A Leadership-Class U.S. Domestic Stellarator Program

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The benefits of stellarators and the motivation for stellarator research have been extensively documented and reviewed, and are widely understood and accepted. The U.S. has long been a leader in the field, with contributions that include:

- Fundamental theoretical discoveries about the properties of 3D magnetic configurations and how they can be manipulated to control the physics properties of a toroidal plasma.
- 3D equilibrium codes that today are at the heart of numerous 3D design and analysis tools, used the world over for both tokamaks and stellarators.
- Development of quasi-symmetric (QS) stellarators, an innovation that manifests the traditional stellarator advantages of steady-state without current drive or disruptions in a low-ripple, low flow-damping, reduced aspect-ratio configuration.

The QS stellarator offers solutions that can mitigate major risks to the successful development of a steady-state MFE system. The concept is sufficiently different from other toroidal concepts as to offer high potential for breakthroughs and new science, yet its design and performance projections have their basis in well-understood physics of stellarators and tokamaks. The importance of and international interest in QS stellarators internationally remains high, but no other countries are in a position to pursue the concept, either because they have existing commitments to the other important stellarator concepts or because they lack the core capabilities for it. U.S. leadership in QS stellarators is both a necessity and an opportunity.

The U.S. is maintaining its involvement in stellarator research through collaborations with LHD in Japan and Wendelstein 7-X (W7-X) in Germany, in addition to concept-exploration experiments at U.S. universities. Recently China, motivated by an appreciation of stellarator advantages for their fusion program, has approached the U.S. to discuss a range of collaboration possibilities. Our attractiveness as a partner is based on our core stellarator physics and engineering capabilities, a product of DOE investment in stellarator research over many years. We can remain active participants in stellarators, and can even lead experiments, through our flourishing international collaborations. However, U.S. leadership in stellarators requires a domestic program to pursue U.S. innovations targeted to materially improve the prospects for fusion and which no one else can lead.

A new U.S. leadership-class program to develop the QS stellarator for fusion applications must be put in place now. By this we mean a national program committed to advancing the concept as an option for DEMO. Construction and exploitation of a new integrated QS experimental facility is necessary, but not sufficient. The program must include expanded theory and design efforts aimed at concept simplification, as well as continued growth in our international stellarator collaborations. Our domestic activities must be exciting enough to attract collaborators from other countries. The R&D priorities identified by the 2009 FESAC Toroidal Alternates Panel and ReNeW can guide the program plan. The highest priorities are, in brief: 1) simpler coils, 2) high performance integration, 3) predictive capability, and 4) power handling.

New Motivations for Stellarator Research

Recent studies comparing options for a steady-state, energy-breakeven (Q_{eng} = 1) version of a fusion nuclear science facility (FNSF) demonstrate important advantages of a QS stellarator over an advanced tokamak (AT) or spherical tokamak (ST). The absence of current drive in the stellarator gives it significant margin in fusion gain Q_{DT} allowing thermal efficiency requirements to be relaxed and opening up possibilities for simpler blanket systems and lower operating temperatures. Elimination of disruptions relaxes disruption-hardening requirements for high steady-state power handling and for tritium–breeding blankets, thus facilitating the quest for practical solutions for those challenges. The absence of current profile control and feedback eliminates entire systems, including components exposed to the plasma. Access to high-density, high beta in steady-state eases divertor design and strongly reduces fast-particle instability drive. Indeed a stellarator-based net energy-producing fusion system could be significantly simpler than one based on a tokamak. While a next-step FNSF may or may not aspire to energy breakeven, the configuration choices for an FNSF must be made with an eye to the requirements for fusion energy. Prematurely limiting the options to tokamaks and STs is risky.

Q&A

Q: Isn't this essentially a proposal to re-start NCSX? A: The stakes are much higher than the fate of NCSX. At issue is whether the U.S. will assert leadership with an innovative program to develop practical solutions for a reliable, steady-state MFE system. NCSX is one viable option for the needed QS facility, but it was designed more than ten years ago in a different context. We must consider whether a new design incorporating physics and engineering improvements should be developed. We will live with the choice for many years. A decision process should be established now to select a course that provides the best value for the program.

Q: Aren't stellarators too complicated and costly? A: The geometrical complexity and tight tolerance requirements of stellarators are indeed cost drivers, as are the sheer number of components and interfaces, in common with other fusion concepts. Both NCSX and W7-X suffered from project management shortcomings but overcame challenges and succeeded in building complex-shaped coils and vacuum vessels and, in the case of W7-X, completing their assembly. Nonetheless, concept simplification must continue to be a goal of the stellarator program to make future stellarator reactors as easy to construct and maintain as possible. In the case of QS stellarators, the NCSX and ARIES-CS designs represent an interim stage of development. Subsequent research has shown incremental improvements and, more importantly, promising strategies for making quantum improvements. But resources and sustained effort are needed to develop and evaluate these ideas and use them to produce better designs.

Q: Can we afford it under flat budget constraints? A: Indeed the proposed program would require significant budgets on several FES budget lines, growing over the next few years by, for example, ~$50M in major facilities, ~$5M in theory and computation, and ~$5M in international collaboration. Within a flat domestic MFE budget, it would obviously require significant shifts in priorities and redirection of work affecting multiple institutions and individuals. Whether we are capable of such a shift is not in doubt. We are much better positioned than China and Korea were 15 years ago, before they grew world-class fusion programs starting from practically nothing. Most U.S. stellarator researchers today came from backgrounds in tokamak research, and the W7-X research team will necessarily be staffed mostly by people with little or no prior stellarator experience. U.S. researchers and institutions are versatile enough to adapt their talents and technical capabilities to new challenges.

Q: Why not wait for W7-X or for growth in the overall U.S. fusion budget? A: One can make that choice, but it amounts to a decision to forego a clear opportunity that exists now to assert international leadership and follow a path with potential for breakthroughs and quantum improvements in the vision for magnetic fusion energy systems.