US STELLARATOR COMMUNITY RESPONSE TO THE "CONSENSUS TECHNICAL SUMMARY OF THE REVIEW PANEL FOR PROOF-OF-PRINCIPLE PROPOSALS IN FUSION ENERGY SCIENCE"

The US stellarator community thanks the review panel for its careful analysis and support of the proposed Stellarator Proof-of-Principle (PoP) Program. We are pleased with the panel's main finding: *'The panel members conclude that the stellarator community is ready for a PoP program with a lead experiment based on the "quasi-axisymmetric (QA) stellarator", which is a concept based on a new direction, rather than a refinement of more standard directions.'* We thank the panel for its complimentary comments regarding the program's innovativeness, the strong basis in theory and experiment, and the strengths of the stellarator team. The panel's main concerns were the program's cost and schedule; we address these and the technical and reactor issues they raised below.

THE STELLARATOR PROOF-OF-PRINCIPLE PROGRAM

As the panel has recognized, stellarators offer solutions to some of the problems that affect the viability of tokamaks as a power source: discharge-terminating disruptions, the difficulty of achieving steady-state operation, and high-beta instabilities. This is why a strong world stellarator program exists, including 0.5-1.0 billion-dollar class experiments in Japan and Germany. The aim of the proposed U.S. program is to develop stellarators with beta values at least as high as that of the optimized German advanced stellarator design (5%), but with much lower aspect ratio (2-4 instead of 10-11), leading to smaller reactors.

The two new elements of the proposed program are based on promising complementary transport optimization strategies for compact stellarators (with plasma aspect ratio A = 2-4): quasi-axisymmetry (QA) and quasi-omnigeneity (QO). Both the QA and QO concepts make use of the internally generated "bootstrap" current to different degrees, rather than suppressing it, to create a configuration with <1/3 the aspect ratio of the currentless W7-X stellarator. Both QA and QO look attractive for compact stellarator reactors, and each has distinct complementary advantages (physics, reactor embodiment). For example, the transport optimization is different and most of the rotational transform is provided by the self-generated plasma current in the QA approach and by external coils in the QO approach. Both must be developed experimentally to establish the needed scientific base for the program's ultimate success. A determination of the optimum strategy to pursue is one of the US stellarator program's goals.

In order to minimize cost, it is planned to construct the PoP facility by modifying the existing PBX-M tokamak and using its supporting infrastructure. We are pleased that the review panel, after carefully examining the pros and cons, endorsed the cost effectiveness of this approach. The panel also supports our plan to test QA first at proof-of-principle scale, and make the PoP facility capable of being converted to a QO geometry a few years downstream. We agree with the panel that the decision and specifics of such a conversion should be reviewed at that time in light of the knowledge gained from QA and QO experiments. Our proposed QOS experiment is necessary now to provide the data base on the QO concept needed to support such a decision.

The panel saw quasi-symmetry (including the quasi-helically symmetric concept being tested in the HSX at the University of Wisconsin as well the NCSX based on QA), as an "alternate tack" to the German program. However, while the German program is developing a high-aspect-ratio low-shear device (W7-X) that is related to the QO concept, W7-X will *not* investigate the key features of the compact QOS approach: accommodation of moderate bootstrap current and balancing the effects of bootstrap and Pfirsch-Schlüter currents in maintaining relative independence of the magnetic configuration on beta, rather than simply minimizing these currents, to achieve a factor of 3 reduction in plasma aspect ratio. In addition, W7-X will not start operation until 2005 at the earliest. Understanding the dependence on the magnetic configuration of thermal transport, energetic particle confinement, and bootstrap current control for the low-aspect-ratio QO configuration is needed at that time to design the best configuration for the PoP-level QO test in the reconfigured NCSX. In order to provide the knowledge base needed for the decision on the reconfiguration of NCSX in a timely fashion, the compact QO concept needs to be developed, initially at the concept-exploration (CE) scale, as an essential component of the stellarator PoP program.

While we agree that each element of the proposed PoP program must undergo a technical review, we emphasize that the QOS stellarator CE-scale experiment is an essential part of a coherent compact stellarator PoP program and should not be considered to be just a part of a "broader CE process". Similarly, the other program elements -- CAT upgrade, theory, international collaboration, and system studies -- are essential elements of the PoP program. The combination of a lead PoP experiment and supporting elements conforms to the 1996 FESAC/Alternates model of a proof-of-principle program, and provides a coherent, mutually supporting framework for developing the compact stellarator concept.

COST AND SCHEDULE ISSUES

The panel expressed concerns on program budget and schedule. Cost and schedule considerations have been in the forefront of our planning for the PoP program, as reflected for example in our decision to use PBX-M as a reconfigurable facility to first test the QA concept at PoP scale and then the QO concept after its further optimization in QOS.

The budget for the entire proposed program, <$30M/year after a 4-year ramp-up period, is reasonable in the context of the Fusion Energy Program. Indeed, the NCSX program budget of $20M/year for operations and for research by a strong national team compares favorably with other elements of the U.S. program, such as the budget for a single tokamak experiment (the DIII-D at ≈$50M/year). As the panel points out, *"Slightly less than $10M/year is requested for the costs of supporting theory, stellarator CE experiments, and international collaborations."*, which is a modest budget for all the rest of the PoP program, especially considering the high value of these elements to the development of the compact stellarator concept.

NCSX is very cost effective. The research capabilities that NCSX will provide (magnetic field strength and shaping, plasma volume, heating, diagnostics) are similar to those of previous tokamak proof-of-principle scale facilities, including the PDX/PBX series itself, which are typically valued at $100M or more, and built up through many years of investment. In comparison, the NCSX is a bargain, made possible by taking advantage of these past investments. The $20M/year operating cost of the NCSX, which covers facility operations, upgrades, and research, is in fact well *below* the upper limit of the FESAC range ($5M-$30M per year for a proof-of-principle facility). In addition, NCSX benefits from economies realized by operating in tandem with the similar-sized National Spherical Torus Experiment at PPPL and using common support systems, spares, procedures, and personnel. The need to minimize both the construction and operating costs, as well as the time to first plasma, is the sole reason for the decision to use the

PBX-M facility and to accept the PBX-M constraints, an approach which the panel has endorsed after carefully examining the pros and cons for itself.

We have tried to minimize cost in scoping the NCSX design. The project scope includes only the minimum set of equipment modifications needed to attack the most critical scientific question about the concept, namely whether 3-D fields from external coils can be used to suppress beta-limit disruptions. Later hardware improvements will be guided by progress in the program and implemented only as needed. As the conceptual design moves forward in the coming year, we will study alternative funding profiles and scope reductions showing the impact on cost and schedule with the aim of reducing the total project cost and advancing the schedule.

TECHNICAL ISSUES

The panel identified four technical concerns. We will follow their recommendation to address these as the stellarator program proceeds.

MHD activity during startup. The panel expressed concern about possible beta limits due to MHD activity during plasma startup, when a QA or QO magnetic configuration may have a hill; however, present experiments are reassuring. Both Heliotron E and CHS have obtained their high beta results starting from this condition. A magnetic well develops automatically as beta increases. In addition, the QA and QO configurations studied have a magnetic well when even a relatively small amount of net toroidal current is present. One possible startup scenario for NCSX would use some initial ohmic current drive to produce a magnetic well before raising beta, but startup in the presence of a hill will also be studied. The proposed upgrade of CAT will investigate MHD stability during ohmic-driven startup prior to operation of NCSX. Other plasma startup scenarios will be analyzed as part of the physics design process.

Power and particle handling. Power and particle handling will be increasingly important for stellarators as they move to higher power and longer pulse lengths. Important information on this will be obtained from the near-term wall conditioning and magnetic island divertor experiments planned on W7-AS and LHD. This information will be available in time for the NCSX and QOS experiments. Initially we will rely on standard wall coatings and conditioning techniques in our experiments. Boundary plasma studies and iteration of the plasma-facing components for improved power and particle handling as power and pulse lengths are increased are part of the research program for the PoP experiment. Consistent with the panel's recommendation, the plans will be developed in more detail and reviewed by our Program Advisory Committee and through the machine proposal review process. Information on the magnetic island structures suitable for divertors will also be obtained from experiments on QOS.

Alfvén instabilities. The panel noted that beam-driven Alfvén instabilities (AE, or Alfvén eigenmodes) were deleterious in some stellarator experiments, but that this did not appear to be the rule, and suggested that better theoretical understanding should be developed. We agree with this comment. Alfvén eigenmodes have been observed in both W7-AS and CHS, but they have either occurred only over limited parameter regimes (in CHS with the magnetic axis shifted out at low density $< 3 \times 10^{19}\ m^{-3}$) or apparently have no significant effect on heating efficiency (as observed on W7-AS). Certainly more detailed theory and experiments will need to be done to understand AE stability physics issues in QA and QO configurations. Both QA and QO stellarators also incorporate high shear regions near the edge (not present in W7-AS) in order to stabilize kink instabilities; this feature should help suppress AE modes by enhancing continuum damping.

Compact stellarator experiments and theory can add significantly to the physics understanding of AE instabilities over what can be accessed in axisymmetric tokamaks.

Verification of code predictions. We agree with the panel's recommendation that there should be continued benchmarking of theoretical code predictions against experimental results at finite beta. One example is the excellent agreement between measured and calculated flux surfaces in W7-AS at $\langle\beta\rangle = 1.8\%$ where the magnetic axis shift was >2/3 the average plasma radius. The US stellarator theory effort has been closely coupled into the international stellarator program, and this has included benchmarking of codes. With the Japanese LHD and the Spanish TJ-II now in operation, the opportunities for code verification will increase. NCSX will allow testing of key theoretical predictions concerning beta limits and the effects of bootstrap currents at low aspect ratio, an unexplored area.

REACTOR ISSUES

We agree with the panel conclusions that *"If the physics issues in the stellarator are solved, a stellarator reactor will have several advantages over the tokamak: lack of disruptions, steady-state operation, and lack of auxiliary current drive. The cost savings of the latter, both in capital and operation, may balance added costs from the complexity of the magnetic coils."* Indeed, the 1993-1995 Stellarator Power Plant Study by the ARIES team showed that the projected cost of electricity was about the same as the second-stability ARIES-IV tokamak reactor for the same costing and engineering assumptions. The compact-stellarator program aims at reducing the major radius, by a factor of ~2, allowing higher wall loading and mass power density and reducing the projected cost of electricity significantly below that of the SPPS design.

Further reductions in cost will depend on beta limits and confinement improvement, key issues for the stellarator PoP program. A conservative 5% beta limit has emerged from MHD stability calculations that do not include potentially stabilizing kinetic effects, effects of flow shear, or nonlinear effects. Some QA and QO configurations have significantly higher beta limits. However, beta-limiting instabilities in stellarators have not been experimentally observed to date, so NCSX will play an important role in clarifying the physics governing attainable beta limits. A major focus of the stellarator PoP program is the reduction of anomalous transport and the physics of transport barrier formation. NCSX, QOS, and HSX will each explore different avenues of generating E x B shear to dampen turbulence including plasma rotation and the ambipolar radial electric field.

An important part of the proposed National Stellarator Program is system studies by the ARIES group to quantify the economic benefits of compact stellarator features and to compare with advanced tokamak reactors. The panel's concluding comment, "*Further innovation and simplification of the stellarator concept may still be needed for it to be a commercially successful fusion energy reactor.*" highlights the underlying reason for the integrated stellarator PoP program that we are proposing.