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CHAPTER 29. HARDBALL PHYSICS

[L. W. Alvarez]

The "associated production" experiment in the 10-inch bubble chamber was led by Lynn Stevenson, Frank Crawford and Harold Ticho, who designed the beam. Had it been possible to mount it a year earlier, it would have been the first confirmation of the prediction of Lee and Yang that parity was not conserved in "weak decays." That theory was confirmed in early 1957 by Wu, Ambler, et al, in the low energy beta decay of cobalt-60 and it may have set a record in that Lee and Yang won the Nobel prize in that same year of 1957. Lee and Yang had suggested that parity nonconservation might be seen in a beta decay reaction, such as that of cobalt-60, or in muon-electron decay, or in the decay of the Λ hyperon. As "everyone" knows, it was first seen in beta decay and the next day--after Madame Wu had reported it to her colleagues at Columbia--it was seen in muon decay by Lederman, Garwin and Weinrich. The latter three did the experiment at the Nevis cyclotron "after dinner" in an experiment that they could have done any evening in the many months before! The 10-inch bubble chamber group did it a few months later, using their measurements of Λ decay, seen in the pictures they took of the important associated production reaction: They published their confirmation of parity non-conservation using the "up-down assymmetry" in the Λ decay in a letter to the Editor of the Physical Review: Detection of Parity Nonconservation in Λ Decay, F.S. Crawford, Jr., M. Cresti, M.L. Good, K. Gottstein, E.M. Lyman, F.T. Solmitz, M.L. Stevenson and H.K. Ticho. Phys. Rev. 108, 1102-3(1957).

If negative pion beam time had been available at the Bevatron a year

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earlier, they would have had the pleasure of being the discoverers of parity non-conservation. But instead, they merely confirmed it in a different reaction; but our group had had the satisfaction of doing some really important strange particle physics with K mesons and also discovering muon catalysis in which Frank and Lynn and several of their colleagues were co-authors.

One of the reasons that negative pion beam time was scarce was that with the exception of our "private" low energy K beam port, part way around one of the curved sections of the Bevatron, there was only one target position and one beam port at the Bevatron. This had been used in the discovery of the antiproton by the Segre group and by almost all other Bevatron users from the time the machine started to operate. I am proud of the fact that when the Segre group was about to run out of beam time and our K^+ lifetime experiment was scheduled to replace it, I told Ernest Lawrence and Emilio Segre that my group thought that Emilio's experiment was so important that we would give up our scheduled time so that they could carry on with their apparent (but not yet proven) discovery of the antiproton. I am perceived as a very competitive perons, which is a correct assessment, but I also have a high loyalty to Ernest Lawrence's Berkeley laboratory. In the case I've just mentioned, that loyalty, plus my own assessment that Emilio's experiment was much more important than our K+ lifetime experiment, was what prompted me to volunteer our hard won beam time to the Segre group.

I debated for some time the propriety of telling of the next few incidents that involve my memories of the conduct of some physicists who are still alive. But I think that it is proper to do so if these memoirs are to reflect what life at the frontiers of high energy physics was really like in this period. I can hear my friends saying after reading this far, "Where is that feisty, competitive Louie Alvarez that I remember? In his memoirs, he seems to love everybody and everyone loves him." I believe that I've so far criticized the behavior of only a few people, such as W.F. Meggers, in the matter of the ^{198}Hg lamp, Wendell Latimer in the case of tritium, and Emilio Segre because he didn't appreciate Ernest Lawrence's great and unusual talents as a physicist. So now I'll tell of another set of circumstances that made me as angry as I had earlier been, relative to Meggers and Latimer.

In the summer of 1957, Lynn Stevenson, Frank Crawford and their colleagues had found a large "up-down assymmetry" in the decay of the neutral Λ hyperon, which was a clear indication that parity was ~~is~~violated in that decay, as Lee and Yang had earlier suggested might be the case. (My memory is particularly good for events happening in mid 1957 because that is when Jan and I met and quickly fell in love.) I wasn't a member of the "sub-group" within my research group that did the work on associated production and on the Λ decay, but I was thanked for my "interest and guidance" at the end of the paper. So I watched all the experimental and sociological aspects of the work from a "ringside seat." The first outside actor in the drama I am about to describe was Mel Schwartz, who was then a graduate student in the Steinberger group at Columbia. Although I may be critical of some of the things Mel did that summer, I want to make it clear that we have not let those things interfere with our friendship over the years. Several years ago Mel, in his capacity as president of Digital Pathways, Inc., designed a microprocessor to compute in real time the numbers to be displayed on the "Vision Analyzer," an optometric measuring device that was manufactured and sold by Humphrey Instruments, Inc., of which I was chairman of the board and principal stockholder. And furthermore, I nominated Mel for a Nobel prize for his important contribution to the discovery that there is a muon neutrino, that is different from an electron neutrino.

On the other hand Jack Steinberger, the other main actor in this drama, and in another I'll describe, and I have never had "good chemistry." I think it started when I heard that Jack was critical of Ernest Lawrence, Pief Panofsky and me, for our participation in the MTA project at Livermore. When Mel Schwartz appeared in Berkeley we found him to be a very bright and very personable young visitor. The Steinberger group had in the past several months taken a large number of bubble chamber pictures in Jack's propane chamber at the Brookhaven Cosmotron. They had divided most of the rolls of film into three parts which were scanned and measured for the most part by the Columbia-Brookhaven groups and by the groups in Pisa and Bologna. (Some was apparently scanned and measured at Ann Arbor, Michigan, but my memory is that Mel told us that the main work was done by the three groups mentioned above.)

Mel reported that the 20 man collaboration had finished their scanning, measurement and data analysis and that there was no appreciable up-down assymmetry in Λ decay," parity was not appreciably violated in the weak decay of this "strange particle", as it had been observed to be in the decay of the muon and of cobalt-60. We enjoyed having Mel with us and he found our way of "doing business" quite different from what he had experienced in the Steinberger group. We were making the transition from the use of analogue computing devices such as "stereoplots," to the fully computerized techniques that were pioneered in our group by Frank Solmitz, Art Rosenfeld, Horace Taft, Jim Snyder and others. Mel kept saying things such as, "If you guys would put all your people on the stereoplots instead of having most of them writing computer programs, you could finish your anaylysis much more quickly." I once responded with one of my favorite stories about two woodmen who each bet he could cut down more trees in a week than the other could. One didn't cut down a single tree the first day, he spent all day sharpening his ax and of course, he won the bet. My message to Mel was that we were sharpening our axes and the day of the stereoplot was about to disappear in our group and then later in the rest of the world. That turned out to be true and before long, representatives of most bubble chamber groups worldwide were sending visitors to Berkeley to learn the new techniques from Frank and Art and to carry magnetic tapes of our programs "back home." And similarly, everyone ordered Frankensteins from a company that Jack Franck set up in Berkeley. I was one of the founders and original stockholders in the company, but quickly severed all my connections with it when I learned that people who really needed to have Franckensteins thought that it was outrageous that I might make some money out of something that was basically my own invention. It was an easy decision to return my stock and walk away from the company; I was having a wonderful time and the jealousy that my involvement in the company apparently aroused, was something that I could "do without."

I was amused by the fact that all the early customers for Jack's Franckensteins insisted that they didn't want the automatic track following that I had originally suggested to Jack and to Hugh Bradner (based on my radar experiences). But Jack once told me that most, if not all, of the contracts for Franckensteins without automatic following were later renegotiated so that that feature was incorporated into each delivered instrument. So after listening to most of our competitors in the bubble chamber business tell us that we were wasting a lot of government money on "useless frills," it was good for my ego to see that everyone quickly came around to our point of view as they watched our scanners measure tracks, semi-automatically, at high speed.

I'll now return to our visit from Mel Schwartz, who kept assuring us that the Columbia collaboration had measured all the Λ's they had, and there was no appreciable up-down assymmetry. He knew that we had only a third more Λ's in our film than they had, so our statistical error wouldn't be much smaller than theirs. He couldn't understand why we continued to waste our time repeating an experiment that they had already shown had a "null result." But during his visit, "our" effect kept getting more statistically significant. Before Mel left in mid-summer he knew we had a "real effect," parity was indeed violated in Λ decay, in spite of Jack Steinberger's group's inability to see it.

Harold Ticho reported in an invited talk before the American Physical Society in Boulder, Colorado on September 7, 1957, that he and his colleagues in our group had seen a "large" parity violating Λ particle decay. The theoretical maximum possible value for the assymmetry parameter was +1.00 and Harold reported that his team had observed 0.44 ± 0.11. (This was in contrast to the null value that the Columbia collaboration had found and not reported formally.) So we had the pleasure of seeing and reporting a large effect before anyone else did. This was of course known to everyone "in the business," because the long distance telephone lines were busy then, as they are now, in reporting the latest "gossip." (As I've probably said before, Robert Oppenheimer once said, "Gossip is the lifeblood of Physics.")

The rest of the story is partly fact and partly rumor and I'll try to keep the two separate and so identified. Mel Schwartz certainly carried back to Columbia the news that the Berkeley group had a real effect so the question was obviously, "How did we miss it." The following is my memory and that of Lynn Stevenson's about what Mel Schwartz later reported to us, but the published dates all "hang together," so it is probably true. Jack Steinberger and colleagues then looked at the three sets of data from the East coast and the two Italian universities, as separate experiments rather than as one larger pool of data, as they must certainly have done originally, in view of the small size of the overall sample--a total of 263 events. It was immediately clear that each set had an effect that was statistically significant, with two of them agreeing with the Berkeley sign, but with the Bologna sign being reversed. So two of the three results "cancelled each other," and the collaboration was left with an effect only one third as large and so statistically insignificant. We later heard that "everyone was mad at Puppi," because they said he had an extra mirror in his film projector system and every extra mirror reverses the parity of the image. If Jack Steinberger had only put a capital R, or some such parity-distinguishing symbol in his bubble chamber, the mistake couldn't have been made, the R's would have turned into Cyrillic ~~R~~'s. Я's

By the time the Columbia collaboration had found the parity-causing error, the alleged extra mirror in Bologna, our group had already announced (but not published) its discovery. The Columbia group announced its results at a meeting in Venice, in the week of September 22-28, 1957, but by that time, "everyone" at the meeting had heard of the Berkeley discovery. But neither group had yet published its results. The Berkeley group beat its rivals into print in the Physical Review by two weeks and had the pleasure of saying to itself, "We discovered the effect in the 10 inch hydrogen bubble chamber and that's just where Jack Steinberger and his friends first saw it." We would certainly have been distressed if the Columbia collaboration had beaten us into print if as we all believed, "they had discovered the effect in our notebooks." (What made us all so angry was that they came so close to beating us into print when we felt sure that had Mel not been in Berkeley that summer, they would not have reanalyzed their data; things were moving so fast in all bubble chamber groups at this time that it is hard to believe that anyone would have had the time, or the inclination to reexamine data that had earlier shown a null effect.)

And now that I've washed a little dirty linen, I'll mention another event in which Jack Steinberger's name appears and that should have alerted us to the possibility that he might beat us into print. The only time that I can remember having written a book review for a physics journal was when I was asked to read and review Bob Marshak's 1952 "Meson Physics." I found it to be a most valuable addition to my library and I gave it a very favorable reivew in Physics Today. Bob Marshak played an important role in the advancement of particle physics far beyond his many contributions to the theory in that branch of physics. (He has never received any credit for proposing with Hans Bethe before the pion was discovered, that it would be found and that it would decay into the muon that was observed in cosmic rays. The pion was discovered by Powell et al. so soon after the prediction was made that the prediction didn't have a chance to "sink in;" physicists only remember with pleasure the experimental discovery of the pion which as I said in my Nobel lecture, "restored sanity to the field"--after the wartime discovery by the young Italians that the negative muon didn't interact appreciably with nuclei.)

Bob Marshak's great monument in particle physics is the biennial "Rochester Conference." Bob organized the first few of these important conferences at his own University of Rochester and invitations to them were highly prized among particle physicists worldwide. I went to all but the first of these conferences when they were held annually in Rochester. (The first one was attended by a small group of theorists, but later invitations were parcelled out about equally between theorists and experimentalists.) After several conferences at Rochester the site of the meeting was determined by an international committee, and I remember attending "Rochester Conferences" in Geneva, Kiev and Berkeley. When the "Rochester meetings" went onto a biennial schedule, there was a "mini-Rochester" meeting held in alternate years, when the younger particle physicists, who weren't well enough known to be invited to the big meeting, ~~not by themselves~~ could attend. I was honored in 1960, by being invited to attend one of the mini-conferences; as I remember, I was almost the only person in attendance who had been regularly invited to the "main meeting." The conference was scheduled for a week in Siena, Italy, and every morning Jan would take the train to Florence where she spent many happy days in the art museums and I would go to the conference which was held in the ornate town hall on the plaza, around which is held the annual and very famous horse race.

I'll describe some of the "Rochester Conferences" in more detail in historical order, but I wanted at this time to tell of Bob Marshak's great invention. An electronics manufacturing company is acutely aware that the end of the fiscal year is approaching so everyone tries at the last moment to "shove all the ordered products out the door," to maximize the yearly sales and profits. Similarly, particle physicists "burned the midnight oil" getting their data in shape for a first presentation at the "Rochester Conference." It marked in a very real sense the end of the "fiscal year," for all particle physicists.

I'll now return to the story of my review of Bob Marshak's very valuable book. (I find on looking through this book that I made a very serious attempt to follow or independently derive, all his quantum mechanical derivations of formulas. That was probably the last time I attempted such an exercise because I found it was very difficult for me and obviously very easy for my young friends who had been properly taught quantum mechanics in the Robert Oppenheimer tradition, whereas my education in such matters was sadly lacking. But I taught myself enough quantum mechanics in this period to be scheduled to teach the undergraduate course in that subject--it was actually printed in the catalogue that I would teach the course. But my only sabbatical year suddenly came into being at that time, so I didn't teach anything for a year and I never did teach quantum mechanics at any time in my career. I spent the year doing bubble chamber physics at Berkeley and not visiting a European laboratory, as is the customary thing to do. When people asked me why, I said, "I'm like the lady in Boston who was asked why she didn't travel. She said, 'I didn't have to--I'm already here.'" Our group was at that time crowded with visitors from all over the world, so it was clearly the place for a bubble chamber physicist to spend his sabbatical year.

At the next Rochester meeting Bob Marshak thanked me for my supportive review of his book and I thanked him in return for having written a book that was so valuable to me in my role as an experimental physicist. I then chided him mildly for what I thought was a bit of chauvinism on his part, as a member of the University of Rochester Physics Department. The facts were the following, as one can see on pages 162 and 163 of Meson Physics; Bob discussed the measurement of the cross section for the reaction $\pi^+ + d \rightarrow p + p$, which together with the cross section for the reverse reaction, $p + p \rightarrow \pi^+ + d$, on page 162, with a footnote 66 to Clark, Roberts and Wilson (of the University of Rochester)--Phys. Rev. 83, 649(1951). These results gave the very important (and correct) experimental result that the spin of the pion was almost certainly 0 and not 1 or higher.

On the next page, Marshak referred to another measurement of the $\pi^+ + d$ reaction, giving the same value of 0 for the pion spin. The reference 67 at the bottom of page 163 was to Durbin, Loar and Steinberger, Phys. Rev. 83, 646(1951). Anyone who is used to reading the physics literature would notice that the Columbia paper appeared in the Physical Review, 3 pages ahead of the Rochester paper, so by the "normal standards of the business," Steinberger's paper should have been referred to first. When I broached this matter to Bob, he nearly "blew up." He said, "Do you know what that (expletive deleted) Steinberger did?" When I replied that I had no idea about what had happened, Bob told me the following story which is consistent with the dates printed in the Physical Review. Art Roberts and his colleagues sent their important Letter to the Editor to the Columbia University Physics Department, where the offices of the American Physical Society were then located. The manuscript was received by the editor on June 8, 1951 and the "Letter" was published as the first one at the end of the August 1, 1951 Physical Review, on page 649.

But three pages ahead of it was published a regular <u>article</u> (not a Letter), from Jack Steinberger and his students, which was "received June 21, 1951--nearly two weeks <u>after</u> the Rochester contribution had arrived at Columbia. The ~~thing~~ reason that Jack Steinberger was able to "beat them into print," was because (they felt sure), he was probably shown their manuscript by the editors, who then let him publish a regular article, four issues earlier than anyone else could get such an article published. (The argument here is that if one looks through later issues of the Physical Review, he finds that the first article with a reception date as late as June 21 is in the October 1 issue of 1951.) So the Rochester group knew that if Jack Steinberger had submitted the expected Letter to the Editor on June 21, it would have appeared after theirs, and the two references would show that they were first. So regardless of whether Jack was shown the Rochester manuscript by the editors, he did "go to the head of the line," by four issues and was given "an earlier reference" than the Rochester group. Some readers will wonder why physicists are so concerned with priority, but in a field where no one expects to become rich, there is great satisfaction in "doing it first." (In patent law, dates "are everything." Alexander Graham Bell filed his telephone patent application earlier in the same day that Elisha Gray filed a similar application on the same invention. Those few hours eventually cost Gray a great fortune.)

Note added by a third party: What Marshak did not know was that the S. group had not started to take data at the time they saw the Rochester paper. Neither of the two graduate students stayed in particle physics.

No one doubts that the Steinberger group did the experiment independently and well; the bad feelings were generated only by the way the publication of their results was handled.

No description of the ethical standards of the physics community in the latter half of the twentieth century would be complete without a review of the discovery of the antiproton, and the legal controversy that surrounded that discovery. I have briefly discussed the scheduling of that experiment, which was first reported in a Letter to the Editor of the Physical Review, Vol. , 947 (1955). The 6.3 Gev (Billion electron volt) energy of the Bevatron was chosen, as I've said earlier, to make it possible to produce antiprotons, if, as almost everyone believed, they could be produced. I am sure that everyone in the Lab gave more than a passing thought as to how he might detect antiprotons in the stream of negative pions and kaons that would be produced in a target that was bombarded with 6.3 Gev protons. I tried on several occasions to devise an experimental arrangement that would accomplish this purpose, but I wasn't successful. I argued to myself (and correctly) that all these three kinds of particles would be moving very close to the velocity of light--the antiprotons at about 0.866c, with the kaons and pions going even faster. I couldn't think of a reasonable way to distinguish between them, and part of my trouble came from the fact that I didn't realize that "strong-focusing" magnetic lenses were really image-forming systems, the way optical lenses are. This was in spite of the fact that my colleagues, Craig Nunan and Bob Watt had equipped our 32 Mev proton linear accelerator with the first set of strong-focusing lenses ever installed in any real accelerator (as contrasted to the model electron accelerator that had been built at

Brookhaven.) In a linear accelerator a focusing system such as the tungsten grids we were then using, or the solenoids we used in the MTA, did not form an image; ~~but was~~ they simply comprised a device to keep the beam from striking the solid faces of the drift tubes, so that it passed through the "holes." Now that "everyone" knows that such lenses do form sharp images, it is hard to imagine that someone like me, who had more experience with quadrupole lenses than almost anyone in Berkeley, didn't realize that simple fact. But that was the situation and that lacurna in my knowledge about modern beam transport systems kept me from devising systems of the kind used by Segre et al, in their discovery of the antiproton, or by Cork, Wentzel, et al, in their subsequent discovery of the antineutron. I think it is probable that had the bubble chamber development not been moving forward so rapidly and occupying so much of my time, that I would have "dug in," and come up with some sort of scheme to detect antiprotons. But the fact is that I didn't and if what I just said sounds like an apology, it isn't; I did have my "eye on the ball," and was well along on a program that would lead me, as it led Emilio Segre, straight to Stockholm.

I do remember being surprised that Emilio devised the beautiful beam transport system that he and his group used in discovering the antiproton because it was quite unlike anything that he had ever done before and I wondered how he knew so much more about quadrupole lenses than I did, when he so frequently showed distain for people who dealt with the "grubby details" of accelerator design. On the one occasion that I can remember wondering how he came to do his beautiful experiment, I decided that Enrico Fermi must have written him a letter, telling how important the experiment was, and how it could be done in a magnetically-focused and deflected beam system, with time of flight measurements to separate antiprotons from the faster negative kaons and pions. The reason for such thoughts was not that I didn't think Emilio was smart enough to have thought up the experiment, because he is one of the brightest people I know and he certainly "knows more physics" than I do. But his mind works quite differently from mine, so I had difficulty in believing that his "kind of smarts" could lead to the design of the experiment he was doing. I was pleased when I learned that he had won the Nobel prize; I thought the discovery of the antiproton was a fitting climax to a lifetime of uniformly excellent experimental work, starting with his "great days" in Rome, with Enrico Fermi, when slow neutrons were first discovered and new radioactive isotopes were being found every day.

But then Oresti Piccioni charged in court that the Segre group had misappropriated the experimental design he had revealed to them in December of 1954, when he visited Berkeley and spent several days in conversation with the group, staying some of the time in Emilio's home. It was certainly much more believable (to me) that Oreste would have preposed the experiment since he had been using the image-forming properties of quadrupole lenses at the Cosmotron, where 3 Gev protons hit a target and produced high speed negative pions and kaons. (One of my reasons for writing this book is to explore how people do the experiments they do. Hardly anyone has a "brilliant flash," in which a completely new experiment springs forth in all its glory. As I've tried to show, each experimenter builds on his own hard-won store of knowledge and adds bits and pieces from conversations with others, or by reading the literature. So my earlier wondering about how Emilio had devised his experiment was immediately resolved if I believed Oreste's story--and I did, because it all fit together with what I knew about Oreste's distinguished career in physics.)

I'll now round out this story with a summary of the extensive documentation which Oreste sent me, piece by piece, as his lawsuit against Emilio and Owen Chamberlain progressed through the courts and was finally rejected by the California Appelate Court in about 1975. (The pile of papers is almost an inch thick.) The validity of Oreste's claims were not evaluated--the judgment was based not on the truth or falsity of anything Oreste said in his brief, but on the three points that the Segre-Chamberlain lawyer presented as their defense: (1) the statute of limitations "had run out," which meant that regardless of whether Oreste's assertions--that Emilio and Owen had stolen his ideas--were correct, the crime, if any, had taken place so long ago, that Oreste could not recover anything. (2) Oreste had waited too long to present his claim. Oreste dwelt at length on this question and used the argument that for a long time the Bevatron "was the only game in town," and Segre had threatened to bar him from the use of that machine if he pressed his charges earlier. He quoted a number of similar cases in which plaintiffs had won, using such a defense. (I found these most interesting and I thought that they were applicable and would sway the judge, but I was wrong. I discussed these matters with one of my golfing partners, who is a professor of law at Berkeley. He was quite upset at the judge and said that he should have disqualified himself in the case because he had been the counsel for the Regents of the University when the alleged crime had taken place, on University property.) And (3) the judge argued that the law did not protect an idea such as Oreste's because it had not been set down on paper, but had instead been described to the members of the Segre group in several sessions of talk at a blackboard.

As far as I could tell and I do not have copies of the Segre-Chamberlain briefs--they never derived that Oreste disclosed the essential features of the experiment to them in December of 1954, in return for participation in the experiment, as a member of the team. (Such actions are frequent occurrences in the physics world.) There are recollections in Oreste's briefs that Ed McMillan and I independently asked to see earlier references in the group's notebooks, showing calculations of fluxes, deflection angles, times of flight, etc., that would show that the group had been thinking about the experiment before Oreste's visit in late 1954. But apparently Ed was told, as I was, that there were no such "previous calculations." So I think there can be no doubt that Oreste did disclose the whole plan (except for the back-up use of a Cerenkov counter in measuring particle velocity) to the Segre group and that they did agree with his request that he be made an official participant in the experiment, as he had every expectation of being--in return for "putting them in business." Oreste's brief included copies of the early February 1955 very cordial correspondence between himself and Clyde Wiegand (a member of the Segre group), in which they both used the word "we" to speak of the people who were designing the magnetic quadrupoles that had been proposed several weeks earlier by Oreste. I asked Clyde for his comments on the legal problems on a couple of occasions and both times he smiled and said, "One of these days, I'll write my recollections of those events." (Clyde is one of the gentlest of souls and he obviously wants to avoid a confrontation with any of his old coworkers.)

In my opinion, it is a shame that such a simple dispute had to be settled in the legal arena by a judge who obviously didn't understand anything about physics or about the way physicists go about their business. I am confident that had the matter been "submitted to binding arbitration" by a panel of three experienced physicists, that a panel would have found in Oreste's favor. The rules of arbitration are quite simple; each side chooses one panel member and those two choose a third member. The first two are not representatives of the parties that chose them and all three agree to act in an impartial and fair manner in hearing the evidence and arriving at a decision. The decision has the force of a legal judgment because both parties agree beforehand not to despute the judgment in court and in the few cases where participants have so challenged the judgment, the courts have found against them.

I once was a member of an arbitration panel that heard a claim by a physicist who felt that he had not been given proper credit by a group in which he had worked; his name was left off one important paper published by the group. I was chosen by that aggrieved physicist and the group's choice and I chose a third physicist, who served as chairman of the panel. We heard evidence from many witnesses and after much deliberation, voted unanimously in favor of the group. That experience is what convinces me that if Oreste Piccioni's suit had gone to arbitration, he would have won his judgment against Segre and Chamberlain.

I will end this story by hazarding a guess as to what would have happened if Oreste Piccioni had stayed in Berkeley from the time he presented his plan to the Segre group. He would have been a coauthor of the definitive paper, instead of just being thanked for having "made very useful suggestions in connection with the design of the experiment." In their Nobel lectures, both Segre and Chamberlain said that Piccioni suggested the use of magnetic quadrupole lenses. But I'm confident that had Oreste stayed several months in Berkeley, everyone would have known that he had been a part of the team from day one and the Nobel prize would most probably have gone to the two native Italians. Both of them had done wonderful Physics before the antiproton was discovered, in contrast to Owen, whose only really important experiment was the one under discussion. (That this is "conventional wisdom" can be appreciated from the fact that Owen is the only one of the "Berkeley Nobel Laureates who never was a "Faculty Lecturer"--the highest honor that the Berkeley faculty can bestow on one of its members. In my opinion, that tells a lot about how Owen is perceived as a scientist by his peers.)

So I think Oreste really did have a valid grievance and I'm sorry that even though he "had his day in court," it was the wrong court.

One of the most fascinating cases of a disputed discovery claim involves the particle that is called the J/ψ by everyone in high energy physics--with the exception of Sam Ting, who calls it the J particle. Everyone else has accepted the fact that it was discovered independently by Sam's MIT group, working at Brookhaven and by Burt Richter's SLAC/LBL group, working at Stanford. The Nobel prize for the discovery was shared by Sam and Burt in 1976 and from personal experience, I know with what care the Nobel Committee studies all aspects of such a discovery. So I think that Sam's claim--that the SLAC group found the ψ particle only because it learned of his discovery through Mel Schwartz--has not stood up under close scrutiny. In fact, one can make ~~an equally good~~ a stronger case that Sam didn't believe his own results until he saw that SLAC had independently found what they originally called the ψ particle. It was only then that he sent his paper in to Physical Review Letters; it arrived one day ahead of the SLAC paper. If Sam had really believed his result earlier, he would have sent a letter to Burt Richter, telling them that he had found the J, with a mass just over 3 Gev/c^2, and asking if they could confirm it. In that case, he would have received the whole Nobel prize and Burt wouldn't have done anything more than confirm the discovery of the J. The fact that Sam didn't do that tells most people that he really didn't believe his results--he knew that SLAC hadn't originally "seen anything" in their survey of that mass region and that must have bothered him.

The story of the discovery of the J/ψ is told in great detail in the 1976 ~~E~~ e volume of "Adventures in Experimental Physics," by Sam Ting and by Gerson Goldhaber of the SLAC/LBL team. The SLAC discovery was made on Sunday morning, November 10, 1974. Just after midday, Gerson started to write the paper, which was completed that evening, with the height of the resonance peak continuing to rise, as its exact energy was found. The telephones started to spread the news in the early afternoon and the SLAC control room was soon filled with admiring visitors. As Gerson says, "The news spread like wildfire." In fact, it spread into the "Eastern camp," as Sam says, "The moment I checked into the hotel" (after flying to Stanford from the East coast for a committee meeting) "I received a phone call from Martin Deutsch who mentioned that there was great excitement at SLAC but he did not know the nature of their results. I traced (Ron) Rau to Los Alamos and informed him of my decision to announce our results and quickly sent out a preprint." (Note that the decision to "publish" was made only after hearing of the Stanford "excitement.") "I then placed a call to Stan Brodsky (of SLAC), informing him of our results. Stan was very excited, but did not want to tell me about the SLAC results. He told me that he would arrange for me to give a presentation the next day. I then called various laboratories like CERN and DESY to tell them of our discovery." (Emphasis added.) "The next morning when I walked into W.K.H. Panofsky's office to show him our results, he informed me that similar results had been obtained by SLAC and LBL over the weekend." One can appreciate how disappointed Sam was at this point, but I must repeat that if he had really believed his own work, a week earlier, he would have asked SLAC to confirm it, as he now proceeded to ask the colliding beam physicists at Frascati to confirm his results (without mentioning that he then knew that the SLAC colliding beam physicists had already seen the very narrow resonance.) I think this last point is a very black mark against Sam, because he had his colleague, S.L. Wu ask the Italians to confirm his results without mentioning the SLAC results, which he then knew. (The Frascati paper says that "on the following day, the information had reached us that this particle had also been observed at SPEAR." (Emphasis added.) The Frascati experiment was considered by everyone to be a confirmation and not a discovery, just as the SLAC experiment would have been if Sam Ting had believed his result enough to tell Burt Richter a few days earlier.)

For some time the East coast physicists called it the J, and the West coast people called it the ψ. But then the reasons that the SLAC people were relooking in the 3 Gev energy region became known and were accepted by just about everyone but Sam Ting. So everyone but Sam now calls it the J/ψ. Sam kept telling everyone that SLAC had "stolen" it from his group and he accused Mel Schwartz (now of Stanford and SLAC) of telling his friend Burt Richter about the "discovery of the J," so that Burt then "jumped back in," and found the J in an energy region they had previously explored. My guess is that Mel did tell Burt that Sam had a suggestive bump which he wasn't ready to publish. But I find Gerson Goldhaber's story of why the SLAC/LBL group did the crucial experiment, when they did it, very persuasive and I believe that what they heard on the "gossip circuit" wasn't as important as what they saw in their old data as they reviewed it before publication.

I'll now contrast the two Nobel lectures given on December 11, 1976 by Burt and Sam. Burt started his lecture with this sentence, "Exactly 25 months ago the announcement of the ψ/J particle by Professor Ting's and my groups (1, 2) burst on the community of particle physicists." That is his only mention of the J, but his first reference is properly to Sam's paper because it appeared two pages ahead of his paper in Physical Review Letters. Burt's second reference is to his own paper and that discovery is properly the subject of his lecture. I see nothing wrong in Burt calling it the ψ/J particle; he acknowledges the simultaneous public announcement of the two discoveries on Monday, November 11, at SLAC and he references the papers in the "correct order." He later tells of the discovery of the ψ′, the ψ" and the ψ"′--excited states of the J/ψ, but since they weren't seen by Sam's groups they should not carry Sam's letter designator. (That is one of the few things about which Sam and Burt agree. Sam never mentions the ψ in his Nobel lecture, but he does tell of the subsequent discovery of the ψ'.)

Sam Ting goes out of his way to let everyone in the high energy physics community know that he still thinks Burt "stole his discovery." That message comes across from the following points Sam makes in his lecture.

1) He doesn't refer to the SLAC/LBL paper in his lecture and doesn't even list it in his three full pages of references--40 in all.

2) He never mentions the ψ, but as stated above he does mention the ψ'.

3) He shows the beam layout at Brookhaven at the time the J was first being seen. That diagram has no relevance to the physics being discussed in the lecture, but its caption tells "everyone in the know" what he is thinking. The caption says, "The AGS East experimental area. The MIT experiment is No. 598 at the end of Station A. Experiment 614 is that of Professor M. Schwartz (see Ref 22)." The diagram shows that the two experiments were set up within a few feet of each other so the experimenters must have been in frequent communication with the implication that they had no secrets from each other. Reference 22 is to Sam's article in Adventures in Experimental Physics, in which he tells how Mel Schwartz wanted "to see the mass plot of the resonance around 3GeV." (Sam, trying to preserve "security," bet Mel $10 that there was no such resonance.) Ref 22 continues, "One member of our group, S.L. Wu, and I later talked with Schwartz and other physicists and learned that at the time of betting not only Schwartz's group knew about the discovery but many others as well." (That is as close as one can get to saying "Mel told the SLAC group," without bumping into the libel laws.)

4) Sam reproduced a section of his typed proposal to Brookhaven for time to search for vector mesons--the J/ψ is such a particle. It says, "Contrary to popular belief, the e^+e^- storage ring is not the best way to look for vector mesons----[a] storage ring is best suited to perform detailed studies of vector meson parameters once they have been found." (The message here is clear.)

If I come across as being too hard on Sam, I must say that I understand exactly how he felt when he gave his lecture. As we shall soon see, in my Nobel lecture I purposely refrained from thanking Ed Lofgren and the people who ran the Bevatron because I knew of the serious roadblocks they had placed in our way while we were engaged in the work for which I received the prize. I had thanked them most sincerely in the earlier bubble chamber papers because they truly had been most helpful. But then, as I will tell, they behaved so badly that Don Gow told me, "I won't stay in this lab another day if I can help it. I'm going to look for a job in industry." Don was my oldest friend and most valued colleague and had always loved the lab in which he had spent his whole adult life. So things had to be really serious for him to make such a statement. And they were, as I'll tell later in this section.

The most difficult thing for me to recall in the remaining part of this chapter is the way my relationship with Ed McMillan detereorated after he became the Director of LBL, in 1958, after Ernest Lawrence's untimely death. I think that in earlier chapters I've made clear my long time friendship and admiration for Ed. I learned a great deal from him at the Laboratory in Berkeley before the war and he loaned me the precious Lauriken electroscope that he had built with his own hands and that I used in discovering nuclear K-electron capture. When we went to MIT in 1940, we were together much of the time, staying in the same hotel and eating most of our meals together. We worked closely on the same project--the development and installation of the world's first airborne microwave radar system. Many people thought of us as being a sort of pair of twins and I couldn't possibly count the number of times that physicists at APS meetings have addressed me as "Ed." And of all the scores of talented physicists who spent a year or two at the old Radiation Lab before the war, the Physics Department asked only the two of us to become staff members. At the end of the War, Professor Birge sent us almost identical letters expressing the hope that we'd come back to Berkeley as full professors in spite of the many "outside offers" he expected us to receive. (I can't speak for Ed, but I didn't receive any such offers; I like to think that it was because everyone knew that Berkeley was the best place for me to be and they didn't want to receive an "automatic rejection" of any offer.)

After the war, Ed and I were treated equally by Ernest Lawrence. He encouraged us to build the new types of accelerators I've described earlier, he nominated us for the National Academy at the same time, he hired us both as consultants to his television company and he appointed us Associate Directors on the same day. And on the personal side, we and our families had a close relationship; Geraldine and Ed's wife, Elsie, became very close friends. (Elsie's sister, Molly, was Ernest Lawrence's wife; the two sisters both married future Nobel Laureates.) When Ed became a member of the Bohemian Club, I invited him to stay at "my camp" during his first encampment and we spent almost all our waking hours together. (Rowan Gaither had invited me to be a permanent member of his camp.) The next year Ed joined Ernest Lawrence's camp, of which Don Cooksey and John Lawrence were members. When my father was my guest at the Grove a year or two later, Ed and Elsie invited the two of us for lunch at the summer home they had rented nearby on the Russian River. I could go on at length, but I think these recollections support my memories of a very close personal relationship with Ed before Ernest Lawrence's death. (And of course, we were both ushers at his memorial service.)

When Ernest died on the operating table at the Stanford Medical Center in the summer of 1958, an obvious question was of course, "who should be the next director." I think that everyone recognized that one of the things that distinguished me from Ed was that I had directed some large and effective organizations--my radar projects at MIT and my present bubble chamber group--whereas Ed had always managed to avoid such management roles. At the same time I believed that we all--certainly I-- knew that I could not do an effective job as Laboratory director because I was completely dedicated to liquid hydrogen bubble chamber physics, and thought that people working on propane bubble chambers and spark chambers were wasting their time and that is not the kind of attitude that would be acceptable to the "time wasters." If Glenn Seaborg had not very recently accepted the job of Chancellor of the Berkeley Campus, I think he would have been everyone's choice for the Laboratory Director, and I would not have experienced the pain that I'll soon describe.

Shortly after Ernest's death the University's President Clark Kerr asked me and my fiancee, Jan, to have a private dinner with him and his wife, Kay. (Kay was the daughter of Edgar Spaulding, the man who married my Aunt Florence, and as I said earlier, for a person like me, who had grown up without any cousins, she was a most welcomed new member of the family. So I had known Kay and Clark from the time they were married, and he joined the Berkeley faculty.) The dinner conversation was about family matters and other trivia, but after coffee, Clark invited me into his study for a serious talk. He let me confirm his belief that in the first place I didn't want to abandon my very exciting research career for one in administration, and secondly, that even if I were appointed, I couldn't run the lab in the "balanced manner" that would make me acceptable to a large fraction of the physicists.

Then he said (approximately), "Well, that leaves only Ed; I don't think he can run the lab, but what do you think?" I agreed that there was nothing in Ed's life history that would suggest that he had any of the administrative skills, or even interests, that one would want in a director. But I said that even though I found it hard to imagine Ed as the director, he had two important things going for him. Firstly, he was the only available really distinguished scientist--he already had his Nobel prize--and secondly and more importantly, he was very smart and if given the job, I was sure he'd work very hard to learn how to do it effectively. And of course I assured Clark Kerr that if Ed were chosen, I'd do everything in my power to help him in any way I could. My memory is that Clark said I had convinced him and I remember clearly that he said he'd try Ed for one five year term and see how he could handle the job.

My reason for recalling the "one term" statement is that five years later, after Clark had appointed Ed to another five year term, I had a visit from a very disturbed Emilio Segre. He said he and Edward Teller had compared notes and found that neither of them had been consulted about the reappointment and agreed that if they had been, they would both have advised against it. Emilio asked me if I had been consulted because he knew that if I had been, I would have agreed with him and Edward. I said I hadn't been consulted and although I had never before told anyone of my evening with Clark Kerr, I then told Emilio about Clark's plan to give Ed one term and then to check up on his performance. We felt quite frustrated and helpless at the thought that Ed would be "running" the lab for another five years and although Emilio and I have disagreed on many things in our lives, we both felt that another five years of Ed as a director would be a disaster that the lab might not be able to survive.

Now that I've mentioned Clark Kerr and am about to tell of the dissolution of my long time friendship with Ed McMillan, I'm reminded that such breaks are not so rare as one might think. When Glenn Seaborg left Berkeley to become John F. Kennedy's Chairman of the Atomic Energy Commission, the man appointed by the Regents to take Glenn's position as Berkeley Chancellor was Ed Strong, a professor of Philosophy and wartime administrative member of Ernest Lawrence's Radiation Lab. Clark Kerr and Ed Strong were members of the same camp at the Bohemian Grove and were close friends, which made them comfortable in their respective roles as number one and number two in the university hierachy. But then, during the "Free Speech Movement in the 1960's, on the Berkeley Campus, Clark Kerr apparently decided that Ed Strong wasn't doing an adequate job in running the campus, and recommended to the Regents that he be fired. Although I can't document everything I said in the last sentence, it was "common knowledge" among the faculty and is consistent with two things that do have some "experimental backing." The first is that Ed Strong did step down as Chancellor and the second is again "common knowledge"--that Clark and Ed haven't spoken to each other in years. That view is consistent with my yearly observation on visiting their camp at the Grove, that they haven't been there together at the same time. The last time I saw Clark there was when he introduced me to the newly appointed Berkeley Chancellor, Roger Hynes. (Roger and I developed a close friendship and an openly acknowledged "mutual admiration society." He later stepped down as Chancellor after a heart attack and I now see him as Director of the William and Flora Hewlett Foundation. He is in charge of spending the money that Bill Hewlett's first wife, Flora, left to charity when she died several years ago.) Ever since the day Clark introduced me to Roger, I've been conscious of the fact that Ed Strong welcomes me to his camp each year and Clark is nowhere to be seen. (I've just checked the list of camp members and I find that Clark is no longer a member. He is still listed as an honorary member of the Bohemian Club, but for all practical purposes, he has given up his association with the Club, no doubt because of the "Ed Strong affair."

My first indication that my relationship with Ed McMillan had changed came one day when I visited him in Erenst Lawrence's old office. I had often visited Ernest there to give him a quick suggestion about some problem that needed attention. Everyone in the lab knew that I was personally and professionally close to Ernest, so they used me as a channel to "get their message through" to Ernest. (In my experience, all successful organizations employ such informal Channels to bring problems to the attention of the Chief Executive Officer. As an example, the famous "Einstein Letter" that started the Manhattan District was not sent to President Roosevelt through official channels, but was instead delivered to Alexander Sachs, an economist who "had the confidence of the President.")

I naturally assumed without giving it any conscious thought, that I would act toward my longtime friend Ed, as he and I had so long acted toward Ernest. But I found that Ed seemed to feel that I was overstepping the bounds of propriety when I brought this particular issue to his attention. (I haven't been able to dredge up from my memory what it was that I told him--all my memories of that visit to his office are concerned with his obvious displeasure at my action, which I felt he interpreted as my attempt to usurp some of his recently acquired power.) So I never made that mistake again.

For a long time I rationalized Ed's change in his attitude toward me was related to the break-up of my marriage. It is generally accepted that when a marriage breaks up, the couple's friends "choose sides." Since Jan and I have had no social relationship with Ed and Elsie since our marriage in late 1958, I assumed for a long time that they had chosen "Geraldine's side," and that was what led to the detereoration of my friendship with Ed. But as I reviewed the situation before starting to write this chapter, I realized that that explanation was quite incorrect. Elsie, Geraldine's close friend, went out of her way to be friendly to Jan during the year we were engaged and waiting for the divorce to become final. She invited Jan to join a group of wives of senior lab personnel, who had lunch once a month at Trader Vic's restaurant. They were all obviously pleased to have a charming young lady join their club and I must confess to be embarrassed to have thought for so long that Elsie was "at the bottom" of my steadily eroding relationship with Ed. On reflection, I realize that she went out of her way to express support for me and my new wife-to-be. (Sorry, Elsie!)

My first hint that "things were really bad," between me and Ed came when Jan and I were extending our honeymoon as guests of Alfred and Manette Loomis at Montego Bay, Jamaica in early 1959. My group had set up the 15-inch liquid hydrogen bubble chamber in a high energy separated beam of K-mesons that had been developed by Harold Ticho and his colleagues for use with that chamber. I was a member of this collaboration, as I had not been in an earlier "associated production" experiment in the 10-inch chamber--described at the beginning of this chapter. The purpose of the new experiment was to see for the first time the Xi Zero hyperon, which was predicted to exist by Murray Gell-Mann's theory of Strange Particles. It was a terribly difficult particle to detect--it was neutral, so it left no track in the bubble chamber and it decayed into two different neutral particles, which also left no tracks. But it was of crucial importance to the theory so we worked hard to find the Xi zero. By the time Jan and I left for Jamaica, we had one "good candidate," which we soon showed was the first Xi zero ever seen.

But while we were enjoying our vacation in Jamaica, I had a terribly distressing call late one night from Myron (Bud) Good, a member of my group and of the "Xi zero" experiment. He told me over one of the worst telephone circuits I've ever used--I had to get under the bed covers with the phone, and shout so that Bud could hear me and Alfred and Manette in the next room, wouldn't be awakened--that Ed McMillan had come in that morning and announced that the 15 inch hydrogen chamber had to be moved out immediately and Wilson ~~Purcell~~ Powell's propane chamber would be put in its place. Ed's rationale was that Wilson would see more neutral Xi's than we could for reasons that I never understood, and which turned out to be wrong, because Wilson never saw a single one. The thing that made me terribly angry was that Ed never consulted me, and asked for my permission to put Wilson's chamber in the beam that had been designed and built by members of my group for use with our chamber to find an elusive particle of which we had one excellent example. I didn't learn that our chamber had been removed and Wilson's installed until it was a fait accompli.

I have never heard of a Laboratory Director who behaved in such a high-handed manner and it is not enough to point out Ed's demonstrably bad scientific judgment in substituting an inferior detecting device for the best one then available in the world. Wilson did in fact not see a single Xi zero and our group has always been properly credited with the discovery of the Xi zero. (Now that everyone believes the Gell-Mann "strangeness rules," the existence of the Xi zero doesn't seem so important.) But the magnitude of Ed's bad scientific judgment, to say nothing of his insulting behavior toward an important member of his laboratory staff, can best be appreciated by noting that the pictures we obtained of high energy K-mesons in hydrogen, led to the discovery of the first three "strange resonances"--the achievement that was the primary citation in my Nobel prize award, eight years later. If Ed had left the 15 inch chamber in its K-beam, we would have had much better statistics when we announced the discovery of the first three strange resonances. (It was more than a year later that the Powell group, knowing of our quite unexpected discoveries, was able to confirm them from their prpane pictures.) I mention this to show how dangerous it is for a man who has not thought seriously about a scientific matter to "second guess" a person who has a "good track record" in a certain field and who has thought deeply about the problems for a long time.

In my view, every decision that Ed made concerning high energy physics in this period turned out later to be an obvious mistake. Ed couldn't do anything about the fact that we were the liquid hydrogen bubble chamber group at the laboratory--that was something he inherited from Ernest Lawrence. And although almost everyone else in the world acknowledged that liquid hydrogen bubble chambers were the "detectors of choice "in that era, Ed did his best to push bubble chambers with "heavier liquids," such as the propane used by the Powell group and the unbelievable Xenon, used by Don Glaser's group, which Ed imported, enmasse, from Michigan. I have unbounded admiration for Don Glaser's invention of the bubble chamber, but in my opinion, every decision Don made after his "proof of the method," turned out to be wrong. He had the choice of substituting some other liquid for his original ether, (a hydrocarbon) and instead of going to the lightest liquid--hydrogen, which history shows was the proper choice--he opted for the nearly heaviest liquid--the extraordinarily rare and expensive Xenon. Again, history tells that that was the wrong choice; I can't think of a single experimental fact that came out of the Xenon chamber.

Don was obviously "Ed's man;" so far as I know, Ed brought him and his group of several post-doc physicists to the lab without consulting any of the lab's senior staff. (One member of the group was George Trilling, who has had a distinguished career at the lab and as Professor of Physics.) But after trying unsuccessfully for some time to do some effective physics with his Xenon chamber, Don won the Nobel prize in 1960 and promptly gave up his career in physics for one in molecular biology. He has been successful in the formation of two companies in the field of "genetic engineering," but my two close friends n the Virus Laboratory tell me that he has not been effective as an academic research worker in their field. But he has been outstandingly successful in two fields and that is much more than most scientists can claim.

Don should certainly have won the Nobel prize for his invention of the bubble chamber which is just what his citation said. But I wouldn't be truthful if I didn't add that many people had led me to believe that I would share the prize with him for the development of the hydrogen chamber and discoveries made with it--my citation for the 1968 prize. (The historical precident is that C.T.R. Wilson won the prize for the invention of the cloud chamber and Patrick Blackett won it for the development of the method and for discoveries made with it.) I know of two joint nominations that were made for me and Don and my memory is that I knew of three such joint nominations. One was prepared by a close associate of Glenn Seaborg's and signed by Glenn. I was given an opportunity to check it for accuracy. So the fact that the Nobel Committee rejected three strong recommendations that Don and I share the prize and gave it to Don alone, indicates to me that there must have been at least one strong nomination for Don alone and saying that what others had done with his invention were mere "exercises for the student," and quite obvious to "anyone skilled in the art," as the patent literature expresses it. I'll leave it to the reader to guess who might have written such a letter. (I had the satisfaction in 1968 of having several people say to me, "Well, Luie, this is the second Nobel prize you've won." That was a code message which we all understood--that without my (group's) demonstration of the great power of the bubble chamber method, Don wouldn't have won the prize for what he had shown to be an "interesting toy.")

I've told a number of people that I am one of the few scientists who knows that the Nobel Committee voted "no" on his nomination for the prize. Another member of that small group is Art Schawlow, who is generally credited with inventing the LASER, with his brother-in-law, Charlie Townes. They were nominated by several people to share the Nobel prize, but in 1964 the prize was awarded to Townes, Basov and Prochorov--the latter two being Russian physicists. Charlie was at Cal Tech the morning he was called by the members of the Press to tell him of his good fortune. He was at that time Provost of MIT, and the heir apparent to the presidency. He had apparently resigned himself to spending the rest of his life in university administration. But that decisiion was suddenly reversed by the following incidents: His wife, Frances, took the calls and she asked, "Did Charlie share the prize with anyone?" (Naturally, she expected to be told that he shared it with Art Schawlow.) When she was told that he shared it with two Russians, Frances blurted out a number of most uncomplimentary comments about them, saying that they really hadn't done anything in the field and that Art Schawlow was the person who had helped Charlie invent the LASER. It was pretty early in the morning and she was terribly disappointed and she didn't realize that all her words were being recorded on tape and would be played and replayed on the radio for the rest of the day. The Trustees of the Institute were apparently quite upset about this performance and the next thing everyone knew was that Charlie Townes accepted a position as Professor of Physics at Berkeley. We were of course all delighted to have him as a member of our faculty and he has since then done some really spectacular work in infrared astronomy. (If he had been home that morning, he would have handled the Press in his typical diplomatic manner and he would no doubt have been a very successful President of MIT and no one would know that "he still had a lot of good science in him.")

I'll now return for a short report on my next few interactions with Ed McMillan. In the early 1960's the Bevatron was shut down for major repairs. Everyone accepted this loss of experimental time and made plans to be "back on the air" as soon as possible. It was agreed that the 72-inch bubble chamber would operate more effectively if the high energy beams we used didn't have to pass through a metal window on their way from the Bevatron vacuum chamber into the atmosphere, and then back through another window into the "electrostatic separator"--such passage scatters the beam and spoils the optical properties of the separator system. We asked Ed Lofgren, the Bevatron director, if we could have the "vacuum coupler" installed on the new Bevatron tank during the shut down. He said that would be impossible for two reasons--they didn't have any spare engineers and all the large machine tools in the shop would be tied up, so we would have to wait until after the shutdown was over to start the design. Don Gow and I discussed the situation with Paul Hernandez, who had designed the 72-inch chamber, as head of the mechanical engineering department at the Lab. Paul said he'd be happy to do the design during weekends when the Lab had no call on his time. He discussed the matter with Bill Salsig, who was chief engineer of the

Bevatron and who said he would check all of Paul's blueprints--also on his weekends--so the design would be fully compatible with all safety considerations that were the proper concern of Ed Lofgren. The two of them talked with the U.S. Navy, at the nearby Mare Island Shipyard and found them willing to make the required parts in the same time and at the same cost as the Lab shop would require. So we felt sure that Ed Lofgren would accept this plan devised by two engineers he trusted. But he would have no part of it. I then asked for a meeting with Ed McMillan, Ed Lofgren, the two engineers, Don Gow and me. Ed McMillan heard the whole story and said we couldn't use Paul and Bill on their free time. They argued that they really wanted to do the job and it would in no way interfere with their normal Bevatron assignments. But Ed McMillan was adamant and since he was "head of the lab," Paul and Bill had to accept his ruling.

After the session ended, we (the petitioners) all agreed that we had never before seen such a blatant exercise in pure cussidness; Ed didn't care at all how unreasonably he was acting--the exercise was one simply to throw a road block into the 72-inch bubble chamber program. I've already recounted Don Gow's reaction--he would not spend another day at the Lab under such a Director. And shortly after that he left the lab for the presidency of a nuclear instrumentation company. Two years later he found himself in a situation that was even more unbearable than the one I just described and he shot himself to death in a period of great despondency. I know the details of the situation in which he found himself and they were not of his making. But that is quite another story that doesn't belong here. So that is how I lost my best friend (and the best man at my wedding to Jan), and the man for whom my young son, Don, was named.

I could go on at length, but if I recounted all the roadblocks that Ed threw in my path during these years the reader might conclude that I was simply paranoid. So I'll just mention the fact that Ed prevented Melvin Calvin from hiring Jan as a research associate in his group. Ed invoked an "antinepotism rule" to assure that Jan couldn't be paid by Melvin, one half mile from my office, when that same "rule" had never prevented Sula Goldhaber from working in her husband's research group, with their desks side by side. But Jan worked as an unpaid collaborator and published a paper with Melvin in this period.

In his last attempt to show that his judgment was better than mine in the field of bubble chamber data analysis, Ed involved the Lab in a huge "seni-automatic" bubble chamber film-measuring effort, known as the "Hough-Powell device," (HPD) after its CERN-based inventors. He transferred Jack Franck's engineering group from its world-renowned position as a sub-group of our bubble chamber group to a position under the direct supervision of Howard White--the computer programmer who had developed the programs for the propane bubble chamber group under Wilson Powell and Bob Birge. All the other bubble chamber groups worldwide used programs that originated in Frank Solmitz's section of our group--not those that came from Howard's efforts. The HPD programs at Berkeley and at CERN were almost "total flops," in that our Spiral Readers always measured several times as many events per year as did the HPD's, even with Ed's unlimited backing of the HPD's. Jack Franck never again did anything of note at the Lab and after a number of years of near obscurity, he took "early retirement," and left the lab where he had been so productive in his many roles in support of the linear accelerator and later of the bubble chamber. Howard White went from programmer to head of a large engineering organization and then quickly back to his old role as a programmer. This is a good example of the old adage that "you can't make a silk purse out of a sow's ear." Both Jack and Howard had talents in their accustomed roles, but Ed put them in impossible positions and the HPM program was a personal disaster for him--in my view and in the view of most of the bubble chamber fraternity, worldwide.

The reader will certainly try to understand why Ed behaved the way he did; the facts are incontrovertable. I think I understand the reasons, but I will refrain from setting down my theory to avoid looking like a "do-it-yourself-psychologist."