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***This Was the
Particle Physics
That Was:***

***The Years from
P and C Violation
to CP Violation***

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BROOKHAVEN NATIONAL LABORATORY

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Introduction

As part of the joint Brookhaven National Laboratory-Columbia University celebration of the 1988 Nobel Prize award to Leon Lederman, Mel Schwartz and Jack Steinberger, a special lecture was held at Brookhaven on February 17, 1989. Gary Feinberg of Columbia recreated for us the status and the style of particle physics at that time, and showed how it led to the discovery of the second neutrino, to new generations of particles - and to another kind of generation - a generation of fundamental investigations using the unique probe of high energy neutrino beams.

In order to introduce such a talented speaker, we felt that we would need at least two very capable people. And who could do this better than Maurice Goldhaber of Brookhaven and T.D. Lee of Columbia? The whole event turned out so well, we decided to publish the lecture, and here it is. I hope you enjoy reading it as much as we enjoyed hearing it.

N.P. Samios

Nicholas P. Samios

Introduction

Maurice Goldhaber

On behalf of Brookhaven National Laboratory I am delighted to welcome back the three men who recently reached a pinnacle of scientific acclaim by sharing the 1988 Nobel Prize for Physics: Leon Lederman, Mel Schwartz, and Jack Steinberger. At the time of their great discovery, which was made at the AGS in 1962, they were Professors of Physics at Columbia. They were cited by the Nobel Committee "For the neutrino-beam method and the demonstration of the doublet structure of the leptons through the discovery of the muon neutrino."

In the past, the Nobel Prize has often been given either for answering an important question or for opening up a new research field. But in this case, it is noteworthy that both reasons were cited.

It has been a long journey from the old Brookhaven cafeteria to the table of the King of Sweden! One could conjecture, that one contributing factor for the delay — though admittedly this is not a record-setting delay — lies in the nature of this discovery. It is unlike some other discoveries which also opened up new fields of research and for which the Prize was given as promptly as possible. For instance, x rays, for which the first Nobel Prize in Physics was given in 1901, and high temperature superconductors, for which the Prize was given just a year before this one, were each discoveries which led immediately to an explosion of research. The μ neutrino experiment, however, was a hard act to follow. There are few places where this type of research can be carried out. The experiments are difficult and time consuming, and it took many years for reproducible, significant new results to emerge with the neutrino beam method. Let me mention just two important results: the discovery of neutral currents, and the determination of the Weinberg angle which have helped make the electroweak theory generally acceptable.

The three musketeers, as our laureates have often been dubbed, as well as those who collaborated with them, are all here today. Their collaborators were our own Gordon Danby, Jean-Marc Gaillard, then an exchange-visitor at Columbia from France (now at Orsay and CERN), and two Columbia graduate students,

Dino Goulianos (now at Rockefeller) and Nari Mistry (now at Cornell).

This prize is an honor for Brookhaven National Laboratory. Our laboratory was started soon after World War II with a new concept in mind. It would have facilities that were not usually available at universities, and at which university researchers would be welcome. The guard at the gate was told to let in anyone with a good idea! One of the most important facilities built was the AGS, and the prize also honors all those people who were responsible for its success. This magnificent machine, where dreams have often been turned into reality, was built by many people. Let me single out just a half dozen for their special contributions: Ernest Courant, Stan Livingston, and Hartland Snyder, who discovered the principle of alternating gradient focusing, and Lee Haworth, Ken Green, and John Blewett, who took the lead in building the AGS.

In his blunt way, Isidor Rabi often asked me: "Why don't those three get the Prize?" This occasion would have pleased him greatly, as acclaim both for them and for the two places on which he left such an indelible imprint, the Physics Department at Columbia and Brookhaven. It is sad that he did not live to enjoy our celebration.

The path to this discovery was partially paved by the insight of someone I call the second Pauli, or the Pauli of the second neutrino, Gary Feinberg, from whom we shall hear later. While Gary was a research associate at Brookhaven in 1958, he pointed out that it would be difficult to understand why the muon did not decay readily into an electron and a γ ray if there were only one kind of neutrino. He had to use a subtler argument than the first Pauli, who could rely on such hallowed conservation laws as energy and statistics. At Brookhaven and elsewhere the feeling was in the air that " μ -ness" was conserved, and that the muon was trying to tell us something more general. Gary pursued his work with Steve Weinberg, and then T.D. Lee and Frank Yang strengthened the argument for a second neutrino further.

Lee Haworth, as Director, had a steering committee for the Cosmotron consisting of most high energy physicists at Brookhaven with one outsider, Bob Serber. About the time when I became Chairman of the Physics Department, in May 1960, Lee Haworth also appointed me Chairman of a high energy advisory committee, which would now have to consider experiments not only at the Cosmotron but also at the AGS, which was then nearing completion. Rod Cool became Secretary of the Committee. A few original members of that Committee are here today: Ralph Shutt, Ronnie Rau and two university representatives, Bob Serber and Bob Adair. Our way of sifting proposals for accelerator experiments became a worldwide model as did many other Brookhaven contributions, organization, al, technical, and scientific. We now compete cheerfully with our intellectual offspring.

The Columbia proposal for the search for a second neutrino was approved by the advisory committee. Now let me tell you a story which the three musketeers may still remember. In the spring of 1961, while Trude and I were on a Nordita Lecture tour, they conveyed to me, through Rod Cool, the urgency with which they wanted to do their experiment, suggesting that they use the only available beam, which led to the hydrogen bubble chamber; if I would allow the bubble chamber to be taken out for six weeks, they would finish their experiment and the bubble chamber could return. As much as I sympathized, I felt that I had to save them from their youthful enthusiasm. I was skeptical of their promise to finish this important experiment in six weeks, and insisted that they would have to wait for their own beam so that they could do their work in whatever time it took. I therefore assigned a beam for their sole use. In the end, it took them eight months to accumulate enough data to demonstrate clearly, in a beautifully executed experiment, that there was indeed a second neutrino. During that time, the AGS had met all the stringent demands. Incidentally, Nick Samios reminds me that there was a bonus from two independent beams: In the uninterrupted bubble chamber run, he and his group found the Ξ^* , so important for SU(3). Brookhaven had a well-attended press conference to announce the arrival of the μ neutrino, and the

whole world quickly learned of this magnificent discovery. A new conserved quantum number was born, now called Lepton Flavor.

But physicists cannot leave well enough alone. There is a trend to question conservation laws, or at least to test the limits of their validity. One of the important research frontiers of today is the effort to find whether there are some unknown interactions which can turn muons into electrons, or muon neutrinos into electron neutrinos. Brookhaven is in the forefront of these searches and researches, and if the three musketeers would like to join the fun and make another proposal they can be assured that the present committee is kinder and gentler; they even vote. But I should warn you that Jeremy Bernstein made a suggestion long ago that we should put up a sign at the AGS: Only one Nobel Prize to a customer. I never got around to it, but perhaps Nick will do so.

Let me close with a few remarks about our laureates. After leaving Columbia they have had continued successes.

Jack Steinberger, in his wisdom, has reached the formal retirement age at CERN without ever having been an administrator with a capital A. He is still very active in research, and is completing a huge detector for the new CERN electron-positron collider, LEP. He calls his detector Aleph. In this century, the nuclear and particle physicists have essentially used up the Greek and Latin alphabets, partly due to Brookhaven discoveries. Perhaps Jack turned to the Hebrew alphabet to give himself room for new particles which he may discover at LEP.

Mel Schwartz achieved something which may be as rare for physicists as a Nobel Prize: He became his own boss, a successful entrepreneur. He runs a company which he has imaginatively named "Digital Pathways", Inc., located in Mountain View, California. This is one way to escape High Energy Advisory Committees.

Leon Lederman became the Director of Fermi Lab, and the place is humming. He has become such a well-known TV personality that my Secretary first spelled his name with two t's. What is worrisome is the prospect that he will soon vie for the title of "Funniest Director Emeritus".

Introduction

T.D. Lee

Very rarely do we encounter an experiment which is so spectacular at the time of its execution and which later on becomes even more important with time. When that happens we know we have met a "true great" and we all feel privileged. I think the award of the Nobel Prize to this discovery of two neutrinos, twenty-six years after the discovery, is a clear tribute to the greatness of the discovery.

There are perhaps three aspects which I may emphasize. One, of course, is the discovery itself. First, one observed the μ -neutrino; and by that same observation, one also discovered the e -neutrino and thereby resolved a paradox: the absence of the $\mu \rightarrow e + \gamma$ decay. But there is a second element: this particular experiment was the beginning of the high energy neutrino program. It launched a brand new direction and changed the course of particle physics for more than twenty-five years. It also added to the massiveness of detectors. The neutrino experiment was 10 tons. If you plot the weight of detectors versus time, I think you will find that the curve has a slope discontinuity in 1962. Since then it has risen from 10 tons to the multi-kilotons of LEP detectors.

I think there is a third element for this discovery and that is its impact on the quest for the pattern of fundamental building blocks. What we all like is a quest for one — we live in one world, and there is only one universe. We would like everything to be made of one fundamental unit, maybe the super string, which is governed by one theory, maybe the super, super

theory, developed by one physicist — the super, super, super physicist.

The oneness is good in dreams, but hard to realize. So we may embark on a quest for two. In the thirties we had as strongly interacting particles the proton and neutron. And then there was another group of the light particles, electron and neutrino. Each group has two members: one charged and one neutral. This was pre-World War II. Then after World War II, the muon came, so that made a family of three. Then with cosmic radiation and the Cosmotron, came the Λ . And that stimulated the Goldhaber model and the SU3 theory of Sakata, which in turn then evolved to the SU3 of Ohnuki, Gell-Mann and Ne'eman. So that became three.

We then had two groups. Each had three members. For the hadrons, proton, neutron, or, in terms of more basic units, up, down and strange quarks. The other three are leptons: neutrino, electron and muon. It was the discovery of two neutrinos that made the leptons into four. As you all know, four is not divisible by three and that made a fundamental impact. It took more than a decade to realize that the only way to make two and three commensurate is to have six — three families with two members each. That is the state that we are in today.

It is a great pleasure for me to join with all of you in paying homage to one great laboratory, Brookhaven, to two elusive particles, the e -neutrino and μ -neutrino and to three superb physicists.

This Was the Particle Physics That Was: The Years from P and C Violation to CP Violation

This talk is meant to evoke something of what particle physics was like in the period leading up to the first high energy neutrino experiments. I will therefore give the talk in the mode that was then universal, i.e., I will not use transparencies.

The period that I will speak about is not literally that described in the subtitle, which would begin in late 1956, but rather begins about a year later, after the space-time structure of the weak currents had been settled, on the one hand by the theoretical work of Richard Feynman and Murray Gell-Mann, Robert Marshak and George Sudarshan and John Sakurai, and on the other hand by the experiments of Lee Grodzins, Andy Sunyar and Maurice Goldhaber on the helicity of the neutrino emitted in beta decay.

I want to avoid a common trap and so I will not refer to the period in question as the 'Golden Age' of particle physics. To paraphrase what someone once said in another context, the Golden Age of physics is when you were twenty-five years old. I was twenty-five at the beginning of the period I will discuss, but I am content to leave it to historians to decide more objectively the merits of the accomplishments of physicists in different time periods. I think that when I am finished you will agree that the particle physicists who worked in the years from 1958 to 1964 had a great deal to be proud of. I was a postdoc at Brookhaven from 1957 to 1959 and a faculty member at Columbia for the rest of the period, two institutions where much of the action I will describe took place, so I had a pretty good chance to observe what was going on. Of course, many of the trends that occurred are much easier to observe through hindsight than with direct vision.

I will begin with a few general remarks about the sociology of particle physics then and now. Looking back on those days and comparing them with what has happened since, a number

of striking differences are immediately apparent.

1. Experiments in particle physics in the late 1950s and early 1960s were done by what would now seem like minigroups of physicists. Figure 1 shows the number of authors of each experimental physics paper published in **Physical Review Letters** in the second halves of 1962 and 1988. The plots hardly overlap at all. The averages were six authors per paper in 1962, and forty-seven per paper in 1988. There is even one experimental particle physics paper in 1962 with a single author! (named Burt Richter). The two-neutrino paper¹, which had seven authors, was fairly typical, in this respect, of papers published in 1962. In 1988, several of the published papers have more institutions involved than there were authors of most of the 1962 papers. Even the size of the typical experimental groups in 1962 represented a major expansion compared to the situation twenty-five years earlier, when it was common for a single scientist to conceive an idea for an experiment and bring it to realization within a few days or weeks. This earlier situation is well described in a recent memoir by Maurice Goldhaber. Thus within the working lifetime of many of those present today, experimental particle physics has gone from an individual effort, to an effort by a small group of close collaborators, to a team effort by scores of individuals, to what, at the next generation of accelerators, may require the combined efforts of whole countries of physicists.

2. Particle physics was a much larger fraction of the total effort in American physics in 1962 than it has become lately. The domination by condensed matter physics had not yet taken hold. On comparing the publications in **Physical Review Letters** in a similar number of issues in 1962 and 1988, I found on the one hand in the earlier year forty-nine papers were published on experimental particle physics and

Figure 1. For each paper in experimental particle physics published in July-December of 1962 and 1988 in *Physical Review Letters*, the number of authors is plotted on the ordinate. The scale for the 1962 papers is given on the right axis, that for the 1988 papers on the left. The abscissa is used only to order the papers by number of authors.

an approximately equal number of papers were published in experimental condensed matter physics. On the other hand, in 1988, only eight papers were published in experimental particle physics and over a hundred were published in experimental condensed matter physics. The priorities of American physicists have evidently changed considerably over this period.

3. A third sociological change that has taken place over this period involves the broadening of participation in experimental particle physics. The forty-nine papers that I just mentioned as published in the second half of 1962 involved authors from twenty-five US institutions, and seven foreign institutions, and the majority of the authors were from just a few places, such as Brookhaven, Lawrence Radiation Laboratory, Columbia, and Cornell. For the eight papers published in 1988, however, the authors came from thirty-five US institutions and twenty-six foreign institutions, and there was no trend towards dominance by any small group of institutions.

4. Finally, there has been a pronounced shift from experiment to theory in particle physics,

at least insofar as this can be measured by the number of papers published. In thirteen issues of *Physical Review Letters* published in 1962, there were twenty-seven papers in theoretical particle physics, and forty-nine papers in experimental particle physics. In 1988, in the same number of issues, there were thirty theoretical papers and eight experimental papers. The ratios of theory to experiment for 1988 are about the same for papers in *Physical Review D*, *Physics Letters B* and *Nuclear Physics B*. If I add together all the current letter journals, then the total number of experimental publications in 1988 is about the same as in 1962, but the number of theoretical papers is seven times greater. I believe that this rapidly diverging production of theoretical papers is an indication of a disturbing trend in our field, about which I will comment later.

What We Knew in 1960

I turn now to the actual content of particle physics in 1960. To give some idea of the state of knowledge in particle physics in 1960, Table 1 lists the 'elementary particles' known at that time.

Table 1. The particles known and considered to be elementary in 1960.

Leptons	electron muon	e-neutrino mu-neutrino ??
Mesons	π -plus, π -zero, π -minus K-plus, K-minus K-zero, K-zero bar	
Baryons	proton, neutron lambda-zero sigma-plus, sigma-zero, sigma-minus xi-zero, xi-minus	
Gaugons	photon graviton ??	

There were three known leptons, with suspicions of a fourth. Among the hadrons, there were seven mesons, with hints of many more that were about to be discovered. There were eight baryons, and again indications that many more were to be found. In fact, many of the vector mesons and excited baryons were discovered in the two years after 1960. In addition, there was one known gauge particle, the photon, another firmly believed in, but unobserved, the graviton, and suggestions of others, such as the W particle.

The passage of time has not been altogether kind to what we thought we knew in 1960 (Table 2).

Table 2 The particles known and considered to be elementary in 1989.

Leptons	electron muon taunon	e-neutrino mu-neutrino tau-neutrino
Quarks	up charmed top ??	down strange bottom
Gaugons	photon graviton ?? W gluons	Z

The known leptons have survived as elementary particles, supplemented by their heavier cousins. The hadrons have been erased as elementary, and replaced by the quarks, of which

there are now as many, if we include color, as there were known hadrons in 1960. The gauge bosons have had the most spectacular increase in number, rising to thirteen, if we include the yet unobserved graviton.

In 1960 as today, much of what we knew could be summarized in terms of conservation laws, which came in two forms, those thought to be exact, and those thought to be at best approximate. Again, the lists have undergone some profound changes. Only one new exact conservation law has been discovered, that of color, whose influence is muted by the fact that all forms of ordinary matter have zero for the conserved quantity. One conservation law, that of CP, then thought to be exact, has definitively been moved to the approximate list. Several others, including baryon number and the number of each of the types of leptons are thought to be only approximate, but evidence is lacking. It is also unknown whether the total fermion number is exactly conserved.

As for the approximate conservation laws, we have found a few more, corresponding to the new hadronic quantum numbers of charm, bottomness and presumably topness. These, together with uppityness, downiness and strangeness are all conserved in strong interactions. But our attitude towards the breakdown of these conservation laws in weak interactions has undergone a subtle change. In weak interactions, half of these conservation laws break down, because of the interaction of the quarks with the W boson, which causes transitions within the quarks of one generation. But for another aspect of the breakdown of approximate conservation laws, that involving inter-generational transitions such as K meson decay, we now believe that such decays are one manifestation of a mismatch between the quark states with definite masses and those with definite weak interactions, a notion already introduced by 1960 as particle mixing. The precise reason why this mismatch should occur remains elusive, and is presumably related to the origin of mass itself.

The 'armament' of theoretical particle physicists in 1960 was much more restricted than it later came to be. We could do perturbative calculations in renormalizable field theories, although with rare exceptions these were not carried out beyond one or two loops. While the

renormalization group had been introduced some years earlier, little use had been made of it to break out of perturbation theory.

The set of known renormalizable theories was not large, including only QED, non-derivative couplings of spinless mesons to nucleons, and ϕ^4 self-coupled meson theories. While non-Abelian gauge theories had been described by Chen-Ning Yang and Robert Mills some years earlier, little use had been made of them, because it was not known how to give the quanta finite mass, and this was deemed necessary in order for the gauge theories to describe anything physical. For the same reason, it was not yet known that such theories could be made renormalizable. Indeed, various proofs had been published to the effect that they were not. Nevertheless, there were some attempts, notably by Julian Schwinger and by Sheldon Glashow to use non-Abelian gauge theories to unify weak and electromagnetic interactions, culminating in Glashow's 1961 article² in which the $SU(2) \times U(1)$ gauge group was first used.

The basic ideas that ultimately made the standard model possible were not yet clearly recognized in 1960. Spontaneous symmetry breaking and degenerate vacua had been discussed already several years earlier by Werner Heisenberg in the context of his non-linear 'theory of everything', but had met with a frigid reception from the reigning priests of quantum field theory. Yoichiro Nambu's effort to find an analogy in particle physics to the symmetry breaking that takes place in the BCS theory of superconductivity seemed to run afoul of the Goldstone theorem. Schwinger had raised the possibility that this theorem might not be relevant to gauge theories, but Peter Higgs' specific realization of that possibility was yet several years away.

Although the notion of higher symmetries of strong interactions had been discussed for several years before 1960, there was not yet any clear indication that these ideas were relevant to physics. The large mass difference between strange and non-strange particles seemed to make useless theories that placed both types into a single multiplet. The only clearly valid symmetry for strong interactions was isospin. Already, there had been an attempt by Sakurai to 'gauge' isospin and to identify the quanta of

such a gauge theory, given mass by means unknown, with the physical vector mesons whose existence was being suggested by electron-nucleon scattering experiments. These suggestions eventually seemed to be borne out by the discovery of the ρ meson, but as we now know, the connection of the ρ meson to gauged isospin turned out to be an illusion. What seemed like an even wilder possibility was suggested in unpublished work by Feza Gursey and myself in 1959, and probably by others as well. This was a unification of the ideas of Yang and Enrico Fermi with those of Yang and Mills. The known spin-zero mesons were all taken to be composites of baryons and antibaryons, bound by forces generated by the exchange of the Yang-Mills isospin gauge particles. Although this idea contains the germ of the ultimately successful color $SU(3)$ theory of hadrons, too many of the crucial ingredients were missing for it to have been taken seriously, even by Gursey and myself. Both the notion of confinement, which allows us to have quarks and color and yet not see them, and the notion of asymptotic freedom, which allows us to do calculations with confined quarks, remained ideas for the future to bring forth.

The Discoveries of the Early 1960s

The period from 1960 to 1965 was an extremely fruitful one for particle physics, although at the time it did not always seem to be that way. A series of experimental discoveries, and a somewhat unrelated series of theoretical ideas laid the groundwork for much of what has come to be known as the standard model. Apart from some of the work that led up to the two-neutrino experiment, I will describe these discoveries only very briefly, as each of them by itself could well be the subject of a talk like this one.

1. The Filling in of the Particle Spectrum.

Perhaps the most striking discovery of the early 1960s was the recognition that there were hundreds of hadronic particle states beyond those known previously. These new states did not carry new internal quantum numbers; instead they were similar to previously known particles, but with greater mass, and sometimes with higher spin. Although

some of these, such as the ρ meson had been anticipated, most of them were unexpected, and certainly nobody expected new particles to appear in such numbers.

An obvious consequence of the discovery of the many new hadrons was that they made possible a precise determination of the correct approximate symmetry of the strong interactions. While there were a number of candidate symmetries that described the previously known quasi-stable hadrons, only what we now call flavor SU(3) survived the test of classifying the excited baryons and mesons. This line of reasoning culminated in 1964, when Nick Samios and his coworkers at Brookhaven found the first Ω^- particle³, with the properties predicted for it by SU(3).

But in spite of this great success, I think that the first main effect of the discovery of the multiplicity of hadrons was psychological. It put an end to the idea that the old familiar particles such as the nucleons and the pions were somehow the basic constituents of matter, and opened the way for the present notion that the forms of matter that are best known to us may be only incidental aspects of a much richer subatomic universe. Of course, that is an idea which may not have yet reached its final fruition, as we are probably many orders of magnitude away in energy from probing the final secrets of matter. With the removal of the idea that pions and nucleons were somehow fundamental, the way was opened to look for something that was more fundamental, and in a few years Gell-Mann and George Zweig responded with quarks and aces.

2. The Discovery of CP Violation

It is just about twenty-five years since Val Fitch, Jim Cronin and their coworkers surprised the world of particle physicists by their discovery⁴ that the long-lived neutral K-meson decays into two pions with a branching ratio of about 10^{-3} . This experiment, like the two-neutrino experiment, was carried out at Brookhaven, and again like the two-neutrino experiment involved what by present standards would be considered a minigroup, containing only four investigators. I think that it is fair to say that this discovery was unexpected, although it is conceivable that some theorist had written a paper predicting it. There are two

main reasons why the discovery of CP violation came as such a surprise. One is that after the discovery of P and C violations, physicists became comfortable with the proposal by Lev Landau, and by Lee and Yang that the 'true' mirror symmetry was CP invariance. To become stripped of that remainder of an earlier time when space-time symmetries were all exact seemed to plunge us into uncharted territory, where there was no real guide to how we should proceed. The other reason for the surprise was that the extent of CP violation, as measured by the ratio

$$R = K_L \rightarrow 2\pi / K_S \rightarrow 2\pi$$

was so small. Indeed, several years before the Fitch-Cronin result, an experiment by Leon Lederman and coworkers had put an upper limit on the ratio R which was not much larger than the actual value found in 1964. Again, the experience with parity, where the observed violation was maximal, had convinced us that arbitrary small coefficients did not appear in physical phenomena.

I think that it was the combination of these two reasons that initially made many theorists grope for alternative explanations to CP violation for the decay $K_L \rightarrow 2\pi$. None of these turned out to be right, and after a few years, everyone was convinced that CP violation was a fact.

The reason for CP violation is another matter. At the time of its discovery there were various hypotheses put forward about the origin of CP violation. Most of these were soon disproven experimentally. One of the survivors was Lincoln Wolfenstein's 'superweak' interaction model, which attributes CP violation to a new interaction that is smaller than the weak interaction. This model, which essentially attributes CP violation to mass mixing between K^0 and $\bar{K}^0$, predicts that the branching ratios of K_L and K_S into different 2π modes should be equal.

It was recognized by Makoto Kobayashi and Toshihide Maskawa⁵ that another viable model of CP violation existed if there were three generations of quark doublets. In the KM model, CP violation is attributed to the occurrence of a complex phase in the matrix that describes the mixing of the different types of quarks. Such a phase can be eliminated for two

quark generations, but not for three or more. The KM model generates CP violation both through induced K^0 - $\bar{K}^0$ mass mixing and through an additional term of direct CP violation. The latter term seems to have been observed recently in experiments at CERN and Fermilab, which show a difference between the $2\pi^0$ to $\pi^+\pi^-$ branching ratios of K_L and K_S .

It should be emphasized that even though it appears that what we know of CP violation is well parametrized by the KM quark mass mixing matrix, this does not mean that we have yet understood the fundamental origin of that violation. In fact, we do not yet really understand how any aspect of the quark mass matrix arises. Gaining that understanding is the main intellectual argument that has been given for the construction of the Superconducting Super Collider. For example if, as many people expect, CP violation arises through some spontaneous symmetry breaking among an extended set of Higgs mesons, then there is every reason to expect that at energies much higher than any we have yet explored, the pattern of CP violation is much more complicated than anything that can be described by a single complex number in the quark mass matrix. In particular, as pointed out by Scott Dodelson, most of the models that have been put forward to explain baryogenesis in the early universe require many more complex parameters in the Higgs interactions than appear in the KM matrix, so that we cannot use what is known of CP violation at low energies to calculate the baryon asymmetry.

3. The Discovery of the Muon Neutrino

a. The New Experimental Possibilities

Several distinct streams flowed together to form the experiment that discovered the muon neutrino. By far the most important of these streams was the recognition by Mel Schwartz that experiments with high energy neutrino beams were possible. In his first paper⁶, published in *Physical Review Letters* in March of 1960, he clearly laid out the directions of future experiments. These included the use of high energy pion beams as the source for the neutrinos, so that a 3 GeV proton beam of 10^{12} protons/sec would generate a flux of about 5000 high energy neutrinos/cm²/sec at the target. Schwartz discusses the need for large

amounts of material as the neutrino detector, using a figure of ten tons. Even with this large detector, relatively low event rates are to be expected, and Schwartz calculates one event per hour for each 10 tons of detector.

It is very interesting to compare these numbers with the actual numbers in the two-neutrino experiment carried out two years later¹. The experiment was done at the Alternating Gradient Synchrotron, which then produced about 3×10^{11} protons/sec, at a somewhat higher energy than Schwartz had considered. The detector was a ten ton spark chamber. The use of a spark chamber was not specifically envisioned by Schwartz, as I believe that none had been successfully operated in 1960. The running time for the experiment seems to have been about three hundred hours, and produced about one hundred events, of which about one-half were used to draw conclusions. Thus the results of the first high-energy neutrino experiment conformed very closely to the suggested parameters described by Schwartz.

In view of that agreement, I was quite surprised when I recently reread Mel's paper, and found that after giving the numbers mentioned for a proposed experiment, he wrote the following: "This estimate places the experiment outside the capabilities of existing machines by one or two orders of magnitude."⁶ He then goes on to discuss the possibility of doing it at several proposed machines, most of which were never built. He does not specifically mention the AGS, which was then a few months away from operation, but I imagine that he was including it among 'existing machines.' Why then was he so pessimistic about an experiment that was successfully done within about two years?

I should let Mel answer that question, but let me offer one bit of speculation. I have already mentioned the gradual change in the sociology of experimental particle physics over two generations. Part of this change was the widening of physicists' time horizons, from considering only experiments that could be done in a few days, to the present situation, in which physicists blithely plan on experiments that will be done ten years from now, by graduate students presently still in high school, on machines that are still to be funded. Mel's

paper was one of the first, if not the first, to expand that horizon to periods of several years. With that change in the time horizon, it became much more difficult to estimate future possibilities, and it was perhaps prudence on Mel's part to be cautious rather than overly optimistic about what could be done. Evidently, his collaboration with Leon Lederman, Jack Steinberger, Gordon Danby, Dino Goulianos, Jean-Marc Gaillard and Nariman Mistry was able to squeeze the extra one or two orders of magnitude out of an "existing machine." (Mel tells me that the reason for his written pessimism about the immediate prospects for high energy neutrino experiments was that the design intensity of the AGS was only 10^9 protons/sec. Thus it was the fact that the actual intensity of the AGS beam was much higher than anticipated that allowed neutrino experiments to be done there.)

b. The Test of a New Conservation Law

The second stream that led to the two-neutrino experiment was the recognition of a need for a leptonic conservation law in addition to that of total lepton number. This need arose from the fact that neutrinoless conversions of muons into electrons were not observed to occur. For example, an experiment⁷ by Steinberger and his student Subramanian Lokanathan in 1955 limited the branching ratio for the decay $\mu \rightarrow e + \gamma$ to less than 2×10^{-5} .

The idea that the non-occurrence of neutrinoless $\mu \rightarrow e$ transitions could be understood by the assignment of different quantum numbers to muon and electron had been put forward by Hormoz Mahmoud and Emil Konopinski⁸ in 1953, in the context of a single four component Dirac neutrino. That proposal was later taken up by Schwinger⁹ and by Kazuhiko Nishijima¹⁰ in 1957. Nishijima also indicated that this assignment would forbid the neutrinos from $\pi \rightarrow \mu$ decay from producing electrons.

What was missing from these early suggestions of the need for a new conservation law was any indication of how significant a problem the absence of neutrinoless muon decay was. After all, one could readily have taken the attitude that it was not a conservation law that forbade such decays, but rather the absence of a specific interaction that would allow it to occur.

My own involvement with neutrinoless muon decays began in a quite indirect way. I spent the year 1956-1957 as a Postdoctoral NSF Fellow at the Institute for Advanced Study in Princeton, where I shared an office with Pasha Kabir. One day, while waiting at the train station, we met Robert Oppenheimer, then the Institute's Director. (Pasha remembers the conversation as instead being in Oppenheimer's office). He mentioned to us a recent report about a bubble chamber experiment at Berkeley by Luis Alvarez and coworkers, which described events in which a negative muon stopped, and a short time later was ejected at high speed. Oppenheimer thought that this observation might be interpreted as implying that there was both a heavy and a light muon, and that Alvarez was observing a radiative transition between them. He proposed that Kabir and I should calculate the expected transition rate. Actually, Alvarez had discovered not a heavy muon but rather muon catalysis of fusion. Nevertheless, Kabir and I set about looking into radiative transitions between two fermions of different mass.

Our effort led to a result that surprised us very much. Instead of coming out with a finite decay rate, we ended up with zero! What we did for the calculation was something that later came to be known as mass mixing. That is, we took an interaction of the form:

$$H_{\text{int.}} = \lambda \bar{\psi}_H \psi_L + \text{H.C.}$$

This interaction, together with the usual electromagnetic interaction, generates two Feynman diagrams for the decay $F_H \rightarrow F_L + \gamma$. But a simple calculation shows that the contribution of these two graphs cancels, giving no decay amplitude. One does no better by taking a derivative interaction Hamiltonian and introducing electromagnetism by the minimal interaction rule

$$H_{\text{int.}} = k \bar{\psi}_H (\partial - ieA) \psi_L + \text{H.C.},$$

a procedure that came to be known as current mixing.

For this case there are three Feynman graphs, which again combine to give zero decay amplitude. A few years later¹¹, Steve Weinberg, Pasha and I proved a general theorem that such mixing could always be transformed away and that it gave no transitions, so long as all

the other interactions of the two species of fermions were symmetric.

The conclusion that we drew in 1957 was that to generate electromagnetic transitions between two fermions, something more than simple mixing was required. It would be necessary to involve some internal structure of the fermions, such as that induced by the higher order effects of weak interactions. But at that time the Fermi theory was generally accepted, and while that theory would generate a decay amplitude for $\mu \rightarrow e + \gamma$, it would appear only in order G_F^2 , which we then thought would make it insignificant.

The situation changed considerably within a year. The proposal of a universal V-A weak interaction, by Feynman and Gell-Mann, Marshak and Sudarshan, and Sakurai, and the rapid indication that their theory was consistent with experiment, reopened a possibility that had been suggested some years earlier, that the known weak interactions are mediated by the exchange of a vector boson, which soon came to be known as the W particle. While this intermediate vector boson (IVB) hypothesis was consistent with all known observations, it was not seen to have any immediate consequences that could distinguish it from a theory in which the weak interaction was a sum of products of various currents. However, one such consequence soon suggested itself to me.

Figure 2. A Feynman diagram contributing to the decay $\mu \rightarrow e + \gamma$ in the IVB theory.

In the IVB theory, the decay $\mu \rightarrow e + \gamma$ should occur to first order in G_F , through the pictured Feynman graph (Figure 2). This would mean that the rate for $\mu \rightarrow e + \gamma$ should not be suppressed compared to that of ordinary muon decay by any powers of G_F . However, this was a loop graph, the first such that had been consi-

dered in the IVB theory. It was known from work in the 1950s that a theory with massive vector bosons, interacting with photons and leptons, was not renormalizable because of the high energy behavior of the longitudinal terms in the vector boson propagator. Nevertheless, I felt that at least some estimate of the rate for $\mu \rightarrow e + \gamma$ could be obtained by calculating the Feynman graph and so I set about doing it in early 1958, when I was a postdoc in the Brookhaven Physics Department.

The calculation was reasonably straightforward, although it was complicated by the fact that the answer was logarithmically divergent, and some gauge invariant method was needed to produce a definite answer. In doing the calculation, I was greatly helped by advice from the late Ralph Behrends, also at Brookhaven then, who was one of the few people who had calculated radiative corrections to weak interactions. Since dimensional regularization had not yet been invented, I used Pauli-Villars regularization. Another complication was that we did not really know what the proper electromagnetic theory was for the charged vector boson. The logarithmic divergence that I mentioned occurs for the minimally coupled theory, corresponding to a boson with a magnetic moment of one magneton. I remarked in my paper¹² that if the boson was given a moment of two magnetons, the answer came out to be finite. I believe that this is the value that corresponds to the Glashow-Weinberg-Salam theory, so that if that theory had been available at the time, there would have been a unique prediction for the $\mu \rightarrow e + \gamma$ rate.

The result of both this finite calculation¹², and of the calculation with any reasonable value of the cutoff gave a branching ratio for $\mu \rightarrow e + \gamma$ compared to ordinary muon decay of about 10^{-4} , which was considerably above the existing limit of Lokanathan and Steinberger. Subsequent searches for $\mu \rightarrow e + \gamma$ over the next few years pushed the limit down by several orders of magnitude. This showed a clear contradiction with the IVB theory under the assumptions that had been made.

My own view at the time was that this result showed there was something wrong with the IVB theory. In fact in a letter that I wrote to my then current girl friend I paraphrased a remark made by Joe Louis about someone he was

going to box, Billy Conn, who was noted for his fancy footwork, and I said about the W bosons, "They can run, but they can't hide."

I did realize that there was another way out which could save the IVB theory. If the neutrino emitted by the muon was different than that which could be absorbed by the electron, then of course the graph I calculated would give zero. This difference in neutrinos would allow for the possibility of assigning different quantum numbers to electron and muon. While this procedure amounted to introducing a second two-component neutrino, I phrased that possibility somewhat differently in a footnote to my paper. Taking advantage of the fact that there were two 'unused' components of the original Dirac neutrino, I suggested that the muon interacted with two components orthogonal to those with which the electron interacted. It was actually known from work by Wolfgang Pauli and others that this was mathematically equivalent to introducing a second two component neutrino. But at the time, it seemed to me and to others that the language using a single four component neutrino was superior. In retrospect that was not the case, since thinking of the interactions as involving a single four component neutrino cannot be extended beyond two generations, while the number of two component neutrinos could be extended indefinitely, as we now have done to three.

My calculation of the rate for $\mu \rightarrow e + \gamma$ was published in June of 1958. It was in fact the last paper to be published as a letter in **Physical Review**. Simon Pasternack, then the editor, gave me a choice between that and having it in the first issue of **Physical Review Letters**, which came out in July 1958. Since I was uncertain of the future prospects of **Physical Review Letters**, I chose to stay with tradition. The view that there were probably two distinct neutrinos gained support over the years directly following the $\mu \rightarrow e + \gamma$ calculation. Builders of group theoretical models of the weak interactions, such as Glashow in his 1961 paper on $SU(2) \times U(1)$, found two neutrinos more convenient than one. Searches for other neutrinoless transitions between muon and electron, such as $\mu \rightarrow 3e$ or $\mu + \text{nucleus} \rightarrow e + \text{nucleus}$ also gave negative results, strengthening the case that a conservation law was at work.

A strong additional argument for this view was given by Lee and Yang, who pointed out that at sufficiently high energies the Fermi theory would violate unitarity. Whether or not W bosons existed, some means would have to be found to cure this disease. Whatever that cure was, it would surely introduce some structure into the weak interaction matrix elements, and such a structure would, in the absence of a selection rule, allow the $\mu \rightarrow e + \gamma$ decay to take place, at a rate that would presumably not be too different from the prediction of the IVB theory. There seemed no way out from the conclusion that a conservation law to forbid the decay was required, and given the other experimental data about muons and electrons, this was possible only if there was more than one type of neutrino. The question then became how to detect that other neutrino.

Incidentally, in the modern age, when all global conservation laws have come to be thought of as only approximate, there has been a revival of interest in searching for $\mu \rightarrow e + \gamma$ and similar decays that change the muon number. The present limit for the branching ratio is something like 10^{-10} and there is an experiment in progress at Los Alamos to look for $\mu \rightarrow e + \gamma$ at the level of 10^{-13} . There is little firm theoretical guidance about the rate one should expect. Various effects, such as neutrino mixing would generate $\mu \rightarrow e + \gamma$, but with really tiny branching ratios. In my opinion the theoretically most promising vehicle leading to an observable rate for $\mu \rightarrow e + \gamma$ involves introducing and breaking some sort of leptonic flavor gauge group, but even that idea has not led to any sharp predictions for $\mu \rightarrow e + \gamma$.

It is also worth mentioning that it still is not known whether the muonic conservation law is additive as usually assumed, or multiplicative, as suggested by Weinberg and myself, and others almost thirty years ago. When we made the latter suggestion, we did not have any plausible model that led to a multiplicative conservation law. However, there now is such a model, in which the neutrinos are taken to be Majorana particles with mass. In that model, transitions between muonium and antimuonium are allowed with a rate proportional to G_F^2 , whereas $\mu \rightarrow e + \gamma$ is not, unless additional neutrino mixing is introduced, so that there is at least an approximate multiplicative conserva-

tion law. (Actually there are up to three multiplicative conservation laws in that model, one for each lepton generation with a Majorana neutrino.) The search for muonium-antimuonium transitions is now approaching the level where an effect might be expected if the law is multiplicative, and it will be interesting to see what these searches give.

c. What Else Can Be Learned From Neutrino Experiments

The final stream that combined to generate the two-neutrino experiment was the recognition that there were many open theoretical questions about weak interactions that might be answered through such experiments. The existence of two neutrinos was only one of these questions. A good summary of the situation in 1960 was given in a paper by Lee and Yang¹³, published in the *Physical Review Letters* directly following Schwartz's paper on the possibility of high energy neutrino experiments. Some of the questions discussed in the paper by Lee and Yang were the following:

1. Are there neutral lepton currents?
2. Do the leptons interact at the same space-time point in the weak current?
3. What are the isospin properties of the weak hadronic currents?
4. What happens to weak interactions at very high energies?
5. Are the weak interactions mediated by an intermediate vector boson?

While several of these questions had been previously posed in various contexts, the suggestion that they could all be investigated by a single experimental technique played a powerful role in stimulating experimentalists to develop that technique. All of these questions were in fact investigated through neutrino experiments over the following decade or two, and all but the last of them were answered through those experiments. The nondiscovery of the W-boson by neutrino experiments was a consequence of its high mass, then unexpected by anyone other than Schwinger and Glashow. The rest of us, misled by the fact that no particles known in 1960 had rest energy more than 2 GeV, expected the W mass to be in that range as well, in which case it would have been easily found in the later neutrino experiments.

One important aspect of the influence of the Lee-Yang paper was that it was an indication of the close ties between theory and experiment in that era of particle physics. At least at Columbia and Brookhaven, and, I am sure, also at the other institutions where particle physics research was carried out, there was an intense interaction between theorists and experimenters, which was beneficial to both groups and to the progress of physics. The fact that in physics theorists and experimenters have something constructive to say to one another seems altogether obvious to us, but it is a point that can be entirely missed by people in other branches of science.

Some researchers in other sciences have criticized particle physics because experimenters often do experiments suggested to them by theorists. Even a little reflection should make them recognize that this situation is actually a beneficial consequence of the fact that particle physicists have some theoretical understanding. If such collaboration does not occur in other fields, it may indicate a lack of any effective theories.

Unfortunately, it seems to me that the intense collaboration between theorists and experimenters in particle physics has waned, and is in some danger of disappearing. One indication of this is the change that I mentioned earlier in the ratio of theoretical to experimental papers. Much of the work being done in theoretical physics today is unmotivated by any desire to make contact with experiment, either past or future. I believe that a good deal of the blame for this lies with theorists. There has come to be a kind of pecking order among us, in which those who retain an interest in what experimenters are doing are disparagingly referred to as 'phenomenologists', and are considered to be at a lower intellectual level than those who pursue mathematical models for their own sake. While creating new models is an important tool for realizing the physics of the next century, and it is surely impossible to tell which of today's mathematical models will turn out to be relevant to experiment, I believe that there is presently an overemphasis on such abstract modeling, as opposed to confronting the phenomena that have been or can be observed. This overemphasis is harmful to both aspects of physics. To paraphrase a

paraphrase. "Theory without experiment is lame, experiment without theory is blind."

It may also be that the great success of the standard model has removed much of the stimulus to theorists to be concerned with experiments, as the experiments now being done appear to some to be unlikely to lead to any real novelties. In any case, such behavior, like social fashions, often run in cycles, and it may well be that, Congress willing, the next generation of experiments will produce enough new physics to lure some theorists back from their mathematical reveries.

The Legacy of the Two-Neutrino Experiment

The two-neutrino experiment was performed in 1962 and the rest is history. In trying to evaluate its legacy, I am reminded of Christopher Wren, the architect who designed St. Paul's cathedral and who was also one of the discoverers of the conservation of energy. On the entrance to the cathedral appear the words "Si monumentum requiris, circumspice", or roughly translated, "If you would see his monument look around." Evaluating the legacy of the two-neutrino experiment is a bit like seeing the work of Wren. Much of our picture of the internal symmetry of the weak and electromagnetic interactions stems from that discovery. While

we don't really understand leptonic (or quark) flavor, the way we think about it is on outgrowth of the two-neutrino experiment. Both the pairing of charged with neutral leptons, and the symmetry of quark doublets and lepton doublets are an outgrowth of the discovery of the muon neutrino.

But I think that even more important than the specific discovery of the muon neutrino was the demonstration that high energy neutrino experiments could be done. This demonstration led to a tremendously influential series of neutrino experiments at Brookhaven, CERN and Fermilab. Neutrino experiments, together with electron-hadron scattering, dominated high energy physics in the late 1960s, and together with electron-positron colliders dominated it in the 1970s. Only in the 1980s has the focus shifted to hadron colliders. A list of the major discoveries for which experiments with neutrino beams played the dominant role include the following:

1. The muon neutrino
2. Hadronic and leptonic neutral currents
3. Charm
4. The quark structure of hadrons

All in all, there has been quite a legacy from a coffee-hour conversation on the eighth floor of Pupin almost thirty years ago.

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