

Dr. Alan Peterkofsky
Oral History Interview A

Higingbotham: Good afternoon. Today is February 15, 2023. I am Haley Higingbotham, the Assistant Archivist of the Office of NIH History and Stetten Museum. Today I am joined by Dr. Alan Peterkofsky. Dr. Peterkofsky is a biochemist who has worked at several different institutes at the NIH, such as the National Institute of Arthritis and Metabolic Diseases, now NIDDK [National Institute of Diabetes and Digestive and Kidney Diseases], the National Institute of Dental Research, now NIDCR [National Institute of Dental and Craniofacial Research], and the National Heart, Lung, and Blood Institute [NHLBI]. Dr. Peterkofsky has been a major contributor to our knowledge of several essential processes in bacteria, such as post-transcriptional modification of transfer RNA, regulation of cyclic nucleotides, and structural biology of sugar transport proteins. Currently, Dr. Peterkofsky is a scientist emeritus at NHLBI. This interview will cover both his early life and education along with his career prior to coming to the NIH. Thank you so much for joining me, Dr. Peterkofsky.

Peterkofsky: Okay, so I'd like to start by telling you about my father, who grew up as a young man in a small town in Poland that was called Lask, spelled L-A-S-K. His first name was Isaac, and even though my current name is Peterkofsky, his name was the Polish equivalent of that, which is Piotrkowski. I'll spell it for you. It's P-I-O-T-R-K-O-W-S-K-I. What he was faced with as a young Jewish man was that the area where he lived was constantly being fought over by the Russians and the Germans; they had possession of that area back and forth. The typical situation that a young man like him was faced with was being conscripted into the Russian army, and then being sent to the front, typically to being slaughtered by the Germans. In order for him to try to evade that conscription, my father had all of his teeth removed – hoping that that would allow him to be exempted from service in the Russian army. It didn't work, and he was still faced with conscription. He ran away and went to the bigger city of Warsaw where he sought out an opportunity to learn a trade. He identified this man that owned a bakery, and he served as an intern in learning the bakery business. It turned out that that man had living with him a young woman who was his niece, whose name was Bertha. My father developed a romantic relationship with that woman, and they got married.

Shortly thereafter, in order to escape from the kind of bad treatment of Jewish people, this man left to go to the United States and settled in New York City. Shortly thereafter, when he arrived in the United States, he became a sponsor of my father and his new wife. They then traveled by boat, leaving from Rotterdam to come to New York City. They arrived at Ellis Island to be processed, and he relates that the immigration officer at Ellis Island looked at the ship's manifest and saw this name of Piotrkowski and said, "This is not a suitable name to live with in the United States." He changed the name to what is my current last name – Peterkofsky. That's how that that originated. That was in the year of 1913.

My father and mother settled and moved to the Bronx in New York City, where they lived very close to this uncle who became a support system, especially for my mother. Eventually, my family grew over a period of 18 years or so to have six children that kept my mother fairly busy. The first born was my sister, who was 15 years older than me. As it turned out, my father and mother never learned to read or write English. They very closely identified with their Jewish community and lead that very restricted life. Eventually, my father made arrangements for his younger brother from Poland to also emigrate to the United States, who also became

trained as a baker. My father and his brother began to collaborate in the bakery business, and eventually they moved to Boston.

Higingbotham: Is that where you mainly grew up?

Peterkofsky: Yes. I was born in Boston in 1930. My father continued to be in the bakery business. During that period of time, in the 1930s, that was the time that was a great depression in this country, and my father was having difficulty financially. There was no work for him in Boston, so the family decided to move back to New York. Most of the family moved back, except for my oldest sister, who had been going to law school. She had just recently graduated from law school. That was a specific law school that allowed women to go to law school. In those days, Harvard Law School wouldn't admit women, so she went to this woman's law school and got a position as an attorney in Boston. While the family moved to New York, at least for a few years, she stayed there in Boston.

Higingbotham: Do you remember the name of the school she went to?

Peterkofsky: It was called Portia, P-O-R-T-I-A, Law School, which was a school that was set up specifically to allow women to go to law school, while Harvard was an all-men's law school at that point in time. I believe that it was not until 1950 that Harvard Law School admitted men and women. I went to elementary school, and I was generally a quite studious student but didn't have any early inklings of what my life would unfold to be. It turned out that because I got reasonably good grades in elementary school, I was put on a list of students to take an exam for an academic high school that was not in my neighborhood in the Bronx. The school was Stuyvesant High School in lower Manhattan. I went there and just studied and did whatever I could. At some point in time, I had a little bit of a sense of what medicine was all about, because when I was 13 years old, I had an appendectomy. In those days, there were no antibiotics, so the incision that I got from that appendectomy became quickly infected. I had to spend my time once a week going to a local doctor to get that infection treated with no antibiotics, where he would swab it with phenol that would burn out the bacteria. I became a bit fascinated with medical practice at that point.

I do remember when there was a choice made about what I would do after high school. I don't have any recollection that there was any kind of career guidance at the school, but my high school yearbook says that I applied to NYU Medical School to become a physician. I was immediately rejected from that, which I assume was partly due to a quota for Jewish people at that point in time. The backup choice was to go to the local college – it was called City College of New York, or CCNY, which was frequently referred to as the Jewish Harvard, so I went there. It seemed to me that at the beginning of my experience at CCNY that the curriculum was not much different than what I had experienced in high school, and it seemed to me that I was wasting my time. After one semester at CCNY, I stopped going; I dropped out. My father responded to that by training me to be a baker. At that point in time, he had his own bake shop in the Bronx. He showed me all the elements of becoming a baker, and he had a connection with the baker's union because he had been one of the early activists in forming that union. I at the age of 17 or 18 became what people thought as the youngest member of the local bakers' union.

Higingbotham: Did you enjoy baking?

Peterkofsky: I thought it was really pretty fascinating to see this phenomenon of dough rising. And I thought it was sort of another form of life.

Higingbotham: Some people compare baking to chemistry.

Peterkofsky: Yeah, there is really interesting chemistry involved there. The kinds of ovens that they had in commercial bakeries were huge. It was really interesting to see the conversion of this puffed up dough to become a bread that tasted delicious. It was an interesting kind of thing, and I really enjoyed that.

Higingbotham: What made you go back to college and science?

Peterkofsky: I assumed at that point that I could have a career as a baker, but my oldest sister, who was 15 years older than me and was sort of a mother figure to me rather than my actual mother, put pressure on me to go back to college. She felt that in the long run, I would be better off doing something more professional than being a baker. I returned to CCNY and took courses. It turned out that in the last year of CCNY, I enrolled in a course called biochemistry. There, I discovered what was really something that captivated my interest. There was a laboratory associated with that course, and one of the professors in the biochemistry department became the supervisor of research. I was partnered with a young man, a fellow student, who became a fairly important influence in my life. His name was Herbert Weissbach, and he was my partner in doing some primitive experiments that by modern standards would have been Mickey Mouse stuff, but it was for me, at that point, very exciting and we had a lot of fun. I met some people in the department at the professorial level – one of whom was a professor whose name was Ernest Borek, who had a joint appointment at the biochemistry department at Columbia, and he had recently come back from a sabbatical at the Pasteur Institute. He was very, very enthusiastic about modern biochemistry and biochemical genetics. He enthused a lot of people, and, interestingly, he became a person whose interest and some aspect of what I did later on overlapped, and I had experience in renewing my acquaintance with him much later on.

As it turned out, I really didn't know what to do when I finished graduating from CCNY. There was not much guidance in terms of what to do. Then a scientist at NHI [then the National Heart Institute] named Sidney Udenfriend wanted to set up a cooperative program for Ph.D. studies in collaboration with George Washington University to accept students that would take coursework at the university and do their research at NIH. Weissbach and I were seniors at CCNY, and it turns out that Udenfriend was a graduate of CCNY. He made an appointment with a CCNY professor, Abraham Mazur to come to CCNY to recruit the first student in that program. Herbert Weissbach was the student he selected. Sidney Udenfriend was a scientist who had come with a group of people from this Goldwater Memorial Hospital in New York City a few years before with a scientist whose last name was Shannon [James Shannon], who became the scientific director of what was then known as the Heart Institute [now NHLBI].

Higingbotham: That's interesting.

Peterkofsky: Yeah. What happened was that a few months after Weissbach arrived at the NIH, he was full of absolute enthusiasm about the experience there, and he was located in a lab in Building 3 of NIH, which housed a community of people that has become quite famous.

Higingbotham: We hear about Building 3 all the time. It was really quite remarkable.

Peterkofsky: You keep hearing stories about the environment in Building 3. He was really enthusiastic, and he got in touch with me and said, "By all means, do anything in your power to come to NIH like I did." He suggested that I contact a scientist in Building 3 who he referred to as Chris Anfinsen [Christian B. Anfinsen]. Chris Anfinsen had, at that point in time, become very well known for some of the studies that he had done on spontaneous refolding of proteins, especially ribonuclease. He was destined to, and then actually did eventually, win a Nobel Prize. He [Weissbach] said, "Get in touch with Chris Anfinsen, and he will be a great opportunity for you." I assumed that when he referred to Chris Anfinsen that was a bit too informal for me as a young man to contact him. I assumed therefore that I should write a letter not to Chris Anfinsen, but to Christopher Anfinsen. Little did I know that his name was not Christopher, it was actually Christian.

Higingbotham: Oh, no.

Peterkofsky: I wrote out a letter of introduction to Dr. Christopher Anfinsen, asking if there was any opportunity to come there. And he responded saying, "I'd be happy to talk to you. Come on down." I drove down to NIH and was introduced to him, at which point he corrected my understanding of what his name was. He quickly told me that it was too bad, but he didn't have any spot in his laboratory for me. While I was disappointed to hear that, he said, "Well, I have a scientist in my laboratory who has recently accepted a position to run a new laboratory at the New York State Department of Health in Albany, New York. And if you like, I'll introduce you to him." He took me down the hall and introduced me to this man whose name was Ray Brown [Ray Kent Brown, Jr.]. Ray Brown told me about this new job that he had. He was going to leave town for Albany very shortly and accept this new job running a laboratory that was going to be involved with a purification of blood proteins. It turned out that during the war, the New York State Department of Health had accumulated a big stock of blood from donors that could be used for transfusion purposes, for wounded people who would be coming back from the war for treatment. He asked me if I might be interested in joining that new group that he was putting together but felt that I had to pass a qualification test. The qualification test was that he gave me a pad of paper and said, "I want you to calculate what the amount of sodium chloride would be necessary for you to make one liter of one molar solution of sodium chloride," which to me at that point was not that challenging, because I knew the molecular weight and was able to do the quick calculation. I handed him the response, and he said, "You're hired." So that that was a quick way of getting an application for a position.

Higingbotham: I wish all were that quick.

Peterkofsky: Yeah, that was pretty quick. It turned out that he had a research assistant in that laboratory in Building 3, whose name was Dorothy [L.] Kauffman, who was very dedicated to him, and she had agreed to come to Albany with him. When I went there, she became a close associate when I was doing research and was very helpful in guiding me through learning some of the stuff involved in protein purification because, basically,

the thrust of the work in the laboratory at Albany was to purify proteins from blood plasma. The situation there was that not only did I have a work position at the New York State Department of Health, but across the street from the laboratories of the State Department of Health, there was Albany Medical School.

Higingbotham: I was going to ask what came first, the job or going to Albany Medical School, so there we go.

Peterkofsky: It was arranged that I would be a graduate student in Albany Medical School at the same time that I was an employee at the New York State Department of Health. The arrangement there was that I would take the same courses that medical students did. I spent my days working in the laboratory, helping to purify proteins, and typically the way in those days of purifying proteins was that you assume that proteins were going to be most stable when they were kept cold. You did all of your purification work by freezing in a cold room. I was equipped with a very heavy parka to be wearing in the cold room. You would slowly add alcohol to a suspension of blood, and when you began to see the suspension become turbid, then you would centrifuge the suspension and then look at the precipitate by some analytical method and determine whether this was a protein of interest. If not, throw that away and continue adding alcohol or a salt like ammonium sulfate to get more precipitates. You would just do that over and over again. Eventually it turned out that that a protein came out that turned out to be a glycoprotein, very pure, out of the fractionation of this blood and that was characterized and published, and I was a co-author on my very first publication, which I thought was extremely exciting.

At the same time, I was going to classes at the Albany Medical College, which were exactly the same courses that the medical students were taking which I found to be of very limited interest to me. I do remember that there was an experience where they had a laboratory at the medical college where you were supposed to be trained as a doctor to do a venipuncture to draw blood, and the first lesson that they gave during that was to give you an orange and a hypodermic syringe with a needle and teach you that as you slowly punctured the orange when you were going through the skin, you met resistance, but when you got into the fruit, then there was less resistance. That was the way you were expected to experience the same thing with a real person, and then the next day, you were partnered up with another medical student. The woman that I was partnered up with, when it came time for her to be the person that I was going to do the venipuncture on, she fainted. So I never got to do it on a real person.

Higingbotham: It seems that you were much more interested in the research side than the being a doctor side.

Peterkofsky: Yeah. So, in any event, I really did not enjoy the medical student curriculum, and in response to my unhappiness in doing that, I got permission to stay on as a registered student at the medical school but go over to the main University, which was called Union University in Schenectady, to take courses in organic chemistry, which I enjoyed much more.

Higingbotham: Much more your speed and less likely of someone to pass out on you.

Peterkofsky: Yeah, so I spent the remainder of my stay in Albany doing organic chemistry courses in Schenectady while I continued with stuff in the laboratory of the New York State Department of Health.

Higingbotham: Beyond working with Ray Brown and Dorothy Kauffman, who you've mentioned, do you have any other mentors or memorable people at either Albany Medical School, Union College or at--

Peterkofsky: It turned out that in the New York State Department of Health the director of that was a gentleman whose name was Gilbert Dalldorf, who was quite famous because he had discovered a virus called the Coxsackie virus, which was actually named after a small town close to Albany. That Coxsackie virus created a disease that was very, very similar to polio, which was a major interest because at about that time both Albert Sabin and Jonas Salk were investigating polio and eventually came up with treatments for that. That was Gilbert Dalldorf. [Dalldorf received the 1959 Albert Lasker Award.]

And there were some people who were recruited to come to the laboratory by Ray Brown. One of them was a nice guy whose name was Lawrence Levine, who had studied immunochemistry at Johns Hopkins University with Manfred [M.] Mayer. He became a very important influence on my career at the State Department of Health. He felt like the kind of stuff that I was doing—struggling with purification of blood proteins—was not going to get me very far, so he took me under his wing and taught me some of the elements of immunochemistry at that time. In fact, he became my research advisor in place of Ray Brown. We did a study of various proteins in blood and developed an assay method using a procedure that was called complement fixation as a method for quantitating the amount of different proteins in blood, and that became the basis for the master's thesis that I wrote that was submitted to Albany Medical College.

There was another investigator that had been recruited to come. The woman's name was Helen van Vunakis, and she had done research in protein chemistry for her PhD degree with a well-known biochemist at Columbia University. His name was Erwin Brandt. Lawrence Levine and Helen van Vunakis became romantically involved, became a couple, and eventually got married. They eventually moved away from that health department and took positions in the newly formed Department of Biochemistry at Brandeis University. I've remained in touch with both of them.

Well, it turned out that because they took a close interest in my welfare, they convinced me that it was not going to be a great future for me to stay at Albany, and said if I was smart, I should move back to New York City. They came up with a couple of names of people who they felt might be good preceptors for me to go on to a PhD degree. Lawrence Levine came up with the name of an immunologist at Columbia University. His name was Elvin [A.] Kabat, and Elvin Kabat was a rather eccentric scientist who had been very well recognized because he had respiratory problems. He was always walking around with a World War II gas mask on, and he was quite recognizable because of that. I went to Columbia to be interviewed by him, and for the life of me, I couldn't pay that much attention to what he was asking me because he was wearing a white lab coat that was wide open and his pants zipper was open. I was so distracted by that, that I just couldn't understand anything he was saying. I went away, telling him that I would consider his opportunity to go to Columbia University.

The other candidate that came up was a man whose name was Efraim Racker, who at that point in time, was at Yale University, but on his way to assume the directorship of a biochemistry unit at a very unique institution in New York City. That was called the Public Health Research Institute of the city of New York, which was actually very distinctive because it was the only city-supported research institute in the whole United States. It turned

out that the then-current director of that biochemistry unit, whose name was DeWitt Stetten, was on his way to move to the NIH, and that position was then offered to Efraim Racker to take over. I went to be interviewed by him, and I was very impressed with him as a dynamic investigator. He seemed to be very interested in having me come as his student. He had a joint appointment at the microbiology department at NYU Medical School and said that if I came there as his student, I would be able to study at the microbiology department. My response there was, "I really don't want to be a microbiologist. I want to be a biochemist." He said, "That is a problem," but it turned out that the chairman of the biochemistry department was a very renowned biochemist his name was Severo Ochoa, a Spanish gentleman who had done some very renowned work, and he eventually went on to win a Nobel Prize shared with Arthur Kornberg, who was a scientist who had actually gone to CCNY and been at NIH before going to Washington University. He told me that Ochoa had decided that he wanted to get the most work done in his laboratory by highly trained scientists and would only take postdoctoral fellows and not students, but Racker got in touch with him and said if he admitted me as a graduate student in the department, I wouldn't take up any laboratory space because I would be doing my research at the Public Health Research Institute. Ochoa said okay. He allowed me to become a student in the biochemistry department, which had a very important consequence in my life after that.

I went to become a graduate student at the Public Health Research Institute in the summer of 1955. Racker arranged for me to get a pre-doctoral fellowship through the NIH that was going to start in the fall of 1955, but I went there in the summer of 1955. The fellowship was going to pay \$1,800 a year, which was for me okay because I was still living at home and didn't have to worry about getting fed or housing at my own expense, so it was an okay arrangement. In the meantime, one of the attractions that Racker felt that I offered was that I had done some work in immunology, and he said, "Well, why don't we, before you start on your thesis project, do an immunology project of making an antibody serum to one of my purified proteins that I brought with me from Yale." There was a method that I had learned from Lawrence Levine at Albany called an Ouchterlony detection method for the interaction of an antigen with an antibody. Racker purchased a rabbit for us to inject with his antibody and told me that he would introduce me to this world-famous immunologist who occupied a laboratory the floor above the laboratory there, and his name was Jules Freund, who was very famous for developing what was known as the Freund's adjuvant that everybody in immunology was using at that point in time. We got the rabbit, and we took it up to the fourth floor with Jules Freund with a sample of the protein. Jules Freund, who was rather a mystical type of scientist, had this little vessel that looked like a small glass vase, and he put a sample of the protein in there and then added this magic mixture that he called the Freund's adjuvant in there and mixed it by squirting it up and down in a syringe to make a suspension, and then said, "Oh, now we have the mixture that will go into the rabbit." And he injected it into the leg muscle of the rabbit and said, "Now we wait a month." A month later, we brought the rabbit back to him and he bled some of the blood out of the rabbit and gave it to me, and then I performed this antigen antibody test using the method that I knew and, lo and behold, we had antibody. Everybody in Racker's lab was very excited. It proved that I was an authentic immunologist, and now I could throw that experience away and start becoming a biochemist. That was my introduction to the laboratory.

The activity in Racker's laboratory was that—because he had been a scientist who got his original interest in science in Germany—he had become an admirer of a very famous German scientist, whose name was Otto Warburg. Otto Warburg had actually won a Nobel Prize in 1931 for studies that he had done indicating that

cancer cells could grow in the absence of oxygen—a very important kind of principle—and it very much influenced Racker's attitude about science. In fact, he so worshipped Warburg that he kept up with all of the stuff that Warburg was doing in his laboratory in Germany. As it turned out, Warburg was a Jewish scientist who had become so famous that Hitler allowed him to continue his research, even during the war, while he was slaughtering all the other Jewish scientists. Warburg was immune to that. The major interest that Racker had in his laboratory was trying to understand the aspect of what was called bioenergetics. That is how energy was formed metabolically. Most of that dealt with activities that take place in mitochondria. He was focusing most of his energy with his group on trying to determine what the basis for mitochondrial activity that resulted in the synthesis of this important factor called ATP [adenosine triphosphate], which is the factor that drives a lot of biochemical reactions. It turned out that another current activity that Otto Warburg was studying was how to understand the first reaction in photosynthesis. Photosynthesis involves, in plants, the assimilation of carbon dioxide into something that eventually leads to energy. There was a series of reactions that was called the pentose phosphate cycle that had been described, sometimes called the Calvin Cycle, by Melvin Calvin who was at the University of California. The notion was that one of those intermediates in that pentose phosphate cycle which was called ribulose diphosphate was going to be the acceptor of carbon dioxide in the first reaction of photosynthesis, and Warburg was very interested in that. Racker said that it would be a good thing for me to try to understand what is going on in the first reaction of photosynthesis and determine the nature of the first product in photosynthesis.

Higingbotham: Is that what you focused your thesis on?

Peterkofsky: That's right, yeah, that was my thesis project. The sort of thing that I started doing was every day or so I would go to the local market and buy some spinach and then grind up the spinach and make spinach juice and then incubate the spinach juice with radioactive bicarbonate, which was a source of carbon dioxide, and then try to see what happens. I was hardly successful in getting much useful information on that basis. Eventually, I switched over to using green algae, which I got from another scientist in New York City. I set up an apparatus where I would bubble air through a big, long tube that stretched from the floor to the ceiling in the laboratory. That was illuminated by a bank of fluorescent lights where I could culture these algae and harvest them to have them break open and make extracts of algae to study that. I would carry out these experiments of exposing algae extracts to this radioactive carbon dioxide and look to see what radioactive products of that were. Based on the expectation from a knowledge of what was going on in the pentose phosphate cycle, the expectation was that the first product would be an addition of carbon dioxide, which was one carbon to a compound that had five carbons, that they should be a six-carbon initial product that would essentially be split to three carbon products in duplicate. I tried and tried to see if I could see that. Eventually, I failed, but what I did see was the formation of a four-carbon product, which was a big surprise. Even though it did not fit in with a preconceived notion of what was going on, it turned out that I had identified a four-carbon product of that initial fixation, which seemed to be very strange. Well, it turned out that a few years later, it was shown that there is an alternate pathway that was eventually described as the Hatch-Slack pathway for the formation of a four-carbon product that was never recognized at the time that I was studying. But that turned out to be the fundamentals of the PhD thesis that I submitted, so it was a rather interesting introduction to surprising findings to contest preconceived notions. It took many years later before the study of the actual products of photosynthesis—the addition of carbon dioxide to this ribulose diphosphate—was characterized and shown that

there was actually no intermediate because all of the reaction took place on the surface of the enzyme itself so that the products that were formed were a three-carbon product in duplicate from the addition of carbon dioxide to this five-carbon intermediate. So that was that.

The life at the Public Health Research Institute was interesting. There were two other graduate students that studied under the supervision of Efraim Racker. One of them was Harvey [S.] Penefsky, who became involved with this study of mitochondria and spent several years after his postdoctoral stuff with Racker; even when Racker left the Public Health Research Institute, Harvey Penefsky stayed with him. There was another student, his name was Dan Levin [Daniel H. Levin], who eventually became a scientist at MIT and had an interesting career as well. There was another important staff person at the Public Health Research Institute, who was a woman whose name was Sarah Ratner, who had done some fundamental scientific research using heavy isotopes at Columbia University, who came to be Racker's deputy at that biochemistry unit. She had two students, and understandably, those two students were women because she was interested in educating women like herself. They were named Olga Rochovansky and Barbara Petrack, and we all became very close. We had this very interesting kind of relationship. Dr. Ratner had been given the responsibility of setting up a laboratory, and she was responsible for creating a big, big central laboratory that housed the students that was called the student lab. The student's lab was distinguished from the laboratories that postdoctoral investigators used by the kind of furniture that was put in there. The student laboratory was furnished with hard wooden chairs, rather than cushioned chairs. I remember that so distinctly. Well, it turned out that there was this peculiar, conflicting incident where a new student came in to become a PhD candidate, but this guy already had a medical degree, so it was determined that even though he would be assigned to work in the student laboratory, he would be eligible to sit on a comfortable, padded chair. Life went on, and there was one day when, since I had become very friendly with this guy, whose name was Joe Alpers [Joseph B. Alpers], we were having a scientific conversation and in the course of this conversation, I sat down in this padded chair. Unfortunately, Dr. Ratner came into the laboratory and saw me sitting in a padded chair, and she immediately traipses down to Dr. Racker's office and reported this infraction of the rules. I had to get delivered to Dr. Racker's office and scolded that I was not allowed to sit in a padded chair. Well, this was one of the kinds of elements that you had to live with in those days. So that was that. One of the interesting guys that I met at the Public Health Research Institute was as a scientist from Israel. His name was Michael Schramm. He became a professor there. I became very friendly with him and had occasions to visit with him in Israel later on in life. That pretty much summarizes the situation there.

One of the important consequences of my being a graduate student who attended lectures at the NYU Medical School was that shortly after I became a student, I went to a lecture that was given by somebody whose name I forgot at this point, because it was all a blur. But what happened in that lecture was that I was sitting in the back of this lecture hall, and there was a woman sitting in the front of the lecture hall wearing a white lab coat. She turned around and looked at me and it was love at first sight. We began to have a secret, romantic relationship. She was working as a research assistant in the laboratory of one of the other members of the biochemistry department who was a very highly regarded guy—generally thought as the smartest guy in the whole department—who was very strict as well. After about six months, we decided that we would get married, and she revealed to her boss that we were thinking about getting married. It turns out that that her boss and Dr. Racker were involved with a weekly luncheon meeting every Saturday where they would go to a restaurant and

talk about science. Well, it turned out that on that particular Saturday, they didn't spend all their time talking about science, but Beverly—my wife's name was Beverly—came up in conversation where this guy, Dr. Gilvarg [Charles Gilvarg], revealed to Dr. Racker this news about Beverly and Alan thinking about getting married. Dr. Racker came back from that luncheon that Saturday where I was working hard in the laboratory, because it was required that you had to be in the laboratory on Saturday, and he said to me, “Charlie told me that Beverly is a very hard worker. Therefore, we decided that since Beverly is such a hard worker that she would not distract you from your studies, we decided to give you permission to get married.”

Higingbotham: They gave you permission?

Peterkofsky: That was the apparent situation, and I cannot absolutely imagine what might have been the case if they decided that they wouldn't give me permission to get married. That was basically the start of my married life. I will go on at another chapter of this to tell you about what happened in my future when I got my degree and left the Public Health Research Institute to come to the next point of my career at NIH.

Higingbotham: Sounds good. Why did y'all have to keep your relationship secret? Was it just by choice?

Peterkofsky: Do you mean my relationship with Dr. Racker?

Higingbotham: With Beverly who was your future wife. Why did you keep it secret to start with?

Peterkofsky: Well, of course, we were, we were immediately in love with each other. We have a common background in biochemistry, and, and she eventually—I'll tell you later—went on to get her graduate degree in Washington when we moved to NIH. She finished her work at NYU by stopping and getting a master's degree there and went on to get a PhD degree at George Washington University while I was a postdoc at NIH. She was at the same time doing the same kind of internship at the NIH, working at NIH and stuff, and getting coursework at George Washington University. Exactly the same kind of an arrangement that my close friend, Herbert Weissbach had done. She was another of the candidates that went on into that program, and I will fill in some more on that on a future occasion.