Mathematical Physics and Quantum Field Theory,
Electronic Journal of Differential Equations, Conf. 04, 2000, pp. 197–206
http://ejde.math.swt.edu or http://ejde.math.unt.edu
ftp ejde.math.swt.edu or ejde.math.unt.edu (login: ftp)

Columbia physics in the fifties: Untold tales *

J. Sucher

Banquet speech given in Honor of Eyvind Wichmann

I've probably known Eyvind Wichmann longer than any of the other participants at this symposium in honor of his seventieth birthday. We were both graduate students at Columbia University in the fifties, a decade of remarkable creativity by a star-studded physics faculty. I thought it would be fun to share with you some reminiscences about our time there. I also want to tell you about the decisive influence Eyvind had on my own career, but not in the way you might imagine. Finally this occasion gives me the opportunity to give Eyvind something which I've been dreaming of doing for more than forty years but which at the time I was not rich enough to buy. But don't get your hopes too high, Eyvind.

When we were at Columbia, the faculty included Isidor Isaac Rabi, already then a father figure, and a younger crowd, including Leon Lederman, Tsung-Dao Lee, Polykarp Kusch, James Rainwater, Jack Steinberger and Charles Townes. When I arrived in the Spring of '52, Hideki Yukawa was completing a sojourn of several years at Columbia, and Willis Lamb and Norman Ramsey had left not long before. *All* of them either already were or eventually became Nobel Laureates. Other luminaries included Henry Foley, Norman Kroll, Robert Serber, and last but far from least, Chien-Shiung Wu, whose historic experiment on parity violation in beta decay took place during that period. There was also a high-powered class of students, which included several future Nobel Prize winners, Leon Cooper and Mel Schwartz, and other stars-to-be, such as Gary Feinberg and Nick Samios.

My first meeting with Columbia physicists came sometime in the fall of 1951. I was to graduate from Brooklyn College at the end of the semester but few graduate schools admitted new students in the spring. Fortunately, one of my teachers at Brooklyn College was Melba Phillips, who not only knew everybody at Columbia but also recommended me for a fellowship that had unexpectedly become vacant. I went for an interview, and after a long, tense subway ride from Brooklyn ("Don't forget to change at 96th St. for the #1, if you get to 125th St. you've gone too far," the secretary had warned me) I found myself in an office with Kusch and Foley. I had never heard of either one but I found them very

Mathematics Subject Classifications: 01A70, 01A80, 81-03.
Key words: Eyvind Wichmann, Columbia University, Nobel Laureates, eightball, billiard ball.

Published July 12, 2000.

intimidating: they both stood more than six feet tall and towered above me. I was only five foot seven. After a while a little man entered the office. He was introduced to me as "Professor Rabi," and he was already famous enough to be known even to undergraduate students. Nevertheless, I immediately began to relax. I towered above him, almost by a head.

My first class that semester was a six-credit course on classical mechanics given by Professor Quimby. Those were the days when real men took, or rather were forced to take, six-credit courses. Quimby's was very demanding, trial by fire for the first-year students full of arcane stuff about spinning tops and "polhodes and herpolhodes," whatever those are. Most scary were Quimby's demonstrations: he would spin tops with great enthusiasm and greater force on the surface of his large desktop. These would invariably launch themselves into space and arrive like bullets in the captive student body, which scrambled for cover. The less hardy of us used to sit way in the back.

Quimby's first name was Shirley, the same as that of my mother-in-law. I found this quite astonishing, as did my father when I reported this fact. He wondered whether an unknown Mrs. Quimby would say, "My husband Shirley . . . " My father had studied mathematics at the University of Vienna in the 1920's and took a great interest in my studies. He would often bring home books on physics or mathematics from the Brooklyn Public Library for me to peruse when I came home for weekends from Manhattan, a respite from my tiny dorm room in Furnald Hall on 114th St. My mother's gefilte fish was also an improvement on student fare. Once my father brought home a book by the renowned mathematical physicist, Hermann Weyl. I happened to mention the book months later and he said: "You mean the book by Hermann and his son Joachim."

I said, "No, the book is by Hermann Weyl alone. He just thanks his son in the preface for help with the English translation."

An argument ensued over who was right about this and finally he said, "O.K. I 'll go to the library next week and check on it."

When I came back to Brooklyn the following week, my father met me at the door with a big smile: "Well, you were wrong! The book was by both of them!"

I was staggered. "But that's impossible. I'm absolutely sure it's by Hermann Weyl alone." Then I added, "Did you actually go back to the library and look?"

"No, I didn't have time this week"

"Then how do you know you're right?"

"Well, I just thought about it some more."

After that, my regard for the power of pure thought was not as high as before. However, it was not so low that it kept me from going into theory.

Eyvind was a year or two ahead of me. We first met in my second year, when I was assigned a desk of my own. I entered the designated room and found it filled to the brim with desks, jammed together in the classic graduate student style. There was no one there, except for a massive, studious-looking figure hunched over a large tome in a far corner. After a few pleasantries and handshakes in the European style, we each went about our business. I began sharpening pencils and daydreaming, while Eyvind applied himself to what I later discovered was his great self-appointed task: solving all the problems in the classic book *A Course in Modern Analysis*, by Whittacker and Watson. These problems were notoriously difficult, many taken from Cambridge University mathematics contests. They often began with the command "Shew that . . . " with, I assumed, "shew" pronounced as Ed Sullivan did on his television show, "shoo." I, for one, was certainly shooed away.

Occasionally, I would find messages from Eyvind on my desk, written in grand rhetorical style. I remember one in particular: "The MINISTRY OF EDUCATION called to inform you that unless the books you have borrowed from the library are returned IMMEDIATELY, serious action will ensue, which will jeopardize your stay at this Institution . . . " Eyvind still enjoys using the phrase "the Ministry of Education." I attribute this to some early childhood trauma involving his schooling in Finland.

The Ph.D. qualifying exam at Columbia was a real ordeal for most students. I studied for it in the summer of '53 up in the Catskills, where my wife Dorothy and I were staying with her grandparents. They had come from Russia in 1905 and never had much education. Dorothy's grandmother in particular was puzzled because I spent so much time studying. One day she asked Dorothy, "Why does he always sit with the books so much? I would rather be splitting rocks in the forest!"

"He's studying physics."

"Physics? What is physics?"

Dorothy thought for a moment. "Grandma, have you ever wondered why the sun rises in the east and sets in the west and never the other way around?"

"No."

"Well, if you have a stone in your hand and you let go of it, it always falls down, never up. Have you ever wondered about that?"

"No."

Dorothy was getting desperate. "Grandma, have you ever heard of Einstein?"

"Einstein!? Of course I've heard of Einstein!"

"Well, Joe is in the same business."

This satisfied Grandma. I was never sure whether it was because of the connection with Einstein or because I was apparently at least in some kind of business.

The qualifying exam was not the only hurdle we had to pass at Columbia. Nowadays there are no foreign language requirements for the Ph.D. degree. But at that time we were required to pass not one but two language exams. Since I had spoken German as a child, I took an exam in German during the first semester. This ensured that I would at least pass something and bolstered my confidence. The following year I took a second exam in French, having prepared for this by studying a little book called *Teach Yourself French*, while still at Brooklyn College. I carried this book around with me at all times so that I could use the elevator without worry, although it was reserved for use by faculty only. Accosted by a professor asking whether I was a teacher I was prepared to say, "Yes, I am." If there was a follow-up question like "And what do you teach?" I would pull out the book and say: "I teach myself French!"

A classic case of the language exam tribulations at Columbia involved a very good student who had finished his thesis and course work and had a job lined up. But one tiny detail prevented him from formally getting the degree. He had been unable to pass his second language exam, in French. Another attempt was scheduled at the office of the professor who administered the exam. As usual he pulled a volume from his shelf of foreign language physics books, opened a page at random and left the room while the student struggled with the text. When he returned, he saw that the translation was almost adequate and probably good enough to warrant a pass, under the circumstances. However, on further inspection a dilemma arose. He had accidentally pulled a Spanish text from the shelf and the student had managed a translation, without realizing it was Spanish! The Talmudic question arose: Had the student passed his second language exam? Spanish was not one of the allowed languages!

All physics students were required to take two semesters of "Advanced Laboratory." This was administered by Professor Lucy Hainer, the only female faculty member until the arrival of C.S. Wu. Many of us, especially those of us who wanted to do theory, tried to avoid taking this time- consuming course by claiming that we had already had a "fully equivalent" course in college. Luckless lads: Lucy Hainer, a tough lady, would have none of it. "This is a *graduate level* course," she would say. However, when I went to plead my case, the gods smiled: My sentence was reduced from two semesters to one. "What does he have that I don't have?" may have been a dark thought of my classmates, especially those who had come from Ivy League schools. The answer was simple but impossible to guess. The lab course which I had taken in Brooklyn College was taught by Bernard Kurrelmeier, whom Lucy Hainer apparently held in high regard. Later I learned that he was her husband! However, the regard was not so high that both semesters were waived. Many years later, one of the rooms in Pupin was named the Lucy Hainer Lounge, in her honor. It's a bit ironic, for lounging was the last thing you would think of doing in Professor Hainer's stern presence.

The level of graduate instruction at Columbia ran the gamut from great to mediocre. The low point, by universal agreement, was a course called "Mathematical Physics," for which the text was *Spherical Harmonics*, published in 1896. The high point, for many of us, was the field theory course given by T.D. Lee, who then as now was wonderful in the clarity, depth, and eloquence of his presentation. Very occasionally he would reveal his ready wit. T.D. was on the Ph.D. final oral exam committee of my friend and classmate, Arthur Schwarzschild, together with Leon Lederman, Jack Steinberger, and Chien-Shiung Wu, Arthur's thesis adviser. At one point Steinberger asked Arthur a question about the behavior of a current-carrying wire in a magnetic field.

Arthur answered, Steinberger accepted the answer, Lederman then disagreed, and a discussion ensued between the two of them on the matter (perhaps I have reversed their roles). Arthur wisely kept his mouth shut. Finally they turned to Lee and asked: “Well, what do you think, T.D.?” The latter raised his hands palms out, to indicate that he was not going to get involved, and said, “I already have my Ph.D.”

For reasons we never understood, the course on statistical mechanics was given by the venerable Isidor Isaac Rabi, who had won the Nobel Prize in 1944 for his development of the magnetic resonance method for studying atomic and molecular beams and was the mentor of many future Nobelists. It pains me to say that he was not a good teacher. His lectures were rambling and unfocused. The word was that if you read Tolman’s massive tome on the subject, you could skip the lectures and pass the final exam. One witty student’s characterization of the course rested on the form of the initials of Rabi’s full name: I.I.R. It so happens that when an electric current of magnitude I runs through a wire whose resistance is R, the energy lost as heat each second is equal to I^2R or "*I squared R*". So the course was referred to by the students as an “I squared R loss.” Nevertheless, most of us attended religiously, because we knew we were in the presence of a great man.

In addition to the regular courses there was a series of lectures by visiting professors, including Abraham Pais, Freeman Dyson, and Murray Gell-Mann. These three as well as T.D. Lee taught the theory students’ favorite course: quantum field theory. Most of us took it over and over again, on the principle made immortal by the song about shoo-fly pie, “You can never get enough of that wonderful stuff!”

Bram Pais was a charismatic lecturer who had great rapport with the students. He once wrote a formula on the board, then started to worry about whether all the factors were correct. After a few fruitless minutes he turned to the class and said: “You know, the hardest thing in physics is getting right the factors of two, pi, and minus one!” We all felt better about ourselves after that.

I found Pais a sheer pleasure to listen to. He had a fine resonant voice, flavored by a slight Dutch accent. We didn’t know it at the time but, as a Jew in Holland during WWII, he had survived the Nazi occupation hidden by a Dutch family. He often eased the complexity of the material with interesting historical asides. One of these exemplified his opinion, which many of us shared, that “particle physics” was the real McCoy. In discussing the so-called adiabatic theorem he stopped and said: "Let me tell you about this theorem. It was first proved in the old quantum theory in 1918 by one of the rising young stars of physics, Johannes Burgers. Unfortunately, a chair in hydrodynamics became open at the University of Delft and the great Dutch physicist, H. Kramers, was on the search committee. He decided that Burgers, young as he was, was the right man for the job and persuaded him to apply.” Pais paused and then added, in a sad tone: “Well, Burgers was appointed and went on to become one of the world’s leading hydrodynamicists. And so he was lost to physics.”

When Freeman Dyson lectured, he discussed recent developments in field theory. His dry wit was much appreciated by us. Taking his cue from a fashion phrase of the day, he referred to some of these developments as "the new look." Once he discussed a topic of relevance to both quantum electrodynamics (QED) and the many-body problem in nuclear physics, the so-called disconnected diagrams. This problem had been solved in QED years before, but a number of nuclear theorists, including one whose papers were notoriously difficult to read, were struggling with it. When a student asked how it was possible that this theorist was unaware of the old solution, Dyson replied: "Well, it just shows that communication with him is difficult in both directions."

Murray Gell-Mann was rather different, a very intense lecturer who was keen to bring us to the forefront and introduce us to his own research. He also had a predilection and talent for neologisms. He discussed the idea of a hypothetical particle previously called the "intermediate vector boson," which would play the same role for weak interactions as the photon plays for electromagnetic interactions. But he called it the "universal X particle," abbreviated as "UXL" and pronounced like "Axel," with broad "a." However, unlike "quark" and "strangeness," it didn't catch on. Still, he hasn't given up. A few years ago he gave a colloquium at Maryland and used the term again in his lecture. Afterwards, I told him that I was in the Columbia class when he introduced the UXL. He grabbed my elbow and pulled me along, saying: "Joe, I want you in court with me."

Polykarp Kusch won the Nobel prize in 1956 for the first determination of what is called the anomalous magnetic moment of the electron: a tiny but definite departure from the value predicted by a long-accepted theory. When he came back from Sweden, he gave a reprise of his Nobel acceptance speech for the faculty and students. Because of construction in the building this was done in a makeshift room with plaster on the floor and ramshackle chairs, especially in the back where I was sitting. As Kusch neared the climax of his talk, he wrote the experimental value of the magnetic moment on the blackboard, to six decimal places, and then the theoretical value. I could not see over the people in front of me so I stood up as he posted these numbers. As the close agreement between theory and experiment became evident, wild applause began. This was interrupted almost immediately, when I sat down and the chair collapsed underneath me with a tremendous crash. All eyes turned back to see me lying on the floor. Kusch looked over the audience and glared at me, but after a moment he turned the event into an accolade and said: "Thank you, Mr. Sucher!" The applause resumed at full volume.

Kusch's collaborator in the analysis of his measurements and coauthor on the decisive papers was Henry Foley, my thesis adviser. Since Foley was very liberal in giving me time to discuss my work, I was often in his office when Kusch came in for discussions. Some time after Kusch got the prize, a new measurement of one of the quantities entering the analysis indicated that the experiment no longer agreed with the theory to the expected accuracy. When Kusch next appeared in the office, Foley said to him, "You know Poly, they're going to take away the prize!" Kusch, who was putting three daughters through school, retorted: "It's too late, Henry, I've already spent it."

My favorite Kusch story is one at which I was not present, but there is good authority for it. Having not long before heard the news from Stockholm, Kusch was in his lab, walking back and forth in great excitement and getting congratulatory phone calls. One of his students, a happy-go-lucky type who didn't pay much attention to what was going on around him, came up to him with a piece of equipment, complaining: "Professor, this isn't working right. Can you figure out what's wrong?"

Kusch, who had a deep voice, boomed: "Oh, please don't bother me with that now. Don't you realize what's happened? "

"No, what?"

"I've just won the Nobel prize!"

Whereupon the student, not easily awed, stared at Kusch in astonishment, dropped the equipment and exclaimed, "Who? YOU?"

I learned a lot from Foley. He was not an expert in field theory but he had great physical intuition. For technical advice I often spoke to Norman Kroll, Eyvind's thesis adviser. Foley taught me how to write physics papers. The first problem he gave me was to derive, from first principles, a short-range spin-spin interaction between two electrons (a delta-function term for the cognoscenti), which Andy Sessler and he had decided must be there on the basis of classical electromagnetic theory. The literature on the subject, which dated back to the work of Gregory Breit in the late twenties, had no reference to such a term. Foley guessed that there must be an error in Breit's papers which, perhaps because of the prestige of Breit, no one had noticed for more than twenty-five years. So I was sent off to study the classic paper of Breit, where he derived an effective potential between electrons, later called the Breit potential. I pored over the paper for several weeks with no progress. My wife, who had gone to the High School of Music and Art, noticed my increasing desperation and when I told her that I felt that I was beating my head against the wall, she said helpfully, "Well, why don't you give up?" A perfect example of the gulf between the arts and the sciences.

A few days later I found Breit's error (an inadmissable integration-by-parts) and jubilantly reported to Foley. He said, "Okay, write it up!"

I presented him with a draft a few days later and stood behind him as he read: "G. Breit long ago derived an expression for the relativistic interaction of two electrons. However, in reducing the result to nonrelativistic form, Breit made a mistake . . . "

Foley blanched and said: "No, no, no! You can't say that!"

He grabbed my pen and changed the second sentence to read: "In reducing the result to nonrelativistic form, Breit found a number of terms, shown below. In addition there is another term . . . " Later I learned that Breit was a tough costumer, extremely sensitive to criticism; I had had a narrow escape.

Foley had a wry sense of humor. The day after our first son was born in early '56, I came to his office to tell him the great news. He congratulated me but then added, in an offhand manner, "Well Joe, you're on the way out now!"

We always referred to Chien-Shiung Wu as Madame Wu, perhaps by association with the first great female nuclear physicist, Madame Curie. She exuded a majestic aura. I first got to know her when I took her introductory course on nuclear physics. Several friends of mine became her research students and I would occasionally hear stories about her. She was an extremely hard worker and expected nothing less from her students. One of them approached her on a Monday morning and said to her: "I thought you might like to know that I got married during the weekend!"

She replied, "Well, congratulations . . . but you know I really prefer my students to be single."

Madame Wu mellowed considerably over the years. I remember a conversation I had with her more than twenty years ago, during a conference in Berkeley. I met her on the street in the late afternoon. She had just come down the hill from the Lawrence-Berkeley Lab where she had picked up a transparent Lucite cube, showing a single electron track. It was quite beautiful and she urged me to go up there to get one. I told her I might do it tomorrow but at the moment I was trying to find a store where I could buy a long-sleeved shirt – I had not expected California to be so cold in August and I was freezing. She looked at me in a worried manner and said: "You should go back to your hotel and take a nice warm bath. And then you should call your nice wife!" I did exactly as she told me and also got the cube the next day.

When I was finishing my thesis in the spring of '57 and looking for a job for the fall, I met Eyvind on the 8th floor of Pupin, the physics building. He was now at the Institute for Advanced Study in Princeton and had accepted a faculty appointment at UC Berkeley starting in the summer. We discussed my job situation and I told him I had postdoc offers from a few well-known places but I also had an offer of an Assistant Professorship at the University of Maryland in College Park. At that time Maryland was not yet an academic stronghold. Eyvind considered the matter for a moment and then asked, in his penetrating analytical style, "Do you have any furniture?" Eyvind was at that time in the throes of organizing the family's move from Princeton to Berkeley and he was finding the task an intolerable demand on his intellectual and possibly his financial well-being.

I answered, "Well, yes, we do have some furniture, we have a baby . . . "

He said, "Could this position become permanent?"

"I suppose so, it's what they call 'tenure track'."

Like a voice from Mount Sinai, he intoned in his deepest rumble, "Take it!"

So I did. As a result we've managed to go almost forty years without having to deal with a moving company. This is what I meant when I said that Eyvind has had decisive influence on my career.

Some years after I came to Maryland, I received a preprint from Eyvind. It was an early draft of his great book: "*Quantum Physics*," which became Vol. 4 of the famous *Berkeley Physics Course*. This is a wonderful book and I wish I had read it carefully at the time – I might really know something now! I only got to about page eighteen, where I was stopped by a footnote, to which

I will return shortly. A few pithy quotes from the book will illustrate Eyvind's magisterial style:

When discussing the term "quantum jump," he wrote: "Through custom the word 'jump' has become well established as a colloquial term in quantum physics. In the opinion of the author it has not been a happy choice of term, and one may surmise that it has caused much needless suffering in the study of physics." Later he commented, "Although the author cannot assume any responsibility for the harmful mental images which might be formed if the reader studies Bohr's planetary model of the atom, he does not want to go as far as to outright *forbid* the reader to consider this model." And here is Eyvind as the guardian of linguistic accuracy: In discussing the unit of length called the Ångström, he added the following footnote: "In honor of the memory of the Swedish physicist A.J. Ångström, English-speaking people might make an effort to pronounce his name correctly. The first vowel Å is pronounced like o in long and the second vowel like the same letter in German or like the vowel *eu* in the French word *deux*."

Now I come to the footnote on page eighteen. In the text Eyvind wrote, "It should be clearly understood that no analysis of the measuring process in purely classical terms could ever lead to an uncertainty relation. The uncertainty relations reflect experimentally discovered facts about nature. The particles occurring in nature do not behave like classical point-particles, nor do they behave like small billiard balls: they behave quite differently, and certain kinds of measurements cannot be made or even imagined."

Following this, there is a footnote which says: "For some reason the *billiard ball* has come to play the role of the prototype of a classical particle in textbooks on quantum mechanics. The author, of course, conforms to this tradition. It may amuse the reader to know that the author has never played billiards and has never held a billiard ball in his hands. His knowledge of the alleged properties of billiard balls is, therefore, book knowledge, derived from texts on quantum mechanics."

I was galvanized. Here was a serious gap in Eyvind's education which I myself could fill! But a search through the Yellow Pages and a few phone calls led to a sad conclusion: I could not afford the cost of the planned remedy. But now, almost forty years later, I would like to present Eyvind with the gift enclosed in this plain brown wrapper, which you may have been wondering about. Eyvind, behold! Accept this brand new eightball as a token of my esteem!

I should add that even as an Assistant Professor, I could have sprung for a single billiard ball. But all the stores insisted that I buy a full rack of fifteen. Fortunately times have changed or I would now be the unhappy possessor of fourteen extra billiard balls.

Of course now that Eyvind has actually held a billiard ball in his hand, the footnote on page eighteen is false. The only remedy for that is a new edition of the book. I expect it to appear well before your 80th birthday, Eyvind.

As many of you knew, Eyvind has a very high standard for publication. What you may not know is that he attempts to extend these standards to others. A Finnish Don Quixote! He's probably inspired by the dictum, attributed to

Pauli I believe, that the worst thing you can do in physics is to spread false information. When the era of preprints began (now long gone, with the advent of e-mail and the physics network) many departments set up special rooms with shelves which displayed all the preprints received during the preceding month. These preprints were often produced in a competitive rush, and of course had not been subjected to any review other that of the author and perhaps some of his friends. As a result a separate shelf had to be created in order to display the *errata* which often followed the original article. This mad rush to publication was a deep affront to Eyvind's scholarly attitude and style. Rumor has that at one point he appeared in the Berkeley preprint library, approached the "erratum shelf" with a red magic marker and wrote in large letters on each sheet: SHAME!

I believe it.

I would like to conclude with one final tale, related to our time at Columbia. Among the students, Eyvind and my late great friend and collaborator, Gary Feinberg, came to achieve a special status. We felt that they knew and understood everything and what they didn't probably wasn't worth knowing or understanding. I will tell you a story about Feinberg which could have been about Eyvind.

As many of you will remember, the 60's saw the appearance of the hippie movement and a phenomenon called a "be-in" or "love in": young people would gather in parks, strolling around, playing the guitar, singing, throwing flower petals at passers-by, or, in the jargon of the day, "doing their thing." A graduate student invited Feinberg, a young assistant professor at Columbia at the time, to attend a love-in at nearby Riverside Park. He duly went, and when he encountered the student in the park the following conversation took place:

"So, what do you think of this, Professor?"

"Well, it seems very nice, but I don't really understand it."

"Oh, man, you're not supposed to *understand* it! You're just supposed to be here and *do your thing*."

Gary replied, "But understanding is my thing!"

We can all hope that understanding will continue to be Eyvind's thing and that he will transmit it to the rest of us for many years to come. Happy birthday, Eyvind!

Joseph Sucher
Department of Physics, University of Maryland
College Park, MD 20742, USA
e-mail: jsucher@physics.umd.edu