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"Be Bold"

An Alternative Plan for Fusion Research

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Government sponsored magnetic fusion energy research in the USA has been on downward trajectory since the early 1990's. The present path is unsustainable. Indeed, our research community and national research facilities are withering from old-age and lack of investment. The present product (tokamak-centric production of electricity) does not yet work, will not be economic, and is clearly not valued or needed by our society. Even if a prototype existed at any cost, DT-based fusion energy would come too late to significantly impact the reduction of CO2 emissions in this century. This white paper outlines what "being bold" could mean with respect to the invention and application of nuclear fusion technologies, and how the USA could once again set a visionary example for the world. I present the discussion in two parts, reflecting on the NAS panel two-part assignment of a plan "with" and "without" ITER.

The present path "even with ITER" does not have a desirable 20 year timeline for US fusion research. We are lacking not only new domestic construction, but also new plans for future facilities, while existing ones are >20+ years old. We should have a pipeline of facilities, coming online every 10 years (or better yet, even in parallel), at a minimum. Our university-based infrastructure (people pipeline) is also not being sustained, and likewise suffers from stagnation and shrinkage. The existing US fusion program needs a complete reboot…with a new mission, a new home, new leadership, and new vitality. We don't need another strategic planning report (there are plenty of those on our bookshelves over the last twenty years) that will be simply filed away.

Let's consider the present pathway "with ITER". Many people are frustrated with the increasing costs of US ITER participation, and with the slippage in ITER schedule. They wonder if ITER will ever be completed due to the complexity of working together internationally, and for all of the difficult political issues. However, my concern is that ITER will never work, even when built, as designed. The reason is that the tokamak has a fatal flaw in the form of sudden loss of the plasma configuration, …disruption of the plasma current… which, on the scale of a machine as large as ITER, will lead to severe damage of the machine components [1]. I was one of the scientists that raised my hand to go ahead with re-engaging in ITER as a next-step in the quest for making and studying burning plasmas at the Snowmass meeting in 2002. That was a mistake. We should have first solved the tokamak disruption problem (which has still not yet been eliminated in any tokamak on the planet).

Disruptions in ITER at currents of 6 MA or greater, have the serious likelihood of creating runaway electron beams which will cause water leaks in the armor in exactly one event. The ITER 2004-2007 Design Description document (DDD 16) for the first wall armor analysis (section 2.4), considers what the impact of 10 MA (20 MJ) of runaway electrons might have on ITER. For the case of 10 mm thick Be armor, 2 mm of said armor was calculated to melt from 50 MJ/m^2 runaway electron events, (runaways with 12.5 MeV exponential energy distribution), while the temperature max (526 °C) at the Cu/Cr/Zr bonding layer to the stainless heat sink in the armor is still within limits. DDD 16 suggests that the armor will survive for 5-15 such "rare" events, based on the expected "statistical distribution of the event location on the event location on the plasma chamber surface". However, since then (to handle higher steady-state heat loads) the armor design thickness has been reduced to 8 mm, and, more importantly, it is now widely recognized that the assumed 20 MJ of initial energy content of the runaways must be increased (over a short time) by a factor 2-3x, due to the conversion of poloidal field energy into more runaways, as the current decays. This means ITER's own calculation predicts 4-6 mm of melt is likely in a single unmitigated event…..for armor that is only 8 mm thick…which strongly suggests within engineering error bars that water leaks and debonded armor can occur in one shot. ITER (optimistically) states that it could repair leaking armor with a two month turn-around time, which implies a maximum of six water leaks per run-year. Another less delicate way of putting it, is that ITER will become the most powerful uncontrolled e-beam welding device ever created on Earth.

While the US has responsibility for providing disruption mitigation system(s), the fact is that simultaneously preventing (every time) the three problems that disruptions cause (large force loads, large thermal radiation loads, and multi-MeV runaway electrons) has not been demonstrated simultaneously in today's tokamaks. In fact mitigation efforts against one of the problems, can make the other two problems worse. After a series of unplanned and unwanted water leaks over the course of a few years, who would ever allow tritium to be put into the machine? Hence ITER is destined to fail in its burning plasma mission. We can find better ideas than spending 20 years of our lives, and (collectively) $20 billion of our gold, on a machine that is easily foreseen to fail in its mission. Had ITER been one of multiple paths forward, perhaps it would be a tolerable learning and engineering exercise. However, as the "only way" forward for the USA, it is most certainly not. The best presently demonstrated and guaranteed solution to the tokamak disruption problem, is to not build a tokamak.

To my way of thinking, the USA should, and could, welcome and embrace a strategic fusion research plan "without ITER". In fact, we can even see what that might look like, courtesy of private research investments in fusion. I can specifically point to the significant, fast-paced, privately funded research efforts exemplified by TriAlpha Energy, General Fusion, and a few other smaller companies around the world. We need facilities to study alternative, more stable, simpler to control, and hopefully higher beta plasma systems. The compact stellarator and linear mirror gas dynamic traps are two that come to mind. The USA needs a diverse fusion research portfolio. Multiple national reports have indicated this over and over again, through the years, and yet it is the first thing that has been forgotten in times of budget austerity.

To be even more bold….indeed, I would even venture to say this is the boldest, grandest, (almost literally in the category of "the-sky-is-falling") research idea that the NAS committee will hear this year, a group of us have recently proposed a completely new (non-electricity) goal for fusion energy research. In a 2016 paper, by G. A. Wurden, T. E. Weber, P. J. Turchi, P. B. Parks, T. E. Evans, S. A. Cohen, J. T. Cassibry, and E. M. Campbell, entitled "A New Vision for Fusion Energy Research: Fusion Rocket Engines for Planetary Defense", J. of

Fusion Energy (2016) 35: 123. (https://link.springer.com/article/10.1007/s10894-015-0034-1), we suggest that it is essential for the fusion energy program to identify an imagination-capturing critical mission by developing a unique product which could command the marketplace. The product known as "electricity" is not such an item, because it can be produced with too many other, simpler, cheaper, functioning, technologies. We lay out the logic that this product could instead be a fusion rocket engine, to enable a rapid response capable of deflecting an incoming comet, to prevent its impact on the planet Earth, in defense of our population, infrastructure, and civilization. As a side benefit, deep space solar system exploration, with greater speed and orders-of-magnitude greater payload mass would also be possible.

The problem with long period comets is that they basically arrive to the inner solar system unannounced. If we are looking, we can pick them up at Jupiter to Saturn distances (when they first begin to warm and get brighter in the infrared). If one were on a collision course with Earth, we would only have 6-18 months of warning. To make a long-distance deflection of the comet, with momentum change delivered by ablation caused by radiation from a stand-off nuclear explosive, you need a fly-by interceptor rocket with 20-40x the performance of our best existing chemical rockets to get out to 5-10 AU intercept distances in only 6-12 months time, with a heavy payload and without gravity slingshot assist. Both large specific power, and high specific velocity are required from this rocket engine. Only one based on fusion could potentially do the mission. Interestingly, the performance metrics and design constraints for a fusion rocket engine are quite different than for the usual Demo fusion reactor. We should get started on this key technology, before we actually find ourselves needing it.

Fusion research should become part of an international planetary defense program, which focuses on three items. 1) Developing better asteroid/comet detection systems, 2) Testing existing deflection technologies against asteroids/comets that aren't going to hit us, and 3) Building fusion rocket engines to enable a quick response. Imagine for a moment the engineering differences associated with a D-Helium3 direct drive fusion rocket engine used for planetary defense: It would carry its own fuel (no Tritium extraction from a lithium blanket… in fact, no blanket, just some magnet shielding), no vacuum vessel, no vacuum pumps, and the mission is over before neutron damage becomes a problem! The chief performance metric would be power/unit mass, no longer cents/kilowatt-hour. How much would/could NASA and ESA and the world pay for a working fusion rocket engine? Quite a bit! And finally, it would be easy to recruit a workforce that would be energized by such an enduring and exciting mission.

[1] G. A. Wurden, "Dealing with the Risk and Consequences of Disruptions in Large Tokamaks", MFE Roadmapping in the ITER Era, Princeton, Sept 9, 2011 http://wsx.lanl.gov/Disruptions/Disruption-Risk-poster-Wurden-LAUR-11-11465.pdf

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