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Testimony to the Subcommittee on Energy and Environment U.S. House of Representatives Committee on Science

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The Restructured Fusion Program and the Role of Alternative Fusion Concepts

Testimony to the Subcommittee on Energy and Environment
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Thank you for the opportunity to appear here today on the subject of the restructured fusion program and the role of alternative fusion concepts. My purpose is to tell you why I believe it is essential to strive for about 25% of the fusion budget to be directed to alternative approaches to fusion energy. I am a physicist in the Magnetic Fusion Energy Program at Lawrence Livermore National Laboratory and have been active in the U.S fusion program for the past fifteen years. I was a member of the first international design team for the International Thermonuclear Experimental Reactor (ITER), and my present research includes the design of fusion power reactors and the exploration of advanced fusion concepts.

Fusion is the only indigenous energy source that will last as long as the earth lasts. For this reason alone, it is deserving of a vigorous and sustained research program. As in cancer research, we have made enormous progress in the fundamental understanding of our field. However, also like cancer research, we have not yet come to closure on our ultimate product. Therefore, by analogy, because of the profound benefit to future humanity of an ultimately successful end point, we must continue with an innovative and, most importantly, diverse research program until that goal is accomplished. Accordingly, my thesis before you today is, first, that a viable fraction of the fusion R&D resources must be invested in alternative fusion concepts that have the potential of leading to an attractive commercial reactor product and, second, that the US should adopt this as one of its primary niche roles in the world fusion program.

The only unlimited, non-fossil options for sustainable, global baseload electricity generation in the long term are fusion, advanced fission and solar-electric. Realizing a successful, economic fusion reactor would be a profound contribution to the well-being and energy security of future humanity, with major global export opportunities in the burgeoning energy industry of the next century. We have made tremendous scientific progress in the world fusion program over the last 40 years or so. Recent achievements in tokamak research give confidence that this route to fusion can conceivably produce a functioning power reactor, and the tokamak will surely continue to be in the forefront of experimental fusion plasma research. However, it is not clear that the conventional tokamak approach alone will lead to a fully practicable commercial power plant able to compete in the energy market place of the next century. This is a consequence of its projected low power density, high capital cost, high complexity, and expensive development path.

It is also commonly asked whether there will be a need for fusion in the next century. To me, the answer is clear: Electrical power generation in the 21st century will be a multi-10-trillion-dollar industry with significant growth in demand predicted for the developing world. Furthermore, if the developing world's demand for power is met by coal and other fossil fuels, we have the potential for a disastrous environmental impact, with major ecological consequences for the entire earth. Thus, what we are really asking is: Do we have a sufficiently attractive fusion reactor product that will sell in this marketplace? If we do, then fusion will be "needed".

It is my contention that advances leading to a clearly economic fusion reactor product will lie in the investigation of potential advanced *physics* solutions rather than simply in engineering the nuts and bolts for the present conventional approach. The smartest investment of our research dollars will be to press for innovation and understanding of the physics of various advanced concepts – because this is where the greatest uncertainties lie and where there is the greatest potential for improving the economics of the fusion power plant. Note also that alternative physics approaches are particularly important if we are to exploit the so-called "advanced" (i.e., non-deuterium+tritium) fusion fuels. The potential advantages of advanced fusion fuels such as deuterium+deuterium, deuterium+helium-3 and proton+boron-11 will likely not be realized in a thermonuclear tokamak, and novel approaches will almost certainly be required.

Thus, at a minimum we must continue to explore advanced physics solutions for the tokamak in an attempt to maximize its ultimate potential as a power reactor. However, we must also vigorously pursue the physics of alternative fusion reactor concepts at a viable level. And, very importantly, we must devote appropriate R&D funds for conceptual and computational development to the stage where new, definitive experiments can be defined. This requires the promotion of intellectual stimulation in *breadth* to encourage parallel approaches with the acknowledgment that perhaps only a very few, if any, may ultimately be successful. However, we only need one. I suggest it is too early and, given the future utility of advanced fission, unnecessary, to put all our eggs in one basket at this stage of fusion development.

It is interesting to define what we mean by "alternative fusion concepts". Table 1 (attached) provides a reasonably complete list of fusion concepts which are, or have been, under study at some level in the world program and classifies them according to their main operating principles.

Other ideas will also surely appear in the future given a sufficiently encouraging environment. The schemes in Table 1 fall into four main classes: (1) Low density magnetic confinement concepts; (2) Inertial confinement fusion concepts; (3) High density magnetic confinement concepts; and (4) Miscellaneous concepts which fall outside the first three groups. Some of these approaches such as the tokamak and standard inertial fusion have a considerable scientific knowledge base. Some others have only been proposed at the conceptual level. Note, however, the fact that such a diverse list of alternative concepts exists does not necessarily imply they are all deserving of funding under an expanded alternatives program. Some may have no reactor relevance while others may have a questionable physics basis.

The class of alternatives in Table 1 under inertial confinement fusion is particularly interesting. These provide routes to fusion power plants which are fundamentally different in many respects from the tokamak, not least offering the facility of liquid walls and the ability to isolate much of their high-technology hardware from multiple, redundant power chambers. In particular, the science of inertial fusion is thoroughly grounded in a strong world effort, while inertial fusion energy research in the US is able to leverage considerable utility from parallel, defense-related programs. Arguably, inertial fusion energy could be considered the primary alternative route and is certainly deserving of an increased budget share under an expanded fusion alternatives program.

Among the recommendations from the recent report from the DOE Fusion Energy Advisory Committee (FEAC) are: (1) That the US fusion program be restructured to focus on the less costly basic science foundation; (2) That future US strategy should be to define and secure niche or supporting roles; and (3) That the concept improvement program be expanded to include a spectrum of alternative concepts. I, like most of my colleagues, am in agreement with the major recommendations of the FEAC report with however, two caveats: First, I caution that concentration on basic science should not been interpreted as "sandbox" science, or science purely for science sake. As above, I suggest that advances leading to a more attractive reactor product will accrue primarily from understanding and extending the physics basis of fusion, both for the advanced tokamak and, in particular, for alternative approaches. Thus, our future investment in the critical basic science foundations *must* be focused squarely at improving the ultimate reactor product. We must not lose sight of the fact that, first and foremost, this is a fusion *energy* program. Second, whereas FEAC underlined the need to pursue alternative concepts, they recommended no quantitative budget level or percentage to this end. At present, I suggest that the fraction of the fusion budget devoted to alternatives is insufficient to promote a critical mass. A budget share of about 25% is appropriate and necessary for these activities.

I also note and applaud the fact that FEAC recommends uniform peer review processes as the primary input to determine funding allocations for new initiatives in the future. It is, however, very important that such peer reviews concern themselves not only with physics viability of a proposed concept but also with reactor viability and, equivalently, prospective development path. That is, to be considered a serious contender for alternative fusion R&D funds, a candidate scheme should be able to point the way to engineering realizations that are a step-change in reactor attractiveness from our present conventional approach. Specifically, this means that the potential for lower capital costs, complexity and development costs should be clearly indicated. Otherwise, why exploit it? This rationale suggests we should be circumspect about funding fusion concepts that have no better reactor potential than the conventional tokamak. An example of this is the stellarator. Although its plasma operation may be qualitatively different from that of the tokamak, its power plant embodiment is really no different in terms of the key attributes of cost and complexity.

My final observation on the FEAC report concerns the recommendation for the US to secure niche roles. Why cannot the US lead the world in the pursuance of a vigorous and innovative alternatives program? This could and should become one of our primary niche roles.

The fusion program should also strive for a unifying element focused on the gathering, generation and objective examination of advanced ideas. The present evaluation of such concepts can be uneven, and ideas tend to be followed in isolation without real appreciation of the underlying potential for power plant development. The assessment of new concepts is typically performed by committee reviews. Experience suggests that such committee processes are unlikely to promote breakthrough ideas that stray from the established path unless accompanied by quantitative supporting analyses. Therefore, the program needs a rational basis to determine which concepts are worth exploring to the next experimental stage. It is my recommendation that a broad, expert team be built to perform physics analysis, configuration design and prospective power plant implementation studies for such ideas. Central use would be made of state-of-the-art computational tools. The primary function would be to provide the results of thorough, quantitative analyses to DOE for their assessment. This could become a cost-effective tool to support DOE in managing an alternates program by taking each idea far enough that they can be down-selected on a rational basis for subsequent definitive experimental programs.

In conclusion, I stress that any breakthroughs leading to a fully economically-viable fusion power plant will lie in the exploration of innovative and alternative physics approaches, rather than in engineering our present conventional route. This is where the "science" of the redirected science-based fusion program should be directed and where a primary niche role for the US should be sought. Equally, under such a program, it is important that the physics of a proposed alternative be coupled with conceptual reactor embodiments to indicate the potential for improvements in capital cost, complexity and development path. That is, based on plausible extrapolations of physics models and experimental data, why should it make a better reactor? If it does not, we should not pursue it. The allure of alternatives to the mainline fusion approach is not new. Over the years, various ideas have been examined both theoretically and experimentally. I recognize that there is no guarantee of ultimate success. Nonetheless, I believe the payoff of a successful alternative to be so high as to warrant a continued and dedicated effort, funded at a viable level.

Table 1. CLASSIFICATION OF ALTERNATIVE FUSION CONCEPTS

LOW DENSITY MAGNETIC CONFINEMENT:

- Standard field-reversed configuration
- Large-orbit field-reversed configuration
- Spheromak
- Spherical tokamak
- Reversed field pinch
- Conventional and advanced tokamak
- Stellarator
- Mirror

INERTIAL CONFINEMENT FUSION:

- Standard inertial fusion (heavy-ion-driven, laser-driven, ...)
- Advanced, fast-ignition systems
- Magneto-inertial concepts
- High-yield pulsed systems

HIGH DENSITY MAGNETIC CONFINEMENT:

- Pulsed Z-pinches (fiber, laser-assisted, staged pinches, ...)
- Plasma foci
- Continuous flow pinches
- Wall-confined, magnetically-insulated concepts

NON-THERMONUCLEAR AND MICELLANEOUS:

- Inertial electrostatic confinement
- Colliding beam systems
- Coulomb barrier circumvention concepts