Columbia Physics 1955-1965
a personal memoir

M. J. Tannenbaum
Physics Department, 510c,
Brookhaven National Laboratory,
Upton, NY 11973-5000, USA
mjt@bnl.gov

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In my career as an undergraduate and graduate student at Columbia, from 1955-1965, the Physics Department must have been the best ever assembled anywhere at anytime. During this period the Physics faculty had (at least) 11 members who either already had Nobel prizes or went on to win them. Not only were they present, but they also taught undergraduates. In Columbia College and graduate school, my Physics Professors were Polykarp Kusch, Charles Townes, Leon Lederman, T.D. Lee, I.I. Rabi and Jack Steinberger. Both Kusch and Lee were my teachers the year after they won the prize. Other eventual prize winners on the faculty during that period were James Rainwater, Sam Ting, Steve Weinberg, Mel Schwartz and Carlo Rubbia. There were also some other great teachers like Robert Serber and C.S. Wu who were equal to any of the prize winners. The only problem with this upbringing, was that I thought that this was the norm. Is it any wonder that I became a Physics major and a Physicist?

I entered Columbia College as a freshman in September 1955. I actually was on the waiting list and didn't get admitted until August. I think that there were two reasons for this, I was on the young side, age 16; and 10% of the Freshman Class of $\sim$ 550 was from my high school, Bronx Science. Columbia was quite a different place in those days: tuition was $750 per year, easy to pay with my N.Y. State scholarship of $350 per year and earnings from a summer job. I actually remember paying my tuition bill in cash one semester. Tuition then increased exponentially from my sophomore year to the present. Columbia had a certain renown in athletics in that era having broken Army (U.S.M.A)'s football winning streak in 1947 and having beat Stanford in the Rose Bowl in 1934, all under the legendary Lou Little, who retired as Football coach after my sophomore year. The freshman class of 1958, when I was a senior, tied for the Ivy Football Championship in 1961, led by Captain Bill Campbell, now chair of the Columbia University Board of Trustees. In my sophomore year, Chet Forte, class of 1957 won the UPI College Basketball Player of the Year Award, beating out Wilt Chamberlain. Sid Luckman and Paul Governali were still alive and Lou Gehrig was well remembered. The President of the United States was Dwight David Eisenhower, previous President of Columbia University and Supreme Commander of the Allied Forces, liberator of continential Europe in WWII. Other memorable events happened while I was in college, and not only in politics and athletics but also in Physics, as will be discussed later on.

My main extracurricular activities in College were as Manager of the Football and Basketball teams. This was difficult due to labs, etc, but not nearly as difficult for me as it was for the players. I was in the locker room for Lou Little's last game as head football coach, at Rutgers in 1956, and heard his memorable pep talk, something like "Don't think about winning this game for me; I've won enough games. Think about winning this game for yourselves." They won; which was quite different from the Rutgers game two years later, my last game as Senior Manager, when we lost 61 to 0. Nevertheless, it turned out that the concepts of teamwork, getting up after you are knocked down, the importance of pep talks and good coaches served me almost as well as my education in Physics at Columbia, since over my career Physics made the transition from an 'individual sport' to a 'team sport.' I also would like to emphasize that the athletic alumni are among the most successful and most devoted of all Columbia Alumni (Fig. 1).

This might be a good time to introduce the Physics majors (and one Chem major) who entered Columbia College in 1955 and who were also successful, many of whom came back to campus to celebrate the class of 1959 50th reunion in June 2009 with a nice reception in Pupin: Bob Eisenstein*[C], Allan Franklin, Norman Gelfand*[C], Alvin Halpern[C], Irwin Jacobs, Paul Kantor, Sheldon Kaufman, Joe Krieger[C], Don Landman*, Bennett Miller[C], David Miller[C], Robin Motz*[C], Uriel Nauenberg[C], Gerald Present, Harold Stahl, Michael Tannenbaum*[C], Cynthia Alff-Steinberger[C] (Barnard), Roald Hoffman(Chemistry-NobelPrize)–the * indicates Bronx Science graduates [1],

a) b)


Figure 1: a) 1957-58 Freshman Basketball Team, 'the flying fleas', Mike Tannenbaum, Jack Kahn, managers, Jack Rohan, coach. b) Class of 1959 flag bearers (all having participated in undergraduate athletics) preparing to lead the procession at our 50th anniversary Class Day, June 2009. (L-R) Mike Tannenbaum (FB, BB MGR), Ben Janowski (Fencing, All-American 1959), Norm Gelfand (FB,Wrestling MGR), Jack Kahn (BB MGR)

[C] indicates Columbia Ph.D [2]. Another amusing thing about this curious combination of physics majors and Columbia sports teams (Norm Gelfand was also the Manager of the Football team as well as the Wrestling team) is that it was a great nucleus of campus politics so that we managed to elect Ben Miller as class president by overwhelming margins.

My first taste of Columbia Physics was in Allen Sachs' Physics 6 course in the spring semester of 1956, the first of the 3 semester introductory course for physics majors. Prof. Sachs' demonstration of frictional losses in a pendulum was memorable, but even more memorable was the blizzard of March 1956 when we commuters from the Bronx managed to get to class on time by coming up the IRT from Columbus Circle but Prof. Sachs arrived 10 minutes late in hip boots, saying that he had walked down Broadway from the 168th Street station. (The exposed ground level tracks north of Columbia were covered with snow, cutting off upper Manhattan on the Broadway line). A different lesson in the importance of arriving in Physics class on time was given the next semester in Professor Polykarp Kusch's E&M part of the 3 semester course. One student whose seat was in the middle of the lecture room walked in about 10 minutes late on a cold day, heavily dressed, with an ear lap hat and lots of books and made quite a bit of noise as he was trying to get to his assigned seat (they took attendance in those days). Kusch turned around and pointed at him: "You! What is your name? Where are you coming from? I get to work every day at 7:28..." You can be sure that nobody ever came in late after that. Another great Kusch story occured when he was deriving Maxwell's Equations at the blackboard, and somehow lost a step in the derivation. People starting making suggestions from the audience, too many at the same time; and it got pretty noisy. Kusch turned around and (incredibly) whipped out a gavel from under the folded New York Times that was always on the table, hammered the class to order and said something like:"Even if I were not the world's greatest expert in this subject; even if I had not been consorting with Kings and Queens last year; by administrative fiat of the Trustees of Columbia University you would have to pay me due respect..." While this was happening, nearly everybody in the class was trying very hard to cover up our laughter. This was truly incredible, kids literally off the city streets being taught by last year's Nobel Prize winner, and he's human. I doubt that many undergraduates today would be taught by last year's Nobel Prize Winner (although the same thing happened to me in my Senior year).

Kusch was quite a character and a great Professor. I once went to his office to complain that I got 99 on a quiz but I should have got 100. The treatment was typical. The office was open and I knocked. He said "What do you want. Can't you see that I'm busy." Even though it was his office hour, I turned and started to walk away. Kusch came out of his office and said: "You! What's your name? What do you want?..." I don't remember whether I got the point or not; but what I do remember is that every undergraduate physics major who asked Kusch about the possibility of a summer job got one.

My job that summer of 1957 was at the Nevis Cyclotron Laboratory, but for the first month or so Bob Eisenstein and I worked for Dick Plano in a corridor on a high floor of Pupin using a beam compass that Plano had constructed to rule circles of precise long radiii on blackened photographic plates. These were to use as templates for the measurement of the track curvatures in bubble and cloud chambers; [3] and were very successful and widely distributed (Fig. 2). Two things that I remember about this period

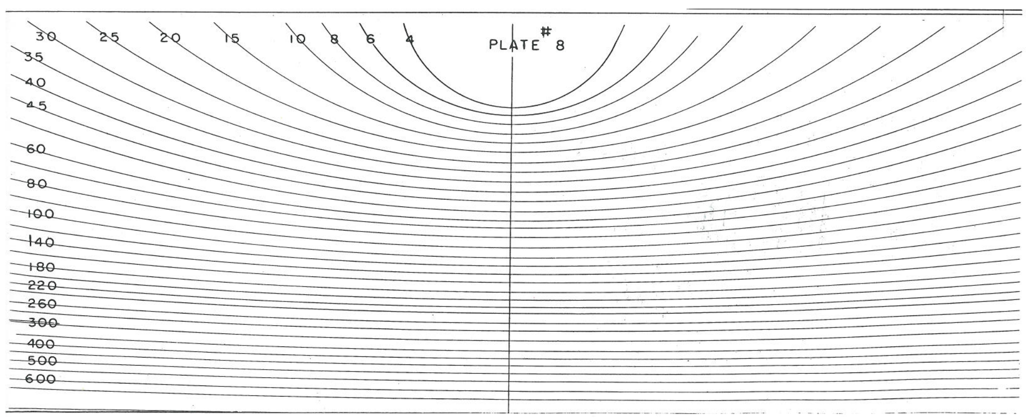
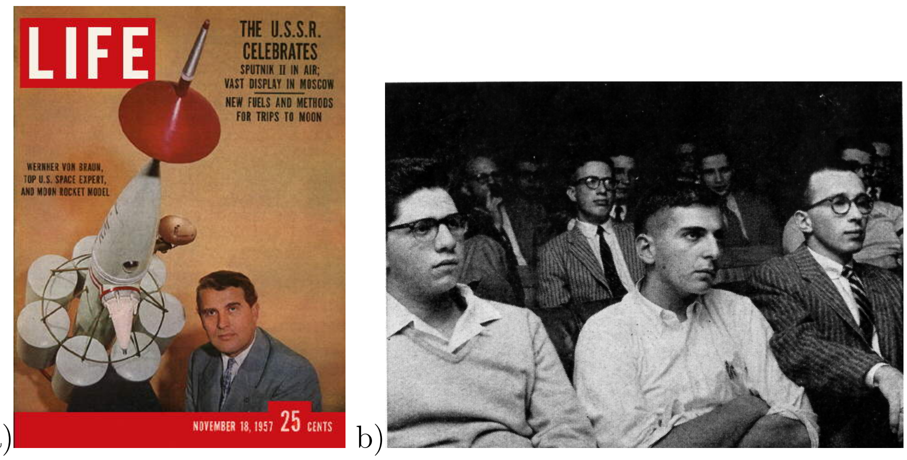


Figure 2: Template of precision arcs of circles with radii in cm. The relative radii should be preserved in this figure.

are that Bernie Margolis, who went on to be a leader in Canadian Physics at McGill University, kept on coming out of his office and telling us to keep quiet. We also met one of the Physics Shop technicians, Ernie Ihasz, if I remember correctly, who told us stories about Enrico Fermi. When I finally got to Nevis, my job was chopping dry ice for the operation of the Diffusion Cloud Chamber. I also made plastic scintillator and fondly remember the great softball games with Lenny Kohler, Jerry Rosen et al. (the graduate students). Also, don't forget that 1957 was the year that Parity Violation in weak interactions, which had been proposed by a Columbia Professor [4], was discovered at the National Bureau of Standards [5] and the Nevis Cyclotron [6] by Columbia Professors—more about that and other major physics developments in this period, later.

The period 1956-1957 was eventful in other ways. On October 8, 1956, Don Larsen pitched a perfect game in the World Series, and I actually watched the end of the game—including Yogi Berra's famous bear-hug—in the TV room in Hartley Hall while waiting for the bus to Baker Field. In this period some of us started an Undergraduate Physics Club and had invited Willy Ley, a writer of science fiction and rocketry, to give a talk at our club. When Prof. Kusch found out about this, he invited the officers of the club and Willy Ley to dinner at the Faculty Club. These same officers were asked to go to the Hartley TV room to watch President Eisenhower's speech to the nation after Russia launched Sputnik on October 4, 1957 which appeared in a photograph in Life Magazine of November 18 1957 (Fig 3).



a) b)

Figure 3: a) Cover of LIFE Magazine with b) the photo of (L-R) Bob Eisenstein, Mike Tannenbaum, Gerry Present watching Eisenhower's speech in Hartley TV room.

Another interesting thing that happened to me leaving the Hartley TV room for the Baker Field bus occurred in the fall of 1958 when I got to shake hands with Nelson Rockefeller who was campaigning for Governor on Amsterdam and 116th. He asked why all these people were taking the bus. I explained that it was the Football team. He asked who we were playing next. I said Dartmouth. He said, "Take it easy on them, it's my alma mater." In the same vein, as Chairman of the Managerial Council in 1958-1959, I was privileged to be sent as the representative of the Athletic Department to hear former President Harry Truman speak on campus. Naturally, somebody asked him how he could have dropped the first Atomic Bombs on Japan, killing so many people. Obviously, he had thought hard about this and had answered this question many times. He calmly answered that this ended the war and saved a planned Allied invasion of Japan with expected U.S. casualties of 500,000 and many more Japanese casualties [7]. There was no sign of anger at the question or regret of the decision in his voice or demeanor. (More about this later, also).

The most memorable thing about my Junior year 1957-58 (apart from taking Optics with Leon Lederman) was that I took intermediate E&M with

Professor Eugene Booth, who along other Columbia Faculty and students under John Dunning and Enrico Fermi, had conducted the first nuclear fission experiment in the U.S. in the basement of Pupin [8] which led, eventually, to the Manhattan Project. Booth, along with Dunning [9] and others then invented the Gaseous Diffusion Process for separating U^{235}. Dunning was Dean of Engineering while I was at Columbia, but I never met him. One day, without notice or explanation, Booth was replaced by Charles Townes, who finished teaching the course. Can you imagine: Townes, the inventor of the Maser [10] and Laser [11], who was doing or had just done his Nobel Prize work was the Substitute Teacher for Undergraduates!!! I also took the advanced Lab course (EKA) which was taught by Prof. Hayner, one of the two women on the Faculty. We each got our own vacuum tube at the beginning of the semester (I don't remember whether it was a triode or a pentode) and did several experiments with it. I also still remember the green mercury lines with the Michelson Interferometer.

My summer job in 1958 was again at Nevis and I got another taste of the Columbia Physics spirit. I was working for Sheldon Penman, building a vacuum tube double-pulser that he designed. It took me a while because it had some parasitic oscillations, which I figured out and fixed. I got it working at the end of one day and went home before I could tell anybody. Sure enough, the next morning it had vanished from my workbench, already in use in an experiment on the cyclotron floor.

In my senior year I had 3 Nobel Prize winners for teachers. I took a seminar course with Polykarp Kusch, which was great—we read original articles in the Physical Review and then discussed them. I took Quantum Mechanics with Jack Steinberger. He covered Landau and Lifschitz' textbook (Non-Relativistic) in one semester and since he tried to derive everything at the blackboard by memory, so a bit slow and stumbling rather than slick and fast, the lectures were incredibly clear and valuable. Vernon Hughes was on leave from Yale that year and taught the other semester of the course, Atomic Physics, which featured a fantastic discussion of Rabi's Molecular Beam Magnetic Resonance Method which was developed a few floors up in Pupin, with lecture notes that I still use [12]. Since I had taken most of the undergraduate physics courses, I also took a graduate course, Mathematical Methods in Physics, with T. D. Lee (the year after his Nobel). Some of my classmates were Richard Friedberg and John Peoples. There were many memorable occasions in this class of which two stand out. One day Prof. Lee came to class in his rolled up shirtsleeves, without his usual tie and jacket.

He said that he had been up all night doing an integral and could he please look at somebody's notes to see where he had left off. Then, he delivered his usual crystal clear elegant lecture. The other story involved the final exam. He asked the class whether we wanted an open book or a closed book exam. The class opted for a closed book exam, thinking it would be easier. Then Prof. Lee said:"Open book, closed book, same exam."

I only applied to one graduate school, Columbia, and was admitted. Those were the days when they admitted lots of students and sat the entering class down and said: "Look to the left and look to the right: one of you won't be here next year." In the summer of 1959, between College and Graduate School, I worked at Los Alamos Laboratory, Weapons Division. I had to get a "secret" clearance. Here, again, the athletic connection helped. One of the FBI agents checking me out visited the Freshman Football coach, Ken German, who was a friend of his. Coach German told me about this and said that he put in a good word for me. I worked where they stored all the tritium at Los Alamos. This helped me understand years later that Pons and Fleischmann's claims of cold fusion [13] couldn't possibly have been correct because somebody at that site at Los Alamos would have noticed the radiation (or worse). I subsequently got a further lesson on this issue in my thesis work.

In my first year of graduate school, 1959-60, I had a two-year NSF predoctoral Fellowship for support, so I didn't have to work for my tuition. Instead I had the job as timekeeper at the Columbia Varsity Basketball home games. One memorable experience occurred in one of the later home games of the season against Princeton. It was a very exciting game with a few seconds left on the clock in the first half after a time-out—Columbia's ball. The ball was put in play and the next thing I knew the Princeton coach was screaming in my face—I had got caught up in the excitement and forgot to start the clock. Needless to say, I didn't get that job the next season. I also had several memorable teachers that year. I took Statistical Mechanics with I. I. Rabi, so I actually did learn about 'chemical potential' in a Physics course at Columbia—I'm sure that my present day colleagues on the PHENIX experiment at RHIC [14] would be surprised. I also had Prof. C. S. Wu that year for Nuclear Physics. She was also a great teacher as well as a great scientist. I got inspired enough to write a 30 page term paper on "Neutron-Proton Scattering at Energies Less than 10 MeV", that she really liked. Prof. Quimby taught Classical Mechanics, a two-semester course. He assigned a long set of problems each week, which were not graded week by week but you turned them in at the end of the semester and somehow they were all graded a few days later. Maybe this was because Quimby didn't give part credit on those long Classical Mechanics problems. If you didn't get the correct answer, you got zero for the problem. His memorable reason was: "If an engineer builds a bridge and it is an inch short and falls down, he doesn't get part credit". He used to arrive to class promptly by bashing open the doors to 428 Pupin (I think) so that everybody would wake up. His definition of a "rigid body" was also memorable. He came in with a piece of 4 by 4, about 2 feet long and smashed it onto the floor—"that's a rigid body", he said, waking everybody up, again. I also still remember his demonstration of the unstable intermediate axis of inertia by flipping a tennis racquet in the air.

One of the great pieces of luck that I had in my second year of graduate school, 1960-61, was not to get a future Nobel Prize winner for a course. The year that I took Advanced Nuclear Physics, it was taught by Robert Serber instead of James Rainwater who was famous for his excellent notes on the subject but not for his delivery. I still remember (and find useful) a homework problem that Serber assigned to us: "Go read the Cook, McMillan, Peterson, Sewell measurement of Total Cross sections of Nuclei for 90-MeV Neutrons in Phys. Rev. [15] and assume that the cross sections σ are expressed in terms of a nucleus of radius R by $\sigma = 2\pi R^2$ [16] and make a plot of R vs. $A^{1/3}$ and see what you learn." I found it amazing and inspiring that by plotting the data in a clever way, the underlying geometry of nucleii became apparent. I still use this type of analysis and what I learned about nuclear radii from this exercise in my present-day work. What I did not know at the time and what I found out only in 1994 when I invited Prof. Serber to present the Pegram Lectures at Brookhaven National Laboratory was how intimately Serber was involved with the first Atomic Bomb dropped on Hiroshima. Serber was a member of the Los Alamos Mission to Tinian (in the Marianas), which assembled the bombs for delivery. He had been Leader of the Group in Theoretical Division at Los Alamos which was responsible for the design of the Hiroshima Bomb. Serber gave me a few yellowing papers and asked me if I could make slides of them for him to show at his lecture. They were incredible. The first was the actual mimeographed orders for Col. Tibbets' Combat Strike mission to Hiroshima on 6 August 1945 with "BOMBS: Special". (My hands shook when I realized what it was.) The other two were Col. Tibbets drawing of the maneuver he planned to execute which he showed Serber when he asked what would happen to his plane after the blast; and Serber's 4 line reply assuring

Col. Tibbets that his plane would be safe. [17, 18] I also found out in the write-up of these lectures (for which I asked Bob Crease to assist Serber [18]) that Serber with help from Gian Carlo Wick deduced the quark model in preparation for a visit by Murray Gell-Mann to give a Physics Colloquium at Columbia in March 1963 [19].

Another issue of seminars and colloquia in this period concerns the Annual Meeting of the American Physical Society, then held the end of January each year in Manhattan at the Hotel New Yorker, diagonally across from Pennsylvania Station on 8th Avenue and 34th Street. This was a great boon for Columbia and other Metropolitan area graduate students since for 15 cents (or whatever the price of a subway fare at that time) you could hear and see and mingle with the greatest physicists in the U.S., if not the world. I think that the other regions of the country got jealous, and hotel prices in New York went sky high, so the Annual meeting was eventually moved around the country and died. The other great feature of the meeting for Columbians was the "unannounced" colloquium that was given in Pupin by one of the meeting attendees. I especially remember a talk by Richard Feynman on the Quantum Theory of Gravitation. It must have been 1962 or 1963, and the big lecture hall in Pupin was full to the rafters. I had a seat in the last row, between Morse and Feshbach. I'm not kidding. Nearly everybody who was anybody from the APS meeting was there at the appointed time except Feynman. He came into the room a few minutes late explaining that he had bet wrong on taking the local or the express at Times square. I don't remember a word of the lecture but I do remember that every time an expert, like Bergmann, would ask a question, Feynman would give a curt answer. But every time a young person would ask a question, he gave a very kind and clear answer.

I finished most of the required graduate courses in my first year and passed the Ph.D. qualifying exams in February 1960. I knew that there was trip to Bikini Atoll in the South Pacific scheduled in the summer of 1960 by Los Alamos Lab in connection with a bomb test and I specifically applied for that job and got it. I also applied to Brookhaven National Laboratory (BNL) for a summer job and got that, which I accepted because it was much closer to home as well as a promising site for my thesis research. As they say, the rest is history. In the summer of 1960, I worked at BNL in the group of "Luke and Sam", Luke Yuan (Prof. Wu's husband) and Sam Lindenbaum (a Columbia Ph.D). I worked directly for Satoshi Ozaki and John Russell at the Cosmotron, designing and testing water-glycerine Cerenkov counters.

However, I spent lots of time in the Cosmotron Library reading about electron elastic and inelastic scattering and talking to Leon Lederman who got me interested in the question "Why does the muon weigh heavy—is there a force of mu-ness?" and in the muon scattering experiment he was planning. Earlier in 1960, Lee and Yang [20] had proposed the intermediate bosons $W^{\pm}$ as the "quanta" that transmit the weak interactions and Mel Schwartz [21] proposed to detect them with high energy neutrinos at the new Alternating Gradient Synchroton at BNL which had started operating that same summer on July 29, 1960. Mel asked me to be a thesis student on the experiment (which discovered the μ-neutrino) [22], but I declined because there were already two thesis students (Dino Goulinos and Nari Mistry) on the experiment. I decided instead to go with Leon Lederman to do my thesis on μ-proton elastic scattering to compare the radius of the proton measured with muons to that of Robert Hofstadter [23] and Dick Wilson's [24] measurements with electrons, in order to deduce the difference in the radii of electrons and muons probed by hitting a proton. There was no effect [25, 26], the proton form factor measured with both muons and electrons was identical all the way out to four-momentum transfer squared Q^2=25.7 inverse fermi^2 (1.0 GeV^2/c^2). Never did I realize that these units would help at RHIC or get me in deep trouble at my Ph.D. oral (see below). Let me add here that we still don't know in 2010 why the muon weighs heavy. Actually, I was first offered by Leon (and I rejected the idea) to do my thesis on extending the search for $\mu \rightarrow e + \gamma$ to below the existing limit [27] of $< 2 \times 10^{-5}$. This low branching ratio had led Gary Feinberg [28] to propose that if intermediate Bosons existed then this reaction should occur with a branching ratio of 10^{-4} by virtual muon decay $\mu \rightarrow W + \nu$, followed by $W \rightarrow \nu + e$ with a γ radiated off one of the legs *unless the neutrinos associated with muons were different than those associated with electrons* [29]. At least this was a bit of luck for me because this experiment is still being done at present without finding anything to a limit of $< 2.8 \times 10^{-11}$ (90% C.L.) [30]. Another experiment that I worked on during this period (since I no longer kept time at the basketball games) was the muon-helicity experiment at Nevis [31]. I worked on this experiment part-time in Fall 1960 (which, including travel back and forth to Nevis, amounted to $\sim$ 30 hours per week—Columbia for part-time!) because I still had to finish my course requirements and couldn't devote full-time to thesis research. I really enjoyed working with Marcel Bardon, who was scrupulous about treating the graduate students to lunch from time to time.

I took up residence at BNL in the summer of 1961 to devote full time to the μ-p experiment. I was first located on the top floor of building T-129 where I was studying how to make a proton-range spark chamber with a thin entrance window. This was different from the neutrino spark chambers that were under construction at the Cosmotron test shack at the same time (Fig. 4a). The chamber was huge and built in the Nevis shop using specially flattened aluminum jig plates, spaced uniformly by Lucite bars on two edges and one corner. This allowed the top edge to be free of Lucite for photography and the front edge to be of very low mass. The chamber was in an aluminum 'pressure vessel' with a plate glass window on top for photography and a thin mylar entrance window on the front (Fig. 4b). Carl Carlson*[C] (now a distinguished theoretical physicist at William and Mary) was the summer student who worked on this project; but we hired him more for his brawn than his brain.

a)  b) 

Figure 4: a) Mike and Dino with Neutrino Chamber (Warner Hayes in the background); b) μ-p proton-range spark chamber in the Nevis shop, with builders.

There were many interesting problems to solve in building the first muon beam at the AGS and the first experiment to use it [25, 26] (AGS-4) which involved several test runs to learn: a) how to filter the original pions in the beam which decayed into muons so that the final π/μ contamination in the beam was $< 10^{-6}$; b) how to collimate a muon beam which has a range of 2 GeV/meter of iron to a full width of $\sim 1^\circ$ since the beam angle was not measured event-by-event; c) how to measure the energy of recoil protons by range in aluminum for the momenta 0.6–1.1 GeV/c where nuclear absorption and multiple scattering effects are large. To solve a), we made a measurement of pion attenuation curves in iron and concrete in an AGS test beam; for b) we used surplus navy cannons 1 ft. i.d. and 4 ft. o.d. which I discovered lying in the scrap yard by the railroad tracks in my daily bicycle trips around the Lab and which gave rise to Leon's famous comment that he could no longer make such collimators because he never again found a student of the right caliber [32]; for c) we made a special small facsimile of the large proton-range chamber, and exposed it to a well defined proton beam of variable momentum at the Cosmotron. We also used these data as a calibration for testing the comprehensive Monte Carlo Code [33] that we wrote to evaluate the elastic event acceptance and detection efficiency in this huge and multicomponent detector which used a 6 ft. long by 1.5 ft. diameter liquid hydrogen target (due to the low rate) as well as recoil-proton angle and range spark chambers, two left over neutrino chambers to measure the scattered muons [34], liquid and plastic scintillation counters and several thick absorbers. It may come as a big surprise to many of my present colleagues that I was one of the first (perhaps the first) to use a comprehensive Monte Carlo (MC) to understand the response of a complicated detector. We first generated Monte Carlo events in the fully simulated detector (checked by applying the same MC to the calibration run) and then ran the simulated events through the same computer program that analyzed the real events.

I designed and built all the trigger and drive circuits [35] for the spark chambers, which supplied the H.V. pulse to the chambers, flashed the fiducials, advanced the frame number and drove the special 70mm cameras. I also designed the control circuitry and system of valves for filling and emptying the 220 cu. ft. range chamber with expensive Ne(88%) He(10%) Ar(2%) gas which was kept at a constant gauge (over) pressure of 0.25 ± 0.10 in. water column throughout the run. The walls of the range chamber would have collapsed at an overpressure of 1 p.s.i. so my filling system first purged out the air with CO_2 , and then pushed out the heavier CO_2 with the Neon mixture. I made a cryopump for the CO_2 (which wouldn't affect neon) by using a steel pressure vessel that I got from the Bubble Chamber Group (which had been used for hydrogen gas storage but was no longer needed) and putting liquid N_2 filled heat exchangers inside. In order to get rid of any residual hydrogen, I pumped down the tank for about two weeks and then had it brought over to the BNL welding shop to cut the top off and make a flange for the heat exchanger. The foreman asked me what had been in the tank. I said "hydrogen gas, but I pumped it for two weeks to clean it". He then put an explosion sniffer into the tank to test the gas. It read non-explosive until he brought the sniffing tube out into the air, whereupon the meter went off scale. This meant that there was nearly pure hydrogen gas (no oxygen so not explosive) in the tank and the gas became explosive when exposed to air!!! The hydrogen had been adsorbed by the iron and can only be removed by purging with helium, not by pumping. Thank goodness for the excellent BNL shops. [Too bad that Pons and Fleischmann missed out on this demonstration.]

I learned electronics by reading the excellent BNL "Nanocard" manual [36] and by trial and error (aka experimenting—I still remember my technician Warner Hayes laughing when I installed an electrolytic capacitor with the wrong polarity and didn't check it before I turned on the power). One of my great achievements is that I made the only really working camera drive circuit for the Flight Research Camera used to record our spark chamber pictures. (In fact the experiment was all covered with canvas tents to keep out the light, Fig. 5). I made it a current pulse instead of a voltage

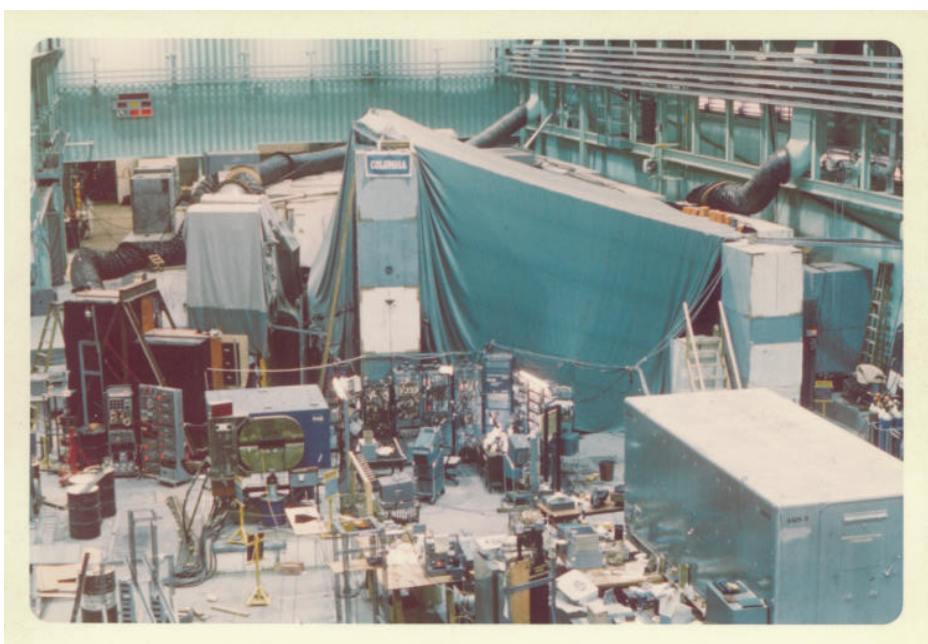


Figure 5: a) μ-p experiment at the AGS. I personally put the Columbia banner on top.

pulse so that the motor didn't burn out (due to loss of the back e.m.f.) if it jammed, which happened frequently. I knew I had something when Val Fitch who was doing a $\pi - e$ elastic scattering experiment in the next beam asked to borrow my camera because his motor had burned out. He promised that he would personally replace the motor in my camera with a new one if he burned it out. I thought a bit; but since he was a Nevis grad and developer of the famous Fitch coincidence circuits and Fitch Cerenkov counters used at Nevis, and he was very convincing, I took a chance.

I also remember discussions at dinner in that period with Leon and Val at the Brookhaven Center where Leon was trying to convince Val of the importance of pushing the limit of the $K_2^0 \rightarrow \pi^+ + \pi^-$ branching ratio to below the value of 0.6% in his discovery experiment [37]. I remember that Leon kept calling it the "demolish Adair" experiment since Bob Adair had generated excitement at BNL in the fall of 1962 with his group's observation of "Anomalous Regeneration of K_1^0 mesons" and his claim of a "fifth force" [38]. However I was too inexperienced at that time to understand whether Leon was suggesting to Val to do the measurement or whether Val had asked Leon's opinion whether it was worthwhile to do it, which I think was the case [39]. I also remember Ray Davis calibrating his chlorine tanks, parasitically, behind our muon beam. After all, there were still plenty of muons—they're hard to stop. I also developed lots of 'theorems' about muon beams during this period. For instance: "10% of a muon beam is outside any radius you can think of." Hugh Montgomery (who worked on the European Muon Collaboration at CERN in the mid 1970's, while I was at the CERN-ISR, and is now director of Jefferson Laboratory) still remembers it!

Lots of other interesting things happened during my 2 1/2 years in full-time residence at BNL. I'll just describe them in no particular order. I moved into the old physics shop in 1962, a beautiful space which was left vacant when the new Physics Building (510) was occupied. This became the Columbia Lab at BNL for the next few years of the heyday of the AGS period. It was connected to the former Shutt Cloud Chamber Group building which was occupied by the MIT group and their technician Tommy Lyons. At that time, I had two technicians from Columbia, Warner Hayes and Jack Haufman, to myself most of the time, and there were two Univ. of Rochester technicians, Bill Murdock and Harold Shulman in full time residence working on my thesis experiment. How was I to know that I would never again have such great lab space and so many technicians available? I also think that we threw the first-ever end of run party in this space when the μ−p data taking ended. My first data-taking shift on the experiment had been with Rod Cool (Fig. 6), who was then the Associate Laboratory Director in charge of High

Energy Physics at BNL. His thesis advisor, J. C. Street, had discovered the muon! [40]

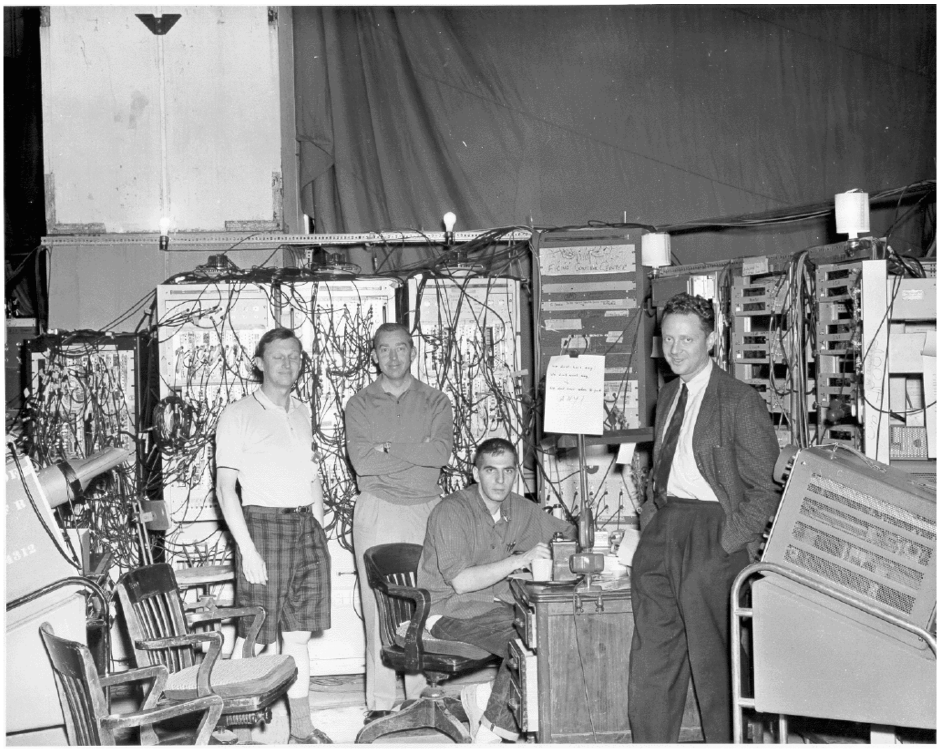


Figure 6: Graduate student with 3 bosses at "counting house" on AGS floor: (L–R) John Tinlot, Rod Cool, Leon Lederman [41].

I learned to drive at BNL and bought my first car from Jack Steinberger, a VW bug, whose engine burned out shortly thereafter. Fortunately, the rear engines were easy to replace. I used to drive into the city every Friday afternoon to attend the High Energy Physics Seminar and Colloquium and sometimes a meeting at Nevis on Saturday morning. There were lots of interesting things happening on campus also. Social life wasn't bad, even for Physics graduate students. The entering class of graduate students in Fall 1960 included Mary Kay Ralph from Hollins College in Virginia, who roomed with two other of her classmates from this women's college. This was a big deal because, apart from Cynthia (who also went to Columbia graduate school), there were very few women in graduate physics at that time. Eventually Mary Kay married Jean-Marc Gaillard, a post-doc from the neutrino experiment; her roommate Peggy married Mimmo Zavattini, another post-doc at Nevis; Cynthia married Jack Steinberger; and all wound up at CERN. In fall 1961, two other Barnard graduates, Naomi Barash and Lillian Hartmann also entered graduate school. Both Cynthia and Naomi did their Ph.D. theses at Nevis, while Lillian and Mary Kay were theorists. I also remember a Christmas party that the Graduate Students gave for the faculty in that period at which we presented "Christmas gifts" to the faculty and other students. Somehow I was selected as emcee and survived (without repercussion) the present I had to give on behalf of the graduate students to one faculty member (who shall remain anonymous for obvious reasons) of a seat-belt to use during Colloquia to restrain him from leaping out of his seat when he questioned the hapless speakers.

It may also be of interest to present-day students to know that there was a foreign language requirement (two foreign languages!) in order to get a Columbia Ph.D. So, although I spoke and read French pretty well (great native speakers in the NYC public school system at that time, and a great teacher, Jules Brody, as a freshman at Columbia College) I had to take a course in Scientific Russian in order to pass the second exam. Another thing that I remember is that when I entered graduate school in September 1959 there was no tuition for the period after one had finished courses and started full time research for the Ph.D. However, in March 1960, the University initiated a one-time charge of $500 as a research fee applicable to all students having entered graduate school on or after September 1959, to be paid when you were certified as a Ph.D. candidate. I had received advanced standing graduate credit for the course in Mathematical Methods in Physics that I had taken as a senior, so I was able to become certified as a Ph.D. candidate in January 1961, while I still had my NSF Fellowship. Since this fee would be covered by the fellowship, I insisted on paying the fee when I registered in February 1961. However, this was 6 months before the University had expected the fee to be paid, so they had no machinery set up to collect it. After a conference of the registrar with the deans, the University decide to waive the fee for all candidates certified before September 1961, so I saved the NSF and other graduate students some money.

Although the political activity on campus was relatively quiet in that period, it was much less so in the real world. First there was the Cuban revolution, with Fidel Castro taking over as Prime Minister of Cuba in early 1959. At that time Fidel was thought of as a friend by the U.S. and he came to New York (and Columbia) in April 1959. I remember literally standing next to him at a Baseball game at Baker Field. John F. Kennedy became

President of the U.S. in 1960, and launched the failed Bay of Pigs invasion in 1961. The U.S. imposed an embargo on Cuba in 1962 which included a ban on travel to Cuba by Americans. I remember vividly (but not the exact date) that shortly thereafter there appeared on the front page of the Daily News, a photograph of a Columbia Physics Graduate Student, Steve Newman, shaking hands with Fidel, in Cuba! Then came the Cuban Missile Crisis in late October 1962 and I remember calmly walking around BNL deciding where I would go when the Russian missile attack came, which we all thought was imminent. I decided that I would camp in the AGS tunnel which had plenty of space, electricity and water and was well shielded. Fortunately, there was a peaceful settlement. The next big and totally horrific event was Kennedy's assassination on November 22, 1963. I was at Columbia (because it was a Friday) when the assassination was announced. Notably, the 5PM Physics Colloquium went on as scheduled. I tried to drive back to BNL on Saturday, but I just didn't have any enthusiasm for work, so I went to my parents' home in the Bronx and while having breakfast on Sunday watched Lee Harvey Oswald being shot by Jack Ruby, live on national TV.

I moved back to campus in 1964 and spent the rest of my time analyzing the data and writing my thesis. The top floor of the Mansion House at Nevis had just been cleaned up and reopened and I was assigned an office there. For quite some time I had the whole top floor all to myself with a beautiful view of the Hudson. Also, in this period Sam Ting was a post-doc at Nevis and we used to spend lots of time discussing kinematics, multiple scattering, time resolution and that kind of stuff. Sam's thesis had been π-p elastic scattering under Marty Perl[C] [42] and I remember that Marty had visited the μ-p experiment. My departmental oral exam, after I had submitted my Ph.D. thesis, was truly memorable. The committee was Lederman, Rabi and Serber. Rabi took the lead. He asked me to come to the blackboard and describe my thesis. I started off with the sentence: "The radius of the proton is 0.8 fermi." Rabi jumped off his stool and started yelling at me: "Fermi, Fermi, what is a fermi? Use the correct units. That guy has so many things named after him!" After that, I was essentially speechless. Rabi then continued: "There were so many people on your experiment: what did you do? What did you contribute?" As I was still basically speechless, I sort of mumbled: "I must have done a few things, let me think.". Then Leon chimed in: "Mike, didn't you propose adding the muon chambers". MJT: "Oh yes, I did that.". Rabi: "What else did you do?" More silence by me. Leon: "Mike, didn't you...", etc. That's how the exam went, but I passed. Actually Serber asked a tougher question. Tests of QED were very popular at that time and Frank Pipkin at the Cambridge Electron Accelerator had presented preliminary results [43] of a breakdown of QED in the photoproduction of wide angle $e^+ + e^-$ pairs. Serber asked me how that affected my results and conclusions. I answered that it didn't affect our results on muons vs electrons because both results agreed with QED [44, 45].

After the PhD department oral, there were two more exams. First, a written exam, basically a few essay questions. My problem assignment is shown in Fig. 7. Although this was assigned in May 1964, the answers to the

May 4, 1964

Ph.D. Problems for M. Tannenbaum

1) List all the quantum numbers relevant in the assignment of resonant states to multiplets in SU_3. Discuss the experimental determination of these numbers for members of the pseudoscalar and vector octets.

2) Discuss the application of SU_3 to the prediction of strong interaction reaction rates.

Thesis: Muon-Proton Scattering

Copy to : Rabi
Serber
Lederman
Tannenbaum

Figure 7: My PhD problem assignment.

problems weren't submitted until January 1965, after my departmental oral. This was followed by the Low Library Exam, which included one Professor from another department to make sure that a Physics student was worthy of a Ph.D. from Columbia University (i.e. could speak and write reasonably well). Also, your thesis had to be published before the Ph.D. was awarded. During this period I had applied for and received a post-doc job at CERN. At that time the dollar was 4.3 Swiss Francs, so CERN salaries weren't that great compared to those at home. Leon thought that the salary wasn't high enough and got me an Ernest Kempton Adams Traveling Fellowship to supplement it. When I proudly informed CERN about the fellowship, they lowered my salary to compensate. A month or so before I was to leave for CERN (on the Queen Mary on St. Patrick's day 1965), I got a phone call from Jack Steinberger from CERN, where he was on leave. He asked whether he could put my name on a proposal [46] that he and Carlo Rubbia were presenting to the CERN-EEC to study, "interferences in the two-pion decay of a beam of K_2^0 mesons which has traversed some matter". This was really hot stuff—a key measurement in the newly discovered CP violation in the K^0 system [47]. I waited a day to accept because I wondered why these two heavy hitters would need a newly minted PhD on the proposal to improve their chances of getting it approved. Also, the procedure at CERN for new post-docs, which was a great idea (and which they made me do even though I had signed up with Jack and Carlo before I even got to CERN), was to visit each group in the NP division to find out what they were doing and then to select the group you wanted to work with. Actually, the original proposal was rejected; but Jack and Carlo (and I, ha, ha) immediately put in another proposal [48], which was accepted, and which we did [49]. I had a great time at CERN in 1965–1966, and then, again, in 1973-1980, and 1991; but that's a whole other story.

Just in case my long narrative didn't get you into the incredible excitement in Physics at Columbia in the period 1955–1965, I append a section covering this period from a talk I gave at my 40th Columbia College reunion in 1999 describing "How Particle Physics Changed Mankind's View of the World Since we Entered Columbia." (Of course, it's Columbia-centric; what do you expect?)

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How Particle Physics Changed Mankind's view of the World While I was at COLUMBIA

M. J. TANNENBAUM

♡ ♡ **1955 Gordon, Zeiger and Townes** invent the Maser.

♡ ♡ **1956 T. D. Lee** and C. N. Yang question the validity of Parity Conservation in Weak Interactions (Radioactive Decays).

♡ ♡ **1956 Lande, Booth, Impeduglia, Lederman and Chinowsky** discover the long lived K_2^0, neutral K meson, at Brookhaven National Laboratory (**BNL**)—resolves the τ-θ puzzle, neutral kaons have two distinct states of opposite CP quantum number, ± 1.

♡ ♡ **1957** Parity Violation in Weak Interactions Discovered **at/by Columbia: (Nevis) Garwin, Lederman and Weinrich; C. S. Wu, et al. (at NBS).** Weak Interactions are not the same when looked at in a mirror—they exhibit a 'screw-sense'. However if all particles are transformed to antiparticles (C=charge conjugation), the invariance is restored $\Rightarrow$ CP invariance of the weak interaction.

♡ ♡ **1957** Goldhaber, Grodzins and Sunyar at **BNL** measure the helicity of the neutrino from β-decay and find that it is "left-handed"

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♡ ♡ **1958 Schawlow and Townes** propose the Laser.

♡ ♡ **1960 T. D. Lee** and C. N. Yang propose a 'Left Handed' Intermediate Boson, W, as carrier of the Weak Force.

♡ ♡ **1962 L. M. Lederman, M. Schwartz, J. Steinberger et al** discover a second neutrino, the μ-neutrino, at **BNL**. Now two families of neutrinos go with two families of charged leptons, electron and muon.

♡ ♡ **1960-63** The Particle Zoo L. Alvarez group (Berkeley) and

N. P. Samios groups (**BNL**), **A. Pevsner, M. Block** and others discover dozens of new particles.

♡ ♡ **1962** M. Gell-Mann and Y. Ne'eman propose that all particles sharing common quantum numbers follow **SU(3) symmetry**. This leads Gell-Mann to predict an as yet unobserved Baryon with Strangeness quantum number= -3, spin=3/2, charge=-1.

♡ ♡ **1964** **Samios** Group at **BNL** observes the Ω^- with mass, charge and strangeness as predicted.

♡ ♡ **1964** Gell-Mann (after visit to **Columbia** and discussion with **Serber**), and Zweig propose that SU(3) symmetry is based on **3 quarks** u, d, s, with fractional charges $+2/3$, $-1/3$, $-1/3$. One problem with this model is that the Ω^- has 3 identical s quarks in the same state, apparently violating the Pauli Exclusion Principle. *Maybe quarks come in 3 different colors.* Formally (Greenberg), quarks are para-fermions of order p=3.

$$\begin{array}{cc} u & \\ d & s \end{array}$$

♡ ♡ **1964** **Christenson**, Cronin, **Fitch**, and Turlay discover CP Violation at a level $\sim 10^{-3}$ in the weak decay of the long lived K_2^0 at **BNL**. Nature can distinguish 'matter' from 'anti-matter'.