Recollections of Graduate School Days
(Columbia, January 1949 to September 1953)

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December, 2009

Introduction

Attending Columbia University, in those days, and in physics, was a special experience. The faculty was exceptional and the graduate students were equally exceptional. The combination made the place vital; the research going on at that time surely exceeded that of almost any other institution at any other time. At the same time that the students were engaging in first class research, under the guidance of the faculty, they were being highly trained by both courses and research experience, and they went on to do great physics at many other places. I hope, in these recollections to give some sense of what it was like.

I was fortunate enough to be a graduate student in those incredible years, and I would like, in this note, to recall my experiences. I want to explain how I became a physicist and why I went to Columbia, and for this purpose I must step back a bit and describe my undergraduate days, at least to the extent that they are relevant to these matters.

Undergraduate Research

At Harvard, as an undergraduate, I was a mathematics major and had only a first course in physics, and advanced courses in electro-magnetic theory, and group theory applied to quantum mechanics. (Looking back, it was really a strange assortment.)

My honors thesis adviser was George Mackey. He was unmarried at that time and lived in the same house (dormitory) as I did. We ate in the same dining room and since he knew very few undergraduates, as he came in to the dining hall (three times a day) he would look around, and if he saw me, sit down next to me. Well, I was really more interested in talking with my buddies than talking mathematics. One might say that despite my intentions I received a quite remarkably good education.

As a thesis topic, Mackey suggested developing Banach Spaces. Now, Banach, and others, had done all that, but I was not to look at the literature, but do it myself. Every week, we had a formal session (not in the cafeteria), where he would look at the theorems I had proved and the progress I had made. And, if I had gotten stuck, Mackey would give me a hint. It was, for me, an exceptional education in research at a very early age (19).

Well, I gathered, from the fact that I could prove anything (whether it was right or wrong) and the lack of any encouraging words from Mackey, that I wasn’t very good at mathematics and switched, when I graduated, from mathematics to physics. Years later, I met Mackey at a National Academy of Sciences Meeting and remarked that I had done okay in physics and it was good that he had made it clear that I shouldn’t remain in mathematics. He replied, “But you were one of the best students I ever had”. If he had conveyed that to me I would not have become a physicist. Actually, a solid mathematics background was excellent for the study of physics, as I always found the math part easy and could focus upon the physics. (In subsequent years, when teaching advanced courses, I found that almost all the students got hung up on the math and had no time left to learn the physics.)

In my senior year at Harvard I took a course titled, Group Theory and Quantum Mechanics, given by J.H. van Vleck. He had assured me that it wasn't necessary to know quantum mechanics in order to take the course. Somehow I survived. I had to select a graduate school to continue my studies and I asked van Vleck for suggestions. He replied that once the major football teams were Harvard and Yale, but now there were many colleges with fine football teams. He said the same applied to physics.

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Columbia accepted students at mid-year (I graduated from college in mid-year and most schools allowed entrance only in the Fall) and they offered me a Teaching Assistantship (TA) (which provided both free tuition and some money). Furthermore, in NYC I could live at home and that would mean free room and board. In short I could make money while going to graduate school. I accepted on the spot, and moved from Harvard to Columbia. I was supported by the TA for two years and then by the Office of Naval Research through a contract with my thesis advisor, Henry Foley, and then in my last year as a National Science Pre-doctoral Fellow.

Professors and Students

In looking back to my days at Columbia, I often recall something that happened close to the last day I was there. Just as I was about to get my PhD, Nobelist and "grand old man" Isidor Isaac Rabi took me out to lunch. As we walked along, he bemoaned to me the fact that physics at Columbia was no longer great as once it had been. He remarked that even if the various theoretical investigations and experiments the staff were engaged in proved successful (and he implied most of the experiments would not work) there would be little interest in the results.

Certainly nothing was of Nobel Prize caliber. Subsequently, and for work going on just then, Nobel Prizes went to 5 of the faculty: Polycarp Kusch, Charlie Townes, Tsung-Dao Lee, Jim Rainwater, and Willis Lamb. In subsequent years, when I was in administrative work, I often thought of this comment, in order to remind myself how much an administrator can "be out of it".

At Columbia I had courses from 7 Nobel Prize winners, all of the above, as well as Rabi and Yukawa. I also had courses from Robert Marschak, Henry Foley, Llewellyn Thomas, and Robert Serber. What a wonderful education! I could see the different approaches to physics and yet they all were successful.

In addition, many of the other members of the staff, with whom I had less interaction, were excellent physicists like: John Dunning, Chien Shiung Wu, and Norman Kroll. And often visiting were future Nobelists Jack Steinberger and Norman Ramsey.

Of my fellow graduate students, five went on to win Nobel Prizes (Leon Lederman, Melvin Schwartz, Martin Perl, Val Fitch, and Leon Cooper). And many of the others had excellent physics careers. I think of fellow graduate students Bob Frosch, Francis Low, Mike Sanders, Bob Mills, Peter Franken, Ali Javan, Jim Gordon, Gabi Weinreich, Arnold Honig, William Chinowsky, Joe Sucher, Eyvind Wichmann, Eugene Commins, and many others.

Research with Henry Foley

Somehow, and I don't remember how, my thesis advisor, Henry Foley, became my advisor. However, I first started doing research in a most curious manner. One day I was going to the library on the eighth floor of the physics building, Pupin, and in doing that I had to pass the department office on the right and Rabi's office on the left. Rabi was standing at the door to his office and asked me to come in. He explained that in old age he couldn’t read the numbers on the slide rule so well and would I please help him. I spent the afternoon doing arithmetic as Rabi called out 17 by 39, or raise 57 to the third power, etc. and I would call out the answers.

The next day, the very same thing happened. At this point I asked what he was doing. He explained the problem he was working on, of theoretically determining the electric field gradient at the nucleus of a complicated atom (q) in order to determine the nucleus quadrupole moment (Q) since the experiments measured only qQ. Since an outer electron polarized the core, it was non-trivial to determine q.

Soon, I was doing complicated quantum mechanics perturbation theory calculations. I remember the first day when I had some result and came into Rabi with great enthusiasm to show him my work. He never even looked at it; he only wanted to know what the result meant. I had no idea. I realized that Rabi’s method of doing physics required understanding at each line what one was doing. I have adopted that method and used it throughout life. That is in marked contrast with some of my colleagues who can simply do formal calculations for pages and pages. On another occasion I came in with clouds of mathematics (remember I had been a math major) out of which popped a hairy formula. Rabi didn’t want to even look at the calculation. He asked, “Explain it in words” Here was a great physicist, and he was uninterested in calculations and wanting the result in words! What I learned was that he had tremendous insight into atomic physics. It was as if he had spent his life inside of an atom. He would say that an electron just can’t do that, or something like that, and he would be right. Through the years I have tried to develop just such insight, call it intuition, in the field I have primarily worked in; namely, particle beam physics.

I told Foley about this and we thought it would be interesting to make the statistical atom (Thomas-Fermi) have some angular momentum and thus determine q, without complicated calculations and in the limit of many atomic electrons. That work resulted in a paper in the Physical Review. Actually I wrote the first draft and it was really bad. I wrote it like a detective story with the plot resolved in the last sentence or two. Gently, Foley taught me how to write a scientific paper with an abstract, an introduction, a main body and then a conclusion. In short, to say two or three times what one had done. It is, looking back, amazing that he didn't consider me hopeless and end my career right there.

Studying under Foley was a wonderful experience. Put it this way: I have never done nearly as well with any of my students. Essentially every afternoon was spent in Foley's office. We talked physics and the discussion ranged far from my thesis or even my current interest. I was encouraged to ask about anything in my courses that I might not understand or found not clear. One day each week the Physical Review arrived and the afternoon was devoted to looking through it and finding interesting articles, no matter what sub-field they were in. We made estimates on the board, or worked though some key point, or sometimes wandered down to the library to read a reference. In short, I learned how to read professional journals (in later years this would be published papers and, also, paper preprints and then electronic preprints) and, of course, I learned a great deal of physics.

However, we didn't seem to be making much progress towards a thesis. We got distracted with the interaction between electrons and after sorting that out, we wrote two Physical Review Letters. Then we studied liquid helium and learned about phonons and rotons, so we had the bright idea that if we quantized hydrodynamics, maybe all that would just "fall out". Well we tried a bit, but got stuck, so Foley suggested we talk with the "Sage of 116th Street"; namely Llewellyn Thomas. (At that time Thomas had his office in the IBM building, although he did teach courses at Columbia. In fact I had two courses from him: one in General Relativity and one in Numerical Methods. One doesn’t need to say any more about his range of knowledge.) In fact we got in the habit of consulting Thomas and went there frequently. He was so generous of his time and on each visit we learned much. However, we got no place with hydrodynamics and we dropped the subject.

Then, after Robert Serber arrived at Columbia (a consequence of the terrible California loyalty oath, a political nightmare that resulted in Berkeley’s loss and Columbia’s gain), Foley and I had the idea that we might do something in meson theory. After all, both of us would learn a lot from Serber, who was perhaps the world’s expert on the subject. Much time was spent talking with Serber, but we were never able to focus upon a specific thesis problem. Dropped meson theory also.

Then, at the start of 1953, Foley suggested that I extend the work Francis Low had done for his thesis on deuterium to H^3 and He^3. With the Korean War draft at my heels and an NSF post-doc to start that September, I went to work—day and night—and completed a rather lengthy thesis (both analytic and numerical work) in nine months. The thesis was read, with a fine-tooth-comb, by Norman Kroll, and after many changes it was accepted and I graduated. The thesis was published so, despite, our thrashing around, I left Columbia with four publications, which – in those days – was rather special.

It was also Foley’s habit to ask me to referee papers that came his way, and even to work thorough the thesis of one of my experimental colleagues on whose thesis examination committee he might be sitting. All this was, of course, wonderful training. In later years – you might say it was out of laziness – I followed the same procedure.

Since I spent most every afternoon, for about three years, in Foley’s office, I was typically there when his friends on the faculty came in. At first, I would excuse myself and step out, but after a while people got used to me and spoke rather freely in my presence. Thus I learned much departmental gossip.

For example, when Rainwater published his paper on the ellipsoidal shape of nuclei, he became the laughing stock of the theory professors, for the work was considered trivial. In fact there was talk of how it was embarrassing to Columbia. Subsequently, Rainwater received a Nobel Prize for this work. When I wrote to Rainwater congratulating him, he responded that that was the only theory paper he had ever written.

A second example was when one day Lamb came in quite furious with Rabi. Rabi tended to have a new idea every day, or at least every week, and with great enthusiasm he would describe latest thought. Usually they were wrong, but about once a year they were great ideas. However it seemed to have fallen upon Lamb to become the “house theorist” and, therefore, to analyze each idea. Lamb was just sick and tired of wasting his time in this activity. He said to Foley that as soon as he had an offer from a good university he would leave. This was after the Lamb Shift and he did not have long to wait.

The year of my graduation was the first year of NSF post-docs. Both Bob Mills and I had both received one, which allowed us to go to any institution (even a foreign one). For Mills there was no question and he was recommended to go the Institute of Advanced Study. (Out of which came the still-quoted theoretical break-through of Yang-Mills.) The faculty that knew me all said that the Institute wasn’t for me, but recommended that I go work with Hans Bethe at Cornell. I remember asking Foley how I would recognize Bethe (I seem to have thought I would meet him on the street; not in an office with his name on the door.) Foley responded graciously, even though it was an awfully stupid question, that there would be no difficulty as Bethe looked just like a theoretical physicist.

About The Teaching

I had a course in Quantum Mechanics, taught by Willis Lamb, and that was certainly a fine course, as Lamb was an excellent teacher. The following year, his first year at Columbia, Serber offered a course in Advanced Quantum Mechanics. That turned out to be the most impressive course I have ever taken, or even ever heard about. Serber simply asked what we were interested in. Shouts of anti-ferromagnetism, molecular structure, meson theory, shell model, and why the water molecule isn't straight, and on and on. Serber responded by saying next week for anti-ferromagnetism (and a series of lectures on spin physics was presented), two weeks later the water molecule (and a series of lectures on group theory and molecular structure was presented) and then we shall see what we want after that. So it went, on through the year, polished lectures on any subject in physics -- on a one-week's notice basis. Simply nothing was beyond him!

Serber took his lectures seriously and prepared them with care. Once I went into his office and looked at the large blackboard on which he had written out the very lecture he would present a few hours later in one of the classrooms. I was very impressed, but have to admit that I never prepared my lectures nearly as conscientiously.

Rabi, on the other hand, seemed not to prepare his lectures. Well, to be more explicit, they weren't even lectures. I took Statistical Mechanics from him and at the end of each lecture he would assign the next chapter in the book. Then, next time he would ask if there were questions about the chapter. Now, most students, to be honest, had not even looked at the next chapter, but for those who did, the hour would become a wonderful discussion between Rabi and the students. I actually found that I learned more from him than from many a polished lecture (and I have experienced some wonderful polished lecturers in my life such as those given by Schwinger and Bethe). In later years, although I did not follow Rabi's method, the deep understanding that came from him, made me enjoy teaching Statistical Mechanics more that any other subject.

One day, Rabi came into class and, as usual, asked if there were any questions about the assigned chapter. No one had any questions (probably because most everyone had not looked at the chapter). Rabi asked once more. Still no questions. "In that case", he said, "there is no need for a lecture" and walked out of the room. After that, there were always questions…

Then there was the professor – name withheld -- who announced on the first day that he would not be concerned with numerical factors. Forget about pi, numbers and even –i. One of the students pointed out that one couldn't forget about –i as it made a big difference in Schroedinger's equation or the commutation rules. The professor, reluctantly, accepted the comment. We students wrote him off as one that would never do anything significant in physics. In fact he made many wonderful contributions, one even of such importance as to be honored with a Nobel Prize.

I had Rainwater for a course in Nuclear Physics. Perhaps it was a clash of personalities (Rainwater was very slow and exact; I tend to be fast and often wrong), but I just couldn't keep awake during his lectures, and probably would have been flunked out of school if the student sitting next to me didn't continually wake me up. That person – we always sat together -- was taken with the subject and had no trouble staying awake. He did go on to win a Nobel Prize in that area.

Perhaps the most important thing to note, is the variety of methods of instruction. All from expert physicists and any student could resonate with at least one of the approaches. It is clear, from the outcome, that the students of that era learned a great deal, both of the facts of physics and in the approach to physics. (And saw that all of the diverse approaches worked!)

I have been urged to tell one final story that, actually, took place after I left Columbia, but I have heard it on good authority. I notice that Joseph Sucher, in his talk for Eyvind Wichmann, tells the same story, so perhaps it actually happened. We all know of people in high places that through various events fall to the lowest. One thinks of sports figures that have been shown to have taken drugs (despite their denials), or highly respected financers that turned out to have been involved in shady deals or even Ponzi schemes, or political figures, even those wanting to be president or vice president, or an up-right former DA then a governor, brought down by the uncovering of various sexual scandals. None of these, however, can match the rapidity with which Kusch went from the heights to the depths: He was in his office when he received a phone call telling him he had won a Nobel Prize. Kusch rushed out and seeing a graduate student rushed up to him, grabbed him by the collars (as was Kusch's warm way) and said, "I just learned I won a Nobel Prize". The student looked at him and said, "Who, you?"

Stories About the Students

Gregory Breit, of Yale, was a regular visitor to Columbia. Actually, he came to talk with various members of the staff, but generally the staff didn't want to spend time talking to Breit. Soon, someone had the clever idea of having me entertain him. That became my regular job and I think much to Breit's annoyance as he wanted to talk with real physicists; not a graduate student. But I learned a lot.

However the experience served me in good stead, for many years later I was in Switzerland at CERN for a sabbatical year and Gersh Budker, from Novosibirsk, came to visit for a couple of weeks. Much consternation amongst the staff, as no one wanted to waste time entertaining him. Then they thought of me (actually I knew Budker reasonably well) and I was asked to be his host. Well, that turned out very well for everyone. Budker certainly didn't want to waste his two weeks out of the Soviet Union talking with physicists (and perhaps he could explain that to me more readily than to some high official at CERN). So my entertainment was minimal, perhaps a few hours here and there, which pleased me. In sum, Budker was happy, I was happy, and the CERN management was happy.

Columbia was all business in those days. Tough and often not very human. Two guys came to graduate school from Cornell and found the atmosphere very different than what they had previously experienced. One left after the first year, went back to Cornell, did very well, and later became a professor in a most distinguished university. The other guy decided to "stick it out". Soon he was doing research in molecular beams, and it was not going very well. One day his apparatus sprung a water leak inside and looking through the window one could see the level of water rising even though "high vacuum" was being maintained. The poor guy – I shouldn't put it that way – left physics. A few decades later I heard from him for the first time. He called me up and asked for advice about his son's collegiate education. I asked about his life and learned that he had done very well and, I gathered, had made much more money than any of us in physics; not poor at all.

Actually Columbia wasn't all business and toughness. One of my fellow graduate students was discovered one night, when some professor turned on the lights in one of the first year physics rooms, having sex with his girl friend on top of the demonstration table. We worried greatly that he would be thrown out of school, but that didn't happen and if there was any reprimand it must only have taken the form of "don't do it again". That was wise, for this fellow both married his girl friend and went on to become a professor at one of our better institutions. My own story is not nearly so dramatic, but I did meet my future wife in the course, Heat and Thermodynamics.

One of the students in the molecular beams lab was very good at horse betting and in those days one had to go to the track to bet. So, we made a pool of money, and it was arranged that others would cover his experiment while he went off to make money for us. Generally that worked well, for he really knew his horses, but I have lost touch with him and don't know how he fared in physics, horse racing or, even, in life.

Despite the incredible vitality of Columbia in those years, one of the graduate students, who had also been an undergraduate at Columbia, decided he would change to Harvard after getting an MA. So, he went into Rabi to tell him this. Rabi asked why he wanted to leave. The student responded that the professors at Harvard were better than those at Columbia. Rabi didn't bat an eyelash at this outrageous, and inappropriate, remark. Rabi simply said, "That is no way to judge a university. You need to study what has happened to the graduates; not how distinguished are the professors." He then pulled out from his desk a list of Columbia graduates, which list contained some very distinguished physicists. The student decided to stay at Columbia and, he himself, in due course, became a fine physicist and a professor at a first class research university.

In contrast with what seems now to be the case – certainly is the case in Berkeley – everyone attended the weekly colloquia.

Essentially all the professors—there was much less travel in those days -- sat in the first three rows, and all – and I do mean “all” the graduate students sat in the many rows behind. There was no compulsion to attend, but we sure all did. And, as a result we learned a great deal of physics beyond the narrow research topic each of us was involved with.

Actually, the graduate students were a close-knit group and we each spent time learning what others were doing. I spent a great deal of time in the molecular beams experimental laboratories of Kusch and Rabi, and also in the molecular structure lab of Townes (where work leading up to the maser was heavily going on). In fact the appreciation of experimental physics continued long after I left Columbia, for I rarely attend the theoretical sessions at American Physical Society meetings, even though I am a theorist. Rather I go to hear the newest experimental results figuring that I can quickly learn some theoretical method, if needed, to understand the new result.

Kusch was a very severe task-master. In those days getting an adequate vacuum in a molecular beam device was not easy. Typically the pumps were turned on early in the morning and then -- maybe – in the evening the vacuum was good enough to start an experiment. Many hours of doing nothing. One day a group of students were sitting around playing cards so as to pass the time of day. Kusch walked in and was irate that the students were not looking lovingly at their apparatus and making small changes if necessary.

Actually, Rabi was just as tough, but the students quickly learned his routine. Every night, just at 10 PM, Rabi would call the lab and ask to speak, one by one, to each of his students. Clearly, he simply wanted to see if they were all there. So, the graduate students made it a point to be there at 10 PM, even if they only got there at 9:55 and left at 10:05.

Learning how to take exams means, in large measure, psyching out the professor and figuring what he/she will ask on the exam. By the time I got to my thesis defense, I was rather good at that, but the best example I have ever seen was from one of my fellow graduate students at a much earlier time in our careers. Three of us had desks in one office (we were TAs and then RAs), and we tended to study together. Preparing for the final in Quantum Mechanics (Lamb's course) two of us were busy studying many different things. The third person got "hung up" on deriving the Rutherford cross section using the Dirac equation. He spent hours and days on this one thing. We thought he was most foolish to waste valuable days before the final on this one thing. Then came the final. It had only one question on it: Derive the Rutherford cross section using the Dirac equation. Naturally this fellow got the top grade in the course. (However, simply because we were in the same office, and despite our efforts to keep our ears closed, and despite ourselves, we had learned enough that we got the next two grades.)

Final Thought

My going to Columbia was almost an accident. As van Vleck had put it, "football had extended beyond Harvard and Yale", but that was hardly a ringing endorsement – as I think it should have been – of Columbia.

But those years at Columbia shaped my life, and I think most of my fellow graduate students would say the same thing. I left Columbia able to be comfortable, in future years, in teaching quantum mechanics, nuclear physics, statistical mechanics, and field theory and able to do research in elementary particles, atomic structure, liquid helium, plasmas, lasers, and particle beams.

I trust I have made it obvious that I had a wonderful – and, of course, greatly appreciated -- few years.