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A Life in High-Energy Physics: Success Beyond Expectations

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physics, CP violation

Abstract

The author describes in some technical detail his career in experimental particle physics. It began in 1955, when he joined Brookhaven National Laboratory, and ended in 1985, when he moved to the field of cosmic-ray physics. The author discusses not only his successes but also his failures and his bad judgments. This period was the golden age of particle physics, when the experimental possibilities were abundant and one could carry out experiments with a small team of colleagues and students.

Contents

1. BEGINNINGS	2
2. MY THESIS	4
3. MY FIRST JOB	5
4. APPOINTMENT AS ASSISTANT PROFESSOR AT PRINCETON	8
5. SPARK CHAMBERS	10
6. THE DISCOVERY OF CP VIOLATION	12
7. A YEAR IN FRANCE	16
8. STRUGGLE TO MEASURE $K_L \rightarrow \pi^0\pi^0$	18
9. MOVE TO THE UNIVERSITY OF CHICAGO: $K_L \rightarrow \mu^+ + \mu^-$	23
10. EXPERIMENTS AT FERMILAB	27
11. THE NOBEL PRIZE	43
12. MY LAST PARTICLE PHYSICS EXPERIMENT	45
13. CONCLUDING REMARKS	46

1. BEGINNINGS

My father was the first of his immigrant working-class family to go to college, and what's more, he earned a PhD in classical languages from the University of Chicago in 1936. My mother met my father in an undergraduate Latin class at Northwestern University. She came from an upper-middle-class family. Her father was a member of the Chicago Board of Trade. I was born on September 29, 1931, as my father began his graduate studies. This period was in the depths of the Great Depression. After graduation, my father found a position at Judson College, a college for women in Marion, Alabama. It was a far cry from the academic position he expected. However, he was not alone. Many of his faculty colleagues had PhDs from major universities, and the intellectual level at Judson was very high. After World War II, many achieved prominent positions at distinguished universities. I had a thorough exposure to rural poverty and Jim Crow, but as a young boy I was insensitive and played with both white and black kids, who all seemed rather carefree. In September 1939, my father took a position as professor of classical languages at Southern Methodist University (SMU), where he remained for the rest of his life. My father had an excessive respect for an academic career, and he certainly had that expectation for me. I was a conscientious student who went to the public schools in the Dallas suburb of University Park. These schools had very high academic standards. Many students were forced to repeat a class if they did not have passing grades. It was a terrible indignity if one failed to move from junior high to high school.

I always had an interest in technology and enjoyed the physics class during my senior year at high school. The class was taught by Mr. C.H. Marshall. The most memorable parts of the course were the projects he assigned, which were important supplements to the standard lab exercises. One was to build an electric motor—something that spun when a 6-V battery was applied. It was up to the student to find the materials and make the construction. More dramatic was a project to make a step-down transformer from 115 V ac to outputs of 12 V, 6 V, and 3 V with a sizable output power. Most of us went to a radio repair shop and bought a discarded transformer shell that had been part of the radio power supply. We wound the primary and secondary with discarded wire, carefully counting the turns. The exam had Mr. Marshall measuring the output to check the voltage, and most students passed. However, many students began with the shell of a small speaker

transformer. When the load test was applied, there was smoke and often fire! It was a memorable lesson on the need to pay attention to power requirements. I cannot say that my interest in physics was due only to my high school teacher, but he certainly encouraged it. In high school, the popular physics book that was the most influential was *One, Two, Three... Infinity* by George Gamow. A prominent feature was the Lorentz transformation of special relativity, the derivation of which I was able to follow.

My father's position as a professor at SMU allowed me to attend the university without tuition. There were no resources to allow the undergraduate school of my choice. So I lived at home and managed to complete the undergraduate program in three years. My father pushed me very hard, but I enjoyed what I was doing and never thought of rebelling. My original plan was to major in engineering, but my father persuaded me to major in physics. My first scientific paper (1) was published while I was an undergraduate. My close friend and fellow undergraduate, Lawrence Curtis, was an amateur herpetologist who worked part time at the Dallas aquarium. The aquarium obtained two electric eels. I placed probes in the water to observe the discharges with my Heathkit® oscilloscope. In December 1950, Lawrence and I gave a live demonstration of the electric eel to the Texas Academy of Science, which was meeting at SMU. Our paper summarized the knowledge about the eel gained from published sources, and the originality was only in the demonstration.

The physics curriculum at SMU was the equivalent of only the first two undergraduate years of a research institution such as the University of Chicago. But SMU was strong in the humanities, and that part of my education was excellent. In retrospect, I am grateful that I did not have to endure the intense undergraduate physics courses taught at Princeton or Chicago. I am not sure that I would have been capable of learning so fast. There was one exception to the weak physics program at SMU. Professor H. Wayne Rudmose, newly arrived from Harvard and a specialist in acoustics, taught a course in electronics (vacuum tubes at the time). This was hands-on work with power supplies, amplifiers, cathode followers, and so on. Rudmose subsequently left SMU to form the electronics firm Tracor. At SMU I discovered that I loved to produce and analyze numerical data no matter what the source. My reports in the undergraduate labs were far more detailed than expected.

By my junior year, I had decided to apply for graduate study in physics. I applied to all the major places and was accepted by all of them. But the only institutions that offered a teaching assistantship were the University of Chicago and the Rice Institute. I chose without hesitation the University of Chicago as it was the institution where my father had studied. My physics education at SMU was not sophisticated or modern, so I did not fully appreciate the extraordinary faculty that Chicago had in 1951, led by Enrico Fermi. I registered for graduate study on September 29, 1951, my twentieth birthday (and Fermi's fiftieth). I found that I was not well prepared, and I had to audit upper-class undergraduate courses as well as register for the graduate classes.

Among my fellow students were many brilliant ones who possessed a far superior knowledge of physics than I did. We were bathed in an environment of intense physics that engendered in most of us a lifelong passion for the subject. I had teachers who were at the forefront of modern physics research. Among my teachers were Fermi, Edward Teller, Maria Goeppert-Mayer, Edward Adams, Gregor Wenzel, Murray Gell-Mann, Val Telegdi, and Subrahmanyan Chandrasekhar. Walking the halls of the Institute for Nuclear Studies were chemists Joseph Mayer, Willard Libby, and Harold Urey. In the early 1950s, there was probably no other institution in the world that could match Chicago. My favorite course was Thermodynamics and Statistical Mechanics, taught by Fermi over two quarters in my first year. Fermi stressed applications that included the statistical atom and stellar interiors. I had a huge amount to learn and was fearful that I would not make it. In the spring I had to take a qualifying exam, required after the first year, which consisted mostly of undergraduate physics. In the winter of 1952, the National Science

Foundation introduced its fellowship program, and we were all ordered to apply. By some miracle I was awarded one of these fellowships! This bolstered my confidence, and I went on to pass the qualifying exam in the spring and the rigorous basic exam required for PhD studies in the winter of 1953.

By June 1952, I had been educating myself continuously, including during summers, since the beginning of high school. I had had enough! I spent the summer working on an oil exploration crew in New Mexico. It was my mini rebellion. This experience and the memories of the rural poverty in Alabama gave me an appreciation of my good luck to find a profession that I loved. The summer experience in the oil fields made me aware of the diversity of peoples' lives and the imperfections of our society.

2. MY THESIS

In the 1950s, Chicago had the rules that a thesis had to have a single author and that it had to be published. It was natural to wish to be sponsored by Fermi. Fermi seemed to be guarded by some tough senior graduate students, who put me off. But a close student colleague, Jerry Friedman, went directly to Fermi and was accepted. In any case, I sought out a sponsor who was warm and not in competition with his distinguished colleagues. This sponsor was Samuel K. Allison, who had worked with Arthur Compton in X-rays before the war and was prominent in the atomic bomb project at Los Alamos. His working tool was a 400-KeV Cockcroft-Walton machine, and he started me on little projects to learn some fundamental techniques: vacuum pumps, counting techniques, target production by evaporation techniques, and the analysis of the products produced when a target was bombarded with 400-KeV protons. Allison used electrostatic analyzers rather than the more common magnetic ones. His pride was a spherical analyzer that had just been commissioned. Allison's research had been nuclear physics, but when I showed up he was beginning new research measuring electron capture and loss cross sections when atoms passed through a gas. These measurements had great practical value for the use of negative hydrogen ions for tandem Van de Graaff accelerators and injection into plasma machines.

By the fall of 1953, getting used to the lab with Allison's equipment came to an end, and I found a thesis project, the study of the reaction $B^{10}(p,\alpha)Be^7$. I was pleased that I came up with this idea on my own. Being aware that there existed a cylindrical electrostatic analyzer permitted a major improvement to the prior work by Lauritsen at Cal Tech (2). The magnetic rigidities of the scattered protons and those of the α particles produced in the interaction with B^{10} are very similar. In a magnetic spectrometer, protons and α particles follow similar trajectories. In an electrostatic spectrometer, the protons would require twice the voltage of the α particles to be accepted, so there was no background problem with my technique. I do not remember if it was Allison who pointed out this advantage; I cannot imagine, with my lack of experience at the time, that I consciously realized the great advantage of the electrostatic analyzer for this particular problem.

The proton beam was provided by a 2-MeV Van de Graaff accelerator built by the High Voltage Engineering Corporation. This machine belonged to the Committee on Radio Biology and was intended to bombard the nuclei of living cells with a highly collimated beam of protons, a feat I do not think was ever achieved. The machine on a good day could barely reach 1.7 MeV, and the highest-energy measurements were made with a beam of 1.63 MeV. The title of the thesis was "Excitation functions and angular distributions of alpha particles leading to the ground and first excited states of Be^7 in the reaction $B^{10}(p,\alpha)Be^7$ " (3). I produced the basic design of the apparatus and had it built in the shop. I kept the accelerator running with the help of a part-time technician. There were always problems with vacuum leaks, detected by ether and solved by glyptol.

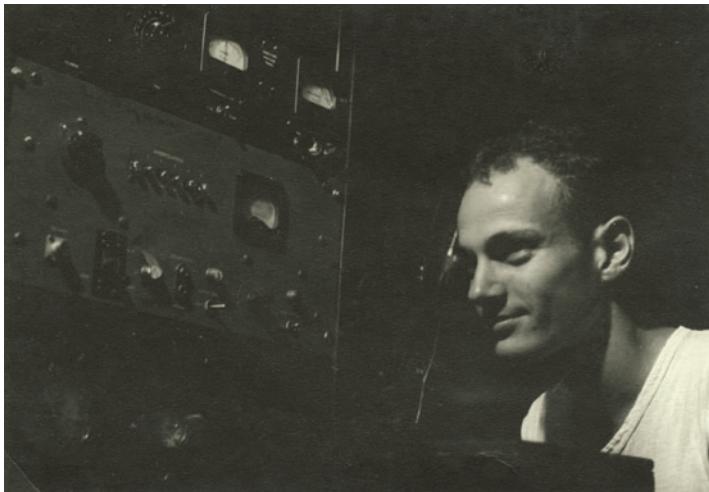


Figure 1

Cronin at the control of his thesis experiment.

In the summer of 1953, I was pleasantly distracted by meeting my wife-to-be, Annette Martin. We were married on September 11, 1954. Annette and I visited my parents in Texas at Christmas 1954. When I returned, I found that Allison had built the proportional counter that was used to detect the α particles. I was never sure whether his motivation was to shame me into working harder or just to give a helping hand. A photo of me with my electronics rack is shown in **Figure 1**.

My thesis was published in *Physical Review* (3). The result of my measurements concerned mainly the spin and parity of two states of the compound nucleus C¹¹ at energies of 9.7 MeV and 10.06 MeV, to which I made the assignments 3/2⁻ and 7/2⁺. These assignments agreed with a review by A.M. Lane published in 1960 (4). The thesis had a total of 13 citations, the latest being in 2004!

3. MY FIRST JOB

In the spring of 1955, it became clear that I would receive my PhD at graduation in August. It was time to look for a job. During that epoch, many physics graduates considered industry for employment. I was interviewed by General Electric (GE) in Ohio to work on a nuclear-powered airplane and in Schenectady for the GE Research Lab. Simultaneously I applied for a postdoctoral position either at Los Alamos or at Brookhaven. Val Telegdi encouraged me to remain in fundamental research, in particular the emerging field of elementary particles, which was just entering its golden age. Telegdi had followed my work as a student, and I am sure he wrote a strong letter on my behalf, as did Allison. I was offered the Brookhaven job and accepted. The 3-GeV Cosmotron had just begun operation. I was assigned to work in the experimental group led by Oreste Piccioni and Rodney Cool. In September 1955, I moved to Brookhaven with Annette and a new baby.

Piccioni was away at Berkeley to work on the detection of antiprotons. Most of my work at Brookhaven was with Cool. At the time, he and Piccioni had just completed the measurement of the total cross section of π^+ and π^- on protons (5). Cool's next project was to be the measurement of the absorption and diffractive cross sections of pions on nuclei ranging from carbon to lead.

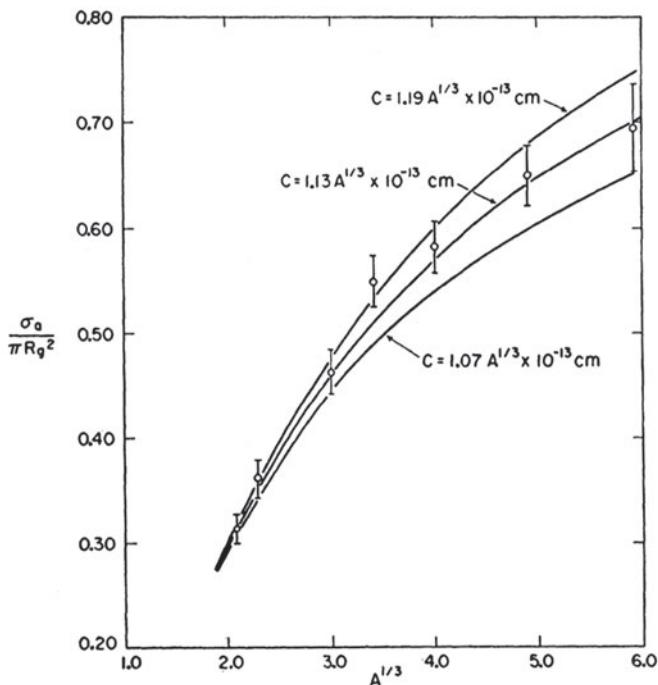


Figure 2

A comparison between the experimental absorption cross sections and the Fermi tapered model. Modified with permission from Reference 6.

We were joined by a Johns Hopkins student of Leon Madansky named Al Abashian. There were only small changes to the pion beam from the Cosmotron. The positive pions were well separated from the protons by time of flight. As long as the Cosmotron ran and we were assigned time, data taking for the nuclear cross sections went very well.

I had found in my character a strong desire to contribute significantly to any project in which I was involved. There was nothing in the way of apparatus to build, so I concentrated on the analysis, using the optical model. This was before computers were readily available. Each weeknight I took home a Marchant calculator, and during several months I worked slowly on the numerical integrations required. I was in contact with Robert Williams at MIT, who was very familiar with the optical model. I evaluated the data with both a square well model and a Fermi shape: $\rho(r) = \rho_0 / [\exp[(r - c)/z] + 1]$, with the width $z = 0.53 \times 10^{-13}$. The value of z was taken from the fits to the electron scattering data. The Fermi distribution fit the data very well (Figure 2). Rodney Cool treated me very well as a young colleague, and perhaps his placing my name as first author of the resulting paper (6) was an expression of his respect.

During the course of these measurements with Abashian and Cool, we made special runs to examine whether in lead the neutron radius was different from the proton radius. Johnson & Teller (7) had proposed a model for heavy nuclei in which the mean radius for neutrons was larger than for protons. Piccioni had suggested that this neutron excess could be measured. At 700 MeV, the $\pi^- p$ cross section is 2.6 times the $\pi^+ p$ cross section, so if the neutron radius were larger than the proton radius, the ratio $q = [\sigma(\pi^-) - \sigma(\pi^+)/\sigma(\pi^+)]$ would be negative, assuming charge symmetry. The factor q , calculated by an optical model, shifts by 6% for a radial difference of 1 fermi. The measured shift suggested a slightly larger proton radius of a few percent and was

in essential agreement with equal radii. This paper was published in the Letters to the Editor of *Physical Review* (8).

I played a minor role in a measurement of the reaction $\pi^+ + p \rightarrow K^+ + \Sigma^+$ by the University of Michigan group led by Donald Glaser. This group placed a small propane bubble chamber in the pion beam we had used for the cross-section measurements described above. I measured for them the pion-to-proton ratio in the positive beam. The protons produced no background for the selected reaction, but it was necessary to know the pion fraction to evaluate the cross section. At the time, this reaction had not been observed. I went to Michigan for a few weeks to help analyze the photos. This was my first experience scanning and measuring photographs, and I found it boring and exhausting. Of course, the scanning was done by professionals, but I vowed not to do experiments requiring scanning and measuring photographs. I was to violate this pledge only a few years later. The reaction was observed, and it was reported in a short Letter to the Editor of *Physical Review* (9).

While I was at Michigan, parity violation was discovered in the β decay of polarized Co⁶⁰ nuclei by C.S. Wu and experts on the polarization of nuclei at the National Bureau of Standards (10). This was a dramatic discovery, and at Michigan a seminar was quickly organized. Prominent participants included George Uhlenbeck and Joaquin Luttinger. I remember Luttinger rushing out of the room muttering “two neutrinos.” It was said that he had some idea how the phenomenon occurred and raced to his desk to calculate!

This discovery led me to the first experiments in which I felt I had a creative role. Before the parity-violation discovery, I had been developing a detector for K^+ mesons that had a large solid angle so it could identify these mesons as secondary particles from a reaction. The goal was to measure the production of K^+ mesons in pion–proton collisions—not a very inspired experiment. Immediately after the discovery of parity violation, it became important to measure the violation in as many weak decays as possible. In pion–proton collisions, the detection of a K^+ meson was a signature for the production of a hyperon. My K^+ detector was just what was needed. The sign of the pion beam and the target proton or neutron (via deuterium) permitted the strangeness –1 hyperons to be selectively produced.

A K^+ meson can be recognized by the fact that its lifetime for decay to pions or muons is 12 ns. The K mesons were brought to rest in a large water-filled Cherenkov detector. There, they produced no pulse, as their velocity was selected to be below the Cherenkov threshold. This selection was done by defining the entrance of a charged particle to the large volume by the requirement of a larger-than-minimum dE/dx in scintillation counters and the passage through a water Cherenkov counter in anticoincidence. However, most of the decay products of the stopped K meson are above Cherenkov threshold and produce a delayed count. One innovation for the Cherenkov counter was the introduction of a wavelength shifter, which enhanced the detectable light and reradiated the light isotropically. The idea was successfully tested, and experiments to measure parity violation in hyperon decay were planned. **Figure 3** shows a typical plot of the delayed decay pulses as observed in the actual experiment.

As the experimental design progressed, the Cosmotron magnet short-circuited and burned. This was a disaster for the Cosmotron program but a stroke of luck for me. Cool arranged for us to move the experiment to the Berkeley Bevatron, where we began a collaboration in the spring and summer of 1958 with two outstanding physicists, Bill Wenzel and Bruce Cork. These two had no fear of building a large experiment with many scintillation and Cherenkov counters and with whatever electronics were required. In those days, the electronics consisted of vacuum tubes and diode coincidence circuits. But the most important principle was that if one were to look for an asymmetry, then the apparatus should be physically symmetric. This is obvious, but the apparatus we were thinking of at Brookhaven did not have this property, and we thought we could correct

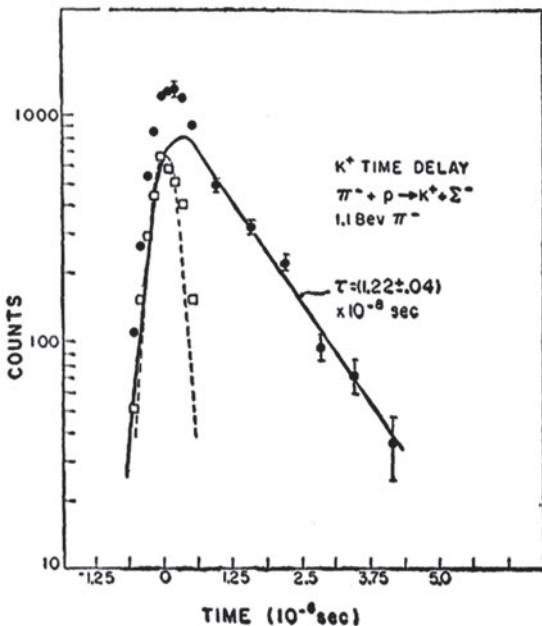


Figure 3

A typical delay plot produced by the K^+ detector described in the text. Modified with permission from Reference 11.

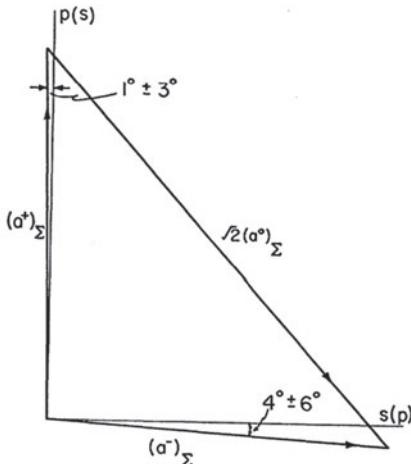
for known physical asymmetries. At best, one could make a correction, but at the price of larger errors. I learned very much from Cork and Wenzel and am forever grateful that I had the chance to work with them during my young and formative years.

With the K^+ meson detectors and arrays of counters we were able to study the parity-violating asymmetries in the decays. The results of this experiment were significant but had large errors. The decay modes studied were $\Sigma^- \rightarrow \pi^- + n$, $\Sigma^+ \rightarrow \pi^+ + n$, and $\Sigma^+ \rightarrow \pi^0 + p$. The π^0 decay mode was measured by the addition of lead sheets in front of the counters that normally counted only the π^+ decay mode. Then one measured a combination of the two Σ^+ decay modes. To measure the asymmetry of the π^0 decay mode, a subtraction and a correction were required.

The beauty of working with these more senior physicists was that I was left alone to analyze the data! Of course, they carefully reviewed my work, but I had the immense pleasure of performing the analysis on my own. This was before the days of the common use of Monte Carlo, and one had to be resourceful to evaluate the efficiencies and solid angles. No significant asymmetries were found for the decay modes $\Sigma^- \rightarrow \pi^- + n$ and $\Sigma^+ \rightarrow \pi^+ + n$. However, for the decay mode $\Sigma^+ \rightarrow \pi^0 + p$ there was an asymmetry parameter, $\alpha P = 0.70 \pm 0.30$ (11), where P is the polarization produced perpendicular to the production plane of the reaction and α is the up-down asymmetry with respect to the production plane for a fully polarized hyperon. Although the statistical significance of the π^0 asymmetry was weak, the result was suggestive.

4. APPOINTMENT AS ASSISTANT PROFESSOR AT PRINCETON

By the spring of 1958, I had been at Brookhaven for more than two years. Oreste Piccioni returned from Berkeley, and my early interactions with him were acrimonious. I decided I had to leave



Experimental determination of the orientation of the Σ^\pm decay amplitudes for $|\Delta I| = \frac{1}{2}$.

Figure 4

A fit of the Σ hyperon asymmetry data to the $\Delta I = 1/2$ rule. Modified with permission from Reference 12.

Brookhaven. Piccioni did me a great favor in forcing me to consider academic positions. I am sure I would have been sufficiently appreciated at Brookhaven and might have spent my whole career there. In later years, I came to admire Piccioni as an outstanding physicist for both his work during the war with Conversi and Pancini and his work on regeneration of neutral K mesons.

In the late 1950s, university physics departments were growing, so many good positions were available. I was asked to give seminars at a number of universities, including Pennsylvania, Carnegie Tech, and Wisconsin. It was unfortunate that Rod Cool had already given seminars on the only experiment for which I could claim a significant contribution, the measurement and analysis of the pion–nucleus cross sections. The only subject I could talk about was the K^+ detector, which had never been used in an experiment. Fortunately I had another entré. In its early days, the Cosmotron was a fickle machine. When a run was scheduled to begin at 7 PM, it usually got going at about midnight. On many of these evenings I played bridge with Val Fitch and his students, and I got to know him well. Of course, we talked about the experiments we were doing. Val was an extraordinarily innovative physicist with a talent for electronics. Val must have had a good impression of me as he was responsible for my receiving an offer of an assistant professorship at Princeton. I did also receive an offer from Penn, but I chose Princeton, which I joined in the fall of 1958.

In the summer of 1959, our team at Berkeley, with the addition of Roy Kerth, repeated the Σ hyperon asymmetry experiment with a much-improved apparatus that identified the π^+ and π^0 decays separately (12). The experiment also used a deuterium target to measure the Λ^0 decays via the reaction $\pi^+ + n \rightarrow K^+ + \Lambda^0$. The results confirmed the large asymmetry for $\Sigma^+ \rightarrow \pi^0 + p$. The asymmetry results were $\alpha P = 0.75 \pm 0.17$ for $\Sigma^+ \rightarrow \pi^0 + p$ and $\alpha P = 0.03 \pm 0.08$ for $\Sigma^+ \rightarrow \pi^+ + n$. The pattern of the Σ asymmetries, very small for the $\pi^\pm + n$ modes and large for the $\pi^0 + p$ mode, is predicted by the $\Delta I = 1/2$ rule. The experimental fit to the $\Delta I = 1/2$ rule is shown in Figure 4.

The results for the Λ^0 decay asymmetries were $\alpha P = 0.55 \pm 0.06$ for $\Lambda^0 \rightarrow \pi^- + p$ and $\alpha P = 0.60 \pm 0.13$ for $\Lambda^0 \rightarrow \pi^0 + n$. The $\Delta I = 1/2$ rule predicts the equality of the two

asymmetries. This was the last of the hyperon experiments that used totally electronic detectors without the assistance of visual techniques. I was gratified to see that the idea of my K^+ detector was subsequently used by other experimenters (13).

During the winter and spring of 1959, I made a brief contribution to the phenomenology of $\pi^\pm p$ scattering. I used the pion–nucleon dispersion relations to evaluate the forward scattering amplitudes. I was exposed to the genesis of the dispersion relations as a graduate student. The office of Reinhard Oehme, a postdoc, was next to mine at Chicago. I overheard the intense discussions between Reinhard and Marvin Goldberger when they were working on their classic paper on dispersion relations (14).

My motivation was twofold: first, to evaluate the real as well as imaginary potential in the optical model analysis of pion–nucleus cross sections, and second, to learn how to use digital computers. There was an IBM 650 computer available, and to do the dispersion relation integrations required scheduling the computer from midnight to 8:00 AM. There was a programing language that preceded Fortran. The most interesting result was the prediction of the forward scattering amplitude in charge-exchange scattering: $\pi^- + p \rightarrow \pi^0 + n$. The results, which were published in *Physical Review* (15), produced very little interest.

5. SPARK CHAMBERS

While at Berkeley in the summer of 1959, my attention turned to a new detector described by the Japanese physicists Fukui & Miyamoto (16). They described a device made of parallel conducting plates filled with a noble gas. When a charged particle, signaled by scintillation counters, passed through the device, a high-voltage pulse was applied across alternate plates. With high efficiency, sparks formed along the track—bright and easily photographed. Fukui & Miyamoto (16) called their device a discharge chamber. A technical detail was that their plates were covered with a thin insulator so that a spark from one region would not rob the possibility of a spark from another region. I and others omitted the insulating layer and called it a spark chamber.

When I returned to Princeton in the fall, I was eager to build such a chamber. I built a four-gap chamber housed in a bell jar filled with neon. When scintillators indicated the passage of a particle, a 10-kV high-voltage pulse was applied to the plates with a 5C22 thyratron. It worked perfectly! It was natural that one added low voltage across the plates to clear out all the ions created by the spark. In studying the properties of the clearing field, I discovered that ~ 100 V made the chamber totally inefficient if the high-voltage pulse was applied with a delay of 1 μ s. This was an exciting discovery because it meant that the spark chamber could be placed in a beam of 10^6 particles s^{-1} , out of which a specific event could be selected. The spark chamber could be used to display the spatial character (say, a hyperon decay) of a single event, which occurred in an intense beam. At the time, many people were working on spark chambers and made similar observations; I claimed no originality for my spark chamber work, but I was very successful in using the technique in several experiments.

Following my initial studies, I designed a spark chamber with 18 gaps (**Figure 5**). We placed the chamber in a Berkeley test beam, and it performed very well. **Figure 6** shows a track of a scattered proton. A full account of our study of the 18-gap spark chamber was presented to the International Conference on Instrumentation for High Energy Physics in Berkeley in the summer of 1960 (17). Over the next several years, we published a number of papers concerning spark chamber properties (18). Our work was honored by an invitation to give the plenary talk on spark chambers at the 1962 International Conference on Instrumentation held at CERN (19). During the 1960s, most of my experiments used spark chambers. I continued to photograph the sparks long after more elaborate data readouts were established. Spark chambers could be built

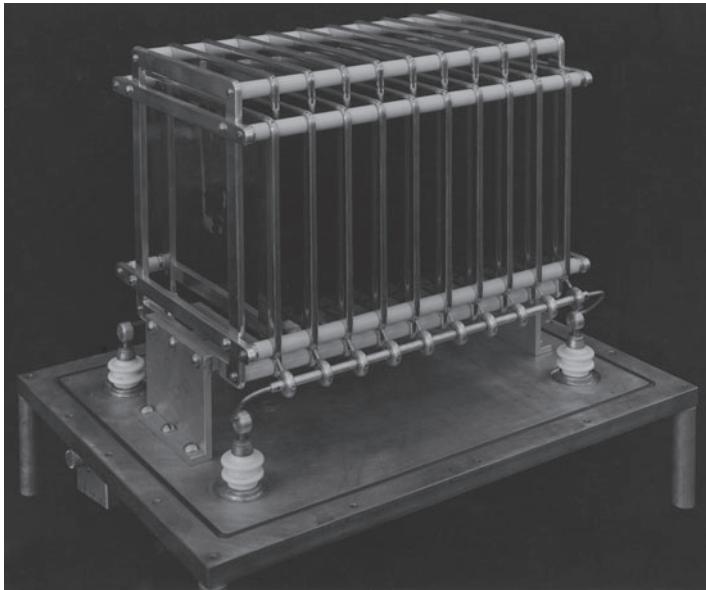


Figure 5

The 18-gap spark chamber.

in very large sizes with large masses. The most sensational and earliest application using these properties was the detection of accelerator-produced neutrinos, which led to the conclusion that there was a difference between neutrinos produced in β decay and those produced in muon decay (20). I had been pleased that Mel Schwartz and his colleagues paid a visit to me at Princeton to learn about spark chambers for the design of their neutrino experiment.

For my own experiments, I chose applications in which there was high selectivity of the desired events by the scintillation counter arrays, which minimized the number of photographs that had to be manually scanned. Automatic scanning came a bit later. An attractive application was to measure the polarization of protons by scattering on carbon plates. We made several trips to the Rochester cyclotron, where a 90%-polarized 200-MeV proton beam was available for calibration. The first experiment we carried out with spark chambers required minor modifications of the 18-gap chamber described above. It was a measurement of the spin correlation coefficients C_{nn} and C_{kp} in proton–proton scattering (21). The experiment was the thesis of my first student, Eugene Engels.

For the second spark chamber application, we returned to Λ^0 decay, which we had studied only with scintillation counters. The objective was to measure the α parameter for the decay asymmetry, which is the longitudinal polarization of the decay proton in the Λ^0 rest frame. The Λ^0 hyperons were produced in the reaction $\pi^- + p \rightarrow \Lambda^0 + K^0$.

A simple apparatus employing three spark chambers was built (Figure 7). The trigger was the recognition of an ionization gap with the production of the Λ^0 hyperon and the K^0 meson, which typically traveled ~ 10 cm before decaying. Only four scintillator counters and a single coincidence–anticoincidence circuit were required. The first chamber defined the direction of the incoming pion and the production point in a 1/4-inch polyethylene slab. The second chamber of thin foils captured the decay. The third chamber, filled with carbon plates, measured the decay proton polarization when a scatter occurred in the plates. This was the ideal application of a spark chamber. A very rare event was picked out of an intense beam, and the geometry of the tracks of production and decay was revealed. Approximately 33% of the triggers yielded a Λ^0 . Of these,

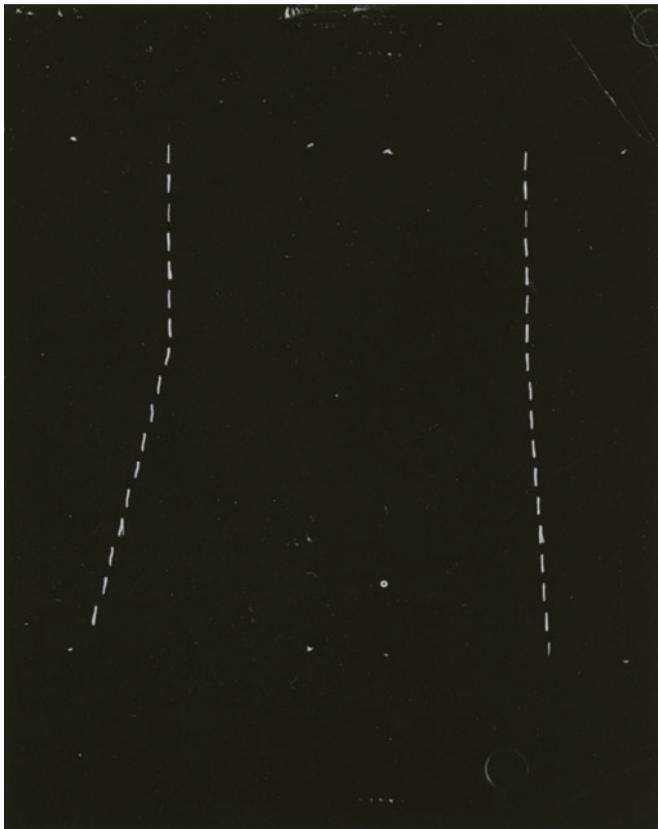


Figure 6

View of a scattered proton in the 18-gap chamber. The proton beam enters from above. Two orthogonal views of the same proton are shown.

~2% had a useful scatter in the carbon-plate chamber. A two-week run produced 1,156 useful events. One of these events is shown in **Figure 8**. The decay proton had a positive helicity and a polarization of 0.62 ± 0.07 (22). The experiment was repeated at the Princeton–Pennsylvania Accelerator (PPA) with the same apparatus by my colleagues Oliver Overseth and Richard Roth, and produced 10 times the number of events. The accuracy was improved, and the longitudinal proton polarization in Λ decay was 0.65 ± 0.02 (23). As a by-product of the Λ^0 polarization experiment, the lifetimes of the Λ^0 hyperon and the K^0 meson were measured (24).

6. THE DISCOVERY OF CP VIOLATION

Following my work on hyperon decay and the use of spark chambers for the detection of polarization, I had to find a new research direction. In strong interactions, a prominent feature was the ρ^0 meson, centered at 750 MeV with a width of 130 MeV. It decayed to $\pi^+ + \pi^-$. There were questions about whether there were other resonances in the region and whether there was a sharp structure in the spectrum due to $\rho - \omega$ interference. To investigate these claims, we built a precision spectrometer to observe the reaction $\pi^- + p \rightarrow \pi^+ + \pi^- + n$ at low momentum transfer. The reaction took place in a 4-foot-long hydrogen target. The pions were selected with nearly symmetric momentum over a mass range between 500 and 1,000 MeV. The dipion mass

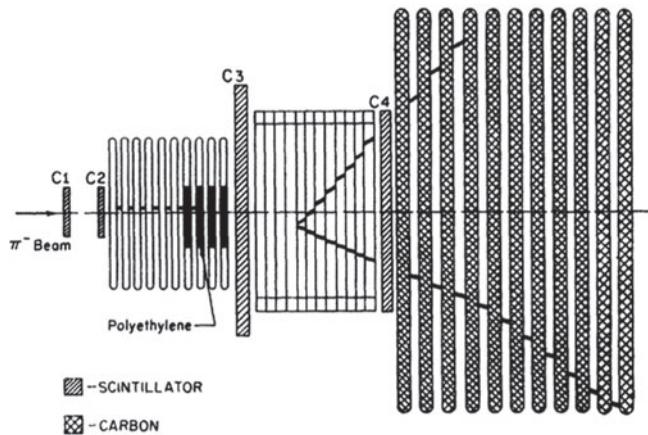


Figure 7

Arrangement of spark chambers and scintillators to study asymmetries and proton polarization in Λ^0 decay.
Modified with permission from Reference 22.

resolution was ~ 3.5 MeV. The results of the ρ study were published in a long paper (25) with no surprising results. This experiment was not one I was proud of, but the spectrometer we built was a magnificent device that was ultimately put to good use.

The 1950s were years in which the physics of the elementary particles, first identified in cosmic rays, flourished. Among the most exciting revelations was that there was a long-lived, neutral K meson (now called the K_L meson) in addition to the familiar, short-lived $K \rightarrow \pi^+ + \pi^-$ (now called the K_S meson). The mesons with definite lifetime are linear combinations of a neutral K meson and its antiparticle, \bar{K} . These combinations are $K_S = 1/\sqrt{2}(K + \bar{K})$ and $K_L = 1/\sqrt{2}(K - \bar{K})$,

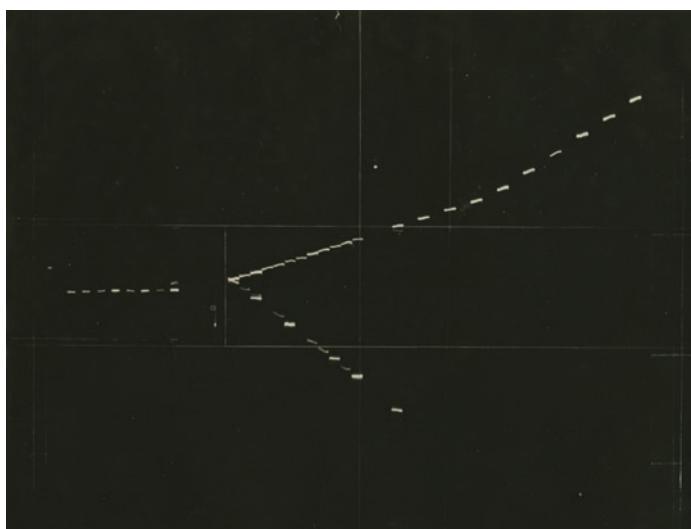


Figure 8

Photograph of a Λ^0 decay with a scattered proton, taken with the arrangement shown in Figure 7. Modified with permission from Reference 22.

which are CP even and CP odd, respectively. The 2π decays are CP even. The CP -odd state can decay to three bodies only and, consequently, has a longer lifetime. An observation of a 2π decay of a K_L meson would indicate a violation of CP . This prediction was made in a remarkable paper by Gell-Mann & Pais (26). This neutral K meson system had another remarkable property: When a K_L meson passed through material, K_S mesons were regenerated. This phenomenon was described in an equally remarkable paper by Pais & Piccioni (27).

Robert Adair is one of the most imaginative physicists I have known. With colleagues, he had performed a study of regeneration of K_L mesons into K_S mesons in a small hydrogen bubble chamber (28). He found an anomalously large regeneration of K_S mesons in the hydrogen, which he interpreted as a new long-range force. The strength of regeneration depends on the forward scattering amplitudes. A weak long-range force that differentiates between a K meson and a \bar{K} meson can be detected only by regeneration. My colleague Val Fitch suggested that the spectrometer I had built be used to confirm the Adair interpretation. The spectrometer, with its high resolution and long hydrogen target, was ideal. The mass resolution was much better than that of the small bubble chamber, and the rate of the anomalous regenerated events was expected to be large. Once we decided to propose the experiment to the Brookhaven Alternating Gradient Synchrotron (AGS), it was natural to propose studies of K meson regeneration and to lower the limit of the CP -forbidden decay $K_L \rightarrow \pi^+ + \pi^-$, which in 1963 stood at no observed 2π events in 300 K_L decays (29).

The spectrometer consisted of two magnets mounted vertically, with the magnetic field horizontal. Thin foil spark chambers were mounted in front of and behind each magnet. The front chambers measured the incident pion directions, and the rear chambers measured the magnetic deflection. The decay particles passed through particle detectors only after their momenta had been measured by the spark chambers. The experimental arrangement is shown in **Figure 9**. The experimental run consumed a total of four weeks from June through July 1963. One week was dedicated to getting the beam and apparatus to function properly. Two weeks were devoted to regeneration in solids and regeneration in hydrogen (the Adair effect). One week was devoted to the search for CP -violating pion decays. During this week, the decay volume was a helium bag (the poor man's vacuum). We did not confirm the Adair effect with the hydrogen run. However, the vacuum decays turned out to be very interesting. We did not expect any events consistent with 2π decay, so no immediate attention was paid to the helium bag data. However, René Turlay took it

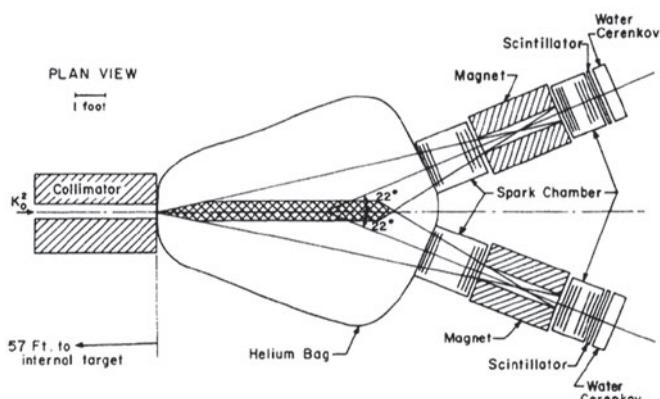


Figure 9

Experimental arrangement for the CP -violation experiment. Modified with permission from Reference 30.

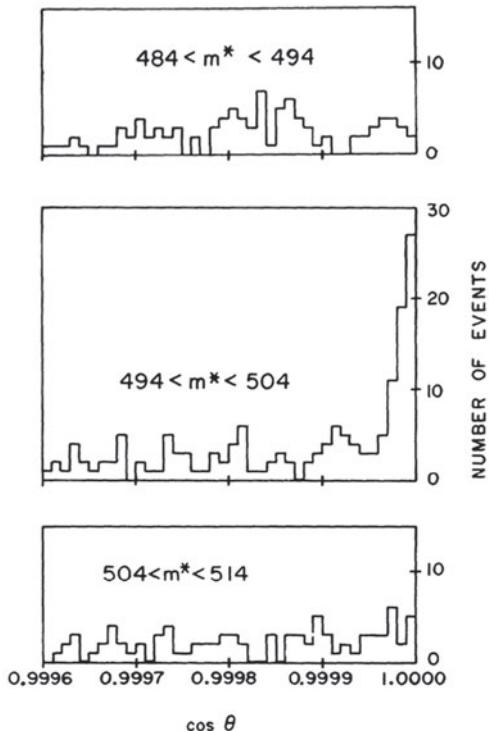


Figure 10

Precision analysis of K_L decay data, showing evidence of $K_L \rightarrow \pi^+ + \pi^-$, where m^* is the effective mass of the two charged particles, assuming each is a pion. Modified with permission from Reference 30.

upon himself to look at the helium (vacuum) decay data. By Christmas 1963, he showed us rather convincing evidence that the K_L meson was decaying to two pions with a branching fraction of $\sim 2 \times 10^{-3}$. We immediately remeasured René's events with a precision bubble chamber measuring machine that we had purchased. The results of this measurement were dramatic (Figure 10). For a vacuum decay, the direction of the vector sum of the 2π momenta must lie in the direction of the beam. Plots are made of the cosine of the angle to the beam for mass bin 494–504 MeV and the two side bands. A clear peak in the beam direction is observed only for the bin at the K_L meson mass. This mass was identical to the mass of the K_S meson regenerated in the same apparatus. For practical purposes here, K_L and K_S have the same mass. Independently, we had determined with regenerated events that the K_L mass was 499 MeV.

We wrote a short paper for *Physical Review Letters* that was published in July 1964 (30). This paper immediately caused a sensation because no one had expected such a violation, and so small at that! In 1958, when parity violation was discovered, the two-neutrino hypothesis quickly accounted for most of the phenomena, although I wonder to this day why nature allows only left-handed neutrinos. We were very confident in our result that the K_L meson decayed to two pions. A few weeks later, a publication by an Illinois group offered some confirmation (31). That group had measured some of the properties of the three-body decays of K_L mesons. On informally hearing of our result, they reanalyzed their data and found some supporting evidence for it. Six months passed before a new experiment at Nimrod in the United Kingdom (32) confirmed our result.

Our paper on CP violation was published just before the International Conference on High Energy Physics, which was held in Dubna in the Soviet Union at the end of July 1964. In some sense, I was lucky to have been viewed as a junior partner in a major discovery. At the conference, Val Fitch gave the formal talk on the CP experiment. He was allowed some 40 min to speak and perhaps 10 min for questions. Our Soviet hosts then asked me to spend an entire afternoon answering every question concerning the minutest details of the experiment. Our evidence was so strong that there was no difficulty convincing the large audience that we had done a good job. A Soviet colleague suggested that the events had been regenerated in a uniform fashion by a mosquito trapped in the helium bag. This was said in jest as the session broke up at the end of the afternoon. It was a thrilling afternoon. But I must add a comment: It was Val Fitch who suggested the use of the spectrometer to check the Adair effect; I am not sure I would have come up with the idea. A clever misinterpretation of an experiment (Adair), a brilliant idea for the application of the spectrometer (Fitch), and the existence of a fine instrument (Cronin) combined to produce a great discovery! It was a fascinating example of how science advances.

In addition to the CP -violation discovery published in 1964, there were extensive results confirming the predictions of K_L -to- K_S regeneration and the details of our lack of confirmation of the Adair anomalous regeneration. These results were all published in a long paper in *Physical Review* (33). It is worth commenting that the regeneration phase in hydrogen and the CP -violating phase produced a constructive interference, yielding approximately four times the events expected. This fact, and fluctuations of low statistics, probably accounts for the results obtained by Adair and his colleagues.

7. A YEAR IN FRANCE

Not only was René Turlay an excellent physics colleague but, as time passed, he also became a close personal friend. He spent the academic years 1962–1964 working with me in Princeton. I had arranged to take a sabbatical to France to work with him in 1964–1965 at the Saclay laboratory. The discovery of CP violation made me consider canceling my sabbatical and remaining in Princeton to exploit that discovery. However, I decided not to change my plan—a decision I have never regretted. Given our experience with K_L mesons and the production of neutral beams from accelerators, René and I proposed the measurement of the details of the three-body K decays $\pi\mu\nu$, $\pi e\nu$, and $\pi\pi\pi$ using the 3-GeV Saturne accelerator at Saclay.

I made my first trip to France in the spring of 1964 to arrange the design and construction of the magnets for our proposed experiments. This was a very important trip not only from a scientific point of view but also from a cultural point of view. I had assumed that it was not really necessary to learn French well because all the physicists spoke English. While I was on this journey, I met a Hungarian physicist, Janos Kirz, who argued that it was essential to learn French, not only to interact with the engineers and technicians in developing the hardware for the experiment but also to properly appreciate the people and culture of the country. I was convinced, and spent three hours each day at Alliance Française for three months learning French. My young daughters attended a French school, and my wife followed a course at Alliance. We had arrived in September, and by Christmas we were all reasonably fluent in French.

At Saturne, we made a neutral beam at an angle of 90° to the internal beam. The mean momentum of the K_L beam was 250 MeV/ c . At this low momentum, pions and muons from the K_L decay could be separated by range, and electrons could be identified by showering or by the production of sparks beyond the range of a pion or a muon.

Figure 11 shows the apparatus that was assembled in the late spring of 1965. The particles were observed decaying transverse to the beam. Thin foil spark chambers placed before and after

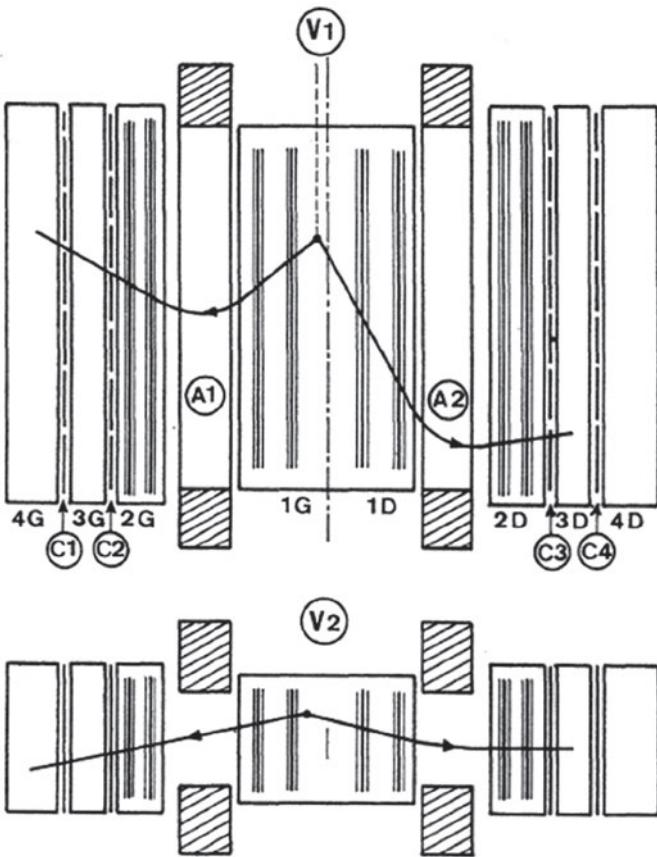


Figure 11

Drawing of the apparatus built at Saclay to study K_L decays to $\pi\nu\bar{\nu}$, $\pi\mu\nu$, and $\pi\pi\pi$. Modified with permission from Reference 35.

long, thin magnets measured the direction and momentum of the decay products. The particle identification was made by measuring the range with steel- and brass-plate chambers. The trigger required at least one of the two particles to penetrate a minimum of 20 g cm^{-2} of steel.

While the experimental equipment was being built at Saclay, I fulfilled a commitment to write an article on spark chambers that consisted of a single chapter in a two-volume book edited by Ralph Shutt titled *Bubble and Spark Chambers* (34). The need for only a single chapter dramatized the simplicity of spark chambers that could be built by almost anyone. Bubble chambers that were extremely complex and made with liquid hydrogen were potentially dangerous instruments. The two instruments were complementary in their application, and the value of the results obtained was in proportion to their complexity.

Once built, the Saclay experiment required several years to complete. Working with the low-energy beam and low-energy decay products proved very difficult. Having the direction measurement and the momentum measurement in the same plane coupled the geometric and momentum errors. For leptonic decays, the reconstruction was a zero-constraint fit with two solutions, depending on the direction of the neutrino in the rest system of the K_L meson—either forward or backward with respect to the beam. There were three papers published from this experiment.

The first (35), and most successful, was a study of the decay $K_L \rightarrow \pi\nu$. It verified that the decay interaction was vector (as other experiments had done) and that the form factor depending on the four-momentum transfer between the K meson and the pion was the same as for the decay $K^+ \rightarrow \pi^0 e^+ \nu$. The second paper (36) concerned the decay $K_L \rightarrow \pi^+ \pi^- \pi^0$. With a sample of 2,500 events, the modulation from uniformity in the Dalitz plot was measured for each pion. The results showed agreement with $\Delta I = 1/2$ and no asymmetry between the charged-pion distributions. A third long paper on the study of $K_L \rightarrow \pi\mu\nu$ was published in *Physical Review* (37). In a few years, all these measurements had been succeeded by similar measurements with huge statistics. These were by-products of the massive effort that was being expended to study CP violation.

I felt bad to have spent such an effort on a statistically limited experiment. From that point on, I told myself and my colleagues that I risked disappointment if I tried to measure anything to a precision better than 10%. Fortunately, René, in collaboration with Jack Steinberger, went on to do beautiful measurements on the decay $K_L \rightarrow \pi\pi\nu$ and on neutrino physics. For many years, René was the director of high-energy physics at Saclay. Sadly, he died in 2002. But my love for France remained, and later in my career I returned to work with French colleagues in a much more successful endeavor that I shall write about in another article.

8. STRUGGLE TO MEASURE $K_L \rightarrow \pi^0 \pi^0$

There was a rush of speculative papers that had as their goal the desire to save CP conservation. In most cases, the cure was worse than the disease. There were, however, two papers that were worthy of consideration. Wu & Yang (38) did a careful analysis of what measurements were required to further understand the violation. They also established a common notation for the needed measurements. A second paper, by Wolfenstein (39), proposed that CP violation was caused by a “superweak” interaction whose only consequence was to produce a small, CP -even admixture ϵ in the predominantly CP -odd K_L decay. Were the Wolfenstein conjecture to be true, it would be nearly impossible to observe any phenomenon with K_L mesons beyond the original effect. Wu & Yang (38) pointed out that there was a second CP -violation parameter ϵ' , which was related to a direct violation of CP in the K_L decay. The consequence of the second parameter was to produce a difference in the CP -violating rates of $K_L \rightarrow \pi^+ \pi^-$ and $K_L \rightarrow \pi^0 \pi^0$. The observation of the second parameter would be crucial to make progress.

Wu & Yang (38) defined the amplitude ratios

$$\begin{aligned}\eta_{+-} &= \text{amp}(K_L \rightarrow \pi^+ \pi^-)/\text{amp}(K_S \rightarrow \pi^+ \pi^-) \text{ and} \\ \eta_{00} &= \text{amp}(K_L \rightarrow \pi^0 \pi^0)/\text{amp}(K_S \rightarrow \pi^0 \pi^0).\end{aligned}$$

In terms of these ratios,

$$|\epsilon'|/|\epsilon| = [1 - (|\eta_{00}|/|\eta_{+-}|)^2]/6.$$

The deviation from unity of $(|\eta_{00}|/|\eta_{+-}|)^2$ was a direct measurement of the second parameter ϵ' .

The most important measurement to eliminate all the speculation was to demonstrate that the pions from K_L decays were the same as those from K_S . This measurement was first demonstrated by Val Fitch and colleagues (40), who showed that the K_L pions coherently interfere with those from regenerated K_S . Following that confirmation, most of the speculations were eliminated. It seemed to me (and many others) that the essential experiment was to measure the ratio $K_L \rightarrow \pi^0 \pi^0/K_S \rightarrow \pi^0 \pi^0$ and determine whether it differed from the corresponding $\pi^+ \pi^-$ ratio.

I began to think about how to measure the rate of the $K_L \rightarrow \pi^0 \pi^0$ decay. As the particles in both the initial and final states are neutral, some wag referred to the decay as “nothing in and nothing out.” The measurement was indeed a challenge because the decay $K_L \rightarrow \pi^0 \pi^0 \pi^0$ was more than

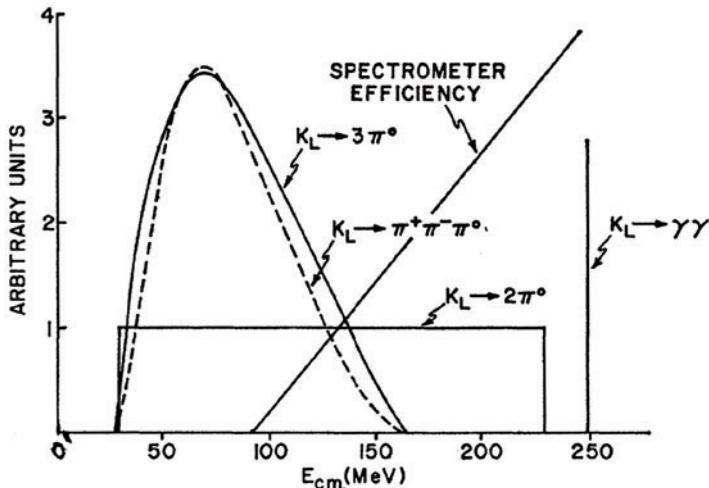


Figure 12

γ -Ray energy spectrum in the center of mass of a K_L decay. Modified with permission from Reference 47.

100 times the expected $2\pi^0$ rate. I searched for some aspect of the $2\pi^0$ decay that could be cleanly selected. I doubted that a brute-force method was appropriate. I found a feature that could be a unique identifier for the $2\pi^0$ decay: the energy spectrum of the γ -rays from the π^0 decays in the K_L center of mass (**Figure 12**). The energy spectrum of the γ -rays extends well beyond the $3\pi^0$ cutoff at 165 MeV. If one could measure the energy spectrum of γ -rays from the decay of a K_L meson at rest, one could select the desired decays. The PPA provided that possibility. The PPA produced a 1-ns-wide proton bunch every 67 ns. The energy of the K_L decay could be measured by time of flight, so the laboratory energy of a γ -ray could be transformed to the center of mass. It was also apparent that the transverse momentum (p_T) was an equally good selector—just not as efficient.

We designed an experiment at the PPA to measure the γ -ray spectrum. The apparatus is shown in **Figure 13**. A unique feature was the incorporation of a wide-gap spark chamber to follow the tracks of the electron and positron following the γ -ray conversion. The chamber was far enough into the fringe field of the magnet so that the sign of tracks could be exactly determined before entering the magnet. As a consequence, errors due to the scattering of the lead converter were eliminated. **Figure 14** is a photograph of an event showing the observable curvature of the electron and positron in the fringe field of the magnet.

In retrospect, it was bold, yet foolish, to think that one could make the desired measurement by the observation of a single γ -ray. Although the observation of γ energies above the $3\pi^0$ cutoff would certainly contain the desired events, there was no guarantee that there were no additional background γ -rays. However, we were eager to run the experiment without further constraints. The spectrum we observed is shown in **Figure 15**. We were impressed by how clean this spectrum appeared. There was a clear peak for $K_L \rightarrow \gamma\gamma$ at the right energy, and the events in the $\pi^0\pi^0$ shelf seemed to terminate at the proper place. The energy resolution of the γ -ray was explicitly shown by the $\gamma\gamma$ peak. These results were mesmerizing, and we convinced ourselves that the background in the shelf was small even though the neutron flux in the beam and its halo was $\sim 1,000$ times the K_L flux! A series of reasonable tests convinced us that the background was not significant. Our quantitative result was $(K_L \rightarrow 2\pi^0)/(K_L \rightarrow 3\pi^0) = (19 \pm 3) \times 10^{-3}$. The results were published in *Physical Review Letters* (41).

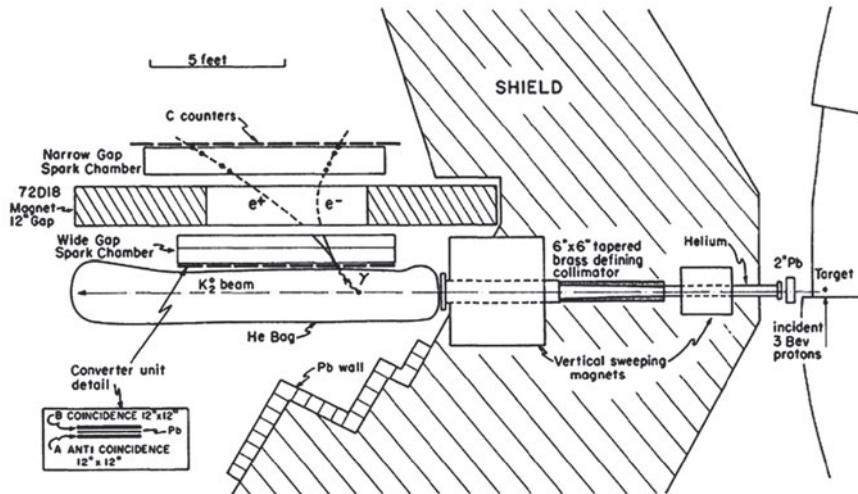


Figure 13

Spectrometer at the PPA for the measurement of the γ -ray spectrum from K_L decay. Modified with permission from Reference 41.

The result of the experiment indicated that $|\eta_{00}| = (4.9 \pm 0.5) \times 10^{-3}$, which was definitely different from $|\eta_{+-}| = (2.0 \pm 0.1) \times 10^{-3}$, the current value for the CP -violating charged decay. It seemed that the entire CP violation was due to the second parameter ϵ' . Of course, we had found a very exciting result. I was asked by the American Physical Society to give a press conference, but something in me said “Don’t do it!” and I refused.

In the fall of 1967, a second physicist from Saclay, Marcel Banner, joined me just in time for the improved PPA experiment with students John Liu and Jim Pilcher. To confirm the result, and hopefully to measure other properties of the neutral decay, we surrounded the decay volume on three sides with massive spark chambers made with stainless-steel plates to detect the other γ -rays accompanying the one detected in our pair spectrometer. We did find the measurement

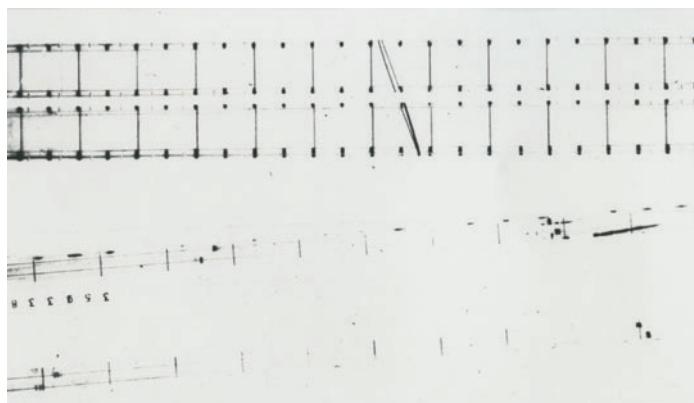


Figure 14

Event in the pair spectrometer. The charge of each member of the pair is revealed by the observed deflection in the fringe field of the magnet. The wide-gap chamber is inverted.

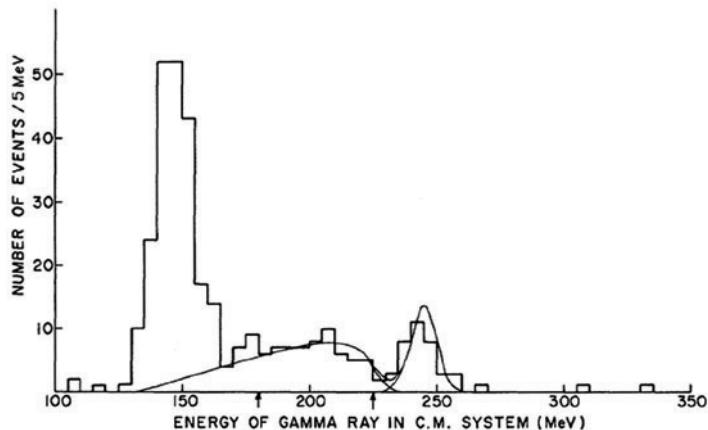


Figure 15

Measured γ -ray spectrum from K_L mesons at rest. Modified with permission from Reference 41.

of the single- γ -ray energy or p_T and an effective way to separate $2\pi^0$ decays from the prolific $3\pi^0$ decays.

A contemporary experiment at CERN (42) had found $|\eta_{00}|^2 = (23 \pm 9) \times 10^{-6}$. In these terms, our result was $|\eta_{00}|^2 = (24 \pm 5) \times 10^{-6}$. The CERN result was reassuring. A measurement by my colleague Fitch and his coworkers (43) about one year later was unsettling. Although they did not observe the $2\pi^0$ decay, they gave an upper limit of $|\eta_{00}|^2 = (-2 \pm 7) \times 10^{-6}$. The most disturbing news was an informal report from a heavy-liquid bubble chamber experiment at CERN, which found $|\eta_{00}|^2 \approx |\eta_{+-}|^2$.

By the summer of 1968, I had become convinced that we had made an error as we began to get results from our improved experiment. Evidence of my concern is found in a footnote in Fitch and colleagues' paper (43, p. 561) submitted in May 1968. The footnote reads:

Professor Cronin kindly informs us that a preliminary analysis from a new experiment shows that his earlier data contained substantial background. We look forward to the final results of this new experiment.

Figure 16 shows the 5 tons of spark chambers installed about the beam to detect the γ decays that were correlated to the γ -ray that triggered the pair spectrometer. There were also some minor improvements to the spectrometer. The analysis of the experiment with the added γ -ray detectors is too complex to describe in detail here. It was found, however, that of some 650 events that appeared above the p_T cut of 165 MeV, some 150 were due to $K_L \rightarrow \gamma\gamma$, and only 25% of the remaining 500 events could be associated with $2\pi^0$ decays. We had severely underestimated the background in the prior experiment. The results of the experiment were presented in two issues of *Physical Review Letters*. The first paper was on the measurement of $K_L \rightarrow \gamma\gamma$ (44), with the result that $(K_L \rightarrow \gamma\gamma)/(K_L \rightarrow \text{all modes}) = (4.7 \pm 0.6) \times 10^{-4}$. The second paper was on the measurement of $(K_L \rightarrow 2\pi^0)$ (45). We found $|\eta_{00}|^2 = (4.9 \pm 1.2) \times 10^{-6}$. In the summary, we wrote, "On the basis of this experiment, we conclude that our previous result [Reference 41] obtained with only the pair spectrometer was incorrect. We have no evidence to conclude that $|\eta_{00}|$ is different from $|\eta_{+-}|$." Our paper was submitted on August 16, 1968. The CERN bubble chamber result (46) was submitted on October 11, 1968, after ours, but that result had been known

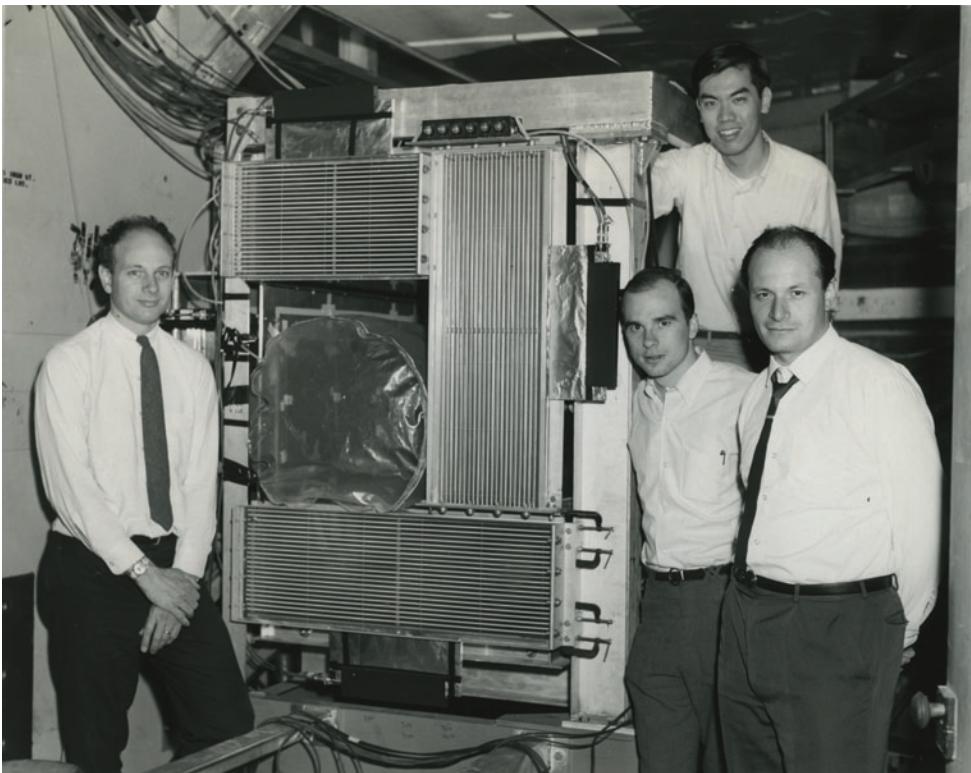


Figure 16

The arrangement of the stainless-steel-plate chambers added to the pair spectrometer. Clockwise from the left are experimenters James W. Cronin, John Liu, Marcel Banner, and James Pilcher.

for some time. As the analysis was quite complicated, a long paper describing the PPA experiment was published in *Physical Review* (47).

It was a great personal disappointment to have published a paper that was totally incorrect. As I review what happened after the passage of 45 years, I do not feel so bad. As in any scientific endeavor, we planned to improve the measurement and in so doing found our mistakes and were still able to publish a correct value of $|\eta_{00}|$ in a timely fashion. I can chalk it up to a bit of gambling, as a more careful physicist would not have been content to allow such an important measurement to depend on a single γ -ray spectrum.

I was stubborn, however, and designed one more experiment to further improve the $|\eta_{00}|$ measurement. I had very able help from Marcel Banner and students Bruce Knapp and Mel Shochet. It became clear that it would be necessary to measure all four decay modes in the same experiment because now any difference between $|\eta_{00}|$ and $|\eta_{+-}|$ would be less than 20%. There was a danger of systematic errors if the conclusion was constructed from the results of different experiments.

We returned to a high-energy K_L beam at the Brookhaven AGS because γ decays produce unmistakable showers in a spark chamber with many lead plates. First, we aimed to detect $2\pi^0$ decays from K_L and K_S decays (by regeneration) with the same apparatus. Second, the apparatus was modified to measure the corresponding $\pi^+\pi^-$ decays. **Figure 17** shows the apparatus in the configuration used to measure the $2\pi^0$ decays. To suppress $3\pi^0$ events, we continued to use a

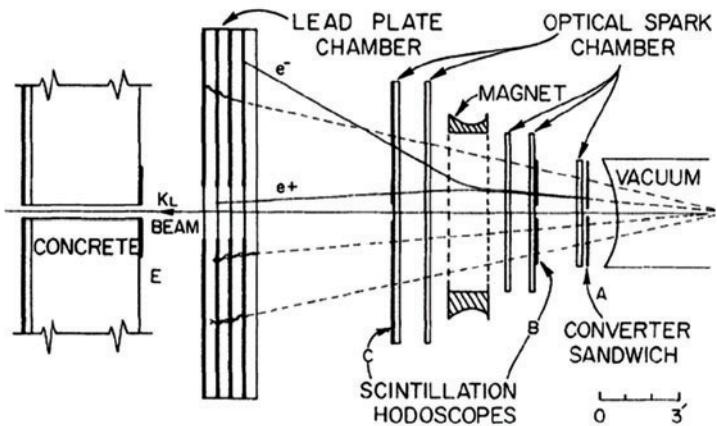


Figure 17

The arrangement for the measurement of $K_L \rightarrow 2\pi^0$ decays at the AGS. Modified with permission from Reference 48.

trigger the detection of an e^+e^- pair with a trigger favoring high p_T of the converted γ -ray. The hodoscope planes B and C were placed symmetrically on each side of the magnet. The trigger logic demanded that the inward-deflected electron pass plane C at a greater distance from the beam than at plane B.

The decays occurred in a 10-foot-long vacuum tank. A uranium regenerator whose downstream face was covered by a 1/16-inch thick anticounter could be moved remotely over the free decay volume. A simple conversion (not shown in Figure 17) sufficed to pass to the charged mode. The lead-plate spark chamber and the pair conversion plane were removed and replaced by Cherenkov counters and a muon filter to suppress the three-body decays. A trigger, typical for the study of charged decays, required that tracks emerge from the magnet approximately parallel.

Figure 18 shows the quality of the $2\pi^0$ data. One can see the tail of the $3\pi^0$ distribution in the bin 160–170 MeV/c. The data were very clean, but even with all the effort there were not many events. The result of this experiment was $|\eta_{00}|/|\eta_{+-}| = (1.03 \pm 0.07)$. It was published in *Physical Review Letters* in March 1972 (48). In the same experiment, the ratio $(K_S \rightarrow \gamma\gamma)/(K_S \rightarrow \text{all})$ was measured to be $(-1.9 \pm 2.4) \times 10^{-4}$ and was published in a companion paper (49).

Almost simultaneously, a similar result, $|\eta_{00}|/|\eta_{+-}| = (1.00 \pm 0.06)$, was published by a CERN group (50). This CERN experiment foreshadowed the future for improved experiments. The detector for the γ -rays was an array of lead glass with appropriate supplementary detectors. It had the ability to accurately reconstruct the $2\pi^0$ events and separate them from the $3\pi^0$ background. This is what I had called the brute-force technique. At this point, I again felt that I had to invoke my earlier observation: “I have great difficulty in measuring anything better than 10%.” I decided that I had done my best.

9. MOVE TO THE UNIVERSITY OF CHICAGO: $K_L \rightarrow \mu^+ + \mu^-$

In the academic year 1970–1971, I took a sabbatical at the new National Accelerator Laboratory (NAL; later named Fermilab), located 40 miles west of Chicago. I was looking for a new area of research to be carried out at Fermilab. During my sabbatical I received an offer from the University of Chicago to become University Professor of Physics. This offer was very attractive, as I imagined spending my future working at Fermilab. I would not have to commute from Princeton to carry

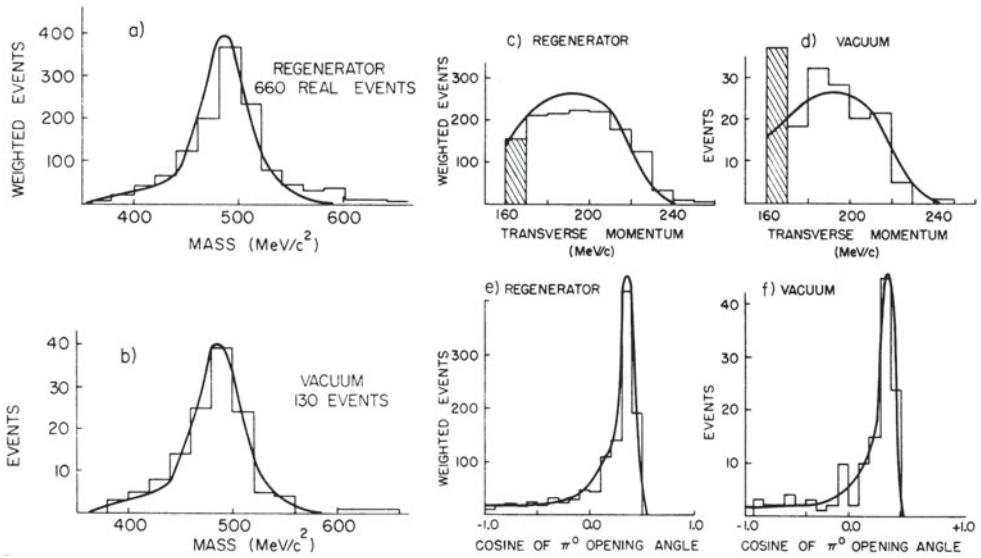


Figure 18

Free decay and regenerated data for $K_L \rightarrow 2\pi^0$ decays at the AGS. (a,b) The reconstructed mass distributions for the regenerator and the free decay. (c,d) The transverse momentum distributions of the reconstructed events. The solid curves are the distributions as predicted by Monte Carlo calculations. (e,f) The opening-angle distribution of the X-rays from the π^0 decay that did not produce the converted pair.

out my research. My wife encouraged me to make the move as well. Her family lived in Chicago, and she was eager to continue her education at the University of Chicago. So, with some regret, I left beautiful Princeton and accepted the Chicago offer. There was an apocryphal tale that the first thing one did after accepting a tenured position at Princeton was to purchase a burial plot, as one would never leave!

The next section is devoted to five years of work at Fermilab. When I arrived in Chicago in the fall of 1971, it was clear that my plans to work at Fermilab would take several years before they could start. A young physicist from Berkeley, Henry Frisch, joined me as a postdoc. His PhD thesis had been a measurement of the branching ratio $R = (K_L \rightarrow \mu^+ + \mu^-)/(K_L \rightarrow \text{all})$. Frisch and colleagues' experiment (51) found no events, providing an upper limit for $R \leq 1.8 \times 10^{-9}$.

This result was very surprising because the measured rate of $K_L \rightarrow \gamma + \gamma$ required a lower limit of R to be $(5.9 \pm 0.6) \times 10^{-9}$. This prediction was based on the well-established principles of quantum electrodynamics and unitarity. There was no simple explanation for the absence of $K_L \rightarrow \mu^+ + \mu^-$ and an upper limit well below the prediction. There was a brief period in which the $K_L \rightarrow \mu^+ + \mu^-$ puzzle had the attention of a number of theorists.

At the time, it seemed reasonable to repeat the measurement with a new instrument that emphasized background rejection and ensured that the signal, if existing, would be observed. The 12-GeV ZGS accelerator was available to carry out the experiment. A neutral beam was produced at an angle of 4° to the 12-GeV extracted proton beam. The beam had a mean K_L momentum of ~ 3 GeV/c. The apparatus was quite complex, and the date of the final publication occurred a year later than all the work done at Fermilab, which is described in the following section. The apparatus is shown in **Figure 19**. It was the first time that I used spark chambers with an electronic readout and multiwire proportional chambers—no more optical spark chambers and no more scanning.

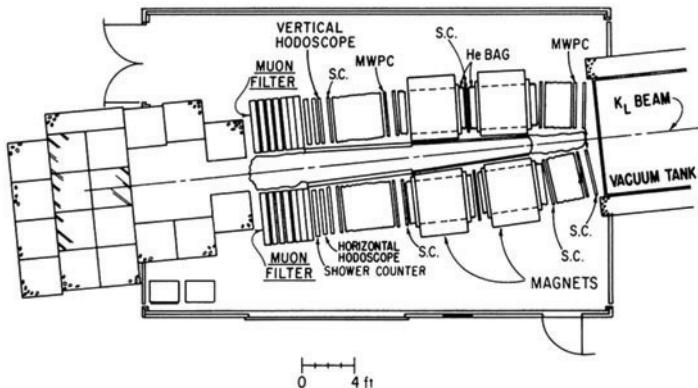


Figure 19

Sketch of the apparatus for the measurement of the $K_L \rightarrow \mu^+ + \mu^-$ decay rate. Abbreviations: MWPC, multiwire proportional chamber; S.C., spark chamber. Modified with permission from Reference 52.

The apparatus was a typical K_L spectrometer used by many experimenters. The only new feature was to split the magnetic deflection of the charged particles into two parts with a measurement of the trajectory at the midpoint of the bending. This feature reduced the background from pion decays in flight from $K_{\pi\mu\nu}$. Although most physicists thought that there was probably an error in the Berkeley experiment, there was the distinct possibility that there would be very few $\mu^+ \mu^-$ decays. For this reason, a lot of attention was paid to background suppression. A high mass resolution was obtained because the charged decay particles passed through very little material before any scintillation counters or any particle-identification elements were crossed. The possible $\mu^+ \mu^-$ decays were normalized to the CP -violating decays. The trigger required a charged particle on each side of the spectrometer. In addition, for the $\mu^+ \mu^-$ trigger each charged track was required to penetrate an amount of steel corresponding to a momentum of 1 GeV/c.

There were many components of the apparatus, which required time to build. We were fortunate that Chicago had excellent, well-staffed workshops and an excellent electronics group led by Tom Nunamaker, who had developed a shift register readout for spark chambers. The delay in mounting the experiments at Fermilab permitted the hardware to be built. However, mounting the experiment at the ZGS, which included building special magnets, took some time. The experiment ran successfully during the years 1974 and 1975. The ZGS did not have a smooth extraction of the proton beam, so the effective spill time of the beam was less than 100 ms per pulse for a repetition rate of 5 s. This forced us to lower the beam intensity because of the need to reduce the multiple accidental hits in the spark chambers, which—even with the clearing field—had a sensitive time of $\sim 1 \mu\text{s}$. The lower beam intensity severely reduced the number of events we were able to collect in a reasonable running time.

The reconstruction and analysis turned out to be quite complicated and required several years to complete. Because the trigger for $K_L \rightarrow \mu^+ + \mu^-$, with the additional requirement for the penetration of steel, was different from the trigger for $K_L \rightarrow \pi^+ \pi^-$, many auxiliary experiments were carried out to measure the relative efficiency of the two triggers. The $\mu\mu$ trigger was $\sim 10\%$ less efficient than the $\pi\pi$ trigger. **Figure 20** shows the square of the reconstructed angle of each $\mu\mu$ candidate with respect to the beam versus the reconstructed mass. Sixteen events were found, and the branching ratio R turned out to be $(8.1 + 2.8 - 1.8) \times 10^{-9}$ —completely consistent with the predicted lower limit of $(5.9 \pm 0.6) \times 10^{-9}$.

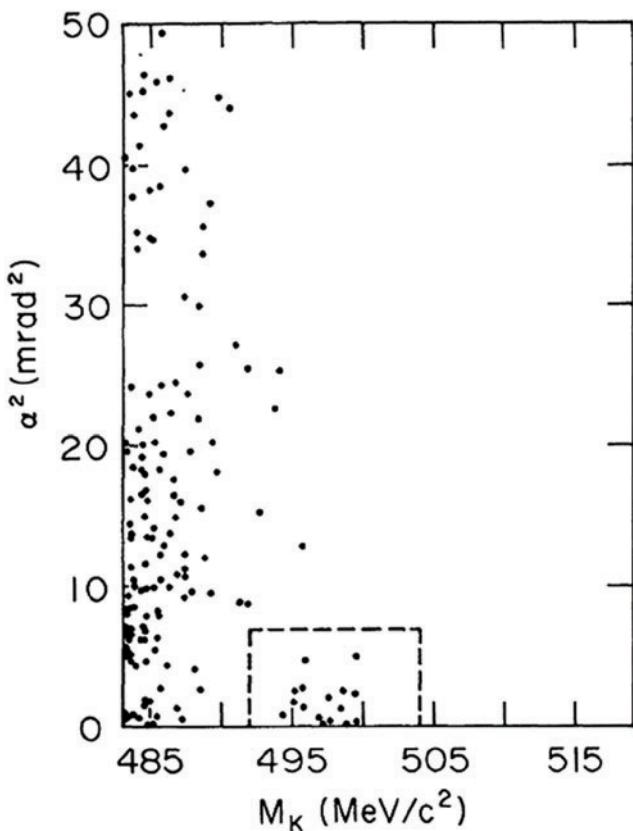


Figure 20

Plot of the angle squared with respect to the beam versus the reconstructed mass for all the $K_L \rightarrow \mu^+ + \mu^-$ events. Modified with permission from Reference 52.

In 1977 we published a short paper describing the results in *Physical Review Letters* (52) as soon as the analysis was completed. A detailed paper about the experiment was published in *Physical Review* (53) a year later. While we were building and running our experiment, two other groups (55) used a modified existing apparatus to show that the missing K_L decay did exist; these two groups found nine and three events, respectively. So our somewhat more sensitive measurement was really no surprise.

Thinking back almost 40 years, I now doubt that it was a good decision to begin this experiment. The absence of $K_L \rightarrow \mu^+ + \mu^-$ was really astounding. The experiment appeared to have been carefully carried out and analyzed by members of the Berkeley team, many of them prior collaborators on the early hyperon experiments. However, there existed several running K_L detectors that could be rather quickly modified to look for $K_L \rightarrow \mu^+ + \mu^-$. Nevertheless, it seemed to make sense to build a brand new apparatus with better background rejection, anticipating a much smaller signal. We had bad luck with the fact that the days of the ZGS were numbered, and there was little expertise remaining to produce a less structured beam spill. The experiment was successful, but the results were only slightly better than those from the other experiments that were modified to measure $K_L \rightarrow \mu^+ + \mu^-$. The most successful experiment, published 22 years later (56), recorded 6,200 $K_L \rightarrow \mu^+ + \mu^-$ events. Its measured branching ratio, R , was $(7.18 \pm 0.17) \times 10^{-9}$.

Our work at Fermilab was more successful. With these experiments we had the pleasure of looking at a brand new energy range in which entirely new knowledge could be gained with a rather simple apparatus. We also had competitors who kept us on our toes. The next section covers the exciting time at Fermilab from 1973 to 1977.

10. EXPERIMENTS AT FERMILAB

NAL was brand new. Robert R. Wilson, its charismatic director, was able to hold the increasingly bureaucratic Atomic Energy Commission at bay with innovative architecture and a nonbureaucratic organization. Wilson encouraged simple experiments that could be set up, run promptly, and then torn down. This philosophy did not work well with many of the larger experiments, such as those on neutrino interactions.

At the time of my sabbatical at NAL, there was much discussion about the parton model. Richard Feynman gave a talk about this model in which all the physics took place at low p_T . This gave me the idea of looking at high p_T : “If there was supposed to be nothing there, then anything we would find would be interesting!” This idea was the only motivation to set up a spectrometer to look at charged particles produced at $\sim 90^\circ$. I had just planned to separate the high- p_T particles into electrons, hadrons, and muons. I discussed the proposed measurements with my colleague at Princeton, Pierre Piroué. He found the idea interesting and suggested that far more information could be obtained with the addition of two gas-filled Cherenkov counters to identify the hadrons. I eagerly accepted the suggestion, and Pierre and I formed a great collaboration that lasted some five years. The experiment was in the simple style of Robert Wilson and was strongly supported by him. Our proposal was promptly accepted by the program committee in 1971. By chance, the proposal was the one-hundredth received by the laboratory, and it was identified as E100. (Even this rather simple experiment defied Wilson’s hope for experiments with a quick turnaround.)

During the years 1955 to 1971, I had designed and was engaged in commissioning all my own apparatus and electronics. The valuable expertise of my Princeton colleagues Val Fitch and Pierre Piroué taught me how to design the electronics needed to carry out a particular experiment. My students became experts on all the hardware we needed. When my collaboration with Pierre began, I realized that my many years with optical spark chambers had left me lacking in a number of experimental skills. This was particularly true concerning the use of computers to control and receive data from the experimental apparatus. Pierre brought to our collaboration new skills and the first computer-controlled experiment.

Our proposal to NAL was to study the spectra of charged particles produced at high p_T in the collisions of 200–400-GeV protons with heavy targets. I had reasoned that the energies involved in the high-energy collisions were so much larger than the nuclear binding energies that the results would be identical whether one used a hydrogen target or a tungsten target. With this reasoning, we chose to produce the particles on a tungsten target because the production rate would be much larger and there was no complication with liquid hydrogen. **Figure 21** shows the layout of the E100 spectrometer. Our spectrometer was essentially a beam line set up at an angle of 77 mrad with respect to the extracted proton beam. This angle corresponds to a center-of-mass angle of 90° for relativistic particles in a nucleon–nucleon collision at 300 GeV. The extracted proton beam at the target was ~ 2 mm in diameter at the target. The magnetic elements in the spectrometer were made of the same-style bending magnets and quadrupoles used in the machine itself. The horizontal magnification between the target and the end of the spectrometer was ~ 2 , and the vertical magnification was ~ 20 . Thin, 10-cm-wide and 5-cm-high trigger counters were located at positions A1 to A4. At these locations, thin scintillation hodoscopes measured the horizontal position of the particle to ± 0.3 cm and the vertical position to ± 0.45 cm. The trigger was the

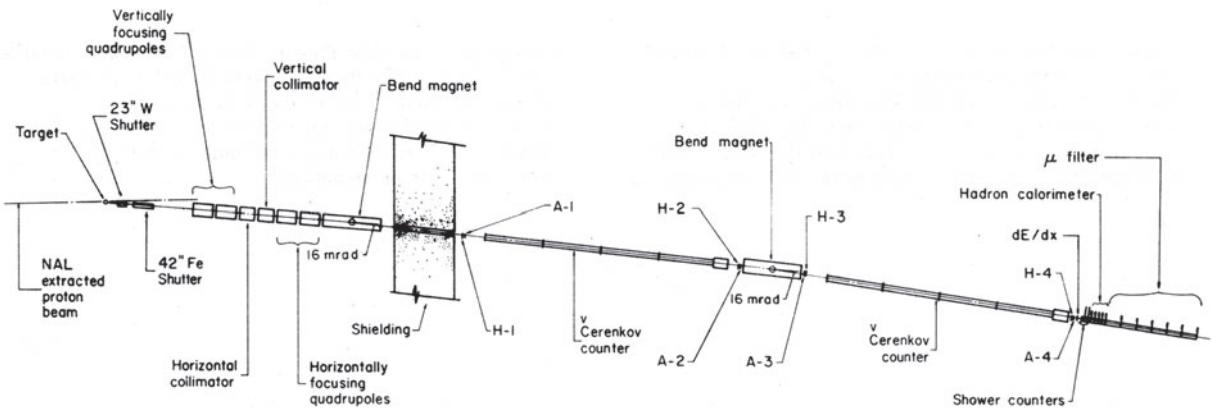


Figure 21

Layout of the high- p_T spectrometer at Fermilab. Modified with permission from Reference 54.

coincidence A2–A3–A4. The hodoscopes were interrogated only after a successful trigger. When high- p_T particles were being measured, the trigger would often contain an accidental coincidence, producing multiple hits in the hodoscope and other ambiguities. With the hodoscope information, the path of the particle could be traced back to the target with good accuracy, particularly for the vertical position with a demagnification factor of 20. For accepted events, we required that the trajectory of the particle be consistent with its origin at the target. In most cases, the width at the target was defined by the beam size. For the low rates at high p_T , the requirement that the particle position at the target be correct was important to eliminate background. **Figure 22** shows the horizontal and vertical distributions of detected particles reconstructed at the target position.

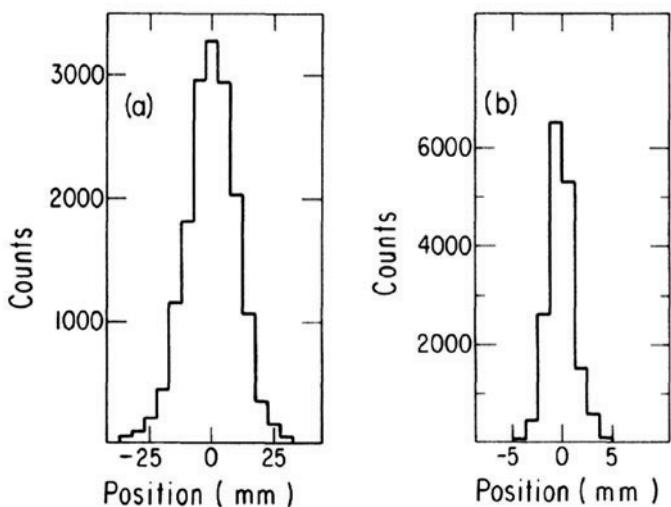


Figure 22

Reconstructed distributions of 40 GeV/c ($p_T = 3.05$ GeV/c) at the target. (a) Horizontal position. (b) Vertical position. Modified with permission from Reference 58.

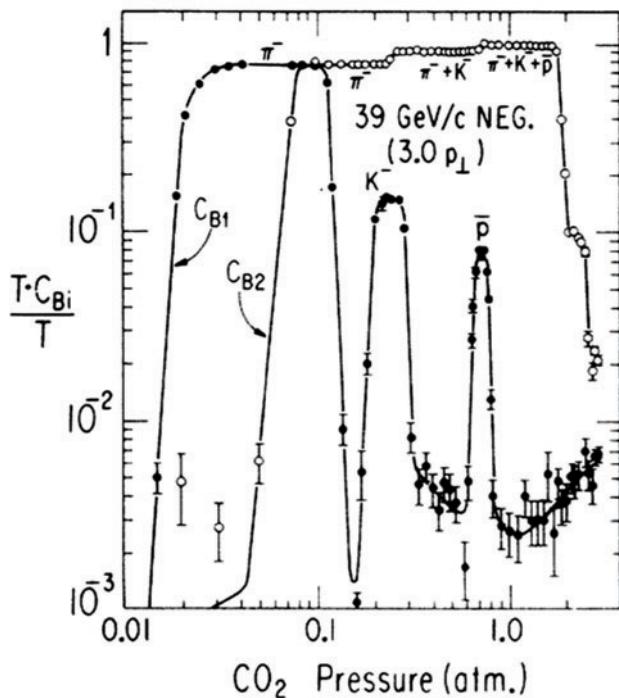


Figure 23

Complete pressure curve of a Cherenkov counter with the spectrometer set to 40 GeV/c. The curve labeled C_{B1} is the rate for the small-angle channel. The curve labeled C_{B2} is the rate for the large-angle channel. Modified with permission from Reference 58.

The Cherenkov counters built by Pierre Piroué were essential for the success of E100 and a number of subsequent experiments. Each was a 25-cm-diameter, 24-m-long tube capable of containing up to 10 atm of radiating gas. The optics split the Cherenkov light into angular bins of 0–9 mrad and 9–38 mrad with respect to the beam direction. These bins were brought to focus on 2-inch photomultipliers (RCA 31000M). The performance of one of these counters is depicted in **Figure 23**. The individual components can be cleanly selected in the small-angle bin. Normally, runs were made with one Cherenkov counter set for pions and the other set for K mesons. No count in either counter signaled a proton or an antiproton. All the information was recorded on a PDP-9 computer, which was beautifully programmed by Dick Sumner, a postdoc with Pierre.

Our idea to focus on particles produced at high p_T was completely original with us, but before our experiment was ready to run, the CERN ISR (Intersecting Storage Rings) began to operate in 1972 with pp collisions with center-of-mass energies up to 53 GeV. Our collision energies at NAL were 19.4, 23.8, and 27.4 GeV. A number of experiments at ISR were set up to make similar measurements, and we were scooped. Our first measurements were published in December 1973 (54). Scooped or not, our first paper had 112 citations, presumably because we measured the particle species in detail. We measured the yields of the π^+ , π^- , K^+ , K^- , proton, and antiproton as a function of p_T for 200- and 300-GeV collisions on a tungsten target. Our first cross-section calculation was based on the naïve assumption that the observed rate depended only on the fraction of incident protons that interacted with the target, independently of its atomic number. Thus, to evaluate the invariant cross section for pion production in proton–nucleus collisions, we measured

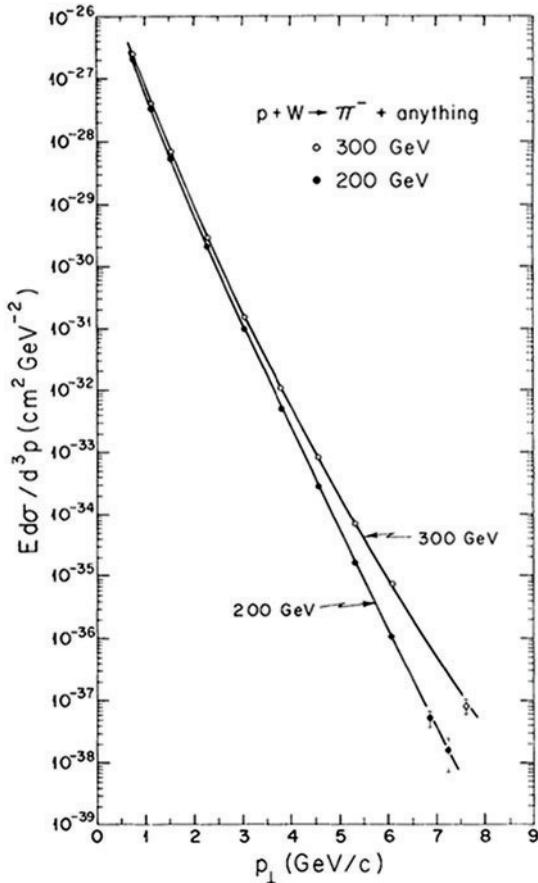


Figure 24

Cross section as a function of p_T for the production of π^- on tungsten for 200-GeV and 300-GeV collisions. Modified with permission from Reference 54.

the pion yield Y per incident proton. We calculated the invariant cross section $E \frac{d\sigma}{d^3p}$ with the expression

$$E \frac{d\sigma}{d^3p} = \frac{\sigma_p Y}{p^2(\Delta\Omega\Delta p/p)f},$$

where p is the laboratory momentum; σ_p is the proton–nucleon total cross section, taken to be 40 mb; $\Delta\Omega\Delta p/p$ is the acceptance of the spectrometer (1.7×10^{-6}); and f is the fraction of the incident protons that interact nondiffractively in the 5-cm-long tungsten target, estimated to be 0.41.

The invariant cross sections, calculated in this simple manner, are shown in **Figure 24**. A striking feature is that the curves are not exponentials, showing that there are significantly higher yields at high p_T than one would expect from an extrapolation at low p_T . The maximum p_T observed, 7.63 GeV/c, is comparable to the maximum allowed by the kinematics at 300 GeV, 11.9 GeV/c. A significant fraction of all the collision energy available had gone into the production of a single pion. Another striking feature is that for a fixed high p_T the production cross section is a strong function of the collision energy; at $p_T = 7$ GeV/c the cross section increases ~ 10 -fold

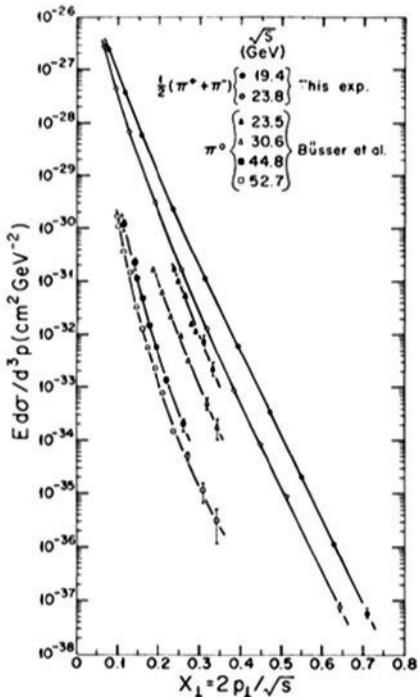


Figure 25

Comparison between our pion data and the π^0 data from the CERN ISR. Modified with permission from Reference 54.

as the incident proton energy increases from 200 to 300 GeV/c. The strong energy dependence suggests that a more important variable is the fraction of the maximum p_T available, $x_\perp = 2p_T/\sqrt{s}$, where s is the square of the collision energy.

Figure 25 shows our average cross section, $1/2(\pi^+ + \pi^-)$ versus x_\perp . The average is plotted for comparison with the CERN measurement for π^0 s (57). Our data show a scaling above $x_\perp = 0.4$. We found that the scaling form, $g(s)f(x_\perp)$, fitted our data very well. For $x_\perp \geq 0.4$, our cross sections were well described by $s^{-5.4} \exp(-36x_\perp)$.

The last sentence in our paper (54, p. 1428) read:

Finally it should be mentioned that auxiliary measurements made with beryllium and titanium targets established that none of the important features observed in tungsten was dependent on atomic-number.

(Note that although we identified “atomic number” as the number of nucleons in a nucleus, the proper description is “mass number,” which is identified by the letter A. Atomic number is identified by the letter Z, the number of protons in the nucleus. Although the letter A is used in all the plots, our identification of A with atomic number was incorrect. It is interesting that no referee of our numerous papers called us on this mistake! We preserve the error in the following text.)

However, quantitatively, there was a discrepancy. At $\sqrt{s} = 23.8$ GeV, our cross sections were approximately three times larger than CERN’s at $\sqrt{s} = 23.5$ GeV (**Figure 25**). In our initial paper (54), we did not make a big issue about this discrepancy, but it led us to measure more

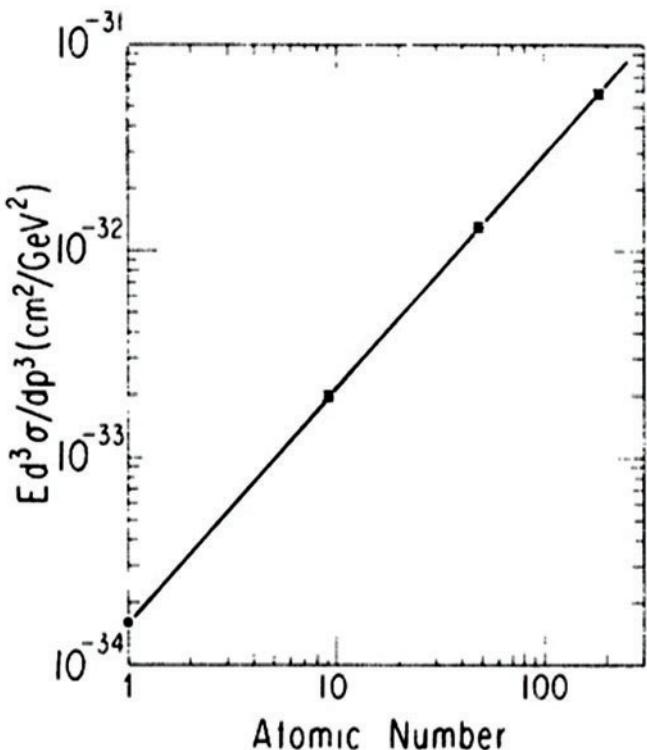


Figure 26

Plot of the π^- invariant cross section at $p_T = 4.6 \text{ GeV}/c$ as a function of atomic number. Plot for 300-GeV incident protons. Modified with permission from Reference 58.

carefully the atomic number dependence of the invariant cross section. This issue was discussed in a comprehensive paper published in *Physical Review D* (58). Following the first measurements reported in Reference 54, we made measurements of the invariant cross section for targets of beryllium, titanium, and tungsten, each 0.2 interaction lengths thick. The invariant cross section per nucleus was measured for pions, K mesons, protons, and antiprotons for 300-GeV incident protons. In every case, the cross section for a fixed p_T fitted the form $E \frac{d\sigma}{dp^3} = A^\alpha$, where A is the atomic number. Our original naïve assumption placed the effective number of nucleons in a tungsten collision as the ratio of the proton–tungsten interaction cross section to the proton–proton cross section, $1,635 \text{ mb}/40 \text{ mb} = 41$, which corresponded to $\alpha \sim 0.7$. **Figure 26** shows the log of the invariant cross section for π^- versus atomic number for $p_T = 4.6 \text{ GeV}/c$. Here the atomic number dependence is $A^{1.1}$, so the yield from tungsten was approximately eight times larger than the naïve assumption. The comprehensive paper presented **Figure 27**, where the value of α was plotted for each species as a function of p_T . Before we were able to have a hydrogen target, we decided to extrapolate curves such as the one in **Figure 26** to $A = 1$ to get a value for the proton–nucleon cross section. We had measured the A dependence only for the incident energy at 300 GeV. We assumed that the A dependence at a given p_T was independent of energy. This procedure was necessary to compare our data with those of CERN and with the many models describing the production of particles at high p_T . We subsequently measured the A dependence for 200, 300, and 400 GeV with very thin (0.03) interaction lengths to be sure our measurements

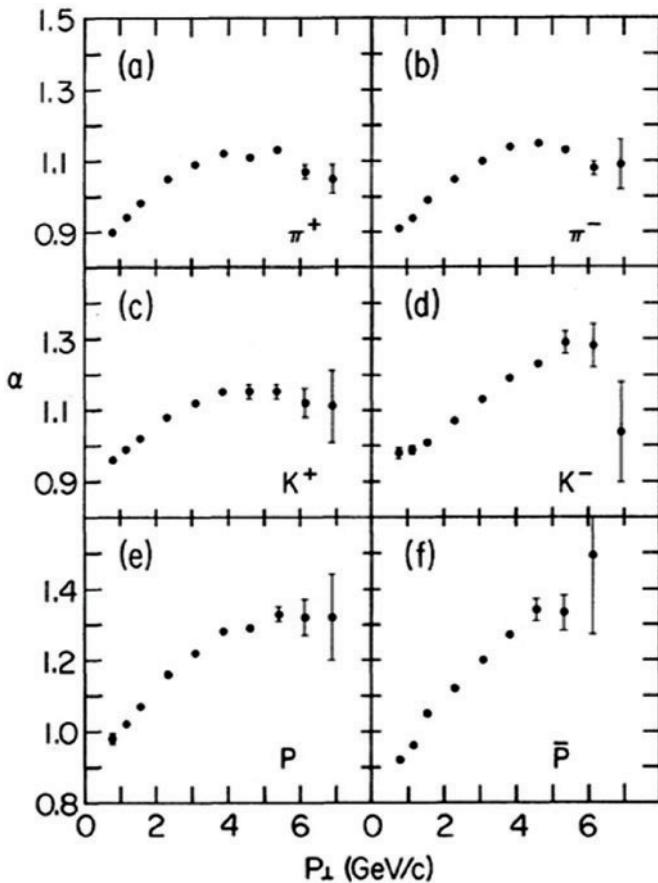


Figure 27

Plots of α as a function of p_T for the various particle species. Modified with permission from Reference 59.

were correct and to experimentally check our assumption that the A dependence was independent of energy. Because these measurements turned out to be of interest to a larger community, we wanted to be certain that the measurements were absolutely correct. These results were published later in *Physical Review Letters* (59).

Our measurements of the A dependence, which for us were incidental because we did not have a hydrogen target, turned out to be interesting to the community that studied proton–nucleus and nucleus–nucleus collisions with fixed targets and colliding beams. Our comprehensive 1975 paper received 331 citations. Unbeknownst to me, the A dependence came to be known as the Cronin effect. I learned of this when I was in a seminar at Chicago, where a young physicist was talking of nucleus–nucleus collisions. He referred to the Cronin effect but was unaware that I was in the audience! At the end of his seminar, I gently introduced myself. A paper in *Physical Review Letters* titled “Cronin effect in hadron production off nuclei” (60) describes the effect from the point of view of nuclear physicists.

Our 1975 paper (59) gave all the results from two years of experimentation. These results presented the proton–nucleon invariant cross sections based on extrapolation to $A = 1$ from measurements with nuclear targets (Figure 26). We found that the invariant pion cross section

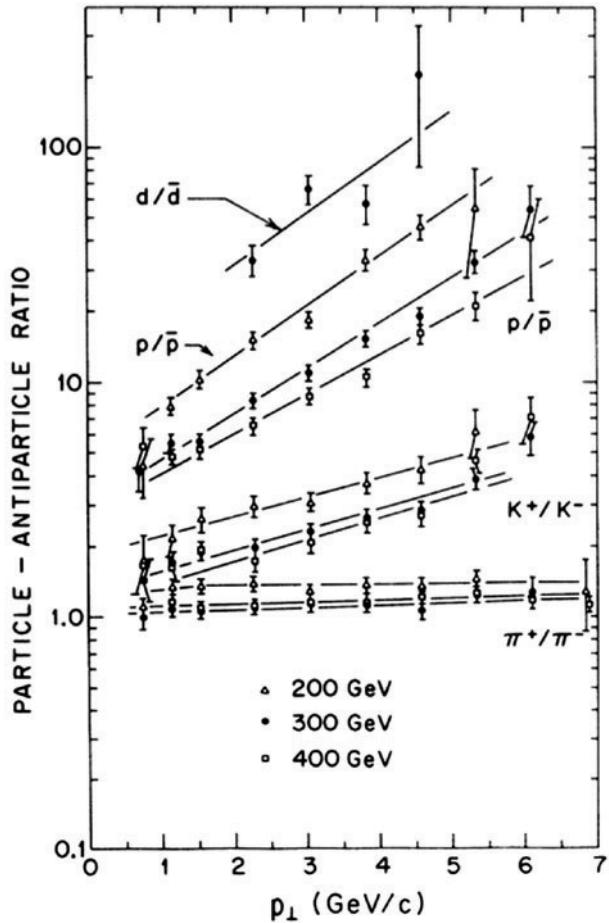


Figure 28

Ratio of particle production to antiparticle production on tungsten as a function of p_T for each of the measured energies. Modified with permission from Reference 58.

above $x_{\perp} = 0.4$ fit the scaling form $(\sqrt{s})^{-n} \exp(-ax_{\perp})$ where $n = 11$ and $a = 36$. A model for the high- p_T pion production, termed the constituent interchange model (CIM), by Blankenbecler & Brodsky (61) had a convincing theoretical basis. In this model, the invariant cross section had the dependence $p_T^{-n}(1-x_{\perp})^b$. For pion production in pp collisions, the CIM predicted $n = 8$ and $b = 9$. The n of the CIM essentially had the same value as the n of our empirical scaling formula. Our $n = 11$ disagreed with the theoretically motivated CIM.

Our paper contained measurements of all the hadrons, which are too numerous to discuss here. We present one figure to show the extent of the measurements. **Figure 28** shows the measured ratios of particles and antiparticles on tungsten as a function of p_T for each of the measured energies. The results are reasonable. Pions of either sign are produced almost equally for any kinematic condition. However, even at low p_T , K^+ mesons are more easily produced than K^- mesons. Raising the energy increases K^- meson production, whereas demanding higher p_T decreases the production. The same reasoning goes for antiproton production. It was gratifying to observe antideuterons. Their production fit the simple idea of the merger of an antiproton and

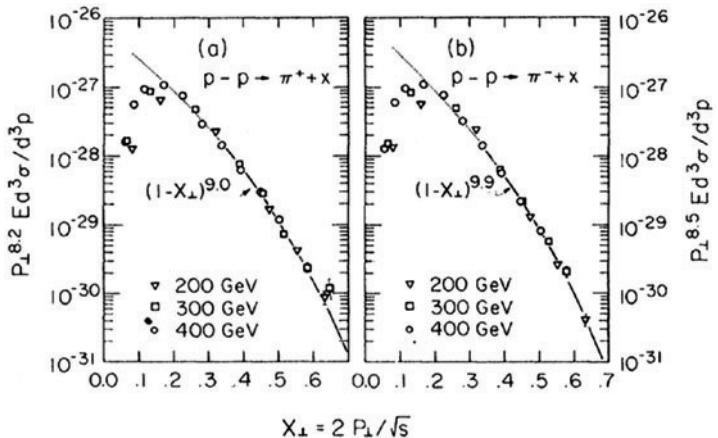


Figure 29

Fits of $p + p \rightarrow \pi^\pm + \text{anything}$ to the constituent interchange model. Modified with permission from Reference 64.

an antineutron produced at the same angle and equal momenta, leading to the relation $\frac{d}{d}(p') \simeq [\frac{p}{\bar{p}}(p'/2)]^2$, where p' is the momentum of the deuteron (antideuteron). The $\frac{d}{d}$ data fit this relation within the statistical errors.

We were able to install a hydrogen/deuterium target in 1975. Finally, we were able to measure directly inclusive particle production in pp collisions. These measurements were much anticipated because our conclusions based on extrapolation to $A = 1$ were not in agreement with the CIM. The measurements at the ISR for high π^0 s were in agreement with the CIM (57). Our first results were published in companion papers in *Physical Review Letters* (62, 63). **Figure 29** shows that the cross sections $p + p \rightarrow \pi^\pm + \text{anything}$ are fitted to $p_T^{-n}(1 - x_\perp)^b$, in good agreement with the CIM. A comprehensive paper published in *Physical Review* (64) describes in detail all the measurements made with the hydrogen/deuterium target.

Even with experience, there are always surprises in research. Our assumption that the particle yields were proportional to the number of interacting protons independently of the nature of the target proved to be wrong. But in the course of understanding, a discovery was made concerning the atomic-weight dependence of the particle production. This dependence turned out to be important in understanding proton–nucleus and nucleus–nucleus collisions.

We used our spectrometer at Fermilab to make a number of other measurements. Most noteworthy was the measurement of the invariant cross section for the production of “direct” muons. These are muons that are experimentally produced in the target—not the decay products from pions and K mesons. They can be decay products of very short lived hadrons, such as the recently discovered charmed particles.

We inserted two absorbers close to the target that could be moved in and out of the spectrometer beam to separate direct muons from pions and K decay muons. The first absorber was a 23-inch-long tungsten block whose upstream face was 9.5 inches from the target. The second absorber was a 42-inch-long iron block whose face was 42 inches from the target. The muons were identified by their penetration of 15 feet of steel at the end of the apparatus. Data were taken without an absorber, with the tungsten absorber only, and with the iron absorber. We verified that the yield of direct muons was negligible when the procedure was repeated without a target. These data were used to measure the ratio of direct muon yield to pion yield for each absorber. An example of

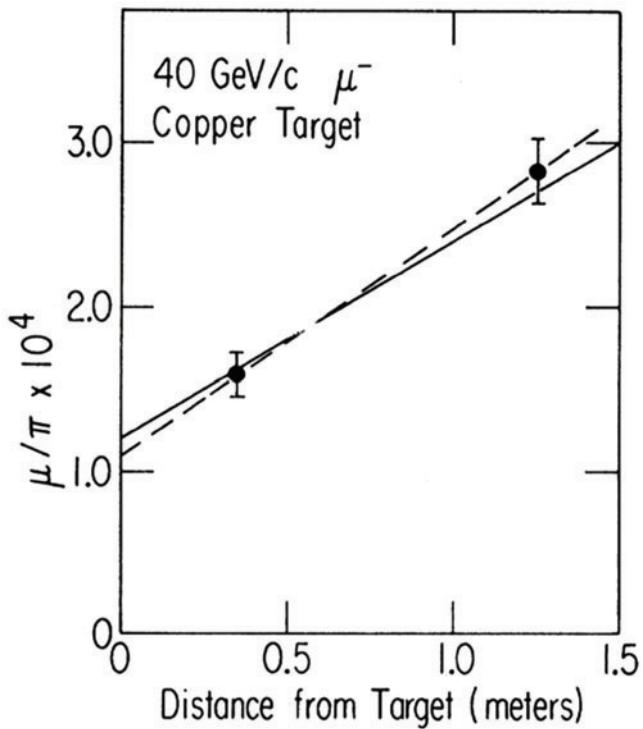


Figure 30

Yield of muons as a function of distance between a hadron absorber and the target. The dashed line is a fit to the measured slope. The solid line is the slope calculated from the known production of pions and K mesons. Modified with permission from Reference 65.

the data extraction is shown in **Figure 30**. The agreement validates our measurement technique. **Figure 31** shows the invariant cross section for direct muon production. The muon-to-pion ratio was an approximate constant, 0.8×10^{-4} . These results were published in *Physical Review Letters* (65) in July 1974. Our paper was one of many efforts to measure direct muon and electron/positron production. In the end, the apparent constant muon-to-pion ratio was the result of a “cocktail” of many direct muon sources arising from the recent discoveries of J/ψ , charmed particles, tau leptons, and Drell-Yan processes.

The period of the mid 1970s at Fermilab was very exciting—a brand new laboratory with many things to measure in a new domain. Our measurements of particle production at high p_T placed us in the thick of the excitement. Of course, when the dust settled most of these results were forgotten, but at the time it seemed clear that the study of dimuon production would offer new discoveries. I was eager to begin measurements of dimuons. We did this in a crude way—perhaps too crude. We built what we called the multihole spectrometer (MHS). We installed 10 $3.6 \times 1.1 \times 0.1 \text{ m}^3$ liquid scintillator counters in a string of holes in the ground displaced 6 m from the incident proton beam line. The configuration is shown in **Figure 32**. Using the tungsten shutter, we could select with the spectrometer direct muons with $\sim 70\%$ purity. The principal experimental problem was to measure accurately the level of accidental coincidences. The Fermilab proton beam arrived in 2-ns-wide bunches every 20 ns, dictated by the RF frequency. Signals from the liquid scintillator were placed on time-to-digital converters and could be timed with respect to the proton pulse with a precision of 5 ns. The time of a direct muon pulse uniquely predicted the RF bin for each

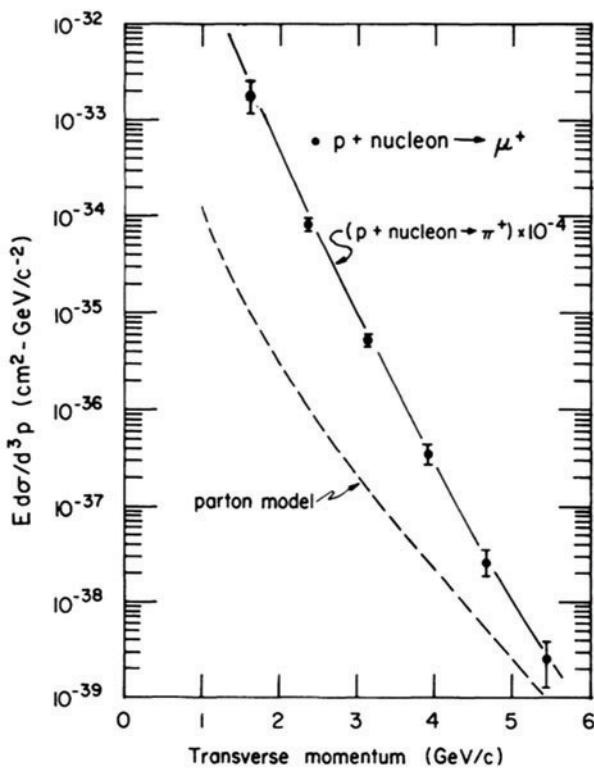


Figure 31

Invariant cross section for direct muon production as function of transverse momentum. Modified with permission from Reference 65.

liquid scintillator, where a coincident muon would appear. **Figure 33** shows the arrival in RF bins of the scintillation counter signal. Here the signal bin is very clear, and the level of accidental coincidences is properly measured. With a model that described the x and p_T dependence of J/ψ production (66), we were able to reconstruct the cross section $d^2\sigma/dM_{\mu\mu} dy$. Our results were published in *Physical Review Letters* in November 1976 (67). **Figure 34** compares our dimuon results with the dielectron results from Lederman and colleagues (69). Our assumed x and p_T dependence were identical to theirs. The quality of our results was clearly superior. We had won a small victory in our friendly competition with Lederman and his colleagues.

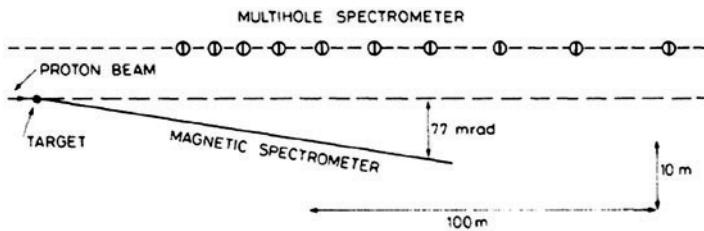


Figure 32

Layout of the multihole spectrometer. Modified with permission from Reference 67.

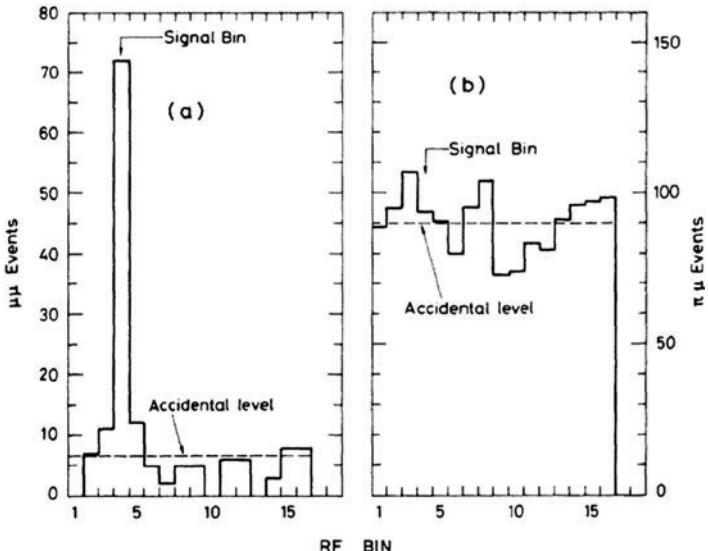


Figure 33

RF bin of muons found in liquid scintillation counters with respect to spectrometer trigger. (a) Spectrometer selection of direct muons. (b) Spectrometer selection of pions. Modified with permission from Reference 67.

A scaling was expected for the dimuon production. A plot of $M_{\mu\mu}^3 d^2\sigma / (dM_{\mu\mu} dy)_{y=0}$ versus $M_{\mu\mu}^2 s^{-1}$ should place all the data on the same line. We made additional measurements with the MHS at 200, 300, and 400 GeV to test the scaling. The results were published in *Physical Review Letters* (68), and the scaling was confirmed with poor statistics (Figure 35).

While our scaling paper was in press, a rapidly published paper by Lederman and colleagues (69) reported a dimuon spectrum obtained with a new apparatus with a $M_{\mu\mu}$ resolution of $\pm 2.5\%$. Our crude measurement had a resolution no better than 25%. More than 9,000 dimuon events with $M_{\mu\mu} \geq 5.5$ GeV/ c^2 were observed. Their mass spectrum is reproduced in Figure 36. In addition to the precise spectrum, a new resonance called Υ was discovered at 9.5 GeV/ c^2 . This great advance marked the end of our dimuon work at Fermilab. The years from 1973 to 1977 were an exciting time for all the participants at Fermilab because a new energy range was being explored and our group was in the thick of it. The study of dimuon production has been extremely effective. The dimuon mass spectrum from 0.3 GeV/ c^2 to beyond 100 GeV/ c^2 was measured in 2012 by the CMS Collaboration at CERN. The spectrum is shown in Figure 37, which displays the huge progress of particle physics from the η , ρ , and ϕ particles of the 1960s to the Z boson of the early 1980s.

One last experiment with the Fermilab spectrometer is worth mentioning. Giuseppe Cocconi and his wife, Vanna, spent a sabbatical at the University of Chicago during the academic year 1976–1977. He proposed that we look for fractionally charged particles (quarks) using our high- p_T spectrometer at Fermilab. The trigger counters A_1 , A_2 , and A_3 were set to count efficiently at a level of one-twentieth minimum ionization. The spectrometer was set to measure, with 400-GeV protons on a copper target, 6.15 GeV/ c singly charged particles that corresponded to 2.05 GeV/ c particles (quarks) with charge $1/3e$ and 4.10 GeV/ c particles with charge $2/3e$. No events were found, and there was no background. The results are shown in Figure 38 and were published in *Physical Review Letters* in 1977 (70). These upper limits are the strongest for high- p_T production of quarks at accelerators.

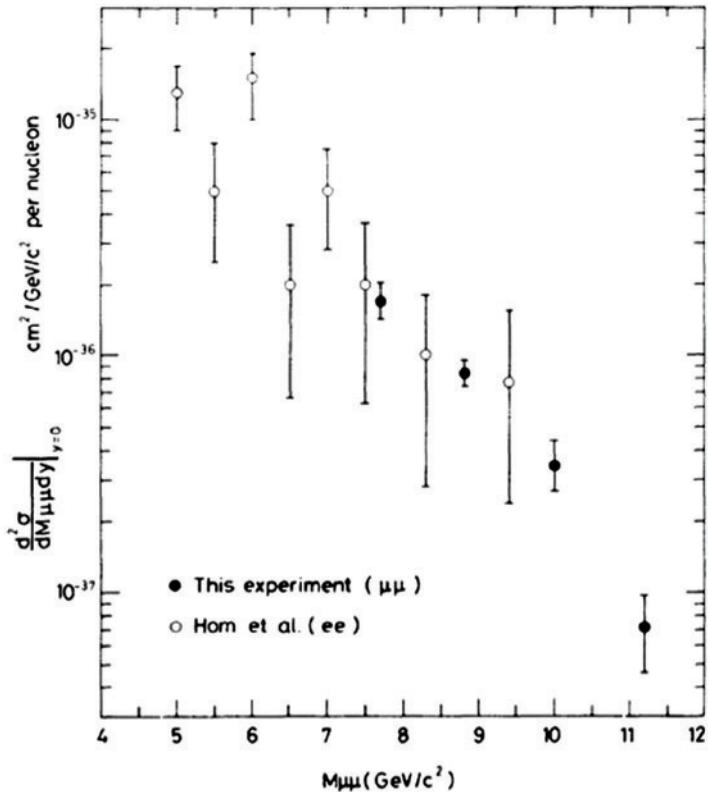


Figure 34

Comparison between our measurement of $d^2\sigma/dM_{\mu\mu} dy|_{y=0}$ for $M_{\mu\mu}$ and the same cross section for M_{ee} . Modified with permission from Reference 67.

The years 1973–1977 were very active, and even with a rather simple apparatus we were participating in a very competitive round of new physics in a brand new energy range in which simplicity was not necessarily a disadvantage. But it was time to move on, and for me the course was uncertain.

The first opportunity was an offer I received from Robert Wilson to be head of a new colliding-beams division at Fermilab. Flattered, I accepted, being quite uncritical about the conditions, the available staff, and the financial resources. In addition, there were intense and unpleasant interactions with Fermilab colleagues who felt they should have been given the responsibility. I accepted the appointment in January 1977 and resigned in frustration that fall. I will not use space here to go through the details that are described in the book *Fermilab: Physics, the Frontier and Megascience* by Lillian Hoddeson, Adrienne W. Kolb, and Catherine Westfall (71, pp. 286–88). Although I was unsuccessful, I did learn a great deal of accelerator physics. Also, I learned something very important about myself. Later in my career I gained a sufficient reputation to have offers to move to academic or scientific administration. My experience with the Fermilab colliding-beams position taught me that I would not be happy with such responsibilities, so over the remainder of my career I turned down every offer of that nature. The only (partial) exception was that I permitted myself to be on the 1985 election ballot for head of the Division of Particles and Fields of the American Physical Society and was elected. My goal as head was to establish prizes for experimental particle physics and for accelerator physics, fields that were not recognized

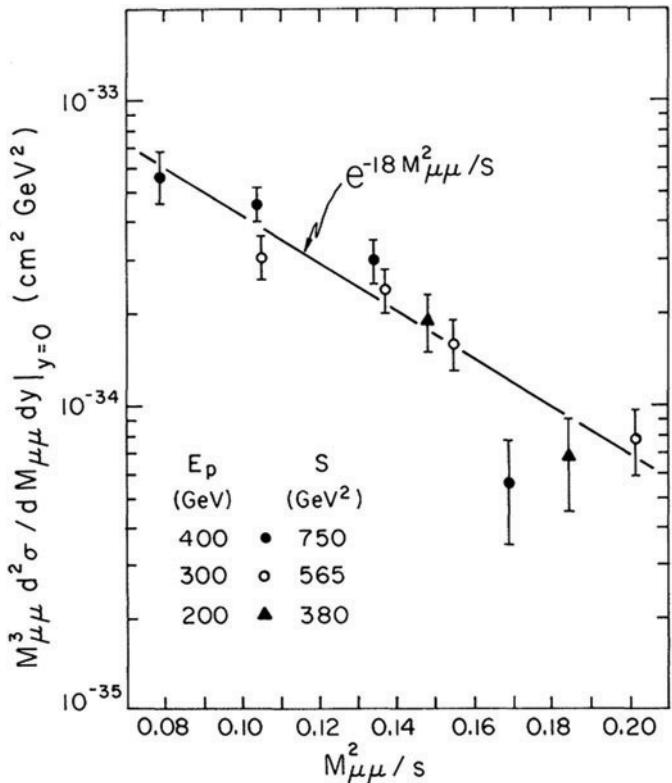


Figure 35

Test of scaling in dimuon production. Modified with permission from Reference 68.

by prizes at the time. I initiated fundraising for what has become the W.K.H. Panofsky Prize in Experimental Particle Physics and the Robert R. Wilson Prize for Achievement in the Physics of Particle Accelerators. The Wilson prize was first awarded in 1987 to Ernest Courant. The Panofsky prize was first awarded in 1988 to Charles Prescott.

I dabbled for some months with the prospects of using ultracold neutrons (velocities $\sim 0\text{--}7 \text{ m s}^{-1}$) to measure the neutron electric dipole moment. A colleague of mine at Argonne National Laboratory, Roy Ringo, had a scheme that used reasonably cold neutrons produced by a pulsed accelerator beam. By Bragg reflection off a moving mica crystal, these neutrons were essentially left at rest. This was an effective way of producing ultracold neutrons, but the question was whether sufficiently high density for a real experiment could be accumulated. As I learned more about the process, I realized that this technique would not lead to a feasible experiment. I did enjoy this diversion, and a brief paper about the measurement was published in *Physics Letters* (72).

Thinking about the electric dipole moment of the neutron led me to consider the whole problem of *CP* violation once more. Sixteen years had passed with little progress in understanding the *CP* violation in the *K* meson system. Within that system, by 1980 the only quantity that was amenable to improvement was the ratio $|\epsilon'/\epsilon|$. My younger colleague (by 12 years) Bruce Weinstein was planning a new effort for this measurement. I decided to join him in this effort. This was the first time I participated in an experiment in which I felt that I was not in the lead. The goal was worthy, however. Bruce was a very able physicist and had a good concept of how to reduce the

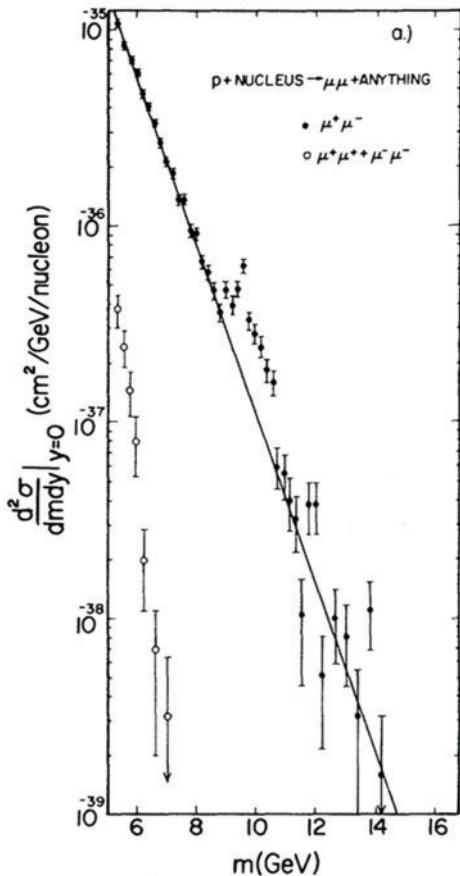


Figure 36

Dimuon spectrum measured by Lederman and colleagues (69). Modified with permission from Reference 69.

systematic errors in the measurement of the double ratio

$$(K_L \rightarrow \pi^0\pi^0/K_S \rightarrow \pi^0\pi^0)/(K_L \rightarrow \pi^+\pi^+/K_S \rightarrow \pi^+\pi^+).$$

He proposed two neutral beams side by side. The K_S mesons were produced by a regenerator. To reduce systematic errors, the regenerator shifted from one beam to the other with each machine pulse. The charged decays were measured with a conventional apparatus with magnetic deflection. In a separate run, the neutral decays were measured with an array of 804 $5.8 \times 5.8 \times 60$ cm lead-glass blocks. The depth was 20 radiation lengths. The neutral trigger required a single photon to convert in a 0.1-radiation-length converter. I took responsibility for the design and construction of the drift chambers. The analysis required some time and yielded the result $|\epsilon'/\epsilon| = -0.0046 \pm 0.0053$. This was a significant improvement over the measurements described above in 1972, with an error of ± 0.02 . The result was published in 1985 in *Physical Review Letters* (73). However, the measurement needed further improvement. In particular, it was important that one determine whether $|\epsilon'/\epsilon|$ was different from zero. Over the next 20 years, the measurement of $|\epsilon'/\epsilon|$ was vastly improved. My colleague Bruce Weinstein continued the measurements at Fermilab, and a group at CERN did as well. The final results, published in 2002–2003, showed a finite value for

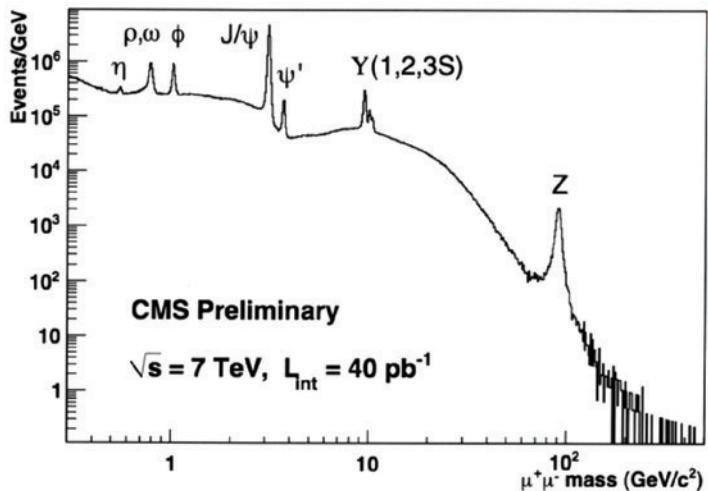


Figure 37

Dimuon spectrum measured by CMS. Reproduced courtesy of Tejinder Virdee.

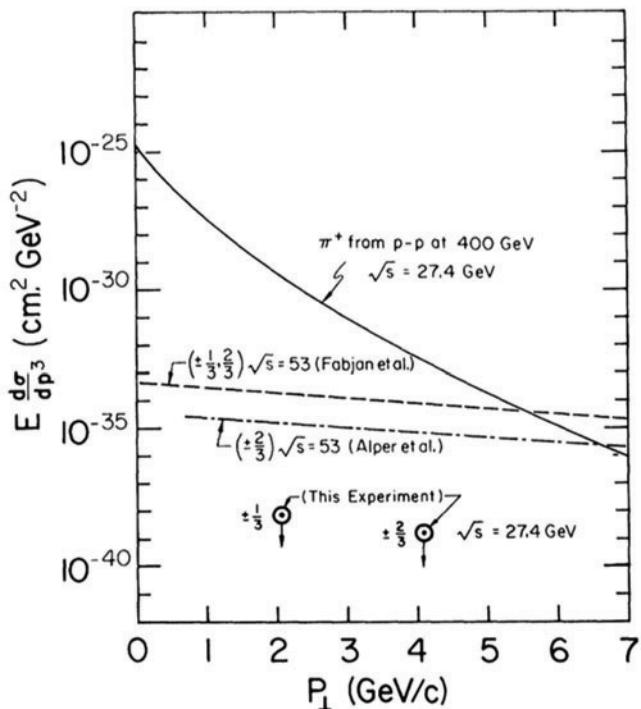


Figure 38

Upper limits for production of quarks at high p_T at Fermilab. Modified with permission from Reference 70.

$|\epsilon'/\epsilon|$. The value obtained at Fermilab (74) was $(19.2 \pm 2.1) \times 10^{-4}$, and the value from CERN (75) was $(14.7 \pm 2.2) \times 10^{-4}$.

Around the time of this experiment, a group (76) claimed that there was evidence for a fifth force. Among a variety of evidence presented was a claim that there was a small momentum dependence in the CP -violation parameter $|\eta_{+-}|$, based on a measurement at $\langle P_K \rangle = 70 \text{ GeV}/c$. The group had found $|\eta_{+-}| = (2.09 \pm 0.02) \times 10^{-3}$, whereas the world average at $\langle P_K \rangle = 5 \text{ GeV}/c$ was $(2.274 \pm 0.022) \times 10^{-3}$. Using our apparatus, described above, my student Dave Coupal (77) measured $|\eta_{+-}|$ with a beam of K_L mesons of $\langle 65 \rangle \text{ GeV}/c$ and found $(2.28 \pm 0.06) \times 10^{-3}$, consistent with the average at low momentum. Here was a case in which an existing apparatus could check a surprising claim with very little effort.

As described in the next section, Val Fitch and I were awarded the Nobel Prize in Physics for our 1964 experiment demonstrating CP violation. Bruce Winstein, like all outstanding physicists, wanted to “run his own show.” After receiving this award, he was loath to work with me because I might be deemed the leader. I certainly understood. At the time, he was being courted for a position at Stanford University. If at all possible, we could not let Bruce leave Chicago. I told Bruce to go ahead with whatever physics he wanted to do, and I would find other things to do. Fortunately, he decided to remain at Chicago. He succeeded in the accurate measurement of $|\epsilon'/\epsilon|$ cited above. He then changed fields and made beautiful measurements of the cosmic microwave background. Sadly, he died after a long battle with cancer in 2011.

11. THE NOBEL PRIZE

At 5:30 AM on October 14, 1980, I received a call from the Swedish Academy announcing that Val Fitch and I had been awarded the Nobel Prize in Physics for our 1964 discovery of the violation of CP symmetry. It was clear to us and many of our colleagues that the importance of this discovery merited the award. But after 16 years life had moved on, and one had essentially forgotten about the possible award. It was a thrill to receive the award, and for a brief time we had the kind of attention that seemed more appropriate for a rock star. At this period, I was attending a course on general relativity given by my colleague Chandrasekhar. I told my secretary to hold all calls and say I could not be reached until 10:30 AM, when the class was complete. Then a press conference was arranged, and my time was consumed by the event for several months.

Val Fitch at Princeton was equally consumed. As we both had to give a Nobel lecture, we divided the task. Val was to talk about the actual discovery, and I was to speak of the experimental progress in the 16 years since the discovery. I made a huge effort working on this talk. I reviewed all the subsequent work on all the relevant measurements. These included a number of experiments looking explicitly for time reversal. With the validity of the CPT theorem, the observed CP violation implied time-reversal violation. An alternative would be the preservation of time-reversal symmetry and the violation of the CPT theorem. A phenomenological analysis first introduced by Bell & Steinberger (78) permitted one to assess the amount of CPT violation. In an appendix to my lecture, I discovered a very simple way to make this analysis, which concluded that the CP violation was not accompanied by CPT violation. I was very pleased with my work on the Nobel lecture because it had, for me, a bit of originality.

However, the delivery of the actual lecture was a disaster! I had assumed that the audience was to be Swedish scientists. I had made my best effort to produce a serious scientific paper with some originality. I should have been aware that, for the Swedes, the Nobel week was a great and proper social occasion. The audience was filled with a broad selection of Swedish society with no particular scientific background. I had no choice but to struggle through the highly technical lecture knowing that only a tiny fraction of the audience could comprehend. What was called



Figure 39

The author (*left*) receiving the Nobel Prize in Physics, 1980.

for was a lecture for a general audience with, hopefully, a dash of humor along with an effort to explain a complicated phenomenon. The serious lecture could then be the published one. This was another lesson I learned. Since 1980, I have given many public lectures with some success. The Nobel lecture was published by *Reviews of Modern Physics* and *Science* magazine (79).

The week in Stockholm was very enjoyable and exhausting. My whole family was there. The Swedes kept tight reins on all the proceedings. You were not allowed to bring your own formal attire. In addition to the trousers and tails, one was issued two shirts. There were three occasions that required formal wear: a dinner at the town hall, a dinner at the Royal Palace, and a party given by Swedish students. We were told to wear a fresh shirt to the town hall and the palace. For the students' party, we were told to wear the shirt that was least dirty! **Figure 39** shows me receiving the prize from the King of Sweden.

It was certainly a great honor to receive the Nobel Prize, the most prestigious prize in science with more than 110 years of history. It rarely failed to recognize the greatest scientists. But one should not let that fact go to one's head. Christenson, Fitch, Turley, and I were lucky to be at

the right place and the right time with a great idea and a fine apparatus. After receiving the prize, one is flooded with invitations to speak all over the world. Offers include first-class flights and accommodations. These invitations last for the rest of one's life, with a shift to conferences that assemble Nobel Prize recipients, among others, to solve the world's problems. The presumption is that we Nobelists are so wise that we can address problems about which, in fact, we know nothing. I have mostly rejected these opportunities. I made one exception: an invitation extended by François Mitterand and Elie Wiesel to such a conference in Paris in 1989. I could not resist the opportunity to drink champagne in the Elysée Palace!

12. MY LAST PARTICLE PHYSICS EXPERIMENT

On returning from Stockholm, I continued working with Bruce Winstein on his $|\epsilon'/\epsilon|$ experiment. As explained above, I was determined to get out of Bruce's way and find some new activity. I took a sabbatical at CERN for the academic year 1982–1983 with my wife and young son. We lived at Ferney-Voltaire in France, just across the border from CERN in Switzerland.

I had always admired an experiment led by Guy von Dardel, who had directly measured the π^0 lifetime. This measurement was described in the first edition of Donald Perkins's classic textbook on high-energy physics (80). It was an elegant example of a simple experiment in high-energy physics. The direct measurement was made at the 25-GeV CERN PS, where the mean life of the produced π^0 was less than 1 μm . I was eager to repeat the experiment with the CERN 450-GeV SPS, where the mean life of the produced π^0 would be of the order of 50 μm . Like von Dardel, I wanted to make a direct measurement. Most lifetime measurements were made indirectly by the Primakoff effect. Also, I wanted to revisit for the last time the experience of a conceptually simple measurement that could be carried out with a small group. I did not hesitate to invite Guy von Dardel to join my CERN proposal. We were also joined by a number of CERN physicists and some other CERN visitors who were attracted to a simple experiment. One could not argue that any pressing issue was at hand. The lifetime was well enough known that there was no problem with the axial anomaly calculation, which agreed with fractionally charged quarks.

The lifetime experiment was accepted by the program committee and was given a budget of \sim 10,000 Swiss francs—probably one of the least expensive experiments at CERN. The longer mean lifetime permitted one to use a different technique from that employed by von Dardel. The target in which 450-GeV protons produced positrons was a precise stage in which two 70- μm -thick tungsten foils could be separated with micrometer accuracy. A schematic view of the target is shown in **Figure 40**.

This stage was built by a French engineer, René Maleyran. The positrons produced in this target can be modulated by the foil spacing. When the foils are closely spaced, many produced π^0 's decay beyond the second foil. When the foils are widely separated, all of the π^0 's decay in the space between the foils and some of the decay γ 's convert to an electron and a positron in the second foil. In general, the positron content in the beam varies as $Y(d) = N[A + B(1 - \exp(-d/\lambda))]$, where d is the foil separation and λ is the mean decay length. Typically, $B/A \sim 0.07$. The coefficient A is the yield of all positrons that are produced by Dalitz decays and other sources in each of the foils and by the beam halo striking the target mountings. This yield is independent of the foil spacing. The important point is that the variation of the positron yield with the spacing d depends only on λ .

We used a secondary beam of 150-GeV positive particles produced at 0^0 by 450-GeV protons. The beam was designed by Niels Doble to be enriched in positrons. All the magnetic elements were tuned to the local positron momentum, which drifts below the hadron momentum due to synchrotron radiation. Over their transport along the kilometer-long beam, the positrons lose 3 GeV by synchrotron radiation and produce an image displaced by 15 mm from the hadron

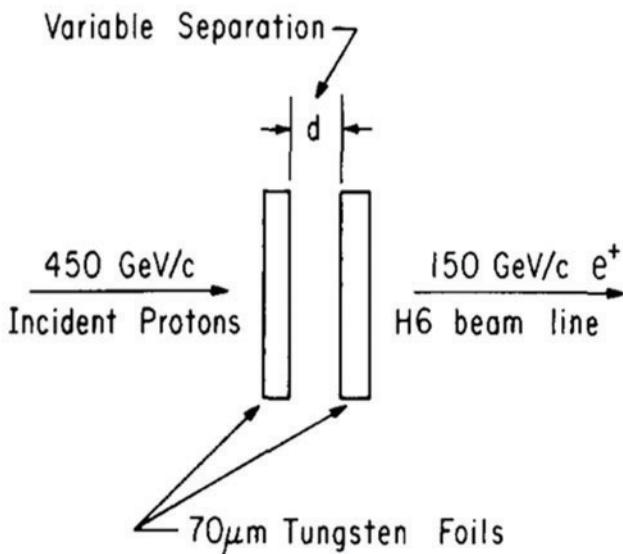


Figure 40

Schematic view of the target for the measurement of the π^0 lifetime. Modified with permission from Reference 82.

image. A collimator at the positron focus reduces the hadron flux by a factor of 300. The positrons are identified by their signal in a large lead-glass block. One of the beam-line elements was a 4.5-T superconducting magnet. The typical synchrotron energy loss in this magnet was ≥ 10 MeV. This radiation was detected by a sodium iodide crystal and was required for the identification of a positron. The hadron contamination in the positron signal was $\leq 10^{-6}$.

The actual runs of the lifetime experiment took place in 1983–1984. It was amazing to observe that the movement of the foils by only 50 μm produced a 5% increase in the positron flux. The measured mean decay length was $46.5 \pm 1.0 \mu\text{m}$. The weak part of the experiment was the need to convert the measured mean decay length to a π^0 lifetime. We made extensive measurements in our beam of the π^+ and π^- energy spectra and assumed that the π^0 spectrum was the mean of the two. On the basis of this assumption, the π^0 lifetime was found to be $(0.897 \pm 0.022) \times 10^{-16} \text{ s}$. A systematic error of $\pm 0.017 \times 10^{-16} \text{ s}$ was added due to the uncertainties in the π^0 spectrum. The Particle Data Group (81) provides a value of $(0.852 \pm 0.18) \times 10^{-16} \text{ s}$. Ours is the only direct measurement; the others were made by the Primakoff effect, which lowered the average value. This was a very satisfying experiment for all involved, but no pillars of physics budged. The measurement was the subject of the thesis of my student Barrett Milliken and was published in *Physics Letters* (82).

13. CONCLUDING REMARKS

For 30 years, I performed experiments at high-energy accelerators. In these pages I have described my pleasures, frustrations, and mistakes as a practitioner of experimental high-energy physics. But the opportunity to work in a small group with a few students and senior colleagues was coming to an end. By the mid 1980s, many of the most important experiments proposed at accelerators were becoming larger and more expensive and often required hundreds of physicists. As I had lived through the golden age of high-energy physics, the evolving nature of the experiments did not

appeal to me. Important as these future experiments were, I realized that they would take place with or without me. For any future experiment in which I would participate, I wanted to make a difference even if the experiment was not of the greatest importance. Thus, I began to look for other areas of research. As far as my publications were concerned, the 1980s were a fallow period.

Since 1985 I have worked in cosmic-ray research. Even the cosmic-ray experiments have become quite large, but at least I was able to create the conceptual design for several experiments. However, the detailed design, construction, and execution of the cosmic-ray experiments involved many physicists who had technical skills beyond my capacity. The details of my last years, 1985–2015, require another story. The present story ends with my departure from high-energy physics. However, in the past 30 years high-energy physics has prospered with the LEP, Tevatron, and LHC colliders. The ultimate discovery of the Higgs boson (83) was a dramatic vindication of high-energy physics in which the close collaboration of huge experimental teams was essential. And last but not least, one could not have succeeded without the ingenuity of the physicists and engineers who designed and built the accelerators.

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Particle Science

Volume 64, 2014

Contents

A Life in High-Energy Physics: Success Beyond Expectations <i>James W. Cronin</i>	1
Hadron Polarizabilities <i>Barry R. Holstein and Stefan Scherer</i>	51
Effective Field Theory Beyond the Standard Model <i>Scott Willenbrock and Cen Zhang</i>	83
IceCube <i>Thomas Gaisser and Francis Halzen</i>	101
Fluid Dynamics and Viscosity in Strongly Correlated Fluids <i>Thomas Schäfer</i>	125
Mesonic Low-Energy Constants <i>Johan Bijnens and Gerhard Ecker</i>	149
Superconducting Radio-Frequency Cavities <i>Hasan S. Padamsee</i>	175
TeV-Scale Strings <i>David Berenstein</i>	197
J/ψ and Υ Polarization in Hadronic Production Processes <i>Eric Braaten and James Russ</i>	221
The First Direct Observation of Double-Beta Decay <i>Michael Moe</i>	247
Weak Polarized Electron Scattering <i>Jens Erler, Charles J. Horowitz, Sonny Mantry, and Paul A. Souder</i>	269
Cooling of High-Energy Hadron Beams <i>Michael Blaskiewicz</i>	299
Status and Implications of Beyond-the-Standard-Model Searches at the LHC <i>Eva Halkiadakis, George Redlinger, and David Shih</i>	319

The Measurement of Neutrino Properties with Atmospheric Neutrinos <i>Takaaki Kajita</i>	343
Properties of the Top Quark <i>Frédéric Déliot, Nicholas Hadley, Stephen Parke, and Tom Schwarz</i>	363
Hard-Scattering Results in Heavy-Ion Collisions at the LHC <i>Edwin Norbeck, Karel Šafařík, and Peter A. Steinberg</i>	383
Index	
Cumulative Index of Contributing Authors, Volumes 55–64	413

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TABLE OF CONTENTS:

- *What Is Statistics?* Stephen E. Fienberg
- *A Systematic Statistical Approach to Evaluating Evidence from Observational Studies*, David Madigan, Paul E. Stang, Jesse A. Berlin, Martijn Schuemie, J. Marc Overhage, Marc A. Suchard, Bill Dumouchel, Abraham G. Hartzema, Patrick B. Ryan
- *The Role of Statistics in the Discovery of a Higgs Boson*, David A. van Dyk
- *Brain Imaging Analysis*, F. DuBois Bowman
- *Statistics and Climate*, Peter Guttorp
- *Climate Simulators and Climate Projections*, Jonathan Rougier, Michael Goldstein
- *Probabilistic Forecasting*, Tilmann Gneiting, Matthias Katzfuss
- *Bayesian Computational Tools*, Christian P. Robert
- *Bayesian Computation Via Markov Chain Monte Carlo*, Radu V. Craiu, Jeffrey S. Rosenthal
- *Build, Compute, Critique, Repeat: Data Analysis with Latent Variable Models*, David M. Blei
- *Structured Regularizers for High-Dimensional Problems: Statistical and Computational Issues*, Martin J. Wainwright

- *High-Dimensional Statistics with a View Toward Applications in Biology*, Peter Bühlmann, Markus Kalisch, Lukas Meier
- *Next-Generation Statistical Genetics: Modeling, Penalization, and Optimization in High-Dimensional Data*, Kenneth Lange, Jeanette C. Papp, Janet S. Sinsheimer, Eric M. Sobel
- *Breaking Bad: Two Decades of Life-Course Data Analysis in Criminology, Developmental Psychology, and Beyond*, Elena A. Erosheva, Ross L. Matsueda, Donatello Telesca
- *Event History Analysis*, Niels Keiding
- *Statistical Evaluation of Forensic DNA Profile Evidence*, Christopher D. Steele, David J. Balding
- *Using League Table Rankings in Public Policy Formation: Statistical Issues*, Harvey Goldstein
- *Statistical Ecology*, Ruth King
- *Estimating the Number of Species in Microbial Diversity Studies*, John Bunge, Amy Willis, Fiona Walsh
- *Dynamic Treatment Regimes*, Bibhas Chakraborty, Susan A. Murphy
- *Statistics and Related Topics in Single-Molecule Biophysics*, Hong Qian, S.C. Kou
- *Statistics and Quantitative Risk Management for Banking and Insurance*, Paul Embrechts, Marius Hofert

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