The Need for a Fusion Science Integration Experiment in the US

R J Buttery, Feb 2009.

EURATOM/UKAEA Fusion, Culham Science Centre, Abingdon, Oxon, OX14 3DB, UK.

The world-wide development of fusion energy creates significant new opportunities of crucial strategic interest for the US fusion science program. The US has made fundamental and pioneering contributions to the field of plasma science and the quest for magnetic fusion energy. Because of this progress, and because of the new large facilities being prepared elsewhere around the world, it is now vital that the US embark on a substantial new development – in order to ***maintain a leading edge***, getting at ***new and different science*** that cannot be studied elsewhere, and ***filling substantial gaps in the world fusion programme*** that must be addressed if the goal of fusion power is to be achieved. It is obvious that a programme as large as the US', and with such comprehensive and leading expertise, should be making a leading contribution to fusion science by developing new capabilities in addition to extracting maximum benefit from existing facilities. Whilst imaginative and innovative developments on present devices may suggest interesting potential solutions, these must be brought to fruition by testing in new physics regimes, that approach and begin to integrate some of the elements required in a fusion power plant – conditions that are unobtainable in present US facilities. ***Thus a major new facility is critical if the high potential of the US programme is to be realized.***

Opportunities for the US

Considering the needs for fusion power research, and activities underway elsewhere, it is clear that major opportunities exist for the US to pursue a leading and unique role spanning a wide range of key areas. This will secure strategic interests for the US in the science and technology of fusion power – as well as non-trivial spin-offs for other major fields and industries. Whilst Asia moves towards superconducting non-nuclear machines, Europe is going to have to focus on making ITER work, and Europe/Japan may pursue materials testing via IFMIF. Although ITER itself is an essential step, and will make key contributions, a fusion power plant will have to address a number of additional issues, due to new phenomena expected or the need for fundamentally different solutions. These needs arise because of the differences in parameters and requirements of a power plant compared to ITER – as, for example, set out in the table in appendix 1. A power plant will have higher plasma energies and heat loads, higher neutron fluxes, and more energetic particles leading to different solutions for the divertor, PFCs and the core. It will also require, fully self-sustaining plasmas, full breeding, self-reconditioning, and quasi-continuous operation at performance levels where new instabilities may need to be controlled. These major gaps must be addressed with research in new and more capable facilities that complement what can be achieved with ITER.

This suggests need for an extensive complementary programme to ITER with new research required at power-plant-relevant parameters to address the further scientific challenges and technology of a burning plasma power plant. For example, the behaviour of steady state 'advanced tokamak' plasma scenarios is particularly important to understand in a dominantly self heated plasma – this is likely to be a much later part of ITER's mission, which in any case is only marginal to accessing the dominantly self-heated regime for such plasmas. Related to this, the viability of various current drive techniques needs to be evaluated at the levels required for power plant plasma control. Equally, it is vital to understand heat load issues and divertor physics – new materials, components and plasma-manipulation based heat load mitigation techniques must be tested, together with their interaction with the plasma scenarios. There are also many issues concerning the behaviour of the plasma wall and plasma facing components in the demanding regime of high neutron environment and quasi-continuous operation. Engineering research must include component nuclear testing, fuel breeding, extensive use of tritium and tritium handling, and high heat flux divertor solutions in a nuclear environment. Even simply developing the operating experience in a highly nuclear machine would bring invaluable experience, scientific advancement and technological innovation.

These needs suggest major gaps in the world programme that the US can fill. Many of the issues are areas where ITER will make vital first steps pointing to integrated approaches – but where the full development will require more extensive and comprehensive investigation. Development of capabilities in the US could take the field further, focus on specific needs, and try out innovative techniques, pushing particular aspects to enable work in the plasma regimes and parameters where solutions can be most rigorously developed and tested.

So, the US has a chance, and there is a clear need, to make a leap in these many crucial areas, which are at least as important as work already planned in the world programme.

The question is does this need a major new facility?

It is clear that imaginative and innovative development of existing facilities can go further in addressing many of the issues above. Energetic ions can be produced by various means as a proxy for fusion alphas. MHD instabilities can be examined – and probed with new coils. Materials and their impact on plasma behaviour can be tested. High heat fluxes can be focussed onto targets. Current drive physics can be developed. Sophisticated methods of plasma diagnosis, control and disruption avoidance/mitigation can be tested. Low torque operation can be explored. Many of these research tools can and should play a role in helping prepare the way for development of solutions at fusion relevant parameters – they are exciting things to explore, and we can try them out efficiently and flexibly with existing devices.

But, the key point is that approaches based on existing US devices cannot address the issues in an integrated way at fusion relevant parameters, or expose the potential solutions to the plasma regimes and challenging environment expected in a fusion power plant. *They won't bridge the gap to DEMO.* In a power plant the challenges will be greater, or require fundamentally different solutions, because of the very different parameter range (even relative to ITER), and because of the requirements of quasi-continuous operation. For example:

- Simply dealing with the high heat loads will be extremely challenging, with DEMO P/R ~5 times ITER levels. This will require a combination of methods based on development and evaluation of new plasma facing components, manipulation of the plasma itself, coupled with precisely controlled operation close to performance limits. Capability to study this interaction at the 10MW/m^2 level, and go further to explore the science of more advanced materials science or plasma based mitigation techniques in needed.
- The high fast particle pressures required may access new classes of Alvénic instabilities in the non-linear regime, which need to be understood. Alpha particles in DEMO may account for ~30% or more of the total energy, and so the interaction of near-isotropic energetic particles with the plasma needs to be studied at these levels.

- Understanding how machine conditions evolve during quasi-continuous steady state operation, with issues such the potential build up of deposition layers, erosion, and simply maintaining good levels of plasma purity is a crucial aspect to study. To really address this, a device needs to operate for timescales of the order of weeks rather than hours.
- Understanding plasma materials science and resilience of components in a high neutron environment is a key issue. Power plant scale devices need to address neutron loads of 10MWyr/m^2, corresponding to 100dpa, although substantial progress on the science could be made in intermediate devices above 1MWyr/m^2 or 10dpa.
- Some elements, such as tritium breeding, simply cannot be explored in present magnetic confinement devices, and need to be understood in a realistic fusion device environment to resolve lifetime issues, cooling, reliability, etc.

The resolution of such questions requires major steps towards power plant parameters. But in addition to this, new phenomena will also come about from the combination of effects such as high fast particle content, high surface heat flux, high component irradiation and the need to operate with a high non-inductive current drive fraction (to pick four issues). For instance,

- Fast particle instabilities may redistribute current or decrease self-heating, requiring careful optimisation of the plasma scenarios and current drive, to identify a mutually consistent solution in a self heating regime. Here access to high β_N, ~3-6, as well as high β_α~3% is required to understand drives and damping, together with the interaction and optimisation of bulk plasma current drive methods (tens of MAmp, ~hundred MW level).
- Development of an acceptable divertor solution may place significant constraints on core plasma scenario design – particularly in terms of radiative power and edge temperatures.
- Irradiation may change heat bearing properties of plasma facing materials, requiring careful study of the materials science, component behaviour, and overall device layout.
- Impurity influxes from PFCs may impact core plasma performance, requiring additional control tools and/or choice of materials and components. Operational experience with a ‘hot wall’ (hundreds ^{o}C) would be important here, to understand the physics of the plasma-wall interaction, the potential for control, and the requirements placed on the wall, including its composition and temperature.

It is therefore clear that to make substantial progress on the resolution of these new scientific challenges at the power plant scale, one needs to take a major step in capability. For the development of viable power plant plasma regimes, it is necessary to embark on self consistent studies such that plasma scenario design is optimised consistent with tolerable edge requirements, while core plasma configuration remains consistent with the impact and limits imposed by fast particle effects, and plasma performance and stability is maintained. Suitable control must be demonstrated while plasmas remain largely self driven, and operated close to performance limits. Fundamentally the solutions need to be developed in realistic power plant conditions, in order to understand and optimise the interaction between hardware, subject to all the challenges of a fusion environment, and a plasma that is in a self-sustaining, controlled high performance state at “approaching-power-plant” parameters and physics regimes.

In order to get at these issues one needs to bridge significant gaps in parameters on the path to DEMO. One such gap is the burning plasma nuclear environment – fast particles and neutrons. Others include plasma temperature, stored energy, and heating/current drive power. There are of course many others. The choice of which gaps are most important to bridge should result from an assessment of which are the most important strategic areas for the US to address – something to come out of this ReNew process. However, it is clear that some form of new Fusion Science Integration Experiment, that closes some of these gaps to move beyond present and planned capabilities is needed, to make transformative discoveries in fusion science that pioneer power plant solutions, rather than just incremental developments in understanding and preliminary testing of promising ideas. This leap in capability will allow the US to maintain scientific leadership in fusion by carving out a new and unique role, securing a key strategic interest.

<u>The crucial point is that the US has the potential and the opportunity to take a major step to new fusion plasma regimes</u>; it should not turn away from this in order to do things that can and will be similarly studied elsewhere at a modest level – even if the US could do such work in a more innovative or complementary way.

Given this need, how do we determine the specification of a such a facility?

The process must start with a rigorous assessment of the critical questions in each field – what aspects need major issues to be address for a fusion power plant? In each area it must be identified what can be done on existing devices – and what requires fundamental (but achievable) steps in capability? Then one must review which areas are most in need of such fundamental steps – areas where opportunities for contribution from other parts of the world are limited, where excellence in US expertise leaves you optimally placed to exploit the new capability, where the development of capability is possible to start now.

Many of these aspects are dealt with by the excellent Greenwald report, and other recent documents cited in this process. ***The important opportunity the ReNew process brings is to now review and prioritise these issues against possibilities*** for small and major developments in US capability, and ***<u>to bring the community to a common view</u>***.

A key element is to work towards a consensus ***by considering strictly on the basis of physics*** requirements for fusion power – starting from this, rather than what a particular new facility can do, or what a particular path requires. The ultimate outcome of this process is far from clear. It might be one of the existing three rival proposals. Or it might involve compromises or evolutions in the design of one or more of these, or even something different. The point is that one cannot know this answer, until one has gone through this process of identifying the physics issues with the whole community, looking at how they can be addressed, and discussing which items are most important for the US to address at the larger scale. That is why this author has not identified a solution. And the elements discussed here are but examples of the underlying issues that should be considered (though I hope perhaps some of the foremost considerations). We should go through the physics discussion first, before we try to arrive at a solution.

However, *it seems clear to this author, that the answer for the US fusion programme, is that its further development must include a new Fusion Science Integration Experiment that moves beyond present devices, advancing the outstanding issues of fusion power, and securing new unique areas in which the US can lay claim to be pioneering the science and technology. <u>I hope that an early consensus can be reached that the US needs to take this crucial next step</u>, and that its specification should be an outcome of a transparent debate through the ReNew process based on what the <u>physics priorities</u> are to resolve the approach for fusion power.*

Appendix 1:

Table from US BPO document "Planning for U.S. Fusion Community Participation in the ITER Program; June 7, 2006"

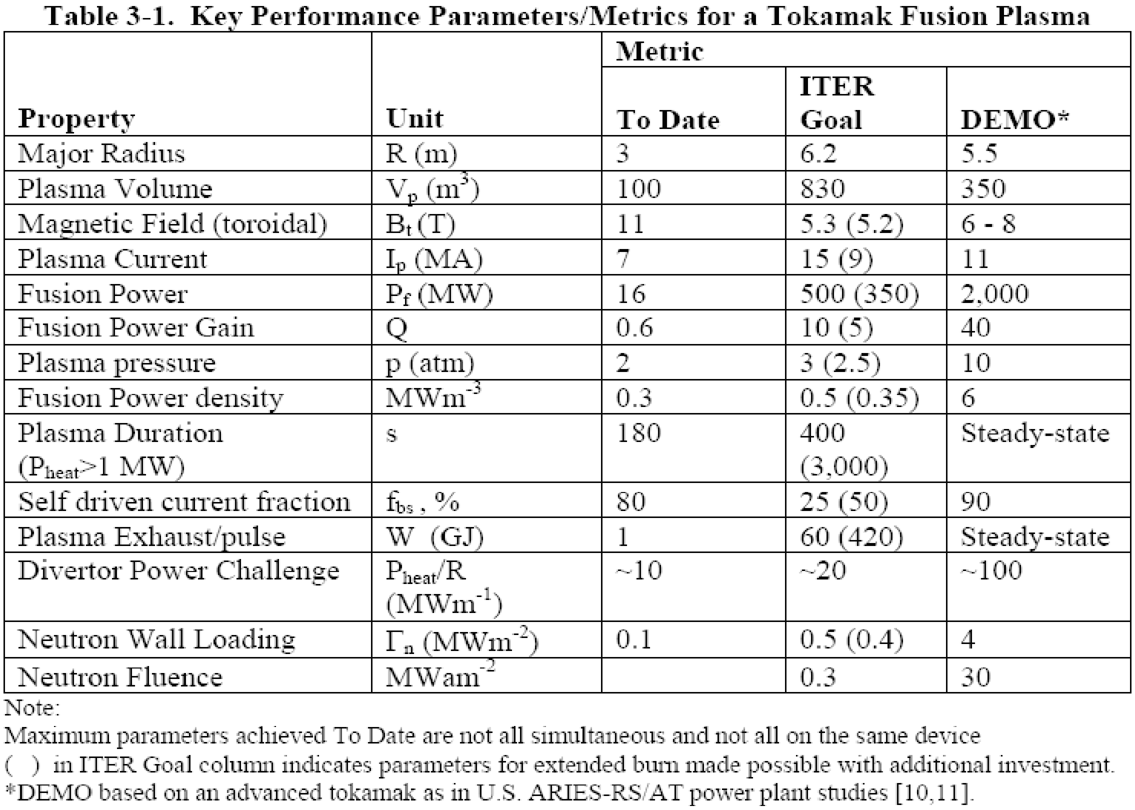

Table 3-1. Key Performance Parameters/Metrics for a Tokamak Fusion Plasma

		Metric		
Property	**Unit**	**To Date**	**ITER Goal**	**DEMO***
Major Radius	R (m)	3	6.2	5.5
Plasma Volume	V_p (m^3)	100	830	350
Magnetic Field (toroidal)	B_t (T)	11	5.3 (5.2)	6 - 8
Plasma Current	I_p (MA)	7	15 (9)	11
Fusion Power	P_f (MW)	16	500 (350)	2,000
Fusion Power Gain	Q	0.6	10 (5)	40
Plasma pressure	p (atm)	2	3 (2.5)	10
Fusion Power density	MWm^{-3}	0.3	0.5 (0.35)	6
Plasma Duration (P_{heat}>1 MW)	s	180	400 (3,000)	Steady-state
Self driven current fraction	f_{bs} , %	80	25 (50)	90
Plasma Exhaust/pulse	W (GJ)	1	60 (420)	Steady-state
Divertor Power Challenge	P_{heat}/R (MWm^{-1})	~10	~20	~100
Neutron Wall Loading	Γ_n (MWm^{-2})	0.1	0.5 (0.4)	4
Neutron Fluence	$MWam^{-2}$		0.3	30

Note:
Maximum parameters achieved To Date are not all simultaneous and not all on the same device
( ) in ITER Goal column indicates parameters for extended burn made possible with additional investment.
*DEMO based on an advanced tokamak as in U.S. ARIES-RS/AT power plant studies [10,11].