Gambling on the Supercollider

The high energy physicists have staked their future on a dream machine; now the trick is to make it a reality

On 11 July 1983 the Department of Energy's High Energy Physics Advisory Panel (HEPAP) endorsed a bold and risky plan to regain the American lead in high energy physics: abandon Brookhaven National Laboratory's controversial, half-finished Colliding Beam Accelerator (CBA, née ISABELLE), and set to work immediately on a 12-year, multibillion-dollar effort to build a behemoth some 40 times more energetic than any now in existence—a "Superconducting Super Collider" that will be by far the largest and most expensive scientific instrument in history.

Of course, it is one thing to make a recommendation and quite another thing to make it a reality. In the weeks since their decision the physicists have begun to grapple with the enormity of that task.

The HEPAP plan—actually formulated by a group known as the "Woods Hole" subpanel under chairman Stanley Wojcicki of Stanford University (*Science*, 20 May, p. 809)—is without precedent. The community is gambling its future on a program for which there is no explicit proposal, no design, no site, no research and development plan, no management plan, no management team, no director, no budget, and no guarantee of long-term federal support. Moreover, the supercollider is an enormous extrapolation from current experience.

The largest and most energetic machine now in existence, the brand-new Tevatron at Fermilab, is designed to accelerate protons to 1 trillion electron volts (1 TeV) in a ring of superconducting magnets some 6 kilometers in circumference. The supercollider will accelerate twin beams of protons to some 20 TeV and smash them head-on, using a ring nearly 100 kilometers in circumference. This is very similar in concept to the CBA, ironically enough, just scaled up a factor of 50 in energy; the cancellation of the Brookhaven machine thus means that the physicists have given up their chance to test the magnets and detectors on a smaller scale.

"The community has had no past experience with either of these actions [canceling a half-built accelerator, or embarking on something as ill-defined as the supercollider]," says William Wallenmyer, head of the Energy Department's high energy physics program. "So now we're doing a good deal of talking and thinking, trying to decide how to go about it."

As ambitious as the supercollider may be, however, the high energy community is virtually unanimous in support of it. There is good reason to think that this 10- to 20-TeV energy range may hold the key to a fully unified theory of the fundamental forces (see box, page 1039). Moreover, a number of studies during the last year or so suggest that the supercollider is quite feasible. The Tevatron project—and for that matter, the CBA project—has demonstrated that the technology of superconducting magnets is mature. Indeed, the Tevatron's demand for high-quality superconducting cable has led to the development of an industry that can supply such cable in quantity. While HEPAP did call for 3 or 4 years of preliminary research and development on the supercollider, it was mainly to get the magnet costs down and the reliability up.

"The course is bold, risky, and perhaps foolhardy," says Wojcicki.

However, the biggest single impetus for the supercollider is the vigor of European high energy physics programs. It is not so much a matter of who makes the flashy discoveries, such as the W and Z particles recently found at CERN in Geneva—although scientists are hardly immune to that kind of competitiveness—but of keeping the cutting edge of research. Most physicists consider the American program to be lean but healthy for the rest of the decade. But after that, if Europe keeps on as it has and if nothing else is done here, American physicists see a future of progressive mediocrity and a not-so-gradual brain drain to Europe.

Thus, the supercollider. This machine has so taken hold of people's minds in the last year or two that the CBA, after more than a decade of planning and construction, has come to be seen by many physicists as a diversion, a machine whose time has come and gone. Supporters argued long and bitterly that the CBA was a necessary intermediate step toward the supercollider (*Science*, 22 July, p. 344). But in the end the Woods Hole subpanel decided that two other projects, the Tevatron and the recently approved Stanford Linear Collider, would be sufficient to test out magnets and detectors and to explore the physics at intermediate energies; given the supercollider, the community would simply not have the resources to build these machines *and* the CBA.

The key assumption in all of this, of course, is that the Department of Energy will indeed put the supercollider on a fast track, moving forward with it immediately. Without that the whole plan collapses like a house of cards. As Stanford's Wojcicki says, "Putting the supercollider on a slow track is equivalent to not doing it." Unfortunately for the physicists, however, from Washington's viewpoint the HEPAP recommendation is only that—a recommendation. A lot of key players have yet to agree on it.

The Energy Department, for example, is still digesting the plan internally. The higher-ups have to be convinced. "There's a sensitivity that we not get out in front of the Secretary [Donald Hodel]," says one insider. Only on 11 August, a month after the plan was announced, did the agency give HEPAP the go-ahead to start drawing up a research and development plan.

Meanwhile, the department is trying to figure out what to ask for this fall in its fiscal year 1985 budget submission to the White House. The supercollider research program will need on the order of $40 million per year to begin with, and the tab is certain to rise sharply in succeeding years. With budgets tight, as always, the Office of Management and Budget will doubtless be taking a critical view of the whole thing.

On the other hand, presidential science adviser George A. Keyworth has been urging the physicists all along to "think big." Last spring, for example, Keyworth met with the Woods Hole subpanel in Washington and told them that if a well thought-out proposal were put forward, one that had the backing of the entire high energy community, then he would support it. Furthermore, given that the project would take a decade, a price tag of a few billion dollars might well be feasible.

It is not clear how much this influenced the subpanel's decision—the vote to cancel CBA was close, 10 to 7—but a lot of physicists will be holding Keyworth to his word when it comes time to support the supercollider in the fall budget negotiations.

For the record, however, Keyworth and his staff have professed delight at the HEPAP plan. "I'm proud of my boys [the physicists]," says physics specialist N. Douglas Pewitt. "They were gutsy and made a tough call on CBA and the supercollider. They are not foolish people and they understand the implications." The prospects for research and development money, he adds, look good.

Perhaps so. But budgets, of course, have to be approved by Congress, and there—to the extent that anyone thought about it during the race to summer recess—members and staff have been puzzled and skeptical. From their vantage point the physicists look not unlike 3-year-olds: "You've already spent $150 million on CBA and now you want to drop it for a bigger goody?" goes a fairly typical reaction. "Why should we support you?" The scientists are clearly going to have to do a lot of explaining, especially about CBA.

A key problem for everybody in all this is that the HEPAP plan essentially boxes the government in. It would be extremely awkward now for anyone to put the supercollider on a "slow track" and reinstate the CBA as an interim machine. The community has already officially repudiated the CBA. But once the government cancels it and starts with the supercollider, then no matter how much the thing ends up costing—and the physicists admit that $2 billion is an optimistic estimate—the government will *have* to finish it or in effect give up high energy physics. Are there no alternatives?

HEPAP chairman Jack Sandweiss of Yale University concedes that at the moment there are none. "It wasn't intended that way," he says. "The decision was made on scientific grounds"—namely, that 10 to 20 TeV is where the physics is. "I believe that the supercollider will be very cost-effective," he adds. "The whole purpose of the R & D is cost reduction. But suppose we discover something really new. The cost comes out, say, $7 billion and the supercollider is politically impossible. Then of course we'd reconsider."

Unfortunately, the alternatives have problems of their own. For example, if the supercollider were built to attain 10 TeV instead of 20 TeV, the cost could be cut roughly in half. This is indeed the most likely cost-saving measure. But the risk is that a 10-TeV collider might just miss the important physics, making the whole endeavor meaningless.

The supercollider could also be financed as an international venture. But the wrangling over a site is going to be acrimonious enough with only 50 states in contention; besides, an international project would have to be stretched out another couple of years just for the paperwork and committee meetings. Alternatively, the Americans could furnish the magnets to make a proton machine out of CERN's Large Electron-Positron accelerator (LEP), which should start operating in 1988. That would settle the site problem, but the maximum energy of such an upgraded LEP would be only 5 to 10 TeV. Besides, it would not do much to ease American anxiety about losing the cutting edge.

The final alternative, of course, is simply to give up on the supercollider and admit that high energy physics has finally gotten too expensive. It may come to that—"[Going for the full-scale supercollider] is clearly what makes the course bold, risky, and perhaps foolhardy," says Wojcicki—but the physicists felt they had to take the chance. Their task now is to convince the decision-makers in Washington that they can deliver a supercollider at the price they promised, without massive overruns.

They are wasting no time. The goal is to have a conceptual design for the supercollider ready within the next 3 to 4 years, together with a cost estimate, a plan for execution, and the criteria for site selection. The project will then be ready for a go-ahead from Washington.

"HEPAP has said from the start that this should be a national effort," says Sandweiss. "We're taking that very seriously." Indeed, laboratories such as Brookhaven and Fermilab are already reorienting their in-house research programs toward the supercollider; at the Energy Department's request, HEPAP is setting up an interim committee under Stanford Linear Accelerator Center director Wolfgang Panofsky to get these efforts coordinated.

"Obviously this is not an ideal management structure," says Sandweiss. "Its virtue is that it lets us start to work instantly." Ultimately, he says, there will have to be a permanent management group and director. But here he plans to tred carefully, because the politics suddenly get very delicate. The most straightforward way to keep this "national" effort from bogging down in end-

Why 20 TeV?

For roughly a decade now, elementary particle theory has been stuck on a kind of plateau. The standard unified model of the electromagnetic and weak interactions has been verified abundantly, most recently by CERN's discovery of the W and Z particles at precisely the predicted masses. Quantum chromodynamics likewise seems in good shape as a theory of the strong interactions. But when it comes to more comprehensive unification schemes, the physicists are stuck with a surfeit of bright ideas and no way to sort them out experimentally.

However, there are now a number of hints as to where the resolution may lie. An essential feature of both the electro-weak theory and the so-called grand unified theories, for example, is a family of enigmatic Higgs particles. (They are associated with the "symmetry-breaking" process that makes electromagnetism, say, look so different from the weak force.) Current accelerators have turned up no sign of them. But the theorists can show from very general principles that if the Higgs exist at all, at least one will have a mass on the order of a few TeV. Moreover, even if the Higgs do not exist, alternative unification schemes such as "technicolor" or "supersymmetry" also seem to predict new phenomena in the 1-TeV range.

Thus, it seems the thing to do is to smash particles head-on at 1 TeV or so; presumably there will ensue a host of new insights. Unfortunately, however, life is not that simple. A proton or antiproton boosted to 1 TeV 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 or even 10 TeV.

On the other hand, the 1-TeV figure 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.—**M.W.**

less committee meetings is to put one of the existing laboratories in charge—Brookhaven, for example, which has already built up a world-class team of magnet researchers for the CBA project, and which would doubtless appreciate a sign of respect after the CBA cancellation. But that would just fan the inevitable inter-laboratory rivalries.

Perhaps the vision of Fermilab director Leon Lederman will prevail: "My personal view," he says, "is that we need a young Bob Wilson [Robert R. Wilson, founding director of Fermilab] with the energy and vision and technical sharpness to allocate the tasks and to take advantage of the capabilities in *all* the labs."

Given sensitivity to this problem, it is a blessing that site selection is still so far off. If the battles over Fermilab in the 1960's are any guide, the process promises to be divisive and political in the extreme. (In the interest of the high-tech development, however, oil-rich Texas has already entered an early bid: Texas A&M physicist Peter McIntyre has talked the state into offering to provide the land and the supercollider tunnel.)

Meanwhile philosophical differences have already begun to appear in people's technical approach to the supercollider. Consider, for example, the matter of the superconducting magnets.

In principle, the physicists could start building the machine today, with current technology. In practice, they are going to be putting approximately 10,000 magnets in that tunnel, and those magnets had better be a lot cheaper and a lot more reliable than anything is now. One school of thought holds that the way to do this is to go with existing, well-known, "low-tech" magnets and turn all efforts toward making them cheaper. This is the approach being taken at Fermilab. "Most of the labor in building a magnet is in the ends," notes Lederman, "so the thing to do is build them very long." The 21-foot, 4.5-tesla magnets used in the Tevatron, for example: "We think we can make it 40 feet long for the same costs. We've even thought about 500-foot magnets—you'd just dig the trench and drop them in from above!"

However, another school maintains that with relatively little effort, new technologies can be brought in for major reductions in cost. The superconducting cabling used in the magnet windings is a prime example. "Everybody, including Fermilab and the CBA, used niobium-titanium cable," says Robert Palmer, associate director and chief magnet designer for Brookhaven. "But niobium-tin is only 1 or 2 years away from commercial availability. For the same cost it would give you two to four times the current capability, so the magnets could go up to 6 or 10 tesla"—which means, among other things, a smaller accelerator and fewer magnets.

Meanwhile, although the magnets are getting most of the attention at the moment, people are beginning to worry about other critical issues, such as the detectors. The supercollider has been planned so far as a high-intensity proton-proton machine (as opposed to, say, a proton-antiproton machine), largely because intense beams imply a correspondingly high data rate. Experiments can thus be done quickly, and rare events can more easily be disentangled from the background. But at supercollider energies the rates will be enormous. Can computer and detectors be built to handle it? "*I* certainly don't know how to build a detector to handle 50 million events per second," says Lederman.

In the long run, of course, these technical problems will almost certainly be manageable. Niggling at the back of people's minds, however, is a far more imponderable issue: the supercollider's implications for that vague, unquantifiable, and utterly critical essence known as "vitality." What happens now in that long gap of the late 1980's and early 1990's, when most of the opportunities for entering students will lie in building the supercollider and its detectors rather than in doing the physics itself? Machine design is an exciting and challenging field in its own right, but it is a very different kind of endeavor. What kind of people will it attract?

This is more than an academic question. In the mid- and late 1980's, just as construction of the supercollider gets under way, the graying cadre of leaders who joined the field in the boom years of the 1950's and 1960's will begin to retire. By the time the supercollider is operational, sometime around 1995, the changeover will be complete. The poignant fact is that few of the people now making the decisions on the supercollider will ever use it. The people who do will be, to no small extent, those who are drawn into the field during the rest of the decade.—**M. Mitchell Waldrop**

Whither Brookhaven?

The cancellation of the CBA hardly spells the end for Brookhaven. Unlike the other big accelerator centers, Fermilab and Stanford's SLAC, it is very much a multipurpose laboratory. Brookhaven is the home of the National Synchrotron Light Source, the High [neutron] Flux Beam Reactor, and an 18-MeV tandem Van de Graaff heavy ion accelerator; it is strong in materials research, nuclear physics, theoretical particle physics, medical physics, and chemistry. In fact, high energy physics takes up only about a third of the laboratory's budget, and not even all of that is the CBA. The 29-GeV Alternating Gradient Synchrotron (AGS) is still an important and productive center of research after more than two decades of operation.

But, nonetheless, the CBA decision hurt—badly. The tunnel is complete and ready for the magnets. The magnet factory is complete and ready to go. And suddenly there is no purpose to any of it. "You can't ignore how people feel," says associate director Paul Reardon. "First they suffered the ignominy of being told 'You can't do the job.' So they worked their tails off for 2 years to prove that they *could* do the job, and now they're being told, 'We don't want it!' "

Reardon vows that Brookhaven will stay involved with high energy physics even without CBA. The laboratory had already launched a program to enhance the intensity and reliability of the AGS, for example. Moreover, planners are now taking a fresh look at the idea of filling the empty CBA tunnel with a relativistic heavy ion accelerator: high-energy uranium-uranium collisions are expected to convert the nucleons into a plasma of free quarks and gluons, thus opening up an exciting new field at the interface of nuclear and particle physics.

Finally, the laboratory's highly regarded magnet design group is already turning enthusiastically to the challenge of the supercollider. "Canceling the CBA was one of the dumbest decisions ever made in high energy physics," says Brookhaven director Nicholas P. Samios, never one to keep his opinions to himself. "A drastic mistake has been made. The question now is how to sort it out and get some good physics done."—**M.W.**