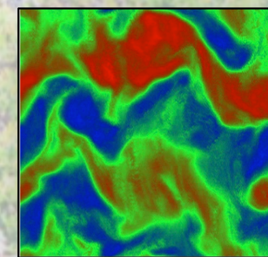
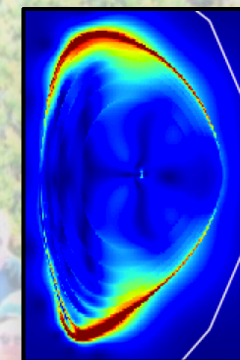
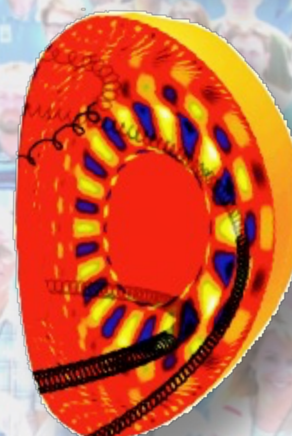
The Advanced Tokamak Path to a Compact Fusion Power Plant

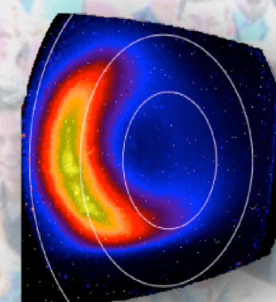
by
RJ Buttery

with acknowledgement of many contributions, ideas and discussions from DIII-D and other colleagues.



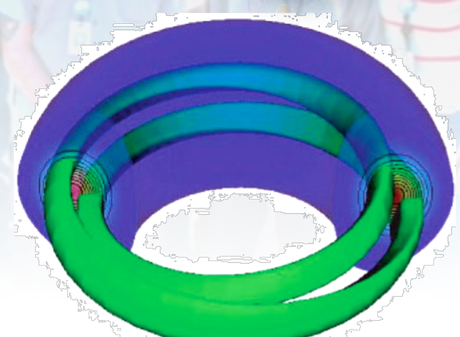


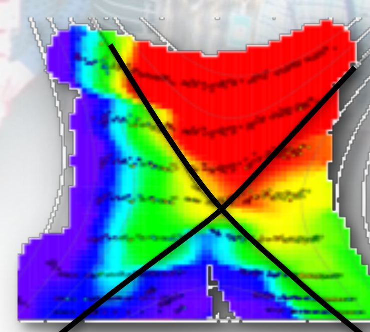




at the
Community Workshop
Madison, Wisconsin

July 25th 2017





Many Scientists Could Not Be Here Today*

These ideas are supported by colleagues who cannot attend – particular those working on the tokamak concept and preparing for ITER*

Scientist who say they support the broad thrust of this talk:

T. Abrams, K. Burrell, P. Byrne, T.N. Carlstrom, X. Chen, C. Chrystal, B. Crawley, G. Campbell, A. Garofalo, J. deGrassie, D. Eldon, H. Guo, L. Lao, J. Lohr, S. Munaretto, C. Muscatello, C. Paz-Soldan, R. Pinsker, J.T. Scoville, E.J. Strait, A. Turnbull, M. Van Zeeland (GA),
R. Sweeney (ITER), E. Schuster (Lehigh), C. Holcomb, A. Jaarvinen, B. Victor (LLNL),
A. Marinoni (MIT), B. Lyons (ORISE), A. Breisemeister, J.M. Park (ORNL),
N. Ferraro, B. Grierson, N. Logan (PPPL),
T.L. Rhodes (UCLA), J. Boedo, C. Holland, E. Hollmann, R. Moyer (UCSD),
M. Austin (UT), G. McKee (UW).

Additional comments from some of these people are pasted at the end of this talk

**Particularly unfortunate in DIII-D run, as many young scientists: of 77 full time on site DIII-D scientists, 38 are <40 yrs old. Additionally DIII-D hosts 21 postdocs on site, and ~25 grad students (partly on site) + many more regular visitors.*

The U.S. Should Establish the Basis to Proceed with a Long Pulse Burning Device within the Next Ten Years

- **ITER is an exciting and vital step**

- **Fusion energy requires research beyond ITER to develop approach for steady state fusion**
  - U.S. has considered a fusion nuclear science facility as a possible requirement
  - ***A compact net electric facility that integrates FNSF & energy generation missions would be a more attractive proposition***

✓ ***These and other possibilities have common research needs to determine device type, parameters and decision to proceed***

U.S. should adopt an "energy philosophy" – take the steps needed to get to a steady state fusion device ASACAP*

**as soon and as low in Capital Cost as possible*

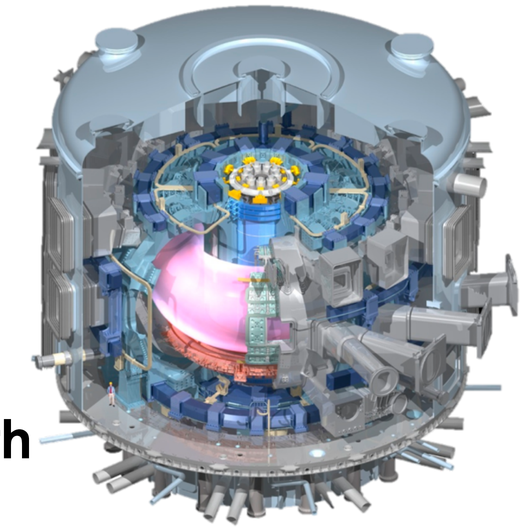
Should we be an Energy Program or a Science Program?

- **We are not doing the things needed to develop fusion as an energy source**
  - Though we do have a quite broad program 😏
- **If we are serious about energy we must add research lines and direct existing lines toward the energy goal**
  - Not necessarily a threat to lose your pie slice – ***must grow the pie!***
  - Of course this is still a science program – but we need to be an energy program as well! This mandates new research lines.

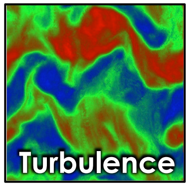
Our focus must be on shortening timescales to fusion energy, resolving key tech & getting to critical test devices ASAP

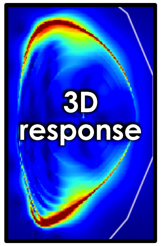
ITER is a Vital and Exciting Element of Our Research Program

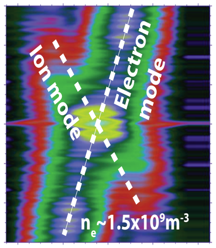
- **ITER will demonstrate the self-heated burning plasma state for the first time**
  - A new regime of plasma behavior
  - New phenomena, test theories, explore new processes

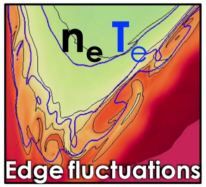


- **ITER provides vital learning for fusion energy path**
  - Already, we are resolving how to build a power plant scale device!
  - ITER tests the physics at power plant scale & mandates resolution of key transient issues
  - Provides crucial validation of theories to project to large scale power plants







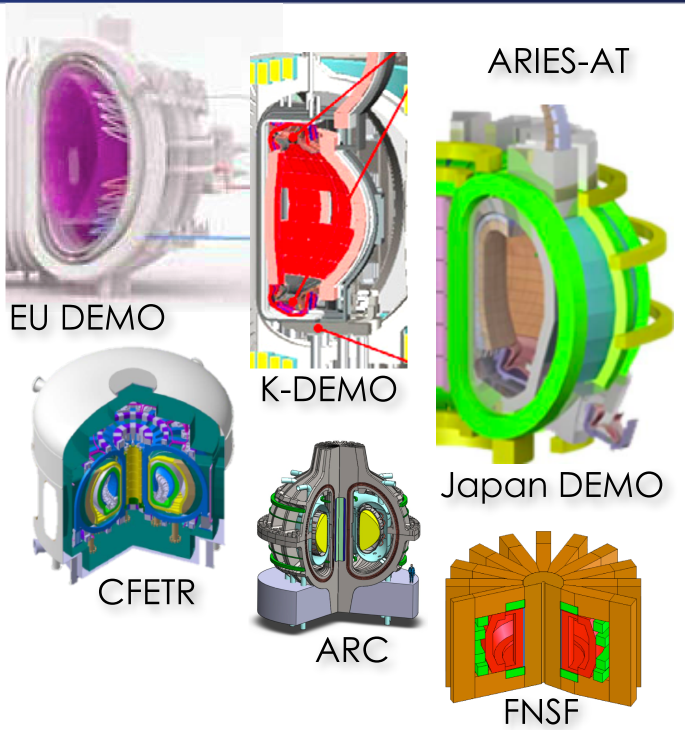


ITER is a cost-effective bridge & an exciting science & leadership opportunity for U.S. fusion

But a continuously operating fusion reactor requires steps beyond ITER

The World Program is Focused on the Advanced Tokamak Path to Steady State Fusion

- **Proposals for next step facilities around the world based on A~3 tokamak**
  - Desirable to reduce scale and cost to meet next step mission



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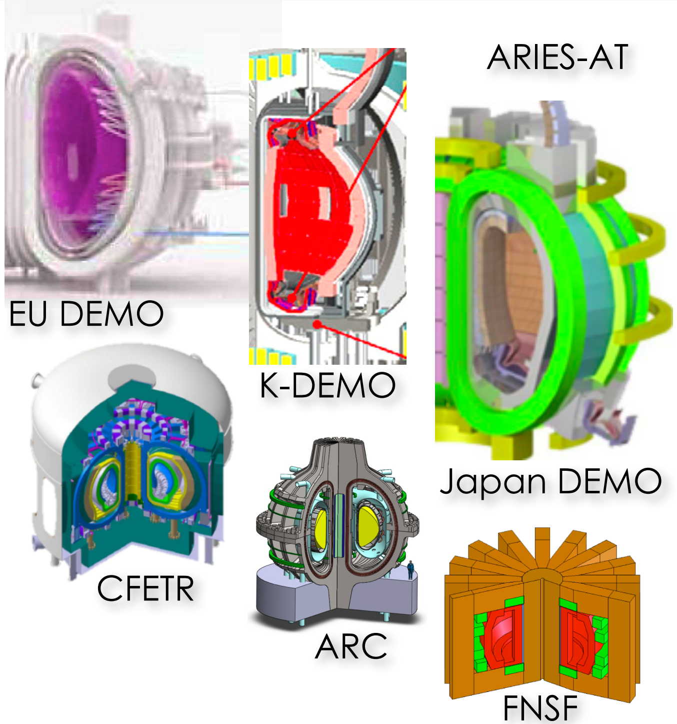
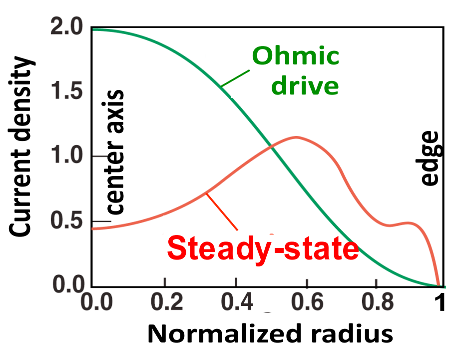
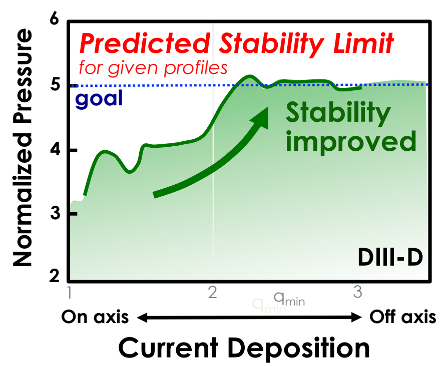
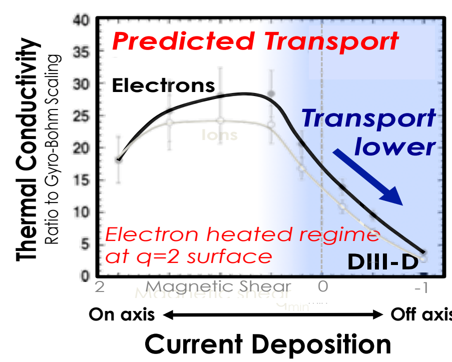
- **Tokamak exploits a synergy between self-driven current & performance**
  - Broad profiles & elevated q improve stability, and reduce transport



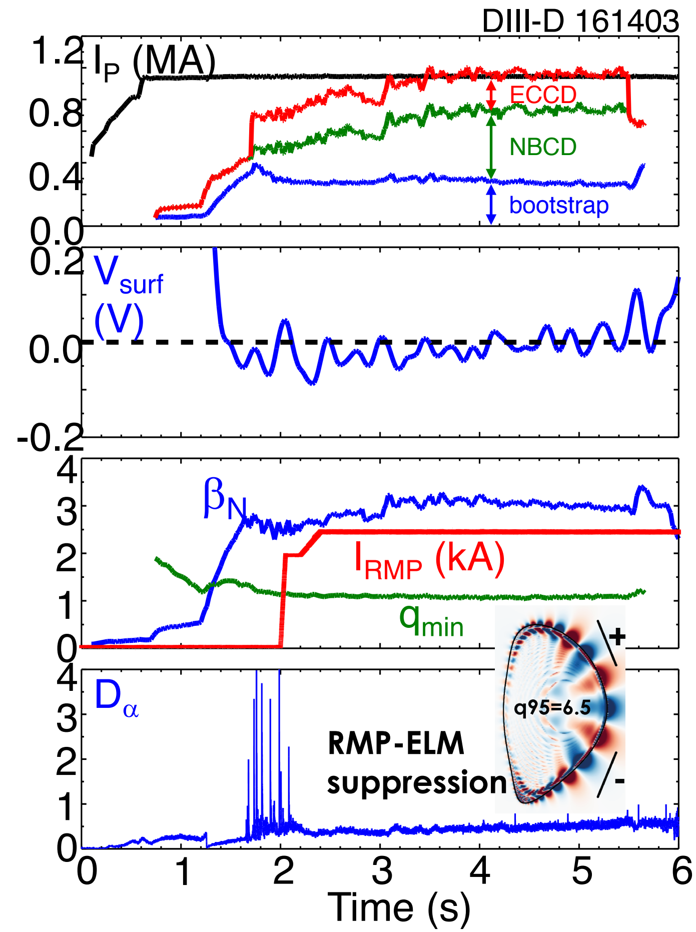






The World Program is Focused on the Advanced Tokamak Path to Steady State Fusion

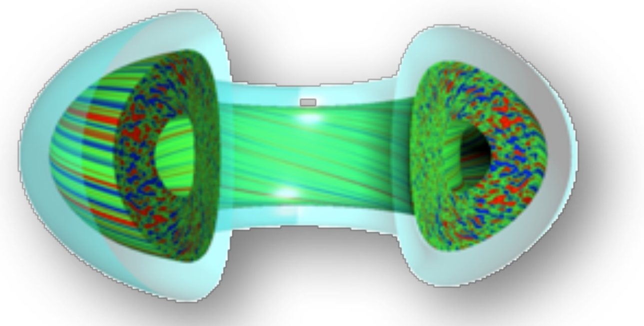
- **Proposals for next step facilities around the world based on A~3 tokamak**
  - Desirable to reduce scale and cost to meet next step mission
- **Tokamak exploits a synergy between self-driven current & performance**
  - Broad profiles & elevated q improve stability, and reduce transport
- **Substantial progress has been made validating key physics elements**
  - Fully non-inductive ELM free →
  - High β_N, transport & fast ion benefits



Analyse this concept to identify R&D for efficient path to fusion

"Full Physics" Analysis Executed to Identify Key Parameters, Trade-offs and Technologies for Reactor

- **FASTRAN simulation suite combines:**
  - TGLF core turbulence simulation
  - EPED first principles pedestal
  - Heating & current drive sources



- **FASTRAN converges these to stationary self-consistent solutions**
  - Identify **fully non-inductive plasmas** for fusion reactor & **predicts H_{98}**

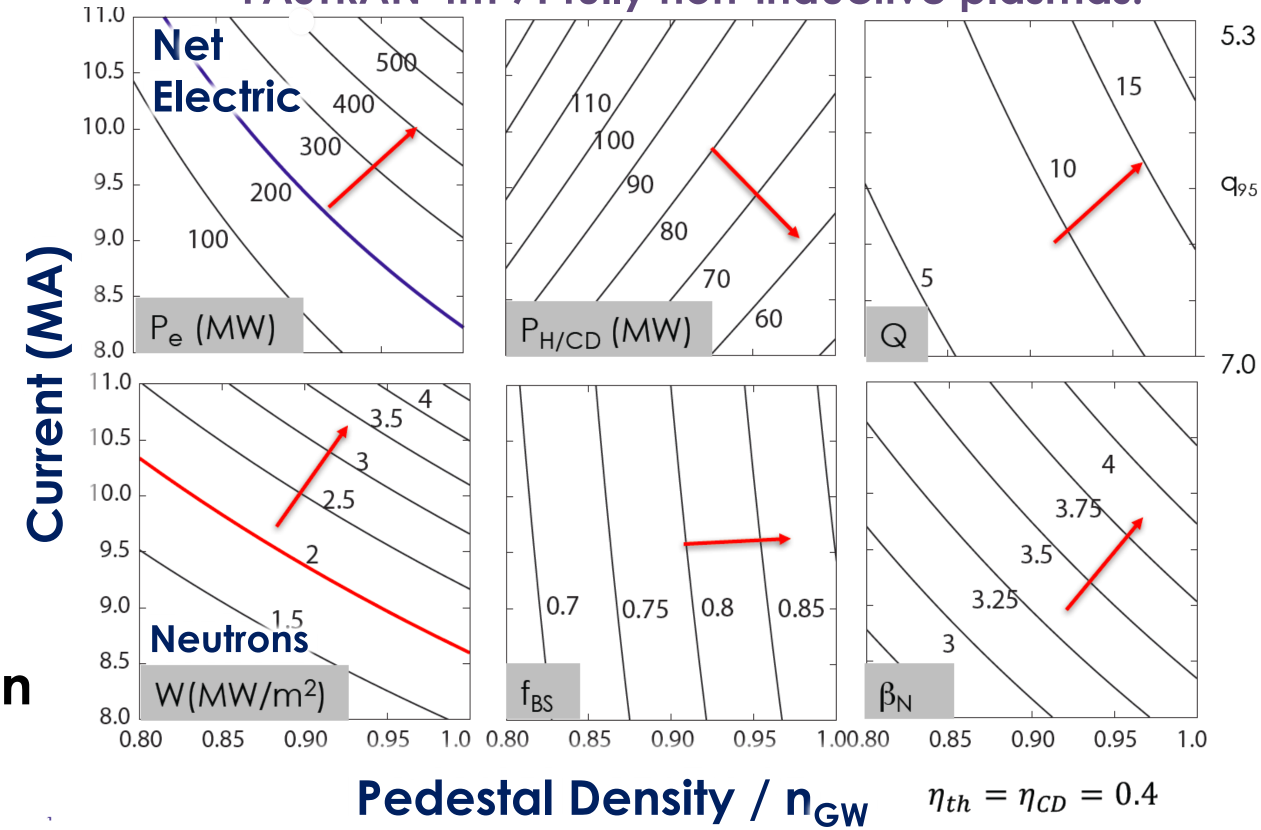
- **Integrated physics solutions identify key dynamics governing reactor design and operating point, e.g.:**
  - Core turbulence gives current dependence, but pedestal adds a toroidal field dependence
  - Need for reactor to generate its own H&CD power can alter dynamic from driven tokamak intuition (can drive up I_P or scale)

Understand parameter optimization to drive R&D needs

Full Physics Simulation Shows Path Toward Compact Self-Consistent Solution

- **Higher density is highly enabling**
  - Raises fusion and bootstrap current
  - Decreases $P_{H/CD}$
- **High β_N increases fusion performance**
  - Raises bootstrap and Q
- **Higher B_T raises margin**
  - 7T vs 6T doubles P_{EL} at fixed q_{95}
  - Enables trade-offs to lower I_P, density or efficiency (tho' 6T soln's possible)

FASTRAN 4m 7T fully non-inductive plasmas:

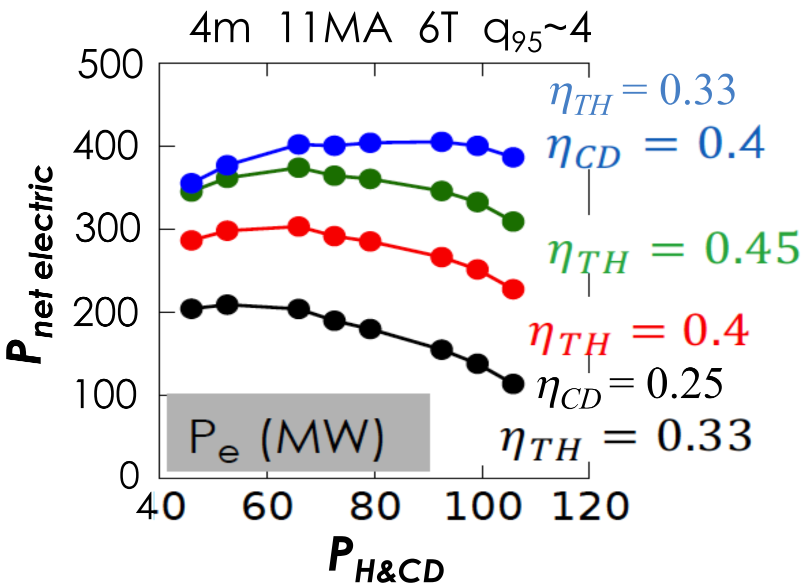


$\eta_{th} = \eta_{CD} = 0.4$

Reducing H&CD demands avoids making electricity to power device
→ Lowers required scale, heat and neutron flux

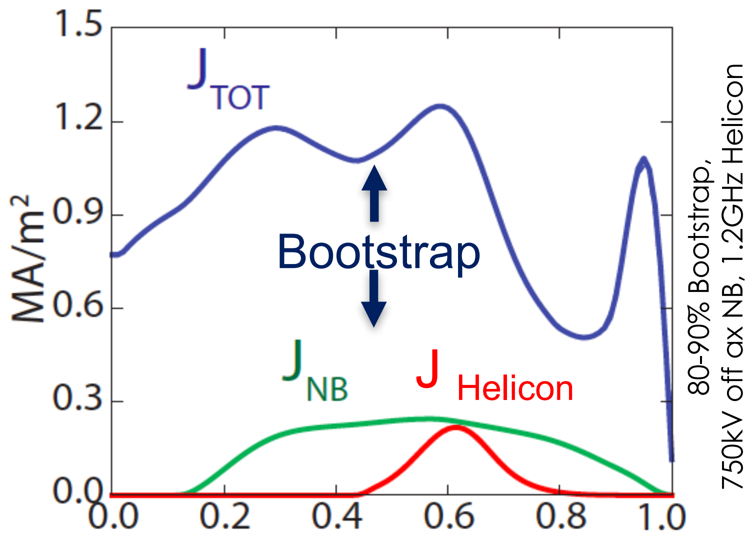
Current Drive and Thermodynamic Efficiencies are Highly Levering

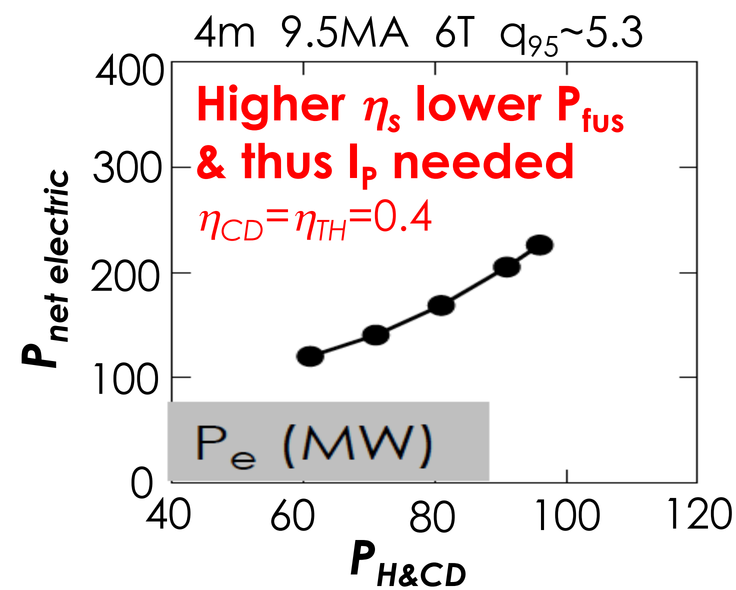
- **Increased efficiencies raise net electric power**



Current Drive and Thermodynamic Efficiencies are Highly Levering

- **Increased efficiencies raise net electric power**
  - Simulations assisted by advanced current drive techniques
    - Eg: Helicon used here:





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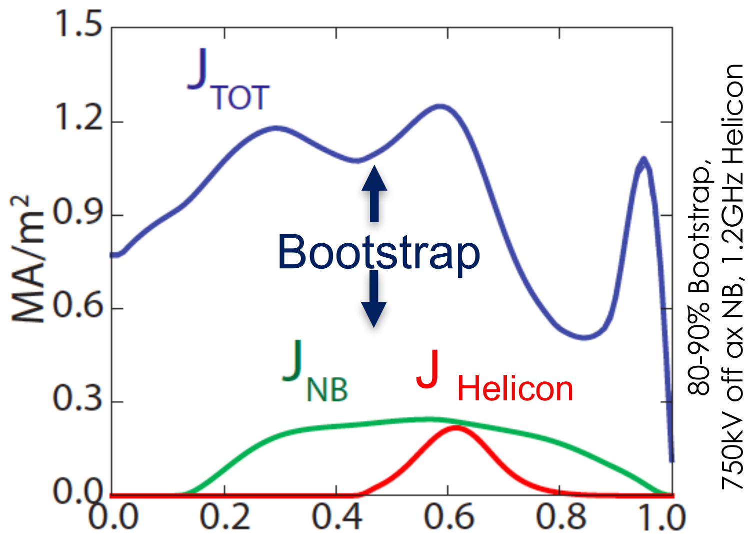

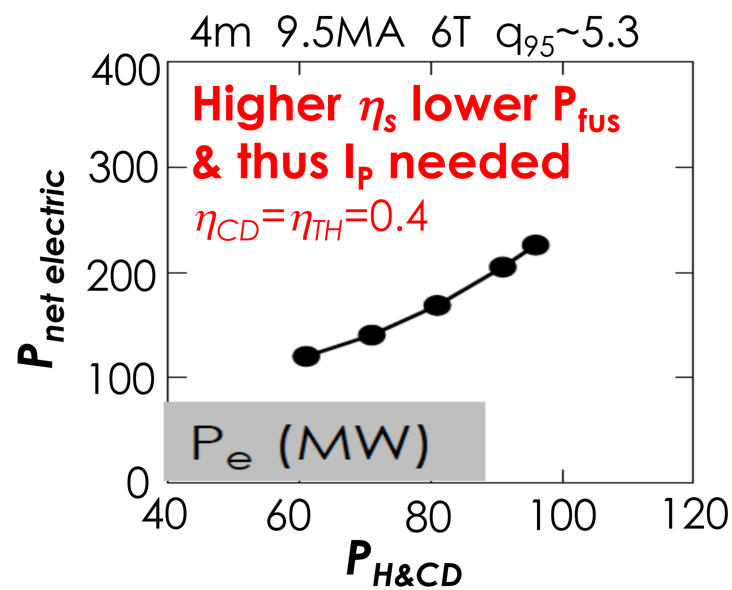

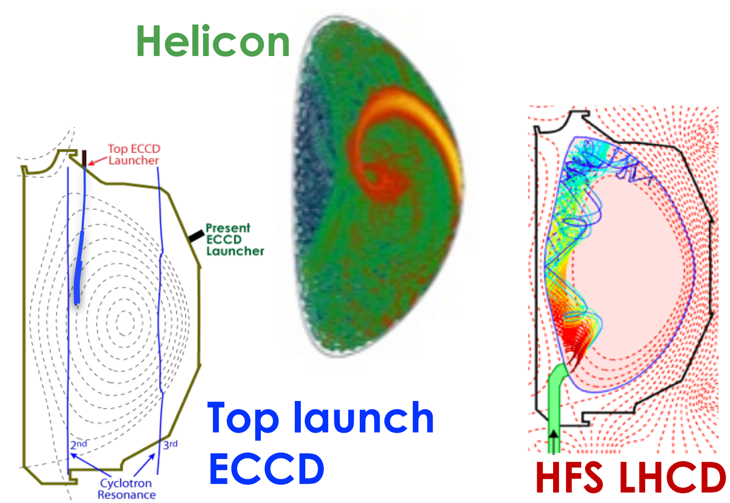

Important to develop and test promising predicted CD methods…

Plasma Exhaust Solutions Most Challenged By Erosion Free Requirement

- **Divertor heat flux handling limited by tile incidence angle, α**

$$q_{div} \sim q_{||} \sin\alpha \sim (P_{SOL} B / N R) \sin\alpha$$

(Using Eich scaling for SOL width) **Divertor challenge metric**

  - Metric easily meets ITER value at 4m scale with 55% core radiation
  - 2.5x margin over L-H threshold. Assisted by 2 divertors cf ITER's 1

Preparation for long pulse D-T motivates divertor, materials & HTC work

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  - Must eliminate erosion in divertor and wall
    - → **Poses a divertor challenge**: need detached configuration that still enables high performance core
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- **Long pulse D-T facility will have mission to qualify nuclear materials and breeding technology for power plant**
  – *Demountable high T_C superconductors highly levering so that facility can efficiently change out & test wall & breeding modules*

Preparation for long pulse D-T motivates divertor, materials & HTC work

Physics Simulations Motivate Urgent Research in Five Key Mission Areas

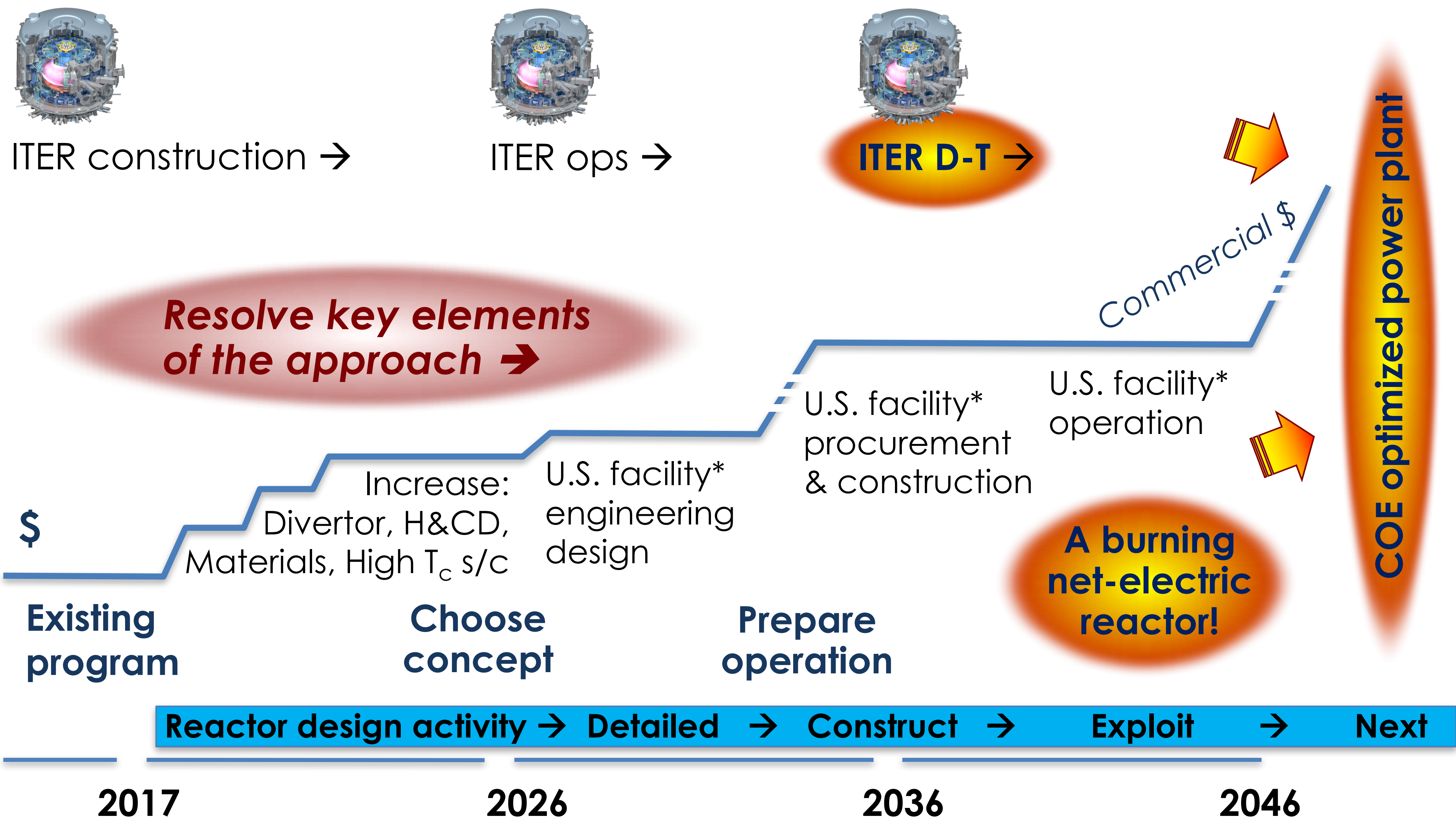
Key missions:

- **Fusion core**
  - Stability & transport, high β_N, high density steady state configuration
- **Plasma exhaust & divertor configuration physics basis**
  - Detachment & heat spreading compatible with fusion core
- **Materials for plasma & nuclear environment**
  - Particle flux & neutron tolerant, low retention, core compatible
- **Heating and current drive technology**
  - Increase net electric, lower device scale. Increase breeding
- **Higher field high T_C demountable superconductors**
  - Raise performance & reactor duty cycle, and more flexibility in experimental materials & breeding program

Engage with a reinvigorated reactor design effort to map paths to a future device, and guide targets & integration for above missions

These elements are actually common to ~all device paths !

U.S. Facility Combines with ITER Learning to Resolve Path to Efficient Fusion Power Plant



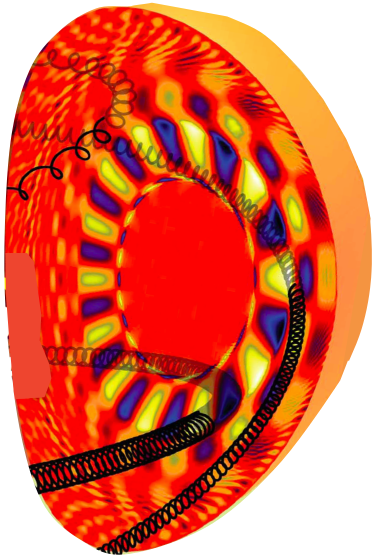
*RJB preferred U.S. facility: compact net electric device

We Should Start Now on an Energy Program to Accelerate the Path to a Fusion Power Plant

- **ITER is an exciting and vital component in the U.S. Program**
  - Critical validation and know-how to advance further devices
- **Pursue required research and technology development to enable decision to proceed with long pulse reactor as soon as possible:**
  - **Core. Divertor. Materials, H&CD. Field. Reactor design.**
- **A compact net electric AT would be an attractive, fundable device to rapidly test and resolve issues for a fusion power plant**
  - *A nuclear test facility may turn out to be required first;* ***this shares many of the same programmatic research needs***
    - *…as do many other approaches*
  - ***Device choice and parameters will drop out of above program***

We can converge on a mission to pursue these lines to enable a decision to start a compact U.S. reactor within ten years

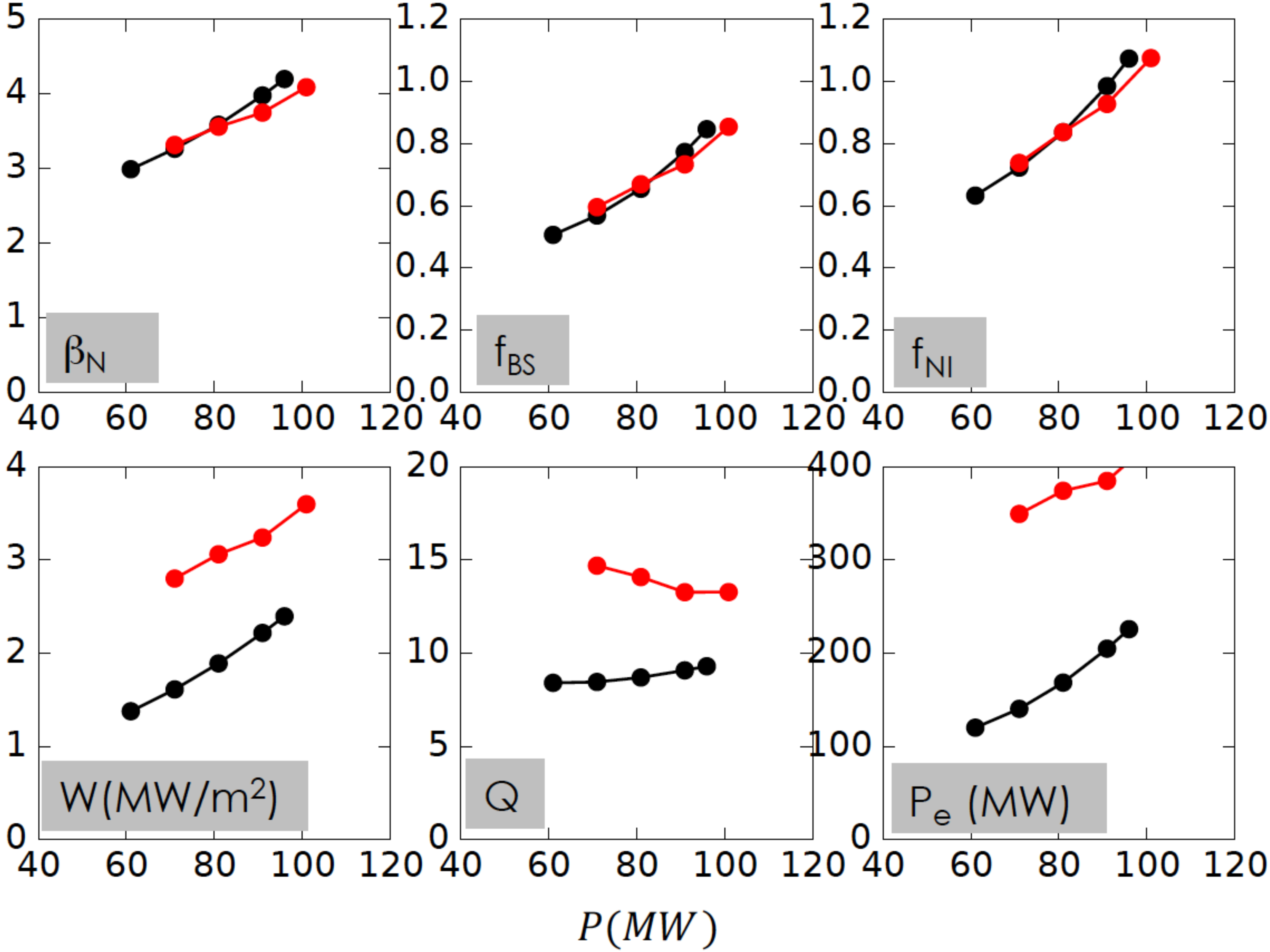
Bonus Slides



Higher Field Increases Fusion Performance:
Power scans varying inductive fraction at fixed q_{95}

Compare: 6T with n_{ped}/n_{GW}=1 to 7T n_{ped}/n_{GW}=0.9

4m q_{95}=5.4

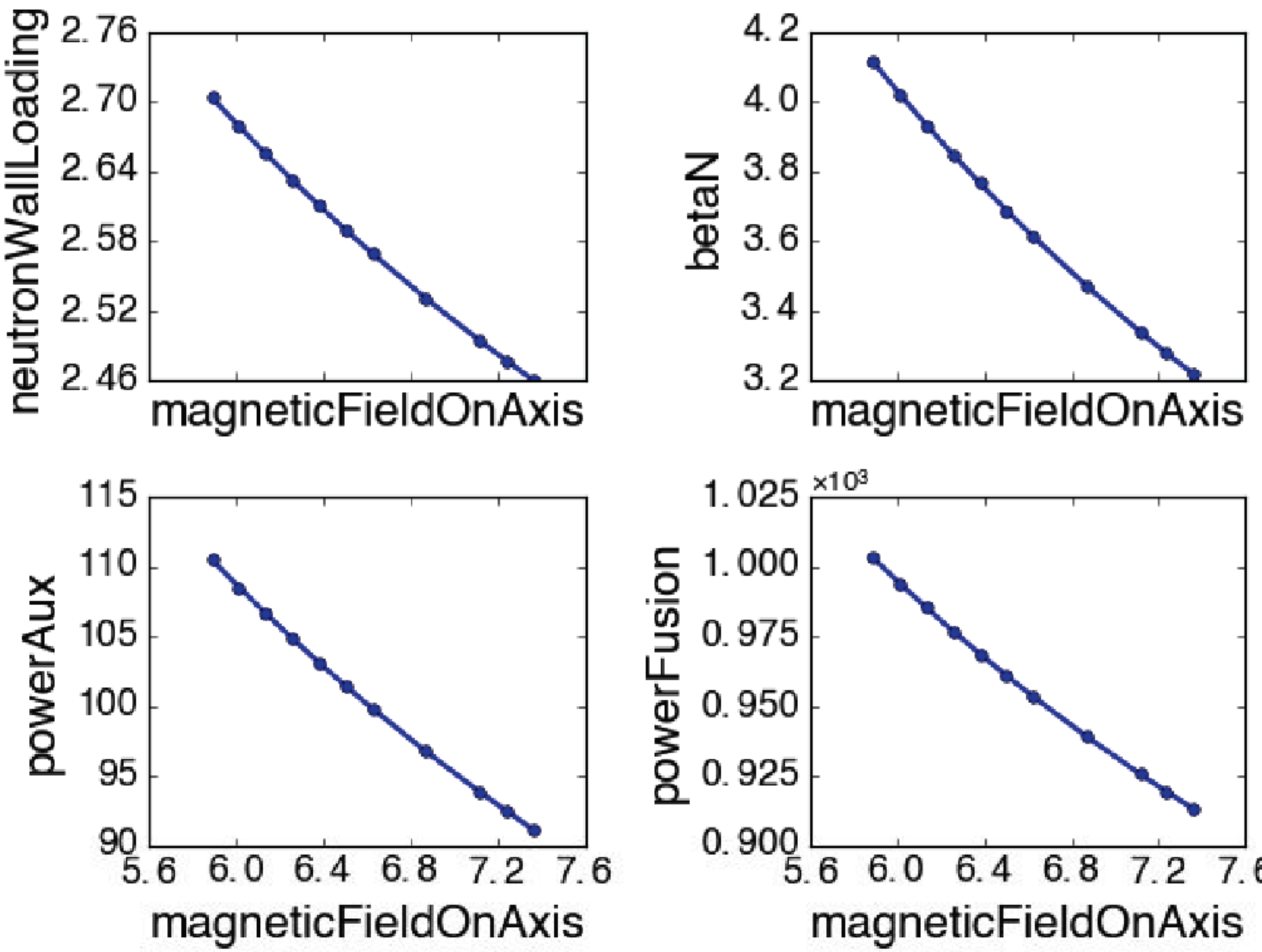


GA System Code Reveals a Trade off in B_T and β_N

η_{th}=0.4,
η_{cd}=0.4
P_{net}=200MW

- **Note y axis ranges!**
- **H_{98}=1.3, f_{GW}=1.33**

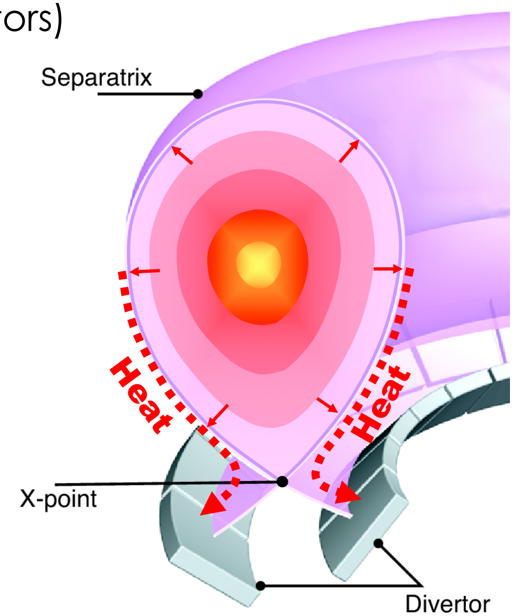
H mode more easily accessed at higher β_N vs higher B_T



GASC **fully non-inductive** simulations at 4.5m,
η_{th}=0.4 η_{CD}=0.4, f_{GW}=1.33, 7T, H_{98}=1.3, P_{el}=200MW

Reserve Slide: Divertor Challenge Metrics: should be PB/NR

- **Power into SOL: $P_{SOL} = P_{alpha\ heat} + P_{H\&CD} - P_{brems/synch/line\ radn}$**
  - Ways to deal with this: core radiation, divertor radiation, spreading
- **Divide P_{SOL} by midplane SOL area: Poloidal heat flux**, $q_\theta \sim P_{SOL} / N\ R\ \lambda_q$
  - Plug in Eich scaling: $q_\theta \sim P_{SOL}\ B_\theta / N\ R$ (N=1 or 2 divertors)



- **But heat flux down flux tube must allows for field pitch at midplane** ***(removes Ip dependence)***
  - Parallel heat flux: : $q_{||} \sim q_\theta\ B / B_\theta, \quad \sim P_{SOL}\ B / N\ R$
- **Expansion factor to divertor S:** $q_{div} \sim q_\theta / S$
  - **S** = flux expansion / sin θ
    (*θ = poloidal tile tilt angle to divertor leg*)
  - So q_θ tell us what S is needed to limit q_{div}
  - But **key limiting factor** is glancing angle to tile, α, so $q_{div} \sim q_{||} \sin \alpha^*$

➔ Thus P_{SOL} B / N R is limiting divertor challenge metric
(but $P_{SOL}\ B_\theta / N\ R$ is relevant to design)

Ip drops out of q_{div} because poloidal field plays a role in divertor incidence angle as well as SOL width; and parallel flux expansion drops α

GASC Suggests Divertor Challenge is Tractable

- **4m 6T H=1.3 point yields modestly higher divertor challenge than ITER ➔**
  - **PB/R** parameter reflects latest Eich SOL scaling
  - **Need bit higher core radiation than ITER**
  - 9.5MA case lower that FASTRAN 11MA due to higher η's used

Potential to trade-off:
- Core radiation vs divertor mitigation
- Influences L-H access

P_fusion = 865 * 20% + P_aux = 81 ➔ 254 MW

P_rad = P_heat*38% = 95
P_brem = 39, P_sync = 6
} Total **55%** radiation

→ P_SOL = 113 MW cf 100 in ITER*

P / NR = 14 cf 16 in ITER
P.B / NR = 85 cf 85 in ITER

→ Lower η's forces higher fusion power, I_P & β_N (e.g. η_{th}=0.33 η_{CD}=0.25 → P_{fus}=1.3GW & **f_{rad}=70%**)

→ May need an even more radiative core or better divertor solution than ITER for AT reactor to avoid erosion.

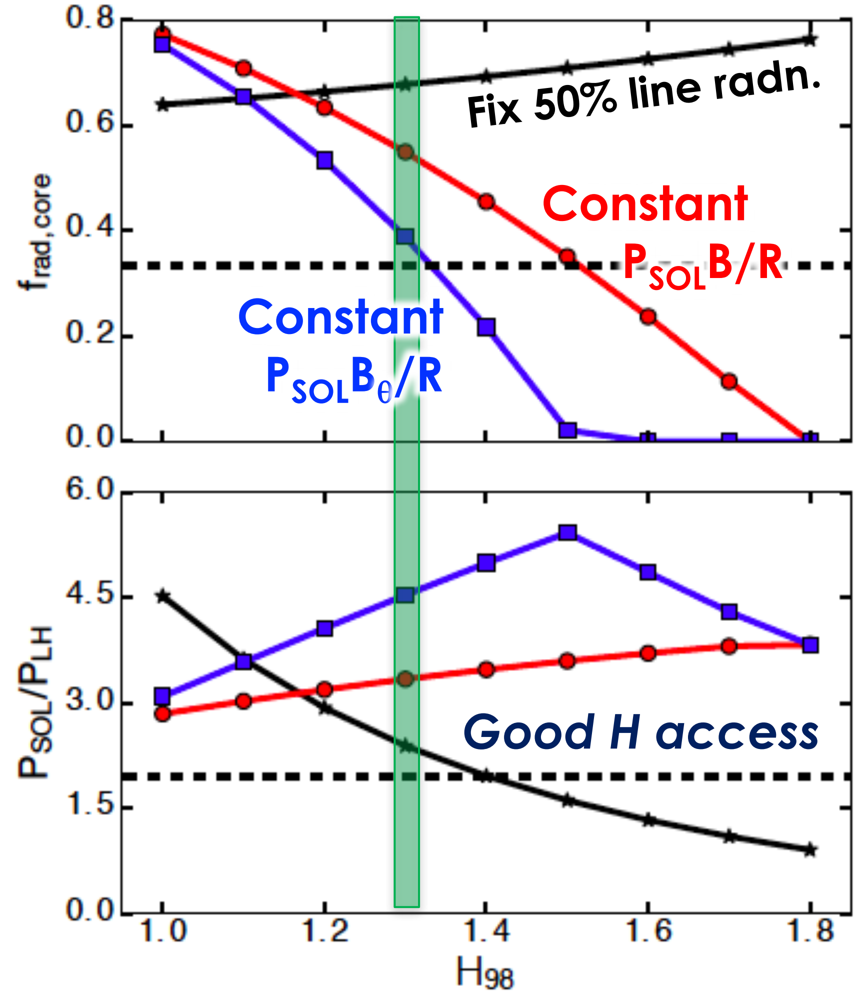
GASC **fully non-inductive** simulations at 4m, η_{th}=0.4 η_{CD}=0.4, f_{GW}=1.3, H_{98}=1.3, P_{el}=200MW

*Pitts, Phys. Scr. **T138** (2009) 014001

Divertor Challenge < ITER with Modest Radiation Rise & H-mode Access Still Maintained

- **ITER Q=10 divertor challenge:**
  - P_{SOL} = 100MW: **P_{SOL} B/R = 85**
- **To match ITER divertor challenge, decrease P_{SOL} by increasing core line radiation**
- **Further increasing radiation eases divertor challenge and maintains good H-mode access**
  - f_{rad} = 67%, f_{LH} = 2.5
  - PB/RN = 63, q_{div} = 7.3 MW/m²
- **Sizable headroom to increase P_{SOL} without melting divertor**
  - And N_W~2-3 MW/m²

Adjust core radiation to match…:



4m 6T η_{th}=0.4 n_{ped}/n_{GW}~1

Input from DIII-D Team Members (RJB highlights)

McKee (UW): Present path a ”dead end” not economical - we need to stimulate the science, innovation, and technological developments that will result in ***quantum leaps*** in performance, reliability, and robustness. Engage basic burning plasma science, materials sciences, reactor technologies with an **aggressive fusion engineering program** and experiment(s) (like DIII-D or other non-SC device) that can ***rapidly*** implement and test new science-based technological developments. Don't pursue DEMO as currently envisioned, a massive tokamak. **Factors of 3-5 improvement in MFE reactor size, reliability, and performance** metrics that translates to substantially lower realistic COE are required.

Holcomb (LLNL): the **vitality of the US fusion program depends strongly on having medium- to large-sized US experiments**. It would be a very bad idea to assume continued enthusiasm and involvement if US fusion researchers were expected to only work on foreign machines, at least until ITER enters the nuclear phase. Without continued opportunities at home, fusion groups at universities will continue to shrink and there will be fewer new people trained, or willing to stay in the field. I wouldn't be surprised if some people point to the high energy physics community in the US as a model for how it's ok to expect fusion to do less at home and more abroad. But I suspect there are significant differences between how work is done in these fields, and **in fusion there is actually an end product that we hope to exploit, which will require more experts at home than a few hundred phd physicists**.

Breisemeister (ORNL): put me down as one vote for: **Support ITER with a "First Plasmas First" mentality**. I don't see a viable alternative to really push Q past "break even" during my career. Extra resources can be put towards looking for a viable alternative, but first priority should given to keeping ITER on track for first plasma.

Guterl (ORAU): Hot walls may help to mitigate two critical issues regarding fuel retention, plus impact on neutrals.

Leonard (GA): The **boundary plasma solution (pedestal top to wall) represents a great (if not the greatest) challenge** and uncertainty in predicting its performance in future devices. Integrated solutions for the boundary cannot be demonstrated in existing scale devices as **dimensionless scaling is not possible**… [need] all the important physical processes [to be] separately validated in existing scale devices and implemented in an integrated model. Among the most difficult… are the interactions between the SOL and divertor plasma and the pedestal. **Validating will take carefully thought out experiments, diagnostics, modeling and other resources**. ITER offers the opportunity to test the coupling of Div/SOL and pedestal in regimes that are not available in existing scale devices, such as low pedestal fueling.

Eidietis (GA): (1) Many, many big questions remain as to the basic adequacy of the ITER baseline DMS. **Alternate DMS strategies must be pursued by the US** (whether you support ITER or not, tokamaks need DMS). However, with only 1/3 of previous devices operational, experimental capability for testing new DMS is extremely limited. The cost & timescale & tradeoffs of using DIII-D for technology demonstration is prohibitive. **US DOE should fund offline, dedicated test stands for DMS design & technology** demonstration (or use university scale projects as test stands) so DIII-D is only needed for final high-performance physics demonstration.

Input from DIII-D Team Members (RJB highlights)

Wilcox (ORNL): the **number one goal of the National Academies review might need to be simply to justify the existence of fusion energy research**. The funding trajectory seems to be turning a corner in a really bad way, and maybe this review is a chance to get vocal support for the top level goals of the program from the broader scientific community. Above all else, **we should be making the case** that:

-Fusion is needed as part of the future energy economy

-It is needed as soon as possible

-We are making good progress considering the resources we have...

-but the progress is extremely slow in absolute terms because of lack of funding

I imagine most presentations will take the form of "we've made some progress in XYZ, and this is super important, so we should expand in this direction". But without a full throated endorsement of the overall goals of our program, I'm afraid that any recommendations about what to do in/out of ITER may not be very helpful (**what can we really accomplish if we're not involved in ITER and we're down to a $230M budget??**).

Rhodes (UCLA): don't try to sell path as too cheap – raises expectations. (pararphrased).

Lohr (GA): Needs to put some Watts on the grid.

Schuster (LeHigh): There are two messages in your talk that I fully support. First, the need of developing a US "compact" DT facility, where "compact" must be a key characteristic of this facility. Second, the need of moving towards an energy-oriented research program. As a side comment, I believe that integrated control design and disruption avoidance must be part of any energy-oriented research program. I have noted that these words are not mentioned in your talk, not even once. Along these lines, I believe that "well-actuated" must be another key characteristic of this facility. We cannot afford building another under-actuated device.

Groebner (GA): My opinion is that the best (and possibly only) scenario for significantly improved funding of fusion research in the US is if the public were supporting it. As it is, I suspect that the general populace probably does not know about fusion or understand its merits. Perhaps more importantly, I am not sure they see a reason that we need to aggressively pursue it. With fracking in the US and low energy prices, there is no sense that we are in danger of running out of energy sources. The main motivation for developing new energy sources is to combat global warming and much of the public does get that. However, I suspect that they (and policy-makers) see solar as the solution. Scenarios that might make people think we need to include other alternatives might be: we go far enough down the solar path that the public starts to understand its limits or there are a few catastrophes that are linked to global warming and people become scared, or maybe something I cannot imagine.

Input from DIII-D Team Members (RJB highlights)

Holland (UCSD): I broadly agree with most of your message, specifically points (ii) and (iii); less on board with (i). I think the title of your third slide is about right- we should be aiming to establish the basis to proceed with a new burning plasma experiment *in the US* within ten years. I broadly agree with your message at the bottom of the slide, but also agree with Terry's and Rick's comments on the ASACAP vs ASAP. I would personally phrase it as "- we should take the steps needed to get to an economically competitive steady state reactor ASAP". I think it's important we identify a program and budget around that goal, and not build the program activities and goals around a cheap as possible budget. I also strongly support the modeling based approach you've taken in identifying the details of what this machine would look like. Modeling is one of the US's strongest areas, and I think we should be leaning on it (and pushing it) more.

Because I see rapid development of an economically competitive steady-state reactor as the most important goal for our program, I've become increasingly less supportive of the ITER program. Specifically, I'm skeptical it will be able to meet its scientific missions, and that even if it did, that it would be relevant to the goal stated above. But that's less important than the program reorientation you describe, which I support.

Marinoni (MIT): I totally agree on slide 16: we cannotdo basic science forever...once we build a reactor we are likely to findmany issue we had not thought about before (just like what happened tofission reactors).

Hollmann (UCSD): I support the main message of pushing toward a D-T confinement device in the US, I think we're getting left behind in the area of new machines. I'm not totally convinced yet on the 3:1 aspect ratio AT approach. Some of the advanced stellarator designs sound pretty attractive and now with new manufacturing techniques that are being developed, one of my main objections which is that building the coils is impossible, is starting to fade. It seems like the tokamak approach will be well represented in new machines (ITER, JT-60U, etc) so I think it is worth thinking about exploring the niche in between tokamaks and stellarators (NCSX type machines). Studies of current drive, ELM control, and disruption mitigation can be done on DIII-D.

Moyer (UCSD): we have a bipolar personality: science program or energy program. If we want to put fusion power on the grid in the next 20-50 years, we need to be an energy program with at least 2x and more likely 3-4x the current level of funding. But this is a tough sell right now: the US is the world's leading oil and natural gas producer right now thanks to tracking, we still have "tons" of coal, so it's not clear that an energy program that doesn't make an impact for 20-50 years is a non-starter. If we accept that to be that energy program we need 4x the funding for at least 20 years, then one has to ask, what could be done with that funding (about 1B$/yr for 20 years) right now - the cost of solar has fallen dramatically and PV capacity could be significantly expanded, or battery technology advanced. I had thought for years that global warming side-stepped the US energy abundance issue but right now, many citizens, and most of the government, don't believe that there's any issue in burning the fossil fuels that are left….so that argument doesn't play well right now either. But as a science program, we will continue to slowly die.

Chrystal (GA): I believe we should not underestimate how much can be learned by simply building a machine that has a new operating space. This is an experimental field after all.

Input from DIII-D Team Members (RJB highlights)

Lyons (ORISE): I would just note that there's a new threat to government funding of fusion research, at least new to me. There is a perception by some that because private companies are getting in on fusion research (e.g., Tri Alpha, Lockheed), the government should take a backseat in the research. While I can appreciate the perspective that the government should not pay for things that the market is willing to, I think it's vital to convey two related points: 1) All of these companies are pursuing designs with much smaller probabilities of success than the tokamak and 2) the tokamak requires financial support that the market cannot currently provide. My point, I guess, would be to motivate your path to fusion energy forcefully by emphasizing both the unique promises and (financial) needs of the tokamak.

Sweeney (ITER): success of the Q=10 mission requires solving a number of difficult problems that are not unique to the ITER design. Researchers with a given specialty can always point to an alternative machine that is better designed with respect to their concerns, however, it is unlikely that a single machine will satisfy the concerns of all researchers. ITER is at a disadvantage relative to the novel machines as it has been studied in great detail, and therefore the challenges are well known.

Chen (GA): We (in US) need real money, sufficient funding to make real and fast progress. But, honestly, not sure how this or other presentations could change the funding situation in US. The government nor the public in US feel the fusion energy as something urgent or really important. While in China (for example), the situation is quite different, it's like the golden years of fusion in US, the government and public give fusion a much higher attention and support. The funding is really good and the progress of fusion program (with the much international help) is really fast in China just like when there were a lot and rapid advance in fusion research back then in US with sufficient funding. Maybe not until US see their 'leading' position in this field is vanishing, will there be real money for US fusion program.

Science vs. energy program, I think that, especially with such limited funding and runtime (which is not just limited by funding, but also the machine ability itself), each machine should be focus on what it's more important or more efficient for making real progress. But I know, it's hard, if there is no science, then it's hard to get funding right now. If we could get such funding, we very much need *new machines* in US which provide, besides what you talked about (a faster and more economical path to fusion), more run time and better/different environments for test/development than just D3D and maybe NSTX-U provide. Speaking of *new machines*, I think that engineering/instruction is something equally important as other aspects you pointed out. Are and will there be enough good people/technique to build such new machines in US? The failures of NSTX-U don't look encouraging. And for *long pulse* operation, is the development required technique such as data collection and storage being considered to keep up with it?