Panel says: Go for a multi-TeV collider and stop Isabelle

A high-luminosity proton–proton collider (often called the Desertron) with 10 to 20 TeV in each beam should be the highest priority for the next US particle accelerator, according to all 17 members of the HEPAP subpanel, headed by Stanley Wojcicki of Stanford. By a slim majority, 10 to 7, the subpanel also recommended killing the Colliding Beam Accelerator (formerly known as Isabelle) at Brookhaven.

The Wojcicki subpanel had met at Woods Hole 5–11 June and was to complete its deliberations then so that it could report to the High Energy Physics Advisory Panel at the end of June. Instead the subpanel gave itself a couple of weeks to decompress and ponder because of the gravity of the decision. The final go-round took place at the Nevis Lab from 29 June to 1 July. Interest was so high that one newspaper reporter, camped out at Nevis, was asked to leave the premises. At the postponed HEPAP meeting, finally held on 11–12 July at DOE in Germantown, Maryland, many particle-physics greats were in attendance.

HEPAP chairman Jack Sandweiss (Yale) asked each of the HEPAP members for his vote. Going around the table, all endorsed the recommendation for the Desertron, now to be called the Superconducting Super Collider; some expressed regret that CBA was being killed, noting that the magnet and management problems have been solved at Brookhaven. But all agreed that the subpanel recommendation to kill CBA should be approved. Summing up, Sandweiss said, "It's a very sad thing about CBA. But we have an unwritten pact, 'we agree to agree.' "

In the transmittal letter dated 12 July to Alvin Trivelpiece, director of the DOE Office of Energy Research, Sandweiss wrote that HEPAP unanimously endorsed all of the subpanel recommendations:

▶ The subpanel unanimously recommended immediate initiation of a multi-TeV high-luminosity collider—the Superconducting Super Collider—"with the goal of physics experiments at this facility at the earliest possible date."

▶ The subpanel recommended rapid completion of current construction projects: at Fermilab—Tevatron II, with 1-TeV protons, and Tevatron I, a $p\bar{p}$ collider with 0.8–1 TeV per beam; at SLAC—the SLAC Linear Collider (50-GeV electrons on 50-GeV positrons); at Cornell—the upgrading of the Cornell Electron Storage Ring and associated detectors and the thorough utilization of all existing facilities.

▶ The subpanel recommended that Fermilab not proceed at this time with the proposed Dedicated Collider, which would have occupied a new tunnel with a diameter of at least 4 km and collided 2-TeV protons and 2-TeV antiprotons.

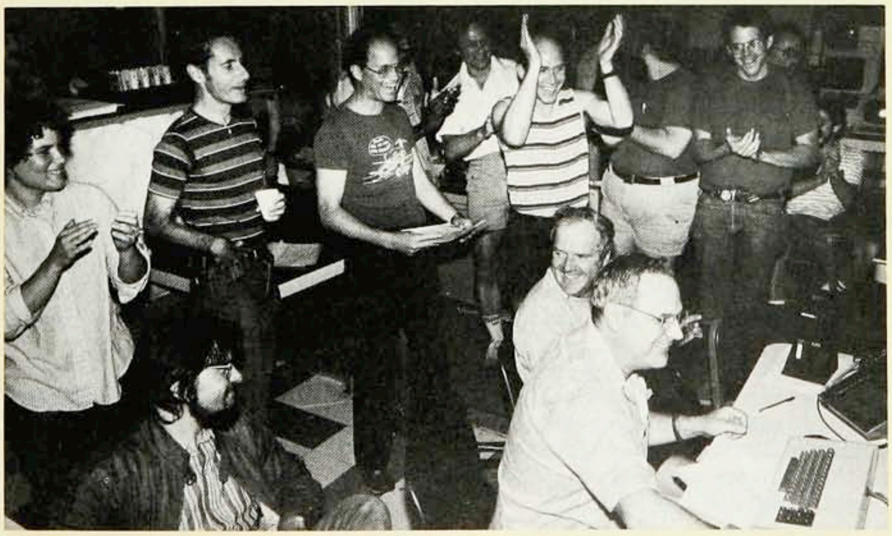
▶ The subpanel recommended by a majority vote that CBA at Brookhaven not be approved.

▶ The subpanel recommended that technology research and development, particularly advanced accelerator research and development, be strongly supported.

The Wojcicki subpanel first met late in February, then held meetings at Fermilab, Brookhaven and SLAC. In addition, the subpanel asked for comments from the members of the APS Division of Particles and Fields, the lab directors, and Federal government officials. Further input came from the Snowmass Summer Study last year (PHYSICS TODAY, January, page 19), a workshop on collider detectors held at Lawrence Berkeley Lab from 28 February to 4 March and a workshop on a 20-TeV (in each beam) hadron collider held at Cornell from 28 March to 2 April.

Presidential Science Adviser George Keyworth had been openly critical of the problems with the Brookhaven accelerator, calling it a "pork barrel squabble" during a talk at the Baltimore APS meeting (PHYSICS TODAY, June, page 45). In the same talk he pushed the idea of a Desertron, saying that "a 20-TeV accelerator should be taken extremely seriously." Some observers feel that the Wojcicki subpanel and HEPAP's actions in July were strongly influenced by Keyworth's cues. However, Wojcicki told us the subpanel decided the case on scientific considerations, not political ones.

The subpanel's highest priority—the Superconducting Super Collider—should have an energy goal of 10 to 20 TeV per beam and a target for comple-



Jubilation at Fermilab on 3 July, as the new ring of over 1000 superconducting magnets was used to accelerate a beam to 512 GeV. The modified synchrotron, "Tevatron II," will eventually produce 1-TeV protons. Operated as a $p\bar{p}$ collider, "Tevatron I," it will have 0.8–1 TeV/beam.

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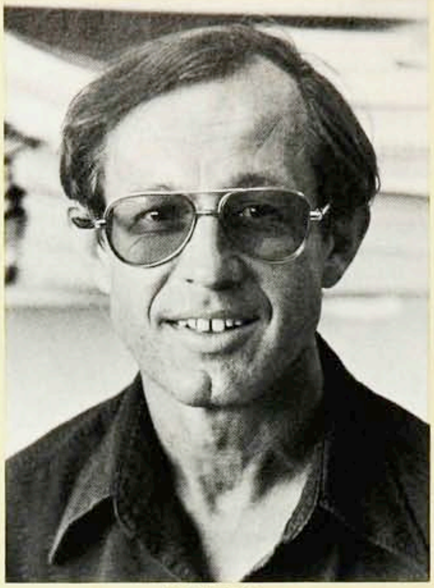
tion in the first half of the 1990s, the subpanel said. To meet such an ambitious time scale, the project must begin in FY 1984 with an intensive R&D program. DOE is rumored to be putting significant funding into its budget proposal for FY 1985.

SSC "has captured the minds of high-energy physicists everywhere. The conjunction of compelling physics arguments for exploration of the mass region up to a few TeV (requiring an order of magnitude higher energy in the center of mass) with the hard-won success of large-scale superconducting accelerator technology creates a unique opportunity for the US," the subpanel report says. Rather general theoretical arguments show that the masses are at most a few TeV for new particles associated with spontaneous symmetry breaking in the standard electroweak theory (for example, elementary Higgs scalars or dynamical bound states). These arguments are applicable to several models, such as technicolor, supersymmetry or compositeness.

Besides the physics requirements, the subpanel said, the scope of the US program should depend on what is happening elsewhere. The Soviet Union has begun building a hadron collider, UNK, whose ultimate energy is to be 3 TeV per beam. The Large Electron-Positron collider, now under construction at CERN, has a 27-km circumference tunnel that could accommodate one or more extra rings of magnets. If high-field superconducting magnets (say 10 tesla) were installed, CERN could build a hadron–hadron facility with 8 to 10 TeV in each beam. The Fermilab Tevatron magnet ring has been installed; Tevatron is to be the first high-energy accelerator resulting from over 20 years of superconducting magnet R&D, including commercial development in the US of high-quality superconducting cable. The subpanel argues, "This technology is now of age." Increasing the energy from the Tevatron's anticipated 1 TeV by a factor of ten or more appears feasible right now, they said. Fermilab has accelerated its first beam with the superconducting magnets in place, achieving 512 GeV at low intensity (about 2×10^{10} particles/pulse). The accelerator was operated continuously at this energy for six hours on 3 July, just in time for the HEPAP meeting.

The subpanel felt that the technology for very high energy hadron colliders is more advanced than for very high energy lepton colliders. Relevant tests on the Tevatron can start now, whereas completion of SLC is still three and a half years away, Wojcicki said. "Hadron machines are more versatile and thus more appealing for a first look." Because of the composite structure of



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hadrons, to make particles of 1–2 TeV would require 10–20 TeV in the center of mass for a hadron–hadron collider. To make intermediate bosons (masses 80–90 GeV) with the CERN $p\bar{p}$ collider (PHYSICS TODAY, April, page 17) required 540 GeV in the center of mass. "We want the highest energy possible to answer the question, is there a desert [with no new phenomena]?" Furthermore, luminosity of 2×10^{32} cm^{-2} sec^{-1} at 20 TeV and 10^{33} cm^{-2} sec^{-1} at 40 TeV would be desirable.

Three different classes of superconducting magnet designs can be considered: super-ferric magnets (2–3 tesla), magnets based on extrapolation of present niobium–titanium technology used at Fermilab and Brookhaven (4–6 tesla), and magnets using high-field, niobium–tin technology (8–10 tesla). For economy, it is widely believed that the pairs of magnets for pp collisions should be placed in a single cryostat. One possibility is the two-in-one-design in which two beam channels are encompassed by a single iron yoke. Such a magnet had been proposed and developed for CBA by Robert Palmer at Brookhaven over the last two years but was considered too untested for use in CBA. Smaller-aperture magnets reduce the total amount of superconducting cable and hence the cost.

The Cornell collider workshop estimated that for a collider with 20 TeV in each beam, a 3-tesla magnet would require a ring 52.3 km in diameter, a 5-T magnet would require 31.4 km, and an 8-T magnet would require 19.6 km.

The total cost of SSC was estimated at less than $2 billion, assuming three or four years of intensive R&D (costing an average of $50 million/year in FY 1983 dollars) before a decision is made. This estimate does not include a fixed-target program nor does it include detectors. The subpanel assumed that the real estate for SSC would be furnished by the host. The hadron collider should be pp rather than $p\bar{p}$ to achieve the highest luminosity, according to the subpanel.

On what timetable could SSC be built? The subpanel heard individual estimates for completion time of R&D plus facility construction ranging from nine to fifteen years. These estimates include five to six years for construction and one to two years for commissioning. A big unknown is the time for decision making, estimated at zero to five years. Wojcicki estimated that the total time is 12 ± 2 years.

The subpanel urged that a director be appointed and a management structure and R&D budget be established at the earliest possible date. "An innovative approach involving Federal government, universities, national laboratories and industry will be necessary." In endorsing this recommendation, HEPAP urged that project planning, cost definition and R&D, followed by construction, "must be a broad-based national effort, centrally managed from the outset." A prime candidate to head the R&D effort (according to corridor talk at the HEPAP meeting) is Maury Tigner, who coordinated the Cornell collider workshop and was both a member of the Wojcicki subpanel and a HEPAP member.

The decision not to approve CBA was a long and painful one. In 1974 Brookhaven had proposed a proton–proton collider called Isabelle with 200 GeV per beam. The 1975 HEPAP subpanel headed by Francis Low recommended authorization of Isabelle in FY 1977. The 1977 HEPAP subpanel headed by Jack Sandweiss recommended doubling the energy in each beam, and Brookhaven went along with the idea. The larger energy of 400 GeV per beam meant the magnets would have to reach 5 T and have a larger aperture than the lower-energy design. At that time Brookhaven had made only one prototype magnet that got anywhere near 5 T—the Mark 5, which had reached 4.8 T. Construction of Isabelle was authorized in 1978; estimated cost was $275 million. Subsequently Brookhaven ran into enormous difficulties producing magnets that reached close to the desired field and could be trained in a reasonable number of quenches (PHYSICS TODAY, April 1981, page 17).

The 1981 HEPAP subpanel headed by George Trilling recommended that Isabelle be completed, provided that particle physics were to receive a certain minimum level of support each year (PHYSICS TODAY, January 1982, page 51). Since then, Brookhaven replaced Isabelle with a new proposal, the CBA, almost a twin to Isabelle—with superconducting magnets up to 5.5 T, 400-GeV protons in each beam, and a design luminosity of 2×10^{33} $cm^{-2}\,sec^{-1}$ in three out of its six interaction regions. CBA would have occupied the Isabelle tunnel and was expected to be done in 1987.

The Wojcicki subpanel agreed with the Brookhaven estimate that CBA construction would cost an additional $218 million. Besides that, R&D and operation would cost $53 million, and detectors (paid for out of the Brookhaven budget) would cost $85 million, totaling $356 million to get CBA running. In deciding the fate of CBA, the subpanel said, "[CBA] represents an issue on which there is no general agreement within the US high-energy physics community. [Within the subpanel] most of the concern centered on the CBA potential for important physics discovery and its possible interference with the rapid SSC construction schedule. After lengthy deliberation, the subpanel voted by a narrow majority against the approval of CBA."

HEPAP chairman Sandweiss said in his letter to Trivelpiece, "Cognizant of the balance of views on this issue and recognizing the importance of agreeing upon goals that are widely supported by the national high-energy physics community, we unanimously endorse the subpanel's recommendation not to approve CBA. HEPAP wishes to emphasize that this recommendation is not a judgment against the superconducting magnet program at Brookhaven. On the contrary, the BNL accelerator R&D team and the new BNL management have been highly successful during the past two years. Brookhaven now has expertise and experience that will be an important resource for the national effort that SSC will require."

The subpanel report summarized the principal arguments for and against CBA. Among the arguments favoring approval: CBA may yield important discoveries in systematic, detailed, high-statistics studies. Recent physics developments, such as the discovery of the W and Z^0 at the CERN $p\bar{p}$ collider, add to the interest in using CBA, for example, in the study of copiously produced W bosons. CBA is capable of operating with polarized protons. It would provide valuable experience for SSC and train a generation of accelerator and experimental physicists familiar with high-luminosity hadron colliders. Terminating CBA might affect the particle-physics community's credibility for obtaining approval of SSC, a much more expensive project.

The arguments opposing approval of CBA included: While difficulties with the Brookhaven magnets caused a two-year lag in the progress of the project, CERN succeeded in storing antiprotons, operated a $p\bar{p}$ collider with a center-of-mass energy comparable to that of CBA (540 GeV instead of 800 GeV), and discovered the W and Z^0. The Tevatron I collider is expected to operate one to two years before CBA with 2 TeV in the center of mass, albeit at lower luminosity. This collider plus a possibly upgraded CERN collider would have explored much of the physics accessible to CBA before CBA would be finished. Theoretical arguments for interesting physics at a mass scale of a few TeV are more compelling than the theoretical arguments for new physics in the CBA mass range. SSC could also address many of the physics questions CBA could study and might be operating six or seven years after the anticipated start of the CBA program. Talented young people are most likely to be drawn into particle physics if they perceive it is at the frontier of discovery, and CBA would not create that perception. CBA would provide valuable experience in accelerator operations and detector development, but some of these studies can be done at Tevatron and at fixed-target facilities.

During the discussion among HEPAP members, Robert Palmer pointed out that the subpanel's cost estimate of $2 billion for SSC did not include the cost of detectors or contingencies. "That is a very optimistic number. And it's not promising that Congress will appropriate it. I'm appalled at the kind of risk you're recommending." Wojcicki commented that this sum was not much different than the $1.7 billion (in FY 1983 dollars) spent on particle-physics construction projects during the decade in which Fermilab was built (although he conceded that during that period other projects were also being built). Sidney Drell remarked that the Wojcicki subpanel differed with previous subpanels about the need for experimental opportunities in the early 1990s. Wojcicki replied, "There's a realization that the scope of experiments is more difficult and requires more effort than people anticipated two or three years ago. Also the time scales of Tev I and Tev II have slipped." Charles Baltay remarked that beyond 1988, 25% of US experimental groups will have no project to work on. Those people could use CBA, he said.

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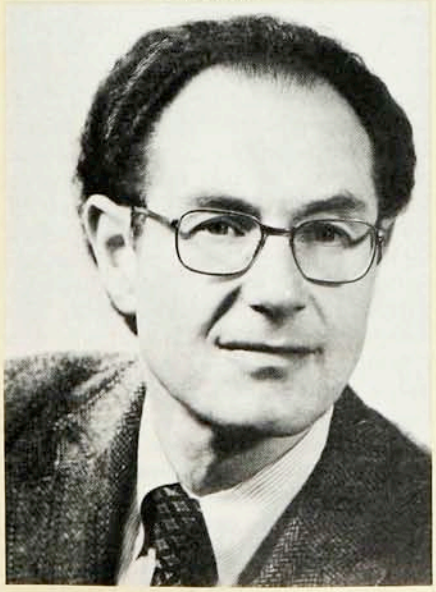

The floor was opened to five-minute comments. Brookhaven director Nicholas Samios, looking grim and speaking softly, joked, "I feel like Zsa Zsa Gabor's latest husband. He knows what to do, but how can he make it interesting?" SSC, he said, "is really a super CBA. But CBA would be one-tenth the cost.... It's an eight-year-old project that'll come in one year late." He said he firmly supports SSC, but "it presupposes the availability of $2–5 billion in 10–15 years. I retire (a young kid) within one standard deviation of the completion date for SSC. At Brookhaven we believe it is a grave mistake not to build CBA."

Samuel Ting said, "Most of my best work hasn't been motivated by theory.... If you know where to look for the W or Z, two events is enough, but if you look for new phenomena, you need 100 events. That places CBA in a unique energy range." Ting thinks the cost of building four or five detectors for SSC would be $1–2 billion.

T. D. Lee said, "If we suddenly drop CBA, who can guarantee that in a few years we won't abandon 20 TeV on 20 TeV for 100 TeV on 100 TeV? How can we justify ourselves to the physics community and the general public? Look at the last 35 years of particle physics. Except for the antiproton and the W, none of the discoveries was anticipated when the relevant accelerator was constructed." In conclusion, he said, "The fact that the proton does not decay shows us that beyond this we know very little." (See page 20.)

Robert Marshak said the high-energy community is very much in favor of an SSC by the end of the century. But CBA would be only a 15% incremental cost. A rational approach, he said, would be for HEPAP to come out for an integrated package, in which the completion of CBA goes hand in hand with intensive R&D work on SSC.

Boyce McDaniel said that the US is in a tough international competition. "We made a couple of bad turns in the past with everybody doing the best they knew how. It's clear we have to be united. There's got to be an aggressive program, and we've got to make sacrifices. The question of who's going to get gored is irrelevant... [Concerning a 20-on-20-TeV collider] we've got to go now, and we've got to go fast... There are a lot of predators hanging around out there. If we don't use the $500 million/year right, they'll take it."

Now that CBA has been rejected, Brookhaven hopes to be one of the principal centers for R&D on the Superconducting Super Collider. Samios is also pushing a multipronged program for Brookhaven particle physics: running the Alternating Gradient Synchrotron 40 weeks/year (instead of 20–30 weeks/year) for the next few years; upgrading the AGS intensity by a factor of two to ten and using it for neutrino physics and the study of rare K decays, and possibly converting the CBA tunnel into a heavy-ion collider with greater than 15 TeV in the center of mass, provided the nuclear-physics community supports such a collider. —GBL

Proton decay not seen at predicted rate

In the year that has already seen the discovery of the three intermediate vector bosons of electroweak unification, it would be wonderful to report that proton decay has also been verified—establishing a grand unification of the weak, electromagnetic *and* strong interactions in one encompassing theory. But, alas, it appears we will have to be patient.

The 8-kiloton water-Cherenkov proton-decay detector built by the Irvine–Michigan–Brookhaven collaboration 2000 feet underground in a salt mine near Cleveland began operation in August 1982. With 130 full days of running now analyzed, the IMB group has not yet seen any evidence of proton decay. The most attractively simple of the grand unified theories—the "minimal SU(5)" unification proposed by Howard Georgi and Sheldon Glashow at Harvard in 1974—predicts a proton lifetime of about 2×10^{29} years, uncertain by perhaps more than two orders of magnitude. The dominant decay mode in minimal SU(5), $p\rightarrow e^+\pi^0$, is expected to account for about 40% of all proton decays. This is in fact the decay mode to which the IMB detector is most sensitive. After 130 days, corresponding to an exposure of 4×10^{32} proton years, one would have expected to see almost *a thousand* $p\rightarrow e^+\pi^0$ events if one believes the central values of the minimal SU(5) predictions.

Early IMB results indicating that minimal SU(5) was in trouble were reported by Maurice Goldhaber (Brookhaven) at the January APS meeting in New York (see PHYSICS TODAY, April, page 35.)

Before one pronounces minimal SU(5) dead, it should be noted that there is some disagreement about the theoretical uncertainties one ought to attach to these explicit predictions. Everyone, however, agrees that the uncertainty in the lifetime prediction is considerable. Although proton decay is an excruciatingly weak process, it is the computational difficulties of the *strong* interactions that cause the trouble. One doesn't really know how to calculate the wavefunction of the bound quark distribution in the proton. Furthermore, the key empirical parameter of the minimal SU(5) calculations, the quantum-chromodynamic scale factor Λ, which determines the energy dependence of the strong coupling, is not well determined.

The larger the uncertainty one assigns the theoretical prediction, the less stark is the present conflict between minimal SU(5) and the IMB data. If one believes the widely accepted estimate by William Marciano (Brookhaven) that the SU(5) proton-prediction is uncertain by a factor of fifty, the prediction becomes $\tau_{e\pi}=4.5\times10^{29\pm1.7}$ years, where $\tau_{e\pi}$ is the proton lifetime divided by the branching fraction for the $e^+\pi^0$ mode. Taking the upper limit of this prediction, the IMB group should have seen at least eleven proton decays to $e^+\pi^0$ during the 130 days in which they saw none. The group therefore now quotes a lower experimental limit on $\tau_{e\pi}$ of 1.0×10^{32} years at the 90% confidence level.[1] As the salt-mine detector continues running, this limit will of course increase linearly with exposure time, so long as no decays or background events indistinguishable from proton decays are seen.

The IMB water-Cherenkov detector, with a fiducial mass (the region in which proton decay vertices are sought) of 3300 metric tons of water, is by far the largest of the proton-decay detectors now in operation (see PHYSICS TODAY, January 1980, page 17). Its total sensitive water mass of 8000 tons is monitored by 2048 photomultiplier tubes arrayed just inside the surfaces of this 20-meter cube of highly purified water, looking for Cherenkov light from nucleon decay products. This arrangement is particularly well suited for the $e^+\pi^0$ decay mode of the proton because both the positron and the two photons from the decay of π^0 would generate strong electromagnetic showers in the water, producing copious Cherenkov light. In addition to providing the Cherenkov light, the water also serves as the source of the decaying nucleons—free protons and bound protons and neutrons in the oxygen nuclei.

Two smaller detectors did in fact report tentative indentifications of nucleon decay events about a year ago, quoting lifetimes and branching fractions apparently inconsistent with the present IMB limits. But since that time, neither the Kolar gold-mine detector in India nor the Italian–CERN detector in a tunnel under Mont Blanc have found any new decay events.

Decay to μ^+K^0. The candidate event that excited the greatest interest was an apparent $p\rightarrow\mu^+K^0$ decay recorded shortly after the Mont Blanc detector began running last year.[2] Both the Mont Blanc and Kolar detectors consist of layers of iron interleaved with track-recording layers of gas tubes. The Mont Blanc candidate appears to have three fully contained, charged tracks emerging from a vertex in the detector—presumably the μ^+ and a pair of charged pions from the decay of a short-lived K^0. The measured track momenta are consistent with the decay of a proton in an iron nucleus to μ^+K^0. However, because two of the tracks run almost parallel to the layers (the direction of poorest resolution in these anisotropic detectors), the recorded event is also consistent with the collision of a cosmic-ray neutrino with a neutron (in an iron nucleus), producing only two charged tracks.

Another problem with believing the Mont Blanc μ^+K^0 event is its apparent inconsistency with the absence of clear evidence for such proton decays in the much larger salt-mine detector. But the Mont Blanc detector is at least four times more efficient at seeing μ^+K^0 than is the IMB water-Cherenkov system. If the Mont Blanc group has seen one $p\rightarrow\mu^+K^0$ decay in 40 fiducial-ton-years of running, the IMB detector cannot be expected to have seen more than four in the 80 days (700 ton-years) that have been analyzed for this mode; and indeed they do have one ambiguous candidate. "There is not *yet* any real disagreement between us and IMB," says Ettore Fiorini (Milan).

Although the branching fraction for proton decay to μ^+K^0 is predicted in minimal SU(5) to be only a few percent, it becomes far more important in modified versions of SU(5) and in "supersymmetric" grand unified theories. The IMB group has therefore made considerable effort to look for this decay mode, even though the water-Cherenkov detector has a much higher efficiency for seeing $e^+\pi^0$.

At two conferences in April, the GUTs Workshop in Philadelphia and the APS Spring Meeting in Baltimore, Bruce Cortez and William Foster (both at Michigan) reported on the search for μ^+K^0 decays in 80 days of running. Having seen one ambiguous event during this period, equally consistent with proton decay *or* neutrino interaction, the group sets a lower limit of 1.3×10^{31} years on $\tau_{\mu K}$, the partial proton lifetime for this decay mode.

When the K^0 from proton decay to μ^+K^0 decays to two neutral pions, lots of Cherenkov light would be generated.