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The Discovery of the b Quark at Fermilab in 1977: The Experiment Coordinator's Story

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Abstract. I present the history of the discovery of the Upsilon (Υ) particle (the first member of the b-quark family to be observed) at Fermilab in 1977 by the CFS (Columbia-Fermilab-Stony Brook collaboration) E288 experiment headed by Leon Lederman. We found the first evidence of the Υ in November 1976 in an early phase of E288. The subsequent discovery in the spring of 1977 resulted from an upgraded E288 — the $\mu\mu$II phase, optimized for dimuons, with about 100 times the sensitivity of the previous investigatory dimuon phase (which had been optimized for dielectrons). The events leading to the discovery, the planning of $\mu\mu$II and the running, including a misadventure (the infamous Shunt Fire of May 1977), are described. Some discussions of the aftermath, a summary, and an acknowledgement list end this brief historical note.

OUTLINE

I INTRODUCTION

The search for di-leptons (charged-charged or charged-neutral) in hadronic interactions has been one of the most rewarding strategies in High Energy Physics. Almost all important discoveries in hadron collisions in the last 25 years have been made in this mode (J, Υ, W, Z, ...), and many others have been made in modes involving leptons (much of bottom and charm physics, top, ...).

The Upsilon discovery in 1977 at Fermilab marked a major landmark in this progression. It initiated the beginning of precision muon searches (in contrast to the J discovery, based on precision electron searches), bringing muon physics to a parity with electrons.

The genesis of the muon searches in hadronic interactions began with ground-breaking experiments by groups headed by Leon Lederman, described in later sections. As a graduate student working at Brookhaven in the beam line next to Leon's BNL dimuon experiment, I remember thinking "Why would anyone be interested in that?" — I think that a lot of hadron-collision experimentalists shared that feeling at that time. (I prefer to forget that my thesis experiment turned out to be a Baryonium experiment — fortunately after-the-fact, so that I was not sucked into that quagmire!)[1]

II THE 1968 BROOKHAVEN DIMUON EXPERIMENT, PRECURSOR TO E288/CFS

In the mid-sixties, Leon Lederman and his collaborators initiated a series of experiments looking first at single muons, then dimuons. This came after Schwartz, Steinberger, and Leon's Nobel-prizewinning second neutrino experiment at Brookhaven (where they missed neutral currents, calling those events "Crapons" — Leon gets my vote for the physicist who missed the most discoveries, as well as one of the, or even THE physicist, after Einstein, making the most discoveries).

Leon was interested in finding the W and Z, at that time postulated particles which could have had masses as low as a few GeV, which would then be accessible at Brookhaven, with proton beam energy of 28 GeV. This was about a decade earlier than the establishment of the electro-weak theory.

The 1968 Brookhaven dimuon experimental setup was based on a novel idea — ranging. An intense extracted proton beam was steered into a Uranium beam dump, where all hadrons, electrons, and photons were absorbed. Only muons — directly produced or from decays — survived. By measuring the range and direction of each muon, one could reconstruct the mass of the dimuon, albeit with poor mass resolution (of order of 1 GeV at a mass

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1) "Never have so many HEP physicists toiled so hard for so little!"

of 3 GeV). Decay muons contributed over 90% of the dimuon spectrum, but could be subtracted using measurements of accidentals. Surprisingly, a large rate of direct dimuon production was found [1]. This led Drell and Yan to publish their famous virtual-photon paper [2], so that their names were added to the HEP lexicon (the "Drell-Yan" process). (Some pundits opined that the correct terminology should be Yamaguchi-Lederman-Drell-Yan, since Yamaguchi's paper inspired Leon's dimuon experiment, but that would be too much of a mouthful.)

A rather enigmatic feature of the direct-dimuon spectrum was a broad bump at 3 GeV (obviously the first evidence for J/ψ in hindsight). However, Leon and his collaborators were not sure what to make of this —

- Could this be just another ρ' resonance (since the bump could be either very narrow, or broad — up to 1 GeV, one could not rule out this hypothesis)?
- Some light-cone theorists claimed that they could reproduce this bump without resorting to resonance.
- Some collaborators were vehemently against making a big deal over the resonance interpretation.

Leon decided to pursue this physics further with proposals at the CERN ISR and the soon-to-be-built Fermilab machine. One of his collaborators, Peter Limon, proposed a follow-up dielectron experiment at Brookhaven using existing detectors from the Lindenbaum group, but that idea died from lack of interest. A year afterward, Sam Ting proposed his BNL dielectron experiment, and the rest was history.

I was witness to an aftermath in August of 1974. Sitting in the Fermilab cafeteria, I heard Mary K. Gaillard (see paper mentioned in Section III) tell Leon that his Brookhaven bump was charmonium. It's clear that Leon's BNL bump was ahead of its time — had his result come after the acceptance of the GIM hypothesis [3], it would have been natural to interpret it as a charmonium state!!!

III WHAT WERE YOU DOING WHEN THE J/ψ WAS DISCOVERED ... (NOVEMBER 1974 REVOLUTION)

The series of experiments E70/E288/E494 was proposed by Leon and his collaborators on June 17, 1970. The co-authors included Taiji Yamanouchi and Jeff Appel; many other co-authors on the proposal to the then-National Accelerator Laboratory (since renamed Fermilab) either started new similar experiments (W. Lee, L. Read), or dropped out before the experiment was approved (J. Sculli, M. Tannenbaum, T. White). This is one prong of Leon's two-prong follow-up of the Brookhaven dimuon experiment discussed above. The Fermilab prong stressed the highest luminosity with the highest-energy accelerator. The CERN ISR prong (CERN-Columbia-Rockefeller collaboration in I1[2]) stressed the highest collision energy, using a two-arm dielectron non-magnetic spectrometer — another of Leon's experiments that missed the J/ψ, but discovered lead-glass darkening [4] and the copious production of high-p_t neutral pions, the first evidence for a power-law-vs.-p_t distribution, and indirectly for jets.

The goal of the E70/E288 CF (subsequently joined by S, for Stony Brook) collaboration at Fermilab was to do a complete survey of all leptons produced using the highest-intensity extracted proton beam from the new Fermilab Main Ring 300 GeV (eventually upgraded to 400 GeV) accelerator. The experiment would be performed in the Fermilab Proton Center hall, which was designed explicitly for the P70 single- and di-lepton experiments.

The first stage of the experiment would study how to do a single-arm electron-spectrometer experiment well. This would be followed by single muons, dielectrons, and dimuons. The electron spectrometer consisted of 1) a target box, with a small aperture whose position could be set at angles between 50 mrad and 100 mrad, 2) a sweeping magnet to sweep out all low-momentum particles and to bend the interesting electrons (along with charged hadrons) into the 3) detectors, which were placed outside the neutral-beam envelope (the boiling sea of photons and neutrons which would have swamped any detector). The detector consisted of scintillator-hodoscope arrays to measure the electron positions and bend angle (from which the momentum could be deduced), backed up by a lead-glass array to measure the electron energy and to differentiate between electrons and hadrons.

David Saxon and Maurice Bourquin arrived at Fermilab in 1972 to lay preparatory groundwork for the experiment. By 1973, Jeff Appel and many others arrived to set up the E70 single-arm electron spectrometer. Irwin Gaines and Hans Paar, the thesis students, Jean-Paul Repellin, Jean-Marc Gaillard, Bruce Brown, and myself arrived to join Leon, Jeff, Taiji, Dave, and Maurice.

By 1973, we started taking data with the single-arm electron spectrometer. Within six months, there were indications that direct electrons (*i.e.*, those not coming from photon conversions or Dalitz decays of the neutral pion) were observed, at a rate a few times 10^{-4} that of hadrons of equal p_t. We were thus diverted from the di-lepton phase to study these direct electrons in detail, taking data at various angles.

While we were on the direct-electron "kick," the November revolution happened. Sam Ting, in redoing Leon's Brookhaven dimuon experiment using dielectrons and the newly-available multiwire proportional chambers for much

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[2] ISR interaction region 1

better mass resolution, discovered the J, with preliminary indications in the late summer of 1974. Unfortunately, he did not publish until the SLAC Mark I experiment found the ψ (the same particle) at the electron-positron storage ring SPEAR in early November. Hence the double simultaneous publication in PRL [5], and eventually the double Nobel Prize.

The J/ψ particle was actually expected, at least by much of the theoretical-physics community. The charm hypothesis was originally a speculation of Bjorken and Glashow as early at 1964 [6]; however, it was not until 1970 that GIM (Glashow, Iliopoulos, Maiani [3]) provided a compelling motivation for charm — it handily explained one of the major mysteries of HEP at that time, the suppression of strangeness-changing-neutral-currents. But much of the experimental community was not impressed, and remained on the resonance kick (Argand diagrams, spin-parity analysis, X, Y, split A_2, ...).

An interesting anecdote is what Shelly Glashow preached to the experimental skeptics at their stronghold — the 4/2–27/1974 4th International Conference on Experimental Meson Spectroscopy, held at Northeastern University in Boston. His prediction for EMS 76, the next conference of the series held every 2 years, was:

> "There are just three possibilities:
>
> 1. Charm is not found, and I eat my hat.
> 2. Charm is found by hadron spectroscopy, and we celebrate.
> 3. Charm is found by outlanders, and you eat your hats."

This was just six months before the November revolution!!! Shelly obviously got to keep his hat.

Another interesting aspect was the paper by Gaillard, Ben Lee, and Rosner [7] entitled "Search for Charm." The preprint was dated August 1974 (Fermilab-Pub-74/86-THY), but was published in Reviews of Modern physics only after the discovery (text unchanged, except for an appendix updating the discovery). All the physics of the charmonium (renamed J/ψ by the discoverers) and charm particles was expounded in glorious detail, and most was correct except for one glaring mis-prediction and one even-more-glaring omission. The mis-prediction was on the branching ratio of charm mesons to $K\pi$, where the paper predicted a BR 10 times higher than measured later — this would in 1976 make some people believe that J/ψ did not represent charmonium. The omission, which came from the experimental naïveté of theorists, was the statement that charmonium would not be discoverable in electron-positron collisions, since it was so narrow — they did not appreciate the radiative tail of the electron beam, which makes a significant fraction of the collisions occur not at twice the beam energy, but at lower energies. Thus, even when data-taking occurs at ($2 \times$ beam energy) $= 3.2\,\mathrm{GeV}$, enough collisions occur at $3.095\,\mathrm{GeV}$ to make the interaction rate 20% higher than normal, enough to make the puzzled Mark I people investigate this point more thoroughly, and discover the ψ.

IV THE EARLY HISTORY OF E288/CFS

Proposal # 288 to the Fermilab management, "A Study of Di-Lepton Production in Proton Collisions at NAL," was dated February of 1974. The text is a short one-page digest, stating the following goals:

"1. Observe and measure the spectrum of virtual photons emitted in p-nucleon collisions via the mass distribution of e^+e^- pairs.
2. Search for structures in the above spectrum, publish these and become famous.
3. [charged hadron pairs]
4. [dimuons]
5. [dimuon structures]
6. [neutral pion pairs via conversion]"

After the November revolution, we at CF/E70 realized that we had missed the boat. The dielectron phase started soon after, featuring newly-installed MWPCs built under the direction of Bruce Brown, and the J/ψ was observed at Fermilab within one year of its discovery. The dielectron phase involved a two-arm spectrometer. Again, the acceptance was small due to the need to place the detectors out of the neutral-beam envelope (at least a factor of 5 loss in acceptance). The incoming proton-beam intensity had to be scaled down since the charged-hadron rates were so high.

We took dielectron data until 1976. In the mean time, a proposal from the Stony Brook group headed by Bud Good implementing the dihadron part of P288 was accepted. This dihadron experiment would run simultaneously with our dielectron search, though with a separate experiment number — E494. The Stony Brook group built gas Cherenkov counters to differentiate among π's, K's and p's. Several of the Stony Brook physicists also joined E288 (hence the S in CFS).

By the middle of 1976, a substantial chunk of dielectron data had been taken. The first look revealed a clustering of events near 6 GeV; the probability of such a clustering anywhere in the plot was estimated conservatively at one chance in 50. We thus gave talks suggesting that this might be evidence for a new resonance. Jeff Weiss did an "availability search" of the Greek alphabet and found that the Greek letter Upsilon was not yet used (Iota was rejected since it resembles a question-mark — in hindsight, it would have been a better choice!). Walter Innes added that the name allowed us to make a Leon-type joke — Upsilon if the resonance is real, and the similar-sounding "Oops-Leon" if the resonance is false. Since our collaboration was a sucker for bad puns (considering our genealogy), we were taken in. Saner heads, such as Taiji Yamanouchi, were ignored. In our Phys. Rev. Letter [8], we backpedaled a little, by suggesting that the name Upsilon could be assigned either to the resonance (if real) or to the "onset of high-mass di-lepton physics."

In the spring of 1976, we took some data in the dimuon mode, using the detector setup optimized for dielectrons. This provided only a factor of 5 increase in sensitivity — but that was sufficient to show that the 6 GeV "resonance" was an "Oops-Leon" and not an "Upsilon."

V FIRST HINT OF UPSILON IN NOVEMBER 1976

As the "Oops-Leon" 6.0 GeV dielectron bump faded with the summer, I kept up with the data coming in, doing data reduction as well as a first look at the spectrum — within days of the data-taking. Soon after we reverted to dielectrons (E288/E494), I noticed another clustering and wrote an internal note dated 11/17/76, entitled "From the people who brought you the Υ, a bigger (but not necessary better) resonance." This note was triggered by two recent dielectron events at 9.51 and 9.67 GeV. When combined with other events from the *ee* spectrum and a cluster of 6 dimuon events near 9.5 GeV, these resulted in a cluster of 10 events within 300 MeV, compared to 7 events in adjacent bins 4 times wider (*i.e.*, a 1.75-event estimated background) — a probability of less than one in 200 or so, even accounting for possible clustering anywhere in the mass plot. As I was writing the memo, another event came in at 9.44 GeV, strengthening the clustering. The significance of this clustering was thus much stronger than the "Oops-Leon." Some collaborators even claimed that I underestimated the significance. My conclusion in that internal note was that "$\mu\mu$II," a phase then under planning and scheduled to run in the Spring of 1977, just 6 months away, "should settle this in 1 month [of running]." I also put a bottle of French champagne (Moët) with the written label "Υ 9.5" pasted on in the refrigerator at the experiment's trailer.

Thus the year 1976, which was so disastrous for CFS in mid-summer, ended on a hopeful note.

VI E288/CFS $\mu\mu$II PHASE — PLANNING AND IMPLEMENTATION

It is well known that by searching for muons in the final state in hadronic interactions, one could reach much higher sensitivity than by searching for electrons. This involved putting absorbers just downstream of the interaction point to absorb all the hadronic debris from the interactions, reducing the rates of particles in the detectors by orders of magnitude. Furthermore, there would be no "neutral envelope" to worry about, making possible the placement of detectors much closer to the bending magnet and giving a factor of 3–5 increase in acceptance. Thus, the sensitivity for dimuons could be about two orders of magnitude higher than for dielectrons.

Unfortunately, the absorbers traversed by the muons would result in multiple scattering, worsening the eventual di-lepton mass resolution (*e.g.*, Leon's Brookhaven dimuon spectrum, and his joke – if memory serves – that "anything that could flatten the [J/ψ] skyscraper into the mound of rubble observed by us at BNL in 1968 should be proscribed by SALT [the anti-nuclear treaty]").

A detailed analysis of this problem was undertaken by Leon, Steve Herb, myself, and others. The trick is to put the densest absorber near the interaction point, and only low-Z absorber afterwards. This leads to a smearing of the production-angle measurement, but not to a large error in momentum determination. The resultant mass resolution would be about 2% near 10 GeV mass, in contrast to the 30% or so mass resolution for Leon's BNL dimuon spectrum.

Initial work on this optimized dimuon phase (called $\mu\mu$II, since $\mu\mu$I was the dimuon phase using the apparatus optimized for dielectrons modified with absorbers, but not optimized for dimuons) began with:

- Leon's 2/12/75 memo starting with the words "We propose to do dimuons without a movable filter using fixed beryllium filter to attenuate hadrons."
- The "Super 288 White Paper," signed by Leon and Taiji, dated January 28, 1976.

Memos flew by with increasing frequency. For example,

- I wrote a note dated 2/17/76 entitled "I: expected E288 I/II signal and backgrounds, II: options for improving E288 I/II signal/background," projecting the two orders of magnitude increase in sensitivity with $\mu\mu$II over dielectrons.
- Bruce Brown wrote a note dated 5/10/76 proposing "Muon momentum confirmation with a steel magnet" (remeasurement), a proposal that was adopted.
- A note by Leon, Walt, and Steve dated 6/22/76 on a proposed PWC system for $\mu\mu$II.
- Steve Herb, in a note dated 7/3/76, gave a detailed PWC proposal; the chambers are much closer to the magnet and the acceptance is much higher than in $\mu\mu$I.
- — etc.

Leon, Steve, and others thus worked hard to design a target box with mostly Be absorber in the aperture, but with an option to place interchangeable Be, Cu, or W absorbers immediately downstreams of the target. Extreme care was taken to avoid cracks, and to angle the possible interfaces to avoid even hairline cracks pointing to the interaction point. This was the major innovation in the $\mu\mu$II phase of E288.

Many other aspects of the upgrade to $\mu\mu$II were worked on by other collaborators: Bruce Brown proposed a "remeasuring" iron magnet to confirm the momentum and provide rejection against backgrounds; Dan Kaplan worked on the on-line system; Walt Innes worked on the track reconstruction; Koji Ueno on the Monte Carlo; Chuck Brown on monitoring and alignment; Bob Kephart and Hans Sens on the Directional Drift Chamber; Steve on gas system and survey; Hans Jöstlein on measuring the iron-magnet field, etc.

The installation of the target box and rigging of the detector and shielding piles were undertaken in early 1977, led by Steve Herb and Karen Kephart, allowing us to take a short test run in April 1977. The 9.5 GeV resonance was alive and well, though not yet definitive.

VII THE DISCOVERY

$\mu\mu$II data-taking commenced at 13:00 on May 13, 1977. I took the three or so data tapes generated each day to the Hi-Rise and submitted a batch job doing the data reduction and subsequent first-pass analysis. Thus, prelimilary results were available within two days of the data-taking.

However, the gods were not through toying with us yet. On May 20 just before 11 pm, barely 7 days after data-taking started, there was a magnet shunt that failed disastrously (rather than fail-safe!). It melted and started a fire in the cables in the adjacent cable tray. Chlorine- and fluorine-laden smoke filled the experiment pit and deposited acidic residue on the amplifier cards mounted on the wire chambers. This residue could possibly eat into the printed-circuit traces and electronic components, and thus increase the failure rate to an unacceptable level — we could be down every few hours replacing electronics!!! The problem was obvious — a finger rubbed gently on a circuit board picked up a sour-tasting coating. Data-taking was stopped for a week while we figured out how to recover.

Leon remembered a similar fire incident at CERN, and, more importantly, was able to find by 3 am (barely 4 hours after the fire) the phone number of a Dutch fire-salvage expert, and convince him to come immediately to Fermilab, bringing his "magic" liquids. However, his visa was a problem — it might take days to obtain. Leon got lucky again — he found a high official at the local embassy who was a Columbia alumnus. Being a Columbia professor, Leon was able to convince him to provide a visa speedily. The expert arrived the next day, and was busy telling us what to do. We (physicists, technicians, girl friends, *et al.*) worked 'round the clock to remove the electronic cards, dip them in the magic liquid, brush them, and dry them. It worked marvelously

— and the failure rate of the electronics was in fact lower than before!!!

By 6/4/77, barely one week after data-taking resumed, the 9.5 GeV-resonance significance was already more than 8σ. We spent the next weeks taking more data and doing studies on efficiency and systematics, to make sure that the effect was not an artifact. We took data with a different analysis-magnet current to make sure that there were no geometric aberrations; we compared the data before and after the fire. My analysis results were checked by many other people. Acceptances were calculated by Koji and Hans, *et al.* Many meetings were held to discuss the results, with many people (Leon, Steve, Walt, etc.) making suggestions of what to check and study. These studies were done by many of the collaborators. Finally, even Taiji was convinced that we had now finally discovered a new particle.

On June 30th, 1977, Steve Herb announced the discovery at Fermilab. The PRL paper [9] was submitted the next day, July 1, 1977. I gave a talk at Brookhaven and Walt at SLAC soon after. The HEP world finally took this seriously after Leon gave his talks at the Budapest EPS and the Hamburg Lepton-Photon meetings in July and August of 1977. Thus, this discovery was made in six weeks (minus one week lost due to the fire), by 16 authors.

The discovery of Υ (or bottomonium) was actually more unexpected than that of the J/ψ (charmonium). The Kobayashi-Maskawa paper [10] speculating on six quarks, though published in 1973, was totally unknown in the U.S., having been published in the obscure Japanese journal Progress of Theoretical Physics. The preliminary evidence for the τ from Mark I in 1975 was weak, and not established for a long time, becoming believable only after more data were collected by PLUTO and Mark I (some Europeans would argue that the first believable evidence for τ was actually that of PLUTO!). However, that did not stop Haim Harrari in the summer of 1975 from speculating that this third charged lepton must indicate a new pair of quarks, which he named bottom and top. This lepton-quark-universality hypothesis was much weaker than the charm hypothesis, since it had no other supporting evidence. Remember that these were the days of the notorious Cline-Mann-Rubbia high-y anomaly and singlet-b-quark evidence!! The third-generation hypothesis only became believable after the discovery of Υ and the b mesons and hadrons. Kobayashi and Maskawa got their belated recognition, and the KM matrix entered the HEP language (eventually the CKM matrix, recognizing Cabibbo's contributions).

VIII AFTERMATH (IS THERE LIFE AFTER ...)

The E288/CFS experiment and its offspring continued for many years. Many people, such as Al Ito, Chuck Brown, Dan Kaplan, *et al.*, worked on the analysis effort, and produced many measurements, such as $\sqrt{s}$ dependencies, p_t dependencies, target material (A) dependencies, etc., as well as many dihadron and other results. Others joined the collaboration and made major contributions.

In September of 1977, I began teaching at Columbia, followed one year later by Steve. Thus, our roles in CFS were reduced. On one of my increasingly rare visits to Fermilab in October of 1977, I read in the CERN courier an advertisement on the availability of the Cornell CESR North Area for a proposal for a small experiment to complement the large CLEO detector being built in the South experimental hall. Steve, Leon, and I discussed proposing an experiment for that area, and it seemed an obvious place to pursue bottom physics. We wrote a proposal, essentially detailing the eventual CUSB experiment, consisting of a 3π-solid-angle non-magnetic tracking system followed by NaI crystals, with lead glass to catch the energy leakage. The forward and backward directions were empty of detectors (except for luminosity monitors). We were asked at the first program-advisory-committee meeting in late 1977 to look for more collaborators, and asked the Franzinis to join. Unfortunately, soon after the experiment was approved, Leon was given an offer he couldn't refuse — directorship of Fermilab. Thus, Steve, the Franzinis, I, and our collaborators built the CUSB detector and discovered the $\Upsilon(4S, 5S)$ (along with CLEO), χ states through photon transitions, and the first evidence for B mesons (via lepton-spectrum cutoff at $\Upsilon(4S)$ mass divided by 4, not by 2, as well as evidence for B to D transitions, not to ρ or π's only). Subsequently I moved to Fermilab to work on CDF; Steve moved to Cornell and eventually DESY to work on the machine there.

While many of our E288 collaborators remained at Fermilab (Appel, Yamanouchi, Bruce and Chuck Brown, Kephart, Jöstlein, Ito), others left — Innes (to SLAC, now on BaBar), Kaplan (now at IIT), Hom (in New York City), Sens (now at CNRS), Snyder (now at Gallaudet College).

IX SUMMARY AND OUTLOOK

The discovery of the Upsilon,[3] coming just three years after the November 1974 revolution, continued the string of new quarks, culminating in the recent top discovery at Fermilab (the 3rd-generation lab?) by CDF and D0.

In some respects, the bottom quark has significance way beyond "just another quark." Due to the long lifetime and mixing, CP violation in the B-meson systems becomes the new "Holy Grail" of HEP. Several B factories are being built. A crude estimate would suggest that roughly 1/3 of current HEP experiments are either studying B physics or using B as tags (*e.g.*, the top discovery).

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[3] I intend soon to put the history of the Upsilon discovery on the Web, with links (hopefully) from the official Fermilab web pages, and with scanned images of crucial memos, pictures of the apparatus and collaboration, links to other B-physics web sites, etc.

Other aspects of B also increase its significance: it's the heaviest quark that still has a real meson (as opposed to the virtual T meson, which lives too briefly to be a real physical meson); the b-quark is heavy enough that theoretical calculations for the various Υ and other bottomonium mass levels can be reliably calculated; ...

(Note that there are other papers covering the discovery of the Upsilon, such as Dan Kaplan's version [11].)

X ACKNOWLEDGEMENTS

As the experimental coordinator of CFS/E288 during the period 1975 to 1977, I participated in one of the most exciting episodes in my physics career. Most of the credit for this discovery belongs with

- Leon Lederman, the "founding father" and "leading light" of the experiment, as well as the collaboration's spokesman. Many of the key concepts of the E70 and E288 experiment designs originated with him. In particular such crucial ideas for $\mu\mu$II as the Be absorber and many other issues (expected rates, detector arrangement, etc.) were first considered by him.

However, many others of the CFS/E288 collaboration played important roles in this discovery. (First, a caveat: the list below is based on my faulty 20-year-old memories, as well as internal notes from that period — fortunately, several are job lists I wrote that have people assigned to each item; nevertheless, some significant effort is likely to be missed in the list below, and I apologize in advance to those collaborators who might feel slighted — note that the contributions listed below are specifically for the months leading up to the discovery, not to any prior or subsequent contributions):

- Steve Herb — the $\mu\mu$II upgrade manager, the major architect with Leon of the target box, shielding pile, etc. Also worked on PWC placement, survey, and off-line work on fiducial cuts, position of detectors, monitoring.

- Walter Innes — the production event-reconstruction expert and architect; off-line work on resolution, etc.

- Bruce Brown — the wire-chamber expert, who also proposed the iron remeasurement magnet.

- Dan Kaplan — $\mu\mu$II thesis student from Stony Brook; on-line expert; off-line work on analysis, MMPWC, magnetic-field study, muon criteria.

- Koji Ueno — the Monte Carlo expert and main architect; worked on acceptances, energy loss, etc.

- Jeff Appel — experimental coordinator from 1973 to 1975, a "founding father," worked on the design of E70 and early phases of E288, $\mu\mu$II beam quality monitoring.
- Chuck Brown — monitoring, triggering, facilities support, off-line work on resolution, chamber alignment.
- Dave Hom — thesis student for previous dielectron data.
- Al Ito — collimator.
- Hans Jöstlein — iron-magnet field measurements.
- Bob Kephart — Cherenkov expert, "keeping John honest," Directional Drift-Tube intensity monitors.
- Hans Sens — target box, DDC (Directional Drift Chamber), Monte Carlo acceptance.
- Dave Snyder — thesis student for previous dielectron data.
- Taiji Yamanouchi — head of the Fermilab contingent; a "founding father;" the "voice of caution."
- I myself (JKY), besides being the experiment coordinator, wrote and ran single-handedly the first-phase data-reduction process, using a "quickie-track-reconstruction" algorithm I wrote that complemented the "full-track-reconstruction" algorithm written by Walt Innes that was used in the reconstruction phase. Also a host of off-line studies (event display, trigger studies, resolution using J/ψ, intensity dependences, etc.). I also did much of the physics analysis and studies for the discovery, along with efforts by Dan and others.

Of course, all collaborators worked on various other aspects of analysis, discussions of results and how to validate them, etc. — far too much to list.

- Our Fermilab-resident technicians — Karen Kephart, Frank Pearsall, and Jack Upton from Fermilab, Ken Grey from Columbia/Nevis, and Tom Regan from Stony Brook. They worked hard on chambers, target box, rigging, etc. and deserve much credit.
- The Nevis (Columbia Univ.) support people, especially Bill Sippach, whose pioneering electronic designs have had a major influence on all of HEP, Yin Au, mechanical engineer who participated in much of the apparatus design, and Art Timm (plastics expert), Herb Cunitz and Ed Taylor (electronics shop), *et al.*

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