MIT-PSFC Makes the Case for QUASAR

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It is broadly accepted that any economically attractive magnetic confinement fusion reactor will operate in steady-state. Currently, the fully non-inductive advanced tokamak approach is the leading candidate, and continued pursuit of solutions to the recognized challenges of steady state, disruption free operation are still required. In parallel, it would be more than prudent to advance the stellarator, which inherently operates in steady-state, without the need for current drive, and, perhaps equally important, has the potential to solve the disruption problem.

Why then hasn't the stellarator overtaken the tokamak as the leading contender for a fusion reactor? There are both scientific and engineering reasons. Scientifically, stellarator energy confinement for reactor parameters may be a problem. Specifically, theory predicts that, in an un-optimized stellarator, neoclassical confinement will become progressively worse as the plasma temperature increases and the corresponding collisionality decreases. This is just the opposite of tokamak neoclassical projections. However, with the pioneering discoveries of quasi-isodynamic and quasi-symmetric stellarator configurations, potential solutions may be in hand. The nearly completed quasi-isodynamic W7-X experiment in Germany and the partially-built quasi-axisymmetric (QAS) NCSX experiment at PPPL, are examples of stellarators, optimized, among other things, to minimize neoclassical transport. The basic idea of QAS in NCSX is to convert the undesirable neoclassical transport in an un-optimized stellarator into the much more desirable regime characteristic of the tokamak.

A second challenge for the stellarator is magnet engineering. The non-planar coils in a stellarator are more complicated to construct than the flat pancake type coils in a tokamak. The result is that there is a longer and steeper learning curve that must be ascended to build stellarator magnets. This translates into substantially higher first-of-a-kind R&D costs. Many of the necessary magnet engineering lessons have now been learned, giving credence to the expectation that future construction costs can decrease substantially.

The final, but perhaps most crucial point concerns the relation between the US and international stellarator programs. The current US experimental stellarator program focuses on international collaboration with W7-X. Why not just wait until W7-X results are in? The main reason is that the W7-X concept, while being a very interesting and highly innovative science experiment, scales to a very large reactor, with a correspondingly large capital cost. In contrast, from the US perspective compactness is essential in order to keep the capital costs sufficiently low for economic attractiveness. Do not underestimate this point. If it is too costly it will not be built in the US.

For these reasons, a substantially increased experimental stellarator program should be a component of our ten year vision for the domestic program. Compactness has been a major motivator for both national and international interest in the QAS. There has been a particularly strong development effort led by PPPL leading to NCSX, now QUASAR. QUASAR has a projected $\nu^* \approx 0.1$ - 0.25 which places it in the near collisionless regime thereby allowing an experimental determination of the critical confinement scaling with temperature. Also, the low viscosity of the QAS allows large velocity shear to reduce turbulent transport. MIT-PSFC strongly supports QUASAR and, if the project moves forward, is ready to partner with PPPL, contributing to engineering, diagnostic and auxiliary heating development, ultimately playing a significant role in the scientific research team. Further optimization of the stellarator concept will be necessary in some areas, including divertors and PMI which are also challenges in tokamaks. Lessons learned from parallel tokamak research can and should be adopted.