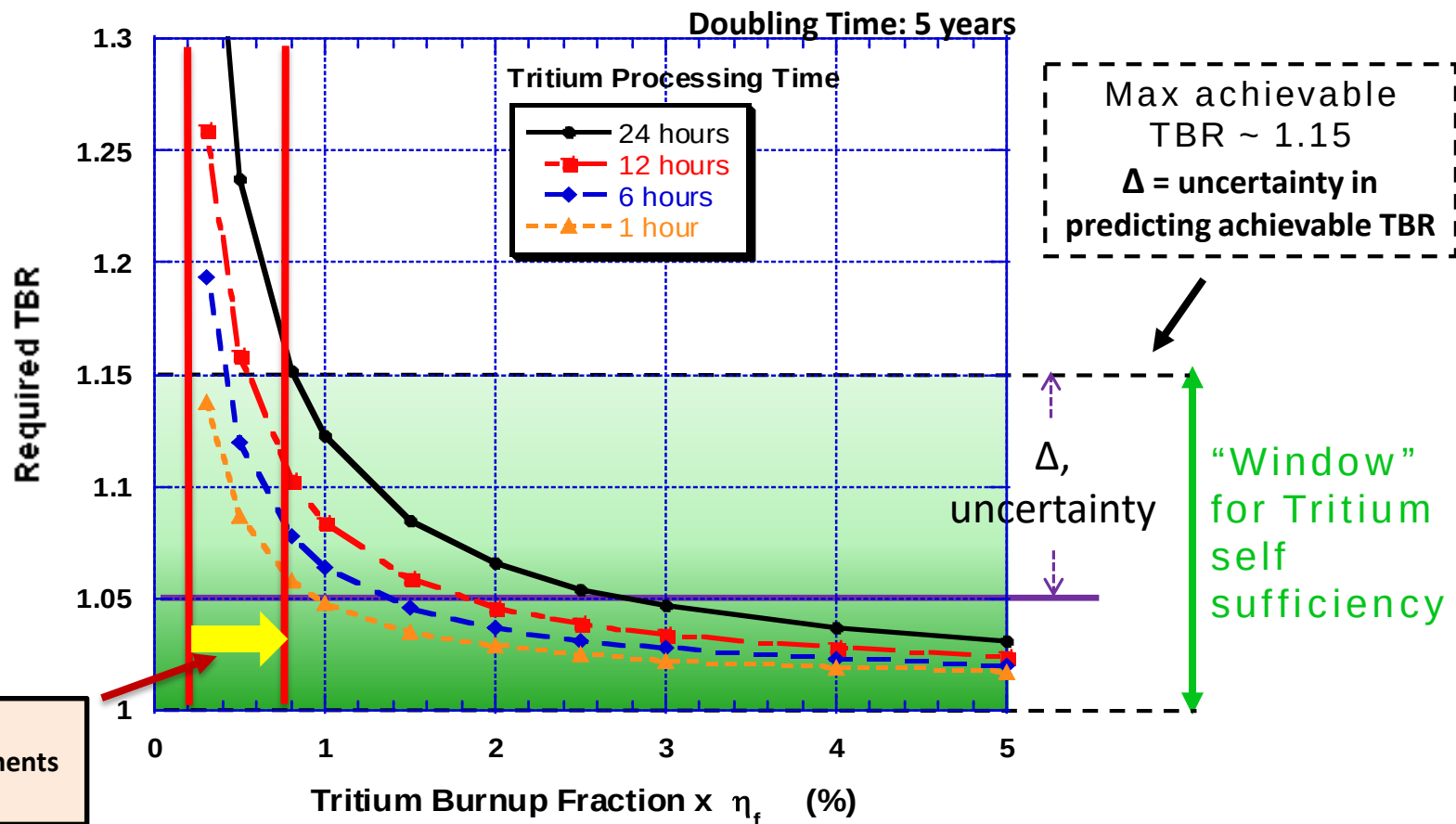
M. Abdou FPA meeting in DC 12-7-2107

Very short talk : no time for details

Scientific bases for this talk are detailed in
Dec 2015 comprehensive paper:

Abdou, M., Morley, N.B., Smolentsev, S., Ying, A.,
Malang, S., Rowcliffe, A., Ulrickson, M.,
"[Blanket/First wall challenges and required R&D
on the pathway to DEMO](#)", Fusion Engineering
and Design, 100:2-43 (2015).

Need to Demonstrate T Self-Sufficiency early (fundamental requirement). But there are large uncertainties and the required R&D is challenging

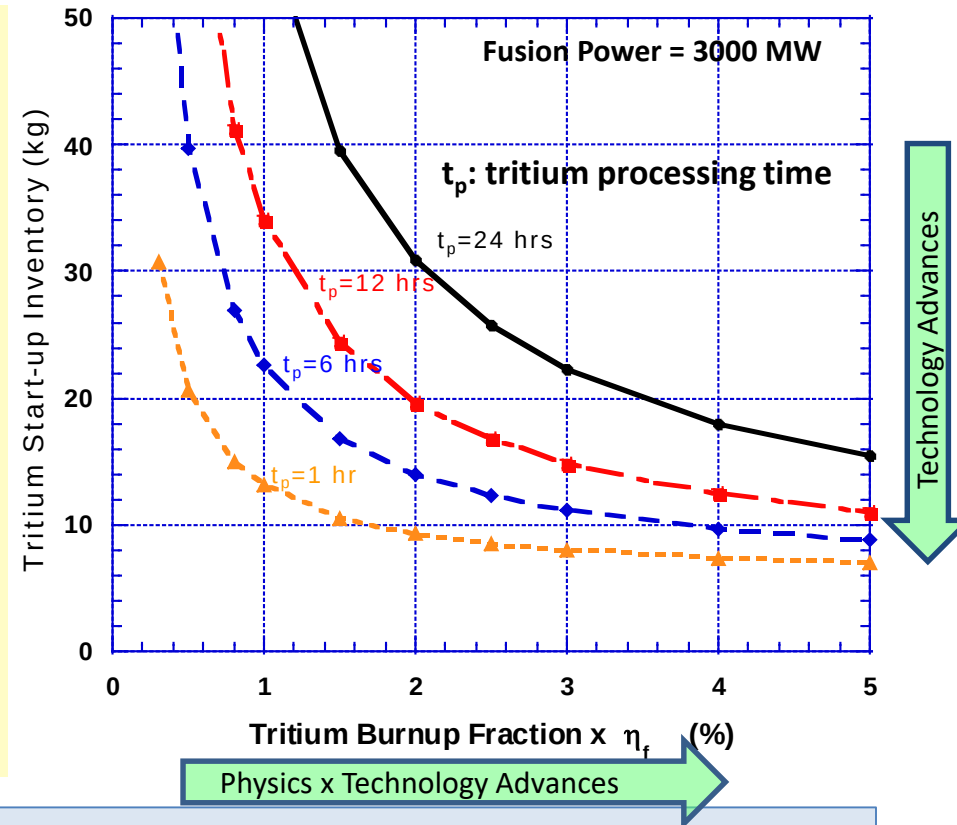
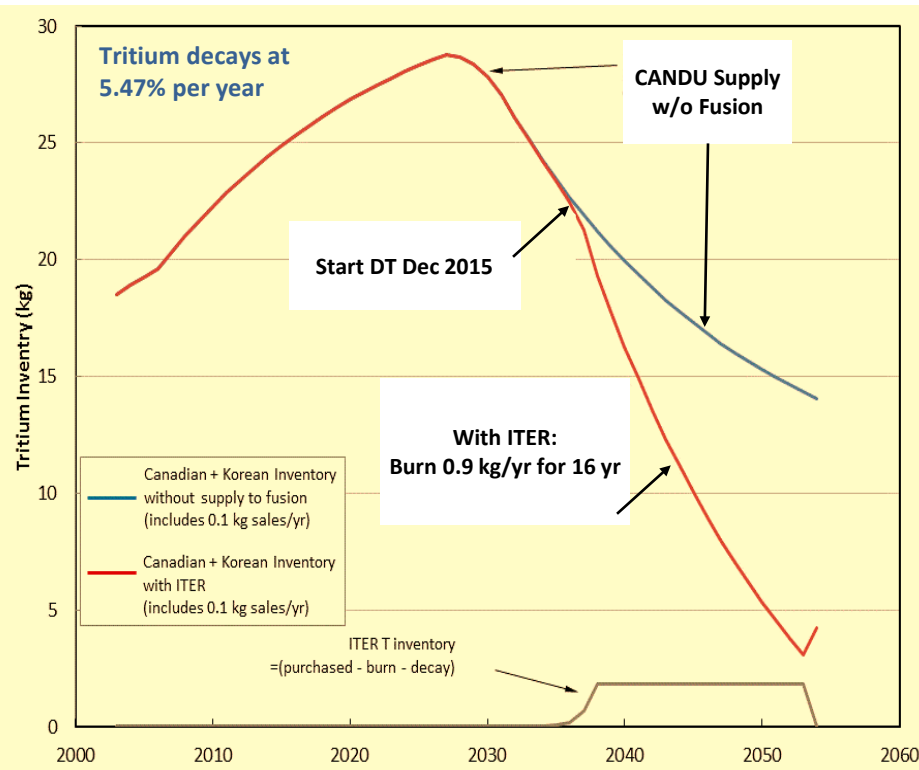


State of the art (ITER: $f_b \sim 0.35\%$) : achieving T self-sufficiency is Unlikely.

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 (some recent progress)
- **Reduce uncertainties in achievable TBR, Δ :** R&D for blanket, and conduct “full blanket” (or at least “full sector”) tests in DT Fusion Facility. **ITER will not do it.** So, **Where** and **When?** (need FNSF)

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
 But the Required T Startup inventory is HUGE unless we do something



What we must do now:

- Physics and Technology R&D to minimize the required T Startup inventory:
 - $f_b \times \eta_f > 5\%$, $t_p < 6$ hours
 - Also minimize T retention inventories in blanket, PFC

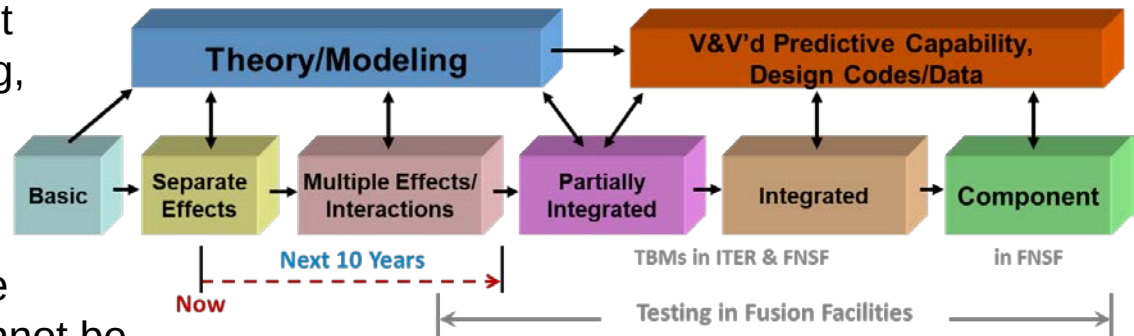
What we also must be working towards:

- Build a small size, low fusion power FNSF early enough to generate excess T to provide startup inventory for DEMO

Multiple Effects/Multiple Interactions Issues for FNST

Technical Issues

- The fusion nuclear environment is multi-field (volumetric heating, surface heating, steep temp gradients, 3-D Magnetic Field, Gravity, etc.)
- The blanket/FW behavior in the fusion nuclear environment cannot be predicted by synthesizing results of separate effects; and predictions are wrong



What to do

- Move forward with Multiple Effects/Multiple Interactions Experiments and Modelling, which are NECESSARY to understand and learn the behavior of blankets in the fusion environment

Pathway Issues and Needed R&D:

- No such facilities exist. New Lab facilities with multiple effect capabilities MUST be built
- But full simulations in the Lab is impossible because volumetric heating can be simulated only in DT Plasma-based facility. Need to build experimental FNSF
- Extrapolation from lab facilities to FNSF/DEMO is extremely problematic (non-linear phenomena similar to plasma physics issues). Launching Major 3-D Modelling Initiative is a MUST

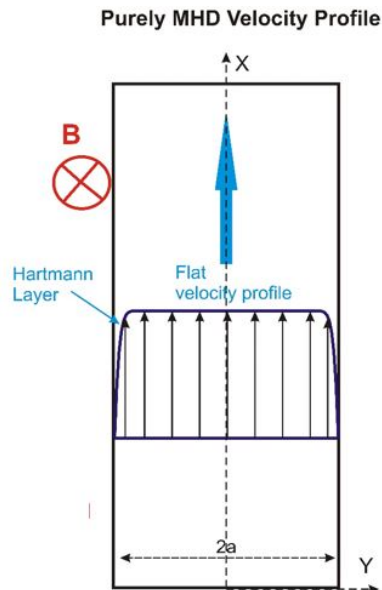
One Example, Being pursued at UCLA (with EUROfusion as partner):

Spatial gradients in volumetric nuclear heating & temperature combined with $\vec{g}$ and $\vec{B}$ lead to New Phenomena that fundamentally alter our understanding of the MHD Thermofluid behavior of the blanket in the fusion nuclear environment

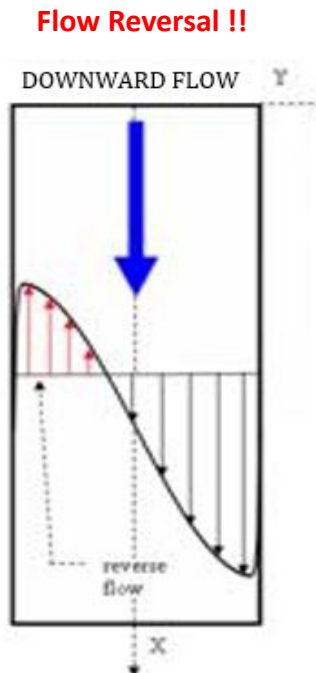
LM MHD R&D in the past 30 years:

Separate effect

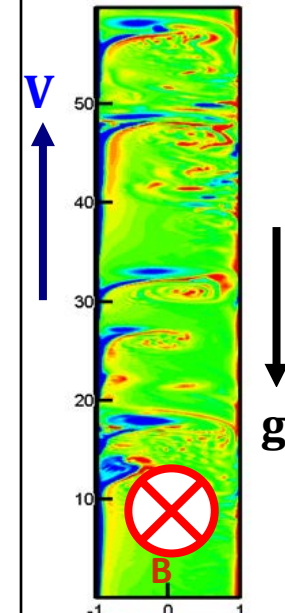
LM with only magnetic field
Flow is Stable, Laminar



Multiple effects lead to **Buoyant MHD interactions** resulting in an unstable “Mixed Convection” flow regime



shows new **instabilities** that phenomena (Heat, T, Corrosion)



- Predictions from separate effect tests for the integrated fusion environment are wrong
- Blankets designed with current knowledge of phenomena and data will not work

R&D must move fast to multiple effects :
3-D Modelling AND Experiments in Laboratory facilities and in FNSF

Reliability / Availability / Maintainability / Inspect. (RAMI)

- Fusion nuclear components INSIDE vacuum vessel in complex configuration lead to fault intolerance and complex lengthy remote maintenance
 - Estimated MTBF << required MTBF
 - Estimated MTTR >> required MTTR
- } Low avail.
- Estimated availability for current confinement schemes with blankets/FW/divertors inside vacuum vessel is only a few percent.
 - No practical solutions yet
 - How to do RAMI R&D? No one has credible strategy

Observations and Suggestions for improving the situation with the “K” Issue of RAMI

- MTBF/MTTR will be the key issue in determining the feasibility of plasma confinement configurations and the feasibility of blanket concepts
- Performance, Design Margin, Failure Modes/Rates should now 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 and safety margins can be achieved and designs simplified & improved
 - Subcomponent tests including non-nuclear tests
 - Build FNSF early as “experimental” facility that focuses only on the FNST components inside the vacuum vessel. Realistic estimates of MTBF and MTTR can be obtained only from a DT device.
 - Be prepared for surprises and be ready to change pathway. Understand that Reliability Growth takes very long time

Imagine We had a facility today in which the fusion nuclear environment is simulated and had enough test volume to do experiments on the fusion nuclear components (in-vessel components: Blanket/FW, T system, remote maintenance)

What would have happened?

- We would have resolved most of these critical go/no-go issues
- We would have had real assessment whether the path we are on now leads to practical fusion
- We would be in better position to address “fusion is always 40 years away”

What kind of facility is needed?

- The only way to simulate the fusion nuclear environment with sufficient volume is to have DT plasma based facility. But plasma performance requirements are modest: driven, $Q \sim 2-3$

Why do we not have this facility today? Why a fusion program with a mission to build a large, high performance powerful DT plasma with very high Q has not yet built a modest small-size low power DT plasma device? Mystery!!

- Physicists need to think of driven DT plasma for FNSF as ENABLER of Fusion Nuclear Science and Technology Development (think of “ENABLING Plasma”. Do not burden FNSF with ambitious physics or superconducting magnet mission)

VNS/CTF/FNSF

Magnetic Fusion Development Requires the Construction of an Experimental DT plasma-based facility that can simulate the fusion nuclear environment with enough volume ($\sim 10 \text{ m}^2$ surface area x .5 m) to perform experiments on fusion nuclear components (components inside the vacuum vessel: Blanket/FW, Divertor)

- To build it soon with affordable cost:

Make it small volume, low fusion power, with small requirement for external T supply, simplest, most reliable, driven plasma with current physics basis to enable the FNST mission

- Is this idea new?

No, it was first proposed in 1984 (in FINESSE) and studied and evolved over many years/decades in many excellent studies

- What name for the facility?

The name was changed over the years VNS/CTF/FNSF. FNSF is the name adopted since 2007

(not to be confused with “FNSF” in the recent FESS study that defined very different type of facility with very different 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 having a very strong EUROfusion program that focuses on DEMO with associated extensive R&D program.
 - The EUROfusion effort on DEMO is absolutely essential to the world program because:
 - It continues to define a vision for the DEMO, which is essential to guide the R&D program toward DEMO.
 - It performs substantial R&D in many key areas with large expenditure and large multi-disciplinary team.
 - It provides effective mechanism to develop human resources and provide excellent training for mid-career and young scientists by experts with 30+ years of experience who understand fusion and the complexity of the multiple interactions among the components and disciplines
 - EU expressed interest in collaborating with others who build FNSF

Who should lead the effort to build FNSF? (cont'd)

- **How about the US?**

- The US provided strong contributions and leadership in fusion in the 70's, 80's and part of the 90's.
- 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

- **How about China?**

- China has made an excellent initiative by introducing CFETR and by rapidly expanding R&D facilities and man-power. With Government-sponsored funding announced this week, the CFETR program is expected to expand rapidly and move toward implementation.
- But 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)
- So, it remains yet to be seen whether such a strategy is technically practical enough and the initial cost is reasonable enough that long delays in construction and operation can be avoided.

Concluding Remarks

- **We cannot continue to talk only about issues we know how to solve and ignore critical go/no-go problems that we do not know how to solve**
- **It is time for all of us to bring in ingenuity, experience, and determination to develop a credible strategy for solving them and begin serious implementation**

Thank You!