**Comments on**

**The Interim Report of the Committee on**

**A Strategic Plan for US Burning-Plasma Research**

S.A. Cohen, February 1, 2018

**Overall**

Based on the membership of the committee, their diverse backgrounds in plasma physics, and their earlier support of and research into alternate concepts for magnetic fusion reactors, it was expected that the report would present an educated and more visionary perspective on burning plasmas and fusion power. Yet, in spite of the **decades of delays**, **10’s of B$ in cost overruns**, broken promises, and continuing inaccurate forecasts in the tokamak program, this report supports a plan of “more-of-the-same,” the Titanic holding a steady course.

This report presents as it central theme and *highest priority*, “keep supporting ITER.” In the past, DOE interpreted the phrase *the highest priority* to mean *the sole activity*. Over the last several decades, support for alternative concepts – mirror machines, stellarators, dense plasma foci, Z-pinches, spheromaks, and FRCs – has been so little that failure was guaranteed. One must recall the greatest advances in tokamaks, in the years 1970-2000, were made when there were many tokamaks operating worldwide, even in the US alone. It is inconceivable that the meager support given to isolated alternate-concept experiments could have produced program-shifting results.

The report hardly recognizes the sizeable venture capital now being supplied to alternate concepts. Are the VCs wrong and DOE right or perhaps it is *vice-versa*?

The report should be re-written to recognize and promote alternate-concept paths to burning plasmas and fusion energy. Alternate concepts should be apportioned a fixed percentage of plasma physics funding with the goal of achieving burning plasmas in 10 years, *i.e.,* ahead of ITER. Fifty percent of the DOE plasma physics budget is about the right level for alternate concepts, not what is left over after tokamaks have feasted on what is on the table. When apportioning the fusion funding, all US tokamak efforts, including ITER and (other) foreign collaborations, should be funded from the same 50% pot. The question will then be what fraction goes to each alternate concept.

**Detailed comments on and criticisms of Summary**

**Assessment 1.**

1. A burning plasma need not be primarily heated by the fusion products. If so, D-T is a bad choice for a fusion fuel because 80% of all its fusion energy escapes the plasma as neutrons. Efficient energy recovery, energy recirculation, and heating methods can avoid the need for direct plasma heating by fusion products and the resulting instabilities, reduction in fuel beta, and ash build-up issues D-T tokamaks face.

2. Why is only **one** burning plasma experiment implied by this section? The assertion that burning-plasma devices need be large and complex, hence very expensive, is unfounded. It is based on 40-year-old tokamak models.

3. Requiring *advanced industrial capabilities* increases the cost and complexity of a device and could delay implementation of fusion reactors. Burning plasma experiments – and their successors – should be designed around current industrial capabilities.

**Assessment 2.**

1. The implication of this section is that the domestic tokamak program has been productive over the last decade. This is not true when compared to the advances made in the previous three decades. One major US tokamak has been inactive for 7 of the last 10 years; one was shuttered two years ago. The question whether the US tokamaks have been cost effective must be answered “no.” Have the C-mod and NSTX tokamak results affected the design and operation of ITER, the basis for considering “cost effective?” No. Perhaps D-IIID will affect ITER’s operations, but how do their results compare with those of JET, JT-60, EAST, and the other 30+ tokamaks now operating worldwide?

2. This assessment oversells the advances in tokamak research that have occurred. It asserts that “Scenarios of burning plasma operation that are expected to simultaneously satisfy the requirements for stability, confinement, fuel purity, and compatibility with plasma facing components have been developed experimentally and explored with computational models.” One need only look at the scatter plot of energy confinement times used to justify H-mode scaling to see the ±40% uncertainty in the experimental data. Would electrical utilities accept such an uncertainty in power production? Another example of cherry picking is: recently it has been touted that advanced SOL *modeling* has predicted for ITER a wider scrape-off layer – hence lower peak heat loads on its divertors – than previously expected. Yet last year that *previous expectation*, based on experiment and theory, was awarded the most “outstanding paper” in *Nuclear Fusion* for 2012. Which is right?

**Assessment 3.**

1. I agree with the statement in this section that ITER is “a long way from being a commercial power plant.”

2. I also strongly agree with the last sentence in this section, that “other fusion energy experiments will need to address remaining science and technology challenges and demonstrate innovative solutions that lead to a reduced size, lower cost, full-scale power source.” One should interpret these “other devices … full-scale” as small, clean, advanced-fuel alternate-concept fusion reactors.

2. There is no need for a commercial power plant to produce over a GW of electrical power. Electric utilities have moved away from the central station model of power generation. Smaller power plants, 1-100 MW, are highly desirable. They can integrate into the market far more readily that 5 GW plants.

3. This section hides, in plain sight, two showstopper problems that D-T tokamak reactors will have: plasma erosion of and neutron damage to the first wall. These problems have been studied for over 40 years. No credible solution for either has been forthcoming. These and other large problems, like tritium breeding and handling – ITER will use up nearly all of the world’s tritium – will be as expensive to research and as slow to yield solutions as the physics-of-fusion has been over the last 60 years. Only when advanced fuels and advanced confinement concepts become the mainline approach, can these problems be resolved.

**Assessment 4.**

1. This section asserts that “*all strategies* *recognize* that the burning plasma regime promised by ITER is the most expedient way to demonstrate fusion energy on commercial scale.” This perspective is not shared by many, such as the supporters of the FRC and spheromaks, mirror machines, or the compact, high-field tokamak community. The phrase *all strategies* *recognize* is inaccurate. It should be replaced by *earlier reviews suggested*. And the phrase *promised by ITER* – we have ample data what promises from tokamak proponents are.

2. This section asserts that the ITER effort *engages* industrial partners. What it does is *buys* their services. Any research program can do that.

3. This section tacitly supports a *very* “long-term plan,” one that would last more than a century.

**Assessment 5**.

1. I agree with the assertion that “many of the scientific and technical issues of importance to the long-range development of fusion are best addressed by research facilities having size and complexity much smaller than that needed for a burning plasma experiment.” Alternate fusion burning advanced fuels devices fit this description very well. The challenge is to fund them at a level where they can truly explore and make progress in fusion physics.

**Assessment 6.**

1. As noted earlier, the scale of a power plant need not be a few GW. Devices far smaller than ITER can provide what electric utilities want and need.

2. The claimed *long-recognized* value of *international collaboration* needs to be re-visited. Arguably, the most progress in fusion has been made with international *competition*, not collaboration. That the ITER project has missed so many deadlines and exceeded budgets by such large amounts must be taken as data of the inaccuracy of the assertion.

3. The assertion of “isolation” is without merit. Success in the US on small, clean advanced-fuel fusion reactors would bring ample foreign attention to the US.

4. In light of the innumerable delays and major budget overruns experienced by ITER, that past studies of MFE “recommended U.S. entrance into international partnerships as the most cost-effective approach to undertake large fusion energy experiments” must now be considered as very poor advice.

5. The committee’s conclusion that “the United States benefits from partnership in ITER as the primary experimental burning plasma component within its own long-term strategic plan for fusion energy” needs to be reexamined with ample hindsight. What other efforts could have been funded if there was no ITER? Perhaps FIRE? Perhaps a slew of alternate-concept devices?

6. This section asserts that there is no “mature burning plasma experiment as an alternate to ITER.” True, but that is a result of starving the alternate concepts over the last several decades.

**Assessment 7**.

1. This section starts out by asserting that the “US [should] maintain the scientific and technical leadership in this field.” That is misleading. The leadership in tokamak research now resides in Europe and Asia. The US can only recover leadership by starting anew.

2. The third bullet in this section, on innovation, should become first.