FROM YEARNING TO BURNING

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Possible broad-brush guidelines for "burning plasma" thinking
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The "yearn to burn" is well motivated. Most of us came into the fusion program with the dream of fusion energy. The dream persists. The US fusion program is now in a phase of broadening its science base. This is exciting and will certainly be profitable in terms of increasing chances of eventual success. Nonetheless we ultimately judge ourselves and are judged by others in terms of progress towards the fusion goal, both in understanding and in performance. From many talks with physics colleagues, as well as with students thinking about entering the field, I sense that the lack of performance progress and prospects in recent years has been a big factor in lowering our image to the external world.

As we have realized for many years, the point at which science and the fusion energy goal converge is in a burning plasma experiment. It is there that we confront the unresolved issues of transport scaling, self-heating, burn control, and alpha physics, and also demonstrate that fusion energy is more than a fantasy. So let me begin from that point and suggest how we should approach the problem of a Next Step by setting down a few principles for discussion.

1-Is there a consensus that we know enough to propose a sensible experiment and at the same time that our knowledge is sufficiently imperfect that such an experiment is needed now? The Fermi paradigm that a good scientific experiment is one with a 50% chance of success may apply here, although for such a major venture the bar should no doubt be somewhat higher, at least for meaningful partial success.

2-We can imagine many possible experiments covering a wide range of goals and costs. Generally there is of course an inverse correlation. A maximal experiment is ITER-FEAT whose fate is now under discussion. If it gets built we should try to get back in the act. If not, we must downscale our objectives and examine the range of lesser possibilities. We should be careful here to recall that "the better may be the enemy of the good" and that we almost certainly can only afford a multinational experiment. In view of past history and present and likely future budgetary climates here and abroad, it seems prudent to look for the least costly experiment which has a high probability of success, both in answering the most critical science issues and in serving to convince the world that fusion is a scientific possibility. Thus a key question I would pose is: What are the truly essential science issues to be resolved? A limited experiment won't be able to resolve all issues, but hopefully it will extend the data base to the point that together with theory and other present and planned experiments , fairly confident science projections to a reactor will be feasible.

3-There are some obvious requirements. There seems to be general agreement that. a Q of 10 for a few energy confinement times is needed to qualify as a convincing burning plasma experiment. Adequate heating, fuelling, and especially diagnostics are essential, as well as credible engineering. Flexibility to explore different confinement scenarios, and adequate power (including Ohmic) for extensive experiments with H or D are highly desirable. At this time it would appear that only the Tokamak is mature enough to qualify for a burning Next Step, although we may well find out in the next several years that the ST is also
.well suited for it.

4-In trying to home in on the key issues to be resolved, there are others which must be compromised to a great extent. The first of these is long pulse behavior. ITER-EDA has explored thoroughly the superconducting magnet design needed for long pulse burning and nuclear technology studies. There is evidently a huge cost saving in going to an inertial Cu high field machine with limited pulse length. Other experiments such as KSTAR will be exploring long pulse issues, and such limited evidence as exists suggests that once a discharge has been established, its disruptivity in late flat top stages decreases radically so that very long pulse physics issues may be secondary. Still, the issue of what pulse is long enough needs to be quantified. Confinement steady state, alpha slowdown, limited information on He buildup and diffusion, and some understanding of current evolution are issues determining pulse length desirability.

5-As the heart of my discussion I list 2 basic science questions where our present uncertainties really impact our ability to plan the optimal future path. Whether these indeed are the crucial issues to be focussed on should be a primary question for a burning plasma study. Any assessment should begin with agreement on the irreducible objectives. I think the 2 key issues are:

A-How does transport scale with size (rho*) as we approach reactor scale? We can expect much progress in theory and simulation over the next years but the problem is so complex that a benchmark at relevant size is surely required.

B-What effect will a high alpha population and self- heating have? New modes (TAE,EPM) as well as effects on such MHD phenomena as sawteeth and neoclassical modes need to be studied. We are very short on experiments and nonlinear theory is still rudimentary. Here is the core of “burning plasma physics”.

6-A decision on whether a divertor is necessary could have a big impact on cost. This seems indicated by cost comparisons between Ignitor and Fire designs. A higher current (and thus plausibly better confinement) can be obtained if the chamber is fully utilized, and difficult disruption engineering problems with shaping coils are avoided with limiter discharges. Of course with a divertor there is more reactor realism, longer pulses are possible without He extinction, and H-modes are easier. We need to study in the next few years other enhanced confinement modes such as those observed with peaked profiles in high field machines. This suggests CMod experiments to supplement those underway on FTU in support of Ignitor .In accordance with the minimal cost-limited objectives philosophy I am suggesting, the non-diverted option with its modified boundary physics must be seriously considered. It may be a large cost reducer.

7-Finally some words on “generics”. First it should be noted that the optimal running condition for the high field machines at reasonable wall loads is likely to be at fairly low beta, although there is of course some flexibility in extending the range at lower fields. This mitigation of MHD effects can be viewed either as an enhancement of success prospects, or a lack of relevance. With the philosophy of minimal cost and risk in pursuit of the 2 key objectives, low beta appears to be a plus. The ability of the high field machines to explore “Advanced” Tokamak regimes needs further assessment. On the one hand it does not appear difficult to freeze in reversed or $q>1$ profiles. On the other hand any precise current profile control will be very doubtful although perhaps not needed at low beta.

8-As far as applicability to other configurations, a strong case can only be made with regard to the toroidal, strong external field concepts, but these seem now the most promising ones. Smaller scale experimental results and simulations normalized by experience with the burning plasma experiment would probably provide a high level of understanding for Tokamak variants such as STs. Stellarators are more problematic. While transport appears similar to that in Tokamaks the evidence and diagnostics are as yet meager. It seems plausible that the same microturbulence is responsible for transport, with similar statistics and scaling. The very limited theoretical looks at alpha effects also indicate that the detailed magnetic structure is not too critical for their behavior. This would particularly apply to quasi-axisymmetric compact designs. In short one can expect considerable generic applicability with the help of simulations, but until more is known about stellarators it will be hard to be sure. Applicability of results from a Tokamak burning experiment to other concepts such as RFPs and FRCs where other phenomena may be dominant is still more speculative. It is likely that we are many, many years away from being able to consider seriously a non-Tokamak burning plasma experiment (except possibly for STs).

Let's move expeditiously from Yearning to Learning!