SOME BITS OF THE HISTORY OF CP VIOLATION

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It is especially appropriate, here in the Panofsky Auditorium, to begin with the story of my first involvment with neutral K mesons. It was the summer of 1956. Bob Motley and I had just finished taking data at the Brookhaven Cosmotron on the lifetimes of charged K mesons as a function of their decay mode, our own small contribution to the $\tau - \theta$ puzzle becoming a major conumdrum. Panofsky, with his family, was spending the summer at Brookhaven. He came into our lab one day and said, “Let’s do an experiment.” Motley and I still had data to analyze but Pief was persuasive and we decided we could sandwich some new activity into our schedule. But what experiment to do? Lederman and his group[1] had just discovered the long-lived neutral K, then known as the θ_2. This clearly needed some confirmatory experiments. We decided to try to measure the total cross section of the θ_2 to confirm that it was, indeed a 50-50 mixture of K^0 and $\overline{\mathrm{K}^0}$. Their charged counterparts, the K^+ and K^-, have vastly different cross-sections at low energy and, if the general picture was correct, one could expect the θ_2 to exhibit a cross-section halfway between, after coulomb effects had been taken into account. That is what we proceeded to do, aided by Walter Chesnut, a new post-doc at BNL. Thanks to Pief, it was my first exposure to neutral K mesons.

Our strategy was to detect, using counter techniques, the neutral Ks through their decay either to $\pi\mu\nu$ or $\pi \mathrm{e} \nu$. On both sides of the neutral beam we placed scintillators with the requirement that, on one side or the other we stop the pion and see the $\pi\mu$ e decay sequence while on the opposite side, a single charged particle in time coincidence. It is easy to say now but then, it must be realized, plastic scintillators were not off-the-shelf items – if you wanted some, you made your own. George Clark at MIT was one of the pioneers in the business. He had recently perfected the process and was making wagon wheel-size pieces (36 inches in diameter) for an air-shower experiment

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at Volcano ranch in New Mexico. He graciously gave us a wheel of his material and from that we made the pieces for our experiment. Pief and I spent many quality hours that summer polishing the plastic after it had been cut to size. In the end we found Colgate toothpaste to be the best final polishing agent.

We got the counters and the neutral beam constructed that summer and were given some running time in the fall. This was still before scheduling committees had been formalized. At the Cosmotron, George Collins was the director and, to a large degree, controlled the experimental program from his pocket. Of course, he did have advice from an ad hoc kitchen cabinet. Still, the whole approval process was highly informal. I don‘t remember ever putting together a proposal for this experiment. We just talked to George.* The results of the experiment were published in Physical Review.[2]

Aside from giving you a flavor of what doing accelerator experiments was like in those early days, what does this have to do with the discovery of CP violation? The first few paragraphs of the paper show an early awareness, at least among the cognescenti, that CP was not necesssarily to be taken as a conserved quantity. In the paper we say the following:

"The analysis of this experiment is not affected by the lack of conservation of Parity and of charge conjugation invariance, and is only slightly influenced by a possible lack of time-reversal invariance in the decay of the neutral-K complex. Even though a small admixture of 2 pi decays to the long-lived component is possible in principle, both experiment[3] and theory[4] indicate that this admixture is small."

The principle point I want to make is that, from the very beginning, the 2 π decay of the long lived neutral kaon was held as a signature of CP and time reversal noninvariance.

In the fall of 1962 there was considerable discussion at Brookhaven about some results of the Adair group[5] which seem to show an excess of K_1s in a hydrogen bubble chamber exposed to a beam of the long-lived K_2s. I was working, at the time, at the AGS with David Cassel involved in an experiment to measure the form factor of the pion. Jim Cronin was at the Cosmotron doing an experiment on ρ production. Early in 1963 we learned that, in the near future, a neutral beam was going to become vacant at the AGS. We realized that by using spark chamber techniques (we were both using spark chambers in our respective experiments) one could improve, substantially, the bubble chamber results. Bubble chambers were great for observing the production of short-lived particles in liquid hydrogen but the multiple scattering in the hydrogen and the relatively short lever arm involved in momentum measurements greatly restricted their intrinsic precision. In addition, for certain kinds of experiments, the bubble chamber technique suffered by not being able to acquire target-empty data – necessary to

*Shortly afterward, review committees for experiments were established and the approval process became formalized.

ascertain the background effects of the material in the chamber walls. Furthermore, much higher beam intensies could be tolerated by spark chambers and, because of their intrinsic memory, they could be selectively triggered on interesting events.

We put together a modest proposal on short notice.[6] In this two page document we stated four objectives for the experiment: 1) to check the results of Adair, 2) to study the regeneration phenomena under a variety of conditions in different materials, 3) to set new limits on the decay of the long-lived neutral K to two pions, and 4) to check for the presence of neutral currents in strangeness changing decays. We finished the experiments in which we were previously engaged, moved the apparatus into the beam by late May, 1963, and took data that summer. The rest is history.

It is difficult to visualize the conditions of those early experiments at the AGS from the perspective of recent times. Most of the electronics was designed by us and home-made. At this time it was a hybrid of vacuum tube and discrete transistor circuits. The triggering and recording apparatus was on the main floor of the AGS beside the beam in which the chambers were placed. It was a hot and noisy environment with no regard for the comfort of the experimenters who sat at a table beside the beam with their log book. Targetting the beam in the AGS was internal in those early years − the circulating proton beam intensity was about a thousand times less than that today. Correspondingly, we were largely responsible for our own actions with regard to radiation hazards. To quote the beginning of a famous novel: “It was the best of times. It was the worst of times.”

The details of the experiment have been described many times[7,6] and I will not repeat them here. Suffice it to say that, previous to our work, the best limit on the decay of K long to 2 π was from a Soviet group[8] who analyzed 597 decays and found no candidates fitting the 2 π decay. We found 48 events in 24,000 decays, about 1 in 500. What about the original Adair results? If they had been correct we would have seen about 15 times as many 2π events in our hydrogen target data than we did, in fact, observe. The Adair experiment was repeated using a bubble chamber with a .07 cm wall instead of the original 1.6 cm. It was shown[9] that the original effect came, to a large extent, from the walls (no empty target data ??).

The original CP paper was published in July of 1964.[10] Almost immediately, within a couple of weeks, theoretical papers began to appear. They were in two categories: those that accepted the results as demonstrating CP (and T) violation and those that attempted to explain the phenomena in terms of a new long-range interaction, as Adair *et al* had done earlier.

The paper of Wu and Yang[11] has endured as a benchmark. It laid the grounds for a phenomenological analysis of the experiment on the basis of CP violation. It has provided a foundation for the interpretation of every succeeding experiment. Aside from factors of 2 the notation and definitions introduced in this paper have remained the same.

Truong[12] had reasons to suggest that the effect might be completely in the Δ I = 3/2 decay amplitude.

Wolfenstein[13] proposed a new Δ S = 2 superweak interaction which has existed as a possibility until the recent results which show ϵ' to be different from zero.[14]

Other papers at the time[15,16] proposed a new particle, a hyperphoton, the existence of which would avoid the necessity of CP and T violation. However, Weinberg gave[17] strong arguments that excluded this possibility. To my knowledge, Weinberg was the first to use the term, “fifth force”. It is interesting to note that when the issue was raised more than 20 years later by Fischbach and collaborators[18] the earlier work had been forgotten and was never referred to.

In any event, the first follow-on experiments demonstrating the interference between the CP violating amplitude and the amplitude for coherent regeneration[19] completely ruled out the possibility of any alternate explanation for the CP violation based on a new particle, such as a hyperphoton. Progress in experimentally quantifying the parameters involved in CP violation was initially slow and painful. Eventually, however, a number of remarkable experiments on the neutral K system using increasingly sophisticated and elaborate instruments have pinned down the parameters, originated by Wu and Yang, with remarkable precision. They are tabulated in the PDB. These experiments have culminated in the recent results[14] for ϵ'/ϵ. Tran Truong was a little bit right in 1964.

A.D. Sakharov,[20] very early, showed that CP violation combined with baryon nonconservation and nonequilibrium dynamics (easily provided in the early times of the big bang) could account for the matter-antimatter asymmetry in the universe. Indeed, one might turn the question around and say that the first evidence, ever, for CP violation was the fact that we exist.

CP violation now has a natural home in the standard model. And exciting times lie ahead. BABAR at SLAC and GEM at KEK promise to explore CP violation in a totally new regime.

References

[1] K. Lande, E.T. Booth, J. Impeduglia, L.M. Lederman, W. Chinowsky, Phys. Rev. **103** 1901 (1956).

[2] W.K.H. Panofsky, V.L. Fitch, R.M. Motley, and W. Chesnut, Phys. Rev. **109** 1353 (1964).

[3] K. Lande, L.M. Lederman, and W. Chinowsky, Phys. Rev. **105** 1925 (1957).

[4] T.D. Lee, R. Oehme, and C.N. Yang, Phys. Rev. **106**, 340 (1957).

[5] L.B. Liepuner, W. Chinowsky, R. Crittenden, R. Adair, B. Musgrave, and F.T. Shively, Phys. Rev. **132** 2284 (1963).

[6] M. Doncel, A. Hermann, L. Michel, and A. Pais, 1983, Eds. *Symmetries in Physics* (Universitat Autonoma de Barcelona, [distributed by World Scientific Publishing]), pp. 453-471.

[7] V.L. Fitch, Rev. Mod. Phys. **53** 367 (1981).

[8] M.K. Anikina, D.V. Neagu, E.O. Okunov, N.I. Petrov, A.M. Rozanova and V.A. Rusakov, JETP **15** (1962).

[9] C.J.B. Hawkins, Phys. Rev. **156** 1444 (1967).

[10] J.H. Christenson, W. Cronin, V.L. Fitch, and R. Turlay, Phys. Rev. Lett. **13** 138 (1964).

[11] T.T. Wu and C.N. Yang, Phys. Rev. Lett. **13** 380 (1964).

[12] T. Truong, Phys. Rev. Lett. **13** 358 (1964).

[13] L. Wolfenstein, Phys. Rev. Lett. **13** 569 (1964).

[14] A. Alavi-Harati *et al.*, Phys. Rev. Lett. **83** 22-27 (1999); G.D. Barr *et al.*, Phys. Lett. B **317** 233 (1993).

[15] J.S. Bell and J.K. Perring, Phys. Rev. Lett. **13** 349 (1964).

[16] J. Bernstein, N. Cabibbo and T.D. Lee, Phys. Lett. **12** 146 (1964).

[17] S. Weinberg, Phys. Rev. Lett. **13** 562 (1964).

[18] S. Fischbach, S. Sudarsky, A. Szafer, C. Talmadge, and S.H. Aaronson, Phys. Rev. Lett. **56** 3 (1986).

[19] V.L. Fitch, R. Roth, J. Russ, and W. Vernon, Phys. Rev. Lett. **15** 73 (1965).

[20] A.D. Sakharov, JETP Lett. **5** 24 (1967).