Wurden White Paper-2 **Risk Reduction** **July 19, 2012**

A long-term fusion R&D program needs to address "show-stopper" issues as soon as possible in a development path. For example, if adequate current drive efficiency and absolute control of disruptions are required for the success of a long-pulse large-size tokamak DEMO, these issues need to be tackled at the smallest possible scale. Making a $23B investment (in ITER), while counting on solving the unproven issue of being able to control/eliminate disruption forces, radiation, and runaways (simultaneously, with extremely high success rates), to protect the operational integrity of the machine, is not a wise or a conservative approach. Making this investment, while essentially eliminating research in alternatives that don't suffer these problems, is also not the action of a prudent investor.

We are not in a crash program "to demonstrate fusion". We are in a very long-term program, with multiple issues that need to be worked in parallel. By having nearly eliminated our "technology development" efforts along with our "alternates" efforts, we have greatly increased the risk of going down a path with a bad outcome in the end. Even the "known knowns" for tokamaks don't look attractive; let alone the other unknowns (to paraphrase a former Defense Secretary).

What if there are no magic materials that will survive the kind of solid wall dpa damage that an economical fusion breeding blanket needs to take? What if the "right" answer for materials damage problems is a thick liquid wall? Then we should be studying fusion concept approaches that are compatible with thick liquid walls. What if driving the plasma current in a long-pulse machine is simply not efficient enough to keep the recirculating power fraction down? Then doesn't it make sense to look at alternatives that don't have any externally driven plasma currents? And ones that don't have this source of current-driven instabilities as well?

Germany has a major investment in alternates. Japan has a major investment in alternates. China has many parallel approaches under development. We should not be left with a program that is effectively ITER and nothing else. We should not plan a program without significant risk reduction efforts. And I mean, risk reduction approaches that are as orthogonal to the tokamak as possible, in key areas that are known to be tokamak weaknesses. Our program can be a world leader in alternates, once again.

What does this mean for the US Fusion Energy Sciences program in the next 5-10 years?

1). We should expand the stellarator program in the USA. One of the three tokamak facilities should be given the opportunity to make a systematic transition towards a mid-sized stellarator (instead of their present tokamak), as a result of a directed competition. This would be the next new US machine.

2). We should restart reactor design modeling efforts, that really look at simultaneous issues of RAMI, blanket replacement, complexity/simplicity, and tritium breeding (not just hypothetical, but can you really get the ratios you need in the presence of other demands on actual in- vessel real estate).

3). I suggest a reasonable split of resources is 30% on ITER, 30% on tokamak (which includes ST and AT R&D), and 30% on alternates. The other 10% can go to basic plasma science and HEDLP, which unfortunately aren't being discussed here today.