WILL HIGH T_c STOP THE SSC?

Lest my motives be misunderstood, let me say at the outset that I support the physics that can be done with the Superconducting Super Collider. The question is not whether the physics is desirable, but whether the SSC is the best machine for the job. A year ago the answer would have been an unqualified yes; today the answer must be "maybe not." Why? Because of advances in superconductor technology!

The original SSC design was based on a competition between two superconductors.[1] They both operated at about 4 K and they differed in their magnetic field strengths (3 T and 6 T). This was state-of-the-art technology at the time. Given these constraints, the radius of the SSC, which is inversely proportional to the magnetic field, had to be huge (180 km and 90 km, respectively). The corresponding costs are also huge ($4.4 billion to $6.0 billion).

In the past few months it has been shown[2] that superconductivity may be possible at temperatures as high as the boiling point of liquid nitrogen, and critical magnetic fields as high as 50 tesla are estimated at 0 K. Suppose for a moment that a high-temperature superconductor (say with a critical temperature above the liquid oxygen boiling point of about 90 K, and a critical magnetic field of 24 T at this temperature) could be produced in quantity. If it was used in the SSC, the radius could be reduced from the presently accepted[3] 96 km to a "mere" 24 km. This size reduction would not detract from the high-energy physics the SSC is intended to study. It would, however, result in a considerable cost reduction. This savings and the potential applications of high-temperature superconductors to communications and transportation[4] make for a potent combination that suggests R&D funds should be channeled into the superconducting infrastructure *before* the SSC is built.

Many in the high-energy physics establishment will undoubtedly argue that the delays would hurt their endeavors. This argument has been used in the past.[1] Is it valid? That depends on the length of time required to achieve a high-temperature superconductor technology capable of functioning in an SSC. Savings in construction time and cost for a smaller, but sufficiently energetic, SSC could compress the development timetable of the SSC after the superconductor technology is in place. These are open questions that should be addressed.

Another view may also be taken, a view that may be desirable from the perspective of European competitors. The superconductor characteristics I mentioned above are now possible. Consider, for example, that critical magnetic fields as high as 40–55 T have been reported for liquid helium temperatures.[5] Should my hypothetical superconductor be built, physicists could study the physics intended for the SSC in the 26-km Large Electron–Positron ring at CERN! The Europeans could retool an existing facility (at a considerable cost savings) while simultaneously demonstrating a potentially very valuable superconductor technology. From this perspective it would be in the best interest of the Europeans, both economically and scientifically, to channel funds into superconductor R&D.

Recent superconductor advances are shifting the race to study SSC physics from acquiring political support to developing high-temperature superconductors with large critical magnetic fields. I expect many high-energy physicists to become defensive about the scenario I have suggested. There is not sufficient space here to preempt their criticisms by responding to them in advance. What is most important is that we Americans not be remiss by failing to recognize the shift in the high-energy physics race because of advances in superconductor technology.

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THE ASSOCIATE DIRECTOR OF THE SSC CENTRAL DESIGN GROUP REPLIES: It would appear that John R. Fanchi has not taken fully into account the quantitative aspects of the points to which he speaks. Before addressing those points I would like to make it clear that we who have been working on magnets for the SSC have been as excited as anyone about the discovery of new superconducting materials that may have properties that would be very useful for accelerator magnets. Work has been undertaken at high-energy physics laboratories to explore possible applications of these materials and to contribute toward a better understanding of their properties. Furthermore, the SSC Central Design Group is providing modest support for an R&D program by an experienced group of materials scientists and engineers to investigate possible high-energy physics applications of the new materials. However, contrary to the view expressed by Fanchi, it is my observation that as the weeks have passed, the theoretical and experimental experts have become more and more conservative in their projections of the time scale on which the materials might be ready for use and even on the question of the degree to which they may be useful for accelerator magnets.

Now to address Fanchi's points directly. A higher field for the SSC would indeed permit a smaller tunnel, but to reduce the circumference to about one-quarter its designed value would require 4 times the present magnetic field intensity, and that would involve forces about 16 times the present value. In turn, this would require a much heavier magnet structure, if containment is feasible at all. (The present forces already tax normal insulation materials to their limit.) Calculations show that this smaller configuration would cost more, not less.

Fanchi speaks about critical magnetic fields as high as 40–55 T at liquid helium temperature. But producing such a field in anything like an economically feasible magnet design would require very high current densities in the coils. At the moment the current densities that the new materials can support are not as great as those we are achieving in niobium–titanium in the present SSC magnet design. There is no evidence that the new materials can support even the presently quoted currents except at very low temperatures (in the vicinity of that of liquid helium), and I doubt that even the most unbridledly optimistic of those scientists working on the new superconductors woud predict that they would support four times the current density achieved in niobium–titanium.

The enormous progress in elementary-particle physics in the past decade has been built upon the development of an extremely complex infrastructure of physicists, technicians and engineers. One cannot put that complex animal in a holding pattern for a decade or two, to be reawakened at will. Its current high level of capability would quickly decay and diffuse. It would be foolhardy to invite such a process for the sake of gaining an improbable economic advantage that would, at best, be small and that would require, in any case, that a set of materials have properties nature may or may not have bestowed on them.

Research and development of the new superconducting materials should be supported at a level that fits the problems, their tractability, and the number and quality of people available to work on them. That probably will require tens of millions of dollars a year. However, I believe that it is scientifically sound to take simultaneously the next logical step in pursuit of an understanding of the structure of matter and of the forces through which it interacts. I further believe that it would be a sad abdication of our position of scientific and technical strength for us to do otherwise. Fanchi himself says that he embraced that view one year ago—before the new superconducting materials were discovered. I can see no reason to revise that judgment.

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CALIBRATING A BUBBLY UNIVERSE

The proposals for "calibrating with bubbles" in Roger N. Shepard's letter (April 1987, page 114) will not work, at least in the sort of universe most cosmologists think we inhabit.

He proposes inspecting redshift diagrams of galaxy surveys for elongations of the voids as a way of testing the distance scale. However, these diagrams are totally independent of the distance scale, since their units are velocity (redshift) and angles.

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