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Essay: Bob Siemann and the meson production by polarized photons

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I. INTRODUCTION

Bob Siemann received his Ph. D. from Cornell in 1969, working with Karl Berkelman. The title was *Wide Angle Bremsstrahlung: Energy Dependence*. After a stint as Research Associate at SLAC, he returned to Cornell as Assistant (later full) Professor in 1973. During the SLAC period Bob was morphing from elementary particle physicist to accelerator physicist. Curiously (since I was at Cornell) that is the period during which I worked most closely with Bob, and the period to which this remembrance will be limited.

This was a period when the field was still optimistic that sufficiently accurate and detailed measurement of pion and kaon scattering and production processes could clarify the strong nuclear interaction force. Vector mesons had recently been discovered and vector meson dominance investigated. More significantly, the quark model had just been introduced, and organization of mesons and baryons into SU3 families had just been understood.

During the period 1970 through 1973, Bob was mainly engaged in measuring meson photoproduction processes as a member of the Richter group at SLAC. Even limited to this brief period, Siemann's research spanned a range far too great to be summarized. Also, though group sizes then were minute by modern standards, it had already become futile to attempt to reconstruct who did what from the author lists of the various publications.

I will restrict myself to a solitary thread through Bob's research during this era—*Polarization Dependence of Meson Photoproduction*. Furthermore I will emphasize one particular paper (or rather, two, one describing the setup [1], one the measurements [2]) which is work that is unambiguously important and for which Bob was unambiguously the lynch pin; i.e., without him, the research might never have been done.

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Of course many scientists were involved in the series of advances to be described here. One assumes the reader is more interested in the general flow of the field of research and in the people involved, than in the technical details. I will, to the best of my recollections, just drop the names of the leading characters (as it happens, four would later become lab directors) along with just enough description of their contributions to support a continuous, somewhat anecdotal, narrative. An analogy to American football may be apt. Moving the ball up the field was the work of many. Bob Siemann carried the ball over the goal line.

As well as honoring Bob, I hope also to make physicists nostalgic for an era in which amazing and varied advances could be made on a time scale of months, not years.

II. POLARIZATION DEPENDENCE OF MESON PHOTOPRODUCTION

GeV-scale photons are produced by bremsstrahlung from material targets. In 1956, Uberall [3] showed that, by using an appropriately oriented single crystal as the target, the radiation would have quite high polarization and would have an energy spectrum far more monochromatic than the ordinary 1/energy, bremsstrahlung spectrum. By 1968, workers at Frascati, especially Diambrini [4], had demonstrated the feasibility of this method.

By 1970, Schwitters and Richter [5], and others (including Siemann) had measured polarization dependence of pi+ photoproduction using a beam polarized by the Uberall process. Their method of measurement used a subtraction procedure relying on an ‘‘edge’’ in the spectrum. Though adequate for these early measurements, they found one feature of the beam to be less than ideal. The edge near which the high polarization exists lies well below the upper end point of the spectrum. Photons of energy above the edge gave background counts seriously limiting the accuracy of their measurements.

By that time rho meson photoproduction from hydrogen, deuterium, and other elements had been measured at Cornell by Mistry, Silverman, Talman, and others [6]. In 1970, Diambrini spent the year at Cornell, bringing with him the goniometer needed to produce polarized photons from an oriented diamond target. Soon we had measured the polarization dependence of rho photoproduction [7] and found it to be maximal—the rho mesons ‘‘remember’’ the photon polarization and the plane defined by the pions into which the rho decays remembers the rho polarization.

Diambrini also called our attention to a paper by Cabibbo and others [8], which described an alternative method for polarizing a photon beam; namely by passing the beam through a thick, properly oriented single crystal, copper for example. In this process, since photons polarized in one transverse plane are preferentially absorbed in the (dominant) electron-positron production process, the surviving beam develops the other polarization.

During 1970, I was on sabbatic leave in the SLAC group of Ritson (who was himself on leave elsewhere). Along with Bjorn Wiik and Dave Gustavson, we contemplated the possibility of one-upping the Richter group by using the Cabibbo process to overcome the low energy limitation mentioned earlier for the Uberall process. There is a quite serious loss of intensity in the Cabibbo process (because polarization develops only through differential absorption, like visible light passing through a polaroid film) and the polarization is not very high. But these disadvantages were not very important for the intended purpose, and they were completely outweighed by the advantage of being able to obtain polarization at the upper end of the energy spectrum.

But we were not particularly comfortable in reciprocal space. We knew that Roy Schwitters had developed an elegant code for calculating the Uberall production process, for installation in the Richter beam line. From QED, we understood the close connection between bremsstrahlung and electron pair production, and assumed the code could be adapted for our purposes. We approached Roy about using his code, (without, it must be confessed, explaining how Gustavson intended to modify it) and he graciously approved.

By that time we were sufficiently confident of success that we had submitted a proposal to use the rho photoproduction process, mentioned above, at Cornell to confirm the applicability of the Cabibbo process and to measure the degree of polarization achievable. McDaniel, the Cornell Laboratory of Nuclear Studies director, had assigned us a few weeks of running time at the Cornell Synchrotron, some months in the future. Such proposals were far less formal in those days than they are today, but it was still a serious commitment. At that time we had no appropriate (foot-long) crystalline target, but we assumed that we could acquire in time, and use, a single crystal of silicon.

Soon, to our horror, through a combination of better analytical understanding of the process and the newly modified code, we realized that the experiment was not going to work with silicon. (The Debye temperature is too low.) For that matter, the original material suggested by Cabibbo, copper, would have failed for the same reason.

There is nothing like the threat of hanging to concentrate a person's mind. After a few feverish days in the Stanford library I came to realize that carbon was the only appropriate material. A foot-long diamond would be ideal. But that was not in the cards. Then I hit upon graphite and, to my amazement, found that carbon exists in crystalline graphite form. Nowadays this form is known as ''graphene.'' Scarcely a week goes by without a seminar on its remarkable properties. At the time, based on its fabrication method, this form of carbon was referred to as ''pyrolytic graphite.'' (To this date, graphene crystals found in naturally occurring deposits have higher quality than can be produced artificially.) But the pyrolytic quality seemed adequate for the Cabibbo process.

A trip to meet A. W. Moore, of Union Carbide, in Cleveland, established that he had some graphene samples. They were shaped like mosaic tiles (few-millimeter-thick, inch-square). With the transverse beam dimension being about a millimeter, a dozen or so of these tiles could be lined up (accurately) to form the equivalent of a foot-long single crystal. They were, however, too expensive for us to purchase. Fortunately, through Moore, Union Carbide graciously allowed us to borrow the material. (Later, when Bob Siemann built a serious polarized beam at SLAC he *purchased* pyrolytic graphite from the same source—possibly including some of the very same tiles.)

With the crisis averted (and no one else even aware it had ever occurred) we completed planning for the test of the Cabibbo theory. The bremsstrahlung beam from a target internal to the Cornell synchrotron, after passing through the azimuthally rotatable graphite polarizer, passed through an amorphous carbon target in which rho mesons were produced. The rho's decayed immediately into two charged pions, which were detected. From their measured 4-momenta, the full kinematics could be reconstructed. The measurements confirmed the (appropriately repaired) Cabibbo theory [9].

III. CONSTRUCTION, CHARACTERIZATION, AND USE OF THE SLAC POLARIZED PHOTON BEAM LINE

Everything up to this point has been *research*. To exploit this research for a usable accelerator facility requires *development*. As well as being a futuristic thinker, Bob Siemann also had a very practical bent. While the rest of us went on to other things, he did the serious design work needed for full implementation of the Cabibbo-polarized beam line. This work is described in the paper [1] mentioned earlier. It might be correct to identify this paper as Bob's first accelerator physics paper and it is the single most appropriate paper for pursuing more technical detail concerning the topic under discussion.

Many engineering tasks needed to be performed: construction, alignment, cooling, control system, installation of the entire apparatus in a magnetic field to sweep the produced electrons and positrons out of the beam, and so on. The precision crystal mounting was actually done by Union Carbide.

Before the beam line could be used for practical experiments, it was still necessary to determine the optimal crystal orientation, and to accurately measure the resultant degree of polarization. As mentioned already the crystal quality was fairly low, so empirical measurement was required. As well as describing the polarization process, the Cabibbo paper [8] also described how the polarization could be measured, using a second, approximately identical, crystal. To obtain the final intensity spectrum (as a function of momentum) it was also necessary, using an electron-positron pair spectrometer in a subsidiary experiment, to measure the attenuation in both polarizer and analyzer. All this is described in Ref. [1].

Reference [2] is the last of a sequence of papers describing polarized photon measurements performed with what could fairly be called this "Bob Siemann beam line." From this paper the full history of the group's work can be reconstructed. Much of this work could not have been done without Bob's beam line.

Preliminary results using this beam line were first reported in 1973 at the Bonn Symposium on Electron and Photon Interactions at High Energies, and I had the good fortune to be the *rapporteur* for that session. Corny though it was, and wishing to publicize the method, I asked the Bonn lecture room apparatus attendant whether he had the sort of wire grid that one uses to demonstrate polarization of microwaves in sophomore E&M lab. The process is the same—one polarization is absorbed, the other is not. He proudly showed me, and allowed me to brandish, the very apparatus that Heinrich Hertz had used for the "same" purpose a century earlier.

Much of Bob's subsequent career has been devoted to a far more ambitious task; overcoming the 100 or so MeV per meter barrier preventing one from obtaining truly high energy electrons. Some would say Bob lived long enough to see the beginnings of success. But Bob would surely disagree. By his standards success could only be claimed after using the beam to perform a significant experiment that could not be performed any other way.

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