editorial

Generalizing the SSC decision

When DOE Secretary John S. Herrington announced that President Reagan has decided to support the Superconducting Super Collider, the news was understandably welcomed by the entire high-energy physics community, especially by those who worked for several years on its design and who produced the voluminous documentation attesting to its scientific and technical soundness.

The physics rationale for the SSC is oft-stated and I will attempt only the briefest sketch here. The central problem facing high-energy physics is the origin of elementary-particle mass. This problem has well-known solid-state analogs, the phenomena of superconductivity and superfluidity. Generation of quark and lepton mass is analogous to generation of the energy gap at the Fermi surface; generation of intermediate boson mass is analogous to Meissner effect screening of electromagnetic fields beyond the London penetration depth. The particle physicist, however, doesn't understand the nature of the condensate, the underlying force (analogous to electron–phonon interaction) or the correct formalism to use (should it be Ginzburg–Landau, or BCS, or something else?). He or she does, however, have a fairly good idea of the value of the critical temperature, roughly 400 GeV. Indeed, a few picoseconds after the Big Bang, the universe is supposed to have undergone a second-order phase transition from a high-temperature "normal" vacuum, leaving us now with a universe containing a mysterious "vacuum condensate." The TeV energy scale associated with this conjectured phase transition invites, indeed demands, a thorough experimental look at the phenomena that would drive it. The specifications of the SSC take all this fully into account.

The most significant physics from the SSC may be unanticipated and may not occur at the highest mass scale. The SSC will be extraordinarily productive at any mass scale up to 1 TeV. For example, the physics at the highest mass scales available to Fermilab's Tevatron collider, now coming on line, will also be available on the SSC, but with an event rate at least ten thousand to one hundred thousand times higher than the Tevatron collider design value. The SSC will be a very flexible instrument in terms of physics potential. I suspect it will take several generations of diverse experimental approaches operating in parallel to properly exploit the physics opportunities.

Although I'm a practicing high-energy physicist, I've been more apprehensive about the SSC than most of my colleagues. My apprehension originates for the same reason as everyone else's—the cost [see the news story on page 47]. Are we overreaching? Might the cost escalate? How long will it really take to get the SSC commissioned? What happens meanwhile to the ongoing high-energy physics program? It also *must* survive despite the burden imposed by the SSC. These issues are a parochial version of those that scientists outside of high-energy physics are concerned about. But the project has so far progressed expeditiously, albeit far from optimally. The original design concept and cost estimate have held firm. Public support, while less than unanimous, is very strong. And the bottom line reasons for success so far are twofold: The science is superb and the project is credible.

If the SSC does go ahead, funding for high-energy physics in the next decade must on average double. Many other fields of basic research are worthy of similar increases in funding.

The SSC must serve as an exemplar; if the public does view high-energy physics as worthy of this kind of support, it follows that equally valuable science—and that covers a lot—is likewise worthy of an increased level of support. The Reagan Administration's decision to support the SSC shows that it recognizes the importance of this element of very basic research to our national health. We physicists are good at generalizations from the special case. We must work together toward generalizing the SSC decision to the rest of basic science.

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