Decision Time for the Supercollider

The research potential is undisputed, but the price tag is $4 billion; with budgets growing tighter, the supercollider has become a lightning rod for scientists' fears and resentments

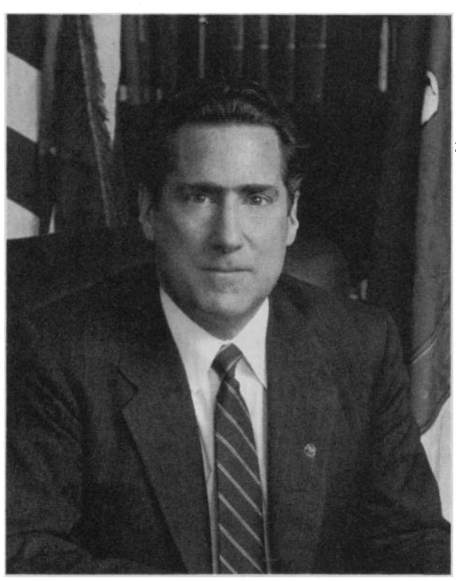
SOMETIME this summer, Energy Secretary John S. Herrington will decide whether to seek funding for the Superconducting Super Collider, a 20-trillion electron volt (TeV) colliding beam accelerator being proposed for the mid-1990's.

It will not be an easy decision to make. The supercollider's potential to do forefront science is undisputed. And yet, with a main ring circumference of 100 kilometers and a price tag of roughly $4 billion, it is by far the largest and most expensive scientific instrument ever contemplated. It comes just as the Gramm-Rudman-Hollings deficit reduction act is beginning to make itself felt in earnest, and it therefore poses a stern test for the Reagan Administration's commitment to basic science. It fosters a sense of paranoia among scientists in other disciplines, who fear that the money will ultimately have to come out of their own programs. And it fans long-smoldering resentments in the scientific community about the money and attention given to "Big" science while "Little" science seems to languish.

In short, the supercollider is not *just* a scientific instrument. It has become a symbol of much larger issues. Thus, the decision process is worth a close look—especially since more and more disciplines seem to be crossing the threshold into big science. The supercollider decision will hardly be the last of its kind.

The physicists' enthusiasm for the supercollider stems from two facts that came to the fore in the early 1980's. First, general theoretical arguments implied that fundamentally new phenomena would begin to appear as particle collision energies approached some 20 TeV. While the details of these phenomena were uncertain, it was clear that they would illuminate such critical questions as the unification of the fundamental forces, the origin of mass, and the role of supersymmetry in nature (see box, page 422). Second, at a 1982 summer study held in Snowmass, Colorado, machine designers realized that this hypothetical 20-TeV collider was actually quite feasible: the technology already developed for the 1-TeV superconducting accelerator at Fermilab could be scaled up in a straightforward way.

The upshot was a remarkable wave of unanimity in the high energy community. In July 1983, only 1 year after the Snowmass summer study, the High Energy Physics Advisory Panel recommended without dissent that the Department of Energy (DOE) proceed with the project. In the same meeting, moreover, the panelists recommended that the department terminate the troubled and controversial ISABELLE accelerator project at Brookhaven National Laboratory—not least because they felt that ISABELLE would divert manpower and money from the supercollider.



John S. Herrington: *A choice between first-rate science and draconian budgets.*

There followed a host of further studies, coordinated by a newly formed Central Design Group headquartered at the Lawrence Berkeley Laboratory. The culmination of those studies came this spring with the Conceptual Design report, a 712-page document that details everything from the supercollider's magnets and cryogenic systems to its scientific rationale and estimated cost (some $4 billion in 1984 dollars, or some $6 billion allowing for inflation over the 10-year construction period). The report was transmitted to DOE headquarters in April, where an independent technical review panel has given it high marks. The department's Energy Systems Acquisitions Advisory Board is now conducting its review of the report's cost estimates.

Once that review is completed, all of this information will then serve as input for Herrington's decision. The key factor now is timing: given the design work that has already been done, construction of the supercollider could begin as early as fiscal year 1988. However, since the DOE's fiscal 1988 budget proposal has to be submitted to the White House Office of Management and Budget (OMB) by the fall of this year, Herrington has to make a Go/No-Go decision on the supercollider this summer. He basically has three choices:

- *Yes—the department will go for the supercollider on schedule.* This choice means that Herrington will have to fly in the face of Gramm-Rudman-Hollings and ask for an increase in his budget for fiscal 1988. Trying to take $4 billion out of high energy physics itself would decimate the program as a whole, even if the cost were spread over a decade. (The current high energy physics budget is about $520 million per year.) Trying to take it out of nuclear physics, fusion research, or any of the department's other research programs would ignite an interdisciplinary civil war. The budgetary impact of the supercollider could be lessened somewhat by international participation; the Japanese, for example, may be willing to contribute up to half the superconducting magnets, which would account for roughly $500 million of the total cost. However, that would not fundamentally alter the problem: the U.S. government would still have to put up billions of dollars.
- *No—it is too expensive.* The physicists themselves are loath to talk about this possibility. When asked about fallback positions, they invariably respond that they do not have one. "A machine of this scale and time frame is critical if the field is to make progress," says Lee Pondrum of the University of Wisconsin at Madison, the current president of the American Physical Society's Division of Particles and Fields. "If we're really told 'No,' we're up the creek."

Of course, a cynic might wonder if the physicists were declining to talk about plausible alternatives lest they undermine the supercollider itself. But in fairness, the alternatives are not all that attractive. For example, one could save a certain amount of money—it is not clear how much—by building a smaller machine. The problem is that the scale of the supercollider is set by the physics, and not vice versa. "As you decrease the parameters of the machine, you decrease the 100% assurance that you will get to the energy range where you really understand things," says Fermilab director Leon Lederman. "You'd still be gambling $1 billion to $2 billion, and an enormous amount of blood, sweat, and tears, and you could come up with nothing."

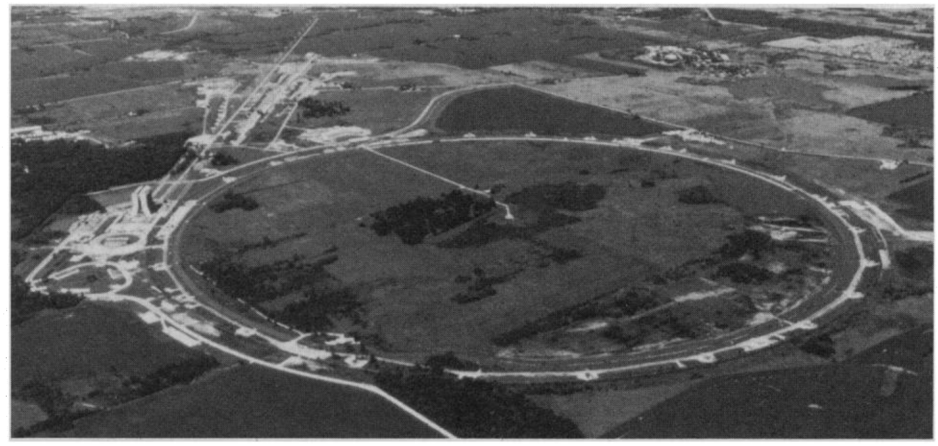
Another alternative would be a crash program to develop new kinds of accelerators capable of doing the same job as the supercollider for less money. Indeed, modest efforts of this sort are already under way; free-electron lasers, plasma beat-wave phenomena, and several other advanced acceleration techniques are showing considerable promise. Even with a crash program, however, practical machines based on these technologies seem to be at least 10 or 15 years away—if they work at all—and even then it is not clear whether the result will be any cheaper than the supercollider itself.

As a final alternative, the American physicists could join with their European counterparts to build the so-called Large Hadron Collider, which would involve placing a new set of magnets in the tunnel of LEP, the Large Electron-Positron machine now under construction near Geneva. This would certainly be a cheaper way of reaching very high energies. But it would be risky: not only would the conversion of LEP require the development of a new generation of high-field magnets, but even then the particle collision energy would only be 9 TeV per beam. Like the scaled-down supercollider, an upgraded LEP might just miss the most exciting physics.

■ *Not so fast.* Given the nature of Herrington's dilemma—first-rate science versus draconian budgets—he may very well choose to delay. For example, he could ask for construction money in fiscal 1988, but with a slow start. He could postpone the decision for a year or more and ask the Central Design Group for more study. He could postpone the new start for a year while making a strong endorsement of the supercollider. Indeed, there are so many variations on this theme that he has a nearly continuous range of options.

On the other hand, he cannot sustain the project in limbo forever. For one thing, a long delay could mean the loss of potential foreign partners: the Japanese in particular may get tired of waiting for the supercollider and instead commit their money to high energy projects of their own. Perhaps more important, letting the supercollider die a lingering death by indecision would be horribly demoralizing for the physicists, and a waste of time and energy that might have been spent on alternatives.

Assuming for the sake of argument that Herrington's decision is "yes," the supercollider will then move into a much more political arena. All along, physicists and DOE officials have emphasized that the supercollider must be a "national" commitment in some sense—meaning that it cannot be accommodated within the existing departmental budgets. The question is what this national commitment will mean in practice.



The main ring at Fermilab: *The laboratory's Tevatron is some 6 kilometers in circumference and produces beams of 1 TeV. The supercollider will have 20 times the energy and will be almost 20 times as big.*

For many scientists, already concerned about their prospects under Gramm-Rudman-Hollings, the supercollider is terrifying. Their fear is that the Reagan Administration budgeteers will make the funding for science into a zero-sum game—if they have not done so already—and that the supercollider will come at the expense of basic research in every other field. "It's clear that the scientific community cannot afford the supercollider as just another project," says William F. Brinkman of Sandia National Laboratory, who chaired the National Academy of Sciences' recent survey on the future of physics. "The Administration has to get behind it in the same way it gets behind projects like the aerospace plane. Otherwise the supercollider will kill the rest of science."

In much the same vein, the supercollider has fanned long-standing resentments over national priorities in basic research—the old Big Science versus Little Science debate. University of Chicago chemist Stuart A. Rice, for example, points out that the most productive research is often carried out by small university research groups, where individual initiative and creativity are highlighted. And yet, funding agencies all too often push big, flashy, and expensive projects as a form of bureaucratic self-aggrandizement—a phenomenon he calls the Edifice Complex. "It's not the size of the project that should determine our attitude, but its intellectual value," says Rice.

Rice is hardly alone in his concern. The health of small-group research has been an underlying theme in all the recent National Academy surveys, including such milestones as the Pimentel report in chemistry, the Brinkman report in physics, and the Field report in astronomy. It is likewise implicit in the perennial complaints about obsolete academic instrumentation. And ironically, it is even felt within the high energy physics community itself. The supercollider will only accommodate a maximum of six detectors at any one time. If some of the existing accelerators are shut down to make way for the supercollider—as they almost certainly will be—then the already huge high energy experimental teams are going to grow even bigger as thirsty researchers cluster around the few remaining spigots for data. So what happens to creativity and initiative in a field where scientists spend their entire professional lives lost in the crowd? How will young researchers ever mature into team leaders in their own right?

At the Department of Energy, Office of Energy Research director Alvin W. Trivelpiece is the first to admit that the Edifice Complex is real. It is always hard to make the case for increasing university research grants, he says, because there is no way to prove that the extra money will make a difference. "It's much easier to get funding for projects like the supercollider or a synchrotron light source because each is a definite thing."

On the other hand, he says, the whole Big Science/Little Science dichotomy is something of a straw man. "It rests on a nonvalid premise: that you can transfer funds," he says. "Each year, you make a case for each element of the program as best you can. If you don't get the supercollider, that doesn't mean the money is going to appear in chemistry." Conversely, he says, the supercollider will not necessarily come at the expense of chemistry and other small-group

Why Go to 20 TeV?

The irony of the supercollider proposal is that it comes at a time when the physicists have theories covering virtually every piece of empirical data they know. The standard unified model of the electromagnetic and weak interactions has been abundantly verified. Quantum chromodynamics is in good shape as a theory of the strong interactions. And general relativity stands as an elegant and compelling theory of gravity. The problem, of course, is that each of these theories operates in isolation from the others. Taken together they have an ad hoc, patchwork air, as if they were only a series of approximations to a more fundamental theory. And indeed, no one doubts that this is the case. The trick is to figure out what the ultimate unification is; at the moment, the physicists are stuck with a lot of bright ideas, a lot of questions, and no way to sort them out experimentally.

However, there are now a number of hints as to where the resolution may lie. In particular, an essential feature of both the electroweak theory and the so-called grand unified theories is a family of enigmatic Higgs particles. Among other things, the interactions of the Higgs particles with ordinary matter is what causes the electromagnetic, strong, and weak interactions to look so different from one another. The Higgs interactions also produce mass in electrons, quarks, and many other particles. And in the first microseconds after the big bang, a spontaneous condensation of Higgs particles may have triggered a period of cosmic "inflation" that shaped the universe into its present form. Current accelerators have turned up no sign of the Higgs. But theorists have shown that if the Higgs exist at all, at least one will have a mass on the order of a few trillion electron volts (TeV); otherwise, certain particle interactions would occur with a probability greater than 100%, which is absurd.

This argument is very general, and sets the mass scale of the Higgs no matter how they work in detail. In some theories, for example, the Higgs particles are composite objects made from elementary constituents analogous to quarks and leptons. If this is the case, the Higgs particles will display a rich new set of interactions and excitations on an energy scale of about 1 TeV. In other theories, the Higgs particles are governed by a principle known as supersymmetry, which postulates that every particle of integral spin (0, 1, 2, . . .) has a partner of half-integral spin (1/2, 3/2, . . .) and vice versa. Such theories imply that the Higgs must have a mass less than 1 TeV, and that supersymmetric partners to the quarks, leptons, and other known particles will also be discovered at masses less than about 1 TeV.

In any case, it might appear that the solution to the Higgs mystery is simple: just smash ordinary particles together at 1 TeV or so, and then stand back as the energy is transformed into a stream of Higgs and other new particles. Unfortunately, however, life is more complicated than that. Electrons and positrons cannot be boosted to 1 TeV by using current technology because they lose too much energy to synchrotron radiation as they circulate around the accelerator; Europe's Large Electron-Positron collider, LEP, is 27 kilometers in circumference and will only reach 100 GeV per beam. Higher energy machines have to use protons or antiprotons, which are not nearly as susceptible to synchrotron losses.

Protons, however, have their own drawbacks. At 1 TeV a proton is not really a single particle, but a swarm of quarks moving along like a blast of buckshot. Since each quark carries only a fraction of the proton's energy, the only way to get an appreciable number of quark-quark collisions at 1 TeV is to have proton-proton collisions at 5 TeV or even 10 TeV. On the other hand, the 1-TeV figure calculated for the Higgs mass scale was only an estimate. The most exciting new phenomena could easily lie just a little higher. So it is probably safest to double the energy again, to 20 TeV.

Thus the supercollider: a proton-proton machine with 20 TeV per beam. And thus the enthusiasm with which it has been greeted by the physics community: "Scientifically, the supercollider is mandatory," says astrophysicist David Schramm of the University of Chicago. "We're at the same place that we were at the turn of the century. We have a standard model that explains almost everything. But there are just a few little nagging details like the Higgs that we know don't fit." Compare that to the quandary posed by blackbody radiation and the mysteriously undetectable ether in 1900, he says: "The solution to those nagging little details gave us quantum mechanics and relativity." ■ **M.M.W.**

endeavors. Quite the opposite: "If you didn't have the big projects," he says, "you probably wouldn't have a good political climate for little science either."

The same point is echoed by the physicists themselves, who argue that abandoning the supercollider on budgetary grounds would be an act of profoundly negative symbolism for the country in general and for science in particular. As Lederman puts it, "What we need is a grand unification of science and scientists armed with the conviction that what is good for science is good for the nation." If science turns away from the cutting edge, it will quickly become a stagnant, spiritless kind of enterprise, given over to jealousy and turf-mongering.

Consider a question already being asked by many scientific critics, and which will certainly be asked by Congress and the White House: "What are the high energy physicists willing to give up to get the supercollider?" The physicists' reply is that they have already given up ISABELLE and have forgone asking for other possible projects such as the dedicated collider, which would be an expansion of the Tevatron at Fermilab. But they cannot absorb the supercollider construction costs without decimating their ongoing program; asking them to do so is tantamount to punishing the high energy community for its scientific success.

In the end, of course, there is no objective way to settle such arguments. Assuming that the supercollider passes muster at Herrington's level, the ultimate decision will have to be made by the political system. And in broad outline, it is clear enough how this phase of the process is going to work: once Herrington gives the go-ahead, his department will ask OMB for increased funding to handle the supercollider construction; OMB, citing Gramm-Rudman-Hollings, will almost certainly say no; and Herrington will appeal to the President. In addition, since he reportedly has a good personal relationship with Reagan, he may take it to the President from the start. Either way, the supercollider will eventually wind up on Reagan's desk.

Of course, Reagan will have any number of inputs on this decision, with Herrington and the White House Office of Science and Technology Policy being two obvious sources. He may also ask for an opinion from one of his two cabinet councils, which respectively cover economic and domestic policy. But in the last analysis, the fate of the supercollider may very well hinge upon whether it strikes the President's fancy. Given the strictures of Gramm-Rudman-Hollings, one can expect Reagan and his inner circle to be exceedingly cautious. Yet Reagan has been known to ignore budgetary constraints before when he likes something, three prime examples being the NASA space station, the "Orient Express" aerospace plane, and the "Star Wars" Strategic Defense Initiative. At this point there is no predicting how he will react to high energy physics.

Assuming that Reagan does give the go-ahead, the supercollider then faces one last hurdle: Congress. At the moment, however, that hurdle does not seem very high. Last April, for example, Representatives Vic Fazio (D–CA) and Ron Packard (R–CA) got 91 of their colleagues to sign a petition urging Reagan to support the supercollider. The petition cited the machine's potential for particle physics research and for technological spin-offs. What it did not mention, but what is a very real issue on Capitol Hill, is that the supercollider will be a prestigious and lucrative catch for whatever state it is located in, and will create an estimated 7000 jobs. Most observers believe that the supercollider will therefore receive a reasonably warm welcome in Congress—at least until a site is chosen and the congressmen from 49 states realize that their state was not the one.

In Washington, of course, this kind of political calculation is routine. From the perspective of the laboratory, however, many scientists find it outrageous that major scientific issues should be decided because the President thinks such-and-such a project is neat, or because a congressman sniffs some pork for the home district.

On the other hand, what are the alternatives? One oft-suggested solution is to institute some kind of national level peer-review system, so that projects on the scale of the supercollider can be evaluated systematically by the scientific community as a whole instead of by ad hoc political infighting. Unfortunately, no one has yet come up with a workable plan for doing that. As Trivelpiece asks, is it really such a good idea to put the future of U.S. science in the hands of a small elite? Indeed, one could argue that a national peer-review system already exists—and that the political system is it. One could even argue that science is inherently political, in the sense that federal support of basic research is itself the result of a political consensus. "The review process exists," says Trivelpiece, "but it is infinitely varied. There are lots of places to make your case, and there is always a second chance."

"It's a confusing and disordered system," he adds, "but it's been very successful. I like it." ■ M. MITCHELL WALDROP

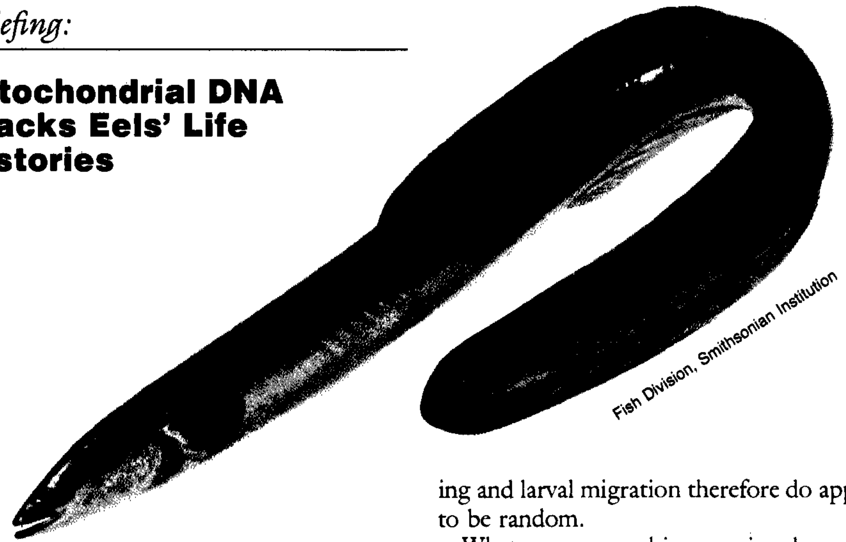
ADDITIONAL READING

C. Quigg and R. F. Schwitters, "Elementary particle physics at the Superconducting Super Collider," *Science* 231, 1522 (1986).

L. M. Lederman, "Science must grow," and S. A. Rice, "Fight the Edifice Complex," *ibid.* 232, 1096 (1986).

Briefing:

Mitochondrial DNA Tracks Eels' Life Histories



John Avise and his colleagues at the University of Georgia have been applying the rich potential information content of mitochondrial DNA (mtDNA) to a series of population genetics problems. Their latest venture involves American and European eels, which pursue a most bizarre life cycle.

These creatures spend their preadulthood in freshwater streams on their respective continents, and then at maturity embark upon a long migration to the tropical mid-Atlantic where they spawn more or less side by side. Examples of fishes making a marine-to-freshwater spawning migration are quite common, but the reverse is rare.

The fact that the American and European eels, named *Anguilla rostrata* (shown above) and *A. anguilla*, respectively, go to the same part of the ocean to breed raises all sorts of issues. For instance, how random is the mating within and even between the populations? And do the larvae find their way back to their continental habitats entirely passively, floating on the Gulf stream as it churns the Atlantic waters in a gigantic clockwise swirl?

Both these factors could potentially affect the genetics of the populations of *Anguilla*, a subject that has intrigued—and puzzled—researchers for half a century. For instance, George C. Williams and Richard Koehn noted a slight difference in allozymes from the Florida to Newfoundland populations, and suggested that it might be the result of local selection. This conclusion must, however, rest on an assumption of random mating among *A. rostrata* and a random distribution of larvae.

Using a series of restriction enzymes, Avise and his colleagues mapped mtDNA digests from eels along this geographic region and found the resulting fragment profiles to be remarkably uniform. Both spawning and larval migration therefore do appear to be random.

What came as a big surprise, however, was the striking difference between the results from American and European eels. Eleven of the 14 enzymes used produced distinct digestion profiles, and the sequence divergence implied by all this was 3.7%, which is substantial.

The two species of eels are virtually impossible to distinguish, the only morphological difference being a difference in the number of vertebrae. Of a series of enzyme loci tested by Koehn and others, only one (malate dehydrogenase) shows a sharp differentiation, and so the idea that the two populations are indeed separate species is obviously in question.

The mtDNA data show a clear genetic distance between the two, which Avise and his colleagues interpret to mean that, although the spawning grounds of the two populations are close together, for the most part they do not mix.

Koehn and Williams have evidence for hybrid populations (based on the malate dehydrogenase locus), which they find in Iceland, a geographical intermediate between the two main populations. The Iceland group might result from a hybrid zone where the *A. rostrata* and *A. anguilla* spawning grounds overlap. How such a hybrid population might also come to occupy an intermediate habitat geographically is still a puzzle.

American eel larvae appear to remain in the water column on their Gulf Stream drift for about a year, compared with between 2 and 3 years for their European cousins. Perhaps a hybrid might be genetically predisposed to drop out at an intermediate time and therefore at an intermediate location? Unfortunately, the Georgia team has not yet obtained mtDNA data from Icelandic eels. ■

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J. C. Avise *et al.*, *Proc. Natl. Acad. Sci. U.S.A.* 83, 4350 (1986).