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The Discovery of the Upsilon Family

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The Discovery of the Upsilon Family*

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Abstract

The discovery in 1977 of the upsilon family of resonances represented the culmination of many years of experimentation in dilepton production by hadron collisions. Following the pioneering experiment at the Brookhaven AGS, a series of Fermilab experiments by the Columbia/Fermilab/Stony Brook collaboration found the three lowest-lying bound states of the $b\bar{b}$ system, establishing the existence of the b quark and confirming the third generation of fundamental fermions. Like most stories, this one had its share of false starts and loose ends, some of which are discussed; a few modest morals are drawn.

1 Introduction

The discovery [1, 2, 3] of the upsilon family of resonances in 1977 established the existence of the third generation of quarks. This discovery was the culmination of a series of experiments carried out under the leadership of Leon Lederman, and he has written extensively on the subject [4]. The reaction used was dimuon production in proton-nucleon collisions:

$$p + N \rightarrow \mu^+\mu^- + X\,.$$

The first such experiment (Fig. 1) was carried out by the Lederman group at the Brookhaven AGS in 1968 [5], and succeeded in observing muon pairs at masses up to the kinematic limit (Fig. 2). The emphasis on measurements at the highest possible masses was characteristic of Lederman's approach and stemmed from the idea that massive virtual photons could be used to probe the structure of nucleons at small distances. To maximize rate capability and get to the highest masses, measurements were performed only after the absorption of hadrons in >3 m of uranium, iron, and concrete. The resulting multiple scattering of the muons limited the mass resolution to $\sigma_m/m \approx 15\%$ at $m = 2\,\mathrm{GeV}$ (decreasing to $\approx 8\%$ at 5 GeV), illustrating the trade-off between rate capability and resolution. Despite the shoulder evident in Fig. 2 at

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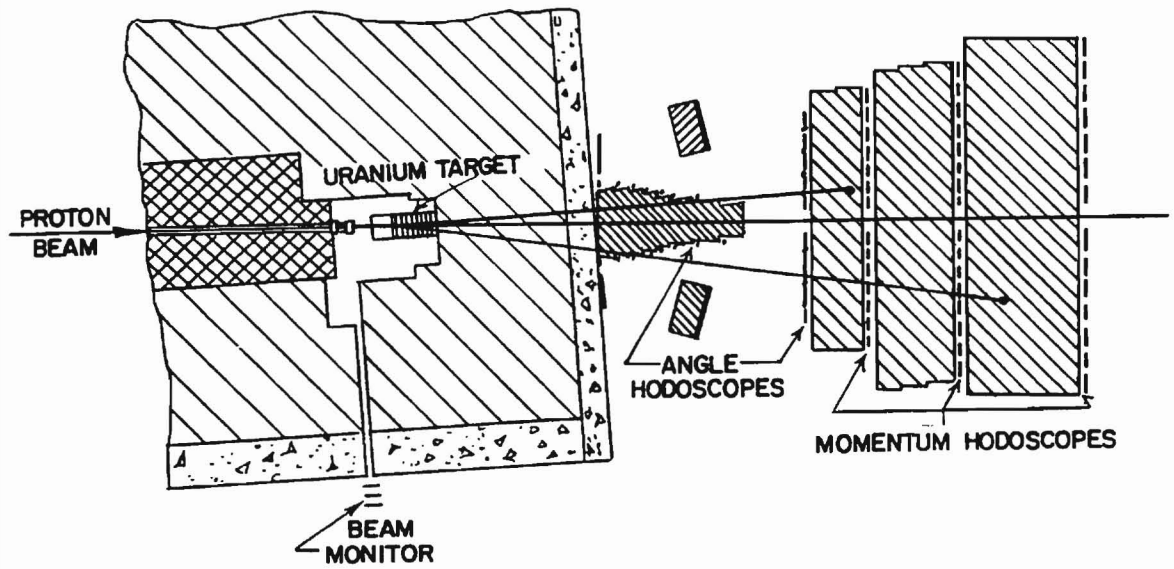

Figure 1: Brookhaven AGS dimuon experiment.

$m \approx 3\,\mathrm{GeV}$, which arguably could have been interpreted as a resolution-broadened resonance, the experimenters concluded [5] that the observed cross section "varies smoothly with mass exhibiting no resonant structure." Although they considered the resonance interpretation, uncertainty whether the shoulder was an apparatus artifact prevented them from espousing it [6]. Their uncertainty can be understood by comparing the "raw" data (Fig. 2b) to the cross section inferred therefrom (Fig. 2a): the shoulder is hardly apparent in the raw data, and was easy to ascribe to a misunderstanding of the shape of the mass acceptance.

In a famous paper [7], Drell and Yan interpreted the mass distribution in terms of the then-recently-proposed parton model of Feynman [8] (see Fig. 2c). This stimulated a series of further experiments by the Lederman group and many others (and a great deal of theoretical activity) which firmly established the "Drell-Yan experiment"[1] as a tool with which to understand hadron structure and test QCD. The dilepton continuum is outside the purview of this article and will not be discussed further here; the interested reader can find many good reviews (see for example those of Ref. [9]).

While the author has no first-hand knowledge of this story prior to the Fermilab dilepton experiments which began in 1975, the motivation for the earlier work has been discussed by Lederman [10]. In the early 1960s (following their work with

[1]It is ironic that the study of massive dileptons produced in hadron collisions is universally known by the names of the theorists who first interpreted it, rather than that of the experimentalist who pioneered the technique!

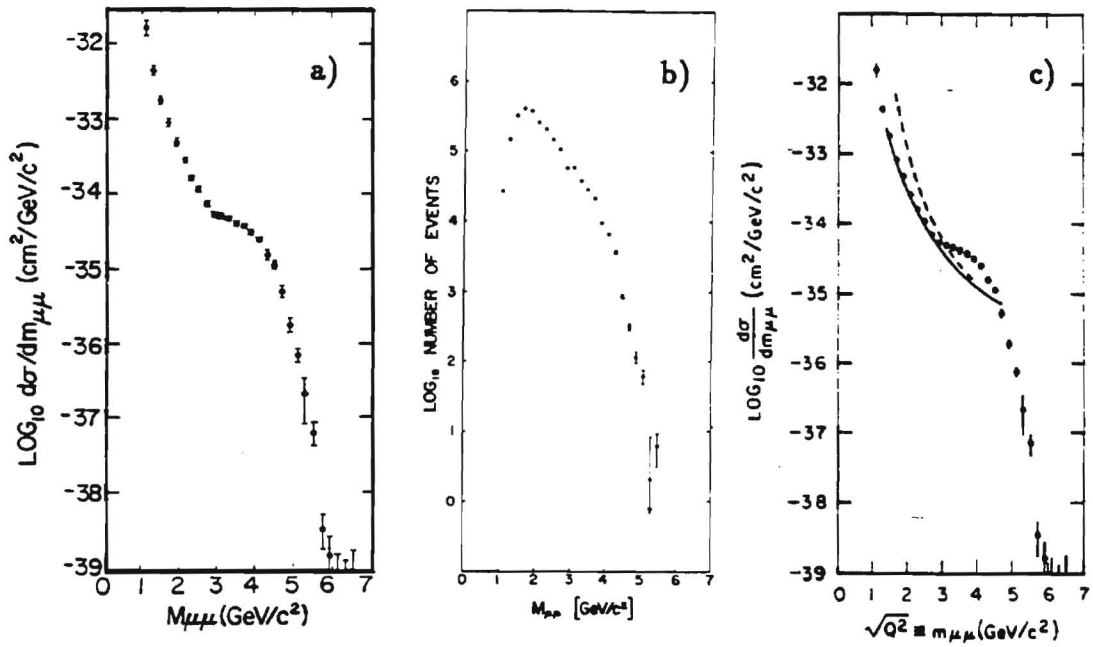

Figure 2: a) Dimuon yield measured at AGS; b) raw data; c) dimuon yield with Drell-Yan curves superimposed.

neutrino beams [11]) the Lederman group was searching for W production at the AGS [12] via the reaction

$$p + N \rightarrow W + X\,,$$

$$W \rightarrow \mu\nu\,.$$

Yamaguchi [13] and Okun [14] pointed out a significant background: production of virtual photons and their decay into lepton pairs. The group realized this "background" could be exploited as a probe of the internal structure of nucleons (complementary to the space-like virtual photons employed at SLAC [15]), hence the AGS dimuon experiment described above. Following that experiment Lederman *et al.* in 1970 proposed dilepton experiments at the new National Accelerator Laboratory and CERN ISR[2] to push the virtual-photon probe to higher masses (shorter distances). A year and a half later, following studies at DESY of vector-meson production and the hadronic component of the photon current [16], Ting *et al.* proposed a dielectron experiment at the AGS [17].

[2]The CCR (CERN/Columbia/Rockefeller) experiment, which later became CCOR when the colaboration was joined by a group from Oxford.

2 NAL E-70: Discoveries Found and Lost

The June 1970 proposal [18] to the National Accelerator Laboratory (NAL) by the Lederman group was designated P-70 and encompassed measurements of electron pairs as well as a W search via single electrons and a study of photon and π^0 production. An additional motivation was searching for the "heavy photon" predicted by Lee and Wick [19]. P-70 emphasized electrons rather than muons in order to improve on the AGS experiment's mass resolution. The proposed apparatus was to consist of two symmetrical spectrometer arms, each designed to measure one member of the pair.

Since funds were tight the group decided in 1973 to build one arm first and get some experience with it before building the second arm. They thus missed the "discovery of the decade," the J/ψ particle: when the single-arm apparatus detected a surprising signal of electrons at high transverse momentum [20], they proceded to investigate this "direct lepton" production in detail, using electrons and muons and running at a variety of spectrometer angles, instead of getting on with the originally-proposed two-arm program. A direct-lepton industry ensued, with experiments springing up all over, including NAL, Brookhaven, CERN, and Serpukhov [21]. In the course of these investigations a crude second arm was built to test the possibility that direct leptons were actually produced in pairs, *e.g.* via anomalous production of ϕ mesons, but insufficient ϕ production was found to account for the effect [22], which was characterized as 10^{-4} of charged-pion production independent of transverse momentum [20, 21]. (We now know that direct leptons are produced dominantly by semileptonic decay of charmed particles, but the direct-lepton signature was too vague to establish unequivocally such an explanation.)

Early in 1974 the NAL Directorate decided enough time had passed since Proposal 70 that a new proposal and experiment number were in order. Lederman responded semi-seriously, with a one-page "E-288 white paper" – a six-point proposal which included as point two to "search for structures in the [e^+e^- mass] spectrum, publish these, and become famous" [23]. This we ultimately did, but not before publishing a presumed structure and becoming infamous!

The November 1974 announcement of the discovery of the J/ψ (and hence the charmed quark) by Ting's group at the AGS using a two-arm electron-pair spectrometer [24], and more or less simultaneously by Richter's group using the SPEAR storage ring at SLAC [25], stimulated Lederman's group to upgrade their apparatus and switch over to dedicated lepton-pair running. Whereas E-70 measured charged-particle trajectories using scintillation hodoscopes, Ting's experiment employed the new technology of multiwire proportional chambers (MWPCs), which provided over an order-of-magnitude improvement in position resolution. Already in early 1974 Fermilab postdoc Bruce C. Brown had begun designing MWPCs for an improved two-arm spectrometer. Construction was completed in the summer of 1975, and newly-arrived Fermilab postdoc Walter I. Innes helped with commissioning and developed the needed track reconstruction software. By the fall of 1975 the pair spectrometer

was in operation.

3 FNAL E-288: The Υ Experiment

Despite its brevity, Proposal 288 was quite broad, including studies of e^+e^- pairs, $\mu^+\mu^-$ pairs, single leptons, charged-hadron pairs, and π^0 pairs. To carry out this ambitious program, which called for the addition to the existing spectrometer of hadron calorimeters and Cherenkov counters as well as MWPCs, Lederman felt the need to expand and strengthen the collaboration, and in 1975 he invited a group from the State University of New York at Stony Brook to join. The Stony Brook group, under the leadership of M. L. "Bud" Good, included the author (then a graduate student) as well as Hans Jöstlein (with whom Lederman had worked on μp scattering at the AGS).[3] The flippant E-288 white paper served for about two years, but in April 1976, after the first ("*ee*I") E-288 run had been completed and while the second ("$\mu\mu$I") was in progress, Fermilab Deputy Director Ned Goldwasser asked for details of the group's plans. A six-page letter from Lederman to Goldwasser, dated May 10, 1976, served as the *de facto* proposal for the subsequent electron/hadron- ("$(e+h)^2$") and muon-pair ("$\mu\mu$II") running.

3.1 *ee*I: The Particle That Wasn't There

E-288 data-taking began in the dielectron mode. The apparatus (Fig. 3) featured small pair acceptance ($\lesssim 10^{-3}$), due primarily to the small accepted range of production angle (± 3.5 mr) in the vertical (bend) direction, which decoupled the momentum measurement from the dominantly horizontal production-angle measurement. The horizontal apertures covered $\pm$ (50 to 95) mr, and were thus centered on $\approx 90°$ in the center-of-mass system at 300 – 400-GeV beam energy. The two analysis magnets were connected in series and operated with the same polarity, so that if one spectrometer arm (the "up" arm) deflected positive particles upward, the other (the "down" arm) deflected negative particles downward. In a technique perfected by E-70 [26] (and used also in Lederman's CCOR experiment at the ISR [27]), electrons were identified and hadrons rejected by means of a total-absorption lead-glass Cherenkov calorimeter segmented both longitudinally and transversely. Using a similar calorimeter, E-70 had

[3]Besides the addition of the new group, there was considerable rearrangement of personnel between E-70 and E-288, since the E-70 program had by then already stretched over some five years. At the beginning of data-taking the E-288 collaboration included Lederman, postdocs H. P. Paar, J. M. Weiss, and J. K. Yoh, and graduate students D. C. Hom and H. D. Snyder (Columbia); Staff Scientists J. A. Appel, C. N. Brown, and T. Yamanouchi and postdocs B. C. Brown and W. R. Innes (Fermilab); and Professor M. L. Good, Associate Professor R. J. Engelmann, Assistant Professors H. Jöstlein and R. L. McCarthy, Visiting Assistant Professor H. Wahl, postdoc R. D. Kephart, and graduate student D. M. Kaplan (Stony Brook). The group was later joined by J. C. Sens from NIKHEF (Amsterdam) and CERN (who provided some drift chambers for $\mu\mu$II), postdocs K. Ueno of Fermilab and A. S. Ito of Stony Brook, and graduate student R. J. Fisk of Stony Brook.

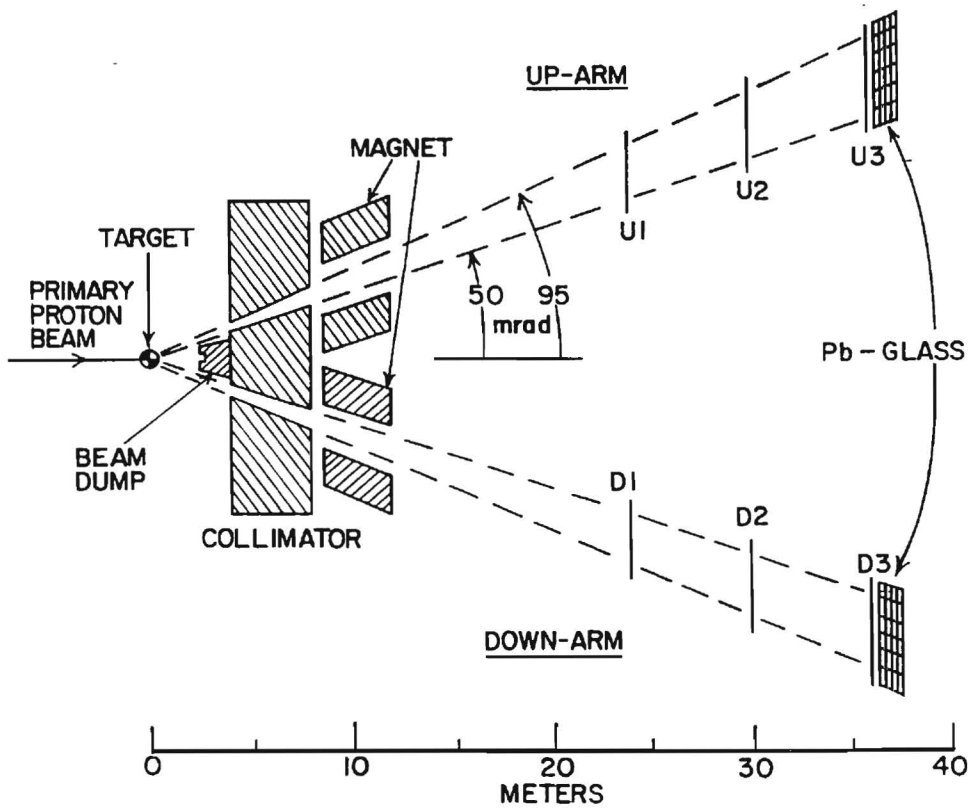

Figure 3: E-288 *ee*I apparatus (plan view); note extensive shielding and absence of detectors upstream of analyzing magnets, which maximized rate capability. In elevation view, each aperture subtended ± 3.5 mr.

achieved hadron rejection of order 10^{-5} by means of longitudinal shower development cuts and energy-momentum matching [20, 26]. The lead glass was placed far enough from the magnets that the charged and neutral fluxes had separated, so that the copious photon flux from π^0 decay (which might have caused the lead glass to yellow and lose sensitivity) could pass below the up arm and above the down arm. In addition, "neutral-beam absorbers" were built out of lead bricks[4] to attenuate the neutrals – an example of the simultaneous use of belt and suspenders.[5] All detectors upstream of the neutral-beam absorber were kept out of the neutral beam; while this minimized contributions of neutrals to counting rates, it imposed an upper momentum cut-off which reduced the acceptance at high masses.

A J/ψ signal was quickly observed (Fig. 4) [28]. In addition, a mass continuum

[4]In these experiments substantial amounts of shielding were stacked by the experimenters themselves, often with Lederman's enthusiastic participation. This can be a welcome bit of exercise in a normally sedentary occupation, though overdoing it may have caused a few cases of "shielding elbow."

[5]Actually the neutral-beam absorbers did serve an important purpose, since the neutrals would otherwise have interacted in and irradiated some of the lead-glass photomultiplier tubes and (in E-494 – see below) counted in the hadron calorimeters.

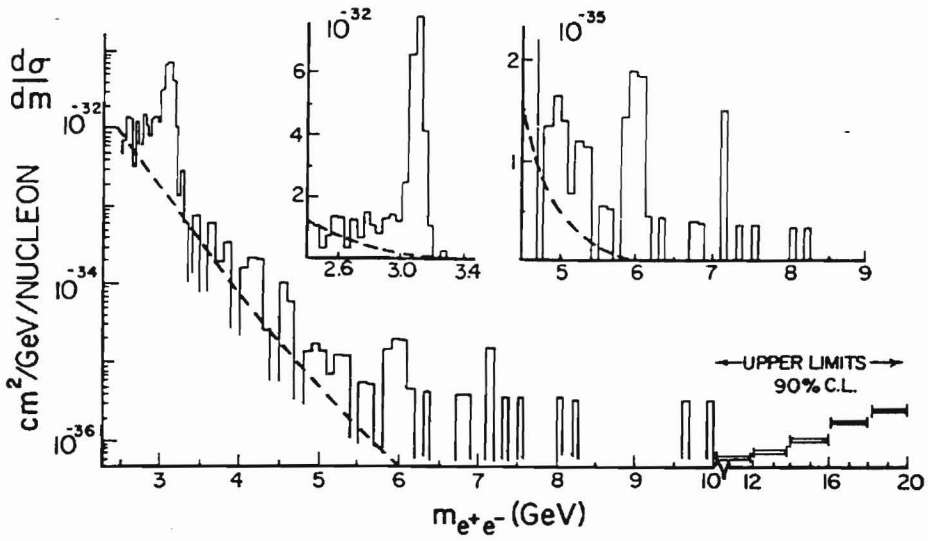

Figure 4: E-288 *ee*I dielectron yield (from Ref. [28]).

extending to 10 GeV was seen. Of 27 events observed between 5.5- and 10-GeV mass, 12 were clustered between 5.8 and 6.2 GeV, where a fit to the continuum (excluding that region) predicted 4 events. This suggested the possibility that still another narrow dilepton resonance was being observed. But it presented a dilemma: the statistical case for the new resonance was marginal (about $2\frac{1}{2}\sigma$), and a conservative person would not claim it as a discovery. But we knew that if the effect turned out to be real, immediately upon publication of our data SPEAR would tune their e^+e^- collider to 6 GeV and make the discovery. Having (by his count) missed the J/ψ three times – at the AGS, at the ISR, and in E-70 – Lederman was undoubtedly anxious not to miss the boat once more. He showed the prepublication data to Wilson, who suggested naming the new resonance "FNAL 6.0,"[6] since it was reasonably to be expected that Fermilab would discover a large number of new resonances, of which this appeared to be the first. The group's reaction to this idea was lukewarm.

Although we guarded against leaks to avoid being scooped by SPEAR, many of us were unsure how seriously to take the 6-GeV signal. Opinion within the group was swayed by a brainstorm of Columbia postdoc Jeffrey Weiss's: during a midnight shift he looked closely at the Greek alphabet and the Particle Data Book and realized that about the only letter not already the name of a particle or resonance was upsilon. At a group gathering shortly thereafter on the 11th floor of the "high-rise"[7] he expounded the reasons our signal should be named upsilon:

- upsilon is Greek for "lofty one,"[8] an appropriate name for the highest-mass

[6]In 1974 the Laboratory's name was revised in honor of Enrico Fermi, hence NAL became FNAL or "Fermilab".

[7]The Fermilab "Central Laboratory" building, later named Robert Rathbun Wilson Hall in honor of Fermilab's founding Director.

[8]Actually a bad pun in Greek: the literal meaning of upsilon is "simple y."

resonance yet found; also

- upsilon is "one up on the psi" or
- the "psi of Leon."

Soon thereafter Innes devised what turned out to be the best reason, which he presented at a Fermilab seminar on the *ee*I results:

- Should it prove a statistical fluctuation, it would be the "oops" of Leon!

This settled the matter of the name; the only remaining question was how to name a $2\frac{1}{2}\sigma$ effect without embarrassing ourselves. Lederman devised the footnote which suggested that "the name Υ (upsilon) be given either to the resonance at 6 GeV if confirmed or to the onset of high-mass dilepton physics" [28]. And so was it published in Phys. Rev. Lett. (In the end, the name upsilon was given to neither effect, although the 6-GeV resonance is still occasionally referred to as the "oopsLeon.")

SPEAR promptly searched the 6-GeV region but was unable to confirm our signal [29]. Though it made it much less likely, this did not necessarily rule out a 6-GeV resonance, which might have been some kind of exotic state with a large hadronic production cross section but extremely small electronic decay width. If anyone was to put the last nail in the Υ-6.0's coffin it would have to be we.

3.2 $\mu\mu$I: It Wasn't There Again!

By the end of 1975 we were the proud possessors of the world's largest sample of massive lepton pairs; no other experiment was even close. The problem facing us was how to increase our statistics by the necessary large factor in order to demonstrate definitively what was going on at 6 GeV. Sensitivity in the dielectron mode was limited by the large background flux of hadrons accompanying the electron pairs of interest ($\sim 10^6$ hadron pairs per electron pair). We decided to switch to muon pairs, which would allow the insertion of shielding material between the target and the analyzing magnets. By absorbing most of the the hadrons before they entered our detectors, the shielding would substantially increase the allowable interaction rate in the target. The deleterious effects on mass resolution due to scattering of the muons in this hadron absorber could be minimized by using beryllium rather than the high-Z materials more commonly used.

The group's long experience in the study of muons (extending back at least to the AGS dimuon experiment and including a significant portion of E-70) suggested further improvements in the spectrometer. In the dielectron mode electrons were distinguished from hadrons using lead glass. In the dimuon mode, hadrons were largely eliminated as a background by the 5 m of beryllium hadron absorber inserted in each aperture of the pair spectrometer. But muons themselves represented a more devious background: muons of low momentum could enter the detectors by scattering

around and through the coils and return yokes of the analyzing magnets, in some cases faking high-p_t trajectories and thus contributing fake high-mass pairs. We therefore added redundancy to the muon tracking and identification with a hodoscope in the middle of each analyzing magnet and a steel/scintillator muon detector at the back of each spectrometer arm. In addition we installed in each arm new apparatus for the upcoming dihadron run: two gas Cherenkov counters and a novel water-Cherenkov hadron calorimeter [30]. These helped reject slow muons via a $\approx$ 12-GeV muon-momentum Cherenkov threshold and increased the rejection against any lead-glass "punch-throughs."

With no detectors before the analyzing magnets, scattering of the muons in the hadron absorber degraded the mass resolution by a factor ≈ 2 compared to the 1.3% r.m.s. achieved in *ee*I. But we were able to increase the interaction rate by a factor ≈ 5. With 159 dimuon events between 5.5- and 11-GeV mass (Fig. 5), the oopsLeon was ruled out [31] (unless, as we noted, a significant deviation from μ-e universality was being observed!).[9] Only John Yoh took seriously the cluster of 6 events at 9.5 GeV. In a memo he estimated their statistical significance (combined with the one event from *ee*I) as greater than that of the oopsLeon, and he labeled a bottle of champagne "9.5" and put it in the group's refrigerator in the P-Center PortaKamp.[10,11]

3.3 $(e+h)^2$ (E-494) (Hadronic Interlude)

$\mu\mu$I was followed by the dihadron run, which we called $(e+h)^2$ since it included electron-pair, electron-hadron pair, and hadron-pair triggers all acquired simultaneously. (In an administrative quirk, Goldwasser decided to designate the dihadron trigger with its own experiment number, E-494.) For this run we borrowed from Ting's group two triplets of MWPCs (previously used to discover the J, hence designated the "J chambers") for installation at the exit of the analyzing magnets. By improving the track pointing accuracy into the magnets, they improved the mass

[9]The situation was described at the time by the author as follows:

> Last night I saw with good chi-square
> A particle that wasn't there!
> Nor is today, the data tell,
> At 95% C.L.

This verse was displayed on the PDP-15 on-line computer (of which the author was in charge) and was later amended (see Section 3.4 below).

[10]Our beamline was called Proton Center. Our data-acquisition electronics, on-line computer, refrigerator, coffeepot etc. were located in a cluster of temporary buildings, of a type (larger than a trailer) still used widely at Fermilab and known as a PortaKamp, located next to the (underground) detector buiding (or "pit" – see Section 3.7 below).

[11]If the CDF top-quark signal [32] is confirmed, then John Yoh and Robert Kephart are the only ones to have discovered two quarks. It is amusing to note that the CDF top signal, twelve events with an estimated background of four, is of the same size as the oopsLeon signal, though CDF claims a stronger statistical case since three of the events are doubly tagged.

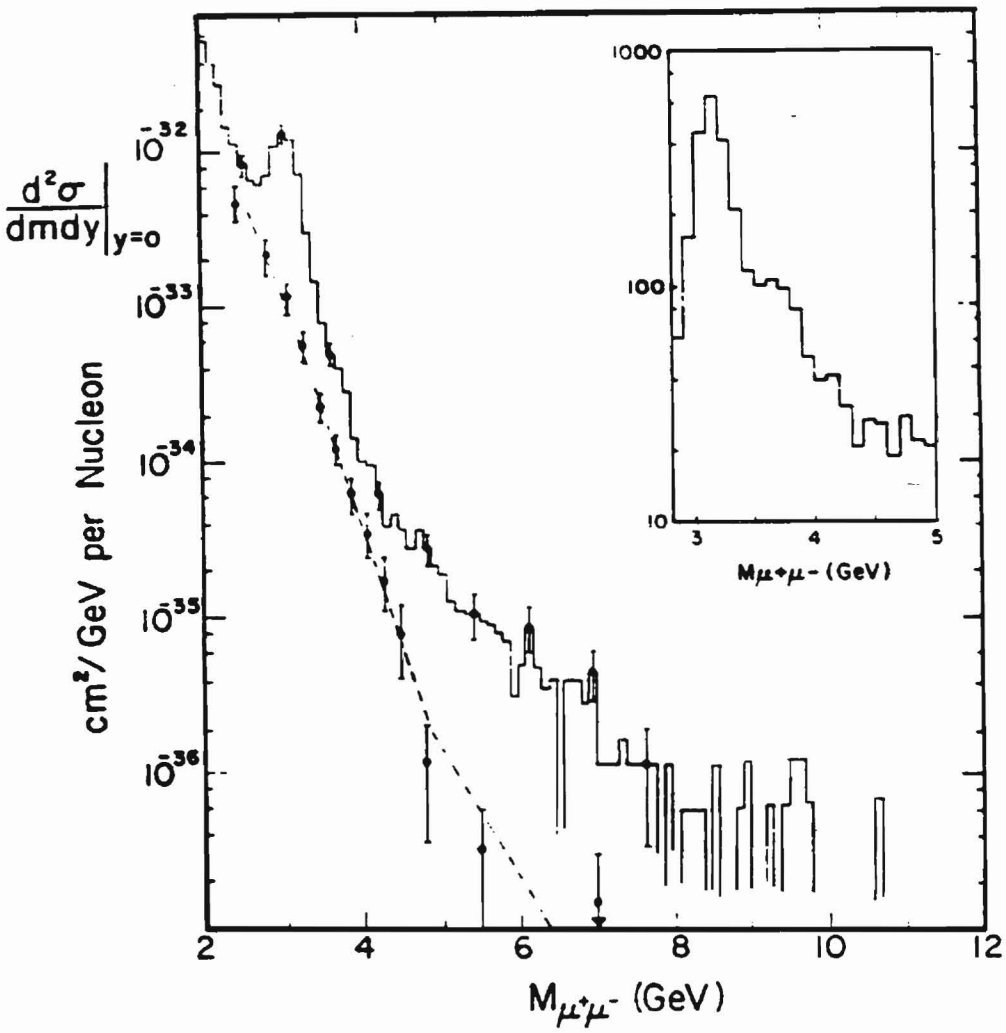

Figure 5: E-288 $\mu\mu$I dimuon yield (from Ref. [31]).

resolution a factor $\approx$ 2 over that of eeI. Since at the location of the J chambers the charged and neutral beams had not yet separated, there had been considerable discussion whether neutral-beam interactions would lead to excessive counting rates. Bud Good of Stony Brook took the initiative in proving that "the neutral beam was a paper tiger" and that the chambers could be successfully operated there with no deleterious side-effects.

Although several significant publications were produced by E-494 [33], they have little bearing on the Υ discovery and will not be discussed here. The Stony Brook group[12] took the lead in planning this run, analyzing the dihadron data, and writing

[12]The author excepted – he had been in residence at Fermilab since summer 1975 (while the rest of the Stony Brook group was home building Cherenkov counters) and functioned as an "honorary" Columbia student.

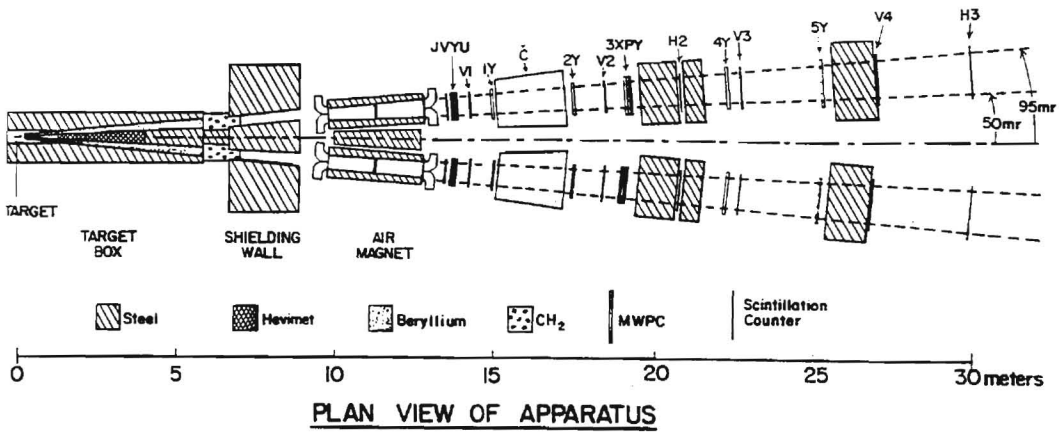

Figure 6: E-288 $\mu\mu$II apparatus.

up the results. In order to concentrate on these tasks, several Stony Brook collaborators chose not to participate in the subsequent $\mu\mu$II run and so were not coauthors on the Υ discovery papers.

3.4 $\mu\mu$II: Three for the Price of One

During the $(e+h)^2$ run we continued preparations (begun already during $\mu\mu$I) for a second dimuon run with vastly improved sensitivity [34]. The key ideas behind the $\mu\mu$II arrangement (Fig. 6) were based on what we had learned in $\mu\mu$I and took full advantage of the elimination of the neutral beam by the hadron absorber. We substantially increased the instrumented solid angle by 1) bringing all detectors closer to the magnets; 2) opening up the aperture-defining collimators upstream of the magnets; and 3) symmetrizing the spectrometer arms vertically so that each was centered on the neutral beam. These improvements were worked out in highly collaborative fashion among the various postdocs and more senior collaborators.[13] Symmetrizing the apertures gained a factor 2 in $\mu^+\mu^-$ acceptance since both positive and negative muons were then accepted in each arm. In addition the acceptance at high masses was substantially increased, since the high-momentum cutoff was eliminated. With a ± 10 mr vertical aperture in each arm, these improvements led to the acceptance *vs.* pair mass shown in Fig. 7; the eeI/$\mu\mu$I acceptance is also indicated for comparison.

We also redesigned the beam dump, shielding, and hadron absorber so as to minimize cracks and gaps as well as scattering of muons into the apertures from the region of most copious flux at small production angles. Since high-Z material close to the aperture would tend to scatter muons in, the beryllium was made substantially oversized with respect to the apertures; it was also ≈ 2 m thicker than in $\mu\mu$I.

[13]Chuck Brown dubbed the process "memo warfare."

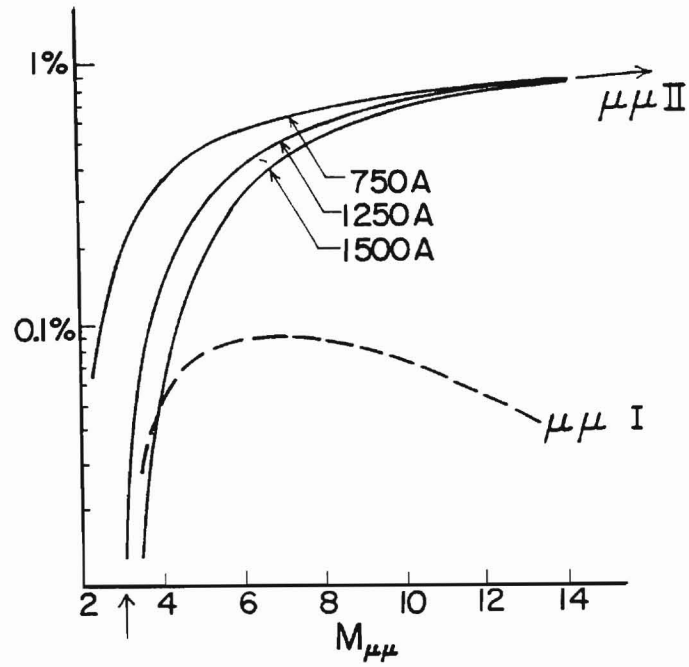

Figure 7: E-288 acceptance comparison.

(The acquisition of the large amount of beryllium needed was nontrivial but was accomplished thanks to Lederman's well-developed "telephone physics" skills.[14]) The redesign was guided by an extensive Monte Carlo study carried out by Columbia postdoc Stephen W. Herb and led to a factor ≈ 10 increase over $\mu\mu$I in interaction-rate capability. Combined with the factor ≈ 10 in acceptance and longer running time, we ultimately acquired three orders of magnitude more high-mass data than in $\mu\mu$I.

In $\mu\mu$I we had observed non-negligible backgrounds at high masses, especially at high pair-p_t where accidental pairs were particularly troublesome. This indicated the need for improved muon identification in $\mu\mu$II, which we achieved by adding a solid-steel magnet in each arm (proposed by Bruce Brown in a May 7, 1976 memo and designed by Fermilab staff scientist Charles N. "Chuck" Brown), followed by MWPCs, for redundant measurement of the muons' momenta. We also improved the trajectory definition by installing an MWPC inside each air-gap analyzing magnet; these were specially designed by Innes for high-rate operation and featured narrow gaps and high-speed electronics,[15] as well as a freon admixture in the gas to limit the duration of the avalanche by absorbing electrons generated $\gtrsim$2 mm from the anode wires.

The redesigned spectrometer was assembled in January and February 1977. After

[14]A humorous account of the search for surplus beryllium for $\mu\mu$II appeared in the January 6, 1977 issue of Fermilab's house newspaper "The Village Crier."

[15]Designed for this application by Columbia University Nevis Laboratories electronics guru F. William Sippach, as was most of the electronics used in E-70 and E-288.

some weeks of tests, studies, and calibrations, some preliminary data runs were taken towards the end of April 1977; steady data-taking commenced in mid-May following a three-week hiatus for accelerator maintenance and studies.[16] Each good day of running was comparable in sensitivity to the entire $\mu\mu$I run, until then the world's largest massive-dilepton sample. John Yoh's aggressive data analysis revealed a small enhancement at 9.5-GeV mass already in his memo of May 2.

Late in the evening of May 21, a recently-installed precision shunt used to monitor the current in the air-gap analyzing magnets overheated, spraying molten metal on its surroundings and starting a fire. The author arrived for his "owl" shift on May 22 to find the parking lot outside P-Center full of fire trucks and the experiment cordoned off. Since burning PVC insulation releases chlorine, the application of water by the firemen created an acidic mist which would eventually have destroyed the thousands of amplifier/discriminators mounted on the wire chambers. Fortunately Lederman was familiar with the consequences of a similar fire at CERN and knew whom to call: a Dutch salvage expert living in Spain, who (a harbinger of the "new Europe") worked for a German company, and was able to come to Fermilab on a few days' notice. The wire chambers were promptly decabled and their electronics dismounted, and an assembly line of of rubber-gloved physicists, technicians, girl friends, secretaries, and anyone else handy dipped each circuit board into the prescribed sequence of secret chemicals, then rinsed and brushed as instructed in order to remove the fire's caustic fallout. (The partially-melted shunt, anodized and mounted on a pedestal by a Fermilab technician, was presented to the Directorate as a souvenir and is still on display on the second floor of Wilson Hall, where it joins a fractured lead-glass block presented by E-70.) The fire cost us about one week of running, but it had a fortunate side effect: it provided us with a "systematics" check of the 9.5-GeV enhancement, which in a June 6 memo by Yoh was shown to be equally significant in both the pre- and post-fire data samples. He estimated the signal's statistical significance in the combined sample to be in the range eight to seventeen standard deviations, depending on how the continuum was modeled.[17]

Extensive additional systematics checks were carried out to ensure that the enhancement was not the result of apparatus or programming artifacts. The verification of statistically significant effects represents (for the most part) a highly conservative aspect of the culture of high energy physics. Ting's group spent several months in this endeavor before announcing the J signal, which may have cost them the primacy of discovery. We were more fortunate in that there was at the time no competitive experiment in our mass range; also (because of our high rate to acquire and analyze these events), we were able to complete the process quite rapidly. Additional data

[16]During which the accelerator ran briefly at 450 GeV before returning to 400 GeV.

[17]At about this time Chuck Brown appended a new ending to the author's "poem" on the PDP-15:

> The Upsilon is still alive,
> But now its mass is 9.5!

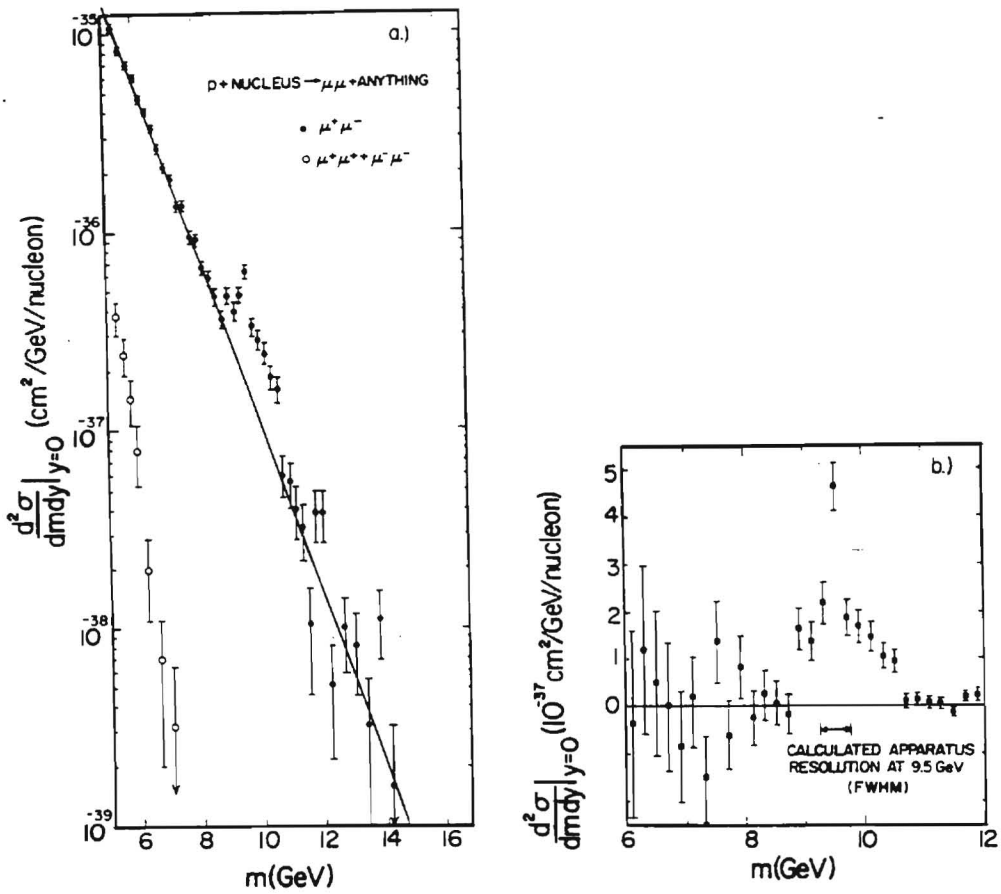

Figure 8: $\mu\mu$II dimuon yield from Herb *et al.* [1]: a) before and b) after continuum subtraction.

were taken at a reduced magnet current and showed a consistent enhancement at 9.5-GeV mass. A sample taken with the target removed contained no event above 6 GeV. In a June 17 memo, Yoh summarized these and other checks carried out by himself, Kephart, Chuck Brown, Herb, Sens, and others (including the author) which established that the signal was not caused by double-counting of events, biases in the apparatus or software, or possible misreconstruction of events from nontarget sources.

Convinced the effect was real, we announced it in a special seminar in Fermilab's Auditorium on June 30. (Seminars were normally held in the smaller "Curia II" meeting room, but a large crowd was anticipated.) Steve Herb gave the talk. A key point was the width of the enhancement (see Fig. 8), significantly wider than the resolution of our apparatus, suggesting that at least two closely-spaced resonances were being observed.

Our excitement and the feverish pace at which we worked are evident from a few dates: Yoh's June 17 memo was followed by a memo on June 20 summarizing the results and reminding us of group meetings scheduled for Wednesday June 22 at noon and Friday June 24 at 7 pm. The paper on the Υ discovery was quickly written, was

received by Phys. Rev. Lett. on July 1, and was published without review in the August 1 issue [1]. It was based on less than two months' data and contained 9000 muon pairs above 5-GeV mass, 300 times the eeI sample, with some 420 Υ events above the continuum. While the paper carefully refrained from interpreting the result, it was quickly followed by a Fermilab press release, including comments from Chuck Brown. Widely quoted in the press, Brown explained "the best guess...is that we've seen the first sign of yet another quark."[18]

We continued taking data, and by the end of August had accumulated 26,000 events above 5 GeV, about 1000 times the eeI sample. With $\approx$ 1200 events above continuum the multiple-resonance interpretation become statistically inescapable, with a single resonance ruled out and three resonances slightly preferred over two (Fig. 9). Another Phys. Rev. Letter was quickly written, received on Sept. 9, and published in the Nov. 14 issue [2]. Operation of the accelerator in a "front-porch" mode[19] provided data on the beam-energy dependence of Υ and dimuon-continuum production [35] (see Fig. 10). Clearly we would not have discovered the Υs had NAL remained at its design energy of 200 GeV.

3.5 Confirmation of the Discovery

Confirmations of the Υ discovery came from experiments at Fermilab, the CERN ISR, and DESY. These were discussed in Lederman's talk at the 1978 Tokyo Conference [4]. They included data taken by the E-439 collaboration at Fermilab at about the same time as ours [36] (Fig. 11), as well as subsequent results from the CCOR [37], ABCSY [38], and CHFMNP [39] collaborations (the first two using electron pairs and the third muons) at the ISR. It is noteworthy that E-439, which used iron magnets, recorded many more events than we, but (in a replay of Lederman's AGS dimuon experience) due to poor mass resolution ($\approx$ 6% r.m.s.) was unable to claim discovery.

The DORIS e^+e^- storage ring at DESY was not designed to reach the 10-GeV mass region, but after the Υ was announced an all-out effort was undertaken to increase its reach. In the fall of 1977 it was converted from two-ring to one-ring operation, and six RF cavities built for the PETRA ring (then under construction) were instead installed in DORIS [40]. A charge-1/3 b quark of uncertain mass is hard

[18]Our favorite version of the story appeared in the August 5 Thunder Bay (Ontario) Chronicle-Journal, in which it was inadvertently intercut with a Tass piece on the discovery of a baby mammoth preserved in the permafrost (*sic*): "A baby mammoth, which ment is that we've seen the first sign of yet another quark...the Upsilon n permafrost, was thawed out indicates that there may be five or more...Upsilon is 10 metre (three feet) long...Each of these collisions pro head, trunk, legs and the duced secondary nuclear fragments..." and so forth.

[19]A mode of operation in which beam was extracted at both the full energy and a lower energy on each acceleration cycle. This required that the current in the accelerator magnets, which "ramped up" as the energy increased, be held constant for a second or so while beam was extracted at the lower energy, before continuing up to its "flat-top" for extraction at full energy. On the real-time display shown on closed-circuit television throughout the Lab, the shape of the ramp then resembled a house with a front porch.

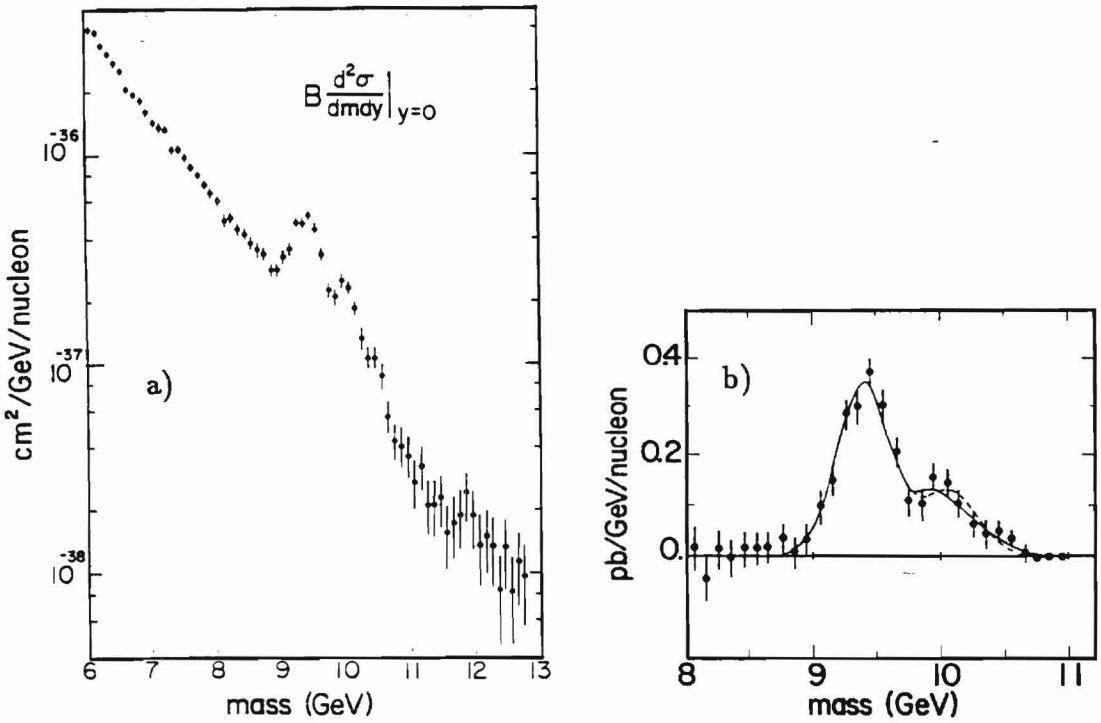

Figure 9: $\mu\mu$II dimuon yield from Innes *et al.* [2]: a) before and b) after continuum subtraction, with solid (dashed) curve showing three- (two-) resonance fit.

to find at an e^+e^- collider, since its fractional contribution to $R \equiv \frac{\sigma(e^+e^-\to\text{hadrons})}{\sigma(e^+e^-\to\mu^+\mu^-)}$ is small. To improve their odds of finding it quickly, the DORIS experimenters asked us for our best estimate of the mass of the Υ. After carefully reviewing our mass calibration, Innes, Chuck Brown, and Jöstlein sketched a distribution of probability *vs.* mass and sent it to DESY; it implied $m_\Upsilon = 9.45 \pm 0.05$ GeV. We were gratified in April 1978 when the DASP-2 and PLUTO collaborations observed the Υ at 9.46 ± 0.01 GeV [41] (Fig. 12). In September, after more RF cavities were added, the Υ' was confirmed as well [42] (Fig. 13).

3.6 $\mu\mu$II'a and b: High-Intensity and High-Resolution Runs

In the meantime we continued accumulating statistics in what we called a "high-intensity" run and a "high-resolution" run: in the high-intensity run a long copper target maximized the yield of good events per background muon,[20] and the trigger was made more selective to maintain acceptable deadtime and more rate-capable by means of transistorized phototube bases [43]. In the high-resolution run we used a beryllium target to minimize multiple scattering, lowered the interaction rate to permit operation of the mid-magnet MWPCs upstream of the magnets, and added

[20]The dominant background was a "thermal muon sea," of typical energy ~ 1 GeV, produced in interactions in the beam dump and "channeled" back and forth between the coils and the flux-return yokes of the air-gap magnets.

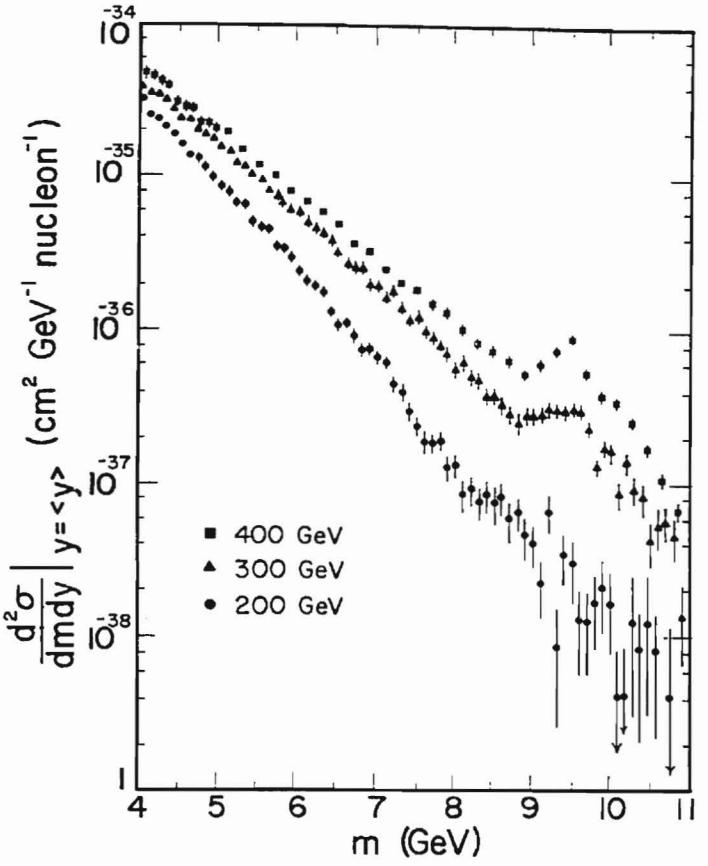

Figure 10: $\mu\mu$II dimuon yield at 200- 300- and 400-GeV beam energies from Yoh *et al.* [35].

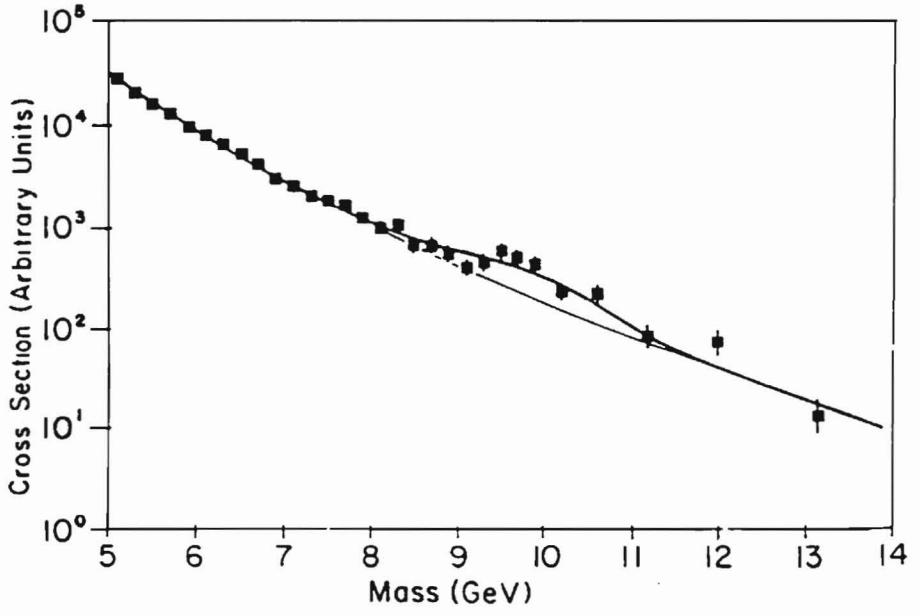

Figure 11: Dimuon yield from E-439 [36].

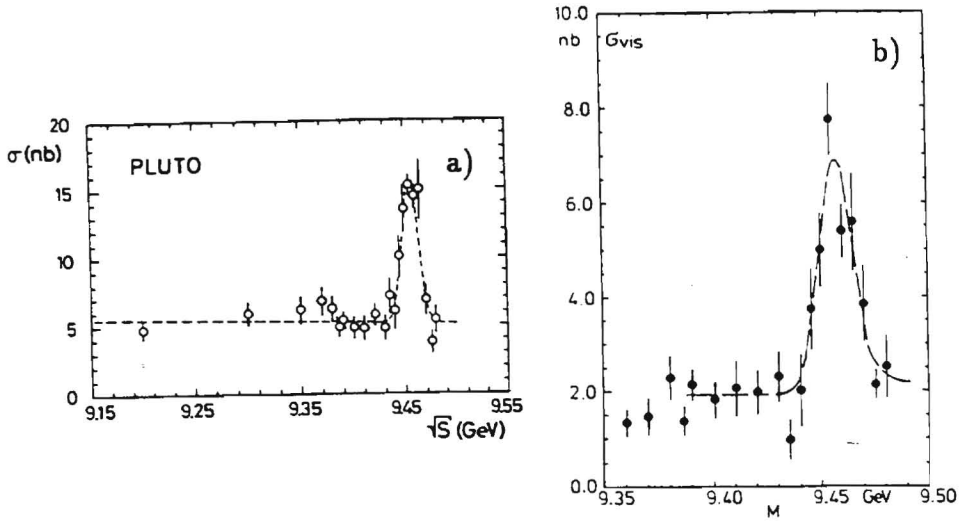

Figure 12: e^+e^- annihilation cross section in the Υ region observed at the DORIS storage ring by a) PLUTO and b) DASP-2 collaborations [41].

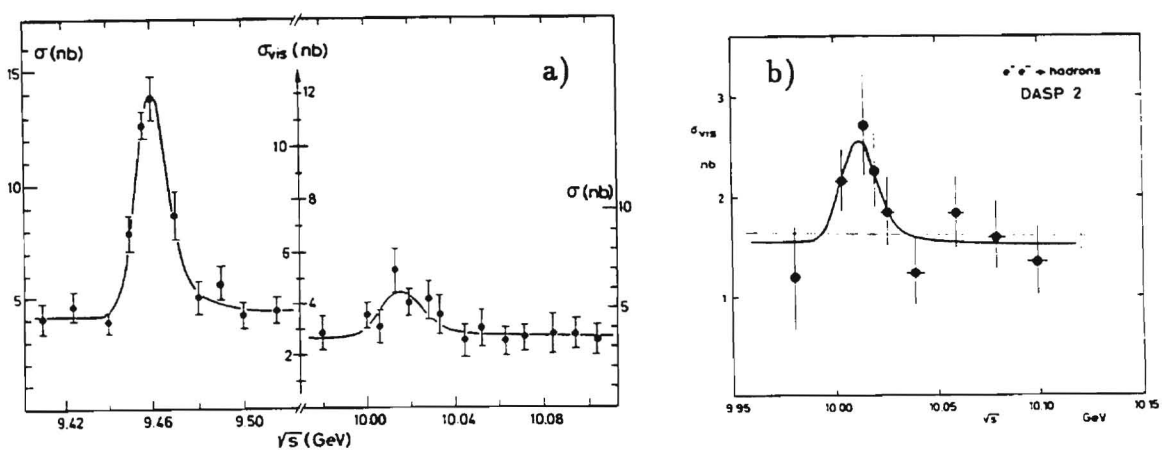

Figure 13: e^+e^- annihilation cross section in the Υ' region observed at the DORIS storage ring by a) DESY/Hamburg/Heidelberg/MPI-Munich and b) DASP-2 collaborations [42].

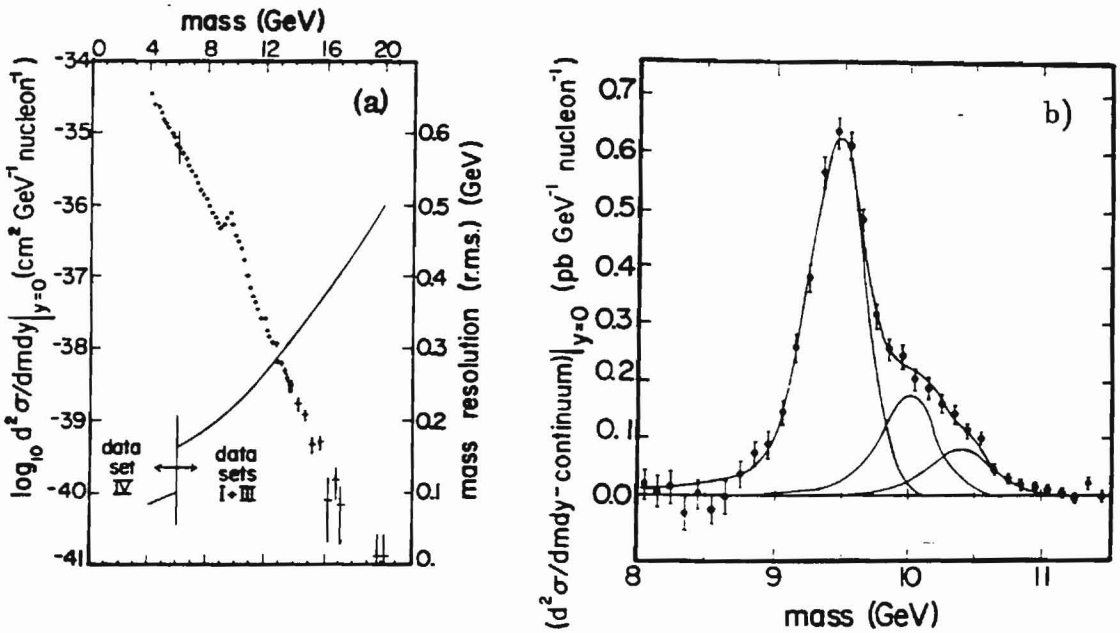

Figure 14: Dimuon yield from $\mu\mu$II high-intensity and high-resolution runs (from Ueno *et al.* [3]): a) before and b) after continuum subtraction.

drift-time measurement to the bend-view MWPC planes in each arm, improving the mass resolution from 2.2% to 1.7% (r.m.s.). With these additional samples (increasing the total Υ sample to 9000 events) and the precise measurement from DORIS of the $\Upsilon' - \Upsilon$ mass splitting, we were able to establish definitively the existence of the Υ'' (statistical significance 11 standard deviations) [3] (Fig. 14). The mass of the Υ'', 10.4 GeV, put it temporarily beyond the reach of DORIS; its observation [44] and that of the Υ''' in e^+e^- annihilation had to await the commissioning of the Cornell Electron Storage Ring (CESR) and the further upgrading of DORIS.

The combination of the Fermilab and DESY measurements strongly favored the "bottomonium" interpretation of the Υs, *i.e.* their identification as bound states of a new b ("bottom" or "beauty") quark with its antiquark. It is difficult in these days of energetically-publishing theorists to discover *anything* not previously predicted by *someone.* The b quark had of course already been postulated (along with top) by Kobayashi and Maskawa [45] as a mechanism for *CP* violation, robbing its discoverers of the opportunity to name it. Furthermore, Kurt Gottfried and Estia Eichten of Cornell [46] had anticipated the discovery of bottomonium, in a paper intended not so much as a prediction of the existence of a 5-GeV quark, but rather to emphasize the physics potential of CESR, then under construction, should such a quark turn out to exist.

3.7 Working Conditions

It is worth commenting on the working conditions at Fermilab during the E-288 period. Wilson encouraged a Spartan approach. Conditions at P-Center were primitive: the pit was basically a large hole in the ground rather than a building, with no crane, corrogated iron walls, and a makeshift roof which leaked whenever it rained. Until the Lab could be convinced to install numerous gutters (*inside* the pit!) to run the water off to the sides, we draped the detectors with plastic for protection. Exposed gravel trenches around the edges of the concrete floor provided for drainage and were inhabited by frogs and snakes.[21] Robert Kephart recalls restacking the last few feet of beryllium hadron absorber in the E-288 target cave to optimize the shielding arrangement, work which had to be performed in a confined space with inadequate lighting and dirty water dripping from the low ceiling (the bottom of a rusty steel shielding pile), with workers frequently rotated due to the proximity of the highly radioactive beam dump. Our PortaKamps were heated mostly by the data-acquisition electronics and cooled by several window air conditioners. Since the air conditioners needed to run even in winter, they frequently iced up, leading to overheating of the electronics and on-line computer. There was no running water. Restrooms were located in the Proton Area Operations Center[22] a hundred meters or so distant: while not a great distance, nevertheless farther than one wanted to walk in wintertime without bundling up. The data were analyzed using an assemblage of obsolete computers,[23] which operated in batch processing mode and were programmed using punched cards, long after most computing installations had moved on to timesharing.

The collaboration bore these difficulties with good spirits and a strong sense of camaraderie. While small in numbers, we were aided by a crack team of technicians and engineers at Columbia[24] and Stony Brook,[25] and ably supported by Fermilab's technical staff.[26] Fermilab in those days was energized by a "can-do" spirit and a determination to overcome all adversity. Bureaucracy was viewed by Wilson as an unnecessary evil, and the staff was imbued with the desire to help visiting scientists in any way possible.

[21] For all of that, we were more fortunate than the E-87 collaboration in P-East: during a particularly heavy storm their sump pumps failed (or were overwhelmed?), and when Columbia University technician Ken Gray made an access the next day he found water up to his knees, upon which floated various scintillation counters.

[22] Built during E-288 and inaccurately dubbed the "pagoda" because of a distinctive central tower reached by a spiral staircase.

[23] Used CDC 6600s acquired from ERDA (U.S. Energy Research and Development Administration, successor agency to the Atomic Energy Commission) surplus. Al Brenner, head of the Computing Department, emphasized the situation with large signs over the doors to the I/O and computer rooms reading "Antique Computer Museum" and "Ye Olde Machine Shoppe."

[24] Herb Cunitz, Ken Gray, Bill Sippach, and Art Timm.

[25] Karen Kephart and Tom Regan.

[26] Especially Frank Pearsall and Jack Upton.

4 E-596: Unstable Beauty

The collaboration naturally considered how to follow up the Υ discovery. Several avenues were taken, including searching for beauty particles[27] and the top quark (bottom's more massive companion) at Fermilab and studying bottomonium at Cornell.

Since the parameters of the third rank of the Cabibbo-Kobayashi-Maskawa quark mixing matrix were then largely unknown, it was not out of the question that beauty particles were stable (this was emphasized to us in a letter to Lederman from Sheldon Glashow). Taiji Yamanouchi reminded us that such massive stable particles could be detected in a quick and simple experiment using differential Cherenkov counters.[28] In a record for Fermilab, E-596 was proposed, carried out (with the help of Richard Vidal, a new Ph.D. fresh from the CCOR group), and published within the span of five months [48]. No stable beauty particles were found, and a 90%-confidence-level upper limit on the lifetime of B particles was set at 5×10^{-8} s.

5 E-605: Bigger and Better

In 1978 a portion of the collaboration (led by Stony Brook's Robert L. McCarthy) proposed a new experiment to improve on E-494's dihadron measurement. Another portion (led by Lederman and Chuck Brown) proposed a new dimuon experiment, in the hope that the top quark might be low enough in mass to be discovered in fixed-target using the new 800-GeV superconducting Tevatron accelerator, whose design and construction were in progress. The Fermilab Program Advisory Committee advised combining the two measurements into a single experiment using a common apparatus. Thus was born E-605, a large mass-focussing spectrometer with several innovative elements (most notably a ring-imaging Cherenkov counter, the first such device to produce published physics [49]). As shown in Fig. 15, the E-605 spectrometer offered an unprecedented combination of rate capability and resolution. However by the first 800-GeV Tevatron run in 1984, PETRA had already established that the top quark was beyond our reach. E-605 thus failed to find it, though it did make significant Drell-Yan and dihadron-continuum measurements. No one at that time suspected the discovery of top would take an additional decade!

[27] "Bare bottom" or "naked beauty," as opposed to the "hidden" beauty of bottomonium, in which the beauty quantum numbers of the b quark and $\bar{b}$ antiquark cancel.

[28] A similar (though less sensitive) experiment, E-187, had been carried out previously by the E-70 collaborators [47].

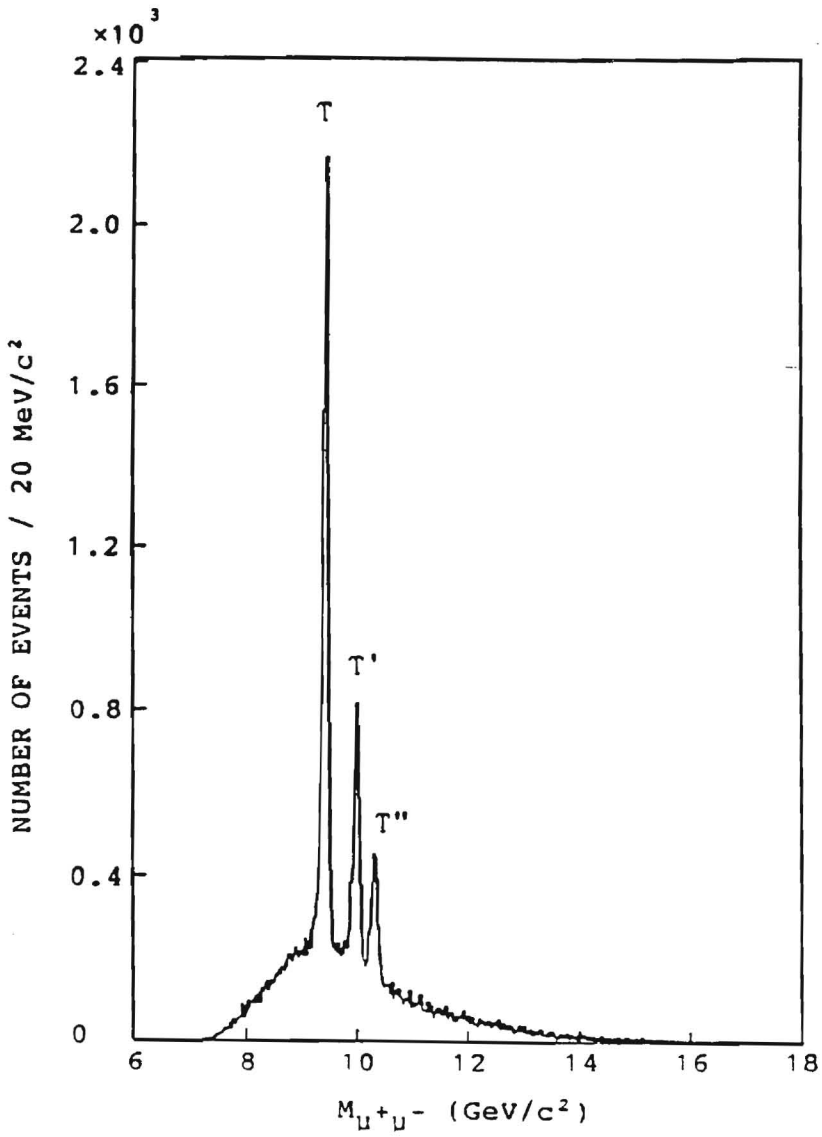

Figure 15: The prettiest Υ spectrum: dimuon yield from the 1985 run of E-605.

6 The CUSB Experiment

Lederman also followed up E-288 by proposing the Columbia University/Stony Brook (CUSB)[29] experiment at CESR. His idea was to complement the general-purpose CLEO spectrometer already under construction with a smaller, less costly, non-magnetic spectrometer optimized for the detection of photons and electrons, which could be used for precise measurements of transitions among bottomonium states. Both the CUSB and CLEO detectors were brought up in time for the first run of CESR. The ensuing discovery of the Υ''' [50], and of its dominant decay into $B\overline{B}$ meson pairs [51], ushered in the present era of detailed beauty studies at "mega-B" sensitivity, carried out largely by CLEO and (until recently) the ARGUS collaboration at DESY. With the coming B factories at SLAC and KEK, the HERA-B experiment at DESY, and the upgraded CDF and D0 detectors at Fermilab, the elucidation of the nature of CP violation through its observation in B decay is widely anticipated [52].

7 Perspectives

The Υ story has many striking aspects. One gathers that experimental discoveries are often "in the air:" several groups may be in position to make a measurement, and if one group had not done it first another would have. This was clearly so in the case of the J/ψ, less so in the case of the Υ, whose confirmation took ≈ 1 year. There is also the role of serendipity: the right experiment might be done for the wrong reasons, while "correct" reasoning may lead to the wrong experiment. Here one can cite the early searches for the W via single leptons, whose critique led to the more productive lepton-pair approach. The "error" of inadequate mass resolution, repeated independently by two groups (Lederman's and the E-439 collaboraton), carried in each case the penalty of failure to discover a quark: first c, then b. In the E-288 case, searching for the W and the Lee-Wick boson in fixed-target was in retrospect certainly the wrong reason to do the right experiment, as was the top-quark search for E-605.

The Υ discovery was the most important result to emerge from Fermilab during its first two decades. Wilson's role in pushing the accelerator beyond its design limitations was crucial: due to the strong energy dependence of the Υ cross section, we would have missed the discovery at 200 GeV. The NAL accelerator in fact was upgraded even while it was being commissioned, turning on in 1973 at 300 GeV and running at 400 GeV during most of E-288.

[29]Largely a different Columbia/Stony Brook group than that of E-288 and E-605, including (Columbia) Herb, Kaplan, Lederman, and Yoh from E-288 but also Profs. Paolo Franzini and (Stony Brook) Juliet Lee-Franzini and Guido Finnochiaro and their groups, and eventually groups from Louisiana State University and Max-Planck-Institute, Munich as well. Since Lederman shortly assumed the Fermilab directorship (following Wilson's resignation in 1978), he ultimately took little part in the CUSB experiment.

The contrast between Fermilab in the 1970s and the 1990s is striking. Key to the Υ discovery was the opportunity to learn from our mistakes. Repeated upgrades of the initial E-288 apparatus over a period of just three years gave four orders of magnitude improvement in sensitivity. Nowadays, with fixed-target runs occurring approximately every five years, such an effort would require more than a decade, and might not even be undertaken due to the daunting prospect. The opportunity to learn from one's mistakes now seems hardly to exist – one must get it right the first time, which is far more difficult. As a consequence, experimenters tend to "play it safe," reducing the chances of failure, but also of finding some truly surprising phenomenon which could reshape our picture of the subatomic world.

8 Acknowledgements

I thank Prof. Leon Lederman for suggesting my participation, my colleagues of the Columbia/Fermilab/Stony Brook collaboration who read and commented on the manuscript, and the organizers and sponsors, Profs. Harvey Newman, Antonino Zichichi, and Tom Ypsilantis, for their generous hospitality and for a most stimulating conference held in remarkable and gracious surroundings.

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