CLNS 02/1784

# A Personal History of CESR and CLEO

Karl Berkelman

updated October, 2001

**Abstract**

This is a chronology of events for the Cornell Electron Storage Ring and its main detector facility CLEO from their beginnings in the late 1970's until the end of data taking above the $B$ meson threshold in June, 2001. It grew out of a talk I was asked to give on the occasion of Maury Tigner's first retirement in 1995 and was updated six years later. I call it a *personal* history because it is based mainly on my recollections and on documents readily available to me; it may therefore emphasize unduly events in which I was personally involved. It is not meant to be systematic or complete, and there may be inaccuracies or lapses in my memory. I wrote it for physicists, particularly for new members of the Laboratory or the CLEO collaboration who may be curious about how we got where we are. If you are reading a paper copy of this with black and white figures, you should know that it is possible to download a copy with colored figures from `http://www.lns.cornell.edu/public/CLNS/2002/`.

# Contents

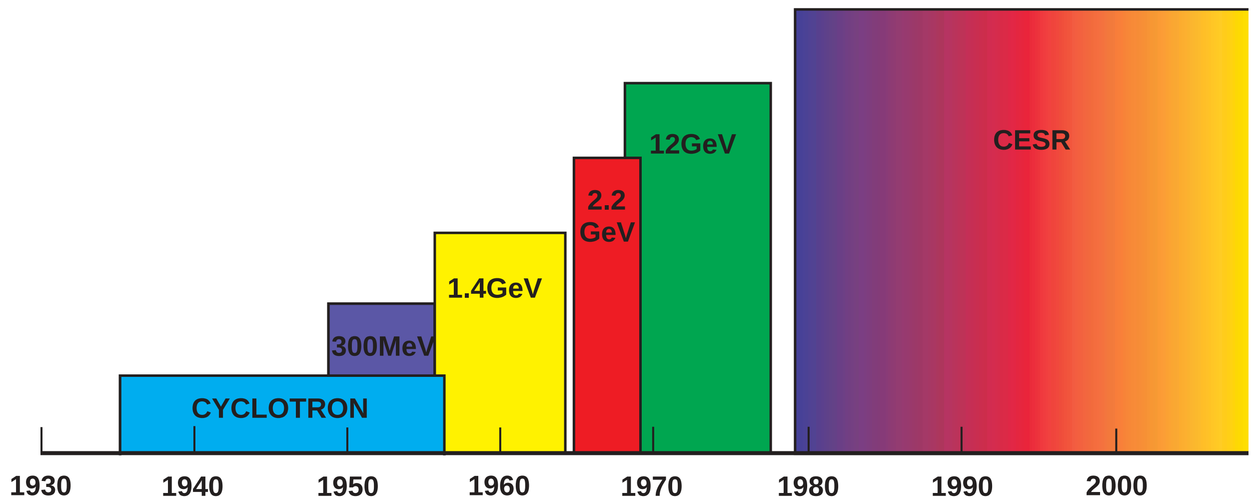



Figure 1: Chronology of accelerators at Cornell.

# 1 CESR Prehistory, up to 1975

The Cornell University Laboratory of Nuclear Studies has managed to keep itself at or near the forefront of particle physics for the fifty years since its founding by periodically rebuilding its accelerator facilities (Fig. 1). By the mid 1970's we had been operating for about seven years the 'Ten GeV Machine' [2], the fourth in a series of Cornell electron synchrotrons of increasing energy [3, 1]. We had built the Ten GeV in a half-mile circumference tunnel about fifty feet below Upper Alumni Field, the intramural football fields at Cornell (see Fig. 1). The early experimental program was carried out mainly by physicists from Cornell, Harvard, and Rochester, and included a wide-angle bremsstrahlung test of QED using the internal electron beam impinging on a target in the beam chamber, and a number of photoproduction experiments using the external bremsstrahlung beam: wide-angle $e^+e^-$ and $\mu^+\mu^-$ tests of QED and production of $\pi^+$, $\rho^0$, $\omega$, $\phi$, and $\psi$ mesons.

Once QED had passed the standard list of high energy tests, and the photoproduction cross sections of the low-lying mesons had been measured, we concentrated attention on meson electroproduction using an extracted electron beam. During this period the main competition was coming from the Stanford 20 GeV electron linear accelerator, which had opened up the field of deep inelastic electron-nucleon scattering. We were motivated by the desire to see what the nucleon fragments looked like in the rather copious yield of pointlike electron-parton collisons. The electrons in the lower energy Cornell machine didn't really have short enough wavelengths to resolve the constituents of the nucleon and explore the deep inelastic kinematic range, though. The electroproduction cross sections were dominated by virtual-photon-plus-nucleon energies in the nucleon resonance region, and the interaction of the photon with the target was telling us more about the vector meson nature of the photon than about the more interesting pointlike constituents of the nucleon. This prompted us to

Figure 2: 1994 aerial view of Cornell University and Cayuga Lake, looking NNW. The oval shows the location of the tunnel for the 10 GeV synchrotron and the Cornell Electron Storage Ring. The building at the south side of the ring is Wilson Laboratory; Newman Lab is at the left edge of the picture, just in front of the seven-story chemistry research building.

upgrade the beam energy from 10 GeV to 12 GeV by adding more rf cavities, but 12 GeV was still much smaller than the 20 GeV available at SLAC.

One advantage we could claim over Stanford was the fact that coincidence experiments, such as required to see the nucleon fragmentation products along with the scattered electron, were much easier with the few percent beam duty cycle of a synchrotron than with the $10^{-5}$ duty cycle of the Stanford linac. The SLAC physicists, however, were getting clever at overcoming this obstacle and Perl's group had actually performed a successful multiparticle coincidence electroproduction experiment. Moreover, the physics of the hadronic final states in deep inelastic scattering was turning out to be rather uninteresting. Once a parton had been punched out of a nucleon, it fragmented into a hadron jet in a way that depended mainly on the total energy available, with little or no memory of how it was produced. Multiplicities, for example, were insensitive to the $q^2$ of the virtual photon. High energy electroproduction final states looked just like the debris of any high energy hadronic collision.

So we began to look for something better to do. Lou Hand went off to do deep inelastic muon-nucleon scattering at much higher energies at Fermilab. Bernie Gittelman (in 1975-76) and I (in 1974-75) spent sabbatics at the DORIS $e^+e^-$ storage ring at DESY. Maury Tigner started up a research group to develop superconducting rf cavities, which would be the only plausible way to make a significant energy gain for the synchrotron. The proposal for 1974-75 NSF funding included a section on "Program for Energy Increase" which mentioned that "A new guide field with about one third of the perimeter dedicated to accelerating cavities might permit operation to the level of about 25 GeV." A superconducting rf cavity had already been successfully tested in the synchrotron in 1974.

The idea of building a storage ring for beam-beam collisions had been in the air at Cornell ever since Gerry O'Neill first suggested it in the 1950's. Bob Wilson had assigned to Maury Tigner in 1959 the task of building a table-top electron storage ring (Fig. 3) as a PhD thesis topic in accelerator physics. This was at the same time that Bernie Gittelman was participating in the operation of the first $e^-e^-$ storage rings at Stanford, and the first $e^+e^-$ rings were being built at Frascati and Novosibirsk. Although Tigner had made a preliminary conceptual design for a Cornell $e^+e^-$ storage ring in 1973 (see Fig. 4), there was still a lot of skepticism at Cornell as to whether one could store enough beam to enable one to do more than just a total cross section measurement, and indeed whether there was any useful physics beyond checking QED. As time went on and storage ring data came in from the Adone ring at Frascati, from the CEA Bypass, and eventually from the SPEAR ring at SLAC, some of us became convinced that the future of the Cornell lab lay in building a storage ring in the 10 GeV tunnel, using the synchrotron as an injector. In fact, the CEA Bypass data on the total $e^+e^-$ cross section, published in 1973, surprized everyone by showing a rise with increasing energy instead of the expected $1/E^2$ dependence. I recall Bjorken's talk at the Bonn conference in August, 1973, in which he speculated on the existence of a fourth quark.

The Cornell interest in storage rings was considerably reinforced by the November Revolution, that is, the 1974 discovery of the $J(=\psi)$ at the AGS by Ting and company, and the discovery of the $\psi(=J)$ at SPEAR by Richter and company. The sight of that colossal resonance at 3.1 GeV $e^+e^-$ energy convinced the doubters here at Cornell, including McDaniel,

Figure 3: Three quadrants of the storage ring that Maury Tigner built for his PhD project.

that there was exciting physics in electron-positron collisions and set us on the course to building CESR. The most convincing physical interpretation was that the CEA had seen the threshold for a new 'charmed' quark and that the $\psi$ was a $c\overline{c}$ bound state.

The energy of a Cornell ring would follow from the circumference of the existing tunnel. The economics of the rf power requirements dictated that it would have to be somewhat lower than the synchrotron energy, say 8 GeV per beam. Moreover, since DESY and SLAC were thinking about larger rings, in the 14 to 18 GeV range, it seemed plausible that an 8 GeV ring could fill a niche between them and the 2.5 GeV per beam available at SPEAR. But could we convert the synchrotron to serve as an efficient injector?

Although the question as to whether there would be useful physics for a Cornell collider seemed to be settled, the question of whether one could inject enough positrons from a synchrotron remained. SPEAR, the most successful storage ring, circulated a single bunch of electrons and a single bunch of positrons. Positrons were produced in a showering target part way down the two-mile SLAC linac and then accelerated to the SPEAR energy in the reversed phased remainder of the linac. Since the linac injector for the Cornell synchrotron only had 150 MeV total energy, a target part way along its length would produce a relatively meager flux of positrons, and it would take much too long to build up a single intense bunch in the storage ring by repetition of the sequence: single bunch positron production, acceleration in the linac, acceleration in the synchrotron, and injection into the storage ring.

Maury Tigner came to the rescue by inventing a fiendishly clever 'vernier coalescing' scheme (Fig. 5). Although the Cornell linac could make only a rather low number of positrons in a single bunch, it would take no longer to fill the storage ring with about 60 such bunches,

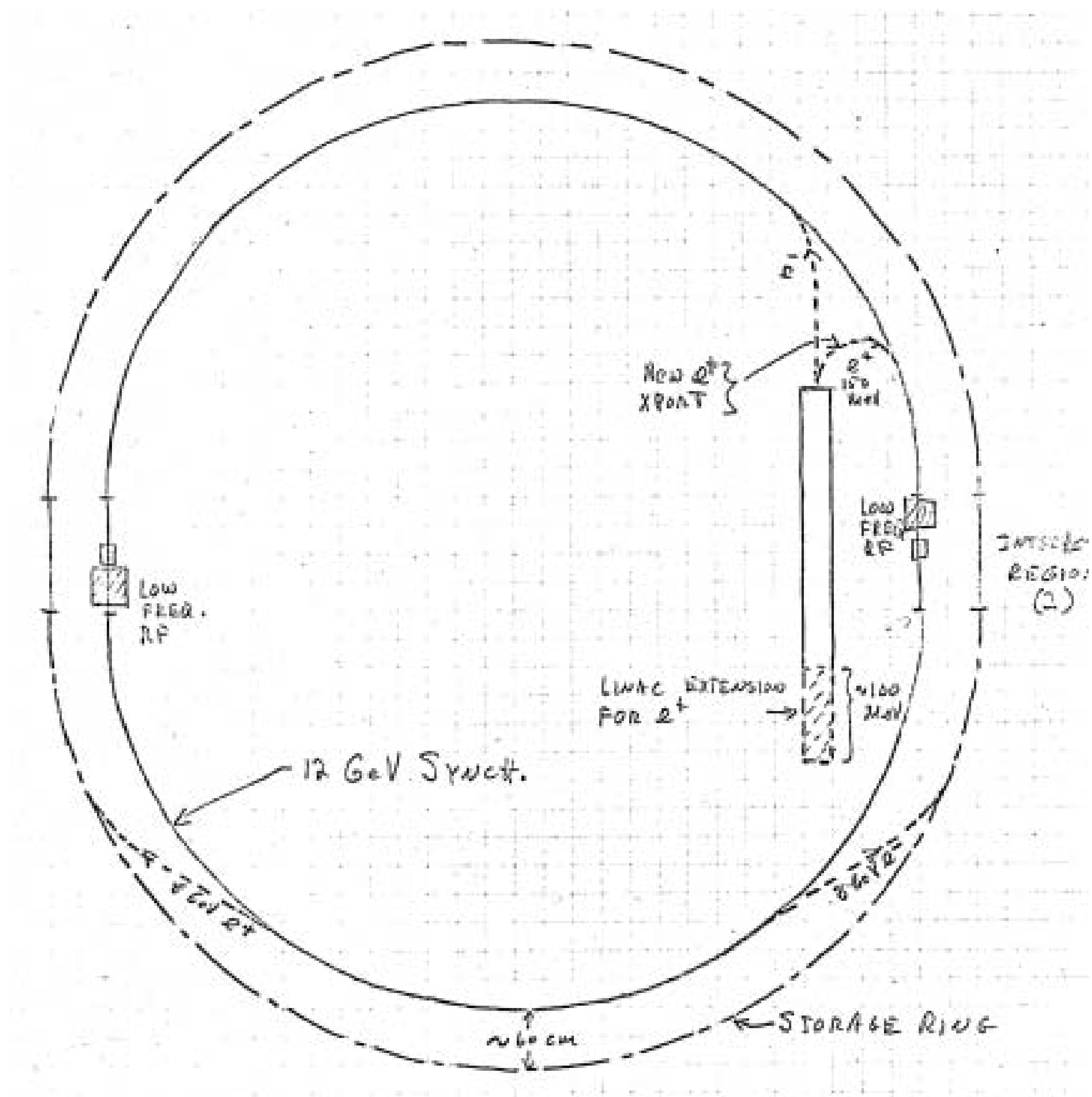



Figure 4: The first drawing of CESR, from "A possible $e^+e^-$ storage ring for the Cornell synchrotron", by Maury Tigner, April 1973.

equally spaced around the ring. Suppose the storage ring was designed to have 61/60 times the circumference of the synchrotron. You could then extract bunch #2 from the storage ring, send it back to the synchrotron for one time around, inject it again into the storage ring and it would fall on top of bunch #1, which had been 1/61 of the circumference ahead of it. Then bunch #3 would be diverted through the synchrotron for two circuits, again falling on top of bunch #1 in the storage ring, and so on until all of the 60 bunches were coalesced into one intense bunch. The whole coalescing procedure could be done in a few seconds. It required very fast pulsed magnets to accomplish the ejection and injection with the whole sequence under precise computer control, but there was no reason why it couldn't be done. This was what we needed to convince ouselves and the rest of the physics community that we had a practical plan for achieving the required beam currents.

One bunch of electrons and one bunch of positrons circulating in opposite directions along the same path will collide at two diametrically opposite points. By correct phasing of the bunches we could arrange one of the two intersection points to occur in the large L-0 ('L-zero') experimental hall on the south side of the ring. The other would occur in the much smaller L-3 area in the north. In the tunnel the new ring would be on the outside wall, opposite the synchrotron, and their beam lines would be 1.5 m apart typically (Fig. 9 top). There was room in L-0 to make a bulge in the storage ring layout to bring the intersection point far enough away from the synchrotron to accommodate a large detector, but in the north the two rings would be no further apart than they were in the tunnel.

Six months after the announcement of the discovery of the $\psi$, in May 1975 the Lab submitted "A Proposal to the National Science Foundation for Construction Funds to Modify the Cornell Electron Synchrotron Facility to provide an Electron-Positron Colliding Beam Capability". The text of the document summarized the CESR design parameters, magnet, vacuum system, rf system, injection, and controls [5]. The luminosity goal was $10^{32}$ $cm^{-2}sec^{-1}$ at 8 GeV per beam, the same as for the higher energy PETRA and PEP rings proposed at that time. The total project cost, estimated at $16.8 million, did not include detectors, but it was stated that "Very sizeable capital investment and annual operating costs will be required to provide the experimental equipment and support the staff of the various experimental programs." The stated physics goals included heavy quarks and leptons, spectroscopy of hadronic resonances, hadronic fragmentation, electroweak effects in annihilation processes like $e^+e^- \to \mu^+\mu^-$, photon-photon collisions, and high energy tests of QED. Also discussed were prospects for a synchrotron radiation facility. The total cost included $1.1 million in civil construction to enlarge the north experimental area.

What would the machine be called? We needed a shorter name than the Cornell Storage Ring. Contemporary machines had been given acronyms like DORIS, SPEAR, PETRA, and PEP. For a while it was open season on creative names. One of the wackiest I remember was suggested by Hywel White: **CORN**ell **CO**lliding **B**eams, or CORNCOB. Eventually, McDaniel ended the debate with CESR, the **C**ornell **E**lectron **S**torage **R**ing, pronounced like "Caesar". The name has weathered well, and has spawned others via Caesar's Egyptian connection: CLEO for the experimental detector and the collaboration that operates it, NILE for a computing project involving the CLEO data stream, and SUEZ for the data

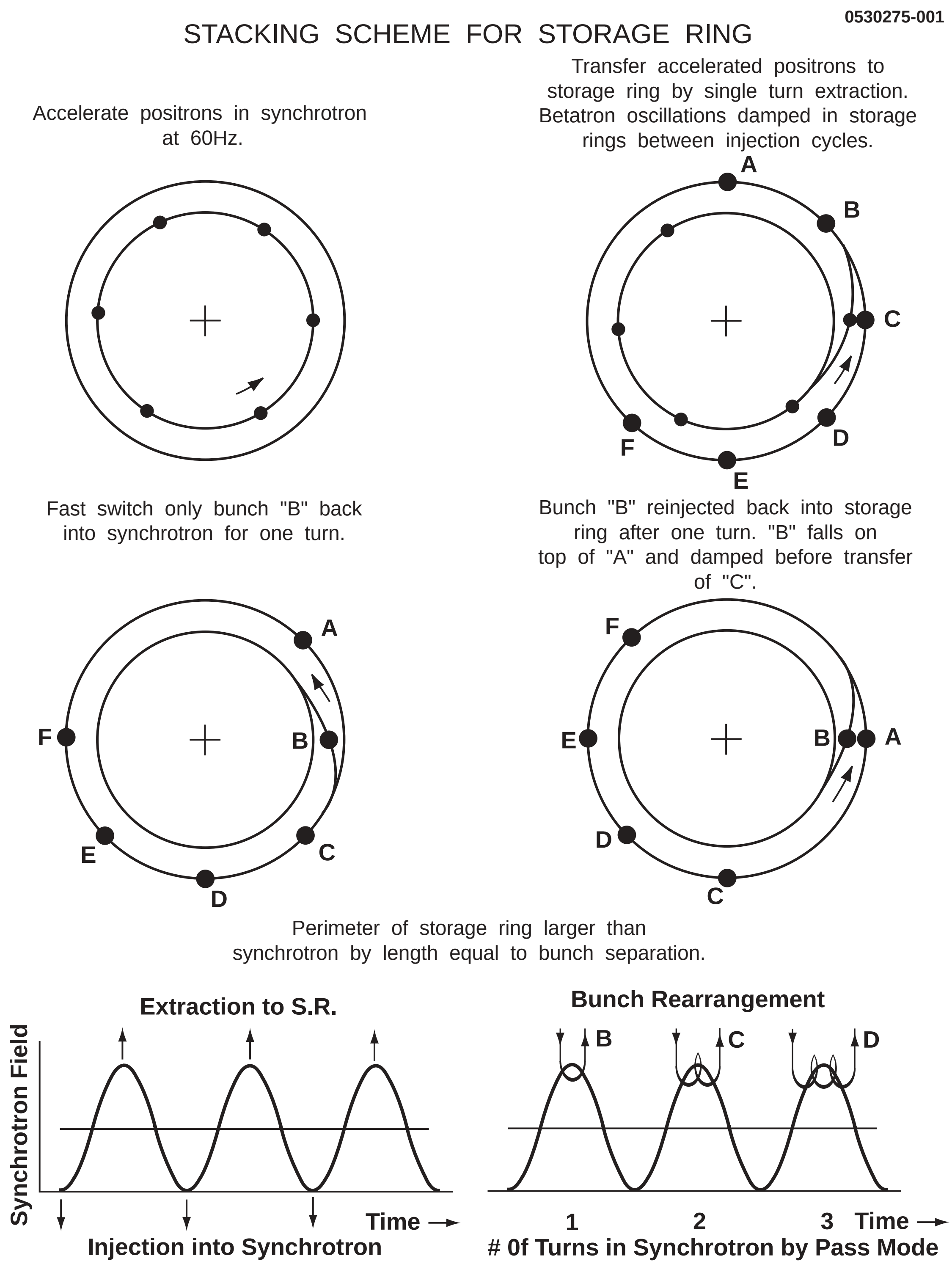



Figure 5: Diagram explaining the vernier coalescing scheme for positron injection into CESR, from "Improved method for filling an electron storage from a synchrotron", by Maury Tigner, CLNS-299, February 1975.

analysis program.

## 2 CLEO Prehistory, 1975-77

Besides a plan for the storage ring, we needed plans for two experimental detectors, one at each interaction point. The tradition at Cornell and other fixed target accelerator facilities had been to entertain proposals for experiments. The winners in the competition for approval would set up their apparatus, which would be torn down and dispersed as soon as the proposed measurements had been made. Over the years the experiments had become more complicated, the collaborations larger, and the equipment more expensive. The idea had evolved at several labs that the most complicated apparatus would serve as a semipermanent, multipurpose, Laboratory-managed facility for the use of many experimenters in a long series of measurements. We decided that the south area was appropriate for such a facility, to be planned, built, and exploited by a collaboration which would be open to all comers as long as the total number did not get too unwieldly. The smaller north area would be opened for competitive proposals by preformed collaborations.

So some time in 1975 a 'South Area Experiment' study group started to form out of the Cornell 10-GeV Synchrotron user community, consisting of faculty, post-docs, and graduate students from Cornell, Harvard, Rochester, and Syracuse. Groups from Rutgers and Vanderbilt joined a little later, as well as individuals from Ithaca College (Ahren Sadoff) and LeMoyne College (David Bridges). We met under the chairmanship of Al Silverman and started to consider the various options for detector technologies.

There was already a bewildering array of possibilities that had been considered by various summer study groups around the world, and some of the detector styles had even been built and used at Stanford, Hamburg, Frascati, and Novosibirsk. First, there had been the nonmagnetic detectors with planar tracking chambers and shower hodoscopes. Then there were magnetic spectrometers along the lines of those used in some fixed target experiments. The latest was the Mark I solenoid-based detector with cylindrical tracking chambers. Other magnetic field configurations, longitudinal, transverse, and toroidal, were being considered. There were serious limitations in every option; there was no detector that would excel in all respects. One had to make serious compromises between what was desirable for the physics capabilities and what one could expect to build with available resources.

It was assumed from the beginning that we would need a magnetic detector to achieve good momentum resolution for charged particles, and that the acceptance solid angle should be as near $4\pi$ steradians as practical. Of the various configurations, the solenoidal style eventually won out. It promised a large acceptance solid angle for charged particle tracking without encumbrances, and the uniform magnetic field would simplify the track recognition and the momentum determination. However, the resolution would be poor for tracks at polar angles $\theta$ near zero and 180°. To get the best momentum resolution the solenoid coil would have to be big and expensive, and the other detector elements, particle identification, shower counters, and muon detectors, would have to be even larger. To keep costs down, all but the tracking chambers would have to be outside the solenoid coil, and we would have to contend with the interactions of the particles as they passed through the coil. And because of their size, the outer detector elements would have to be made using low-tech, cheap technology. Still, it was probably the best choice.

The solenoid coil was a problem. We needed a high magnetic field for momentum resolution, but we had to minimize the thickness. The obvious solution seemed to be a thin superconducting coil. The TPC group at Berkeley was developing one for their detector at PEP, so we assigned a Cornell post-doc, David Andrews, to the job of copying their design and getting us a 1.5 Tesla, 1 meter radius, 3 meter long solenoid. It was clear, however, that this was not going to happen quickly, so we decided to build a temporary conventional 0.5 Tesla coil as well.

For the tracking chamber, we chose a cylindrical drift chamber of 17 layers of square cells, alternating axial and 3° slanted wire layers to get stereo information. This was inspired by the drift chamber for the Mark II detector, which was being planned at that time to replace the Mark I at SPEAR; Don Hartill had just spent a sabbatic at SLAC participating in the design. In the space between the drift chamber and the beam pipe we planned a proportional chamber with cathode strips to measure the track $z$ coordinate parallel to the beam line. Each component of the detector got a two-letter mnemonic to identify it in the software, and was usually referred to by these two letters in the local jargon. The big cylindrical drift chamber was 'DR' and the inner $z$ chamber was 'IZ'.

Outside the coil and inside the iron of the flux return interleaved with planes of drift chambers for 'MU' muon detection, the detector was to be arranged in octants, each octant consisting of a separate trapezoidal box containing, in outward order, the 'OZ' planar drift chamber to track each charged particle after it had passed through the coil, a device for particle identification, a plane of scintillation counters 'TF' for triggering and time of flight measurement, and the 'RS' shower detector array of alternating lead sheets and proportional tubes. To extend the solid angle for photon detection we also covered the ends of the solenoid with similar 'ES' shower detectors mounted on the iron poles, and another set of such detectors 'CS' mounted at the ends of the octant modules.

While these components were agreed on with a minimum of controversy, we were not able to agree on a scheme for high momentum charged particle identification. Measuring pion, kaon, proton, electron, and muon masses was possible by combining the measurement of momentum $p$ by curvature in the magnetic field with the measurement of velocity $\beta$ by time of flight ($m = p\sqrt{1-\beta^2}/\beta$), but only up to $\beta \approx 0.95$ above which their flight times became indistinguishable. Thus pions and kaons of the same momentum could not be separated above 800 MeV/c using just momentum and time of flight. Two other physical processes that depend on particle velocities are energy loss by ionization $dE/dx$ and the Cerenkov effect, and there were partisans for each. The Harvard group proposed to use high pressure gas Cerenkov counters. They chose the gas pressure to give an index of refraction $n$ that would yield a threshold value, $\beta_{min} = 1/n$, such that for momenta in an interesting range pions would count and kaons would not count. Provided we could solve the light collection problems, achieving this limited goal would be straightforward. The more ambitious and risky $dE/dx$ scheme, championed by Vanderbilt and Cornell (Richard Talman), promised to distinguish pions and kaons cleanly to momenta slightly higher than possible with time of flight, and also provide some minimal separation in the $dE/dx$ relativistic rise region, $p > 1.2$ GeV/c. Moreover, since one would have a $\beta$ measurement rather than a signal only when $\beta > \beta_{min}$, the device would help in separating other particle species, for example, $p$ versus $K$ and $e$ versus $\pi$. The actual performance, however, would depend critically on the resolution in the measurement of $dE/dx$; could we achieve 6% r.m.s.? We postponed the decision by agreeing to equip two octants with high pressure 'CV' gas Cerenkov counters and two octants with 'DX' proportional wire chambers, so we could try them both out for a while before committing all the octants. Later, Steve Olsen of Rochester suggested building simple low pressure gas Cerenkov detectors to fill the empty octants temporarily and help distinguish electrons and pions by velocity threshold. While the inner part of the detector demonstrated general agreement within the collaboration, the outer components, with three different kinds of particle identification, reflected the conflicts.

The collaboration needed a catchy name. Other collaborations tended to have acronyms like DASP, LENA, TASSO, JADE, DELCO, or were named for 'personalities' like PLUTO, or had unpronounceable initials like TPC, or HRS. It was a graduate student, Chris Day, who suggested 'CLEO', short for Cleopatra. To make it into an acronym no one could think of a better set of words than 'Cornell's Largest Experimental Object' or '...Operation' or '...Organization', so we decided not to make it an acronym — it's just a name. By now, people have stopped asking what it stands for.

Meanwhile, plans for the north area experiment were being resolved. A call for proposals was sent out, and on February 15, 1978 the Program Advisory Committee met to consider the submissions from three groups: Columbia + Stony Brook, Chicago + Princeton, and the University of Massachusetts. All three involved compact nonmagnetic detectors emphasizing calorimetry, quite complementary to the CLEO detector design. The winning entry was the sodium iodide and lead-glass array proposed by the group from Columbia and Stony Brook with Leon Lederman as spokesman. Lederman left almost immediately after to become director of Fermilab and the direction of the collaboration, called CUSB, was taken over by Paolo and Juliet Franzini.

# 3 Construction, 1977-79

In the summer of 1975 a HEPAP Subpanel under the chairmanship of Francis Low met at Woods Hole to advise ERDA, the predecessor of the DOE, on new high energy physics facilities. Even though the NSF was not bound to follow the recommendations of HEPAP or its subpanels, it was important for us to get an endorsement for CESR. There were four project proposals on the table: the PEP 18 GeV $e^+e^-$ ring for SLAC, the ISABELLE 200 GeV $pp$ collider for Brookhaven, the 'Energy Doubler/Saver' superconducting-magnet proton synchrotron ring for Fermilab, and the CESR proposal from Cornell. The previous year's subpanel under Victor Weisskopf had already endorsed PEP for construction and had recommended continued r&d efforts for ISABELLE and the Fermilab ring, however the PEP project had not yet received funding.

Figure 6: Joe Kirchgessner and the body of the Mark I CESR 14-cell rf cavity.

I was a member of the Low Subpanel, but I was in the minority on most issues. Most of the members felt that getting PEP launched was the first priority, and that it was therefore impolitic to recommend a second, cheaper $e^+e^-$ collider as well. So in the final priority list CESR came in fourth. PEP was reaffirmed for construction, ISABELLE and the Energy Doubler/Saver were recommended for increased r&d funding, and although there was some faint praise for the Cornell proposal, the report said, “we do not consider the construction of a second electron-positron colliding beam facility as one of our highest priorities.”

This was a dark moment for CESR, but McDaniel was undaunted. Once the PEP project obtained government approval a few months later, he contacted individually the members of HEPAP and the Low Subpanel and got many of them to support the CESR proposal. To allay fears of embarking on a whole new facility, the project was officially called a *conversion* of the existing synchrotron facility, and assurances were given that the experimental program would have greater participation by non-Cornell groups. We were very fortunate in getting the enthusiastic support of Al Abashian and Marcel Bardon at the NSF Physics Division [6],[10]. During 1976 and 1977 the NSF provided enough funds for a vigorous program of prototyping and firming up the design. We were even able to lengthen and upgrade the linac for positron production, using sections from the decommissioned Cambridge Electron Accelerator. Finally, late in 1977 the official NSF approval came, and $20.6 million was eventually awarded for construction of CESR and the CLEO detector.

It is ironic that of the four facilities rated by the Low Subpanel, CESR was the first to come into operation, and by the 1990’s the two with the lowest priority, CESR and the Fermilab ring, now called the Tevatron, were the only two producing physics results.

McDaniel put Maury Tigner in charge of CESR construction, while Al Silverman led the CLEO effort. The schedule was ambitious and called for first beam trials on April 1, 1979, less than two years away. The various systems — magnets, rf (Fig. 6), injection, vacuum, controls – all had challenging performance goals. Many of the Cornell experimenters joined the CESR effort. For example, I worked on the synchrotron to storage ring injection beam transport while my involvement with the CLEO detector was in building the luminosity monitor and writing the trackfinding program for the DR. The CESR construction program was extremely well organized under Tigner, and plowed along relentlessly in spite of difficulties.

CLEO, on the other hand, was a free association of individual, far-flung university groups accustomed to working independently on much smaller projects with little or no time pressure. It needed all of Silverman's skills at negotiating and coaxing to keep it on any semblance of schedule. The various components of the detector were parceled out to the university groups, as in the table below. Typically, each group took responsibility also for the readout electronics and software associated with its detector component. Figures 7 and 8 show the stringing of the DR chamber and the assembly of the detector in the I.R. pit.

| | | |
|---|---|---|
| LM | small angle luminosity detector | Cornell |
| IZ | inner $z$ proportional chamber | Syracuse |
| DR | main cylindrical drift chamber | Cornell |
| | solenoid coil | Cornell |
| OZ | outer planar drift chambers | Syracuse |
| CV | high pressure gas Cerenkov | Harvard |
| | low pressure gas Cerenkov | Rochester |
| DX | $dE/dx$ gas proportional chambers | Vanderbilt |
| TF | time of flight scintillators | Harvard |
| RS | octant shower detector | Rutgers |
| ES | pole-tip shower detector | Harvard |
| CS | octant-end shower detector | Harvard |
| | magnet yoke | Rochester |
| | additional iron for muon filter | Harvard |
| MU | muon drift chambers | Rochester |

By the end of March 1979 the installation of the essential CESR components in the tunnel was just about complete (see Fig. 9 top). On the evening of April 1, Nari Mistry and a few helpers were pushing to get the vacuum system in shape for the scheduled beam turn-on. As I recall, the first electrons were injected into CESR the next morning, on what would have been April 2 if we had not stopped the clock. The next several months were spent establishing electron beams and then positron beams in the storage ring [7] (Fig. 9 bottom). Beam trials were held in the evenings so that installation work could go on during the days, especially for the CLEO detector. It was on August 14 that we had the first measurable colliding beam luminosity. By October the luminosity was enough to schedule the first experimental run of the CLEO detector. It was still missing two octants of outer detector and half of the muon chambers, but enough of it was ready to start looking for electron-positron elastic scattering and annihilations into hadrons.

Figure 7: Elsa Adrian and Gino Melice stringing the wires of the CLEO DR1 drift chamber.

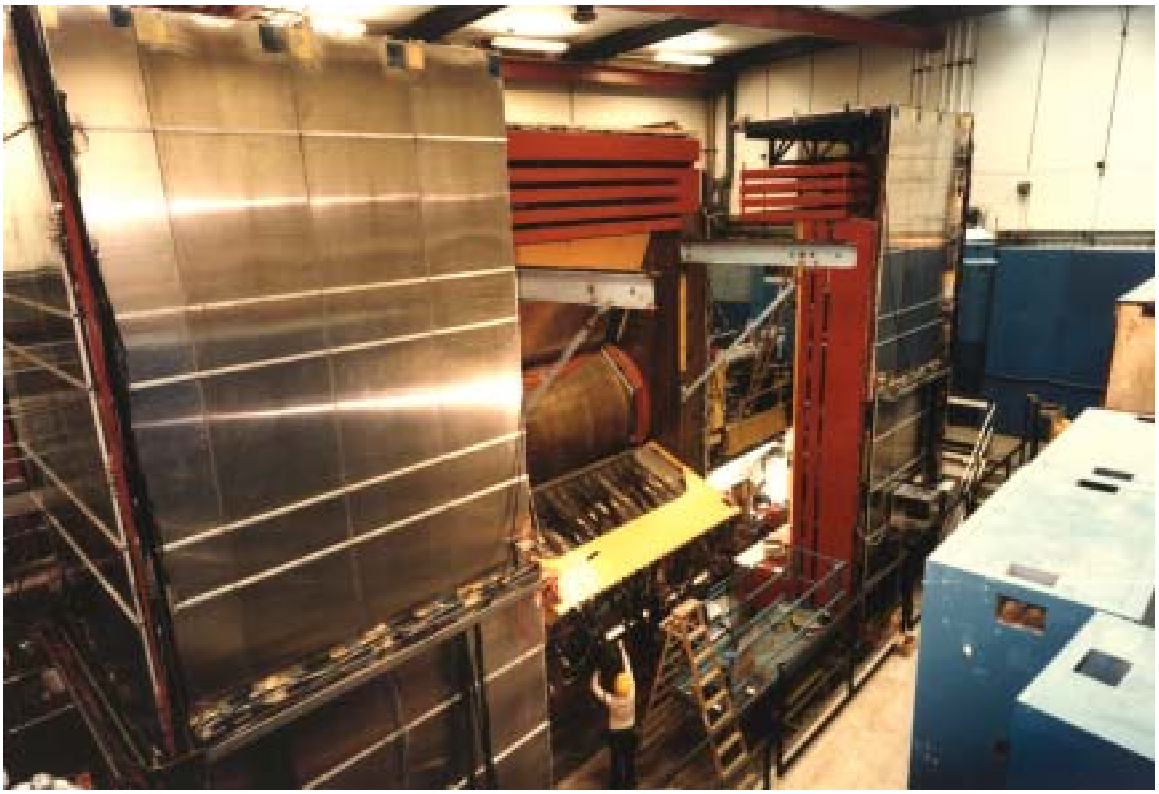


Figure 8: The CLEO-1 detector partially installed in the L-0 pit. The sheet metal surfaces are the MU drift chambers. One can see the solenoid coil and one of the octant modules containing OZ, TF, DX, RS, and OZ.

Looking back now, it seems to me that the building of CESR in less than two years well within the modest $20 million budgeted was a major achievement. There were many who had said we could not do it, and certainly not for that price. In fact, we had beaten the PEP turn-on time by about a year, and they had started earlier.

# 4 First Data, 1979-80

The world of particle physics had changed since we first submitted the CESR proposal in 1975. The discovery in 1977 by Lederman's group [8], [11] at Fermilab of the upsilon states at 9.4-10.4 GeV masses was a replay of Ting's finding the J (or $\psi$). The upsilons were immediately interpreted as the $b\overline{b}$ bound states of a new, heavier, $b$ quark. The CESR energy would be ideal for the study of the physics of the $b$, a fabulously serendipitous gift of nature that would guarantee the productivity of CESR and the viability of the Cornell Laboratory of Nuclear Studies for decades.

The original 1975 CESR proposal had said, after a discussion of the charm quark threshold, "present theory is quite inadequate to predict whether further hadronic degrees of freedom exist at even higher energies, . . . we do not know at what energies such new thresholds occur." But in a later version of the proposal, written in October 1976, there is a prophetic section by Kurt Gottfried in which he speculates on the consequences of a threshold for a hypothetical heavy quark in the CESR energy range [4]. He includes a figure labeled "The spectrum of $Q\overline{Q}$ bound states for a heavy quark having a mass of 5 GeV", which is an amazingly accurate prediction of the upsilon bound state energy levels and the $\pi\pi$ and $\gamma$ transitions among them, a year before the upsilons were to be discovered or named.

The Fermilab experiment [8] could not resolve the three bound states, although the shape of the mass peak clearly favored more than one. Lederman and co. even claimed the presence of a third state not obvious to the naked eye, but I doubt if they would have had the courage to do so if the 'Cornell' potential model of Gottfried et al. had not already predicted a third level. In a replay of the Adone post-discovery of the $\psi$, the DORIS $e^+e^-$ storage ring at DESY was quickly beefed up to run at an energy high enough so that in early 1978 the PLUTO and DASP detectors could locate the lowest upsilon state and later in the year the DASP and LENA detectors could see the first two states, then called the $\Upsilon$ and $\Upsilon'$. DORIS would not be able to reach the third state until several years later, after they had converted from two ring operation to a single ring machine running in the SPEAR single bunch mode.

So at the time that CESR came into operation the questions were

- was there really a third bound state resonance?
- would the $b\overline{b}$ spectroscopy look like $c\overline{c}$ spectroscopy, that is, was the binding potential flavor independent?
- was there a threshold for 'open-$b$', that is, for the production of $B$ meson pairs, and if so, what energy did it occur at?


Figure 9: (top) View of the CESR tunnel showing the injector synchrotron on the left, the storage ring on the right, and Boyce McDaniel in the middle. (bottom) Celebration in the CESR control room on the occasion of the first storage of a positron beam. Seated is Maury Tigner. Behind him are Joe Kirchgessner, Gerry Rouse, Chuck Chaffey, Raphael Littauer, Ernie vonBorstel, Boyce McDaniel, Bob Siemann, Ron Sundelin, Mario Gianella, Nari Mistry, Dave Rice, Al Silverman, Dave Andrews, Gordon Brown, an unknown, Dave Thomas, Dave Morse, C.O. Brown, Ken Tryon, Jim Fuller, Karl Berkelman, and Peter Stein.

- would there be a quasibound resonance just above threshold, analogous to the $\psi''(3770)$, where the cross section for $B\overline{B}$ would be enhanced?
- could one find evidence for a new quark flavor by seeing leptonic decays above the open-$b$ threshold?
- would the value of $R = \sigma(e^+e^- \rightarrow hadrons)/\sigma(e^+e^- \rightarrow \mu^+\mu^-)$ well above threshold confirm that the $b$ had charge $-1/3$?
- would there be a $t$ quark, the partner of the $b$, and if so where was it?

In November we started measuring the total hadronic cross section as a function of beam energy in small steps around where we expected to find the upsilon resonances. It took a few days to get the trigger and data readout working reliably. Also, since no one had any confidence in my tracking and hadronic event selection software (least of all I), we tried several independent schemes for determining the cross section, for example, scanning the pictorial display of the tracks by eye (Fig. 13) and counting the ones that appeared to be beam-beam annihilations, or picking the events on the basis of shower energy. To the accuracy we needed, the various analysis methods agreed, and there was the CUSB experiment for confirmation, too. At the luminosity CESR had achieved by then, about $10^{30}$ /cm$^2$sec, the hadronic event rate off resonance was about 10 per hour. Once we started the energy scan, it didn't take long to find the first resonance. When we reached at the right energy, the rate rose to about 1 per minute. This calibrated the CESR energy scale relative to that of DORIS. Since DORIS had already measured the energy difference between the $\Upsilon$ and the $\Upsilon'$, CLEO and CUSB were able to locate the second resonance almost immediately.

McDaniel had the idea of announcing CESR to the world by a Laboratory holiday greeting card, showing the data for the two resonances, cross section versus energy. By the time he was ready to get it printed up, we had found the third resonance, thus confirming its existence and making the first accurate measurement of its mass. So the $\Upsilon''$ data were added to the card (see Fig. 10).

For the next few months while continuing to take data, CLEO and CUSB worked out the efficiencies and corrections, wrote their papers announcing the "Observation of Three Upsilon States", and submitted them back to back to Physical Review Letters on February 15, 1980 (see Appendix, Table V). The first CLEO paper (see Fig. 11) had 73 authors from 8 institutions. Twenty-two of the authors and 6 of the institutions were still in CLEO fifteen years later.

By now CLEO was an established collaboration with elected officers, regular monthly meetings, and written minutes. Of all the large collaborations in high energy physics it is probably the most democratic. Collaboration policies, officers, physics goals, what to publish, whom to admit for membership, are all decided by majority vote of all the members – faculty, post-docs, and students – at the regular meetings. Elections are held every spring and the new officers take up their positions in the middle of the summer (see Appendix, Table III). Opportunities to represent CLEO at conferences are pooled and assigned to CLEO members by a broadly representative committee so that most members who want

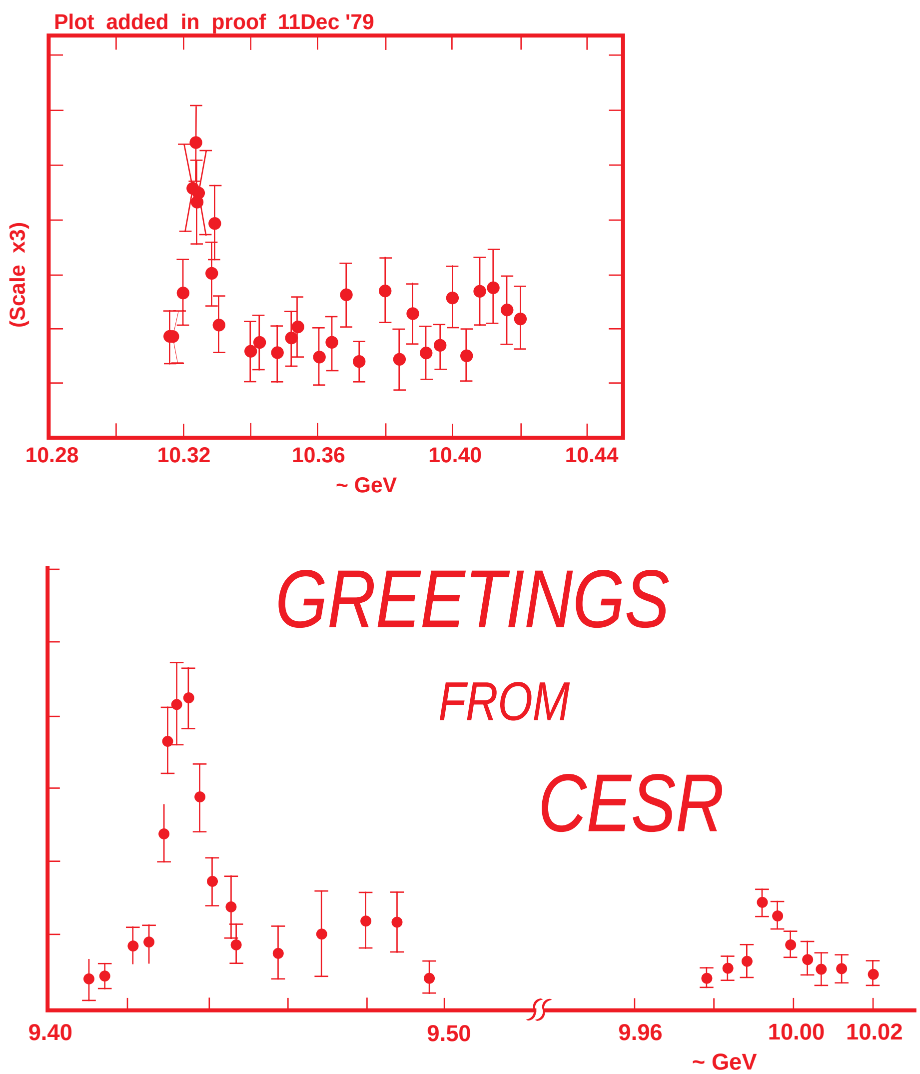
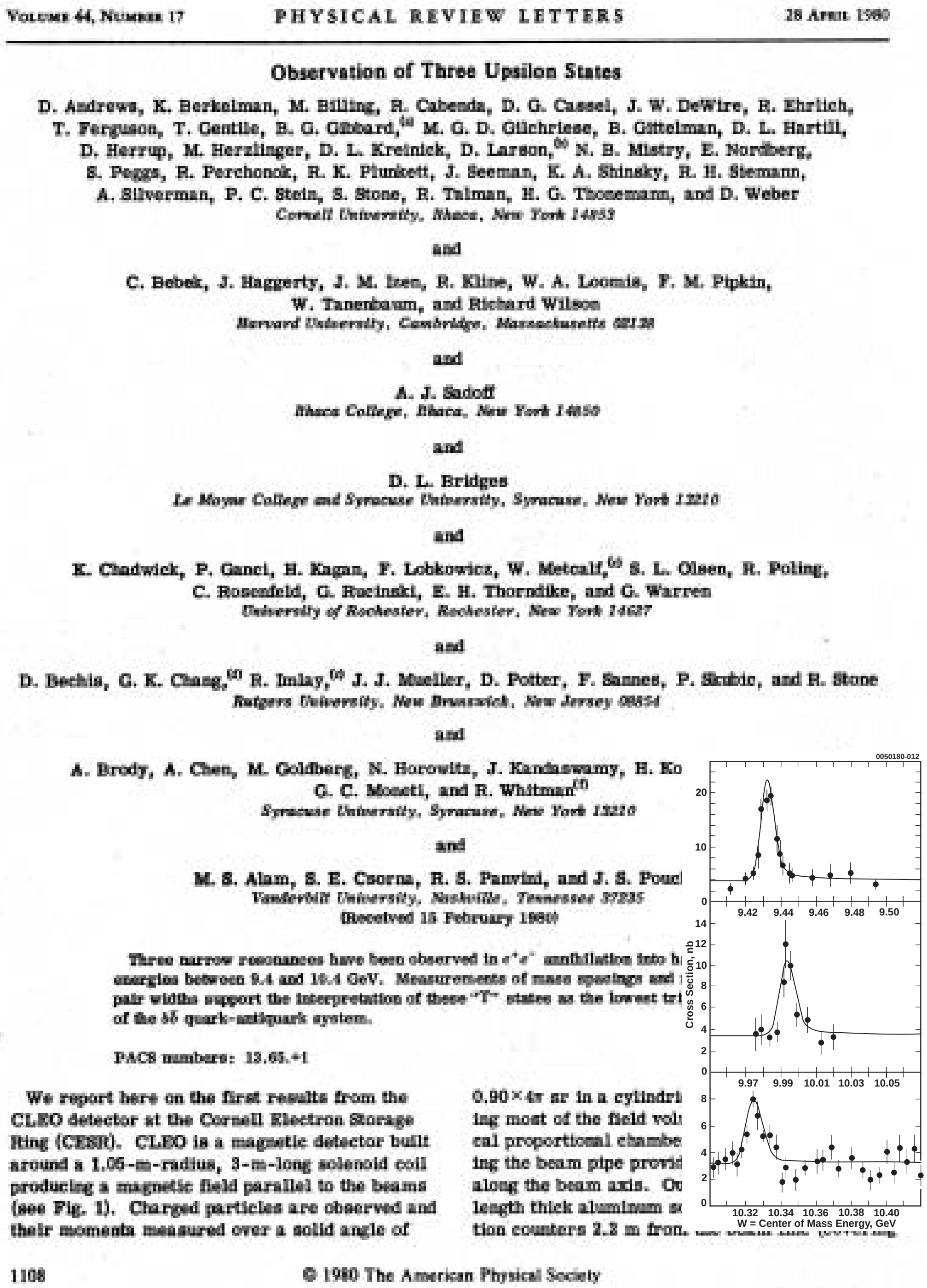



Figure 10: 1979 holiday greeting card from CESR.


VOLUME 44, NUMBER 17 PHYSICAL REVIEW LETTERS 28 APRIL 1980

**Observation of Three Upsilon States**

D. Andrews, K. Berkelman, M. Billing, R. Cabenda, D. G. Cassel, J. W. DeWire, R. Ehrlich, T. Ferguson, T. Gentile, B. G. Gibbard,[a] M. G. D. Gilchriese, B. Gittelman, D. L. Hartill, D. Herrup, M. Herzlinger, D. L. Kreinick, D. Larson,[b] N. B. Mistry, E. Nordberg, S. Peggs, R. Perchonok, R. K. Plunkett, J. Seeman, K. A. Shinsky, R. H. Siemann, A. Silverman, P. C. Stein, S. Stone, R. Talman, H. G. Thonemann, and D. Weber
*Cornell University, Ithaca, New York 14853*

and

C. Bebek, J. Haggerty, J. M. Izen, R. Kline, W. A. Loomis, F. M. Pipkin, W. Tanenbaum, and Richard Wilson
*Harvard University, Cambridge, Massachusetts 02138*

and

A. J. Sadoff
*Ithaca College, Ithaca, New York 14850*

and

D. L. Bridges
*Le Moyne College and Syracuse University, Syracuse, New York 13210*

and

K. Chadwick, P. Ganci, H. Kagan, F. Lobkowicz, W. Metcalf,[c] S. L. Olsen, R. Poling, C. Rosenfeld, G. Rucinski, E. H. Thorndike, and G. Warren
*University of Rochester, Rochester, New York 14627*

and

D. Bechis, G. K. Chang,[d] R. Imlay,[e] J. J. Mueller, D. Potter, F. Sannes, P. Skubic, and R. Stone
*Rutgers University, New Brunswick, New Jersey 08854*

and

A. Brody, A. Chen, M. Goldberg, N. Horowitz, J. Kandaswamy, H. Ko
G. C. Moneti, and R. Whitman[f]
*Syracuse University, Syracuse, New York 13210*

and

M. S. Alam, S. E. Csorna, R. S. Panvini, and J. S. Pouc
*Vanderbilt University, Nashville, Tennessee 37235*
(Received 15 February 1980)

Three narrow resonances have been observed in $e^+e^-$ annihilation into h
energies between 9.4 and 10.4 GeV. Measurements of mass spacings and
pair widths support the interpretation of these "Υ" states as the lowest tr
of the $b\bar{b}$ quark-antiquark system.

PACS numbers: 13.65.+i

We report here on the first results from the CLEO detector at the Cornell Electron Storage Ring (CESR). CLEO is a magnetic detector built around a 1.05-m-radius, 3-m-long solenoid coil producing a magnetic field parallel to the beams (see Fig. 1). Charged particles are observed and their momenta measured over a solid angle of

0.90×4π sr in a cylindri
ing most of the field vol
cal proportional chambe
ing the beam pipe provi
along the beam axis. O
length thick aluminum s
tion counters 2.2 m fro

1108 © 1980 The American Physical Society



Figure 11: The first CLEO publication.

it get a chance to speak for the collaboration. Although the CLEO governance structure can sometimes be rather cumbersome and the collaboration often finds it difficult to make decisions, I believe that the tradition of equality has done wonders for the group spirit and loyalty of the membership, especially the younger members. All share in the decisions and no one feels exploited. Originally some collaborators feared that Cornell might be too dominant in CLEO; for example, Frank Pipkin of Harvard had written McDaniel, "It strikes me that the most sensible user arrangement is one in which the groups are composed in part from Cornell people and in part from outside users with sufficient balance in talent and contribution of apparatus that each needs the others in a very real way. One should avoid the SLAC model in which the inside component always has the upper hand." I believe that over the years we have managed to allay those fears; in fact, as the collaboration has acquired new members, the relative weight of the outside groups has steadily increased. The collaboration owes a lot to Al Silverman for guiding it in the formative years and establishing the effective and collegial CLEO traditions.

Although there was a lively CLEO interest in the upsilon bound states, attention moved to the search for $B$ mesons, the bound states of $b$ quark and $\overline{u}$ or $\overline{d}$ antiquark. The nonrelativistic $b\overline{b}$ potential models could predict the relative masses of what we now called the $\Upsilon$(1S), $\Upsilon$(2S), and $\Upsilon$(3S) bound states, but they were not able to predict masses for $b\overline{u}$ and $b\overline{d}$ mesons, that is, locate the threshold energy at which the $b\overline{b}$ produced in the $e^+e^-$ annihilation would appear as $B^+B^-$ or $B^0\overline{B^0}$. What would be the cross section above threshold, and would we be able to see it? There was the possibility that nature might be kind and give us a quasibound $\Upsilon$(4S) resonance just barely above threshold so that (a) its decay would be inhibited enough for it to be narrow, and therefore tall, and (b) so that it would decay only to $B$ pairs without even an additional pion. But since the spacing of the upsilon levels was several hundred MeV ($M_{3S} - M_{2S} = 332$ MeV for example), it seemed too much to hope for.

We started scanning the total cross section between 10.3 and 10.6 GeV $e^+e^-$ total energy. By April we knew we had hit the jackpot. Nature had been incredibly kind and had given us a beautifully high and narrow $\Upsilon$(4S) resonance just 22 MeV above $B\overline{B}$ threshold. At the resonance energy one in every four hadronic events was $e^+e^- \to B^+B^-$ or $e^+e^- \to B^0\overline{B^0}$. This would be the energy (5.29 GeV per beam) at which CESR would run for most of its life.

If the Standard Model was now assumed to be based on 6 quarks and 6 leptons with the quarks coming in three colors, the $b$ quark should decay to a $c$ or $u$ quark plus a $W^-$, and naive counting rules would imply that $W^-$ should materialize 1/9 of the time to $e^-\overline{\nu}_e$ and 1/9 of the time to $\mu^-\overline{\nu}_\mu$. Thus one would expect to see inclusive $e$'s and $\mu$'s at the $\Upsilon$(4S) at rates much higher than in the continuum off the resonance. Indeed we did, and the rates were compatible with the counting rules. This established the existence of the new quark flavor, and can be considered the discovery of the $B$ meson. By the end of 1980 CLEO had submitted four important papers (see Appendix, Tables IV, V, XI) that set the course of CESR physics for the next decades.

- Observation of three upsilon bound states

- Observation of a fourth, wider upsilon state in $e^+e^-$ annihilations
- Evidence for new-flavor production at the $\Upsilon$(4S)
- Decay of $b$-flavored hadrons to single-muon and dimuon final states

Al Silverman's review talk at the 1981 Lepton Photon conference in Bonn is a nice summary of the early CLEO and CUSB results.

# 5 The CESR-II Blind Alley, 1980-83

Flushed with their success in building CESR and turning it into a productive upsilon physics facility, Maury Tigner and the Cornell accelerator physics crew were looking for new worlds to conquer. The superconducting rf cavity development effort that Maury had started back in the mid 1970s as a way of increasing the synchrotron energy was showing promise, but not for CESR. At the CESR beam energy and intensity the rf power dissipated in the cavity walls was not high enough (compared with the power radiated by the beam) to make the superconducting alternative economically attractive. But if high enough field gradients could be obtained, superconducting cavities might significantly reduce the size and cost of a very high energy $e^+e^-$ ring.

In 1980 the CERN UA1 and UA2 experiments were turning on at the SPS collider, with the major goal of finding the $W^\pm$ and $Z^0$ weak intermediate vector bosons in $\overline{p}p$ collisions. CERN and SLAC were proposing to build the LEP and SLC $e^+e^-$ colliders to exploit the physics at the $Z^0$ resonance at about 91 GeV in the center of mass. LEP was to be a 26.7 km ring costing over half a billion dollars, and the SLC would depend on an untried linear collider scheme with rather risky prospects for luminosity. It seemed to Maury and others that here was an opportunity. They drew up in May 1980 a "Design Study Proposal" for a 50-on-50 GeV $e^+e^-$ ring with a circumference of 5.485 km, luminosity $3 \times 10^{31}$ /cm$^2$sec (the same as for LEP), to cost about \$150 million. It was actually just an r&d proposal, since the project needed additional design work and superconducting cavity development. At first a site not far to the east of the existing CESR ring was considered; later another site was projected northeast of the Tompkins County Airport.

The Lab hosted workshops in November 1980, and in January and April 1981, to advance the machine design, discuss the experimental detectors, and generate interest in the high energy physics community outside Cornell. Several thick reports on 'CESR-II' were produced.

The superconducting rf cavity work had advanced to the stage of building a prototype accelerating system suitable for a test in CESR, which took place in 1982. There were problems, however. Maury had invented a clever 'muffin tin' structure for the cavities (see Fig. 12), that was relatively cheap to build and prevented synchrotron radiation from striking the cavity walls. But the resonant multiplicative emission and reemission of electrons from the cavity walls (called multipacting) limited the achievable field gradients in spite of measures taken to suppress the effect. In Europe they were having much better success with axially

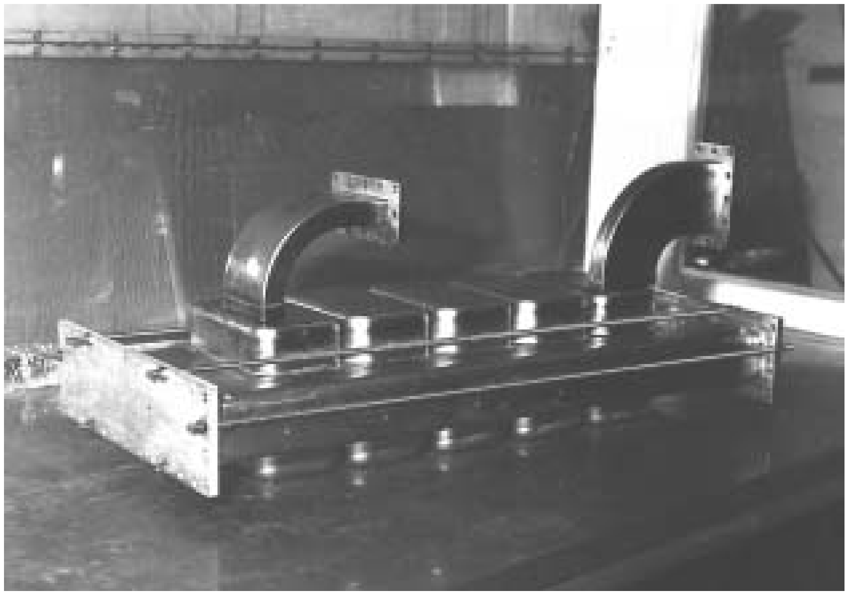


Figure 12: A five-cell muffin-tin niobium cavity.

symmetric ellipsoidal cavities, which channeled the emitted electrons to lower-field regions where they would not multiply when they struck the cavity walls.

The NSF encouraged the superconducting rf development effort, but was cool to the idea of spending \$150 million on a $Z^0$ factory. HEPAP formed another subpanel in August 1981, chaired by George Trilling, to consider the various accelerator proposals: the Brookhaven ISABELLE *pp* collider, the Fermilab Tevatron, and the $e^+e^-$ SLAC Linear Collider (SLC). Since it was not yet a real construction proposal, CESR-II got only passing mention in the subpanel report. The subpanel did recommend, however (I was a member), that the SLC project be funded, the major motivation being the potential advance in accelerator technology. Most of us were not completely convinced that the SLC would succeed in making enough luminosity to compete favorably with LEP — and we were right.

Now that LEP and SLC were practically launched, the prospects for NSF funding for CESR-II got even dimmer. The enthusiasm was lost. Instead, Maury in April 1983 convened the workshop at Cornell that started the design work for the Superconducting Super Collider, and in 1984 left Cornell to head the SSC Central Design Group at Berkeley. At various times, others like Murdoch Gilchriese and Richard Talman joined the effort. McDaniel took on the chairmanship of the SSC Board of Overseers and retired as Director of LNS in 1985. I succeeded him as the Laboratory's fourth Director.

One consequence of the death of the CESR-II Z-factory idea was the loss of mission for the superconducting rf (SRF) group in the Lab. Over the years the group had grown under Tigner's direction to include Ron Sundelin, Joe Kirchgessner, Hasan Padamsee, Peter Kneisl, Charles Reece, and Larry Phillips — all Research Associates or Senior Research Associates. They had turned the old Newman Lab synchrotron area into an impressive microwave and superconductivity research and development complex. Looking around for a mission for SRF, Sundelin managed to convince the designers of the CEBAF (Continuous Electron Beam Accelerator Facility) 4 GeV electron accelerator project for nuclear physics research in Newport News, VA that what they needed was a CW, superconducting, recirculating, linear accelerator. While CEBAF was getting approved and set up as a laboratory Sundelin got DOE funding to turn the latest model SRF ellipsoidal cavities, recently tested in CESR, into industrial prototypes for the CEBAF accelerator. When this work was completed in 1987 most of the SRF group, all except Padamsee and Kirchgessner, left to work for CEBAF. I had a hunch that SRF would eventually prove useful for the future Lab program, and since the NSF wanted to preserve the Cornell effort in SRF, I decided to replace, at least partially, the personnel losses and keep the research effort alive. For the next several years the SRF group under Padamsee's direction concentrated on basic studies of the phenomena that limited attainable field and $Q$, and on development of cavity structures that would be an economical possibility for a future TeV energy linear electron-positron collider. The group also studied the performance of $Nb_3Sn$ and high-$T_C$ superconductors in microwave fields. In later years my hunch proved correct and SRF cavities became an important component of a high luminosity CESR upgrade.

Another idea for a future direction for the Laboratory surfaced in 1983. The Major Materials Facilities Committee, named to advise the President's Science Advisor, recommended the construction of a dedicated 6 GeV Synchrotron Radiation Facility. To many people this seemed like a natural for Cornell. In April 1984 McDaniel and CHESS Director Boris Batterman circulated a prospectus for such a $66 million ring in a separate tunnel near CESR. Bob Siemann also got enthusiastic about it. It would provide a guaranteed future for the accelerator physicists and other lab employees, even though it would have no interest for the high energy physicists. After a meeting of all concerned, however, it was clear that the majority of the accelerator physicists also had no interest in working on a machine that did not serve high energy physics. Later, Argonne National Laboratory built the 6 GeV 'Advanced Photon Source'.

Meanwhile, by the mid 1980's $b$ quark physics at CESR had turned out to be more exciting than anyone had expected, and the future of the Laboratory seemed to be pointed in that direction.

# 6 The CLEO-1 Years, 1981-88

The users of CESR in the 1980's were CLEO at the south interaction point, CUSB in the north, CHESS using the x-ray beam lines to the west of CLEO, and the Cornell accelerator physics faculty, staff, and graduate students.

In the early days of CESR a separate organization was set up to manage the exploitation of synchrotron radiation for Cornell and outside users interested in the x-ray capabilities. The Cornell High Energy Synchrotron Source (CHESS) was under the direction of Boris Batterman of the Department of Applied and Engineering Physics, with Neil Ashcroft of the Physics Department as Associate Director. Three beam lines (called A, B, and C) were set up to exploit the intense radiation from the electrons passing through the CESR hard bend magnets just to the west of the south intersection region. Except for occasional brief periods, no longer than a month, the synchrotron radiation program was to be parasitic to the high energy physics running. In spite of this and competition from Brookhaven and other dedicated facilities with many more beam lines, CHESS always had a very active program with many loyal users from Cornell, outside universities, government labs, and industry. Until the arrival of the Grenoble and Argonne rings in the mid 1990's the CHESS beams had the highest available energies, and as the luminosity of CESR was increased for high energy physics, CHESS continued to lead the world in x-ray intensities.

Although there was always the potential for friction because of the conflicting requirements for beam conditions and other resources, the symbiosis between CHESS and LNS worked remarkably smoothly. Each obviously benefited from the presence of the other. CHESS got their beams for free, while the good will that LNS gained from a broad community at the NSF was often crucial in getting support for CESR funding.

The Columbia – Stony Brook (CUSB) detector in the north area had photon energy resolution clearly superior to that of the CLEO-1 detector, and was ideally suited for the study of radiative transitions among the upsilon bound states. Both detectors could measure total annihilation cross sections well, and therefore competed in bump-hunting for $^3S_1$ vector $Q\overline{Q}$ states. Because of its magnetic field, the CLEO detector was superior for the study of the dipion transitions, like $\Upsilon$(2S)$\rightarrow \pi^+\pi^-\Upsilon$(1S), but the 4-prong events in which the final $\Upsilon$ decayed to a lepton pair were so obvious that also CUSB could identify them cleanly without momentum measurement. CUSB was a small, close-knit, dedicated group that excelled at making the most of a limited detector, a very confined experimental area, and scarce resources.

Most of the CLEO experimenters were more interested in the weak interactions of $B$ mesons than in the strong interaction physics of the bound state resonances. So whenever it came time each year or so to present their requests to the Program Advisory Committee, CLEO asked for running time on the $\Upsilon$(4S) resonance above $B\overline{B}$ threshold, and CUSB asked for time on the narrower $\Upsilon$(2S) or $\Upsilon$(3S) resonance. Typically, the PAC would award CLEO about two-thirds of the priority and CUSB the remainder, but of course both experiments ran all the time.

From 1981 through to 1986, when the drift chamber was replaced, the CUSB priority runs resulted in 13 published CLEO papers on the $\Upsilon$ bound states: one on the total cross sections, 4 on the $\Upsilon$(2S or 3S)$\rightarrow \pi^+\pi^-\Upsilon$(1S) transitions (see Fig. 13), 3 on the dilepton decays of the upsilons, one on the $\Upsilon$(2S) radiative decay to the $\chi_b$(1P) states detecting pair-converted photons in the drift chamber, and 4 on radiative $\Upsilon$(1S) decays (see Appendix, Table V). Many of these were paralleled by CUSB papers based on the same CESR running. Most of the results confirmed the expectations from the potential models; that is, $b\overline{b}$ spectroscopy repeated the $c\overline{c}$ spectroscopy that had been worked out mainly at SPEAR a few years before. The pattern of energy levels and the kinds of transitions between them were similar, implying that the carrier of the strong force was flavor blind and that the quark-antiquark potential in the relevant range of separation was intermediate between Coulomb-like $(1/r)$ and linear in $r$.

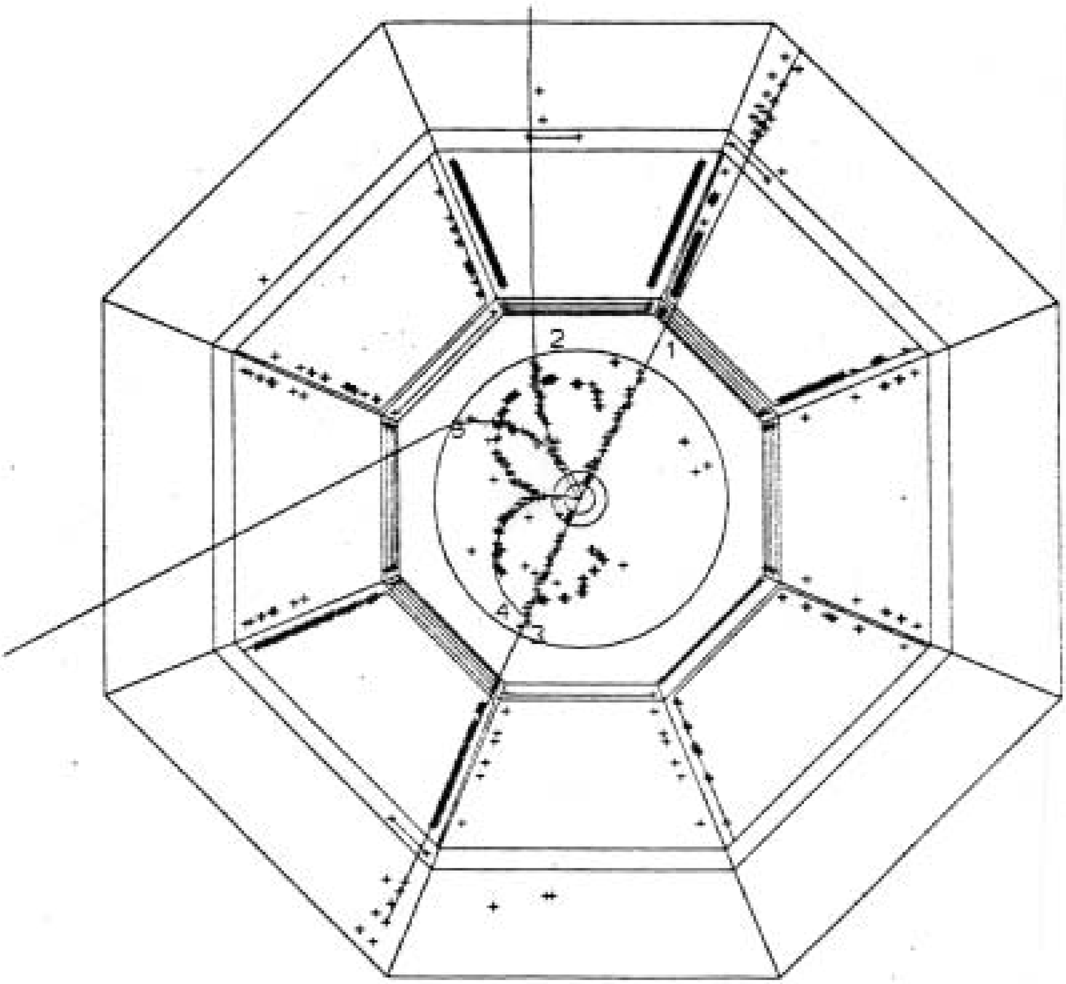



Figure 13: Computer display of a candidate for $\Upsilon$(2S)$\rightarrow \gamma\chi_b$(1P), $\chi_b$(1P)$\rightarrow \gamma\Upsilon$(1S), $\Upsilon$(1S)$\rightarrow$ $e^+e^-$ in the CLEO-1 detector. Each photon converts to an $e^+e^-$ pair.

One new feature was the presence of three $^3S_1$ $b\overline{b}$ bound states below $B\overline{B}$ threshold, instead of the two $c\overline{c}$ states below $D\overline{D}$ threshold. Thus the $\Upsilon$ system has about double the number of states and transitions. With more energies and rates to measure, one could constrain the theory much more tightly. Also, with a much more massive quark the non-relativistic theory had a better chance of being numerically reliable.

The one unanticipated result was the double-hump shape of the dipion effective mass spectrum in $\Upsilon$(3S)$\rightarrow \pi^+\pi^-\Upsilon$(1S). It prompted speculations about $\pi\pi$ interactions and $\Upsilon\pi$ interactions, and still lacks a universally accepted explanation. Presumably, it is not something basically wrong with QCD.

Our competition in this era came from the DORIS $e^+e^-$ storage ring at DESY, upgraded in energy after 1982 to reach as high as the $\Upsilon$(4S) and equipped with the ARGUS and Crystal Ball detectors. ARGUS was a solenoid-based magnetic detector, at least as good as CLEO-1 and perhaps better, built and operated by the German, Russian, US, Canadian group that had taken over the DASP detector from its builders in 1978. The Crystal Ball, a mainly spherical array of sodium iodide scintillators, was optimized for photon resolution and had already proved itself at SPEAR by mapping out the energy spectrum of the three photon lines from the $\psi'$ to $\chi_c$ transitions in charmonium. As soon as the Crystal Ball came into operation at DORIS, the running concentrated on the $\Upsilon$(2S). The Crystal Ball produced the best looking data on the $\Upsilon$(2S)$\rightarrow \gamma\chi_b$(1P) lines, but ARGUS, CLEO, and CUSB also had pretty decent data, too.

Later, after a run on the $\Upsilon$(1S), the Crystal Ball group claimed a significant peak in the high energy inclusive photon spectrum, which they named the zeta ($\zeta$), suggesting that it may be the Higgs boson. This prompted DORIS and CESR both to make long runs at the $\Upsilon$(1S) energy. No one saw any sign of the $\zeta$, and the Crystal Ball had to retract their discovery. After that the Crystal Ball retired from the field, leaving ARGUS as the only high energy physics experiment at DORIS. The Crystal Ball never attempted the radiative spectrum from the $\Upsilon$(3S); CUSB had the best data on that.

In 1981 CLEO and CUSB made an energy scan of the total cross section above the $\Upsilon$(4S). Peaks appeared at 10.865 and 11.019 GeV center of mass energy. They were assumed to be the next two $^3S_1$ $b\overline{b}$ states and we named them $\Upsilon$(5S) and $\Upsilon$(6S). At the time, they offered no advantages over $\Upsilon$(4S), so we didn't spend much time there.

When not running at the CUSB choice of energy, or chasing the zeta, or bump hunting at high energies CLEO ran at the $\Upsilon$(4S) and concentrated on $B$ meson decay physics. Because of the high $B$ mass, the typical decay produces a large multiplicity of particles and the number of different exclusive decay channels is huge. Kinematic reconstruction of $B$ decays was therefore very difficult. Most of our data came from inclusive rate measurements. That is, we would measure the total rate for some particle species at the $\Upsilon$(4S) resonance energy, and subtract the corresponding rate at an energy just below $B\overline{B}$ threshold (called 'the continuum'), scaled to account for the $E^{-2}$ dependence of the continuum cross section. Since the $\Upsilon$(4S) was assumed to decay entirely to $B^+B^-$ and $B^0\overline{B^0}$ approximately equally, the subtracted inclusive rates corresponded to the decay rates averaged over the two $B$ charge states.

The standard running mode settled down to two weeks of resonance alternating with one week of continuum. We measured and published papers on inclusive $B$ decays to $e$, $\mu$, $K^{\pm}$, $K_S$, $D^0$, $D^*$, $D_s$, $\phi$, $\psi$, baryons, charmed baryons, and all charm (see Appendix, Tables XI and XII). We measured charged multiplicities in $B$ decays, and from various combinations of inclusive lepton rates we obtained upper limits for neutrinoless leptonic decays, flavor changing neutral currents, $b \to u$ transitions, and $B^0\overline{B^0}$ mixing. So far, no surprises. The $b$ quark decay was consistent with charged-current decay to the next lighter doublet; that is, $b \to cW^-$, no flavor changing neutral currents, no exotic modes, and little or no $b \to uW^-$. The $b$ quark was acting like the charge $-1/3$, lower-mass member of a weak doublet, and the six-quark Standard Model seemed to be working well. There had to be a top quark and we expected it would be found momentarily at PETRA, then at TRISTAN, then at LEP. We assumed that there was a window in time for $b$ quark physics; as soon as the top was found, upsilons and $B$'s would no longer be interesting. As it eventually turned out, the 174 GeV mass of the $t$-quark is so high that it does not form bound meson states before it decays, and there is essentially only one decay mode, $t \to bW^+$. Except for its possible coupling to a Higgs boson, the study of the top quark is not likely to provide much new information about the Standard Model or its generalizations.

As the $B$ event sample got larger CLEO started to look for reconstructable exclusive decay modes. The only modes for which the number of spurious random mass combinations would be low were those with the lowest final state multiplicities. And since the favored decay path was $b \to c$, there would be a $D$ in the final state, for which we also had to require a low multiplicity decay. The typical product branching ratio we were looking for was therefore only $\sim (5 \times 10^{-3})(4 \times 10^{-2}) = 2 \times 10^{-4}$, for $B^- \to D^0\pi^- \otimes D^0 \to K^-\pi^+$, for instance. Sheldon Stone [11] led a group that found the first evidence of a mass peak, and in January 1983 CLEO submitted a Phys. Rev. Letter [**50**, 881 (1983)] showing a signal in four modes combined: $D^0\pi^-$, $D^0\pi^+\pi^-$, $D^{*+}\pi^-$, and $D^{*+}\pi^-\pi^-$, with $B$ branching ratios 4.2, 13, 2.6, and 4.8%, respectively, with large statistical errors. Better measurements a few years later from ARGUS and CLEO showed that our early measurements were mostly wrong. The reported $B$ mass was two standard deviations (5 MeV) too low and the branching ratios were an order of magnitude too high. Most likely, we had indeed seen $B$ decays, but we didn't realize that modes with an additional unseen pion could feed down and masquerade as simpler modes with only a minimal effect on the reconstructed mass. This was a rare example of CLEO publishing a wrong result, outside of quoted error limits.

In 1986 CLEO submitted to Physics Letters B [**183**, 429 (1987)] the results of its first (unsuccessful) search for rare exclusive $B$ decay modes via the effective neutral current $b \to sg$ and $b \to s\gamma$ loop mechanism, called penguin decays because of the alleged resemblance of the Feynman diagram to a penguin. Although the branching ratios are of the order of $10^{-5}$, the efficiencies are high because you don't have to reconstruct a secondary $D$. The upper limits we reported were all above $2 \times 10^{-4}$, though, and not yet in the interesting range.

At energies below the $B\overline{B}$ threshold 4/10 of the hadronic annihilation cross section is charm production, $e^+e^- \to c\overline{c}$, and even at the $\Upsilon(4S)$ resonance there is more charm production than $b\overline{b}$. As we learned to reconstruct $D$'s and $D^*$'s in $B$ decays, it became obvious that

CLEO had significant capability for charm physics. Although the final states in $e^+e^- \to c\overline{c}$ were more complicated than those at the SPEAR threshold energies, they were no worse than $B$ decay final states, and the higher CLEO energies actually brought some advantages. We could be sure that any charmed particles found in the continuum data runs, or with momenta above the $\sim 2.5$ GeV/c maximum for $B$ decay secondaries, had to be from direct $e^+e^- \to c\overline{c}$ production and not from $B\overline{B}$ events. In the CLEO-1 era 11 charm papers were published: inclusive cross sections, momentum spectra, and decay branching ratios for $D^*$, $D_s$, $\Lambda_c$, $\Sigma_c$, and $\Xi_c$, as well as $D$ lifetimes and a search for charm changing neutral currents (see Appendix, Tables VIII, IX, X).

The most notable of the early CLEO charm papers was the 'discovery' of the $F$ meson, now called the $D_s$. Sheldon Stone and others found a clear mass peak at $1970 \pm 7$ MeV in $\phi\pi$ consistent with the decay of a $c\overline{s}$ meson. The problem was that DASP had in 1979 reported a few $\eta\pi$ events at a mass of $2030 \pm 60$ MeV, confirmed by a CERN OMEGA photoproduction experiment which had claimed to see events in $\eta\pi$, $\eta\pi\pi\pi$, $\eta'\pi\pi\pi$, and $\phi\rho$ at masses centered on $2030 \pm 15$ MeV. Moreover, the Mark I detector at SPEAR had run above the $e^+e^- \to D_s\overline{D_s}$ threshold and had not reported seeing anything. We were sure our data were right, however, so we prepared it for publication. When our Albany collaborator, Saj Alam, saw the draft he revealed that when he had been a member of the Mark I collaboration he had worked on the $F$ search and had convinced himself that they were seeing $F \to \phi\pi$ at a mass around 1970 MeV, but the statistical weight was not enough to convince his colleagues. Sheldon called Burt Richter to see if Mark I wanted to be referenced in our paper, and was told that they preferred that we not mention their earlier unpublished work. Our paper, "Evidence for the $F$ meson at 1970 MeV" A. Chen et al., Phys. Rev. Lett. **51**, 634 (1983), was greeted with scepticism for a while, but was confirmed by TASSO and ARGUS at DESY, and by ACCMOR at CERN. Whether you should count the $D_s$ as a CLEO 'discovery' depends on whether you think the earlier $F$ claims were spurious background fluctuations or mass mismeasurements. A Fermilab proposal to look for the tau neutrino, based on the expectation of $D_s \to \tau\overline{\nu}_\tau$ events in a beam dump, was withdrawn because our lower mass for the $D_s$ significantly reduced the expected branching ratio to $\tau\overline{\nu}_\tau$.

Beyond upsilon, $B$, and charm physics there was a miscellany of topics that accounted for ten papers: four on tau leptons, two on inclusive particle production in the non-$B\overline{B}$ continuum, one on Bose-Einstein correlations, and one each on unsuccessful searches for axions, multipoles, and the $\xi(2200)$ seen by Mark III.

Just as noteworthy as some of the papers that CLEO published was one that we did not publish. Some time after the finding of the $D_s$, Hassan Jawahery, a research associate with Syracuse, discovered a narrow mass peak in $K^+K^-\pi^\pm$ somewhat below the mass of the $D$ meson. Although the statistical significance of the 'Jawaherion' looked moderately compelling, more than three standard deviations as I recall, it did look suspicious to most collaboration members. There was no plausible explanation for the existence of such a narrow state at that mass, and there was no confirming evidence in other modes. For more than a year we suppressed the news until we could check it with more data. No one mentioned it in public, not even Jawahery, who to his credit respected the will of the majority. The later data showed no peak; the original signal was apparently only an unlikely statistical fluctuation in the background.

The last CLEO paper (submitted in late 1988) based entirely on data obtained with the CLEO-1 detector before the installation of the new DR2 drift chamber, dealt with $\Sigma_c^{++}$ and $\Sigma_c^0$ charmed hyperon continuum production and decay. It listed 91 authors from 12 institutions (see Appendix, Table II). LeMoyne College and Rutgers University had left the collaboration, but seven new university groups had joined. The new CLEO institutions were acquired in two ways: (a) a CLEO research associate takes a faculty job at another university and leads a new group that joins CLEO (Ohio State, Florida, Minnesota, Maryland); or (b) a university with no previous connection to CLEO petitions to join (Albany, Carnegie Mellon, Purdue).

In March 1983 McDaniel decided that it was time to reopen the question of what best use could be made of the north interaction region. The Columbia — Stony Brook (CUSB) collaboration had been running their nonmagnetic NaI and lead-glass detector there since the turn-on of CESR in 1979, and had done good work on upsilon spectroscopy. The CUSB collaboration had grown to include Richard Imlay and others from Louisiana State University and Eckart Lorenz and others from the Max Planck Institute in Munich. LSU had contributed a muon detector outside the rest of the CUSB detector and Munich had added small-angle sodium iodide scintillator arrays. McDaniel called for proposals for the experiment to replace CUSB. The Program Advisory Committee met in January 1984 to choose between two north area options:

- a Columbia — Stony Brook proposal to upgrade CUSB (to CUSB-II) by replacing the tracking chambers with a cylindrical array of bismuth germanate (BGO) scintillation counters;

- a proposal by the UPSTATE collaboration (CalTech, Carnegie Mellon, Louisiana State, MPI Munich, Princeton, and Stanford, with Donald Coyne and Eckart Lorenz as cospokesmen) for a new compact detector with tracking chambers inside a BGO ball inside a superconducting solenoid.

Upon the recommendation of the PAC, McDaniel decided for the CUSB-II proposal. This was when the Carnegie Mellon group decided to join CLEO.

# 7 Improving CESR, 1981-88

In the early days of CESR operation the accelerator physics activity was concentrated on learning how to operate the linac, synchrotron, storage ring complex reliably. The positron coalescing scheme worked, but its complexity made injection difficult. Eventually the linac was upgraded to allow positrons to be produced at a rate sufficient to do without coalescing.

Like all the other storage rings proposed in the 1970's, CESR was supposed to have a peak luminosity of $10^{32}/\text{cm}^2\text{sec}$ [5]. It was calculated from the formula

$$\mathcal{L} = \frac{N_{e+} N_{e-} f_c}{4\pi \sigma_H \sigma_V}$$

under the following parameter assumptions.

| | | |
|---|---|---|
| Beam energy | 5 GeV | 8 GeV |
| Luminosity, $\text{cm}^{-2}\text{sec}^{-1}$ | $0.59 \times 10^{32}$ | $1 \times 10^{32}$ |
| Number of bunches per beam | 1 | 1 |
| Number of particles per bunch, $N_{e\pm}$ | $1.34 \times 10^{12}$ | $1.5 \times 10^{12}$ |
| Circulation frequency, $f_c$ | 390.134 kHz | 390.134 kHz |
| Horizontal r.m.s. beam size, $\sigma_H$ | 1.0 mm | 1.0 mm |
| Vertical r.m.s. beam size, $\sigma_V$ | 0.09 mm | 0.06 mm |
| $\beta_V^*$ | 10 cm | 10 cm |
| Vertical tune shift, $\xi$ | 0.110 | 0.061 |

As in all the other storage rings, this luminosity was never achieved under these conditions. One reason was that CESR never ran at its 8 GeV design energy. But more importantly, the nonlinear focusing effects of the beam-beam interaction at high beam currents made it impossible to keep to the quoted beam size. The beam-beam effect is parametrized by the linear tune shift $\xi$. It was found empirically that as the beam current is increased $\xi$ reaches a saturation value beyond which the beam size increases in such a way that the luminosity becomes proportional to $N$ instead of $N^2$. The limiting tune shift value is not well defined, but tends to be about 0.03, instead of the much higher value implied by the CESR luminosity projection. Moreover, we were not able to collide beams with more than about $2 \times 10^{11}$ particles per bunch, presumably because of the destabilizing wake field effects of the interaction between the beam and the vacuum chamber and rf cavity walls.

In the saturated tune shift limit the luminosity is given in the usual c.g.s. units by

$$\mathcal{L} = 2.17 \times 10^{32} \frac{E e N f \xi}{\beta_V^*},$$

where $E$ is in GeV, $e$ is the electron charge in Coulombs, $N$ is the number of particles per bunch, $f$ is the frequency of bunch passages, and $\beta_V^*$ is the focusing depth of field parameter in meters. With $N = 2 \times 10^{11}$ and $\xi = 0.03$ the formula implies a luminosity of only $4 \times 10^{30}/\text{cm}^2\text{sec}$ at 5 GeV. By the end of 1980 we had reached $3.1 \times 10^{30}$, and it was clear that we had to do something drastic to get much further.

If $N$ and $\xi$ are limited, the only remaining variables are $f$ and $\beta_V^*$. The latter parameter is proportional to the focal length of the interaction region quadrupole doublet. Since all the interesting physics was at energies well below the 8 GeV design energy, it was relatively easy to move the quads in closer to the CLEO and CUSB experiments and increase the currents in them, once we had learned to do without the solenoids that compensated for the focusing effect of the CLEO solenoid. We found that the compensation could be done with rotated quadrupoles located outboard of the I.R. focusing quads. So in the summer of 1981, we did it and reduced $\beta_V^*$ to 3 cm; this was called the 'minibeta' configuration. By the end of the year the luminosity had reached $8 \times 10^{30}$ and by the following year it was $1.2 \times 10^{31}$.

Increasing the bunch frequency $f$ would not be so straightforward. That would require storing more than one bunch per beam. The beauty of the original single bunch scheme was that the electrons and positrons could travel in precisely the same orbits and would collide head-on at only two points. With $n$ bunches per beam there would be $2n$ collision points, all but two occuring where there were no detectors and where larger $\beta$ values would imply enhanced beam-beam disruption that would seriously limit the achievable luminosity at the CLEO and CUSB interaction points.

Raphael Littauer came up with the solution by proposing that we install electrostatic fields outboard of each of the two interaction regions. The fields would deflect the electrons and positrons oppositely, each trajectory making several horizontal betatron oscillations through the $< 180°$ magnet arc before returning to the undeflected orbit just before the next interaction region. These 'pretzel' orbits would separate the electrons and positrons transversely by several cm at the $2n-2$ undesired interaction points. The maximum bunch number $n = 7$ would be limited by the betatron tune of the ring.

The electrostatic separators were built and installed in June 1983, but making the multi-bunch configuration work was not easy. The separators had to maintain very high fields over several meters length, and every time there was a spark or high voltage glitch, the stored beam would be lost. Injection into pretzel orbits was difficult, and the available aperture in the beam chamber for keeping the beams separated from each other and from the walls was barely enough for usable beam lifetimes. For a while CESR ran with $n = 3$ bunches per beam, but the multibunch luminosity gain was only about $\times 1.5$. The missing factor of 2 was apparently the effect of the aperture limitations. By 1985 we had reached a peak luminosity of $3.9 \times 10^{31}$. David Rubin succeeded in improving integrated luminosity in the same year by working out a procedure for keeping the stored positrons at the end of a beam run instead of dumping them; positron injection was much faster in this 'topping-up' scheme.

The next step was 'microbeta', a further reduction in $\beta_V^*$ to 1.5 cm. In order take the minibeta idea a step further we had to make a quadrupole that would fit inside the hole in the CLEO magnet pole and come within 60 cm of the center of the detector. Steve Herb built permanent magnet quadrupoles to be supported from the end plates of the CLEO drift chamber. They were installed in 1986.

Since the $\beta$ function goes through an hour-glass minimum at the interaction point, low $\beta_V^*$ can help only over an interaction region length $\sigma_L/\sqrt{2}$ that is of the order of $\beta_V^*$. The bunch length $\sigma_L$ is determined by the rf overvoltage and was about 2 cm. By 1988 CESR was running with luminosity $10^{32}$, a record for $e^+e^-$ rings. The improvement came not only from microbeta, but from increasing the number of bunches per beam to the limit of $n = 7$. The full benefit of microbeta, however, would have to wait for more rf voltage.

The rf system, in fact, was becoming a major headache. The original intent had been to run at 8 GeV per beam with two 14-cell rf cavity assemblies. At 5.3 GeV only one of them was required to make up the 1 MeV/turn energy loss due to synchrotron radiation. But the rf cavities were severly stressed by the increasing power transferred to the beam as the number of bunches increased, and by the higher voltages required for bunch shortening. We had chronic problems with arcing at the input power windows and with vacuum leaks at many of the welds. Of the three cavity assemblies we had on hand, usually only one was in working order. It was frustrating to diagnose and fix problems. A bad cavity had to be removed from the ring, the water jacket had to be drained and dried out, the problem had to be located and fixed, the cavity had to be baked and pumped down, then it had to be vacuum tested and power tested. If the tests failed, as they did often, the procedure would have to be repeated. The turnaround time could be several months. The cavities were becoming the major reason for lost running time. Eventually, Don Hartill solved the window arcing problem, but the vacuum troubles continued to get worse.

# 8 The First Upgrade, CLEO-1.5, 1984-89

In 1981 the remaining CLEO detector components were completed and installed (see Appendix, Table I): the superconducting solenoid coil, the rest of the muon chambers, and all eight octants of $dE/dx$ detectors. Dave Andrews had designed the coil more conservatively than the TPC coil he had copied it from, and although it was supposed to run at a field of 1.2 Tesla, we never had the courage to run it above 1.0. As a result, it ran well and never had the kind of mishaps that the TPC coil had.

In 1984 a ten-layer 'VD' cylindrical drift chamber built at Ohio State replaced the original IZ proportional chamber. Although the inner radius was a conservative 8.1 cm, we had trouble initially with synchrotron radiation and had to learn to be careful with masking. The improved spatial resolution close to the beam pipe made it possible to reconstruct separated decay vertices for charmed mesons and taus.

Although the CLEO detector was an excellent match to the requirements of the early upsilon and $B$ meson physics at CESR, as time went on and the easy measurements had all been done, we became more aware of the limitations of the detector:

- the arrangement of cells in the drift chamber was such that at particular $\phi$ angles the sense wires lined up radially making it impossible to resolve the left-right drift ambiguity for tracks in those directions;

- although the solenoid coil was only 0.7 radiation length thick, a sizable fraction of the particles passing through it interacted, thereby confusing the information from the outer detector components, particularly the DX proportional chambers and the RS shower chambers;

- the $17\%/\sqrt{E_{GeV}}$ resolution of the RS proportional tube and lead calorimeter made it pretty useless for looking at photons from upsilon radiative transitions, or for reconstructing $\pi^0$'s and $\eta$'s in $B$ decays;

- the MU chambers were so far away from the interaction point that decay muons from pions and kaons were a significant background;

- the detector was fragmented into too many separate subsystems, causing the less useful ones (OZ, CS, etc.) to be ignored.

We began to realize that the ARGUS detector being built at DESY might outperform CLEO.

It was clear that we could make a significant improvement if we could accomplish the shower detection and the $K/\pi$ separation inside the solenoid coil, but it was not immediately clear how. Informal groups within CLEO started working on both problems. We discussed three options for particle identification: (a) a drift chamber with more layers than the present one, optimized for $dE/dx$ measurements as well as tracking, (b) a time projection chamber with $dE/dx$, as in the TPC experiment at PEP, and (c) a conventional drift chamber supplemented with a ring imaging Cerenkov detector. After heated debate we chose option (a) as being simpler and less risky, although not optimally effective for hadron identification at high momentum. The shower calorimeter discussions quickly centered on a high-$Z$ scintillator: sodium iodide, cesium iodide, bismuth germanate, or barium fluoride. Cesium iodide, although not widely used, seemed to offer a good compromise of performance and price, so we decided to test some prototypes in a SLAC beam.

To get started we fixed the new DR2 drift chamber outer radius at 91 cm, the same size as the existing DR1 drift chamber. This would allow us to build the new one and run it in the original CLEO-1 detector, then later install it in a new CLEO-2 detector. There would probably be enough money in the annual operating budgets to build DR2, but we would have to convince the NSF to support a sizable capital upgrade project to build the rest of CLEO-2; that is, the new time-of-flight scintillators, CsI scintillator array, superconducting coil, magnet iron, and muon detector.

The team that built the new drift chamber included David Cassel, Gil Gilchriese (who left soon after for the SSC project), Dan Peterson, and Riccardo DeSalvo, a very energetic and inventive Research Associate from Pisa and CERN. The chamber was a close-packed array of single-sense-wire square cells arranged in 51 cylindrical layers. Through most of the chamber the sequence was $aaapaaan\ldots$, where $a$ is axial, and $p$ and $n$ are slanted at small $+$ and $-$ stereo angles. The sense wire positions in successive axial layers were staggered by half a cell, to help resolve the left-right ambiguity in drift azimuth. Instead of an inner and outer layer of field wires, there were cathode strip layers to give extra measurements of the $z$ coordinate. There were a total of 12,240 gold-plated tungsten 20 $\mu$m sense wires and 36,240 110 $\mu$m field wires, most of them gold-plated aluminum — a very large number of wires to string.

The completed DR2 drift chamber replaced the original DR1 during a five month shutdown in 1986 (see Fig. 14). The microbeta quadrupoles, installed at the same time, required a reduction in the beam pipe radius from about 7.5 cm to 5 cm, so we filled the space between the new beam pipe and the VD with a new 3-layer straw tube drift chamber called VD-insert or IV, built by Ohio State. Along with the tracking chambers we also replaced one of the two proportional-tube+lead end-cap shower detectors with a prototype cesium iodide array, to get running experience with the new kind of calorimetry. We ran this 'CLEO-1.5' detector configuration for the next two years while we were building the new CLEO-2 magnet and outer detector components. The increase in number of drift chamber tracking layers from

Figure 14: Installation of the microbeta quads and the VD drift chamber into the new DR2 drift chamber. Clockwise from left are Joe Kirchgessner, Steve Herb, Mike Ogg, Bryan Kain, Steve Gray, and someone else.

the original 17 of DR1 to the 64 of IV+VD+DR2 improved the pattern recognition and momentum resolution, and the reoptimized electronics allowed us to get 6.5% resolution in $dE/dx$ from the up to 61 pulse heights on a track. I rewrote the original SOLO track finding program to take advantage of the half-cell stagger in triplets of axial layers, hence the new program name, TRIO.

The modified detector came into useful operation quickly, but it took a while to build up enough of a data sample to rival that from the previous six years. In all, 33 CLEO-1.5 papers were submitted for publication between October 1988 and January 1992 (see Appendix, Tables V – XII). Six were on upsilon spectroscopy, 5 on $B$ semileptonic decays, 8 on $B$ hadronic decays, 8 on charmed mesons, 3 on charmed baryons, and 3 on the tau. the highlights were the measurements of $B\overline{B}$ mixing and of the $b$-to-$u$ decay.

The first measurements of the $B$ lifetime, made at PEP in 1983, had yielded an unexpectedly high value, about 1.2 picoseconds. This raised the possiblility that, since the neutral $B$, like the neutral $K$, had no conserved quantum number that could distinguish it from its antiparticle, the $B^0$ might oscillate to $\overline{B^0}$ or vice versa before it decayed. When the neutral $B$ decays semileptonically, the sign of the lepton can label its flavor at decay time, so one could look for $B^0\overline{B^0}$ mixing to $B^0B^0$ or $\overline{B^0}\,\overline{B^0}$ by running at the $\Upsilon(4S)$ resonance and observing like-sign lepton pairs with momenta high enough to exclude the cascade leptons from the semileptonic decays of $D$'s from $B$ decays. In 1986 CLEO reported an upper limit for the mixing probablility: $\chi_d < 19\%$. Meanwhile, the UA1 CERN $\overline{p}p$ collaboration had seen like-sign dimuon events and reported an average $\overline{\chi} = 12 \pm 5\%$ for the $B_d$, $B_s$, and $\Lambda_b$ mixture (mostly $B_d$) produced at high energies. This was confirmed by ARGUS [H. Albrecht et al., Phys. Lett. **B192**, 245 (1987)] with a measurement of $\chi_d = 17 \pm 6\%$. We were scooped! This happened when CLEO was shut down for the DR2 installation. ARGUS had accumulated a data set comparable to the CLEO data set, and their superior shower detector and shorter path for background from $\pi \to \mu\overline{\nu}_\mu$ gave them more dilepton sensitivity. This was their finest hour. In February 1989 we had enough data from CLEO-1.5 to make a high statistics remeasurement; we reported $\chi_d = 16 \pm 6\%$, confirming the ARGUS result.

In lowest order the $b$ quark was expected to decay to a lighter charge= 2/3 quark through the charged current weak interaction, that is, by emission of a $W^-$. Whether it would be $b$-to-$c$ or $b$-to-$u$ would depend on the values of the off-diagonal Kobayashi-Maskawa matrix elements $V_{cb}$ and $V_{ub}$. The CLEO inclusive $B$ decay data were consistent with $b$-to-$c$ dominance, and everyone wanted to know whether $V_{ub}$ was zero or not. The Kobayashi-Maskawa mechanism for $CP$ violation depended on $V_{ub}$ being nonzero. There were two main ways to look for $b$-to-$u$. One could look for hadronic $B$ decay modes that did not have a charmed particle or charmonium in the final state, for example, $B^0 \to \pi^+\pi^-$; so far, from CLEO-1 we had only upper limits [P. Avery et al., Phys. Lett. **B183**, 429 (1987)]. Or one could look at the lepton momentum spectrum in semileptonic $B$ decays, $\overline{B} \to X_u\ell^-\overline{\nu}$, beyond the end point for $\overline{B} \to D\ell^-\overline{\nu}$. A lower mass recoil, say $\pi\ell^-\overline{\nu}$ for instance, would allow a higher momentum lepton. Since semileptonic decays were the best understood theoretically, this second method had the advantage that a signal could be more reliably related to the value of the $V_{ub}$ KM matrix element. CLEO-1 data had not shown a high momentum excess [S. Behrends et al., Phys. Rev. Lett. **59**, 407 (1987)], implying an upper limit $|V_{ub}/V_{cb}| < 0.16$.

In mid 1988 we got word from ARGUS that they had seen charmless $B$ decays into $p\overline{p}\pi$ and $p\overline{p}\pi\pi$, that is, evidence for the $b$-to-$u$ transitions. These were modes we had not looked for. Had we been scooped again? There were not yet enough CLEO-1.5 data to check the ARGUS claim, so we went back to the CLEO-1 data. They were not conclusive. I had to give a review talk on nonleptonic $B$ decays at the Stanford Heavy Flavor Symposium in July. I waffled. "CLEO confirms the effect in the $p\overline{p}\pi$ channel, but not in the $p\overline{p}\pi\pi$. As to whether it is really from correctly reconstructed charmless $B$ decays, or from some yet to be determined cocktail of spurious misreconstructions, ... one has to start out with a skeptical bias ... I am suspicious of the biases inherent in the back-to-back angle cut. Lastly, the $\sin^2\theta$ distribution of the CLEO candidates, while not conclusive, does not favor the ARGUS hypothesis."

By October we had accumulated 212 $\text{pb}^{-1}$ of CLEO-1.5 data, which when combined with the 78 $\text{pb}^{-1}$ of older CLEO-1 data, was sufficient to confront the ARGUS report, based on 103 $\text{pb}^{-1}$. With more data, the indication that we had seen in $p\overline{p}\pi$ had gone away, and in the first paper based on CLEO-1.5 data [C. Bebek et al., Phys. Rev. Lett. **62**, 8 (1989)] we were able to set upper limits well below the branching ratios claimed by ARGUS. We had not been scooped; the ARGUS results were spurious. Some CLEO members claimed that ARGUS had tuned their event selection cuts to pump up a statistical fluctuation. To avoid this bias, cuts must always be determined a priori, without reference to the actual data, say by using Monte Carlo events.

We returned to the lepton momentum spectrum, and a year later had convinced ourselves that we had a high momentum excess that could come only from $B \to X\ell^-\overline{\nu}$ with recoil hadron masses $m_X$ below that of the lightest charmed meson [R. Fulton et al., Phys. Rev. Lett. **64**, 16 (1990)]. ARGUS confirmed it almost immediately, but we had scooped them. Indeed, $|V_{ub}|$ is not zero, it is 5 to 10% of $|V_{cb}|$. This discovery established the main outlines of the KM matrix governing quark weak decays and provided a basis for the violation of $CP$ invariance. Suddenly $B$ meson physics had caught fire. Here was a chance to understand the mystery of the baryon-antibaryon abundance asymmetry in the universe.

In 1988 we ran for a couple months (113 pb$^{-1}$) on the $\Upsilon$(5S). My student, Sumita Nandi, and a Kansas student, Sangryul Ro, looked in the data for evidence of the $B_s$ ($= \overline{b}s$) meson. There were several marginal indications: the inclusive lepton momentum was best fit with a $B_s/(B + B_s)$ fraction $f > 41\%$; the inclusive $D_s$ rate was consistent with $f = 30 \pm 18\%$; the dearth of reconstructed $B$ decays implied $1 - f < 59 \pm 34\%$; and five exclusive $B_s$ hadronic decay candidates suggested $f > 10\%$. The problems were (a) the low cross section at the $\Upsilon$(5S), (b) the relatively high background from the $u$, $d$, $s$, $c$ continuum, (c) the unknown background from $B$ and $B^*$ production, and (d) the fact that contributions from $B_s\overline{B_s}$, $B_s\overline{B_s^*}$, and $B_s^*\overline{B_s^*}$ caused three different mass peaks for reconstructed $B_s$. CLEO concluded that the evidence was not good enough to claim the discovery of the $B_s$. From time to time, the collaboration debated taking more $\Upsilon$(5S) data, but always decided that LEP or the Tevatron collider could produce $B_s$ much better than CESR. Eventually, several LEP experiments did publish convincing evidence for the $B_s$ and also the $\Lambda_b$.

There was one paper that CLEO did publish, but later wished it hadn't. In the course of our study of inclusive $\psi$ production in the non-$B\overline{B}$ continuum we took a look at the $\Upsilon$(4S) also. We had known for some time that there were $\psi$'s from the sequence,

$$e^+e^- \to \Upsilon(4\mathrm{S}) \to B\overline{B} \to \psi X X',$$

with momenta below the kinematic limit, $(M_B^2 - m_\psi^2)/2M_B = 2.0$ GeV/c. We were surprised, however, to see a signal for higher momentum $\psi$'s, at a rate above that from the background measured below $B\overline{B}$ threshold, that is, $0.22 \pm 0.07\%$ per $\Upsilon$(4S). Apparently, the $\Upsilon$(4S) had a significant branching fraction to non-$B\overline{B}$ final states. This was completely unexpected, since the decay rates of the lower upsilon states (to non-$B\overline{B}$, of course) were $\Gamma < 0.05$ MeV, compared with $\Gamma(\Upsilon(4\mathrm{S}) \to B\overline{B}) = 24$ MeV. Later we discovered that we had been fooled by an unlikely statistical fluctuation in the non-$B\overline{B}$ continuum under the $\Upsilon$(4S). The probability of a fluctuation beyond three standard deviations is only 0.26%, but if you make enough measurements, you will eventually be stung.

# 9 CLEO-2, CESR, and CHESS Upgrades, 1985-89

A detailed progress report [15] on the CLEO-2 design and prototype work (CBX-83/77) was presented to the Program Advisory Committee in December 1983; more definitive updates

(CLNS 84/609 and 85/634 [14]) appeared in May 1984 and January 1985. Major NSF funding started in November 1984, and continued for five years. The total upgrade cost was $37,380,000, of which $23.2 million went for the CLEO-2 detector and the rest for upgrading CESR and the Laboratory computing facility.

It's hard for me now to explain why the NSF was willing to support such a costly upgrade effort. We were asking to spend about $14 million on cesium iodide alone. Our competition at the time was the Laser Interferometry Gravitational Observatory proposal (LIGO). Some possible reasons for our success are (a) the CLEO-2 design was elegant and superbly adapted for the physics goals, (b) the CLEO collaboration had a good reputation for cost effective detector design and construction, and for productive exploitation of the physics potential, (c) the competition from ARGUS gave a sense of urgency, (d) we were ready to go and LIGO was not, (e) the CHESS program in x-ray science would benefit from the planned increase in CESR circulating currents, and (f) we had the confidence and enthusiastic support of David Berley, our NSF Program Officer, and of Marcel Bardon, the Physics Division Director. Maybe they wanted to cheer us up after our defeat on the CESR-II proposal.

McDaniel appointed Bernie Gittelman to manage the CLEO upgrade project (see Fig. 15, 16, and 18). I list below the major components and the institutions having primary responsibility for them. Other groups also had responsibilities for various parts of the electronics, software, and so on.

| | | |
|---|---|---|
| PT | inner 6-layer straw tube drift chamber | Ohio State (Kagan) |
| VD | intermediate 10-layer drift chamber | [existing] |
| DR2 | main 51-layer drift chamber | Cornell (Cassel) |
| TF | barrel scintillator trigger and TOF array | Harvard (Pipkin) |
| CC | cesium iodide shower scintillator array | Cornell (Stone) |
| | superconducting solenoid coil and iron | Cornell (Nordberg) |
| MU | muon proportional chambers | Syracuse (Moneti) |
| TFend | end-cap TOF scintillator array | Albany (Alam) |
| CCend | end-cap cesium iodide array | Cornell (Kubota) |
| LM | small angle luminosity monitor | Carnegie Mellon (Engler) |

The most expensive and time consuming part of the CLEO-2 construction effort was the cesium iodide scintillator array (see Fig. 15 bottom). This was a new kind of detector on a scale never before attempted. Many new problems had to be solved:

- specifying the cesium iodide purity, doping, and surface quality to be sufficient for good acceptable energy resolution while keeping the crystal cost as low as possible;
- finding a way to pay the vendors (BDH in England and Horiba in Japan) enough up front to set up for mass production, without breaking the first year's budget;
- getting crystals delivered at a rate consistent with installation of the full detector in 1988.

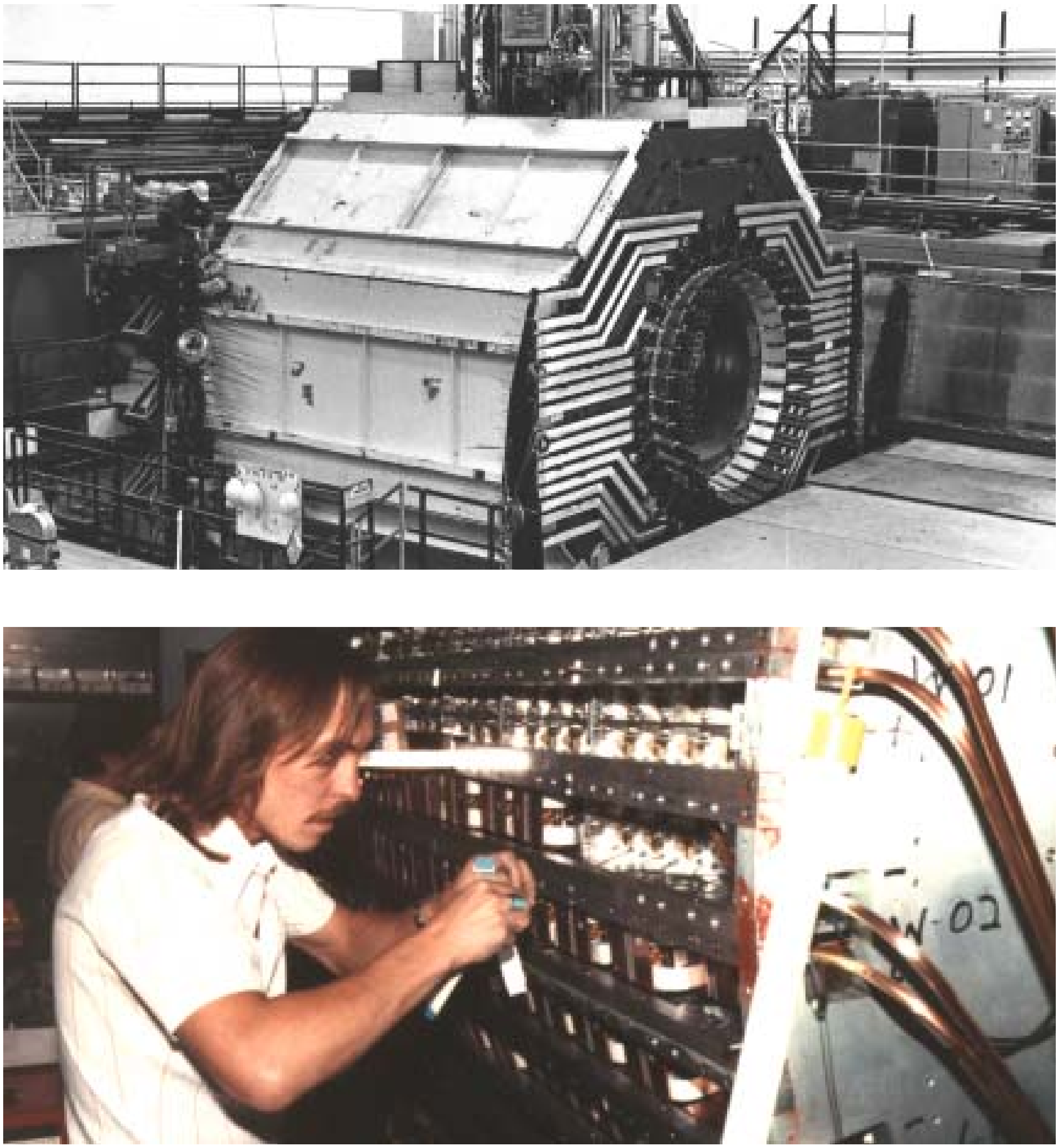


Figure 15: (top) The CLEO-2 magnet during installation. (bottom) Technician wiring the preamps on the CLEO-2 cesium iodide scintillator array

Figure 16: The CLEO-2 detector in the L-0 pit, with the west pole being inserted.

- finding photodiodes and preamplifiers that would have acceptably low noise and low cost;
- mounting the 7800 barrel scintillators in a robust, almost-massless structure with each crystal aimed at the interaction point;
- guaranteeing a failure rate low enough to survive many years of no access to the detector.

The details of the various solutions are given in several instrumentation publications (Appendix, Table XIII).

Oxford Instruments in England made the superconducting coil. Compared with the CLEO-1 solenoid, it was about 50% larger, ran at 50% higher field, and was much thicker, since now only the muons had to get through it. Most of the magnet iron (see Fig. 15 top) was machined out of pieces of the SREL synchrocyclotron magnet (Newport News, VA) by Dominion Bridge in Montreal. The Syracuse group built the muon detection chambers in the Iarocci style, that is, plastic channels with one anode wire per channel and crossed cathode strips. Instead of the Iarocci streamer mode, they used proportional mode, since the electronics was already available from the CLEO-1 $dE/dx$ system.

The last few weeks before the installation shutdown Riccardo deSalvo made a test of a 2 cm radius insert in the I.R. beam pipe to see how close we could get to the beam without being overwhelmed by backgrounds. The test was marginally successful; we backed off and decided on a 3.5 cm radius. Ohio State built a 5-layer straw tube PT (precision tracker) replacement for the former IV chamber. The installation shutdown started in April 1988 and lasted until August 1989. The CLEO-1 magnet was shipped off to Brookhaven.

In parallel with the hardware effort, Rohit Namjoshi led a team to write the software required for the new detector. They decided to throw out the rather cumbersome CLEO-1 structure that had evolved over the past decade and start fresh with a system built on the ZEBRA data-base program from CERN. For a while they seemed to be stuck in the block-diagraming stage, but they eventually emerged with some new code. Nobu Katayama was largely responsible for the data base management. The tracking was still based on TRIO for online and DUET for offline. There were two cesium iodide photon algorithms, one from Brian Heltsley, and the other derived by Tomasz Skwarnicki from the Crystal Ball experiment. The Monte Carlo simulation of the detector was based on the CERN GEANT program. All in all, the system worked quite well, although later, as we got more experience and got more clever with corrections that improved the efficiency and resolution, each data set got recompressed (reduced to data summary files) at least twice.

During the shutdown for the installation of CLEO-2 there were improvements made also to CESR and to CHESS. The CESR improvements, aimed at higher luminosity, were mostly evolutionary upgrades designed to make multibunch and microbeta work better. We began to realize as the upgrade progressed that the main obstacle to higher luminosity was the rf system. Running with two 14-cell cavities (Fig. 6) was definitely an improvement over running with one — it shortened the bunches, making the microbeta more effective — but it was getting increasingly difficult to keep two of the three 14-cell cavities in working order. As the CLEO upgrade was nearing completion well within the projected budget, the money was available to replace all the cavities with new ones. The NSF approved the reprogramming of upgrade funds, so we resolved to build four 5-cell cavities, plus one more for a spare. The new cavities had the following advantages:

- they were tuned for higher beam currents;
- we could put more power into each cell with 5 cells per rf window instead of 14;
- more of the vacuum joints were electron beam welded;
- the outer water jacket was made more easily demountable;
- the high order mode probes were more accessible;
- we corrected other mistakes in the original design.

The rf upgrade program started as the rest of the upgrade effort was coming to an end; the last of the 14-cell cavities was finally retired in 1993.

In order to keep CHESS competitive with existing and planned synchrotron radiation facilities at Brookhaven, Berkeley, Argonne, and other places, Boris Batterman and I felt that we had to expand the number of x-ray beam lines available. After toying for a while with the idea of digging into the hillside west of the existing CHESS areas to create more space, we realized that it would be more economical to build a CHESS-East area to use the x-rays emitted by the positron beam on the opposite side of the south I.R. from the existing CHESS(-West) electron beam area. This CHESS upgrade not only doubled the CHESS experimental facilities, but provided the opportunity to create a special station dedicated to irradiation of biologically hazardous specimens at the BL-3 level. The new CHESS-East area also got a 24-pole wiggler magnet to make very high intensities.

# 10 The CLEO-2 Years, 1989-95

Following the recommendation of the Program Advisory Committee, we spent the first six months or so after the CLEO-2 installation running at the $\Upsilon$(3S) energy. CUSB had the priority, and CLEO used the data to tune up the new detector. The monoenergetic photon lines from radiative transitions between $b\overline{b}$ bound states were especially useful for calibrating the cesium iodide. Although the first few months of data were rather ragged, the CLEO-2 detector turned out to be a great success, exceeding its projected performance goals in every respect. Never before had there been a detector with simultaneous momentum–energy resolution for charged particles and photons better than 2% (say at 1 GeV). CLEO published four papers based on the $\Upsilon$(3S) data (see Appendix, Table V). The first one showed the power of the cesium iodide calorimeter; the three photon lines from $\Upsilon(3S) \rightarrow \chi_b(2P)\gamma$ were beautifully resolved. In another paper we photon-tagged the $\chi_b(2P_0)$ and $\chi_b(2P_2)$ decays to $gg$ in order to make a direct comparison between gluon jets and quark jets from continuum $q\overline{q}$ production.

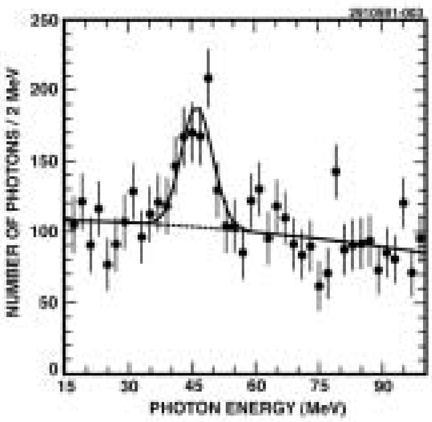
Following the $\Upsilon$(3S) run CESR did an energy scan in the region of the $B\overline{B^*}$ threshold. In the first published paper based on CLEO-2 data, (see Fig. 17 and Appendix, Table XII) CLEO used the inclusive rate for the 46.2 MeV photon line from $B^* \rightarrow B\gamma$ to measure the $B^* - B$ mass difference (improving on the earlier CUSB data) and the energy dependence of the inclusive $B^*$ production cross section. The experiment was motivated by Sheldon Stone's suggestion that one could produce $B\overline{B}$ in a charge conjugation +1 state via $e^+e^- \rightarrow B\overline{B^*} \rightarrow B\overline{B}\gamma$, and thus measure $CP$ violation in $B^0/\overline{B^0} \rightarrow \psi K$ interfering with mixing, without having to observe the time dependence, as you would have to do in the favored scenario with $C = -1$ pairs from $e^+e^- \rightarrow B\overline{B}$. This would have allowed CESR to measure $CP$ violation with equal beam energies, but unfortunately the $B\overline{B^*}$ production rate turned out to be seven times lower than the $B\overline{B}$ rate at the $\Upsilon$(4S). From then on, practically all of the running was at the $\Upsilon$(4S) resonance and immediately below $B\overline{B}$ threshold.

The accelerator physicists had made a convincing presentation to the PAC [15] that more luminosity could be obtained for the south interaction point (CLEO) if CESR were operated without collisions in the north (CUSB). Upon the advice of the PAC I decided to terminate the CUSB experiment after one more run on the $\Upsilon$(4S). The new CLEO-2 cesium iodide calorimeter could do everything that the CUSB BGO-plus-NaI detector could, and had good resolution for charged particles as well. CUSB under the leadership of the Franzinis had run in the north interaction region for eleven years and had accomplished a lot of good physics with a small collaboration, with limited resources, under hardship

VOLUME 67, NUMBER 13 PHYSICAL REVIEW LETTERS 23 SEPTEMBER 1991

**Measurement of the Inclusive $B^*$ Cross Section above the $\Upsilon(4S)$**

D. S. Akerib,[1] B. Barish,[1] D. F. Cowen,[1] G. Eigen,[1] R. Stroynowski,[1] J. Urheim,[1] A. J. Weinstein,[1] R. J. Morrison,[2] D. Schmidt,[2] M. Procario,[3] D. R. Johnson,[4] K. Lingel,[4] P. Rankin,[4] J. G. Smith,[4] J. Alexander,[5] C. Bebek,[5] K. Berkelman,[5] D. Besson,[5] T. E. Browder,[5] D. G. Cassel,[5] E. Cheu,[5] D. M. Coffman,[5] P. S. Drell,[5] R. Ehrlich,[5] R. S. Galik,[5] M. Garcia-Sciveres,[5] B. Geiser,[5] B. Gittelman,[5] S. W. Gray,[5] D. L. Hartill,[5] B. K. Heltsley,[5] K. Honscheid,[5] J. Kandaswamy,[5] N. Katayama,[5] D. L. Kreinick,[5] J. D. Lewis,[5] G. S. Ludwig,[5] J. Masui,[5] J. Mevissen,[5] N. B. Mistry,[5] S. Nandi,[5] C. R. Ng,[5] E. Nordberg,[5] C. O'Grady,[5] J. R. Patterson,[5] D. Peterson,[5] M. Pisharody,[5] D. Riley,[5] M. Sapper,[5] M. Selen,[5] H. Worden,[5] M. Worris,[5] P. Avery,[6] A. Freyberger,[6] J. Rodriquez,[6] J. Yelton,[6] K. Kinoshita,[7] F. Pipkin,[7] R. Wilson,[7] J. Wolinski,[7] D. Xiao,[7] A. J. Sadoff,[8] R. Ammar,[9] P. Baringer,[9] D. Coppage,[9] R. Davis,[9] P. Haas,[9] M. Kelly,[9] N. Kwak,[9] H. Lam,[9] S. Ro,[9] Y. Kubota,[10] J. K. Nelson,[10] D. Perticone,[10] R. Poling,[10] S. Schrenk,[10] M. S. Alam,[11] I. J. Kim,[11] B. Nemati,[11] V. Romero,[11] C. R. Sun,[11] P.-N. Wang,[11] M. M. Zoeller,[11] G. Crawford,[12] R. Fulton,[12] K. K. Gan,[12] T. Jensen,[12] H. Kagan,[12] R. Kass,[12] R. Malchow,[12] F. Morrow,[12] J. Whitmore,[12] P. Wilson,[12] F. Butler,[13] X. Fu,[13] G. Kalbfleisch,[13] M. Lambrecht,[13] P. Skubic,[13] J. Snow,[13] P.-L. Wang,[13] D. Bortoletto,[14] D. N. Brown,[14] J. Dominick,[14] R. L. McIlwain,[14] D. H. Miller,[14] M. Modesitt,[14] E. I. Shibata,[14] S. F. Schaffner,[14] I. P. J. Shipsey,[14] M. Battle,[15] H. Kroha,[15] K. Sparks,[15] E. H. Thorndike,[15] C.-H. Wang,[15] M. Artuso,[16] M. Goldberg,[16] T. Haupt,[16] N. Horwitz,[16] V. Jain,[16] R. Kennett,[16] G. C. Moneti,[16] Y. Rozen,[16] P. Rubin,[16] T. Skwarnicki,[16] S. Stone,[16] M. Thusalidas,[16] W.-M. Yao,[16] G. Zhu,[16] A. V. Barnes,[17] J. Bartelt,[17] S. E. Csorna,[17] T. Letson,[17] and M. D. Mestayer[17]




EO II Collaboration)

of Technology, Pasadena, California 91125
Santa Barbara, Santa Barbara, California 93106
iversity, Pittsburgh, Pennsylvania, 15213
orado, Boulder, Colorado 80309-0390
iversity, Ithaca, New York 14853
Florida, Gainesville, Florida 32611
sity, Cambridge, Massachusetts 02138
ollege, Ithaca, New York 14850
f Kansas, Lawrence, Kansas 66045
nnesota, Minneapolis, Minnesota 55455
w York at Albany, Albany, New York 12222
University, Columbus, Ohio 43210
klahoma, Norman, Oklahoma 73019
rsity, West Lafayette, Indiana 47907
ochester, Rochester, New York 14627
iversity, Syracuse, New York 13244
iversity, Nashville, Tennessee 37235

eceived 20 May 1991)

Using the CLEO II detector at the Cornell Electron Storage Ring, we have determined the inclusive $B^*$ cross section above the $\Upsilon(4S)$ resonance in the energy range from 10.61 to 10.70 GeV. We also report a new measurement of the energy of the $B^* \to B\gamma$ transition photon of $46.2 \pm 0.3 \pm 0.8$ MeV.

PACS numbers: 13.65.+i, 11.30.Er, 13.40.Hq, 14.40.Jz

We report a measurement of the inclusive $B^*$ cross section as a function of center-of-mass energy just above the $\Upsilon(4S)$ resonance. The value of the $B\bar{B}^*$ [1] cross section is necessary to determine the feasibility of observing time-integrated $CP$-violating asymmetries at a symmetric $B$ factory [2]. We can also compare the hadronic cross section in this energy region with predictions of several potential models [3]. The inclusive $B^*$ cross section is determined by measuring the yield of photons from the transition $B^* \to B\gamma$. The branching fraction for this transition is 100% because the mass difference between the $B^*$ and the $B$ mesons is too small to allow for the emission of a pion.

The data used in this analysis consist of 57.8 pb$^{-1}$

Figure 17: First paper based on CLEO-2 data.

conditions. Among other accomplishments, they discovered the radiative transitions to the $\chi_b$(1P) and $\chi_b$(2P) states in the upsilon system, and they had the first indication for the $B^*$. The CUSB detector was dismantled, the trailers on Upper Alumni Field were hauled away, and the CUSB experimenters went on to work with the DØ experiment at Fermilab. Later the Franzinis moved to Frascati to set up the KLOE experiment at the DAΦNI phi factory. The luminosity advantage for CLEO was in fact real; by the end of 1990 it had reached $1.5 \times 10^{32}/\mathrm{cm}^2\mathrm{s}$.

The CLEO collaboration had begun to grow more rapidly during the construction of the new detector. The first CLEO-2 paper had 133 authors from 17 institutions. The new institutions that had joined in the CLEO-1.5 era (see Appendix, Table II), Kansas, Oklahoma, UC Santa Barbara, Colorado, and CalTech, represented a westward shift in the center of gravity of the collaboration and included a group previously in ARGUS (Kansas) and groups from the SLAC orbit (the latter three). The new groups also broadened the spectrum of CLEO physics interests; for example, UC Santa Barbara (Morrison, Witherell, et al.) brought experience in charm decays from the Fermilab E-691 experiment, and CalTech (Barish, Stroynowski, et al.) was especially interested in tau physics. As the collaboration grew, Cornell became less dominant, and the proportion of DOE supported groups increased to about two thirds.

With increasing size the collaboration became more bureaucratic (see Appendix, Table III). In 1990 the CLEO Analysis Coordinator, David Besson, set up number of Physics Topic Analysis groups (PTA's): $B$ semileptonic decays, hadronic $B$ decays, rare $B$ decays, charmed mesons, charmed baryons, taus, and $QCD$ physics (a miscellany of upsilon spectoscopy, two-photon physics, fragmentation, etc.). The monthly CLEO meetings were supplemented the preceding or following day by PTA meetings, where most of the physics discussions took place. It became impossible for one individual to keep track of all the physics analysis activities going on in the collaboration. Some members in fact were interacting only with their PTA's and were unaware of anything else. I guess this sort of trend is inevitable in large organizations. The average shift running obligation per CLEO member was getting so sparse (shifts were manned by two physicists) that it was difficult to maintain continuity and familiarity with the running of the experiment.

There was not enough space in Wilson Lab to accommodate the increasing number of CLEO collaborators (see Appendix, Table II) and transient CHESS users. The extra wing added on the west side of the lab in 1985 was already inadequate. Several times LNS and CHESS submitted to the NSF a proposal for adding a fourth and fifth floor to the lab. It failed, either because we were not able to get Cornell to commit matching money, or because the guidelines for NSF infrastructure grants excluded additions to existing facilities. So we set up 'modular units' (like mobile homes) in the yard, three in 1989 and five in 1993. Space continued to be a perennial problem.

The increased CLEO manpower and the new ability to reconstruct kinematically final states with $\pi^0$'s and $\gamma$'s along with charged particles boosted CLEO's physics productivity. Up through year 2000 167 CLEO-2 papers were submitted for publication (see Appendix Tables): 18 on semileptonic $B$ decays, 39 on nonleptonic $B$ decays, 27 on charmed mesons,

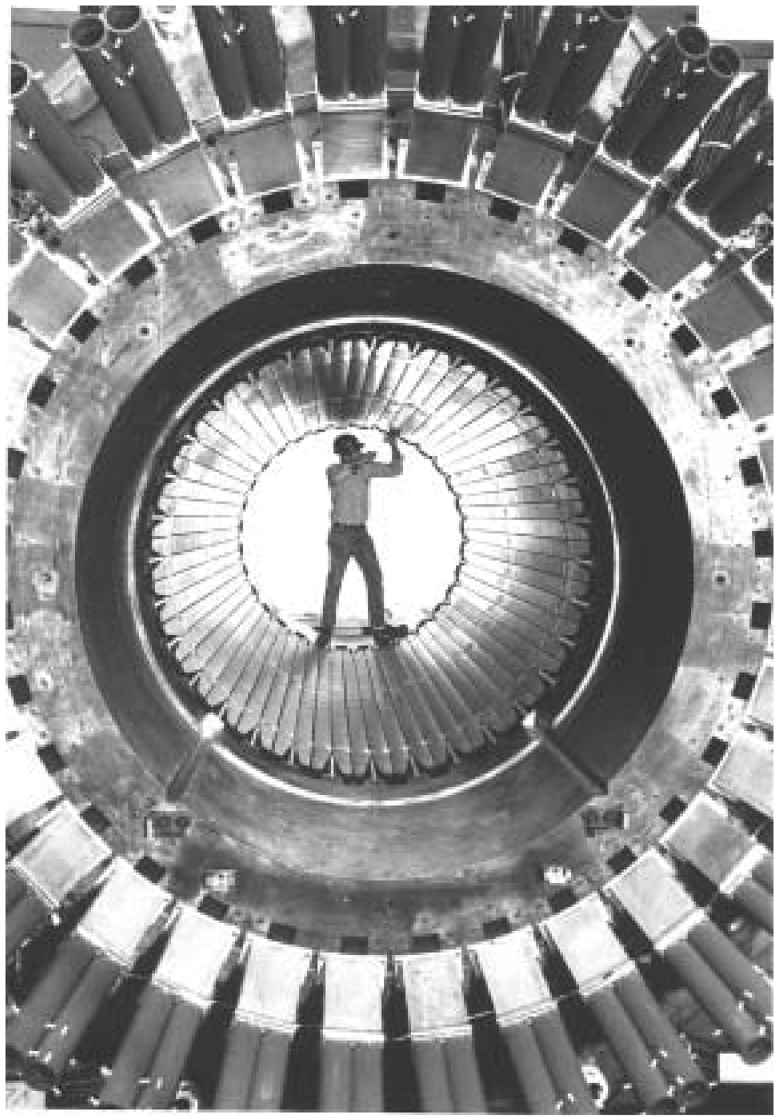


Figure 18: The time-of-flight scintillators in the CLEO-2 detector.

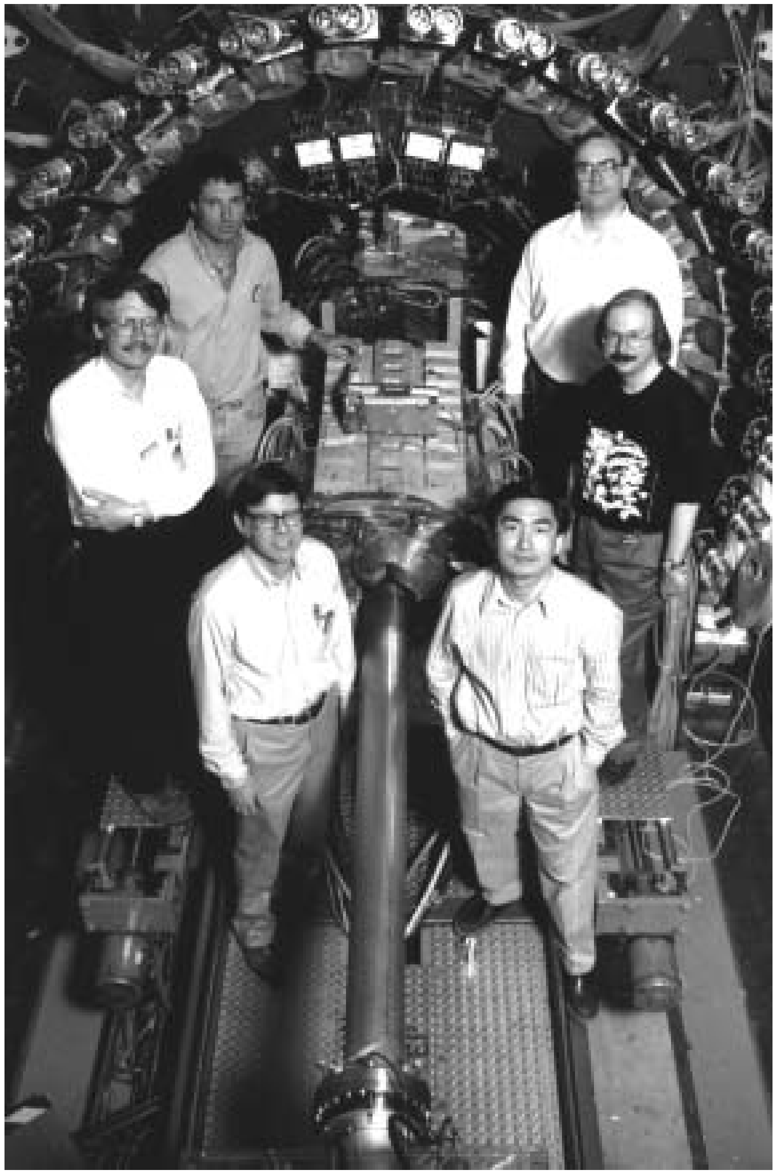


Figure 19: Clockwise from upper right: Mike Billing, Steve Playfer (Syracuse), Peter Kim, Ed Thorndike (Rochester), Dave Rice, and Yoram Rozen (Syracuse).

19 on charmed baryons, 34 on $\tau$ leptons, 11 on upsilon bound states, 5 on two-photon processes, and 4 others. CLEO had developed a machinery for producing papers. A small group or even a single individual would circulate a draft and make a presentation at a meeting. The collaboration would vote on whether it was publishable, perhaps in amended form, and if so, in which journal. The Analysis Coordinator would appoint a committee of experts to meet with the authors to suggest more work, rewrite the paper, or whatever they felt necesssary. The new version would be circulated to the collaboration with an invitation for comments. If the Analysis Coordinator felt that the changes warranted it would there be another collaboration vote before submitting for publication.

The CLEO-2 efficiency and resolution for $\pi^0$ and $\eta$ led to measurements of previously inaccessible $\tau$ branching ratios: $h^- \mathrm{n}\pi^0\nu_\tau$ ($h = \pi$ or $K$, n$= 1, 2, 3, 4$), $\pi^-\pi^+\pi^-\nu_\tau$, and $\pi^-\pi^0\eta\nu_\tau$. All the single-charged-prong tau decays were measured and previous measurements of the major tau branching fractions were improved, with the result that Martin Perl's deficit of exclusive decay rates relative to inclusive disappeared. Tau decay branching ratios agreed well with Standard Model predictions; no surprises there.

In the charmed meson domain CLEO made definitive measurements of the branching ratios for the five $D^*$ to $D$ transitions, involving $\pi^\pm$, $\pi^0$, and $\gamma$. Previous SPEAR measurements had been quite wrong. Using $D$'s tagged by the $D^*$ to $D$ transition, CLEO made precise absolute measurements of the branching ratios for the normalizing $D$ decay modes to $K^-\pi^+$ and $K^-\pi^+\pi^+$, important for charm cross section determinations. The spectroscopy of the $L = 1$ $D^{**}$ states was explored, and CLEO's results on the semileptonic decays of the $D$ tested the newly revealed heavy quark effective theory. The list of measured branching ratios was extended to include good measurements of $\pi^+\pi^-$, doubly Cabibbo suppressed $K + \pi^-$, and decays involving $\overline{K^0}$ and $\overline{K^{*0}}$.

CLEO also continued its dominance of the physics of the strange charmed mesons. The spectroscopy was advanced by papers on the $D_{s1}(2536)$, the $D^*_{s2}(2573)$ (a new discovery), and the $D^*_s - D_s$ mass difference. New $D_s$ decay modes involving $\eta$ or $\eta'$, plus $\pi$ or $\rho$ were studied. A measurement of the semileptonic decay $D_s \to \phi\ell\nu$ put the absolute normalization of all the $D_s$ modes on a firmer basis and produced more form factor measurements for heavy quark effective theory. The highlight of the $D_s$ work was the observation of the purely leptonic $D^+_s \to \mu^+\nu_\mu$ decay. Since this has to proceed through the annihilation of the $c$ and $\overline{s}$ quarks, it provides a measure of the decay constant $f_{D_S}$ characterizing the quark-antiquark bound state overlap. The interpretation of $B^0 - \overline{B^0}$ mixing in terms of Kobayashi-Maskawa matrix elements depends on knowledge of the decay constant for heavy-quark + light-antiquark, and the leptonic $D_s$ decay is so far the best source of experimental information.

Collaborators from Albany, Florida, Carnegie Mellon, and Ohio State specialized in charmed baryons. They produced a wealth of discoveries that made CLEO the prime source of data on baryon states containing the $c$ quark. Several papers were published on the decay modes of the $\Lambda_c$ — $pK\eta$, $\Lambda\pi$, $\Lambda\eta\pi$, $\Lambda K\overline{K}$, $\Lambda\ell\nu$, $\Sigma \mathrm{n}\pi$, $\Sigma\eta$, $\Sigma\rho$, $\Sigma\omega$, $\Sigma K\overline{K}$, $\Sigma^*\eta$, $\Xi K$, $\Xi K\pi$; the $\Lambda^*_c(2593)$ and $\Sigma^+_c$ were discovered; and the decays $\Xi_c \to \Omega K$ and $\Xi_c \to \Xi\ell\nu$ were observed.

During this period there was talk at several laboratories about building a Tau-Charm Factory, a high luminosity $e^+e^-$ collider to operate in the $c\overline{c}$ threshold region. For a while people at SLAC, Seville, CERN, Dubna, Novosibirsk, and Beijing were enthusiastic, but eventually financial realities and the wealth of tau and charm data coming out of CESR and LEP discouraged almost all of them. The SLAC tau-charm partisans eventually joined the CLEO collaboration. By 2001 it appeared that funding was assured for a Beijing tau-charm factory and that CESR would be modified to run at charm threshhold. Many of the original tau-charm goals have in the meantime been achieved at CESR operating at $b\bar{b}$ threshold energies and by LEP at higher energies.

Although the close proximity of interaction region quadrupoles makes electron tagging of the two-photon process, $e^+e^- \to e^+e^- hadrons$, impossible in CLEO, the very high effective rates for $\gamma^*\gamma^* \to hadrons$ makes it tempting to do $\gamma^*\gamma^*$ experiments in which only the *hadrons* are detected. The UC San Diego group, which had formerly been part of the PEP TPC-Two-Photon collaboration, joined CLEO in 1991 with the idea of exploiting the detector for two-photon physics. Previous measurements elsewhere of $\gamma^*\gamma^* \to \pi^+\pi^-$, $K^+K^-$, and $p\bar{p}$ were carried to higher energies and $\gamma^*\gamma^* \to \chi_{c2}$ was observed.

Of course, the *raison d'être* for CLEO-2 was $B$ physics. With a better detector and a larger data set we could improve many of the measurements that had been made with CLEO-1 and CLEO-1.5 — the semileptonic branching ratio, exclusive $B$ to charm decay modes, mixing, and $V_{ub}$ — and also push down the limits on various $b$-to-$u$ modes, semileptonic and those involving a $D_s$, as well as $B \to \ell^+\ell^-$. Several interesting new decays were observed: $B \to \Sigma_c X$ and the Cabibbo- and color- suppressed $B \to \psi\pi$.

Most notable was the discovery of several rare charmless decay modes. $\overline{B^0} \to \pi^+\pi^-$ involves $V_{ub}$ by having the $b$ and its partner $\overline{d}$ exchange a $W$ to become $u+\overline{d}$, and $\overline{B^0} \to K^-\pi^+$ is an effective neutral current $b$-to-$s$ transition that can occur through an intermediate $W^-+c$ (or $u$ or $t$) state. The Feynman loop diagram, complete with the spectator $\overline{d}$ and emitted gluon, was alleged by John Ellis to resemble a penguin in order to win a bet (or repay a debt) by getting 'penguin' printed in Phys. Rev. Letters. CLEO observed a peak in the reconstructed beam constrained $B$ mass, consistent with a branching ratio of $(2.4\pm0.8)\times10^{-5}$ to either $\pi^+\pi^-$ or $K^-\pi^+$ or a mixture of both. In order to suppress the rather serious non-$B\overline{B}$ background, a Fisher discriminant was formed from an optimal linear combination of several variables that had marginally different distributions for signal and background, according to Monte Carlo simulation. There were two ways to separate the $\pi\pi$ and $K\pi$ hypotheses: energy conservation ($E_h + E_\pi = E_{\rm beam}$), and $dE/dx$. Each method gives somewhat less than two standard deviations separation between $\pi$ and $K$, so the best we could do for the individual modes with the available statistics was to quote upper limits. This measurement was either the first observation of an exclusive $b$-to-$u$ mode or the first observation of a hadronic penguin decay, or both. It caused quite a stir among the theorists. Both channels are important for the measurement of $CP$ violation in $B$ decays.

Along with the $b \to sg$ hadronic penguin modes, one expects also $b \to s\gamma$ radiative penguin modes, the most likely channel being $K^*\gamma$. A team from Rochester, Syracuse, and Cornell (see Fig. 19) found signals in three charge combinations: $K^-\pi^+\gamma$ (Fig. 20), $K^-\pi^0\gamma$, and $K^0_S\pi^-\gamma$. The challenge in this measurement was the suppression of background from non-$B\overline{B}$ continuum $q\overline{q}$ jet events. Several distributions that were expected by Monte Carlo

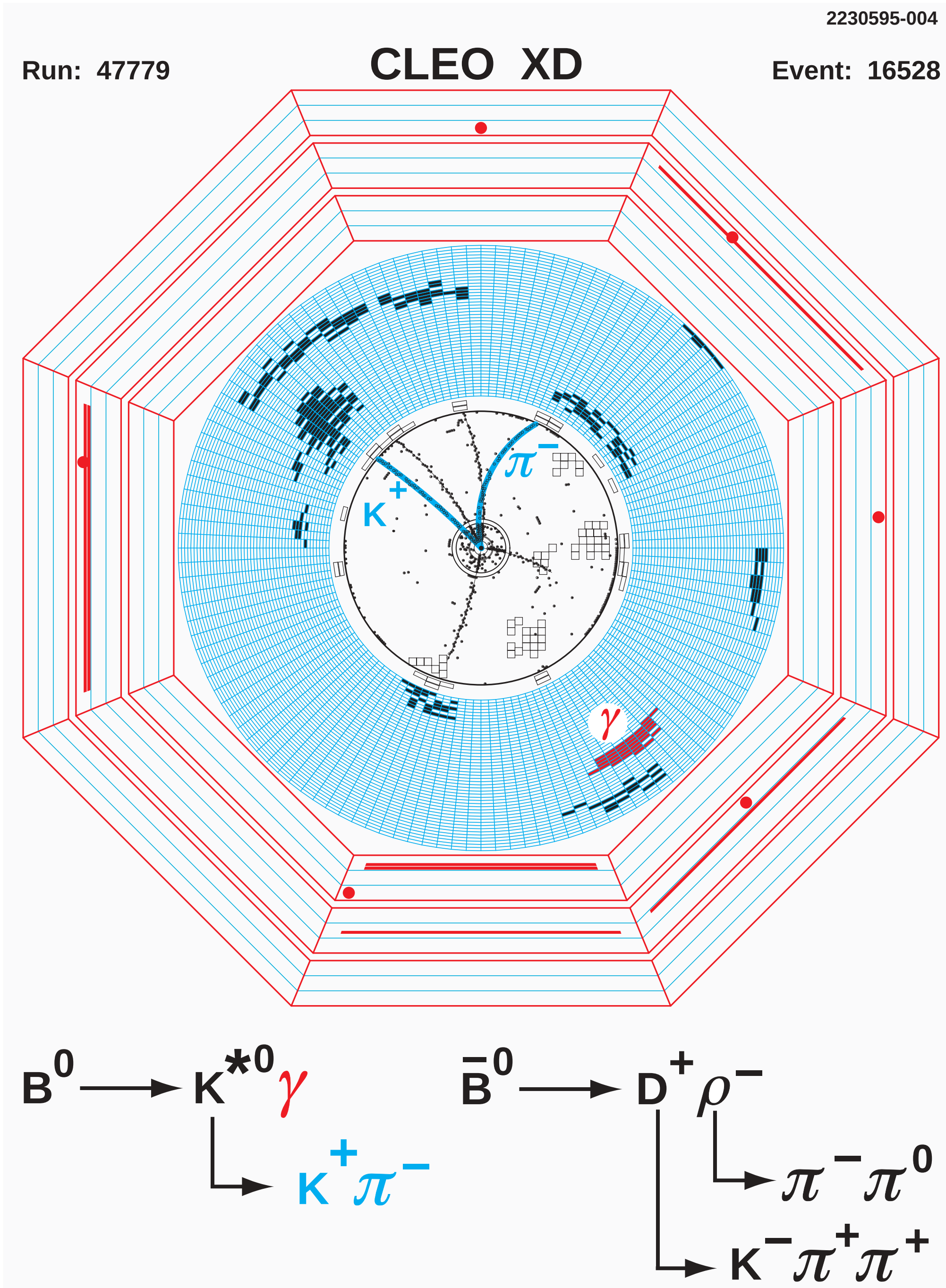



Figure 20: End view computer display of a $e^+e^- \rightarrow B^0\overline{B^0}$ event in CLEO-2, showing a radiative penguin decay: $B^0 \rightarrow K^{*0}\gamma_1$, $K^{*0} \rightarrow K^+\pi_1^-$. The other tracks come from $\overline{B^0} \rightarrow D^+\rho^-$, $D^+ \rightarrow K^-\pi^+\pi^+$, $\rho^- \rightarrow \pi_2^-\pi^0$, $\pi^0 \rightarrow \gamma_2\gamma_3$ ($\gamma_3$ escapes unseen). The CsI shower counter array is shown with the near end at the outer circle.

studies to be slightly different for signal and background were used to form a likelihood ratio for each event, for which a cut was defined. Standard Model expectations are rather explicit for inclusive $b \to s\gamma$, but there is no theoretical consensus on how much of the $s$ quark fragmentation should show up as $K^*(890)$. So although the measured average branching ratio, $\mathcal{B}(B^{+,0} \to K^{*+,0}\gamma) = (4.5 \pm 1.7) \times 10^{-5}$, was the first confirmation of the existence of the penguin mechanism, it was not a quantitative test of the theory.

This was remedied soon by an inclusive measurement of $b \to s\gamma$. The problem again was background suppression, but in this case, we were looking not for a reconstructed $B$ mass peak, but only for a high energy photon — not a distinctive signature. Two competing techniques gave consistent results. In the first, Ed Thorndike and his student, Jesse Ernst, combined eight event topology distributions into a single variable, using a neural net algorithm trained with Monte Carlo $b \to s\gamma$ signal and background from continuum $\gamma$ radiation and $\pi^0$ production. A fit of the data spectrum in this variable to Monte Carlo signal plus background spectrum shapes showed a significant signal. In the other analysis, Tomasz Skwarnicki required that the event have a reasonable $\chi^2$ for reconstructing as a $K + \mathrm{n}\pi + \gamma$ (n=1 to 4). The averaged branching ratio, $(2.3 \pm 0.7) \times 10^{-4}$, was consistent with the Standard Model prediction, using the value of $|V_{ts}|$ inferred from the measurements of $b$-to-$u$ decays and $B\overline{B}$ mixing, or alternatively, could be used for an independent measurement of $|V_{ts}|$. The amplitude for the loop diagram is sensitive to the presence of hypothetical particles. The agreement with the Standard Model can, for instance, be used to exclude a charged Higgs with a mass below 244 GeV, a much more restrictive limit than available by any other technique. The 1994 Lab holiday season card showed a snow scene with penguins.

Several important lessons followed from these first observations of rare $B$ decays.

- The loop decays opened an exciting window on nonStandard physics, competitive with experiments at multi-TeV facilities.

- Although CESR had reached the luminosity level required to see the most prominent KM-suppressed and penguin-loop decays, much more luminosity would be needed before CLEO could really explore the new field.

- Powerful, novel techniques were available for separating rare signals from copious backgrounds: cell list, Fisher discriminant, likelihood ratio, neural net.

- To capitalize on the potential of rare decays, CLEO would have to do a better job of $K - \pi$ separation at momenta above 1 GeV/c.

Semileptonic $B$ decay was a hot topic (see Table VIII). Not only is the lepton a good flavor tag (i.e., $b\overline{b}$ versus everything else, or $b$ versus $\overline{b}$ from the lepton charge sign), but the measurement of semileptonic branching ratios is our best source of information on the values of the KM matrix elements $V_{cb}$ and $V_{ub}$. The fact that the weak interaction is always $b \to W^- c$ (or $W^- u$) and $W^- \to \ell^- \overline{\nu}$ minimizes the confusion from multiple amplitudes, final state interactions, higher order effects, and so on; and the end point of the lepton momentum spectrum distinguishes $b \to W^- u$ from $b \to W^- c$. Comparison of the various exclusive rates as functions of the $\ell+\nu$ four-momentum $q^2$ tests the Heavy Quark Effective Theory used to understand the effect of the heavy-light $Q\overline{q}$ QCD dynamics on the weak decays. There are two discrepancies between theory and experiment. Naive counting rules corrected for phase space imply that the branching ratio $\mathcal{B}_{sl}$ for $B \to e\ell\nu$ should be about 16.5%. Corrections for hadronic enhancement of the nonleptonic rate can bring $\mathcal{B}_{sl}$ down as low as 11.5%. The CLEO inclusive data, however, imply $\mathcal{B}_{sl} = 10.4 \pm 0.3$. For a while it was possible to blame the disagreement on the model dependence of the correction for the unseen lower end of the experimental lepton momentum spectrum, but in the later measurements made with tagged events this was no longer a significant correction. Also, the LEP data, which initially implied a higher branching ratio, eventually fell into line with the CLEO result. The other problem was the fact that the sum of the measured exclusive branching fractions and upper limits for $B \to [D,\ D^*,\ D_1(2420),\ D_2^*(2460),\ \ldots]\ell\nu$ accounted for only two-thirds of $\mathcal{B}_{sl}$.

In the CLEO-1.5 era the main competitor for CLEO had been the ARGUS experiment at DORIS. Over the years, the friendly rivalry had benefited both groups by raising the level of enthusiasm within and outside the two collaborations, and by keeping both sides honest through checking of each other's results. The prospect of a new and better CLEO-2 detector prompted the ARGUS experimenters to make a complementary upgrade, that is, a precision vertex drift chamber located next to the beam pipe. Rather than cylindrical, it was planar, with pentagonal symmetry, and was built by their Canadian collaborators. Unfortunately, it developed a short circuit after it was installed and much of the solid angle was lost. Later it was replaced by a silicon microstrip detector. This time it was an accidental beam overexposure during machine studies that ruined the detector. Ultimately, it was the fact that DORIS lost the luminosity race that was the undoing of ARGUS. Since the construction of PETRA in the late 1970's, DORIS never got the primary attention of the DESY accelerator physicists, even though for the 1980's DORIS was the prime source of physics data for the laboratory. During the construction of HERA, DORIS luminosity continued to suffer from low priority, and half of the running time was dedicated to synchrotron radiation users. By 1993 the CESR peak luminosity had reached almost $3 \times 10^{32}/\mathrm{cm}^2\mathrm{sec}$, while DORIS had an order of magnitude less, so the running of ARGUS was terminated; actually, they had had very little successful data taking for several years. There was no more competition for CESR in the $b\overline{b}$ threshold region.

The $b\overline{b}$ production cross section is even higher at the $Z^0$ resonance than at the $\Upsilon(4\mathrm{S})$. As LEP gradually accumulated more integrated luminosity, and the properties of the $Z^0$ itself became well established, many of the ALEPH, DELPHI, L3, and OPAL experimenters turned their attention to $B$ physics. $B$ physics at high energies suffers from several disadvantages — accompanying fragmentation particles, poorer mass resolution, no reliable non-$b\overline{b}$ subtraction — but there are compensating advantages — resolvable decay vertices, separation of the $B$ and $\overline{B}$ into separate jets, and concurrent production of $B$, $B_s$, $B_c$, $\Lambda_b$, and other $b$-hadrons. The LEP experimenters learned to cope with the disadvantages and made good use of the advantages to produce results on the various $b$-hadrons, their lifetimes and semileptonic branching ratios, as well as on $B^0 - \overline{B^0}$ and $B_s - \overline{B_s}$ oscillations. LEP luminosity was never high enough, however, to rival CESR for rare $B$ decays. Although there was some overlap, $B$

physics at LEP tended to be mostly complementary to the the work at CESR. Once the LHC $pp$ collider is completed, the LHC-B experiment will be the focus of B physics at CERN.

The CDF experiment at the Tevatron $\overline{p}p$ collider enjoys a large $B$ production cross section, but is almost overwhelmed by backgrounds. CDF has been able to pick out good signals for decays involving muons, such as $\psi K_s$ and $\psi K^*$, allowing them to measure $B$ lifetimes and mixing, and they have the tightest upper limit on the forbidden $B \to \mu^+\mu^-$. For years there was talk at Fermilab of a dedicated $B$ physics collider detector. Although the Fermilab PAC approved the B-TeV proposal, funding is still in doubt.

# 11 The CESR B Factory Proposal, 1989-1993

The $CP$ operation, which takes particles into their mirror-image antiparticles, is not a symmetry of the universe, at least in our immediate vicinity, since we see mostly protons, neutrons, and electrons and hardly any antiprotons, antineutrons, and positrons. It is a puzzle how the the universe got to be this way. Sakharov suggested that the asymmetry must be related to the 1964 discovery by Fitch, Cronin, Christianson, and Turlay that $CP$ symmetry is violated in about 0.2% of the weak decays of neutral kaons.

In 1973, even before the discovery of the charmed quark, Kobayashi and Maskawa realized that in the Standard Model the $3 \times 3$ quark doublet rotation matrix connecting the energy and flavor eigenstates for six quarks could give rise to $CP$ violation at the level observed in kaon decay. It requires that each of the nine KM matrix elements $V_{ij}$ be nonzero and at least one be complex. If so, then $CP$ violation should occur also in $B$ decays, with the magnitude of the effect being proportional to the area enclosed by the triangle in the complex plane defined by the unitarity relation,

$$V_{td}V_{tb}^* + V_{cd}V_{cb}^* + V_{ud}V_{ub}^* = 0.$$

After the CLEO discovery of $b$-to-$u$ decays there was enough experimental information on the matrix elements to conclude that the enclosed area was indeed nonzero.

Bigi, Sanda, and others worked out the various ways in which $CP$ violation in $B$ decays could manifest itself experimentally. The interference term between two amplitudes involving different KM matrix elements but with same exclusive final state could contribute with the opposite signs to $CP$ conjugate $B$ and $\overline{B}$ partial decay rates. For example, there would be an asymmetry between the branching ratio for $B^+ \to K^+\pi^0$ (which can occur through a $V_{ub}V_{us}^*$ tree amplitude or a $V_{cb}V_{cs}^*$ loop amplitude) and the branching ratio for $B^- \to K^-\pi^0$.

There are two problems with this, one experimental and the other theoretical. The decay modes with the largest predicted asymmetries $\mathcal{A}$ have very low branching ratios $\mathcal{B}$, and vice versa. The size of the $B\overline{B}$ event sample required for a statistically significant $\mathcal{A}$ measurement is of the order of $1/\mathcal{B}\mathcal{A}^2$, with $\mathcal{B} \sim 10^{-5}$ and $\mathcal{A} \sim 10^{-2}$ for a favorable mode. The event sample would have to be several orders of magnitude larger than available in the 1990's. But once an effect were observed, its interpretation would be confused by the fact that the asymmetry is proportional not only to the imaginary component in the KM matrix but also to the sine of the strong interaction phase in the decay amplitude, and such phases are not yet reliably calculated.

Both difficulties are made easier by considering instead the interference between the two ways that a neutral $B$ can decay to a non-flavor-specific final state, like $\psi K_S$ or $\pi^+\pi^-$. The $B^0$ can either decay directly, or it can first oscillate to a $\overline{B^0}$ before decaying to the same final state. Since the mixing probability is rather large, the interference term can be significant. Another advantage is that the measurable asymmetry depends only on the KM matrix elements and not on strong interaction phases. Two experimental complications arise, however. First, you cannot tell whether you started with a $B^0$ or a $\overline{B^0}$ by observing the final state, as you could in the $K^+\pi^0$ versus $K^-\pi^0$ case. You have to rely on the fact that the $B$'s are produced in opposite flavor pairs, and *tag* the decay by observing the partner decaying into a flavor-specific mode, like $X\ell^{\pm}\nu$. Second, the two $B$'s oscillate coherently in such a way that for a $B^0\overline{B^0}$ pair produced in a $C = -1$ state (as from a virtual photon in $e^+e^-$ annihilation) the net asymmetry in the tagged rates is always zero. However, provided your detector has vertex resolution finer than the mean decay length, you can observe an oscillating *time dependent* asymmetry.

This does not yet solve the problem, because for $B\overline{B}$ produced from an $\Upsilon$(4S) at rest the mean decay length is only 30 $\mu$m, which is difficult (though probably not impossible) to resolve experimentally. In 1987 Pier Oddone of LBL suggested increasing the decay length by boosting the $\Upsilon$(4S) rest frame in the lab; that is, by colliding electrons and positrons of different energies. The two beams would have to be stored in separate, intersecting rings and brought to a common focus. The idea of separate rings for electrons and positrons was not new; Tigner (in CBN 82-24) had considered it as early as 1982 as a way of increasing beam currents. But the concept of asymmetric energies had to be checked out with realistic interaction region optics designs and beam-beam interaction simulations. A lot of design activity, workshops, and internal debate at many laboratories — Paul Scherrer Institute (Villigen, Switzerland), CERN, DESY, Novosibirsk, KEK, CalTech, SLAC, and Cornell — along with numerous workshops around the world, resulted in serious 'B Factory' proposals from KEK, SLAC, and Cornell. Our four-volume CESR-B proposal [14] was submitted to the NSF, and the SLAC PEP-2 proposal was submitted to the DOE, simultaneously in February, 1991.

The CESR-B luminosity goal was $3 \times 10^{33}/\text{cm}^2\text{sec}$ and the ring energies were 8 and 3.5 GeV. The design made use of the existing CESR 8 GeV ring and tunnel (see Fig. 21), the linac-synchrotron injector, and the CLEO-2 detector. The major items of new construction were a 3.5 GeV magnet ring, a copper vacuum chamber for both rings, a superconducting rf system, focusing magnets for the interaction region, and some additional building space. The CLEO-2 detector would be upgraded in data acquisition rate, vertexing capability, and particle identification.

In 1991 the agencies reviewed the CESR-B and PEP-2 proposals separately. An NSF cost review panel, chaired by Gerry Dugan, verified the CESR-B cost estimates and set the total project cost at \$116 million, including upgrades to the CLEO detector. A DOE panel set the PEP-2 cost at \$167 million *plus* the price of a new detector (unspecified, but about

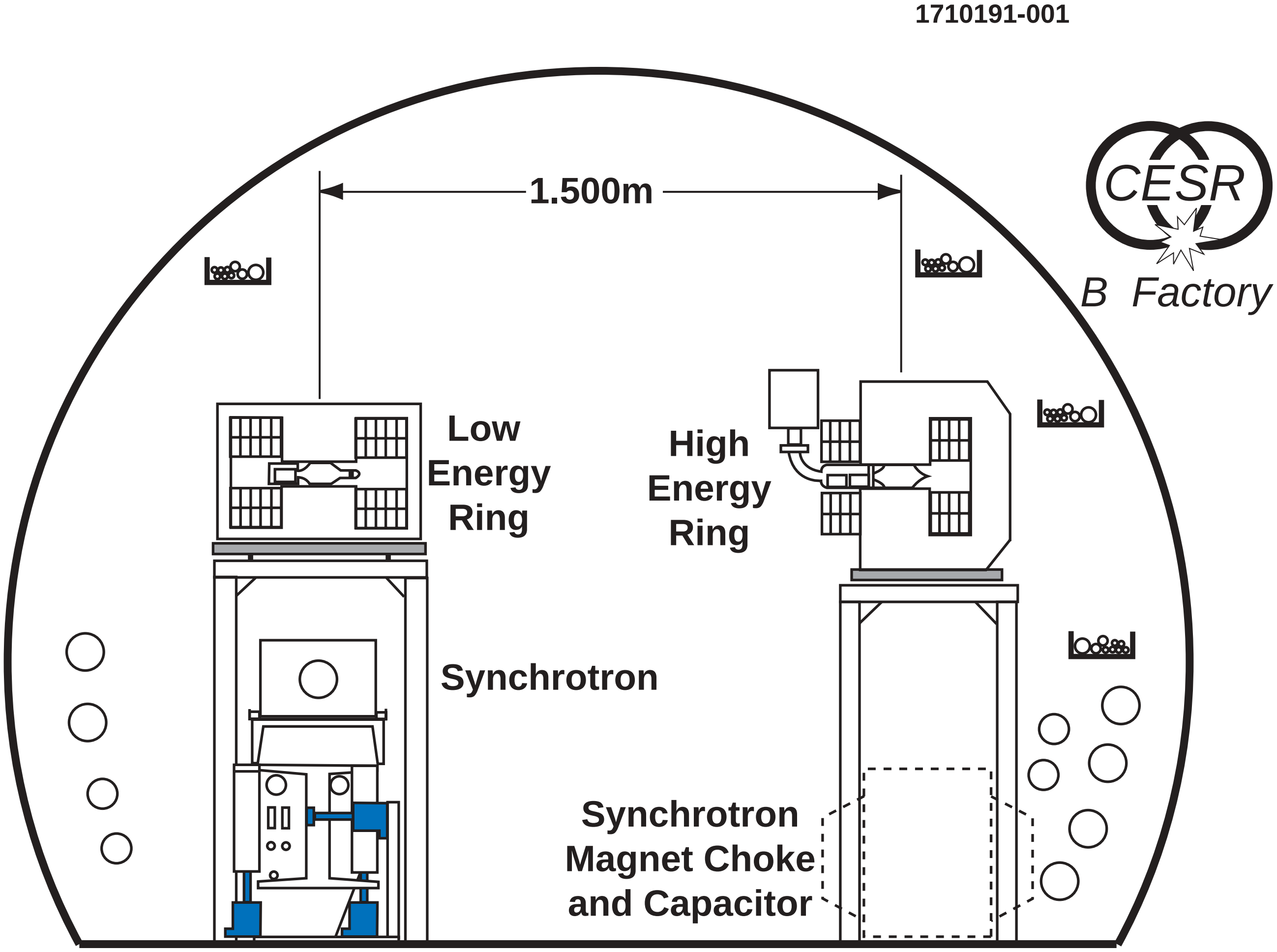



Figure 21: Sketch of the tunnel cross section showing the CESR-B rings and the synchrotron injector.

$50 million). It was pretty clear that at most only one of the two B Factory proposals would be funded, so the NSF and DOE decided to have a joint technical review of both proposals. But the NSF was already overcommitted on LIGO, and the DOE had no money for new initiatives beyond the Fermilab Main Injector project, so before the review could take place, Happer (the DOE director of Energy Research) and Sanchez (the NSF Associate Director for Mathematical and Physical Sciences) postponed further consideration indefinitely.

Meanwhile, with the the encouragement and support of the NSF, under Maury Tigner's leadership we were carrying out a program of B Factory r&d. Maury had returned to Cornell from the SSC Central Design Group in 1989.

- We built a full size superconducting protoype rf cavity and tested it successfully to full field at the specified $Q$ value.
- We tested a one-third size crab cavity model to full field.
- We tested a prototype high power window up to 250 kW.
- We measured the rf and vacuum properties of several brands of ferrite to confirm the practicality of the beamline loads for absorbing parasitic higher-order modes.
- CESR beam tests confirmed that there is no significant degradation in the luminosity for uncompensated beam crossing angles up to 2.8 mrad.
- Experiments with CESR tested ion trapping predictions.
- Theoretical studies and experimental measurements on transverse beam tails at high $\xi$ were carried out.
- Titanum sublimation pumps installed for test in CESR resulted in significant improvement in the vacuum near the IR.
- We studied beam related backgrounds with a small beam pipe in CLEO-2.
- We tested the performance of a fast multibunch feedback electrode in CESR with high current bunches spaced by 28 ns.

Most of this work would be useful for the future performance of CESR whether or not a B Factory were built.

Although the DOE budget request for fiscal year 1994 did not originally mention funding for a B Factory, California lobbying efforts resulted in adding $36 million for the first year of construction of the SLAC B Factory. New York's Senator Moynihan, knowing that there was also a proposal from Cornell, insisted that there be a stipulation that the site be selected only after there was a review of both proposals. The eventual DOE budget request made public in April 1993 contained the wording, "In addition . . . $36,000,000 . . . provided that no funds may be obligated for construction of a B-factory until completion, by October 31, 1993, of a technical review of the Cornell and Stanford Linear Accelerator proposals by the

Department of Energy and the National Science Foundation." Suddenly and unexpectedly we were in direct competition with SLAC for DOE (not NSF) B Factory funding.

The DOE and NSF set up a review committee under the chairmanship of Stanley Kowalski of the MIT Bates Laboratory, and charged it to make a joint technical review of the CESR-B and PEP-2 proposals and report back to the DOE Secretary by July 1993. Maury and his task force had about a month of hectic work to update our design and cost estimates and prepare our presentations. In June the committee spent a week at SLAC and then a week at Cornell.

Each proposal involved supplementing an existing ring by building a new low-energy ring, and the luminosity goals were the same, to be achieved in both cases by storing a large number of beam bunches. The CESR and PEP circumferences were 770 m and 2200 m, and the proposed energies were 8 & 3.5 and 9 & 3.1 GeV, respectively. In CESR-B the beams would collide at a 12 mrad crossing angle (necessitating rf transverse-mode 'crab' cavities to compensate) and all rf cavities would be superconducting. In PEP-2 the beams would collide head-on and the rf cavities would be copper. Mainly because of the smaller circumference and the existing detector, the Cornell proposal would be about half the price of the SLAC proposal. One could characterize the Cornell proposal as taking maximum advantage of new technology, while the SLAC proposal was pushing old technology beyond where it was tested. There were technical risks in both, of course.

The Cornell review went very well, I thought, and we convinced the committee that we had a viable design and the ability to carry it out. The report supported our position, that both proposals could meet the goals of a B Factory and that the Cornell proposal would cost about $100 million less. Even so, it seemed to be too much to expect that the new DOE Secretary, Hazel O'Leary, would decide for Cornell in preference to a traditional DOE laboratory. Indeed, in October President Clinton announced on a trip to San Francisco that the B Factory was being awarded to SLAC. From the context of the President's announcement it seemed that the basis of the award was political and not technical.

This of course was a disappointment for us at Cornell, especially for those who had worked so hard on the CESR-B planning and r&d. There were some who were relieved, though, that we would not be getting involved with the DOE bureaucracy. Anyway, we picked up the pieces and managed to reconstruct a viable future program for CESR.

There was some consolation in the fact that the Japanese B Factory designers adopted many of the features of CESR-B, and that both the BaBar detector design for PEP-2 and the Belle detector design for KEK-B were patterned on CLEO-2.

# 12 CESR and CLEO Phase II Upgrade, 1990-95

By 1991 CESR had reached a luminosity plateau corresponding to peak values from 2 to $3 \times 10^{32}/\mathrm{cm}^2\mathrm{sec}$, and for the next four years it delivered between 1.1 and 1.5 $\mathrm{fb}^{-1}$ (inverse femtobarns) of integrated luminosity per year. We had squeezed all we were going to get out of the microbeta, seven-bunch, one-interaction-region configuration. Although this was a world's record for luminosity, and we had beaten all the competition, it didn't seem enough.

When you run for more than four years at the same event rates, you eventually run out of interesting measurements that you can make in a reasonable time. Once we had discovered the first loop decay, $B \to K^*\gamma$ in 1993, we knew we had opened up a window on physics beyond the Standard Model, but at the rate we were going it would take forever to exploit it. The competition for the B Factory inspired accelerator physicists at Cornell to think creatively about luminosities like $3 \times 10^{33}$. Losing the B Factory would put CESR into competition eventually with facilities with that sort of performance.

The big question was how to put more beam bunches into CESR. When the beams collide head-on, the minimum longitudinal spacing $\Delta s$ between successive bunches has to be longer than the 73 m spacing between the two electrostatic separators flanking CLEO, or else you get multiple collision points. Since this distance is more than the length of one loop of the pretzel (typically $C/2Q_H = 768\text{m}/(2 \times 9.4) = 41$ m), the multibunch scheme allows only as many bunches $n_{e+} + n_{e-}$ as can fit into separate loops in the pretzel; at the normal $Q_H \approx 9.4$ CESR betatron tune, that means $n = n_{e+} = n_{e-} = 7$.

One obvious idea was to increase the horizontal tune $Q_H$. For a while we considered a 'CESR-plus' scheme to make more pretzel loops, raising $Q_H$ to about 13.9, allowing $n = 14$ [Dave Rubin, CLNS 89/902 and CON 90-1]. But it turned out to involve major changes in the CESR lattice. The next idea was the '$\Delta E$ scheme', also invented by Dave Rubin. He suggested [CON 90-2] using a $\sim 1.8\%$ energy difference between the two beams, so that they could circulate at separate radii. By arranging the dispersion he could get the beam orbits to coincide at the experimental I.P. without using electrostatic separators. In order to have two equilibrium orbits at different momenta, you need to have different field integrals $\int B d\ell$ along the two orbits, so Dave envisioned a bypass in the north area for one of the beams. Since the two beams would avoid each other everywhere except near the south I.P., there would be no loop constraint and it would be possible to reduce $\Delta s$ to perhaps 24 m and thus store $n = 32$ bunches. But the effort and expense involved in the bypass gave us pause, and there was no adiabatic way of approaching the new configuration step by step. We wouldn't know whether it would work until we had invested all the effort and money of building it.

In July 1990 Robert Meller wrote up a "Proposal for CESR Mini-B". Inspired by the B Factory proposal to intersect beams from separate rings at a $\alpha = 12$ mrad angle, he suggested using pretzel orbits in a single ring, crossing horizontally at $\alpha = 2$ mrad instead of colliding head-on as is usual in single ring machines ($\alpha$ is the angle between the beam and the no-pretzel beam line). The fields in the electrostatic separators would be adjusted to continue the pretzel through the I.R. with a node at the center. This would permit filling each pretzel loop around the ring with a train of bunches spaced by a distance $\Delta s$ just sufficient to separate electron and positron bunches by the minimum transverse miss distance at the first parasitic crossing point nearest to the desired intersection (see Fig. 22). The Meller scheme had several very attractive features.

- It would allow CESR to store up to five bunches in each pretzel loop, with a proportional gain in luminosity.

- It did not require extensive CESR lattice modifications.

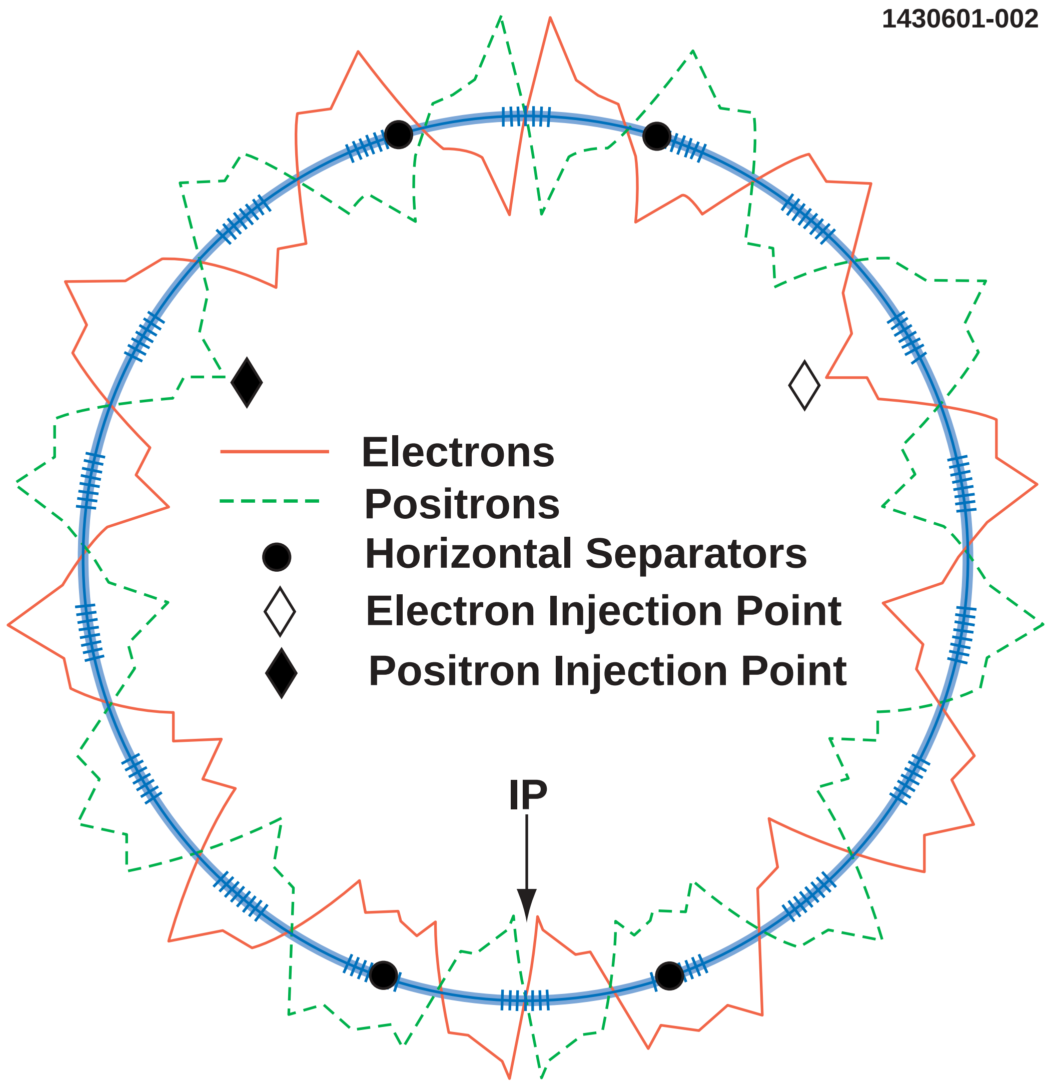



Figure 22: Diagram of the separated pretzel orbits crossing at an angle at the interaction point. Distances transverse to the nominal orbit curve are exaggerated. The tick marks show the points where electron and positron bunches pass each other.

- The various pieces of the scheme could be tried out one by one: asymmetric pretzels, crossing angles, bunch trains, high currents, and so on.
- Development of the technique would have the dual purpose of preparation for two-ring B Factory operation with high beam currents and nonzero crossing angle.

There were several problems to be solved.

1. The beam-beam interaction with an *angle crossing* can couple transverse and longitudinal oscillations and therefore excite synchro-betatron resonances. This was the phenomenon that limited the luminosity of DORIS in the original two-ring configuration. Theory said that the effect would be minimal though if the crossing angle were less than the $x$-versus-$z$ aspect ratio of the beam bunch: $\alpha < \sigma_x/\sigma_z$. This was verified in CESR; that is, the one-bunch-on-one-bunch luminosity was not significantly affected by crossing angles $|\alpha|$ up to 2.8 mrad with the 0.55mm × 18mm CESR bunches.

2. The long range beam-beam interaction at the *first parasitic crossing* a distance $\Delta s/2$ from the I.P. had to be minimized. The kick (horizontal or vertical) given to one beam bunch by the opposing one is proportional to $\sqrt{\beta_{H,V}}$, so the minimum separation $\Delta s$ that you can achieve for a given $\alpha$ depends on the $\beta$ functions at the parasitic crossing point. For the $\Delta s$ values that we needed in order to make bunch trains of 2 or 5 bunches ($\Delta s = 8.4$ or 4.2 m), the parasitic crossings occured near where $\beta_H$ and especially $\beta_V$ were going through large maxima in the final focus doublet. In order to get high luminosity we would have to reconfigure the I.R. quads for shorter focal lengths, bringing the $\beta$ maxima in closer to the I.P.

3. The crossing angle orbits made large excursions near the I.P., coming closer to the *aperture limits* in the final focus quadrupoles, especially during injection. The quadrupole apertures would need to be enlarged.

4. The *long range beam-beam interactions* of the many electron-positron near misses all around the ring could give a cumulative tune shift effect that would limit the beam current and the luminosity, especially if the separate beam-beam kicks added coherently. Measurements were made under various conditions in CESR, and Sasha Temnykh invented an empirical model that seemed to fit most of the data. Extrapolated to the many-bunch, high-current regime where we had no good data, it suggested that the luminosity might increase only as the square root of the number of bunches instead of linearly.

5. Wakefield effects would be worse with more bunches and shorter $\Delta s$. To combat them Joe Rogers designed and installed a fast, digital multibunch *feedback* system to stabilize the beam. After some initial troubles, this was quite effective.

6. It was not clear how much beam current the four five-cell copper *rf cavities* could sustain. First, there was a limit to how much power one could couple through the

cylindrical quartz windows in the input waveguides. But probably more importantly, the broad-band impedence of the cavities would allow the wakefields generated by the many high-current bunches to resonate and limit the achievable beam current. It is difficult to design a cavity shape with high shunt impedence (accelerating Volts$^2$ per input Watt) at the fundamental accelerating frequency and simultaneously low broadband impedence (wake Volts per beam Amp). The solution to this problem is superconducting cavities. With the low wall dissipation you can produce accelerating Volts with minimal input Watts, thus giving you the option of sacrificing some shunt impedence in the cavity geometry to get low broadband impedence. The decreased power dissipation also allows you to put more power into the beam before reaching the window heating limit. Since the same reasoning holds for B Factory cavity design, the SRF group was developing a special single-cell superconducting cavity to be used either in a CESR B Factory or for the Meller-scheme luminosity upgrade.

7. The intense synchrotron radiation from high beam currents would bombard the *vacuum* chamber walls and cause increased heating and outgassing. Unless pumping speed were improved, the effect would be shorter beam lifetimes and increased backgrounds in the CLEO experiment. Step by step the most critical vacuum system components would have to be upgraded as the beam currents increase.

8. Increased beam currents require higher *injection* rates (Amps per second). Fortunately, we were not trying to increase the per bunch linac currents, but upgrades would be necessary in the injection efficiency and in the power capability of the positron converter target.

9. The I.R. focusing could not be pushed significantly closer to the collision point (see item 2, above) without intruding on the *CLEO tracking chambers*, which would therefore have to be replaced.

After we had submitted the B Factory proposal in 1991, and received the Happer-Sanchez letter postponing indefinitely any action on the proposal, we began to make serious plans for the no-B-Factory alternative, that is, a more modest CESR luminosity upgrade, combined with an upgrade of the CLEO detector. We decided on two stages, dubbed 'phase II' and 'phase III' by Dave Rice. Phase I corresponded to previous upgrades already completed.

*Phase II* included whatever we could do on a short time scale with minimal commitment of resources: (1) reconfiguration of the I.R. focusing with enlarged bore for the electromagnetic quads and lengthening the permanent magnet quads (using pieces from the ones retired from the north area), (2) new water cooled beryllium beam pipe and masking near the I.P. (see Fig. 23 top), (3) replacement of the CLEO PT straw tube chamber by a three-layer, double-sided, silicon detector (Fig. 23 bottom), (4) various CESR improvements to the vacuum, linac, and feedback system. Of the list of problems above, this would deal partially with #2, 3, 5, 7, 8, 9. The CLEO silicon tracker was motivated by (a) the hope that more charm decays would be identified, both from $B$ decays and continuum production, and (b) the experience that we would get for the kind of measurements that would be important at a B

Factory. Phase II would begin immediately, but anything beyond it could be displaced by CESR B Factory construction.

*Phase III* would involve more time, three or four years, and more money, over $30 million counting contributions from CLEO collaborators, and would therefore require a special NSF commitment in order to proceed. It would include (1) a superconducting upgrade of the I.R. quads, (2) superconducting rf cavities, (3) a rebuilding of the inner part of the CLEO detector, and (4) more improvements to the CESR vacuum and linac; addressing problems #2, 4, 6, 7, 8, 9.

One of the beauties of this plan was that it coincided almost exactly with the r&d to demonstrate the feasibility of the B Factory proposal, plus some parts of the actual B Factory construction. In early 1993 the phase III proposal was submitted to the NSF as part of the five-year proposal for CESR/CLEO operations for fiscal years 1994 through 1998. Meanwhile progress on phase II was being paced by the assembly of the silicon tracking detector for CLEO, which took longer than anticipated. The various responsibilities were apportioned as follows.

| | |
|---|---|
| Beryllium beam pipe & masks | Harvard (Yamamoto) |
| Silicon detector assembly | UC Santa Barbara (Nelson) |
| electronics | Cornell, Illinois, ... (Alexander) |
| movable shielding | Cornell, Carleton (Dumas) |
| software | Cornell, UCSB, Ill. (Katayama) |
| VD repair and recabling | Ohio State (Kagan) |
| Pipe, silicon, VD assembly | Purdue, Cornell (Fast) |
| I.R. installation | Cornell (Kandaswamy) |

The CLEO delays prompted the accelerator physics crew to run CESR in the crossing-angle, bunch-train mode before the installation of the I.R. focus modifications, even though the configuration was far from optimum for high luminosity. It went much better than anyone expected, and running in the 9-train × 2-bunch mode with $\alpha = 2.0$ mrad and $\Delta s/c = 28$ ns was declared the standard in early 1995. A month before the phase II installation shutdown began in April, CESR made a new luminosity record, $\mathcal{L}_{pk} = 3.2 \times 10^{32}/\text{cm}^2\text{sec}$.

The work during the shutdown involved more than just CLEO and the interaction region. The linac, vacuum system, electrostatic separators, and rf system were all refurbished, and the shielding between CESR and CHESS was upgraded. CLEO took advantage of the shutdown to repair a broken wire in the VD and to convert the gas system to operate the DR and MU chambers with a helium based gas instead of the former argon-ethane mixture. This involved the installation of a system for flushing nitrogen through the time-of-flight photomultiplier housings in order to avoid leakage of accumulated helium through the glass. There was a scare when the second half of the CLEO silicon detector (Fig. 23 bottom) came from Santa Barbara damaged, but the loss in number of good data channels turned out to be minimal. CESR and 'CLEO-2.5' turned on in October 1995.

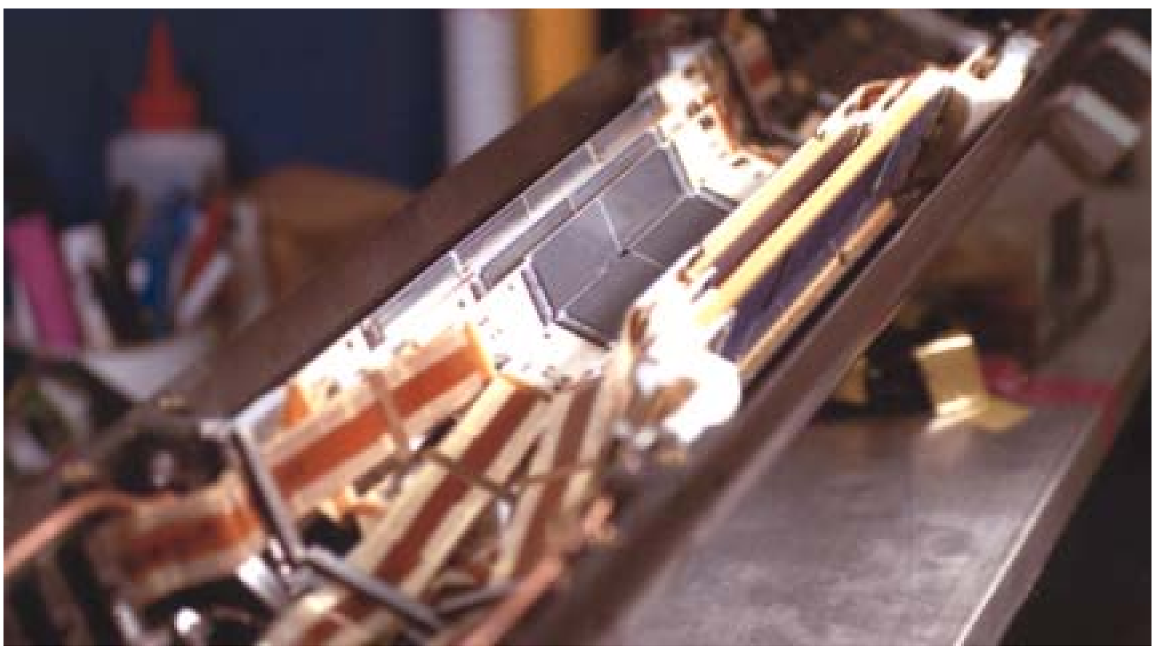


Figure 23: (top) Jeff Cherwinka, Denis Dumas, and Ken Powers assembling the I.R. beam pipe for the phase II upgrade. (bottom) One half of the phase II CLEO silicon tracking detector.

# 13 The CLEO-2.5 Years 1995-1999

The upgraded CLEO detector, called CLEO-2.5 or CLEO II.V, was a new device as far as charged particle tracking was concerned. Not only was the inner straw tube drift chamber replaced with three layers of double-sided silicon strips, but the gas in the main drift chamber was also changed from argon-ethane to helium-propane. With its hit resolution of 20 $\mu$m in $r\phi$ and 25 $\mu$m in $z$ the silicon had the potential of significantly improving the resolution for extrapolation of tracks into the vertex, and the new drift chamber gas with its 14% improved hit-on-track efficiency and reduced multiple scattering offered significantly improved momentum resolution. Understanding the new configuration and getting the ultimate efficiency and resolution was a considerable effort though. It led to a better understanding also of the earlier CLEO-2 tracking. This prompted a desire to reap the benefits of this improved understanding by repeating the event reconstruction for the past CLEO-2 data and Monte Carlo with updated tracking software. So eventually we had three data sets to compare: the original CLEO-2, CLEO-2-recompress, and CLEO-2.5. At first there were significant disagreements among all three in event efficiencies – and for a while the two newer data sets did not look so good. This had to be understood during a time when most CLEO members were involved in building CLEO-3. It took much longer than anyone anticipated, but was eventually accomplished. As a result, the publication of many CLEO-2.5 data analyses was delayed, and CLEO-2 data were still being studied many years beyond the start of CLEO-2.5 running.

The rapid growth of the collaboration slowed. It peaked in 1996 with 212 authors on the CLEO papers (see Table IV). Although new members joined in later years (Table II), the outflow to other collaborations, mainly westward to BaBar and BELLE, caused the membership to plateau for a while and then to decline slowly. With so many members and so much work to do, the management of the collaboration became complex enough to require a change from single spokesman to two co-spokesmen, starting in 1997 with Ed Thorndike and George Brandenburg. This also made it easier for non-Cornellians to take on the leadership responsibility. The CLEO data taking shifts evolved from two CLEO members for each of the three shifts, to one per daytime shift (plus two on each of the other two shifts), eventually to one CLEO physicist per shift plus one hired technician to handle the routine operations. In December 1999 we celebrated 20 years of CESR, CLEO, and CHESS with invited outside speakers reciting the accomplishments in the Theater Arts Center, followed by an evening banquet in the Statler Ballroom. Twenty years of running is a very long time for any high energy physics collaboration, perhaps a record.

A nice demonstration of the power of the silicon detector was Dave Cinabro's observation for the first time of the beam-beam pinch effect in the horizontal width of the beam at the collision point. The silicon data on displaced track vertices enabled CLEO to improve significantly on previous measurements of the $D^+$, $D^0$, $D_s$ and $\tau$ lifetimes. CLEO also set a limit on $D^0 \leftrightarrow \bar{D}^0$ mixing, using the time dependence to separate the mixing from double Cabibbo-suppressed decays.

As before, however, the main thrust of the CLEO analysis effort was in the area of $B$

meson decays, with data taken at the $\Upsilon(4S)$ resonance and just below $B\overline{B}$ threshold in 2:1 ratio. For most $B$ analysis topics the new CLEO-2.5 data were combined with the data set available from the 5 /fb of pre-silicon CLEO-2 integrated luminosity. Much of the published work (see Appendix Tables) in this period involved improvements in the accuracy of the measurements that fix the sides of the unitarity triangle in the complex plane representing the relation

$$V_{td}V_{tb}^* + V_{cd}V_{cb}^* + V_{ud}V_{ub}^* = 0$$

among the elements of the Cabibbo-Kobayashi-Maskawa matrix.

- $|V_{cb}|$ was obtained from the branching fraction for semileptonic $B$ decays to charm – inclusively, and in the exclusive channels $B \to D\ell\nu$ and $D^*\ell\nu$.
- $|V_{ub}|$ came from the branching fraction for semileptonic $B$ decays to noncharm final states – inclusively from the tail of the lepton momentum spectrum beyond the end point for decays to charm and exclusively from $B \to \pi\ell\nu$ and $\rho\ell\nu$.
- $|V_{td}|$ could be obtained from measurements of the rate for $B^0 \leftrightarrow \overline{B^0}$ mixing. Here the LEP and Tevatron measurements of the time dependence of the oscillation and limits obtained for the $B_s \leftrightarrow \overline{B_s}$ oscillation eventually eclipsed the data from the CLEO time averaged measurements.
- $|V_{ts}|$ could be obtained from the rate for $B \to X_s\gamma$. This measurement was steadily improved with more data. Although it was viewed mainly as setting a limit on exotic high mass objects contributing in the loop, one could take the Standard Model as given with $W$ and $t$ in the loop and get a determination of $|V_{ts}|$.

Each of these measurements reached a level such that the accuracy of the determination of the area of the unitarity triangle, upon which the strength of $CP$ violation in $K$ or $B$ decays depends, was eventually limited by model uncertainty in the connections between experiment and $|V_{ij}|$.

Another major thrust of the CLEO analysis in the CLEO-2.5 period was measuring and setting limits for branching ratios and charge asymmetries of rare charmless hadronic $B$ decays. CLEO discovered the decays to $K\pi$, $K\eta'$, $K\phi$, $K^*\eta$, $K^*\phi$, $\pi\pi$, $\pi\rho$, and $\pi\omega$, typically in several charge combinations. Measured branching fractions ranged from $8 \times 10^{-5}$ for $K\eta'$ down to $4 \times 10^{-6}$ for $\pi^+\pi^-$, and limits were obtained in many modes ranging as low as $2 \times 10^{-6}$ for $K^+K^-$. These results set off a wave of theory papers discussing the decays in terms of amplitudes involving $b \to u$ tree diagrams, $b \to sg$ gluonic penguin loops, electromagnetic penguins, and occasionally $W$ exchange or annihilation diagrams. Of special interest was the unexpectedly high $K\eta'$ rate, still not understood. The motive for much of this work was the possibility that at least some of these decays would show direct $CP$ violation from the interference of tree and loop amplitudes. CLEO looked for asymmetries in five of these modes (and also in $\psi^{(\prime)}K^\pm$, $K^\pm\gamma$, and $X_{s,d}^\pm\gamma$) but did not see any at the 12 to 25% level of sensitivity.

In the charm decay sector CLEO-2 and -2.5 data provided precision measurements of key normalization modes in charm and bottom physics: $D \to K\pi$, $D \to K\pi\pi$, $D_s \to \phi\pi$, and the $D^*$ to $D$ decay modes. It was also possible to improve the measurement of the rate for $D_s \to \mu\nu$, an important check on lattice determination of decay constants. The collaboration continued its dominance of the field of charmed baryon spectroscopy with the discovery of over half of all known states of the $\Lambda_c, \Sigma_c$ and $\Xi_c$.

With the world's largest sample of $\tau$ decays, CLEO specialized in rare and forbidden decay modes, with sensitivities in the $10^{-4}-10^{-6}$ range. The large data sample also enabled the tau specialists in the collaboration to pursue a detailed exploration of hadronic spectral functions. However, efforts to improve the upper limit on the tau neutrino mass were disappointing.

# 14 Building CLEO-3 1996-2000

The outstanding weakness of each CLEO detector has always been high momentum particle identification – in particular, distinguishing kaons from pions of the same momentum. There are three observables that can be used, at least in principle, to measure particle velocity: time of flight, ionization, and the Cherenkov effect. Once velocity and momentum are known, mass follows from $m = p\sqrt{1-\beta^2}/\beta c$. The problem for each of these techniques is that at high momentum, $\beta$ gets immeasurably close to one, whatever the mass is. One therefore has to measure flight time ($= L/\beta c$), ionization ($\sim const/\beta^2$), and/or Cherenkov angle ($\cos\theta = 1/\beta n$) to very high precision, and over most of the solid angle. For CLEO-2.5 (or -2), the $K-\pi$ separation in ionization at the 2.5 GeV/c momenta important for distinguishing $B \to K\pi$ from $B \to \pi\pi$ was only 2.0 (or 1.7) standard deviations, and the resolution in time of flight at that momentum was useless. Three parallel r&d efforts were carried out to find a better solution: an aerogel threshold Cherenkov counter system, a high pressure threshold sulfur-hexafluoride gas threshold Cherenkov counter array, and a ring imaging Cherenkov counter. The latter (RICH) appeared to be best able to provide at least three standard deviations of $K-\pi$ separation over the full momentum range. Giving up the existing time of flight counter array and reducing the outer radius of the drift chamber would provide enough radial space for a proximity focused RICH counter with photon detection by TEA, wires, and cathode pads.

The other main motive for upgrading CLEO was the interference between the existing drift chamber flat end plates and any significant improvement in the IR focusing quadrupoles. A new design with an endplate stepped inward for shorter wire length at smaller radii would allow us to install superconducting quadrupoles close to the interaction point (see Fig. 25). Tracking resolution could be maintained in spite of the reduced outer radius by replacing the inner silicon layers and the VD chamber by a new, larger 4-layer silicon detector. This would be an opportunity to take advantage of recent advances in radiation hardening of silicon and replace the limited life 3-layer silicon detector before it died.

Chris Bebek had the job of managing the CLEO upgrade – budgeting, scheduling, co-ordinating parallel activities. The Syracuse group took the main responsibility for building the RICH detector with help from Southern Methodist, Albany, and Wayne State. The

Figure 24: Assembly of the Ring Imaging Cherenkov detector at Syracuse University.

detector was to be a cylindrical shell, comprising in radial order lithium fluoride crystal radiators, both planar and saw-toothed, a gas volume, calcium fluoride crystal windows, TEA+methane gas to produce photoionized charges, wires for multiplication, cathode pads, and readout electronics. The main bottleneck turned out to be the production of LiF and $CaF_2$ plates. Optivac, the supplier, had problems with quality control and it was only with intensive intervention by the Syracuse crew (Ray Mountain, in particular) that enough radiator and window pieces were finally delivered – about a year late. The RICH, which had to go into CLEO first, was installed starting in June, 1999. CLEO-2.5 data taking had already stopped in February, 1999, the inner part of the detector had been dismantled, and the remainder had been running as a test bed for the new, faster readout and trigger electronics that had been developed by Ohio State, Illinois, Purdue, and Cal Tech.

The drift chamber went in immediately after the RICH. DR3, as it was called, was designed and assembled at Cornell under the direction of Dan Peterson. Most of the wires were strung by hand; the innermost layers in the stepped "wedding cake" part of the endplate were strung by a robot constructed by the Vanderbilt group. Rochester provided the outer cathode z-strip layer.

The new silicon detector consisted of four layers of double-sided silicon wafers arranged cylindrically around the new 2.1 cm radius water-cooled, double-walled beryllium beam pipe. Ohio State, Cornell, Harvard, Kansas, Oklahoma, and Purdue shared the job of producing the silicon detector and its electronics. It turned out to be a much more time consuming project than anyone anticipated. There were serious delays in component deliveries and assembly was labor intensive. The silicon detector was not ready for installation when

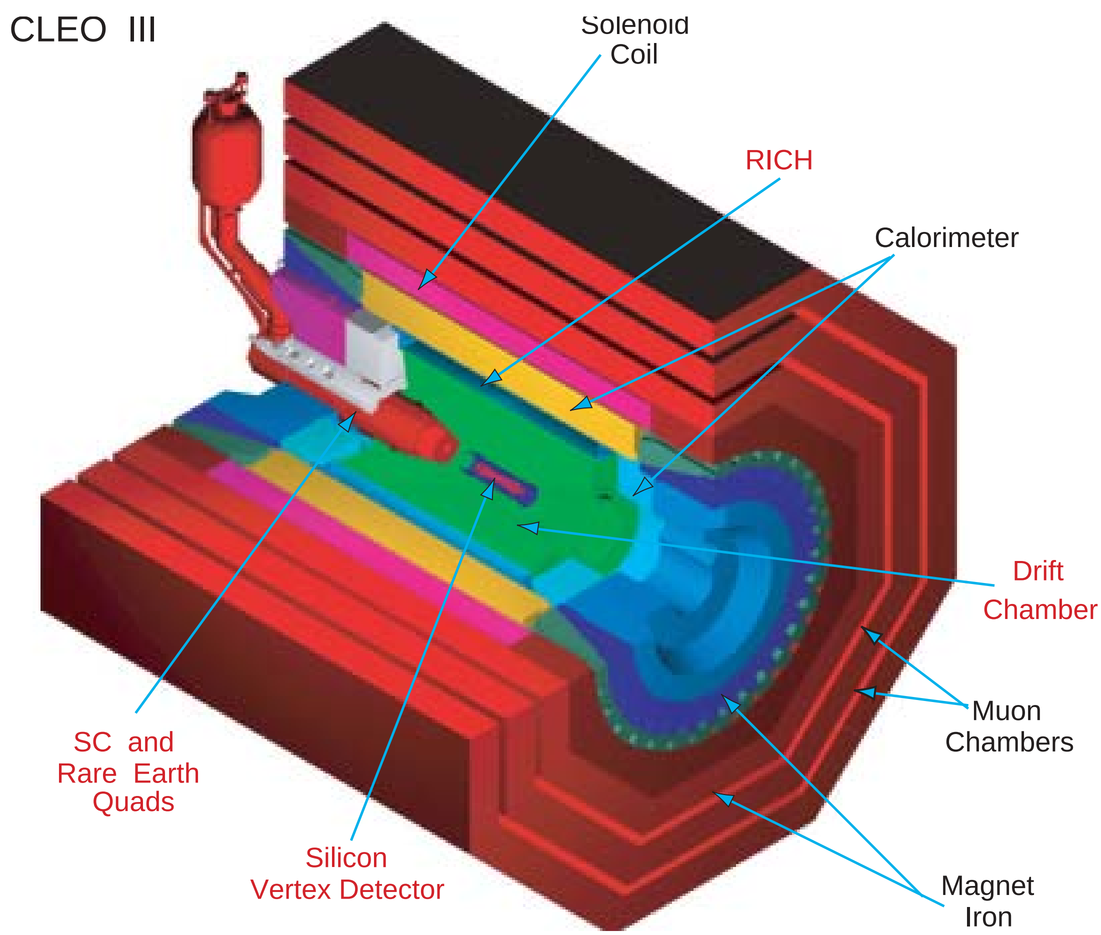



Figure 25: Cutaway view of the CLEO-3 detector.

Figure 26: Some of the CLEO collaboration members and the newly completed CLEO-3 detector in 1997.

the other major subsystems were installed. So CLEO-3 had an "engineering run" with a dummy in place of the silicon detector until the real silicon was ready for installation in February, 2000. The shakedown run was not entirely wasted time, because DR3, the RICH, the data acquisition system, and the new C++ software all required a lot of tune-up and bug fixing. The new beam pipe that went in with the silicon was connected to the rest of the CESR vacuum with a cleverly designed remotely actuated " magic" flange. Figure 25 shows a cutaway view of the new detector configuration. Figure 26 shows some of the CLEO collaborators posing by the completed detector.

Although the new CLEO-3 detector eventually worked well (see the event display in Fig. 27) and produced good physics results, it cannot be counted as a complete success. The delays hurt CLEO productivity at a time of intense competition with the BaBar and BELLE

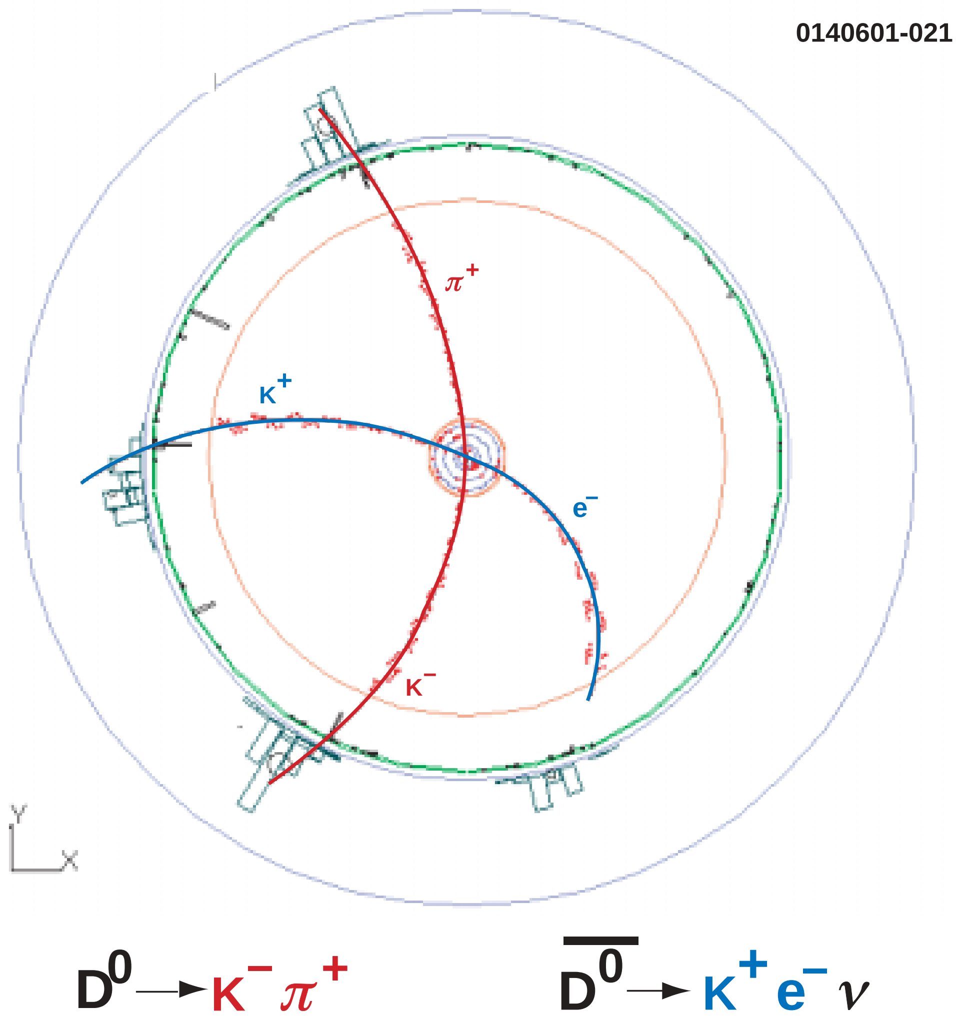



Figure 27: Computer generated display of one of the early CLEO-3 events.

collaborations. Also, the silicon $r - \phi$ side efficiency began to degrade almost immediately, starting with the innermost layer and advancing outward. Something was definitely wrong with the batch of silicon we got from Hammamatsu; it seemed to be extraordinarily sensitive to radiation, although our dosimeters in the interaction region were telling us that the dosage was actually well within safe limits.

# 15 Phase III CESR Upgrade, 1996-2001

The Phase III CESR upgrade was proposed to the NSF immediately following the unsuccessful competition with SLAC for DOE funding for an asymmetric B-Factory. Phase III, following on the earlier phase II, was the much less expensive "plan B" alternative. The goal was to improve CESR luminosity so that it could compete with the PEP-2 and KEK-B rings on the many physics topics that did not require asymmetric beam energies. At the same time we planned to increase the number of available synchrotron radiation user stations by instrumenting the back-fire radiation from the opposite-sign beam passing through the wiggler magnet that generated the x-ray beams for the A, B, and C lines. This new G-line was to be taken out to a separate new experimental area built into the hillside west of Wilson Lab.

The phase II I.R. focusing for the small-angle beam crossing configuration installed in 1995 and the complement of four 5-cell normal conducting rf cavities were optimal for colliding beams of 18 bunches per beam, that is, two bunches spaced by 42 ns in each of nine trains spaced by 284 ns. In this condition CESR had reached beam luminosities of $4.4 \times 10^{32}$ /cm$^2$sec with currents of 180 mA per beam and a beam-beam tune shift of $\xi_v = 0.041$. The stated goal of phase III was to increase the peak luminosity to at least $1.7 \times 10^{33}$. This was to be done by increasing the number of circulating bunches in each beam to 45, keeping the charge per bunch about the same. The separated pretzel orbits required to accommodate the bunches without parasitic collisions are diagramed in Fig. 22.

In order to handle the higher beam currents we had to complete the following:

- replace the copper RF cavities with four single-cell superconducting cavities,
- replace the focusing quadrupoles nearest the collision point with stronger, superconducting magnets,
- refurbish the linac injector to provide higher positron currents more reliably,
- upgrade the vacuum in the interaction region,
- upgrade the feedback systems for beam stabilization.

Most of the input power in a normal copper accelerating cavity is wasted in $I^2R$ losses in the cavity walls. In order to minimize the power level for a given accelerating field, the beam aperture has to be as small as it is in the magnets. This unfortunately also facilitates the trapping of higher mode parasitic fields in the cavity by the passage of the short beam

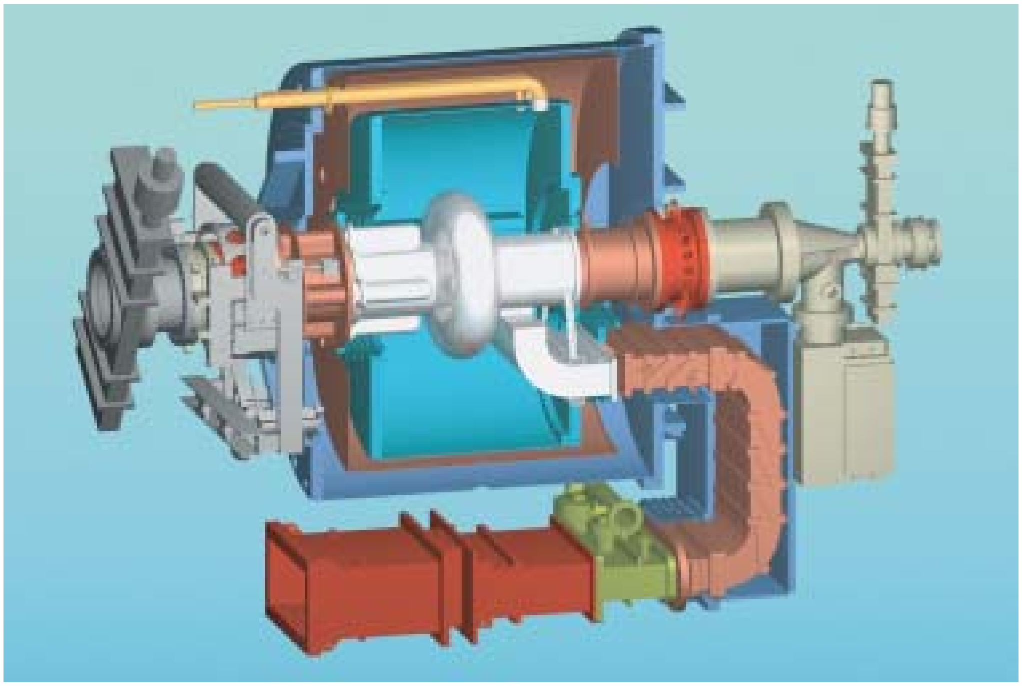


Figure 28: Cutaway view of the single-cell superconducting cavity in its cryostat.

bunches. Higher order mode fields can destabilize the beam and limit the achievable beam current. In superconducting rf cavities the wall losses are negligible, so the beam aperture can be made much larger. Beam-cavity higher mode coupling is reduced and the highest trapped frequency is lowered.

To get some operational experience with the new system the SRF group installed in September, 1997, the first of four single-cell SRF cavities (see Fig. 28) in place of one of the 5-cell NRF systems. It operated well with beam currents up to 360 mA. Three more niobium cavities were delivered in November, 1997. Fabrication of the four cryostats took longer but was eventually accomplished. The assembly, testing, and installation of the four cavities took place serially, the last one coming into operation in September, 1999. Meanwhile we completed a major cryoplant – three big helium refrigerator-compressor sets installed in a new room excavated under the transformer pad at the Kite Hill entrance to the Lab. We also acquired two new klystron power supplies designed by SLAC. Although there were some early problems with power limiting phenomena – window arcing, cavity surface defects, vacuum leaks – the SRF system eventually turned out to be as reliable as the previous NRF. Beam instabilities were still encountered, but at higher beam currents than before. By early 2001 (before the superconducting IR quad installation) over 350 mA were circulating in 45 bunches in each beam and the peak luminosity had exceeded $1.2 \times 10^{33}$ (see Figs. 29 and 30).

When two bunches are colliding at the IP, the following $e^+$ bunch and the preceding

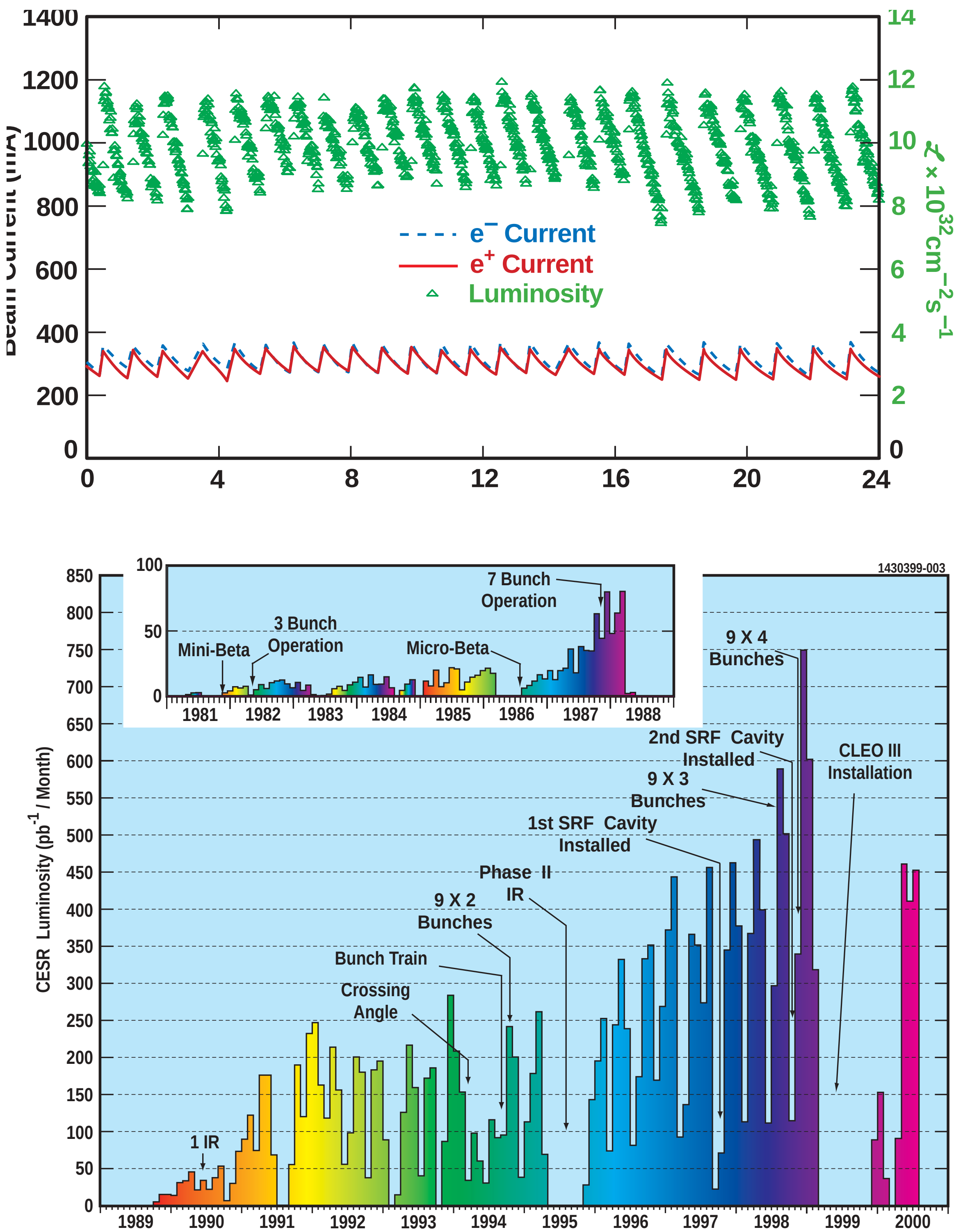



Figure 29: (top) Scoreboard showing 24 hours of instantaneous luminosity, positron and electron beam currents, for a better-than-typical day. (bottom) Integrated CESR luminosity per month.

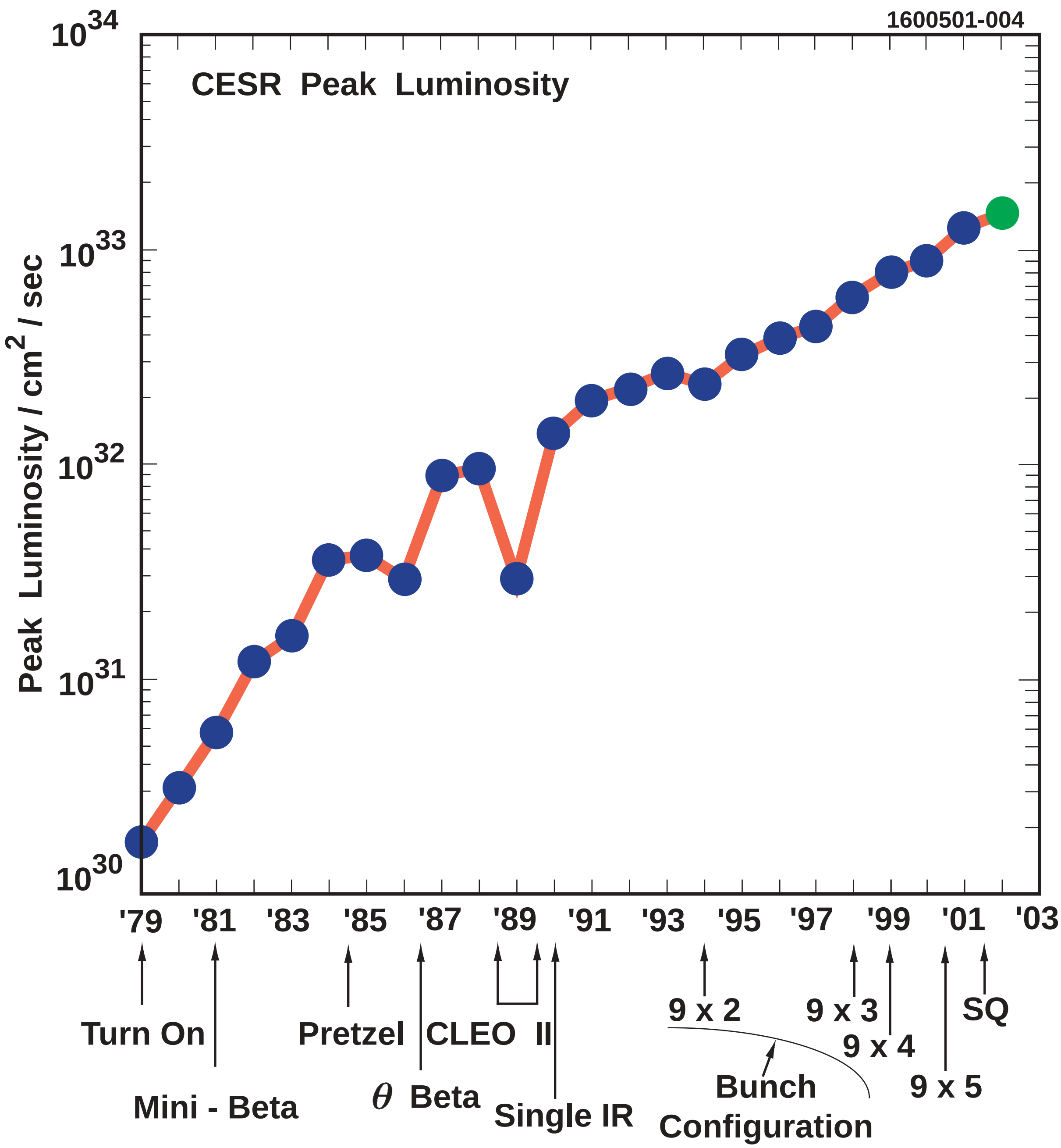



Figure 30: Evolution of the annual highest CESR peak luminosity.

$e^-$ bunch are passing by each other at a transverse separation equal to the product of the longitudinal bunch spacing and the crossing angle. They perturb each other and limit the achievable beam current. The effect is proportional to the magnitude of the $\beta$ function at that point in the orbit. This occurs near the location of the IR quadrupoles where $\beta$ can be quite large. To minimize the perturbation one wants to get as much focusing strength as possible as close to the IP as possible. In phase II the innermost focusing elements are permanent magnets. The phase III design replaces most of the permanent quadrupole with superconducting quadrupoles having much stronger gradient. This reduces the maximum of the $\beta$ function, pushes it to closer to the IP, and thereby decreases the beam-beam disturbance.

The new focus system had four combination quadrupole, skew-quadrupole, and steering dipole magnets in two cryostats. They were built by Tesla Ltd. from designs made in consultation with CERN and Cornell. Personnel turnover and inexperience at Tesla delayed delivery and testing of the completed magnets and cryostats beyond the day when the last of the CLEO-3 components were installed. To give CLEO-3 enough integrated luminosity to make a good showing at the summer 2001 conferences we decided to postpone the phase III quadrupole installation shutdown until June 2001. In the same summer 2001 shutdown we finished the installation of the special magnet components for the new G-line for synchrotron radiation users. We also put in a new positron production target in the linac, with better alignment control and stronger solenoid focusing. This immediately gave a factor of two improvement in positron injection time.

# 16 A New Director and a New Direction, 2000-...

My third five-year term as Director of LNS was due to expire on June 30, 2000. As the SLAC and KEK B-factory projects started to produce physics results, I could see that the the Laboratory would soon have to make a major shift in direction in order to stay viable. I could see my retirement from research coming in a few years, and it seemed to me that someone more likely to be an active participant in the future course of the Lab should be the dominant voice in deciding what the course should be. So I announced in summer 1999 that I would not be a taking another term as director.

The Cornell VP for Research appointed Persis Drell to head a search committee for a new director. The committee solicited suggestions of candidates from inside or outside the Lab. Maury Tigner was on everyone's list, even though he had retired back in 1995 for reasons of health. He was certainly best qualified for the job: he had been the project manager for the building of CESR, he had run the Central Design Group for the SSC, his accelerator expertise and management know-how were unexcelled. Back in 1995 he had a serious operation on his spine, though, and the doctors had apparently discouraged him from going back to work. On the off chance that Maury might have second thoughts about retirement, the committee asked if he would consider the directorship. It was not such a far-fetched idea, since he had been spending much of the intervening time working at the Beijing laboratory, helping them with plans to upgrade their machine. He accepted. Maury is about my age, maybe a year or two younger. Although I had originally had the idea of a younger replacement, this was an ideal outcome. I could resign with a clear conscience, knowing that the Lab would be in good hands and would have the best possible chance of weathering the coming storm.

The storm was the competition from the new asymmetric B-Factories coming into being. CESR was a victim of its own success. The exciting field of flavor physics opened up by the CLEO discoveries of the last decade, and the demonstration at CESR that one could reach luminosities well above $10^{32}$ cm$^{-2}$s$^{-1}$ had inspired the other laboratories to follow our lead. They were much larger and had more resources, however. It was clear by 2000 that they were going to meet their luminosity goals, in spite of the complexities inherent in asymmetric energies. CESR would have a hard time keeping pace. For the past year or two we had been actively considering a Phase IV CESR – an attempt to leapfrog PEP-2 and KEK-B in luminosity to reach $10^{34}$. Phase IV would have involved building a new dual-aperture equal-energy storage ring on top of the synchrotron, turning the old CESR over to CHESS. Alexander Mikhailichenko developed and prototyped a new dual-aperture superconducting quadrupole, and Joe Rogers layed out a design for new bending magnets and vacuum chamber. In early 2000, though, we began to consider seriously other options for maintaining physics productivity in the next decade.

- Join an existing collaboration at another laboratory – say LHC, BTeV, or BaBar. This was not a popular idea. It was already too late to have much of an impact on the important decisions. Also, there would be no role for the accelerator physicists at the Lab.

- Run above the $\Upsilon$(4S) concentrating on $B_s - \overline{B_s}$ production at the $\Upsilon$(5S), for instance. This idea did not catch on either. The $\Upsilon$(5S) does not stand out much above non-$b\bar{b}$ background, and the $\Upsilon$(5S) peak is mainly non-$B_s$ $B$ states. The competition from experiments at hadron machines with their much higher production rates would likely overwhelm us.

- Run on the bound state resonances $\Upsilon$(1S), $\Upsilon$(2S), $\Upsilon$(3S). This topic has had very little attention since the early 80's, soon after the upsilons were discovered. These states are bound and decay through the strong interaction, so they tell us about QCD. No one expects to see surprises or upset QCD, so ARGUS, CLEO, BaBar, and BELLE have concentrated instead on $B$ physics at the $\Upsilon$(4S). However, there are theorists anxious to test nonperturbative strong interaction calculation techniques, most notably lattice QCD. New experimental results would challenge them to improve their approximations. Where are the intermediate D states (Fig. 31)? Where are the singlet states $\eta_b$, $\eta_b'$, and $h_b$? Can we understand the pattern of hadronic decay modes? Most of us felt that it would be worth our while to spend about a year running on the resonances. There did not seem to be a long term viable program here, though.

- Run CESR in the tau-charm threshold region. There has been a history of unsuccessful proposals for tau-charm facilities: SLAC, Spain, Russia, China. There were reasons for the lack of enthusiasm. Charm physics has been considered less interesting than

$b$-quark physics, because of the structure of the CKM matrix. Charm decays involve only the upper left $2 \times 2$ corner of the matrix, which depends on just the Cabibbo angle and has no lowest-order imaginary component to give rise to a measurable Standard Model $CP$ violation. The $c$-quark can decay to its weak-isospin partner, the $s$-quark, while the corresponding $b \to t$ transition is energetically forbidden. So although rare $b$-decay processes can compete with the suppressed $b \to c$ transition, the corresponding rare $c$-decay processes can't compete so well with the nonsuppressed $c \to s$ transition. Moreover, most of charm physics – and also tau physics – is available for free when you run on the $\Upsilon$(4S). Since the first tau-charm factories were proposed, many of the early physics motivations have since been accomplished by CLEO and other experiments at or above the $b$-quark threshold.

In spite of the history of tau-charm proposals, the latter option was the one we decided on. The situation had changed. The interpretation of the $B$-decay data in terms of basic weak interactions of the $b$-quark depends critically on the understanding of the strong interaction effects – binding, rescattering, gluon processes, form factors, strong phases, and such. Accurate measurements of $D$-decay processes, where the strong effects were larger and the weak interaction physics was well understood, would allow theorists to test and refine their nonperturbative approximation techniques and thereby put $b$-physics on a reliable quantitative footing. The most important advantage of doing charm physics near $D\overline{D}$ threshold rather than at $B\overline{B}$ threshold would be the cleanliness of the $D\overline{D}$ final states at threshold. One could expect to tag at least 20% of the decays and thus make a substantial impact on lowering systematic errors in branching ratio measurements.

In order to run CESR at the charm threshold – one third the usual CESR energy – and have enough luminosity to produce $D\overline{D}$ at a rate comparable to the $D\overline{D}$ rate at $b$-threshold, several requirements would have to be met.

- The CESR magnet guide field, including the focusing in the IR, would have to scale with energy. Thus the major part of the permanent magnet final focus quadrupoles would have to be replaced by an electromagnets. The installation of the phase III upgrade superconducting quadrupoles would accomplish this. This would at the same time enable us to gain luminosity by reducing the $\beta_v^*$ at the interaction point. Since the luminosity ($\propto E^2/\beta_v^*$) tends to decrease at lower beam energies, one will have to regain as much of that as possible.

- Because of the hour-glass effect, the luminosity gain with low $\beta_v^*$ comes only if the bunch length is shortened to match $\beta_v^*$. That requires high rf cavity voltage. The superconducting cavities, including the two additional ones on order, were actually ideal for this. Instead of providing mainly for the power radiated at high beam energies, they would be shortening the bunches at low beam energies.

- At the lower energies the radiation damping of the transverse oscillations of the beam particles is rather ineffective. One has to introduce wiggler magnets to shorten the characteristic damping time and thus keep the transverse size of the beam small for high luminosity. Fourteen 1.33-m long superferric wigglers spread around the CESR lattice would do the trick. In terms of money and effort this would be the only major hurdle in turning CESR into CESR-c; that is, about $4 million and 2 years. In the meantime we could be running on the $\Upsilon$(1S), $\Upsilon$(2S), and $\Upsilon$(3S). Figure 31 shows the current status of the energy levels in upsilon spectroscopy.

# 17 Concluding Remarks

The Cornell Electron Storage Ring and the CLEO experiment have been quite successful. For a while CESR boasted the world's highest colliding beam luminosity, the first to exceed $\mathcal{L}_{pk} = 3 \times 10^{32}/\mathrm{cm}^2\mathrm{s}$ (see Fig. 30). The $\Upsilon$(4S) resonance has been the ideal energy for studying the properties and interactions of the $B_d$ and $B_u$ mesons, and for 21 years the CLEO collaboration was the leader in heavy quark and lepton physics. The $\Upsilon$(4S), $\Upsilon$(5S), $\Upsilon$(6S), $\chi_b$(1P), $\chi_b$(2P), $B$, $B^*$, and $D_s$ mesons were discovered at CESR, as were the transitions $b \to c$, $b \to u$, and $b \to s$. Over half of the entries in the Particle Data Group tables for $B$ mesons and for charmed mesons and baryons are based primarily on CESR results.

Largely through the research effort at CESR we have learned the following facts about heavy quarks and leptons in the Standard Model.

1. The spectroscopy of bound $b\bar{b}$ states confirms the expectations of quantum chromodynamics. The perturbative and nonperturbative predictions are nicely confirmed by the masses of the $\Upsilon$ and $\chi_b$ states, the radiative transition rates between the states, and the value of the strong coupling $\alpha_S$ derived from the $\Upsilon \to gg\gamma$ and $\Upsilon \to ggg$ decay rates.

2. The decays of the $B$ mesons support the Kobayashi-Maskawa picture of six-quark universality in the charged-current weak interaction. Heavy Quark Effective Theory and models based on factorization provide an adequate description of a wide range of data. Before the $t$ quark was discovered, the $b$ quark data pointed to its existence and gave indications of its mass. Our information on the values of the KM matrix elements $|V_{cb}|$, $|V_{ub}|$, $|V_{ts}|$, and $|V_{td}|$ comes from data on $B$ decays and $B\overline{B}$ mixing.

3. The discovery of the $b \to u$ transition established that all of the KM matrix elements are nonzero, thus allowing $CP$ violation in $B$ decay. This determination of $|V_{ub}|$ and the measurements of $|V_{cb}|$ and the $B^0\overline{B^0}$ mixing rate give us the three sides of the unitarity triangle, and challenge us to measure the three angles through the observation of $CP$ asymmetries. Agreement between the sides and angles should tell us whether the Kobayashi-Maskawa mechanism is sufficient to account for $CP$ violation.

4. We have now available an extensive data base on the strong, electromagnetic, and weak interactions of particles containing the $c$ and $b$ quarks, that can be used to test speculations on new physics beyond the Standard Model. So far, though, there is no statistically significant evidence for new physics.

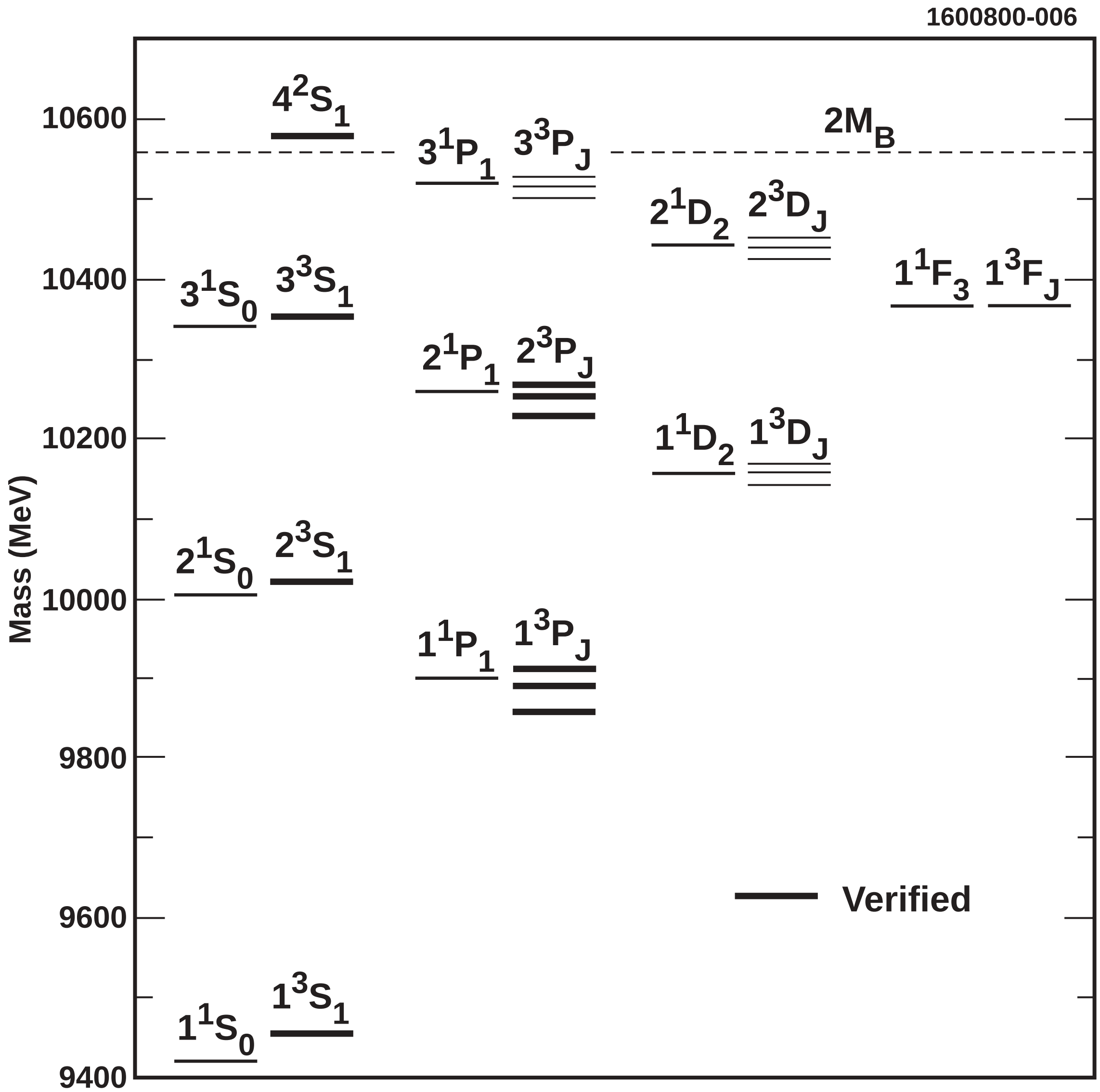



Figure 31: Energy levels in the upsilon bound state system.

5. The decays of the $\tau$ lepton confirm $e$, $\mu$, $\tau$ universality.

In spite of the successes, there are still puzzles. There is no compelling explanation for the double-humped spectrum of $\pi\pi$ invariant masses in the $\Upsilon$(3S)$\rightarrow$ $\Upsilon$(1S)$\pi\pi$ transition. There is no understanding of the pattern of quark and lepton masses and KM matrix elements, nor an answer to the question of why there are six quarks and six leptons. And we do not know whether the baryon-antibaryon asymmetry in the universe requires another source of $CP$ violation.

What has been CESR's secret of success? How has the Cornell laboratory managed to prosper when practically all of the once numerous university facilities in high energy physics have closed down — CalTech, Carnegie, Chicago, Columbia, Harvard-MIT, Princeton-Penn, Rochester, Purdue? There are several reasons, I believe.

*People.* Bob Wilson was a great physicist, a clever inventor, an inspiring leader, and he had great ambitions for the Laboratory. With this rare combination of gifts he launched the Laboratory on the course it has followed ever since. His can-do attitude and commitment to keeping the Lab at the forefront infected everyone, even for decades after he left. And it attracted other creative minds to the enterprise — McDaniel, Littauer, Tigner, Siemann — just to name a few. CESR would not have been possible without the leadership of McDaniel and the inspiration and project direction of Maury Tigner. But leadership isn't everything. Many times in the past outsiders have expected the Lab to falter when one of the big names left the scene. Each time, the Lab demonstrated a depth and breadth of talent, experience, and commitment sufficient to carry on in the established Cornell tradition. In CLEO, which is dependent on the contributions of many people from many institutions working together, the democratic structure of the collaboration has been an important factor. Everyone works hard and enthusiastically because everyone participates in the decisions.

*Innovation.* A crucial aspect of the Cornell tradition is the continual renewal of the accelerator and the experiments, keeping them productive at the physics frontier. For fifty years the Lab has built a new accelerator — 300 MeV, 1 GeV, 2 GeV, 10 GeV, CESR — or started a major upgrade — as in 1985 and 1994 — every 8 or 9 years. It has always been considered important to look far enough ahead to prepare for the time when the current capabilities are no longer exciting enough to justify support.

*Focus.* For the past 16 years the top priority of the Lab has been the performance of CESR and its experimental program. There are no other priorities. During the 1980's CESR competed on a par with DESY, a lab with an order of magnitude advantage in resources. CLEO eventually outperformed ARGUS so decisively that DESY gave up the competition. The main reason was that DESY was never able to devote its full attention to the DORIS ring and the ARGUS experiment. DORIS had to play second fiddle to the HERA project.

*Cost Conciousness.* One of Wilson's legacies is the impulse to save money by being clever. As a result Cornell and CESR have always had a reputation for delivering the most for the least. Of all the 1991-99 Phys. Rev. D and Phys. Rev. Letters papers based on HEP experiments at BNL, CESR, Fermilab, and SLAC, 23% have come from CESR, while CESR has accounted for only 3.9% of the total HEP spending of the four labs. The ability to upgrade the facility periodically has depended on matching the Lab appetite to the capabilities of the NSF to provide funding. It is a fact that no capital project proposal requiring less than a doubling of the annual funding level has been refused, and also that no proposal that required more than a doubling has been accepted. So we can credit the NSF as well as Wilson for the Lab's parsimony.

*The NSF.* There is no escaping the fact that the trust and generosity of the Physics Division of the NSF have been crucial to the success of CESR and CLEO. We owe an enormous debt to the Division directors and program officers over the years: Al Abashian, Marcel Bardon, David Berley, Chuck Brown, Willi Chinowsky, Joe Dehmer, Bob Eisenstein, Alex Firestone, Norman Gelfand, and Tricia Rankin among others. They considered CESR the flagship of the NSF program in high energy physics, and worked hard to keep it afloat.

*Luck.* While we are thanking people, we have to remember that Mother Nature and Lady Luck have been extraordinarily kind to CESR. Although the CESR energy was fixed by the existing tunnel length before the discovery of the $b$ quark, the energy turned out to be just right for covering the threshold region for $b\bar{b}$ production, from the $\Upsilon(1S)$ to the $\Upsilon(6S)$ and beyond. Other $e^+e^-$ machines built around the same time (DORIS, PETRA, PEP, TRISTAN) have all ceased to produce useful physics, because the their energy choices were not as lucky. The fortuitous occurrance of the $\Upsilon(4S)$ resonance just above $B\overline{B}$ threshold was ideal for producing $B^+B^-$ and $B^0\overline{B^0}$ copiously and cleanly. The value of $V_{cb}$ was small enough for rare processes, such as $b \to u$, $b \to s$, and $b\overline{d} \leftrightarrow \overline{b}d$, to compete with the dominant $b \to c$ decay. And $V_{ub}$ is nonzero, allowing for the possiblility of $CP$ violation in $B$ decay.

The primary goals for the CESR facility and the CLEO experiment are clear.

1. Continue important, productive research in heavy quark and lepton physics as long as it is interesting as a window on the Standard Model and beyond: $CP$ violation, rare loop decays, leptonic decays, tagged studies, heavy meson and baryon spectroscopy, charm decays, rare $\tau$ decays, and so on.

2. Continue to improve and extend CESR performance, not only to advance the CESR/CLEO HEP goals but also to serve the worldwide accelerator community as a testbed for innovations.

3. Continue to serve the US high energy physics program by providing a user-friendly facility for faculty, post-docs, and graduate students from many universities to pursue world-class research in particle physics.

4. Continue to provide, as a byproduct, high intensity x-ray beams for the hundreds of users of the Cornell High Energy Synchrotron Source (CHESS).

# 18 APPENDIX

TABLE I. Luminosity and Major Upgrades

| Year | $\mathcal{L}_{pk}/10^{30}$ $cm^{-2}s^{-1}$ | $\int \mathcal{L}dt$ $pb^{-1}$ | CESR upgrades | CLEO upgrades |
|---|---|---|---|---|
| 1979 | 2 | 1 | CESR completed | CLEO-1 partially complete |
| 1980 | 3 | 8 | | |
| 1981 | 8 | 17 | 2nd rf cavity, minibeta | completed DX, MU; sc coil |
| 1982 | 12 | 90 | muffin-tin srf test | |
| 1983 | 16 | 60 | separators, 3 bunches | started DR2 construction |
| 1984 | 37 | 104 | | VD, new DR electronics |
| 1985 | 39 | 143 | $e^+$ topping | |
| 1986 | 30 | 96 | microbeta REC quads | installed DR2, IV |
| 1987 | 92 | 420 | 7 bunches | |
| 1988 | 100 | 160 | higher linac energy | $\mu$VD test |
| 1989 | 30 | 45 | | CLEO-2 detector |
| 1990 | 150 | 394 | single IR | |
| 1991 | 220 | 1100 | | |
| 1992 | 250 | 1470 | | |
| 1993 | 290 | 1390 | 5-cell rf cavities | |
| 1994 | 250 | 1370 | 2 mr crossing, 18 bunches | |
| 1995 | 320 | 816 | new IR focusing | 3-layer Si detector, He in DR |
| 1996 | 400 | 2690 | $e^+$ target | |
| 1997 | 470 | 3400 | SRF cavity in E2 | |
| 1998 | 720 | 4442 | SRF in E1, 36 bunches | |
| 1999 | 820 | 1010 | SRF in W1,2 | CLEO-3 DR3, RICH, DAQ |
| 2000 | 880 | 6250 | | CLEO-3 Si |
| 2001 | 1250 | | sc IR quads, $e^+$ target | |

TABLE II. CLEO Collaboration Membership
*NSF* and <u>DOE</u> supported

| Year → | 1980 | 1990 | 2000 |
|---|---|---|---|
| *Cornell* | + − − − − − − − − − | + − − − − − − − − − | + − − |
| <u>Harvard</u> | + − − − − − − − − − | + − − − − − − − − − | + − − |
| Ithaca | + − − − − − − − − − | + − − − − − − − − − | + − − |
| LeMoyne | + − | | |
| *Rochester* | + − − − − − − −   − | + − − − − − − − − − | + −   − |
| <u>Rutgers</u> | + − − − − − | | |
| *Syracuse* | + − − − − − − − − − | + − − − − − − − − − | + − − |
| *Vanderbilt* | + − − − − − − − − − | + − − − − − − − − − | + − − |
| <u>Ohio State</u> | − − − − − − − − | + − − − − − − − − − | + − − |
| <u>Albany</u> | − − − − − − | + − − − − − − − − − | + − − |
| <u>Carnegie Mellon</u> | − − − − − − | − − −   − | + − − |
| <u>Florida</u> | − − − − − | + − − − − − − − − − | + − − |
| <u>Purdue</u> | − − − − | + − − − − − − − − − | + − − |
| <u>Minnesota</u> | − − − | + − − − − − − − − − | + − − |
| <u>Maryland</u> | − − | | |
| *Kansas* | − − | + − − − − − − − − − | + − − |
| <u>Oklahoma</u> | − − | + − − − − − − − − − | + − − |
| <u>UC Santa Barbara</u> | − | + − − − − − − − − − | + − − |
| <u>Colorado</u> | − | + − − − − − − − − − | |
| <u>Cal Tech</u> | − | + − − − − − − − − − | + − − |
| *UC San Diego* | | − − − − − − − − − | + − − |
| <u>So. Methodist</u> | | − − − − − − − − − | + − − |
| McGill | | − − − − − − − − | |
| Carleton | | − − − − − − − − | + − − |
| <u>Illinois</u> | | − − − − − − − | + − − |
| <u>Virginia Tech</u> | | − − − − − − − | + − |
| <u>Hawaii</u> | | − − − − − | |
| <u>SLAC</u> | | − − − − − | |
| Wayne State | | − − − − − | + − − |
| UT Panamerican | | | + − − |
| <u>UT Austin</u> | | | + − − |
| <u>Pittsburgh</u> | | | + − − |
| <u>Northwestern</u> | | | −− |

TABLE III. CLEO Officers

| | Spokesman | Analysis Coord. | Run Manager | Software Coord. |
|---|---|---|---|---|
| 1979 | A. Silverman, Cor | K. Berkelman, Cor | B. Gittelman, Cor | |
| 1980 | N. Horwitz, Syr | B. Gittelman, Cor<br>E. Thorndike, Roc | E. Nordberg, Cor | |
| 1981 | E. Thorndike, Roc | M. Gilchriese, Cor | E. Nordberg, Cor | |
| 1982 | E. Thorndike, Roc | B. Gittelman, Cor | T. Ferguson, Cor | |
| 1983 | E. Thorndike, Roc | K. Berkelman, Cor | T. Ferguson, Cor | |
| 1984 | K. Berkelman, Cor | R. Kass, OSU | R. Galik, Cor | |
| 1985 | A. Silverman, Cor | M. Gilchriese, Cor | S. Gray, Cor | |
| 1986 | G.C. Moneti, Syr | T. Ferguson, CMU | S. Gray, Cor | D. Kreinick, Cor |
| 1987 | R. Galik, Cor | A. Jawahery, Syr | J. Kandaswamy, Cor | R. Namjoshi, Cor |
| 1988 | D. Cassel, Cor | S. Stone, Cor | B. Gittelman, Cor | R. Namjoshi, Cor |
| 1989 | R. Kass, OSU | Y. Kubota, Min | B. Gittelman, Cor | B. Heltsley, Cor |
| 1990 | E. Thorndike, Roc | D. Besson, Cor | J. Kandaswamy, Cor | B. Heltsley, Cor |
| 1991 | E. Thorndike, Roc | D. Besson, Cor | J. Kandaswamy, Cor | A. Weinstein,CIT |
| 1992 | D. Miller, Pur | D. Besson, Cor | R. Ehrlich, Cor | D. Kreinick, Cor |
| 1993 | D. Miller, Pur | T. Browder, Cor | R. Ehrlich, Cor | S. Patton, Min |
| 1994 | D. Miller, Pur | S. Menary, SBa | D. Cinabro, Har | D. Kreinick, Cor |
| 1995 | R. Poling, Min | R. Kutschke, SBa | M. Sivertz, SDi | S. Patton, Min |
| 1996 | R. Poling, Min | L. Gibbons, Roc | W. Ross, Okl | K. Lingel, SLAC |
| 1997 | G. Brandenburg, Har<br>E. Thorndike, Roc | R. Briere, Har | M. Palmer, Ill | J. O'Neill, Min |
| 1998 | G. Brandenburg, Har<br>E. Thorndike, Roc | F. Wuerthwein, CIT | B. Behrens, Col | R. Baker, Cor |
| 1999 | D. Cinabro, WSU<br>K. Honscheid, OSU | D. Jaffe, SBa | G. Viehhauser, Syr | R. Baker, Cor |
| 2000 | J. Alexander, Cor<br>J. Thaler, Ill | K. Ecklund, Cor | T. Pedlar, Ill | D. Kreinick, Cor |
| 2001 | J. Alexander, Cor<br>I. Shipsey, Pur | K. Ecklund, Cor | D. Hennessy, Roc | J. Duboscq, Cor |

TABLE IV. CLEO Refereed Publications
not including reviews, etc.
by year of submission

| Year, 19... | PRL | PRD | PLB | NIM.. | TOTAL | authors |
|---|---|---|---|---|---|---|
| 80 | 4 | | | | 4 | 73 |
| 81 | 1 | | | | 1 | 69 |
| 82 | 4 | 2 | 1 | 2 | 9 | 70 |
| 83 | 7 | 1 | 1 | | 9 | 75 |
| 84 | 4 | 4 | | | 8 | 75 |
| 85 | 5 | 4 | | 2 | 11 | 76 |
| 86 | 6 | 5 | 1 | | 12 | 87 |
| 87 | 3 | 3 | 1 | | 7 | 88 |
| 88 | 4 | 2 | | | 6 | 91 |
| 89 | 4 | 6 | 3 | | 13 | 90 |
| 90 | 5 | 4 | 2 | | 11 | 105 |
| 91 | 4 | 8 | | | 12 | 120 |
| 92 | 7 | 3 | 3 | 1 | 14 | 166 |
| 93 | 13 | 6 | 4 | | 23 | 184 |
| 94 | 9 | 6 | 7 | 1 | 23 | 198 |
| 95 | 9 | 7 | 4 | | 20 | 200 |
| 96 | 9 | 8 | 4 | | 21 | 212 |
| 97 | 17 | 15 | 2 | 1 | 35 | 211 |
| 98 | 11 | 7 | 1 | | 19 | 205 |
| 99 | 9 | 8 | | | 17 | 203 |
| 00 | 12 | 14 | 1 | | 27 | 190 |

TABLE V. CLEO Publications: Upsilons

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $e^+e^- \to \Upsilon$(1S,2S,3S) | D.Andrews | PRL **44**,1108(80) | 15 Feb 80 |
| $e^+e^- \to \Upsilon$(4S) | D.Andrews | PRL **45**,219(80) | 18 Apr 80 |
| $\Upsilon$(2S)$\to \Upsilon$(1S)$\pi^+\pi^-$ | J.Mueller | PRL **46**,1181(81) | 23 Feb 81 |
| $\Upsilon$(3S)$\to \Upsilon$(1S)$\pi^+\pi^-$ | J.Green | PRL **49**,617(82) | 16 Jun 82 |
| $\Upsilon$(1S,2S,3S)$\to \ell^+\ell^-$ | D.Andrews | PRL **50**,807(83) | 6 Jan 83 |
| $\Upsilon$(1S)$\to \tau^+\tau^-$ | R.Giles | PRL **50**,877(83) | 19 Jan 83 |
| $e^+e^- \to X$ | R.Giles | PRD **29**,1285(84) | 3 Nov 83 |
| $\Upsilon$(2S)$\to \gamma X$ | P.Haas | PRL **52**,799(84) | 21 Nov 83 |
| $\Upsilon$(2S)$\to \Upsilon$(1S)$\pi^+\pi^-$ | D.Besson | PRD **30**,1433(84) | 5 Mar 84 |
| $\Upsilon$(2S)$\to \ell^+\ell^-$ | P.Haas | PRD **30**,1996(84) | 20 Jul 84 |
| $e^+e^- \to \Upsilon$(5S,6S) | D.Besson | PRL **54**,381(85) | 11 Oct 84 |
| $\Upsilon$(1S)$\to gg\gamma$ | S.Csorna | PRL **56**,1222(86) | 23 Dec 85 |
| $\Upsilon$(1S)$\to \gamma X_{excl}$ | A.Bean | PRD **34**,905(86) | 16 Jan 86 |
| $\Upsilon$(1S)$\to \gamma X_{low\tau}$? | T.Bowcock | PRL **56**,2676(86) | 28 Feb 86 |
| $\Upsilon$(3S)$\to \pi^+\pi^- X$ | T.Bowcock | PRL **58**,307(87) | 29 Sep 86 |
| *CLEO-1.5* | | | |
| $\Upsilon$(1S,3S)$\to \mu^+\mu^-$ | W.Chen | PRD **39**,3528(89) | 14 Feb 89 |
| $\Upsilon$(1S)$\to \psi X$ | R.Fulton | PLB **224**,445(89) | 28 Apr 89 |
| $\Upsilon$(1S)$\to \gamma X$ | R.Fulton | PRD **41**,1401(90) | 5 May 89 |
| $\Upsilon$(4S)$\to \psi X$ (non-$B\overline{B}$) | J.Alexander | PRL **64**,2226(90) | 17 Jan 90 |
| $\Upsilon$(3S)$\to \pi^+\pi^- X$ | I.Brock | PRD **43**,1448(91) | 24 Sep 90 |
| *CLEO-2* | | | |
| $\Upsilon$(3S)$\to \chi_b$(2P)$X$ | R.Morrison | PRL **67**,1696(91) | 30 May 91 |
| $\Upsilon$(3S)$\to \chi_b$(2P)$X_{excl}$ | G.Crawford | PLB **294**,139(92) | 26 Jun 92 |
| $\Upsilon$(3S) hadronic transitions | F.Butler | PRD **49**,40(94) | 8 Jul 93 |
| $\Upsilon$(1S)$\to \tau^+\tau^-$ | D.Cinabro | PLB **340**,129 (94) | 22 Sep 94 |
| $\Upsilon$(1S)$\to \gamma X$ | B.Nemati | PRD **55**,5273(97) | 30 Oct 96 |
| $\Upsilon \to gg\gamma$ vs. $e^+e^- \to q\bar{q}\gamma$ | M.S.Alam | PRD **56**,17(97) | 30 Dec 96 |
| $\Upsilon$(2S)$\to \Upsilon$(1S)$h$.. | J.Alexander | PRD **58**,052004(98) | 26 Feb 98 |
| $\Delta m(\chi_{b,J})$ | K.Edwards | PRD **59**,032003(99) | 12 Mar 98 |
| $\Upsilon$(1S)$\to \gamma\pi\pi$ | A.Anastassov | PRL **82**,286(99) | 5 Aug 98 |
| $\Upsilon' \to \Upsilon\pi\pi$ | S.Glenn | PRD **59**,052003(99) | 10 Aug 98 |
| *CLEO-2.5* | | | |
| $\Upsilon$(4S)$\to B^+B^-$ vs. $B^0\bar{B}^0$ | J.Alexander | PRL **86**,2737(00) | 1 Jun 00 |

TABLE VI. CLEO Publications: Soft Hadronic Physics and New Particle Searches

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $\Upsilon$(1S)$\rightarrow$ axions? | M.S.Alam | PRD **27**,1665(83) | 22 Nov 82 |
| $e^+e^- \rightarrow \xi(2200)X$? | S.Behrends | PLB **137**,277(84) | 29 Nov 83 |
| $e^+e^- \rightarrow \Lambda X$ | M.S.Alam | PRL **53**,24(84) | 12 Apr 84 |
| $e^+e^- \rightarrow \pi X, KX, \ldots$ | S.Behrends | PRD **31**,2161(85) | 18 Oct 84 |
| Bose-Einstein correlations | P.Avery | PRD **32**,2295(85) | 11 Apr 85 |
| $\Upsilon$(1S)$\rightarrow \zeta\gamma$? | D.Besson | PRD **33**,300(86) | 14 Aug 85 |
| Magnetic monopoles? | T.Gentile | PRD **35**,1081(87) | 27 Oct 86 |
| *CLEO-1.5* | | | |
| $B \rightarrow H^0 X$? | M.S.Alam | PRD **40**,712(89) | 13 Feb 89 |
| Fractional charges? | T.Bowcock | PRD **40**,263(89) | 29 Mar 89 |
| $\gamma\gamma \rightarrow X_{c\overline{c}}$ | W.Chen | PLB **243**,169(90) | 27 Mar 90 |
| *CLEO-2* | | | |
| $gg$ and $q\overline{q} \rightarrow$ jets | M.S.Alam | PRD **46**,4822(92) | 1 Jun 92 |
| $\gamma\gamma \rightarrow p\overline{p}$ | M.Artuso | PRD **50**,5484(94) | 1 Sep 93 |
| $\gamma\gamma \rightarrow \chi_{c2}$ | J.Dominick | PRD **50**,4265(94) | 4 Oct 93 |
| $\gamma\gamma \rightarrow \pi^+\pi^-$ or $K^+K^-$ | J.Dominick | PRD **50**,3027(94) | 11 Mar 94 |
| $\Upsilon$(1S)$\rightarrow \gamma$ neutralino? | R.Balest | PRD **51**,2053(95) | 11 Aug 94 |
| $\gamma\gamma \rightarrow \Lambda\overline{\Lambda}$ | S.Anderson | PRD **56**,R2485(97) | 17 Jan 97 |
| $\gamma\gamma \rightarrow f_J(2200)$? | R.Godang | PRL **79**,3829(97) | 18 Mar 97 |
| $\eta \rightarrow e^+e^-$? | T.Browder | PRD **56**,5359(97) | 3 Jun 97 |
| $\sigma_{tot}(e^+e^- \rightarrow h..)$ at 10.52 GeV | R. Ammar | PRD **57**,1350(98) | 7 Jul 97 |
| $F_{PS,\gamma}$ at high $q^2$ | J. Gronberg | PRD **57**,33(98) | 12 Jul 97 |
| *CLEO-2.5* | | | |
| $\gamma\gamma \rightarrow f_J(2200)$? | M.S.Alam | PRL **81**,3328(98) | 28 May 98 |
| $\eta' \rightarrow$ rare? | R.A.Briere | PRL **84**,26(00) | 22 Jul 99 |
| $\eta_c:\ m,\ \Gamma,\ \Gamma_{\gamma\gamma}$ | G.Brandenburg | PRL **85**,3095(00) | 20 Jun 00 |
| $e^+e^- \rightarrow \tilde{b}\bar{\tilde{b}}$? | V.Savinov | PRD **63**,R051101(01) | 17 Oct 00 |

TABLE VII. CLEO Publications: Tau Lepton Hadronic Decay Modes

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $\tau \to \eta X$ and $\omega X$ | P.Baringer | PRL **59**,1993(87) | 29 Jul 87 |
| *CLEO-1.5* | | | |
| $\tau \to K^* X$ | M.Goldberg | PLB **251**,223(90) | 30 Jul 90 |
| *CLEO-2* | | | |
| $\tau \to \eta X$ | M.Artuso | PRL **69**,3278(92) | 4 Aug 92 |
| $\tau \to h\pi^0$'s | M.Procario | PRL **70**,1207(93) | 14 Oct 92 |
| $\tau \to \pi\pi\pi\pi^0\pi^0\nu$ | D.Bortoletto | PRL **71**,1791(93) | 6 Jul 93 |
| Cabibbo suppressed decay | M.Battle | PRL **73**,1079(94) | 4 Feb 94 |
| $\tau \to h\pi^0\nu$ | M.Artuso | PRL **72**,3762(94) | 1 Apr 94 |
| $\tau \to 5\pi$ | D.Gibaut | PRL **73**,934(94) | 22 Apr 94 |
| $\tau \to 3h^\pm\nu,\ 3h^\pm\pi^0\nu$ | R.Balest | PRL **75**,3809(95) | 14 Jul 95 |
| $\tau \to K_S^0$.. | T.E.Coan | PRD **53**,6037(96) | 10 Jan 96 |
| $\tau \to K\eta\nu$ | J.Bartelt | PRL **76**,4119(96) | 12 Jan 96 |
| $\tau \to \phi X$ | P.Avery | PRD **55**,R1119(97) | 15 Oct 96 |
| $\tau \to 3\pi\eta\nu,\ f_1\pi\nu$ | T.Bergfeld | PRL **79**,2406(97) | 25 Jun 97 |
| $\tau \to 7\pi^\pm\pi^0\nu$? | K.Edwards | PRD **56**,R5297(97) | 9 Jul 97 |
| $\tau \to 5\pi^\pm\pi^0\nu$ | S.Anderson | PRL **79**,3814(97) | 9 Jul 97 |
| $\tau \to K^{*-}\eta\nu$ | M.Bishai | PRL **82**,281(99) | 15 Sep 98 |
| $\tau \to$ 3-prong with $K^\pm$ | S.Richichi | PRD **60**,112002(99) | 15 Oct 98 |
| $\tau \to \pi^\pm 2\pi^0\nu$, $\nu$-helicity | D.M.Asner | PRD **61**,012002(00) | 16 Feb 99 |
| *CLEO-2.5* | | | |
| $\tau \to \pi^\pm 2\pi^0\nu$ $h$-structure | T.Browder | PRD **61**,052004(00) | 16 Aug 99 |
| $\tau \to 3\pi^\pm\pi^0\nu$ resonances | K.Edwards | PRD **61**,072003(00) | 8 Sep 99 |
| $\tau \to \pi^\pm\pi^0\nu$ $h$-structure | S.Anderson | PRD **61**,112002(00) | 21 Oct 99 |
| $\tau \to K^\pm 2\pi^\pm\nu$ resonances | D.M.Asner | PRD **6s**,072006(00) | 25 Apr 00 |

TABLE VIII. CLEO Publications: Other Tau Lepton Papers

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| Michel parameter | S.Behrends | PRD **32**,2468(85) | 15 Jul 85 |
| $M(\nu_\tau)$ | S.Csorna | PRD **35**,2747(87) | 3 Nov 86 |
| $\tau(\tau)$ | C.Bebek | PRD **36**,690(87) | 13 Apr 87 |
| | P.Baringer | PRL **59**,1993(87) | 29 Jul 87 |
| *CLEO-1.5* | | | |
| $\tau \to$ no-$\nu$? | T.Bowcock | PRD **41**,805(90) | 13 Oct 89 |
| $\tau \to e\nu\overline{\nu}$ | R.Ammar | PRD **45**,3976(92) | 2 Dec 91 |
| *CLEO-2* | | | |
| $\tau(\tau)$ | M.Battle | PLB **291**,488(92) | 22 Jul 92 |
| $\tau \to \gamma\mu$? | A.Bean | PRL **70**,138(93) | 24 Sep 92 |
| $\tau \to e\nu\overline{\nu}$ | D.Akerib | PRL **69**,3610(92) | 28 Sep 92 |
| $M(\tau)$ | R.Balest | PRD **47**,3671(93) | 9 Feb 93 |
| $\tau \to$ no-$\nu$? | J.Bartelt | PRL **73**,1890(94) | 6 Jun 94 |
| $\alpha_S$ from $\tau$ decays | T.Coan | PLB **365**,580(95) | 19 Jun 95 |
| $\tau \to 3\ell 2\nu$? | M.S.Alam | PRL **76**,2637(96) | 22 Nov 95 |
| $\tau(\tau)$ | R.Balest | PLB **388**,402(96) | 6 Jul 96 |
| $\ell$ universality | A.Anastassov | PRD **55**,2559(97) | 6 Nov 96 |
| $\tau \to e\gamma,\ \mu\gamma$? | K.Edwards | PRD **55**,R3919(97) | 12 Nov 96 |
| Michel parameters | R.Ammar | PRL **78**,4686(97) | 26 Dec 96 |
| $\nu$-helicity from $E(h)$ correl. | T.E.Coan | PRD **55**,7291(97) | 22 Jan 97 |
| $\tau \to \pi^0, \eta$, no-$\nu$? | G.Bonvicini | PRL **79**,1221(97) | 17 Apr 97 |
| Michel parameters, $\nu$-helicity | J.Alexander | PRD **56**,5320(97) | 15 May 97 |
| $\tau \to$ no-$\nu$? | B.Nemati | PRD **57**,5903(98) | 8 Dec 97 |
| $M(\nu_\tau)$ | R.Ammar | PLB **431**,209(98) | 3 Apr 98 |
| $CP$ in $\tau$ decay | S.Anderson | PRL **81**,3823(98) | 21 May 98 |
| $\tau \to$ B or L violating | R.Godang | PRD **59**,091303(99) | 15 Dec 98 |
| *CLEO-2.5* | | | |
| $M(\nu_\tau)$ from $\tau \to 3\pi^\pm\nu$ | M.Athenas | PRD **61**,052002(00) | 4 Jun 99 |
| $\tau \to \ell\gamma\nu$ | T.Bergfeld | PRL **84**,830(00) | 7 Sep 99 |
| $\tau \to \mu\gamma$? | S.Ahmed | PRD **61**,R071101(00) | 25 Oct 99 |

TABLE IX. CLEO Publications: $D$ Meson Hadronic Decay Modes

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $D^0 \to \overline{K^0}\phi$ | C.Bebek | PRL **56**,1893(86) | 5 Feb 86 |
| *CLEO-1.5* | | | |
| $D^- \to K\overline{K}$ or $\pi\overline{\pi}$ | J.Alexander | PRL **65**,1184(90) | 13 Jun 90 |
| $D^0 \to \pi^0 X$ or $\eta X$ | K.Kinoshita | PRD **43**,2836(91) | 18 Dec 90 |
| *CLEO-2* | | | |
| $D^0 \to \overline{K^0}$ and $\overline{K^{*0}}$ | M.Procario | PRD **48**,4007(93) | 14 Oct 92 |
| $D \to \pi\pi$ | M.Selen | PRL **71**,1973(93) | 11 Jun 93 |
| $D^0 \to K^-\pi^+$ | D.Akerib | PRL **71**,3070(93) | 23 Aug 93 |
| $D^0 \to K^+\pi^-$ | D.Cinabro | PRL **72**,1406(94) | 2 Dec 93 |
| $D^+ \to K^-\pi^+\pi^+$ | R.Balest | PRL **72**,2328(94) | 17 Jan 94 |
| $D^0$ FCNC decays | A.Freyberger | PRL **76**,3065(96) | 10 Jan 96 |
| $D^0 \to K^-\pi^+\pi^0$ | B.Barish | PLB **373**,335(96) | 5 Feb 96 |
| $D^0 \to K\overline{K}X$ | D.M.Asner | PRD **54**,4211(96) | 16 Apr 96 |
| $D^+ \to K^0_S K^+,\ K^0_S \pi^+$ | M.Bishai | PRL **78**,3261(97) | 27 Dec 96 |
| $D^0 \to K^-\pi^+$ via partial $D^{*+}$ | M.Artuso | PRL **80**,3193(98) | 17 Dec 97 |
| *CLEO-2/5* | | | |
| $D^0 - \bar{D}^0$ mixing? | R.Godang | PRL **84**,5038(00) | 3 Jan 00 |
| $D^0 \to K^-\pi^+\pi^0$ Dalitz | S.Kopp | PRD **63**,092001(01) | 17 Nov 00 |
| $CP$ in $D^0 \to K_S/\pi^0 K_S/\pi^0$? | G.Bonvicini | PRD **63**,R0701101(01) | 19 Dec 00 |

TABLE X. CLEO Publications: Other $D$ Meson Papers

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $e^+e^- \to D^+X$ | C.Bebek | PRL **49**,610(82) | 26 May 82 |
| $e^+e^- \to D^{(*)}X$ | P.Avery | PRL **51**,1139(83) | 21 Jul 83 |
| $\tau(D^0, D^+, D_s)$ | S.Csorna | PLB **191**,318(87) | 29 Jan 87 |
| $e^+e^- \to c\overline{c}$ | T.Bowcock | PRD **38**,2679(88) | 26 Feb 88 |
| $e^+e^- \to c\overline{c}$ | D.Bortoletto | PRD **37**,1719(88) | 26 Oct 88 |
| $D \to \ell^+\ell^- X$? | P.Haas | PRL **60**,1614(88) | 23 Nov 88 |
| *CLEO-1.5* | | | |
| $e^+e^- \to D_J X$ | P.Avery | PRD **41**,774(90) | 24 Aug 89 |
| $e^+e^- \to \vec{D}^{*+}X$ | Y.Kubota | PRD **44**,593(91) | 25 Jan 91 |
| $D \to$"unusual" | R.Ammar | PRD **44**,3383(92) | 22 Apr 91 |
| $D^0 \to K^{*-}e\nu$ and $K^-e\nu$ | G.Crawford | PRD **44**,3394(92) | 10 May 91 |
| *CLEO-2* | | | |
| $D^* \to D\pi$ and $D\gamma$ | F.Butler | PRL **69**,2041(92) | 15 Jun 92 |
| $M(D^*) - M(D)$ | D.Bortoletto | PRL **69**,2046(92) | 13 Jul 92 |
| $D^+ \to \pi^0\ell\nu$ | M.S.Alam | PRL **71**,1311(93) | 16 Jun 93 |
| $D \to X_{excl}\ell\nu$ | A.Bean | PLB **317**,647(93) | 30 Sep 93 |
| $e^+e^- \to D_1^0 X$ and $D_2^{*0}X$ | P.Avery | PLB **331**,236(94) | 8 Mar 94 |
| $e^+e^- \to D_1^+ X$ and $D_2^{*+}X$ | T.Bergfeld | PLB **340**,194(94) | 30 Sep 94 |
| $D^0 \to \pi^-e^+\nu$ | F.Butler | PRD **52**,2656(95) | 27 Jan 95 |
| $CP$ in $D^0$ decays | J.Bartelt | PRD **52**,4860(95) | 22 May 95 |
| $D^0 \to Xe\nu$ | Y.Kubota | PRD —bf 54,2994(96) | 25 Oct 95 |
| $D^+ \to \pi^0\ell^+\nu,\ \eta e^+\nu$ | J.Bartelt | PLB **405**,373(97) | 1 Apr 97 |
| $D^{*+} \to D^+\gamma$ | J.Bartelt | PRL **80**,3919(98) | 19 Nov 97 |
| $D^{*\pm}$ spin alignment | G.Brandenburg | PRD **58**,052003(98) | 26 Feb 98 |
| *CLEO-2.5* | | | |
| $\tau(D_{(s)})$ | G.Bonvicini | PRL **82**,4586(99) | 8 Feb 99 |

TABLE XI. CLEO Publications: $D_s$ Charmed Mesons

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $e^+e^- \to D_sX$ | A.Chen | PRL **51**,634(83) | 22 Jun 83 |
| *CLEO-1.5* | | | |
| $D_s \to X_{excl}$ | W.Chen | PLB **226**,192 (89) | 19 May 89 |
| $D_s \to \phi\ell\nu$ | J.Alexander | PRL **65**,1531(90) | 28 Jun 90 |
| *CLEO-2* | | | |
| $D_s \to \eta^{(\prime)}\pi$ | J.Alexander | PRL **68**,1275(92) | 27 Sep 91 |
| $D_s \to \eta^{(\prime)}\rho$ | P.Avery | PRL **68**,1279(92) | 27 Sep 91 |
| $D_s \to \eta^{(\prime)}\pi$ and $\eta^{(\prime)}\rho$ | M.Daoudi | PRD **45**,3965(92) | 30 Sep 91 |
| $e^+e^- \to D_{s1}^+X$ | J.Alexander | PLB **303**,378(93) | 5 Feb 93 |
| $D_s \to \mu\nu$ | D.Acosta | PRD **49**,5690(94) | 3 Aug 93 |
| $e^+e^- \to D_{s2}^{*+}X$ | Y.Kubota | PRL **72**,1972(94) | 14 Jan 94 |
| $M(D_s^{*+}) - M(D_s^+)$ | D.Brown | PRD **50**,1884(94) | 27 Jan 94 |
| $D_s \to \phi\ell\nu$ | F.Butler | PLB **324**,255(94) | 1 Feb 94 |
| $D_s \to \phi e\nu$ form factors | P.Avery | PLB **337**,405(94) | 27 Jul 94 |
| $D_s^{*+} \to D_s^+\pi^0$ | J.Gronberg | PRL **75**,3232(95) | 21 Jul 95 |
| $D_s^+ \to \eta^{(\prime)}\ell^+\nu$ | G.Brandenburg | PRL **75**,3804(95) | 24 Jul 95 |
| $D_s^{\rightarrow}\phi\pi^{\pm}$ | M.Artuso | PLB **378**,364(96) | 2 Feb 96 |
| $D_s \to \omega\pi^{\pm}$ | R.Balest | PRL **79**,1436(97) | 1 May 97 |
| $D_s \to \mu\nu$ for $f_{Ds}$ | M.Chada | PRD **58**,032002(98) | 10 Dec 97 |
| $D_s \to \eta^{(\prime)}\pi^{\pm},\ \eta^{(\prime)}\rho^{\pm}$ | C.P.Jessop | PRD **58**,052002(98) | 31 Dec 97 |
| *CLEO-2.5* | | | |
| $c \to D_s^{(*)}$ | R.Briere | PRD **62**,072003(00) | 25 Apr 00 |

TABLE XII. CLEO Publications: Charmed Baryons

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $e^+e^- \to \Lambda_c X$ | T.Bowcock | PRL **55**,923(85) | 5 Jun 85 |
| $e^+e^- \to \Xi_c^0 X$ | P.Avery | PRL **62**,863(89) | 21 Nov 88 |
| $e^+e^- \to \Sigma_c^{++,0} X$ | T.Bowcock | PRD **40**,1240(89) | 13 Dec 88 |
| *CLEO-1.5* | | | |
| $\Lambda_c$ decay asymmetry | P.Avery | PRL **65**,2842(90) | 10 Aug 90 |
| $e^+e^- \to \Lambda_c X$ | R.Fulton | PRD **43**,3599(91) | 27 Aub 90 |
| *CLEO-2* | | | |
| $\Xi_c \to \Omega K$ | S.Henderson | PLB **283**,161(92) | 15 Jan 92 |
| $\Lambda_c \to \Xi K$, $\Sigma K K$, $\Xi K \pi$ | P.Avery | PRL **71**,2391(93) | 6 May 93 |
| $\Lambda_c \to \Sigma^+\pi$, $\omega$, etc. | Y.Kubota | PRL **71**,3255(93) | 24 Jun 93 |
| $e^+e^- \to \Sigma_c^+$X | G.Crawford | PRL **71**,3259(93) | 24 Jun 93 |
| $\Lambda_c \to \Lambda\pi\pi$, $\Sigma^0$n$\pi$ | P.Avery | PLB **325**,257(94) | 17 Dec 93 |
| $\Lambda_c \to \Lambda\ell\nu$ | T.Bergfeld | PLB **323**,219(94) | 20 Jan 94 |
| $\Xi_c \to \Xi e\nu$ | J.Alexander | PRL **74**,3113(95) | 12 Oct 94 |
| $\Lambda_c \to \eta X_{excl}$ etc. | R.Ammar | PRL **74**,3534(95) | 10 Nov 94 |
| $\Lambda_c^*(2593, 2625) \to \Lambda_c\pi^+\pi^-$ | K.Edwards | PRL **74**,3331(95) | 21 Nov 94 |
| $\Lambda_c \to \Lambda e\nu$ | G.Crawford | PRL **75**,624(95) | 13 Jan 95 |
| $\Lambda_c \to \Lambda\pi^\pm$, $\Sigma'^+\pi^0$ asyms. | M.Bishai | PLB **350**,256(95) | 22 Feb 95 |
| $\Lambda_c \to p\phi$ | J.Alexander | PRD **53**,1013(96) | 25 Jul 95 |
| $\Xi_c^* \to \Xi_c^+\pi^-$ | P.Avery | PRL **75**,4364(95) | 15 Aug 95 |
| $\Xi_c^+ \to \Sigma^+K^-\pi^+$, $\Lambda K^-\pi^+\pi^-$ | T.Bergfeld | PLB **365**,431(96) | 7 Nov 95 |
| $\Xi_c^+ \to$ new modes | K.Edwards | PLB **373**,261(96) | 23 Jan 96 |
| $\Xi_c^* \to \Xi_c^0\pi^+$ | L.Gibbons | PRL **77**,810(96) | 1 Mar 96 |
| $\Sigma_c^* \to \Lambda_c\pi^\pm$ | G.Brandenburg | PRL **78**,2304(97) | 26 Sep 96 |
| $\Lambda_c \to p\overline{K}\pi$.. | M.S. Alam | PRD **57**,4467(98) | 10 Sep 97 |
| $\Xi_c^* \to \Xi_c^{+,0}\gamma$ | C.P.Jessop | PRL **82**,492(99) | 19 Oct 98 |
| *CLEO-2.5* | | | |
| $\Xi_c' \to \Xi_c^*\pi$ | J.Alexander | PRL **83**,3390(99) | 8 Jun 99 |
| $\Lambda_c \to pK^-\pi^+$ | D.E.Jaffe | PRD **62**,072005(00) | 28 Mar 00 |
| $c \to \Theta_c \to \Lambda X$ | R.Ammar | PRD **62**,092007(00) | 28 Apr 00 |
| $\Sigma_c^{*+}$, $M(\Sigma_c^+)$ | R.Ammar | PRL **86**,1167(01) | 19 Jul 00 |
| $\Omega_c^0$ observation | D.C.-Hennessy | PRL**86**,3730(01) | 11 Oct 00 |
| $\tau(\Lambda_c)$ | A.Mahmood | PRL **86**,2232(01) | 15 Nov 00 |

TABLE XIII. CLEO Publications: $B$ Leptonic Decays to Charm

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $b \to \ell\ell q$, etc.? | A.Chen | PLB **122**,317(83) | 20 Dec 92 |
| $B \to \ell^+\ell^- X$ | P.Avery | PRL **53**,1309(84) | 25 May 84 |
| $\tau(B^0)/\tau(B^+)$, $B^0 \rightleftharpoons \overline{B^0}$? | A.Bean | PRL **58**,183(87) | 24 Jul 86 |
| $B \to \ell^+\ell^- X$ | A.Bean | PRD **35**,3533(87) | 1 Apr 87 |
| *CLEO-1.5* | | | |
| $B^0 \rightleftharpoons \overline{B^0}$ | M.Artuso | PRL **62**,2233(89) | 16 Feb 89 |
| $\overline{B} \to D^*\ell\nu$ | D.Bortoletto | PRL **63**,1667(89) | 14 Jun 89 |
| $\overline{B} \to D\ell\nu$, $DX\ell\nu$ | R.Fulton | PRD **43**,651(91) | 1 Aug 90 |
| $\overline{B}^{0,-} \to X\ell\nu$ | S.Henderson | PRD **45**,2212(92) | 8 Jul 91 |
| *CLEO-2* | | | |
| $\theta(\ell)$ in $\overline{B} \to X\ell\nu$ | S.Sanghera | PRD **47**,791(93) | 28 Jul 92 |
| $B^0 \rightleftharpoons \overline{B^0}$ | J.Bartelt | PRL **71**,1680(93) | 29 Apr 93 |
| $\overline{B} \to D^*\ell\nu$ | B.Barish | PRD **51**,1014(95) | 23 Jun 94 |
| $\overline{B}^{0,-} \to X\ell\nu$ | M.Athenas | PRL **73**,3503(94) | 24 Jun 94 |
| $B \to X\ell\nu$ with $\ell$-tag | B.Barish | PRL **76**,1570(96) | 16 Oct 95 |
| $B^0 \to D^{*+}\ell^-\overline{\nu}$ | J.Duboscq | PRL **76**,3898(96) | 27 Nov 95 |
| $B \to X\ell\nu$ spectrum | M.Artuso | PLB **399**,321(97) | 17 Feb 97 |
| $B \to D\ell\nu$ $\Gamma$, $F$ | M.Athenas | PRL **79**,2208(97) | 30 May 97 |
| $B \to D^{**}\ell\nu$ | A.Anastasssov | PRL **80**,4127(98) | 18 Aug 97 |
| $B \to \Theta_c\ell\nu$ | G.Bonvicini | PRD **57**,6604(98) | 2 Dec 97 |
| $B \to D\ell\nu$ $\mathcal{B}r$, $F$ | J.Bartelt | PRL **82**,3020(99) | 25 Nov 98 |
| *CLEO-2.5* | | | |
| $B^0 - \bar{B}^0$ mixing parameters | B.Behrens | PLB **490**,36(00) | 10 Aug 00 |

TABLE XIV. CLEO Publications: $B$ Charmless Leptonic Decays

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $B \to e\nu$ | C.Bebek | PRL **46**,84(81) | 1 Oct 80 |
| $B \to \mu\nu$ | K.Chadwick | PRL **46**,88(81) | 31 Oct 80 |
| $B \to \ell\nu$ | K.Chadwick | PRD **27**,475(83) | 9 Aug 82 |
| $b \to u\ell\nu$? | A.Chen | PRL **52**,1084(84) | 30 Jan 84 |
| $b \to u\ell\nu$? | S.Behrends | PRL **59**,407(87) | 4 May 87 |
| *CLEO-1.5* | | | |
| $b \to u\ell\nu$ | R.Fulton | PRL **64**,16(90) | 8 Nov 89 |
| *CLEO-2* | | | |
| $B \to \rho\ell\nu$, $\pi\ell\nu$? | A.Bean | PRL **70**,2681(93) | 21 Dec 92 |
| $b \to u\ell\nu$ | J.Bartelt | PRL **71**,4111(93) | 7 Sep 93 |
| $\overline{B} \to \ell^+\ell^-$? | R.Ammar | PRD **49**,5701(94) | 1 Dec 93 |
| $B \to \ell\nu$? | M.Artuso | PRL **75**,785(95) | 31 Mar 95 |
| $B \to \pi\ell\nu,\ \rho\ell\nu$ | J.Alexander | PRL **77**,5000(96) | 1 Jul 96 |
| $B \to \ell\nu\gamma$? | T.Browder | PRD **56**,11(97) | 12 Nov 96 |
| $b \to s\ell^+\ell^-$? | S.Glenn | PRL **80**,2289(98) | 1 Oct 97 |
| *CLEO 2.5* | | | |
| $B \to \rho\ell\nu$ for $\lvert V_{ub}\rvert$ | B.Behrens | PRD **61**,052001(00) | 24 May 99 |
| $B \to \ell^+\ell^-$? | T.Bergfeld | PRD **62**,R091102(00) | 19 Jul 00 |
| $B \to \tau\nu,\ K\nu\overline{\nu}$ | T.Browder | PRL **86**,2950(01) | 26 Jul 00 |

TABLE XV. CLEO Publications: $B$ Nonleptonic Decays to Charmonium and to Baryons

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $B \to$ baryons | M.S.Alam | PRL **51**,1143(83) | 24 Jun 83 |
| $B \to \psi X$ | P.Haas | PRL **55**,2348(85) | 26 Jun 85 |
| $B \to \psi X$ | M.S.Alam | PRD **34**,3279(86) | 2 Sep 86 |
| $B \to Y_{c,baryon}$ | M.S.Alam | PRL **59**,22(87) | 28 Apr 87 |
| *CLEO-1.5* | | | |
| $B \to$ baryons | G.Crawford | PRD **45**,752(92) | 15 Apr 91 |
| *CLEO-2* | | | |
| $\overline{B} \to \Sigma_c X$ | M.Procario | PRL **73**,1472(94) | 21 Dec 93 |
| $\overline{B} \to X_{c,excl}$ and $X_{c\bar{c},excl}$ | M.S.Alam | PRD **50**,43(94) | 4 Jan 94 |
| $B \to \psi\pi$ | J.Alexander | PLB **341**,435(95) | 14 Oct 94 |
| $B \to c\overline{c}X$ | R.Balest | PRD **52**,2661(95) | 13 Dec 94 |
| $B \to \psi\rho$ | M.Bishai | PLB **369**,186(96) | 11 Dec 95 |
| $B \to$ baryons | R.Ammar | PRD **55**,13(97) | 12 Jun 96 |
| $B \to \psi K^{(*)}$ | C.P.Jessop | PRL **79**,4533(97) | 24 Feb 97 |
| $B \to \Xi_c^{0,+} X$ | B.Barish | PRL **79**,3599(97) | 8 May 97 |
| $B \to \Theta_c X$ | X.Fu | PRL **79**,3125(97) | 12 Jul 97 |
| $B \to$ baryons, rare | T.E.Coan | PRL **82**,492(99) | 19 Oct 98 |
| $B \to \psi\phi K$ | A.Anastassov | PRL **84**,1393(00) | 3 Nov 99 |
| $M(B)$ from $B \to \psi^{(\prime)} K$ | S.E.Csorna | PRD **61**,R111101(00) | 5 Jan 00 |
| $CP$ in $B^\pm \to \psi^{(\prime)} K^\pm$ | G.Bonvicini | PRL **84**,5940(00) | 2 Mar 00 |
| $B \to (c\overline{c})$.. | P.Avery | PRD **62**,R051101(00) | 28 Apr 00 |
| $B \to \eta_c K,\ \chi_{c0} K$ | K.Edwards | PRL —bf 86,30(01) | 7 Jul 00 |
| $B \to \psi(2S) K^{(*)}$ | S.Richichi | PRD **63**,R031103(01) | 16 Sep 00 |
| $B \to \chi_{c1,2} X$ | S.Chen | PRD **63**,R031102(01) | 19 Sep 00 |

TABLE XVI. CLEO Publications: Other $B$ Nonleptonic Decays to Charm

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $B \to K^{\pm}X,\ K_S X$ | A.Brody | PRL **48**,1070(82) | 28 Jan 82 |
| $\langle n_{ch} \rangle$ in $B \to X$ | M.S.Alam | PRL **49**,357(82) | 26 May 92 |
| $B \to X_{excl}$ | S.Behrends | PRL **50**,881(83) | 24 Jan 83 |
| $\overline{B} \to D^0 X$ | J.Green | PRL **51**,347(83) | 20 May 83 |
| $B \to X_{excl}$ | R.Giles | PRD **30**,2279(84) | 10 Sep 84 |
| $\overline{B} \to D^{*+} X$ | S.Csorna | PRL **54**,1894(85) | 11 Feb 85 |
| $\overline{B} \to D^{*} \rho$ | A.Chen | PRD **31**,2386(85) | 25 Feb 85 |
| $B \to \phi X$ | D.Bortoletto | PRL **56**,800(86) | 21 Oct 85 |
| $\overline{B} \to D_s X$ | P.Haas | PRL **56**,2781(86) | 7 Apr 86 |
| $\overline{B} \to X_c$ | D.Bortoletto | PRD **35**,19(87) | 21 Jul 86 |
| $B \to K^{+,-,0} X$ | M.S.Alam | PRL **58**,1814(87) | 29 Dec 86 |
| $B \to X_{excl}$ | C.Bebek | PRD **36**,3533(87) | 26 May 87 |
| *CLEO-1.5* | | | |
| $\overline{B} \to D_s X$ | D.Bortoletto | PRL **64**,2117(90) | 15 Jan 90 |
| *CLEO-2* | | | |
| $e^+e^- \to B^* X$ | D.Akerib | PRL **67**,1692(91) | 20 May 91 |
| $\overline{B} \to X_c$ and $X_{c\overline{c}}$ | D.Bortoletto | PRD **45**,21(92) | 29 Jul 92 |
| $B \to \eta X$ | Y.Kubota | PLB **850**,256(95) | 22 Feb 95 |
| $B \to D_s X$ | D.Gibaut | PRD **53**,4734(96) | 19 Oct 95 |
| $B \to 3h^{\pm}$ | T.Bergfeld | PRL **77**,4503(96) | 23 Aug 96 |
| $B \to D^{(*)} X$ | L.Gibbons | PRD **56**,3783(97) | 28 Feb 97 |
| $B^0 \to D^{*+} D^{*-}$? | D.M.Asner | PRL **79**,799(97) | 23 Apr 97 |
| $B \to D^* \pi$ | G.Brandenburg | PRL **80**,2762(98) | 25 Jun 97 |
| $B \to$ color suppressed | M.Bishai | PRD **57**,5363(98) | 28 Aug 97 |
| $B \to$ charm | T.E.Coan | PRL **80**,1150(98) | 12 Oct 97 |
| $B \to D_{s1}(2536) X$ | M.Bishai | PRD **57**,3847(98) | 23 Oct 97 |
| $B^+ \to \overline{D}^0 K^+$ | M.Athenas | PRL **80**,5493(98) | 3 Mar 98 |
| $B^0 \to D^{*+} D^{*-}$ | M.Artuso | PRL **82**,3020(99) | 16 Nov 98 |
| *CLEO-2.5* | | | |
| $\langle n_{ch} \rangle$ in $B$ decay | G.Brandenburg | PRD **61**,072002(00) | 8 Sep 99 |
| $B \to \gamma$.. | T.E.Coan | PRL **84**,5283(00) | 23 Dec 99 |
| $\bar{B}^0 \to D^{*0} \gamma$? | M.Artuso | PRL **84**,4392(00) | 3 Jan 00 |
| $B^0 \to D^{*+} D^{*-}$ | E.Lipeles | PRD **62**,032005(00) | 29 Feb 00 |
| $B \to D_s^{(*)} D^{*(*)}$ | S.Ahmed | PRD **62**,112003(00) | 8 Aug 00 |
| $B^0 \to D^{*-} p \overline{p} \pi^+,\ D^{*-} p \overline{n}$ | S.Anderson | PRL **86**,2732(01) | 5 Sep 00 |

TABLE XVII. CLEO Publications: $B$ Charmless Nonleptonic Decays

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| $b \to u, s$ excl.? | P.Avery | PLB **183**,429(87) | 6 Nov 86 |
| *CLEO-1.5* | | | |
| $B \to p\overline{p}\pi$? or $p\overline{p}\pi\pi$? | C.Bebek | PRL **62**,8(89) | 3 Oct 88 |
| $b \to u$ excl.? | D.Bortoletto | PRL **62**,2436(89) | 16 Feb 89 |
| $b \to s$ excl.? | P.Avery | PLB **223**,470(89) | 10 Mar 89 |
| *CLEO-2* | | | |
| $B \to K^* \gamma$ | R.Ammar | PRL **71**,674(93) | 24 May 93 |
| $B \to \pi\pi,\ K\pi$ | M.Battle | PRL **71**,3922(93) | 11 Aug 93 |
| $b \to u$ with $D_s^{(*)}$? | J.Alexander | PLB **319**,365(93) | 8 Nov 93 |
| $b \to s\gamma$ | M.S.Alam | PRL **74**,2885(95) | 13 Dec 94 |
| $B \to$ charmless exclusive | D.M.Asner | PRD **53**,1039(96) | 19 Jul 95 |
| $B \to K\pi,\ \pi\pi,\ KK$ | R.Godang | PRL **80**,3456(98) | 17 Nov 97 |
| $B \to \eta^{(\prime)}$.. 2-body | B.Behrens | PRL **80**,3710(98) | 5 Jan 98 |
| $B^+ \to \omega K^+$ | T.Bergfeld | PRL **81**,272(98) | 20 Mar 98 |
| $B \to \eta' X$ | T.Browder | PRL **81**,1786(98) | 28 Apr 98 |
| *CLEO-2.5* | | | |
| $B \to \eta^{(\prime)}$.. 2-body | S.Richichi | PRL **85**,520(00) | 23 Dec 99 |
| $CP$ in $B \to$ charmless | S.Chen | PRL **85**,525(00) | 23 Dec 99 |
| $B \to K^{\pm,0}\pi^0,\ \pi^+\pi^-$ | D.Cronin-H. | PRL **85**,515(00) | 27 Dec 99 |
| $B \to$ charmless PV | C.P.Jessop | PRL **85**,2881(00) | 30 May 00 |
| $B \to \phi K^{(*)}$ | R.A.Briere | PRL **86**,3718(01) | 18 Jan 01 |

TABLE XVIII. CLEO Publications: Instrumentation

| Topic | Author et al. | Reference | Submitted |
|---|---|---|---|
| *CLEO-1* | | | |
| SC solenoid | D.Andrews | Adv.Cryo.Eng. **27**,143(82) | |
| CLEO-1 | D.Andrews | NIM A**211**,47(83) | 23 Aug 82 |
| *CLEO-1.5* | | | |
| Drift chamber DR2 | D.Cassel | NIM A**252**,325(86) | |
| *CLEO-2* | | | |
| CsI calorimeter | E.Blucher | NIM A**249**,201(86) | 19 Mar 86 |
| Trigger | C.Bebek | NIM A**302**,261(91) | 5 Dec 90 |
| CLEO-2 | Y.Kubota | NIM A**320**,66(92) | 16 Jan 92 |
| Luminosity measurement | G.Crawford | NIM A**345**,429(94) | 1 Feb 94 |
| CLEO-2.5 | | | |
| Dynamic $\beta$ effect | D.Cinabro | PRE **57**,1193(98) | ? 97 |

# References

[1] A. Silverman, "Birth of Electron Synchrotrons at Cornell", CLNS 88/875 (unpublished). This covers the same period as Wilson's anecdotal account, but with more emphasis on the experimental data.

[2] B.D. McDaniel and A. Silverman, "The 10-GeV Synchrotron at Cornell", Physics Today **21**, 29, (Oct 1968). This covers the design and construction of the synchrotron and the early experimental program.

[3] R.R. Wilson, "An Anecdotal Account of Accelerators at Cornell (For Bethe's Festschrift)", May 18, 1966 (unpublished). This is a good source for the history of the Laboratory of Nuclear Studies from 1945 to 1966.

[4] K. Gottfried, "Heavy Quark Spectroscopy Before the Discovery of $\Upsilon$, CLNS-97/1411, presented at the $b$20 Symposium, Illinois Institute of Technology, 29 Jun - 2 Jul 97.

[5] "Design Report: Cornell electron Storage Ring, April 1977", CLNS 360.

[6] A. Abashian, "Recollections on the Birth of CESR", presented at a talk at Cornell University, 9 Dec 99. This details the events at the National Science Foundation leading to the approval and funding for CESR construction.

[7] B.D. McDaniel, "The Commissioning and Performance Characteristics of CESR", IEEE Transactions on Nuclear Science, **NS-28**, no. 3, Jun 81. CESR in the first year of operations.

[8] D.M. Kaplan, "The Discovery of the Upsilon Family", paper presented at the International Conference on the History of Original Ideas and Basic Discoveries in Particle Physics, Erice, Italy, 29 Jul - 3 Aug 94. An account of the discovery of the $\Upsilon$(1S) and $\Upsilon$(2S) (and perhaps $\Upsilon$(3S) at Fermilab.

[9] G. Salvini and A. Silverman, "Physics with Matter-Antimatter Colliders", Phys. Reports, **171**, 231 (1988).

[10] J. Warnow-Blewett et al., *AIP Study of Multi-Institutional Collaborations, Phase I: High-Energy Physics*, Center for History of Physics, American Institute of Physics, 1992. See especially the "Probe Report on the CLEO Experiment at CESR" (Report no. 4, part D) by Joel Genuth.

[11] R.A. Burnstein, D.M. Kaplan, and H.A. Rubin, eds., **Twenty Beautiful Years of Bottom Physics**, AIP Conference Proceedings 424. Presentations at a symposium on the history of $b$-physics held at Illinois Institute of Technology in 1997. See in particular the talks by Silverman, Stone, Lee-Franzini, Poling, and Honscheid.

[12] K. Berkelman, "Upgrading CESR", Beam Line (publ. by SLAC), vol. 27, no. 2, p. 18 (summer 1997).

[13] Quarterly and annual reports from CESR/CLEO to the NSF.

[14] Annual funding proposals from CESR/CLEO to the NSF.

[15] Presentations to the CESR Program Advisory Committee.

[16] Minutes of monthly CLEO collaboration meetings, N. Horwitz, Syracuse University.

[17] Internal CESR and CLEO reports. The important series are denoted CLNS, CBX, CBN, SRF.

[18] `http://www.lns.cornell.edu` Current information on the Laboratory facilities, research program, publications, etc.