

RUTGERS, THE STATE UNIVERSITY OF NEW JERSEY

NEW BRUNSWICK

AN INTERVIEW WITH PAUL L. LEATH

FOR THE

RUTGERS ORAL HISTORY ARCHIVES

WORLD WAR II * KOREAN WAR * VIETNAM WAR * COLD WAR

INTERVIEW CONDUCTED BY

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and

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NEW BRUNSWICK, NEW JERSEY

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TRANSCRIPT BY

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Sandra Stewart Holyoak: This begins an interview on April 28, 2011 in New Brunswick, New Jersey with Professor Paul Leath and Professor Paul Clemens along with Sandra Stewart Holyoak. Thank you Professor Leath for coming and talking with us today, in what we hope will be several sessions of wonderful information. To begin, just for the record, please tell us where and when you were born.

Paul Leath: I was born on January the 9th, 1941, in a town in north-central Missouri called ... Moberly, Missouri in the Woodland hospital. I was a breech birth; it took two doctors to deliver me.

SH: Oh, wow.

PL: But in any event, they did it successfully, and I survived.

SH: Let us start talking by talking briefly about your family background. Can you tell us about your father?

PL: ... I grew up in Moberly, Missouri, where I was born. My father was a railroad conductor for the Wabash railroad. He had worked for the railroad basically all his career. Moberly was a junction spot for the Wabash railroad. So, it was a place where a lot of railroad people lived. That's about all I can say. My father had gone to junior college. [He] didn't have a bachelor's degree, but he had something that--whatever you get at a junior college--an associate's degree I suppose. My mother had gone to just a little bit of college; maybe one year. She was a housewife. She was not employed, per say, although she worked very hard. I was the oldest of four boys. They kept trying to have girls, and never succeeded, [and] finally gave up after four boys. So, it was a great place to grow up. Moberly was a town that was about, I don't know, twelve thousand people. It was small enough that almost everybody in town either knew me or knew of my family, so the whole town was my babysitter. So, if I did anything wrong, my mother would know about it before I got home. So, I had a lot of neighbors that looked after me. It was a great place to grow up. I don't know what more you would like to know. [Editor's Note: The Wabash railroad was chartered in the 1830s and completed in 1856. It went from Buffalo, New York to cities in the Midwest such as Chicago, St. Louis, and Omaha.]

SH: Were your parents' families originally from that area?

PL: Yes, generally. My father grew up in a small village just north of Moberly. His father ran a grocery store, and my mother grew up in another town just west of Moberly. So, they were from that area. So, our grandparents lived in the area as I was growing up. So, it was all very close. I could walk to ... both of my grandmother's houses and that sort of thing. ... Everything was very close. ...

SH: What were some of your earliest memories of events in the early 1940s?

PL: Well, World War II had started. ... My father was exempt from the army, because he was considered a critical person running the trains that carried troops across the US. ... He continued working. ... He wasn't working on troop trains, but he ... just worked for the Wabash railroad

during that period of time. I had two uncles--my mother's twin brothers--who were in the army and served in the Philippines. I don't think they saw any really active duty, but they were in the Philippines. Well, I mean, it was active duty but by that, I meant, they weren't in battles. I heard about a great uncle, that I never knew, that was killed in World War I. [laughter] ... We didn't have any real tragedies related to the war that were in the family, although obviously there were many families ... in the town that had had problems. I remember rationing, and having to have ration coupons. I remember things like, when margarine first came out, in order not to compete with butter, it was white, and so you got a separate little packet of color, and you had to squeeze it into the ... margarine. I remember when we had an ice box, and the ice man would come and deliver ice, before refrigerators. I remember lots of power failures--the power would go out--so we would always have candles to handle the power failures. ... We didn't have a lot of electrical appliances in those ages--light bulbs, mostly, I think. We had a party-line telephone, with an operator, before dials. My telephone number was 435-J, which was very good. It always allowed me to remember how many members there were in the House of Representatives. [laughter] ... The other party was 435-W--I don't know what J and W stood for--but in any event, we had that. ... I had a very nice and easy childhood. I went to an elementary school, which was just a few blocks away, that I walked to. One big event I remember in the second grade, is my mother sent me to school in the second grade with an envelope, which was a letter, supposedly a message for the second grade teacher. It turned out it was invitations to all the second graders to come to my birthday party. So, I had a big birthday party in the second grade. So, it was ... generally a happy time. So, I don't have sad stories. I had a very happy and good life. My parents were very supportive of getting a good education although they didn't have a lot of money to help with that. So, later on, that affected my going to college.

SH: Was there someone in high school who steered you towards college?

PL: Well, I don't know, not particularly. I didn't do terribly well in elementary school, just very average grades in elementary school. In fact, in seventh grade, I failed spelling, but somehow, when I got to junior high school [I improved]. I went to a junior high school, and then to, finally, high school. I had some very good teachers. I guess you would call them the mentors, but they were just the teachers of the class, the English teachers and the math teachers especially, and whatever, Latin teachers. ... I blossomed really at that time, I guess it was the time of adolescence and puberty, and I was growing up. I don't know, I changed, and so I graduated from high school as co-valedictorian of my high school class. In fact, my co-valedictorian ... became my wife. So, we were co-valedictorians, and because of that I got a scholarship that paid for my tuition at the junior college. So, I went to the junior college. I graduated very quickly there. I had been allowed to take some college classes when I was in high school. My full-tuition scholarship was twenty-five dollars a semester. [laughter]

SH: Was this junior college in Moberly?

PL: Yes, well, it was in the same building with the high school actually. [laughter] So, it was just a continuation of high school. ... I graduated from there in a year and a half. ... I was valedictorian of the junior college class, also. So, I was automatically admitted to the University of Missouri, which was thirty-five miles away.

SH: May I ask, did your wife continue to the junior college as well?

PL: No, she went away to Northeast Missouri State Teachers' College, which is now Truman State University. ... She got some kind of a scholarship there as well, but she went ... away at that point, and I finished locally.

SH: What year did you graduate from high school?

PL: I graduated from high school in 1958.

SH: 1958.

PL: So, I graduated in 1958. ... Then, I went to the University of Missouri, and declared myself as a physics major.

SH: What prompted that?

PL: Well, I had a very good physics teacher in high school. I had an absolutely excellent math teacher that ... taught me calculus and gave me special courses in combinatorics and probability, and all kinds of things that were, really, just fun. It was like a game, so I was very interested in math, but I liked the physics very much. I also liked chemistry. I can't really tell you. I just decided that physics was for me, and so I very quickly declared myself. I had to declare quickly, because I was already well-advanced. I graduated from the University of Missouri in 1961. So, I was only three years in college through this process, and had very good grades. It wasn't straight "As" like it had been in junior college, but it was very good. In fact, I had the grades to get into Phi Beta Kappa, but I had a big argument with them. They wouldn't let me into Phi Beta Kappa, because I didn't have enough credits at the University of Missouri to get into Phi Beta Kappa, even though my grades were sufficient. ... [laughter] So, in any event, I graduated in 1961. The summer before I graduated--so it must have been the summer of 1960--I got a summer job. There were interviewers on campus from various places, including the US Navy, and I got a job working at US Naval Ordnance Laboratory in Silver Spring, Maryland.

Paul Clemens: Silver Spring, Maryland.

PL: Well, it was just a student intern job in the summer.

SH: You are only twenty years old.

PL: Yes, something like that.

SH: Yes.

PL: So, I went there, and I had a great summer. I actually invented a little device, and got a little publication out of it. [It was] a device for measuring high-speed air flow, at something like seven times the speed of sound. So in any event, they offered me a permanent job as a civil servant, as soon as I graduated from college, and so that was my plan. ... I was going to go back

to the Naval Ordnance Laboratory and be a scientist at the Naval Ordnance Laboratory, but that senior year, I took, among other courses, a course ... called "Introduction to Theoretical Physics" by Professor Bernard Goodman, and he asked me what I was going to do. ... I said, "Well, you know, I have this offer for a permanent job at the Naval Ordnance Laboratory. That's what I'm going to do. I'm going to become a scientist, ... a kind of engineer for the navy." ... He says, "Well that's ridiculous." He says, "You have got to go to graduate school." ... So, he arranged for me to get a NDEA Title IV fellowship, which was a National Defense Education Act Fellowship, a very lucrative fellowship, to attend the University of Missouri. Thus, I never applied to any other graduate school, I didn't even really apply there. I was just, there I was, and I had a fellowship. [laughter] ... So, I enrolled in the University of Missouri. I got married in 1962, to my wife, who had ... by then graduated from Northeast Missouri, and had a job in the area, back in my hometown, as a high school home economics teacher. ... So, we had plenty of money, actually, for a graduate student. She had a full-time job and I had this fellowship. ... I then studied theoretical physics in graduate school at the University of Missouri. In 1965, ... my thesis advisor, who was Bernard Goodman, the same guy that talked me into going to graduate school, got lured away by the University of Cincinnati, and left to go to the University of Cincinnati. So, in my final year, I moved to Cincinnati, and finished my thesis in Cincinnati, but it was a thesis still formally from the University of Missouri. [Editor's Note: The National Defense Education Act was passed in 1958 and provided for increased funding across all levels of education institutions, including doctoral work in the sciences.]

SH: Oh, was it?

PL: So, I graduated from there. He simply then contacted a number of people that he knew, and I got several offers of post-doc jobs straight away, and one I considered was at Michigan State, but the one that was most attractive to me was at Oxford University in England. So, I went to work with Professor Roger Elliot at the University of Oxford in England. ... He had a US Army research grant.

PC: Was it common at that time for people like yourself to go directly into a post-doc, as opposed to a teaching position at a university?

PL: Fairly common in physics. ... There were people that went ... directly into teaching. If you were going to a small college, you might go directly into teaching, but if you were going to go to a major university position, you would usually--in physics--go to a post-doc job first. ... So, Roger Elliot had a US Army research grant, believe it or not, even though it was in England, and in Oxford.

SH: Was he American?

PL: No, he's British--very British. In fact, after I left Oxford, he became the head of Oxford University Press, and was the head of Oxford University Press for a long time. ... He's a very famous man, but in any event. ... The result of that was he could pay me the maximum salary ... that Oxford was allowed to pay post-docs, which was a whopping seventeen hundred pounds a year, which was about, I don't know, about four thousand dollars a year in those days. ... Which was a gigantic salary, because I had neighbors who had post-docs that paid only five hundred

pounds a year. So, I was treated royally, but in any event, it was a three year position. I had written a very nice thesis, ... and published a number of papers with Dr. Goodman, which was no doubt why all of this happened, and why I had been invited to give seminars at a number of universities, one of which was Rutgers University. So, on my way to England, I stopped. I had to change planes in New York City anyway, because I had a flight out of JFK, which wasn't called JFK in those days, Idlewild, or whatever it was called.

SH: That is right.

PL: From New York City, I took the bus, Suburban Transit, down to New Brunswick, and gave them a seminar on my research and didn't think anything more about it. I went on to Oxford. In the spring of my first year of the three year post-doc at Oxford, at tea time--you know, tea time ... at English departments is a big deal, and we were all talking about, probably physics, but I'm not sure when the call came. Professor Peierls was the head of theoretical physics there; theoretical physics was a separate department at Oxford in those days and Professor Peierls was the professor. Rudolf Peierls, he was a very famous man, but in any event, he came and said, "Paul." He says, "There's a telephone call for you in my office," and I said, "Oh, in your office?" I never had a phone call in his office, that was really an impressive thing. So, I went and it was Peter Weiss on the phone. He was the chairman of the Physics Department back here at Rutgers, offering me an assistant professorship if I would come back to Rutgers, and I hadn't even applied. [laughter]

SH: You were not even on the market.

PL: Right, I wasn't even in the market, I still had years to go at ... Oxford. ... The physics and the math department here at Rutgers had recently gotten a big NSF [National Science Foundation] science development grant, which was a big grant that, in particular, included monies to hire new faculty. ... NSF agreed to pay the salaries for, I think it was something like five years, and the university, in order to accept that grant, agreed to pick up those salaries at the end of five years. So, it was a time of big expansion of the physics and math departments.

SH: What year did you get the job offer?

PL: Well, I came to Rutgers in the fall of '67. So, this was in the spring of ... '67, that I was called. I got my PhD in 1966, okay, and I came back to Rutgers in '67. ... I went back and talked to Professor Elliot about various things, including Rutgers, and we decided that it was an up and coming department, and it was a very good place to go. He knew some of the faculty here, and we decided that it was as good an offer as I was ever likely to get, I should just take it. ... I did, and I left Oxford and came back to Rutgers, and I've been here ever since. Thus, I came as an assistant professor here in 1967, as part of the expansion associated with the NSF science development grant, along with a number of other faculty that came around about that same time.

PC: What was the department like in the fall of 1967?

PL: Well, the Physics Department had been in Van Dyck Hall. Van Dyck Hall is named after Francis Van Dyke, who was the founding chairman of the Physics Department here at Rutgers.

Prior to Van Dyck Hall, the Physics Department had been up in Geology Hall, in a few rooms up there. But in 1925, the state voted some money, which allowed Van Dyck Hall to be built. Van Dyck was the longest-ever serving department chair; he served for thirty-eight years. ... The department had been in Van Dyck Hall until just before I came. ... Mason Gross was the president. They had recently gotten some money to build the Physics Department in the early '60s over on the Busch campus, and so it was newly built. I'm not sure exactly of when it was built, but it was already here in '66, when I came and gave my seminar, because I gave my seminar there. [Editor's Note: The new physics laboratory and lecture hall was built on Busch campus in 1963.]

SH: You were already on Busch.

PL: Yes, they were already over on Busch which was then called University Heights. So, I went to Busch, but the building as it was built, had no classrooms. It was just, basically, research laboratories and offices. ... So, our small classes were still taught in Van Dyck Hall. So, I came back over and I taught--when I taught a small class--I taught it in Van Dyke Hall, and the teaching laboratories were in a bunch of barracks left over from Camp Kilmer, basically. ... They were on the Busch campus, right there. A bunch of old barracks, which fortunately, are now gone. ... So, we taught laboratories there. Well, if it wasn't raining. If it was raining, the rain made so much noise on the tin roof, you couldn't hear, but in any event, [laughter] that's the way it was in those days, until more facilities were built. ... I even had a couple of classes in the river dorm basements, which are awful rooms. I remember those, too. ... So, that's what I did. ... [Editor's Note: Camp Kilmer was a staging area for troops headed for the European Theater during World War II. After the war, acres of land were acquired by the surrounding communities, including Rutgers University.]

SH: What was your impression of Rutgers when you came through and gave that seminar on your way to England?

PL: Well, it was a very active department. They asked lots and lots of questions. They had a number of well-known people in solid state physics, as it was called in those days. Now, it's called condensed matter physics, which is my field. Well, the one, probably, that was most responsible for getting me invited, was Elihu Abrahams, who was a very well-known [and] ultimately a member of the National Academy--theoretical physicist. ... Peter Weiss, who was the department chairman, was a theoretical, condensed matter physicist, or solid state physicist. ... There were two very well-known people in the department. One was Henry Torrey, who was also, later, the dean, or maybe at the time, was dean of the graduate school. He was the first person ever to see an NMR [Nuclear Magnetic Resonance] signal in bulk matter. He should have gotten the Nobel Prize for it, but instead his post-doc advisor got it, but he was the person that actually saw the first signal. [Editor's Note: The NMR signal in bulk matter was observed at Harvard on December 15, 1945 by Henry Torrey who was working with Edward Purcell and Robert Pound. Within a month, Felix Bloch, published the independent discovery of NMR in solids. In 1952, Purcell and Bloch received the Nobel Prize.] ... Bernie Serin, after whom the building is now named--great physicist. Serin was the person who discovered the isotope effect in super-conducting materials and mercury. I don't know if you know that the first material ever to be recognized as super-conducting--that is the electrical resistance going absolutely to zero,

which happens at about four degrees absolute--is mercury. ... What he was able to show was that the superconducting transition temperature depended upon the mass of the mercury atom, and different isotopes have slightly different mass. ... So, he would get a sample of one isotope of mercury, and another isotope of mercury, and measure the super-conducting transition temperature, and saw that it depended upon the mass of the mercury atoms, even though it was an electronic transition. ... That was the proof that ultimately led Bardeen, Cooper, and Schrieffer to their theory of super conductivity, that it was an interaction between the electrons and the atoms, or the ions, that led to the superconductivity.

SH: Had some of these professors studied at MIT?

PH: Henry Torrey had been up at Harvard. He was part of something called the "Rad Lab" during World War II. One part of the Manhattan Project was to develop radar, and that happened at a place called the Rad Lab, and he was involved with the early development of radar during the war. [Editor's Note: MIT's Radiation Laboratory, or "Rad Lab" was where most of the US microwave radars were developed during World War II.]

SH: I was mistaken.

PL: No, that was in ... New York where that was, but after the war, he went with Purcell, who he had also worked with, who was a Harvard professor. He went back up there as his post-doc at Harvard. So, that was after the war that he saw the NMR signal, but ... they worked in that electronic area in those days. Bernie Serin, I don't know what he did during the war, he was a student at Penn, and I think he came to Rutgers, maybe directly from Penn, I'm not quite sure, I don't know. He discovered the isotope of mercury right here at Rutgers though. ... That was an experiment he did here once he was a professor. He came as assistant professor.

SH: When you gave your seminar, did you give it at Van Dyck?

PL: No, there is a physics lecture hall ... over there, which is still there. That little round building, I gave it right there.

SH: Oh, okay.

PL: So, that's where I gave it, back in those days.

PC: How much teaching were you doing at that time? What was expected of you as a faculty member in physics?

PL: Well, what was typical in those days, is that you would either have one big class of some sort of your own, or you would teach a number of sections of a course. When you had recitation, and laboratory sections, those were mostly taught by faculty. So, I would maybe have, if I was teaching over in Van Dyck, sections, I would maybe have four sections that I would teach. ... I guess it was a little while before I actually lectured in a course, and organized all the sections. If you do that, that would be your teaching job. A couple of semesters, I had a more advanced course or a graduate course in solid state physics, and then that would be my teaching job. So,

that was typical, and that still is typical, not just at Rutgers, [but] throughout the nation, in physics.

SH: You started teaching as soon as you came to Rutgers?

PL: Oh yes, that was part of the job.

PC: I assume that in the upper-level courses you have mostly majors and that the classes are fairly small.

PL: Yes, sure. I mean, they would be our physics majors, plus some engineers would come over and take them. So, we might have twenty students in the class. That would be typical if you would teach a high level class. It varied, obviously, but yes.

SH: Was there any competition between engineering, the physics, or the chemistry departments?

PL: No--physics and engineering and chemistry are sufficiently different subjects that we had our own turf. [laughter] So, it wasn't a particular problem. At times, people would get upset that the engineers would be trying to teach their own physics course, or the math department would be upset that the engineers would be trying to teach their own math course, but some way, that was all sorted out. Yes, we have our turf somehow, I don't know, it's like, probably the difference between history and political science.

PC: The only major turf issues I have ever heard is between electrical engineering and computer science.

PL: Yes, computer engineering and computer science. Typically, though, computer scientists are working with algorithms, and computer engineers are working with the hardware, for the most part, but they get intertwined these days.

SH: Throughout your graduate career, you would have been exempt from the draft?

PL: I was, yes, throughout all of this. All the time I was a student, I was 2-S, which is a student deferment. ... My son was born in 1966, the year I got my PhD, and so, I went from 2-S to 4-A, as a father. So, I was never in the draft.

SH: Okay.

PL: So, that was all avoided--the ages and times simply worked out. I never thought about it very much, but in any event, they worked out in that particular way.

PC: Did your wife continue teaching?

PL: Yes, she taught the whole time I was a graduate student. She taught home economics in high school in Moberly, Missouri. ... Then, when we had to move to Cincinnati, she got a job teaching home economics in the high school in Cincinnati. An inner-city school, called Hughes

High School, and I think that was the point at which she decided she wanted to have children, and she never taught after that with that experience in the inner-city Cincinnati school with all kinds of major problems. She had lots of unwed mothers, and all kinds of student problems in her home economics. Home economics was mostly girls, you know. ...

SH: It was part of the mandatory curriculum for women.

PL: For women, it was in those days, and so there were major problems in ... the school. I think she handled it very well, but it was a drain, it was very hard. ... We were only there for a year, and then we went to England, and in England, she didn't have the credentials, and so she wouldn't have taught in England, anyway. ... When we came back to New Jersey, she thought about it but, you know, [but] didn't do it. Here in New Jersey, we purchased a house that had been a general store, and she opened a shop, where she sold--her expertise in home economics was needlework, and she made much of her clothes, and that sort of thing. So, her shop was basically a shop with fabric and yarn, and she gave knitting lessons, quilting lessons, and all kinds of sewing instruction. It was a ladies shop, and we had it right in our house, for many years.

SH: Is that where you initially moved?

PL: No, we initially rented a house in Piscataway for two years. Then, and had an option to buy it for 26,000 dollars. That was really a mistake, not to buy that house. ...

[TAPE PAUSED]

PL: ... The shop--was that what we were talking about?

SH: We were talking about the house you did not buy.

PL: Oh, the house I didn't buy. We were offered it for 26,000 dollars. It's a fabulous house with a great piece of property, and a stream running through it--marvelous property, within walking distance to the Busch campus. ... We couldn't figure out how to pay 26,000 dollars in those days, so I didn't buy it, and the owner had purchased another house along River Road. ... Within two years, he decided he didn't like living there, and so he wanted to come back, so we were kicked out. So at that point, I did buy a house, just off of Franklin Boulevard in Franklin Township, and we were there for five or six years. ... Then, we bought--we found--this house in Millstone, which had been the general store, and we moved there, and she opened her shop.

SH: Do you have children?

PL: I have a son--had a son, he died in 2005.

SH: Oh, I'm sorry.

PL: ... I presently have a daughter. ... My son was born in 1966, my daughter was born in 1970. ... My daughter lives in England. ... We've gone back to England for sabbaticals a

number of times. ... She was admitted to all the colleges at Rutgers, and the honors program at Douglass. ... But she went to Vassar and junior year, she decided to go junior year abroad, and, of course, decided to go to Britain. So, she actually went to Wales, but in any event, she met her future husband in the junior year abroad in Wales. ... They didn't get married at the time, she came back, and finished up her degree at Vassar, and took a job in New York City for a couple years, but then she got a chance to apply for a scholarship--bursary--or whatever they call it, to study textile conservation at the Courtauld Institute at the University of London.

PC: Yes.

PL: With classes at Hampton Court Palace. It was a wonderful opportunity, and so she took it. It fit in with her background perfectly, because first of all, she'd grown up with all the needlework in the shop. She had started out her freshman year at Vassar as a chemistry major, but then switched to art history. So, she graduated in art history. ... She had art history, and chemistry, and needlework, and put all that together, into textile conservation. ... So, she did all kinds of things. Learned how to restore, or stabilize tapestries, and things of that sort, and costumes. She was involved in a project to prepare a mummy's wrapping for a museum. ... Then, she was in charge of an exhibit of Queen Victoria's costumes at the Museum of London, which she did. ... That was after she graduated. She was working, I guess, in freelance, but anyway, working for the Museum of London. In fact, there was something on the news recently, about Kate Middleton comparing the dress she might have, to one that Queen Victoria had, and so, I sent the article to Kim, and she says, "I know that dress!" [laughter]

SH: Amazing.

PL: She did that, but nevertheless, she married a computer person, and then they moved up to Stafford in England and there aren't many jobs for textile conservators up there. So, she retooled herself, and she's now a financial officer for the County of Staffordshire. So, what can I tell you, I don't know. I don't know why I'm telling you all of this.

SH: Coming back to Rutgers, was there a lot of construction going on when you came here as an assistant professor?

PL: On the campus, there was construction. The Hill Center was being built, for the mathematics department. I believe they were doing some additions to engineering. They may have been doing something to Nelson Hall for biology, I can't remember. Library of Science and Medicine may have been built around that time. The chemistry department, Wright Laboratory, was already there. At some time along in there, they added the Rieman Wing to the chemistry building. I don't know when the College of Pharmacy went up. So, there was some construction going on, and some changes that took place. For example, there used to be a road that went right past the physics building, and past the chemistry building, that you'd drive a car down. That road was taken out, so they made that area into a quadrangle. There were all kinds of changes that were happening as it was becoming a campus. When we went there, the engineering building already existed, the Wright chemistry laboratory all existed. Of course, the first building over there was the Waksman Institute. ... Most of Nelson Hall for biology was certainly there. I don't remember psychology being there, but I can't remember when psychology was built. I

wasn't involved with any of those things. Anyway, the ... physics building was there at least. ... What is now the east wing of the physics building. The west wing was built later. In fact, it was built, back I guess, when I was provost. So, the campus has sprung up mostly since then. There was no student center on the Busch campus in those days. There was no dining hall, so we would come across and eat in the Brower Commons, where I learned to sit with some of the old-timers over here. ...

SH: Who do you remember having conversations with?

PL: Well, Dick McCormick, senior, and the economist, Bob Alexander--and another old guy--Sidney Simon.

PC: He was a character. [laughter]

PL: He was a character. So, but in any event, ... we ate in the Brower dining room over here, until we ... had a dining hall. ... They started building dormitories at Busch, also. So, the campus really appeared later.

SH: When you came here, you talked about the grant that would pay your salary for the ensuing five years. What activities were you expected to participate in?

PL: Well, I was expected to do research, and the department had to send an annual report to the NSF, about what they were doing, what faculty they had hired, what the faculty were doing. I'm sure I had to write a report on what research I was doing, what I had published, and all of that got put into a report to the department [and] relayed back to the NSF. They had to give regular reports on that, and it got renewed a bit for a while after that. In some way, probably at a lesser level, I don't know the details of that, ... it was run by the department. I was just an assistant professor in those days, so they would just ask me for reports on what I was doing, but that's basically what happened. ... At some point, I applied separately to the National Science Foundation and got my own separate research grant. So, then for a number of years, I had my own research grant, which allowed me to basically, pay for graduate students and post-docs, other expenses.

PC: What was the tenure expectation in physics at that time? What did you think you had to do in order to get tenure?

PL: Well, you had to do something important. The Physics Department was very much against just counting papers. It was important to have research grants--that was very important--but more important was that you had done something that was important for the field that people recognized. It was during the days before we had all these devices for counting citations. But also important were letters of recommendation ... at the time of tenure. ... The Physics Department would always send for letters from people who were the stars in the field, who had nothing to do with you other than reading your papers or listening to your talks at meetings or elsewhere. ... So, it was very important that you made an impact in the field. It was the quality of what you did. In fact, they would put you down for publishing stuff that wasn't very high quality. It wasn't just how much you published, but whether you did anything important.

PC: Did most of the people that physics hired get tenure when you first came here?

PL: There were lots of people that didn't get tenure, I can think of several.

PC: Really?

PL: They typically did not get to the level of being turned down by the PRC [Promotion Review Committee]. They weren't even ... recommended by the department, and so, they, early on, were advised, "Well, you aren't going to make here, so you should apply someplace else."

SH: Really?

PL: So, ... that happened. I can't think of many people for which that happened. I can think of a few for which that happened, but they hired some very good people. So, we had a lot of success with very good people, and those very good people made it.

PC: Was there a sense that this was a change in the way physics thought of itself?

PL: Well, it was a change in the way the Physics Department at Rutgers thought of itself if that is what you mean. Sure, I mean at that point, it had been recognized by the National Science Foundation. It had been successful in hiring a number of outstanding faculty. It was involved in things that it hadn't been involved in before. For example, they got the money to build the tandem accelerator. There was an accelerator here, and so we hired. ... I wasn't a nuclear physicist, but nuclear physics was big in those days. ... So, we hired lots of nuclear physicists, and not only that, established, an arrangement with Bell Labs, and some of their top scientists were coming over and doing experiments at the accelerator. So, it was a joint Rutgers-Bell collaboration, and ... that was a big deal. ... We had people in other fields. We had people in high-energy physics, that were going and doing experiments at the big accelerators at Brookhaven and Fermilab, and places like that. ... In condensed matter physics, ... or solid state physics as they called it in those days, ... may have been the area we were most well-known, because we had some famous people in those fields. So, we were establishing ourselves and it was clearly known that we were an up and coming department. ...

PC: Part of the reason I ask is that in my department in the late 1960s, young people who came in had reason to believe that if they were good teachers, they could get tenure. By the time I came in in the middle of the 1970s, that was no longer true.

PL: Oh, no.

PC: In that short period of time, it changed from being the department which had a very traditional Rutgers emphasis on teaching to one in which you had to publish, not only a book, but in the way you put it, an important book, to get tenure.

PL: Yes.

PC: In one cohort of hires, it changed from being the old-fashioned idea of what a liberal arts college is to a research-oriented university.

PL: That had already happened, and I think that change took place earlier in the Physics Department.

PC: It sounds like it.

PL: It took place after the war, when people like Henry Torrey and Bernie Serin and Elihu Abrahams, and such people came to the department, that changed the department's way of looking at things. They weren't going to promote somebody just because they were a good teacher, but it probably had been that way before the war [for] some of those old timers that were here, back in those days. It probably was that way, but it changed with the influx of the great new scientists that came after the war.

SH: Were you assigned a mentor to help you when you first came to the department?

PL: Not really, I mean the department had its groups of people in different areas, and so, I was part of a group of, four condensed matter theoretical physicists, and we worked together, and had joint seminars, and things of this sort. ... So, I guess I had informal guidance of various sorts. Oh, I should tell you, that I did something which undoubtedly changed my life. ... Well, there's five, I forgot another person that's very important in the scheme of things, is George Horton. He was a theoretical, solid state person, also. Lesser known in Physics, but I'll just tell you one thing about George Horton.

SH: Yes, please.

PL: George Horton was hired at some point to be the chairman of the Douglass College Physics Department. This was back in the days before the academic reorganization, when there was a Douglass College Physics Department, and there was a separate Rutgers College Physics Department. There was never, as far as I know, a University College Physics Department. ... In fact, another very important person in that Douglass department was Mary Wigner, the wife of Eugene Wigner, the very famous Nobel Prize winner. He was at Princeton at the time, his wife Mary taught at Douglass in the Physics Department. [Editor's Note: Dr. Eugene Wigner was a Manhattan Project physicist and 1963 Nobel Prize winner for his theory of the atomic nucleus. He married Dr. Mary Wheeler Wigner in 1941.] ... She had retired, and when George Horton got here, or maybe a year after he got here, he found himself, except for a young person, more or less isolated over there, and he wanted to get involved in research and join us over on the Busch campus. ... So, he basically arranged to merge the Douglass College Physics Department with the Rutgers College Physics Department. Thus, we were the first among the major departments at Rutgers, the first department to be reorganized, well before the academic reorganization in the early '80s. There was only one Physics Department at the time of the academic reorganization in the early '80s. ... That was basically due to George Horton. He came over and basically brought the Physics Department with him. So, some of our duties were to teach in various places. Some of the times, my assignment was to teach at Douglass. So, we assigned faculty to various places to teach the Douglass College students or the Rutgers College students. It was all done out of

one department over on the Busch campus. [Editor's Note: Dr. Mary Wheeler Wigner obtained a PhD in physics in 1932 from Yale University. In 1957, she joined the Douglass College Physics Department. The merge of the Douglass and Rutgers College Physics Departments occurred in 1961, and moved to Busch campus in 1963.]

PC: When was the first time you saw women in your physics classes with any regularity?

PL: When I taught over at Hickman Hall, it was mostly Douglass women students, but there were male Ag students over there, too. So, Ag students and Douglass students would be in the class over there. It was men and women mixed over there, but I would have many women in the class. It's not unusual now. In the fall, I tell you, I taught the physics class for pharmacy students, I don't know if you know, but the pharmacy school is about two-thirds women now. Yes, it's amazing.

PC: Did you have women physics majors?

PL: Occasional.

PC: In the 1960s?

PL: Maybe not in the '60s. Well, no, I think there were a few from Douglass College. It was kind of discouraged by Douglass College. There had been physics majors when they had a department over there, but once they had merged it and moved it over across the river, ... they could take their introductory physics class over at Douglass, but to take their advanced physics major classes, ... since there wasn't enough enrollment to duplicate classes they had to take a bus or whatever, and come over to the Busch campus. ... So, it was actually discouraged, I would say, by the Douglass College advisors for girls to major in physics, because they would have to come to the Busch campus. Because there wasn't a Physics Department at Douglass, so there were few, but there were a few. ... We weren't graduating a lot of physics majors. Physics majors at major universities in those days, would graduate maybe, a dozen physics majors a year, and that would be all. So I mean, we might have one Douglass woman among the bunch. ... We had women majors, but there were very few, and they were Douglass College students, for the most part.

PC: Do you have any sense what the women who got physics degrees did? Did they go on to teach high school or did they go on and do graduate work or post-doctoral work?

PL: I don't have a sense. ... You know a person we should include in your project is Noemi Koller from the Physics Department. She's been around for a very long time. Also, she was the first woman, I think, the first women faculty member, in Rutgers College, and can talk a lot about women in the early days. ... There was then a faculty member from botany that ... came to Rutgers College shortly after, but I think Noemi was the very first women on the Rutgers College faculty, and she can talk about that. She's been a fighter for women in science for a long time. ... [Editor's Note: Noemi Koller received her PhD from Columbia University in 1958. In 1960, she was appointed as assistant professor and earned tenure in 1965, becoming the first women to be a tenured faculty member at Rutgers College.]

SH: She was in the department when you got here.

PL: Oh, yes, ... she came in the '50s.

SH: How many women were on the faculty in the Physics Department at that point?

PL: I guess she was the only--one at that time--when I came.

PC: I would be shocked if there was more than one.

PL: Right, I think she was the only one. ...

SH: Were there any minorities in the 1960s?

PL: There were, when I came, there were no blacks. There were a few Asian faculty. There were a few Chinese and Japanese. Only once, in the history [of the department], have we had a Hispanic. We subsequently hired black faculty. The problem in physics with black faculty is once you get them, you can't keep them, because everybody's trying to hire them. One of our famous black faculty was Shirley Jackson, who now is the President of RPI. [Rensselaer Polytechnic Institute] Who we refused to make PII [Professor II], because she wasn't good enough. [laughter]

SH: Going back to the tenure process, what do you remember? Was it stressful to go up for associate professor then?

PL: Well, I was a little stressed about it. I wasn't terribly worried, because I thought I had a good record. Let me, I have to tell you about what I did as an assistant professor that I perhaps shouldn't have done, and I wouldn't advise assistant professors to do, and that's how I got to talking about George Horton. George Horton, along with Alan Robbins from the Physics Department, and Hans Fisher from nutrition, and Dick Peskin from engineering, and a number of other people in those early days were involved with organizing the AAUP [American Association of University Professors] as the bargaining unit. I guess what had happened is that the state was giving salary raises to those state employees who were organized, such as the state highway patrol and other groups, but not to Rutgers, and Mason Gross thought that in part it was because there was no union. So, Mason Gross actually urged the faculty to form a union, and there was never even an election. Also, he wanted it to be the AAUP rather than the NEA or other national body. The AAUP had a reputation of supporting strong academics, and he was very afraid of whatever the opposition was, the NEA [National Education Association] or something. ... So, he wanted to see to it that the AAUP became the union, rather than one of these big national organizations which is known primarily as union, which the AAUP wasn't in those days. ... So, there was simply a petition, and if they could get enough people--and I now no longer remember how many that had to be--enough of the faculty to sign a petition saying they supported making the AAUP as the union, then Mason Gross would simply recognize it as the faculty union, and that's all it was, and that's what happened. ... George Horton and Alan Robbins and Hans Fisher and Dick Peskin, and I don't know who all else, I may be able if I had

to dredge them up, think of names of those people, were involved in organizing, basically, going around, and going to faculty meetings and other things, and getting people to sign a petition, which ultimately led to the AAUP recognition as the faculty union, but nevertheless, once it became the faculty union, then they had to do the things that unions do, including collective bargaining. ... George Horton convinced me to join the bargaining team, and so I did as an assistant professor [laughter] and so, and very quickly found myself the chairman of the bargaining team, and it was great fun. It was terrific, because, in particular, we were bargaining with Mason Gross and Dick Schlatter, who were very supportive, and our bargaining sessions were just brainstorming sessions on how we could make Rutgers better. [Editor's Note: The Rutgers AAUP chapter was designated by Rutgers faculty as their labor representative in 1970.]

SH: What year is this?

PL: So, I guess it must have been about; I was still an assistant professor, so, and I came in the fall of '67. So, it must have been about '70.

SH: Okay.

PL: Approximately, I don't know exactly. ... I'm sure you can find the records of when it became the union. I wasn't involved in forming the union, but I got involved shortly after, and in a weak moment, I let George Horton convince me that I should get involved. He was looking for people to help and in particular, he wanted people on the bargaining team that could help with some of the numbers. Thus, when, for example, that we were arguing for this percent salary increase, I would do a calculation, and say how much this was going to cost or, if they wanted to do some alternative, what that was going to cost. So, I did lots of calculations for them in those days. In any event, I became the chairman of the bargaining team. ... I was the chairman of the bargaining team for a few years. I cannot tell you exactly how many, but for several years. ... It was quite an experience. First of all, the bargaining sessions were actually very fun. What was less fun sometimes was going back and meeting ... with the faculty, who were wanting things we couldn't deliver, but we did lots of things in those days. ... Since we became state employees, we had to be put on the state salary table, which we had not been on before. ... The state salary table in those days had something like seven or eight steps. ... There were ranges of eight steps and you had sort of a normal increment. Each year, you would move up until you'd been here for eight years in a particular category, and then you really couldn't move any further. ... So, that caused a particular problem at the full professor level, because people were full professor for a very long period of time, and we needed a much bigger range of salaries for full professors than you could get at single full professor range. So, one of the things that was created was a second range, which ultimately became Professor I and Professor II. ... It was done just to give the kind of range you needed for salaries for full professors. ... Not only that, when it was first set up, ... they were tied together. So, that you weren't stopped at step eight, you could just go right on to the next range. ... So, some people--the last one I ever remember saying he was one of those--was Bob Alexander. ... He got the Professor II automatically, all you had to do was stay around long enough, and you became Professor II. ... It was only subsequently, and I didn't have anything to do with that, it was probably Kenneth Wheeler, but subsequently, it became a real meaningful thing, going from Professor I to Professor II, and they made it into a full rank, which ... made Rutgers a rather unique place, actually. ... Getting

people to write letters about whether you should be promoted to Professor II was always complicated to explain to the outside world, what Professor II was, but in any event, that was created in those days. Later on, we, and I'm not terribly proud of this, but ... it was decided--the administration wanted it--and we agreed it was a good thing, that we should not only have, sort of regular step increases for faculty that had already existed, but there ought to be an easier way for merit increases for faculty, if they had done an outstanding job. ... To make a long story short, we invented this incredible matrix of salary tables. ... It was initially set up that you would get your normal step increment going across, for a number of years, but you could get promoted with a mini promotion, which was a merit increase down to a higher salary range. That's the way it was supposed to work, and never quite worked that way, but in any event, we created this matrix, which now Furmanski has done away with it. We did that in the early days. I'll tell you several other things. .. We created a sabbatical program. I'll tell you about that if you want to know about that.

SH: I wanted to ask one more question before we move on. How resistant do you think the faculty were to the AAUP?

PL: You mean, I'm sorry, how resistant was who?

SH: How resistant were some of the faculty to there being a union?

PL: To there being a union?

SH: The AAUP.

PL: Well, the Physics Department was fairly supportive. They had seen that it was impossible to get the salary raises if we weren't unionized. ... The state had really done a stupid thing of only giving salary raises to the people that had collective bargaining, and it was seen as a way of doing something about that. Also, in those days, Rutgers was very far down in the ... national faculty salary tables that were published. ... The average salaries at Rutgers were very low. We appeared in a low percentile, and particularly that was a problem at the full professor level, where we weren't competing, and were losing faculty because other universities were paying more. It was felt by everybody that we needed higher salaries, and by going this route ... we could in fact [obtain them], and in fact, it happened. We ended up moving up dramatically up the salary tables, over the years, through this process.

SH: I had thought that maybe there were some old-school professors who were resistant to this idea.

PL: Oh, well, I'm sure there were. ... I think the majority of the faculty supported this. It was strange going to another university, and tell them, "At Rutgers University, we were unionizing," because it was a very strange thing. There were no other major universities that were unionized. There were places like Oakland University in Michigan, and other smaller universities that were unionized, but ... I think, in fact, we may even be the only public university in the AAU that's unionized, I'm not sure. ... We've been very unusual throughout this process. I was, in those days, put on a number of national committees, Committee N in particular, of the AAUP, the

national AAUP, which is charged with collective bargaining. I was on that for a while, and they couldn't believe that Rutgers had done this, but in any event, in fact, I was invited not only to go to other universities and talk about my physics, but to go to other universities and talk about the union experience. ... Another person who basically recorded all of this, and wrote it up, is Jim Begin. Do you know Jim Begin? ... Barbara Lee was the Dean of SMLR [School of Management and Labor Relations] and her husband is Jim Begin. He sat in on all of our collective bargaining meetings in those early days, and recorded it and published a book on it. He was a professional labor studies person, so he's a good guy. So, you ought to interview him. He's retired now, but he's still around.

PC: I can definitely track him down through Barbara.

PL: Yes, track him down through Barbara. ... Let me tell you about some other progress. One was the tenure issue. We had lots of discussion about tenure, and the upshot of that, was a creation of a committee of the University Senate, chaired by Sidney Simon, which I was on, which wrote the present tenure regulations in university regulations. ... They codified a bit about tenure and ... how the process goes and all of that, but I suppose at the time, the most controversial thing they did is they gave these auxiliary faculty, library faculty, extension faculty at the agricultural experiment station, extension agents, and the county agents, extension specialists, and the county agents, and perhaps some other people, faculty status, and put them on tenure tracks, which had not existed before. ... That came out of Sidney Simon's committee.

PC: Now when you say committee, was this a university committee?

PL: It was a committee of the University Senate. It was not an AAUP committee, but there was some kind of an agreement, when it was setup, that some AAUP members would serve on that committee. ... It was in that respect that I was put on that committee.

PC: Did this codified version of the tenure requirements take AAUP approval to go into effect? In other words, was there collective bargaining on tenure requirements?

PL: No, in fact, one of the things we did, and this was done with the knowledge of ... Mason Gross and Dick Schlatter, is that there were many things, such as tenure regulations, we decided we did not want to put it in a contract and have to renegotiate every few years. We wanted this engraved in stone, and so we wanted it part of university regulations, okay? ... To do that, you had to go through the University Senate, and through the regular process, and ultimately had to be adopted by the Board of Governors. So, that's ... what we decided to do. We did other things in that way, and I've got a great story to tell you about how we got a sabbatical program.

PC: Let me stop and ask you a question first about the procedure, because I am curious about it. At that time, did the University Senate have a composition that looked roughly like it does today, which is to say, administrators, students, and faculty? Or was it solely a faculty body?

PL: There were no students, just administrators and faculty, as I recall. The students were admitted a little later.

PC: It was faculty deciding on tenure regulations for faculty members?

PL: Right, but the Board of Governors had to approve.

PC: Okay.

PL: But there were administrators who were actually voting members of the senate, but it was basically a faculty senate I believe. I don't remember students being involved in those days. I think that was later.

PC: I was just curious.

PL: Yes, it must have been Bloustein or somebody who brought the students in, I can't remember. ... Maybe Gross did it in his last days but I--in those days--I don't remember any students involved. There certainly weren't any students on this committee. This committee was a faculty committee, with faculty from different parts of the university, including Newark and Camden. It was the University Senate, and Sidney Simon was the chair, I can tell you that.

SH: But there would have been faculty from Douglass and University College as well as Rutgers College?

PL: Of course, they were in the University Senate, but they might not necessarily have been on this committee.

SH: Right.

PL: Yes, right.

SH: Douglass faculty were also part of the AAUP?

PL: Oh, the AAUP represented all of the faculty in the university, including Newark and Camden. ...

SH: Newark and Camden were part of it.

PL: Oh, yes, there were people like Leo Troy, from Newark, who was active, also on the bargaining team, and somebody from Camden, whose name I forget, yes.

SH: Okay.

PL: So, right. So, there were people from all these campuses that participated in all we did. So, whatever we did applied to Newark and Camden as well. ... The promotion procedures and all these things applied to Newark and Camden, just as they did to New Brunswick.

SH: This is taking place in the late 1960s into the early 1970s?

PL: Yes, into the very early '70s, yes. So that's what was going on.

SH: Before the Livingston College.

PL: Right.

SH: Okay.

PL: Can I go on to something else now?

SH: The sabbatical?

PL: Yes, well the sabbatical begins with dental program. Oh, well the other thing I should tell you, before I get into that is, and I wasn't myself particularly involved in that, but I can tell you that George Horton was--George Horton died recently, so you can't interview him--but his son had some major hormone problem. He needed growth hormone, he was ... dwarfed and had all kinds of physical problems because of that. ... So, George was very concerned about medical care, and pushed unrelentingly; George Horton, when he went after something, was like a bull. I mean, you just couldn't hold him back, and he went after the creation of what became our (CHP?) The first HMO [Health Maintenance Organization] in New Jersey, I think, and was actually a model [for it], but in any event, he played the major role in negotiating that with (John Martin?), who was then the vice-president for personnel. ... They setup committees of various sorts with AAUP people, I wasn't on them, and university people, and probably got in experts of various sorts, and they hired this guy, Roger, whatever his name was, who was the first head of (RCHP?), and set it up, and built that building out there on Route 1, and (RCHP?) was there. It was a terrific thing, until it was bought out by (HAP?), and destroyed. I mean it was wonderful, as long as it was just a local thing. Certainly for its time, it was way ahead of things, and it was a good system. So, that's all I know. George Horton did that. ... The AAUP played a major role in that, so we would talk about it from time to time at our bargaining sessions, and again it was something that was so important, that we didn't want to make it a part of the contract. We wanted it setup as an institution that was separate. So, we were constantly doing that kind of thing.

SH: Did that include the dental plan that you had mentioned?

PL: Okay, let's go to dental. Dental is something that we didn't [have]. It wasn't included in RCHP. There was no dental program, there was no statewide dental program. ... Whenever we would meet with the faculty, they would say, "We want a dental program, we have got to have a dental program," and a few people did some research on it, and discovered that if you had a group plan, the cost per person of dental insurance was much less than if they went go on their own to try to buy a dental plan. So, we went to Dick Schlatter and said, "Can we form a dental group and let the faculty join it, and the total premiums would be paid by the faculty themselves, out of payroll deductions, so that it wouldn't cost the state anything?" The administration tried to get the state to allow this but whomever they talked to, kept getting refused by the state, that they couldn't allow a dental plan to Rutgers employees because the highway patrol and other state employee don't have dental plans, or agencies in Trenton don't have dental plans, or whatever.

... So, we got nowhere with that, ... but George wouldn't give up. So, George, one day, set up a meeting with Ralph Dungan. ...

PC: The Chancellor of Higher Education.

PL: Yes, the Chancellor of Higher Education.

PC: Chancellor of Higher Education, yes.

PL: Thus, we met in Princeton for breakfast, George Horton, and Ralph Dungan, and I, just the three of us, and the purpose of the meeting was to talk about dental plans. ... So, we went, and one of the first things we did is presented to him our plan, and showed him our data, and said, "What we want to do at Rutgers is just setup a group for Rutgers employees. The premiums would be totally paid by the faculty themselves by payroll deduction, and we wouldn't ask the state for any money. We're just asking permission to setup our own group plan." ... Dungan thought about that for a moment, and he says, "Well, if it doesn't cost the state anything," he says, "Why should I object?" ... He says, "I don't have any problem with that." So, that gave us another idea. We had been trying for, since the beginning to create a sabbatical program at Rutgers. There was no sabbatical program at Rutgers in those days.

SH: Really?

PL: That's right. The only thing that existed was through the Dean of Graduate School, Henry Torrey, a research fellowship that you could get. You had to apply to the graduate school for it. ... If you got one of these, it allowed you to take a semester, or a year off, and go do research some place. You'd make a proposal to the graduate school of where you wanted to go do your research, and if you were among the few chosen--they didn't have a lot of these--then you were allowed to go off to Cambridge or Stanford or some place to do your research, and they gave the department a TA [Teaching Assistant] to replace your teaching while you were away. That's the way it worked, and you just stayed on the payroll. You just got your pay, and the department got a TA to replace you while you were gone, but there weren't many of these since the budget was tight. ... So, that's the way it worked, but there were so few, and then it was such an elite system, you know. I mean there were it seemed at most a dozen, or twenty in the university that could go in any year. They just didn't have that many of them. So, we had this bright idea right there while we were meeting with Ralph Dungan. We said that, and we'd been turned down by the state for sabbatical programs totally. We had tried to set it up out of the package of money that was coming, salary raises or something to fund a sabbatical program. They wouldn't have anything to do with it. So, what we said to Dungan, since he had said, "Well, the dental program is fine, as long as it's not costing the state anything." George Horton said, "We have a new idea for a sabbatical program. Our sabbatical program would be this. That every seven years a faculty member could go on sabbatical. When they go on sabbatical, they have to take a twenty percent salary reduction," and that money from this reduction would go back to the department to pay for the replacement of teaching while they're away. It would be totally paid by the person that's going away. It wouldn't cost the university anything, wouldn't cost the state anything and would be totally paid--just like the dental program--by the individuals who are involved." He

[Ralph Dungan] says, "Well if it's not costing us anything, I don't have any problem with that."
[laughter] ...

SH: This was not on the agenda for the day, but you decided to go for it because he was receptive to the dental program.

PL: It was because we had gotten the dental program, but we had talked about sabbaticals for a long time. So, ... we didn't actually create it on the spot. We had had many discussions about this before but the fact of putting it together and presenting it to Dungan had not occurred to us until we saw his reaction to the dental program. So, that's basically all that happened. When we came back at our next bargaining session we proposed this to Dick Schlatter. ... He said, "Well, this is ridiculous. I've talked to them about this before, they're not going to approve this." ... We said, "Well, we've talked to them, and we think they will now, if you present it in this particular way," and we told him how to present it. ... So, he apparently called Ralph Dungan, and presented to him this program, and ask him if there's a chance the state would, or that he, or whatever, would approve this. ... His response, apparently, was, according to Dick Schlatter, "Approve it?" He says, "I invented it." [laughter] ... So, we got a sabbatical program.
[laughter]

SH: This was approved in the early 1970s?

PL: Yes, it was called the FASP [Faculty Academic Study Program] program when it was first created, and they took out, I think it was twenty percent of your salary, and that money was to go back to the department, which they could use to hire a TA, or whatever, to replace you, while the faculty member was away on sabbatical.

SH: When something like this gets approved, does it go into effect immediately?

PL: Well, ... we didn't put it in the contract. It was, again, we didn't want it to be something that you had to renegotiate every few years. But, I think it got put into the contract in subsequent years after my time, but in those days it was not. It was just simply the university announcing a sabbatical program, and the rules for it were promulgated. ... It was run by the university, and it didn't take extra money in a sense, because it was all being paid for by the people going. ... It didn't cost the deans or anybody, anything to a degree. It cost the departments, because they didn't have your teaching, but had to make do with a TA. So, the departments were basically paying for it.

PC: You were paying dues in the AAUP back then?

PL: Yes.

PC: That was from the very beginning.

PL: Oh, yes. Yes, there were dues.

PC: You said you weren't as active over time.

PL: I wasn't involved in setting those dues.

PC: Was there a point at which you remember the union turning into something different than the one you set up?

PL: Yes, subsequently, after Bloustein came, there was change. The administration was not as supportive of the union as Mason Gross and Dick Schlatter had been. I mean, when we met with them, it was like a college faculty meeting with brainstorming on how to make this a better place. ... We would try ideas out on each other, and see how they felt, but all of us involved were really just trying to make this a better place, and certainly were supportive of merit increases, and faculty doing research and all kinds of things like this. So, we were in tune with the administration. Also, we were all Rutgers faculty ... involved in research and teaching and everything else. Sometime ... after Bloustein became President, and different people were involved with negotiating with us, they became more hardnosed. Maybe, there were also less good times economically in the state, I'm not sure, and so, it became more adversarial. ... I don't know, some of the novelty had worn off, so some of the good faculty that had been involved in the early days dropped out, and the Wells Keddies [Wells H. Keddies], and the others more professionally involved with collective bargaining, came along and pushed hard to do various things. ... So, there developed an antagonism between the AAUP and the administration, and people like myself simply dropped out of the process. I didn't want to be involved in that at that particular point. ... Of course, we had done our business by that time, so it was more hardnosed bargaining about salaries, and how much would be going into the merit pot, and all these kinds of things by that time. ... I can't really tell you since I really dropped out then, after that, and devoted myself back to my research, and spending time in the department. I guess, partly because of what I had done there, I was appointed by Harry Zapolsky when he came as the chairman of the Physics Department, as the Associate Chair, and Graduate Program Director. I got involved in the department again.

SH: Did your activities impact your personal tenure process in any way?

PL: Well, I was tenured.

SH: Tell us about the process.

PL: I was still doing research and actively publishing in those days. It took some of my time. You know, it didn't take that much of my time, but it took some of my time. ... I was still aware of the tenure process, and still kept very actively with my students. I had some good fortune, some good luck in making some good discoveries, and having some good publications in those days. We did some very fun things. I'll tell you about one of them. I'm not an experimental physicist, I'm a theoretical physicist, and so I do calculations, and one of the problems in those days was the process of percolation. I don't know if you know what percolation. ... Well, you probably know something about percolation, for example. If you were putting in a septic tank, they wouldn't send somebody out to run water and see if it runs through your soil. It's also percolation when you run water through coffee grounds. If you have a pile of sand and you pour water on it, the water flows around the channels between the grains, and flows through the sand,

but if you compress that sand, those channels block up, and just become pockets and at some point it no longer conducts, and it's actually like a phase transition. It goes from being a conductor to being an insulator at that particular point, and people were debating what is the nature of that transition. ... We had some mathematical theories that we worked out about it, but one day sitting around at lunch, we had a great idea. I said, "Well, we could do a simple experiment, and figure this out." So, we went to the local hardware store, and we bought a three foot by three foot section of something called hardware cloth, which is what's used to make rabbit cages. ... It consists of a square mesh of steel wires, which are put into a steel mesh and then they're dipped in solder. So, that they're well-connected electrically across. It's not done for electrical applications, but nevertheless the wires are metallicity connected. ... So, you can send a current through it, and measure its electrical resistance. I had a graduate student, Brian Watson, one of my more successful graduate students. Together, we hooked it up, and got Bernie Serin to lend us his electronics, to measure its electrical resistance. ... We gave Brian a pair of wire clippers, and we used random numbers from the computer to give us coordinates to clip and we, yellow paint, painted lines so we could count the wires. ... The computer randomly gave us coordinates, and Brian would go in there with wire cutters, and we would cut out a piece of the wire at that particular point, and we would just randomly do that until the system finally fell apart. ... As it fell apart, it was percolation, because the electrical current would have to find a path around so that it could still go through, and we watched it fall apart and stop conducting, and measured the electrical conductivity, just like it was a phase transition. We measured what's now called the critical exponents of the phase transition, and published them. ... I think I've gotten more citations from that paper, than any other one, and all he did, is he sat there for two weeks with a pair of wire clippers, and clipped it apart. ... There were a lot of very funny things. One of them was how do you know when you finally clipped it apart. Well, we knew we had finally clipped it apart, because we had it hooked up to a constant current source that always upped the voltage, and so the current was the same, and when we finally clipped it apart, it started arcing. [laughter] ... So, you knew when there was no longer a path. So, that was one of the few experiments I've ever done. ... We submitted it to *Physical Review Letters*, but they wouldn't take it, because they said, "This is just a high school experiment." But we got it published in the *Physical Review* but not the more prestigious *Physical Review Letters*. It became a classic, because nobody had done this before. That was done in 1974, which was at the same time as I was working with the AAUP. ... I'm telling you this because I was doing good physics at the same time as all of this was going on, enough to get me tenure, anyway, although the AAUP work probably cut into my productivity.

SH: I wondered if there had been any kind of repercussions for your involvement in the union.

PL: Well, there may have been some people in the department who thought I ought to spend more time on my research. It was countered, I guess, by the fact that I had some nice publications.

PC: Was most of what even a theoretical physicist did driven by grants? Were you dependent on grants?

PL: Well theoretical physicists are much less dependent on grants than experimental physicists. Experimental physicists need equipment in order to do an experiment. They can't do it without

funds for the experimental equipment and supplies. I happened to do a simple experiment that didn't cost any money. Oh, I was going to tell you about that, also. We borrowed all of this equipment from Serin's lab, so we didn't need any electronic equipment, but we did have to go out and buy the sheet of hardware cloth, and a few other things to attach to it. So, we held a lottery in the department as to when in fact it was going to fall apart, and we used the lottery money to pay ... for the apparatus, but that was unique. Most of the experimentalists were doing low temperature or high magnetic field experiments, or something else. So, they needed very expensive equipment. Indeed, some ran the tandem accelerator in the Department and other things that were very big expenses. So, they had to have grants. Theoretical physicists are much less dependent on that. They need the grants to pay summer salary, so very important for one's salary. Second, the grants pay for graduate student research assistantships and post-docs. We always had too few graduate students. If you want to convince a graduate student to come work with you on a project, or get a post-doc to come from someplace else to work with you on a project, you needed a grant for that. ... It was vital to have colleagues, so that was important. Somewhat less important is that it also paid for money to go to a conferences and incidental expenses.

SH: How big was the department when you came in the late 1960s? I read this morning that you have sixty faculty now.

PL: There were thirty-one faculty when I came.

PC: In history, we have amorphous fields that overlap. In physics, you have groups and they are real.

PL: Oh, they're real.

PC: How do groups work in physics?

PL: Well, if you have a group, they work together, and they talk to each other. It's very important to talk to other people about your research. That's a way, in fact, that you learn. The groups have their own seminar programs. So, they invite seminar speakers. So, it's a way that you learn the latest things going on in your particular field. ... So, ever since I came we had separate seminars in solid state physics, nuclear physics, and high energy physics. We didn't have astronomy in the early days. So, that was added later.

PC: Would these groups be the same groups you would find at NYU or Princeton?

PL: No, not necessarily. You go to another place, and they may have groups in plasma physics, or they may have groups in atomic physics, or they may have groups in some other specialty. Even in these areas we may focus in a particular subspecialty that you don't find elsewhere. So, there are many areas of physics that we just don't do. ... By concentrating in a particular area, you develop a reputation in that particular area. ... So, it's probably important for your productivity and for your reputation to have people that interact strongly with each other.

PC: Did the reputation of the Physics Department depend in part on the quality of the groups you had, not simply on the quality of the people?

PL: Yes, there are different topics that are hot topics for research ... at different times, and you either have to have people that are flexible that can move into whatever is productive at the particular time, or you have to hire people in those areas, and so, over the years, we've done that. We've found a particular area which is particularly promising, and we hire in that particular area. Sometimes, you have to be prescient and hire ahead of the game. We did that for a while, hiring all these strength areas, before other universities had done that. ... So, for a little while, we were the best university in the world in terms of string theory. Now, all the best universities have taken this up and stolen away some of our best faculty. ... Two of our string theorists just got admitted to--it was in yesterday's *Targum*, the American Academy of Arts and Sciences. [Editor's Note: Physics professors Gregory Moore and Thomas Banks were inducted in the American Academy of Arts and Sciences in 2011.]

SH: Since I've been at Rutgers the Physics Department is continually in the news in some fashion.

PL: Yes, it does a lot. Actually, they're in the news more these days than in the past because we've developed a strong astronomy program. Astronomy is something that the public is very interested in, so that you find lots of articles on the astronomy. That's a very exciting field right, now. This is a golden age of astronomy, with the Hubble space telescope and other major telescopes. There weren't astronomers here back in those days. We had an astronomer back, long before my time, we had one astronomer, but we didn't have any astronomers when I came. Well, we had a guy that taught an astronomy course, Maurice Bazin, but we really didn't have any astronomers. ... In fact, I taught the astronomy course once myself, early on. ... Do you know the Schanck observatory?

PC: Yes.

SH: Tell us about that.

PL: Well, it's in dire disrepair now. What happened, more than once, is that people broke into the building and stole parts of the telescope. ... Its final demise was somebody came in and actually stole the objective lens out of the telescope, and it wasn't worth replacing. So, it's just a shell of a building now. ... Well, there's the frame of the telescope maybe, but we have a new telescope over on the Busch campus, so if you want to see the stars and planets that we used to see, we can do it up on the physics building now. [Editor's Note: The Robert A. Schommer Astronomical Observatory is located on top of the Serin Physics Laboratory on Busch Campus.]

SH: The Schanck Observatory is at the corner of George and Hamilton.

PL: Right. The other thing that's happened there, is that they have let trees grow up around it, and the other thing that's happened is that they put street lights along there, and a parking deck across the street, so the light pollution is impossible there. So, it's not a good place anymore, but ... in its day, in the nineteenth century, it was a very important place. It was a place where they

actually, accurately, measured time. There is a little building that's associated with it that has a little slit of a roof going across, and they would measure the transit of the sun precisely, they'd measure the time there. [Editor's Note: The Schanck Observatory was built in 1865 to study astronomy at Rutgers. Named after its donor, Daniel S. Schanck, it was patterned after the Tower of the Winds in Athens, Greece.]

PC: In the Physics Department, was it commonplace to share resources with other universities?

PL: Well, there are pieces of equipment that are too expensive for anyone place. ... So, there are national laboratories like Brookhaven National Laboratory and Oak Ridge National Laboratory and Argonne National Laboratory where major expensive equipment is built, and scientists from universities throughout the country, and maybe throughout the world, come and do their research. That's even gotten more so with CERN in Switzerland. ... In some of those fields, they absolutely work together. In fact, one of my problems, when I was department chair, was dealing with promotions of these faculty members. ... Especially, these crazy rules you got about you can't have co-authors write letters of recommendation. Well, we have a professor who on all of his publications he has two thousand co-authors, everybody in the field is a coauthor, so now what do you do. [laughter] In fact, a very funny thing happened when I was department chair. Physics department chairs throughout the country get together in a couple different places each year, but at least once a year we get together in Chicago, sit around a big long table, and give highlights of what's happening at our university, and try to make sense of what's happening nationally in physics. In one year, we went around the table, and talked about highlights of what was happening at our universities. These are the major universities. One year, we discovered that all of our physics departments had something in common, and that was that one of our faculty members had discovered the top quark. [laughter] It was because we all had some faculty member that was on the paper that reported the discovery of the top quark.

SH: Who started that?

PL: The size of experimental groups have gradually grown as the size of the experiments that they're doing have gotten bigger and bigger. I mean, some of these experiments are so big that single countries can't afford to put them on. It takes a coalition of countries, I mean the CERN accelerator in Geneva, Switzerland is in a big tunnel underground, that goes around the entire city of Geneva, and so it's like twenty-nine miles in diameter. With the equipment all the way around, they have particles racing at near the speed of light and colliding with each other. It cost billions of dollars to build this. It's not something that can't be afforded build more than once in the world. So, all the scientists in the world want to come there and do experiments there. Not only that, but they have such big teams working, it's difficult to give people credit for the big discovery that comes out as to who it was that discovered it. If there were all these people on the project, I keep telling them that what they need to do is give credits like in the movies. They need to say "data analysis by," and "sample preparation by," and all this kind of stuff, so that the true role of each person can be seen from the paper, because it's a very big crew, but they don't do that. Their social relations are so complicated, that all they can do at most is to list a few of the most important people first or last--the director and producer. [laughter] ...

SH: I wanted to know more about the department chairs meeting.

PL: Yes, the department chairs get together once every year at O'Hare Airport in Chicago. There's an airport hotel, a Hilton, attached to the airport. They reserve a big room there, and everybody flies in, and we exchange all our information in one day and we fly home. That's all-- it's a one day conference.

SH: How long has it been in existence?

PL: Since the early '70s. Anyway, I don't know, a very long time.

SH: Perhaps at this stage we can kind of chronologically go through your career.

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Reviewed by Nicholas Molnar 9/26/11

Reviewed by Paul Leath 2/14/12