Wurden/Evans/Parks White Paper-3: **Leading Fusion Research via Alternative Pathways** 8/8/2012

The USA has clearly ceded the lead in tokamak research to other countries in the last 15 years. Our present (30 year old) copper machines, while workhorses with excellent diagnostic sets and research teams, epitomize the peak of research capabilities possible in the line of short-pulse tokamaks. They have a limited research life ahead of them. Furthermore, even though the NSTX-Upgrade is welcomed (by most people), it is still is only an intermediate facility in the tokamak line. Given our present state of knowledge about long-pulse issues, disruptions and ELMS, blanket issues, and the cost of DT tokamaks, we might as well recognize that FNSF is not only premature but carries substantial financial and technical risks if undertaken before successfully operating burning plasmas in ITER. It doesn't make sense for the USA to embark on this path before ITER is shown to be successful.

Where can (and does) the USA lead in magnetic fusion research? In theory, computation, and simulation, without a doubt. Another answer is in (call it what you will) "alternates" at the concept exploration and proof of principle stage of development. Both of these areas have the advantage of big returns for small investments. But both of these areas have faced heavy reductions in recent years. The Fusion Simulation Project is presently idling at best. The advantage of pursuing approaches that don't face the known limitations of tokamaks, are that outstanding science can still be done at the $5M/year level of effort, while making high-risk, high-long-term payoffs possible. We could easily envision a diverse spectrum of such efforts, spread throughout the country, even with our present financial limitations. A US program built on a portfolio of several small "advanced concept" plasma physics devices tied together with an intermediate sized state-of-the-art 3D magnetic confinement device would provide a unique strategic platform for the base program regardless of the outcome of the ITER project.

But to do this, in flat or declining budgets requires the planned and orderly closure of Alcator C-Mod and DIII-D experiments, while simultaneously transitioning the research teams to other, new efforts, whether within the USA or international. It also requires looking very carefully at the wisdom of NSTX-U versus (for example) a renewed stellarator effort at PPPL. This transition should be well planned and carefully executed to: 1) maintain strong support for a lead US role in one or more key ITER physics areas, 2) maintain continuity in providing critical high-temperature plasma data with which to validate advanced numerical codes and theories and 3) provide vibrant, cutting-edge, experiments that will facilitate quid-pro-quo international scientific exchanges.

(I) The world fusion community (rightly or wrongly) believes that ITER is the only device capable of accessing a stable, steadily burning plasma state: the cutting edge frontier of high-temperature plasma physics; a supreme technical challenge.

(II) It is unacceptable to put forth a scientific fallacy that our existing large rho-star number, non-burning, copper tokamaks in the U.S. can somehow "look beyond" ITER, or "circumvent" ITER to produce meaningful scientific information about a Demo fusion plasma state twenty years off. The secondary argument that we need to continue operating these machines to sustain "the workforce" and attract/train young scientists for ITER is also out of step with reality. Scientific research drives the graduate workforce, not the other way around. Other training platforms are possible. When ITER is ready "they will be attracted and they will come": It only takes 5 years to get a PhD.

(III) The US should use the knowledge gained in ITER to establish a path forward beyond ITER. Begin to do this now by strengthening its predictive understanding, using theory, simulation and integrated modeling programs, as the most cost effective way to participate in ITER. Here the U.S. can sustain its world-class status in the fusion arena for decades to come, despite a lackluster investment in fusion energy science.

(IV) The US fusion program must invest in, and investigate within the peer review process, high-gain, non-toroidal fusion approaches that use magnetic fields such as the MTF and MagLIF pulsed power options, as well as foreign tokamak and upgraded devices coming on line. The most attractive fusion pathways have always had elements of high density, high beta, and high magnetic fields. Jim Tuck recognized this in the 1960's, and it is still true today.