

Chapter 36

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Arlington, Virginia

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VM = Vivian Moses; PY = Peter Yankwich

VM: This is a discussion with Peter Yankwich in the NSF Building in Washington on July 30th, 1996. It's actually not Washington is it? It's...

PY: This is Arlington, Virginia; we're across the river.

VM: OK, fine. Can I ask you what your career had been before you ever went to Calvin and how you got there?

PY: I graduated from high school in Los Angeles in 1940, went to Berkeley as an undergraduate, got a Bachelor's degree in 1943. In 1944 I started work for the Manhattan Project in hopes of keeping myself out of the South Pacific and I took my PhD in 1945. Just before that, it became apparent that the war was going to be drawing to a conclusion and everybody in the Radiation Lab. was sort of scrambling around, looking for other opportunities. Even before the actual end of hostilities, Melvin had this idea of his and I had started my graduate work with Samuel Ruben.

VM: Had you? I hadn't realised that.

PY: Sam was killed in an unfortunate laboratory accident and I finished my degree with Gerhard Rollefson. To make a long story short, I was already in the Radiation Laboratory and Melvin had this idea that he was selling to Ernest Lawrence. After the end of hostilities, he began to set up this group. I was still working for the Project (*Manhattan Project*) on various things and one day he appeared in my lab. and asked me if I'd entertain a reassignment to this new group that he was starting up. I asked him "why?" and he said "well, we're going to be doing a lot of work with radioactive carbon and you're the only person around here who has had any experience."

VM: What had been your experience with radioactive carbon?

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PY: Well, in 1943, when I started my graduate work, I was doing carbon-11 work associated with one of the South Pacific gas cloud movement projects that Sam Ruben was working on at the time. After his death, a young instructor — my own faulty memory is going to kick in here — Tom Norris, who was working in the same building, we worked in what was called the “Rat House” at that time, was sort of keeping an eye on me and he suggested that I work with Gerhard Rollefson on almost anything I wanted to. But he, Norris, had an interesting idea. That was he knew several years before Ruben and Kamen had created a barrier made of stainless steel tanks filled with solutions of saturated ammonium nitrate. This was their carbon-14 factory. He wanted...he suggested that I analyse the effluvia of these tanks, or get anything I could out of them, to discover what chemical form the carbon-14 was in and perhaps that would be an interesting research project.

VM: And you were, of course, a chemist by training?

PY: Yes; I was a physical chemistry undergraduate. So, I started to work on this. The problem arose immediately on how you counted all this stuff. At that time, all we had available that would really work was a counting system that was developed by Willard Libby for very low-energy β -emitters. I can't even remember what we called it, but it was made of a couple of standard taper joints and from one end came a screen — the anode or the cathode — the screen...it was called a *screen-wall counter*, I remember now, a screen-walled counter and there was this screen that was one electrode and the wire ran down the middle, which was the other electrode. The sample tube went over this array so that the particles from the inner surface of the sample tube went through into the active volume of the counter. And as you can imagine, you had to create the counter fresh for every sample, which didn't lend much to the statistics. You tilted it one way and got the sample away from the active volume, and that gave you the background, and then you tilted the sample over, usually losing part of it on the way, and so on. I had counted carbon-11 this way for the gas project and then I developed the technique for counting carbon-14 this way. While this was going on, by accident we stumbled on the small plate stratagem for counting these things, taking an alcoholic suspension of barium carbonate in water and evaporating it under infrared lamps onto a thin aluminium disc and then counting it with an end-window counter.

VM: When you say you “stumbled upon”, you invented it or came across it?

PY: I don't claim to have invented it, believe me, because I am sure this was a well established technique but nobody had ever used it to solve the problem of counting barium carbonate containing carbon-14. So when I started work with Melvin, my task was to count the carbon. There was no laboratory available to us, so I spent the first few months in a little cubby-hole on the third floor of Gilman Laboratory under the eaves splitting mica...

VM: ...to make your end-window counters.

PY: ...to make end-window counters. We tried all sorts of designs for the end-window counter. We knew from much bitter experience what the spacing of the grid had to be

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and all sorts of things like that to prevent instant implosion. I got to be very skilful at splitting mica. I remember my personal record was 0.9 mg/cm^2 which I thought was astonishing. But anyhow, we prepared these end-window counters and while everything else was going on in the laboratory, we were building up the apparatus necessary to use them routinely in counting. We built the lead houses to our own design, we had all these brass slides in and out, so on and so forth. Eventually we ended up on the second or third floor of Donner Laboratory, ready to go.

VM: Did you develop all the corrections for infinite thickness and variable thickness or was that known already at the time?

PY: No, it was not known and I have a paper, I can't remember who's on me with it (*sic!*), in *Science*, of all places, on backscattering and its relation to counting. We got the infinite thickness thing, we developed curves so that you could tell where you were on this and, therefore, you could use a sample of any thickness and so on and so forth. Because in the early days, sometimes we were working with very, very small amounts of material. The corrections for backscattering and coincidence came from one of the mathematicians...we called him a mathematician; he actually was an astronomer who was working as a journeyman mathematician in the Radiation Lab. on any problem that came up. It was he who developed the backscattering corrections that we used and the coincidence stuff came out of an article by Truman Coleman who was then at Carnegie University.

VM: You mentioned, but I missed it. You mentioned that Calvin recruited you from a project you were already working on.

PY: Right.

VM: What was that project?

PY: Well, when I was a graduate student, I was operating under an edict of that gentleman whose portrait you see there, that's my father and he was a federal judge. He wanted very much for me to follow him into the law and I wasn't interested at all. So, when I started to get ready to go to school he laid down certain conditions. One was that when he knew I was going to go into chemistry, I should go either to Berkeley or to CalTech, that a bachelor's degree was a triviality and that I should not stop until I got a PhD. And that it was his task to support me while I was being educated and I was under no circumstances to take remunerative employment for that reason. So, there I was.

Well, when the war came along, and I was anxious not to get drafted, the purpose of employment was not remuneration, it was to proof oneself against conscription. I am very sympathetically inclined towards Bill Clinton, by the way, for obvious reasons. So I figured that working at the Radiation Laboratory would be about the best thing. Well, I was a chemist and a lot of us were chemists and you can imagine the kudos that is available to a chemist in an organisation that is run by physicists. E.O. Lawrence lumped us with the janitors and other staff when he thanked everybody for their glorious war efforts. But, he learned.

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So I became a shift supervisor, on rotating shifts — day, graveyard, swing — a horrible way to live in the analytical laboratory up on The Hill. The function of that laboratory was to prepare the samples for what we called α -counting, and it actually was mostly α -counting. What they did was, your colleagues from Britain were there in droves and they would do a run on the early calutrons and they would take these things out and they would take them down and every piece had a sample of the uranium that had spluttered all over the place sent to α for counting to see what the enrichment was and to find out where in the hell the stuff was going. Because when you take a mass spectrometer that normally works with submicrogram quantities of material and are building up to the point where you are trying to get a couple of hundred grams through it, you've got problems. So, that's what I was doing. I was supervising this laboratory.

VM: You also mentioned that you had worked with Sam Ruben. What were you doing with him?

PY: He was working on a defence project for, I guess, the Army. Bill Gwinn was a member of that project, Professor Giauque actually worked on that project for a while. Their task, as I understood it, and I never was very privy to useful information, was to devise a technique which would permit them to either predict, or if not to predict to then follow the motion of gas clouds in forests.

VM: You never worked with him on his photosynthesis work?

PY: No.

VM: Had Andy Benson gone by the time you were working with Sam?

PY: Yes. He was in spike camp.

VM: What about Martin Kamen, was he still there?

PY: Martin Kamen was sort of around but not around because this was the period of the witch hunt beginning. I talked with Martin only a couple of times and then mostly to find out what he could remember about the preparation of these tanks and other samples of high nitrogen concentration substances that I was analysing for my doctoral thesis.

VM: We talked to Martin a month or so ago. So, you were in Calvin's lab. then at the beginning of the carbon-14 activity which must have been at the end of 1945 or very early 1946. There were very few of you there at the time.

PY (*Jim*) Reid, myself, (*Bert*) Tolbert and Calvin were the group. The first addition to the group from the outside was Charlie Heidelberger and the second addition came from the inside and it was (I'm repressing his name. He's the other person who worked for Melvin whose father was a federal judge).

VM: Dick Lemmon.

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PY: Dick Lemmon. My father and Judge Lemmon were old friends. They thought it was kind of hilarious that we two should end up in the same room. Then Al Bassham came along and that was the very early group.

PY: You were never part then of the photosynthesis activity.

PY: Not really.

VM: How long did you stay there at that stage?

PY: I wanted to go into academic work and in the spring of '46 I had a long heart to heart talk with Wendell Latimer and he said "I'll tell you what we'll do. You are a little young (I was 22 at the time) and", he said, "we've got to season you a little bit, so I'll make you an instructor". So, I was an instructor in the Chemistry Department in the academic year '47-'48. During that year, I got an offer to go to the University of Illinois and all I knew was that Roger Adams was a famous organic chemist and I didn't really know what an organic chemist was. You know Branch and Calvin weren't exactly organic chemists. T.D. Stewart was sort of an organic chemist in the Berkeley mould. I remember asking Rollefson whether I should be seriously interested in this offer; I had another offer from the University of Washington and one from the University of Rochester. And he said "Peter, that offer from Illinois is the finest offer that one of our graduates has gotten in a decade. Take it." Ed King was in my class and he went on the same kind of advice to the University of Wisconsin. We had good advice. So I left in August of 1948.

VM: Having been there two and a bit years.

PY: I was there essentially for the first two and a half years of the laboratory's existence. During the last year of that I was teaching in the Chemistry Department a little bit of the time.

VM: Did you spend all your time developing the C^{14} chemistry and technology?

PY: Not directly. Melvin was very good to me because he knew of my interest in establishing an academic career of my own so he let me pick projects that were consistent with my interest provided they had some aspect that was of interest to him. The first one of these things had to do with beryllium nitride because it was a very concentrated nitrogen source. There had been a lot of work done on beryllium compounds for other reasons. It has a very, very low neutron cross-section; the beryllium has a very low neutron cross-section. I suggested, hey maybe we can get some really potent C^{14} barium carbonate out of beryllium nitride targets and, by dissolving beryllium nitride in various solvents, I can pursue my hot atom chemistry. So, he said "great". It was, you know, a mutual back scratching situation. He got his ultra-high concentration of C^{14} and I got more papers out on hot atom chemistry which was then my interest.

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Then John Otvos and a guy named Wagner out at Shell Development did some work in which they demonstrated that if you decarboxylated malonic acid, there was an isotope effect. Melvin was absolutely fascinated by this. He said “How would like to take a look at this?” So I said “love to”. I took a look at that and we did malonic acid, we did bromomalonic acid — which turned out to be a horrible disaster — and that started me on a career to which I devoted thirty years to kinetic isotope effects. I did some work on hot atom chemistry at Illinois but very quickly it became apparent that the isotope effect work was much more interesting.

VM: Did you see the beginning of photosynthesis in Calvin’s lab.?

PY: Oh yes. Because Al Bassham and Dick Lemmon and, indeed, Andy Benson had been working on that. Benson just sort of came and went. He was the best connection with Sam Ruben’s photosynthesis work. I was a very poor one because I never had been associated with it. Then, just before I left...not just; a while before I left, Andy came back. That was the start of the original heavyweight team on photosynthesis.

VM: From the standpoint that you had, working on isotopic carbon in Donner, were you party to the developments in photosynthesis even if you didn’t work on it? Was it something which was discussed generally in the group?

PY: Oh, yes. There were all sorts of group meetings that we had. There were some personality clashes in the Chemistry Department. I really don’t know what they were, but they had more to do with the internal politics of the Department than anything else. But Melvin tried various devices to get our research group sort of mingled with the other groups that were working, and vice versa, because he recognised the benefits of conversation and cross fertilisation as well as anybody did. Some of these were successful and many of them were not. People just weren’t taking his work very seriously and so on and so forth. The group itself, though, had all sorts of informal conversations. I don’t recall whether we really actually had a weekly research conference of our own although I’m fairly sure we did because Calvin used to invite people from all over the campus to come up and talk to us about things that he thought we ought to know about and not necessarily have anything to do with photosynthesis or intermediary metabolism, or anything like that.

VM: So he was a good, stimulating leader?

PY: Yes, absolutely, absolutely. And very catholic in his tolerance. I felt that I was able to do really anything I wanted to. My formal duties were exceedingly clear and kept to an absolute minimum and I was asked to bring to the group such additional expertise as I acquired but I was not pointed forcefully in particular directions in order to acquire it.

VM: In those days also, was there adequate funding for anything you needed to do? Was funding an issue?

PY: I was not aware of that. This was all handled by Melvin and it was an interaction between him and Ernest Lawrence.

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VM: You weren't enjoined to save money and be careful and things of that sort?

PY: No, not that I'm aware of. If we needed something, we got it. If we needed something built, it got built. The shops and the glass-fabricating facilities in Chemistry, in Physics and in the Radiation Laboratory itself were right there, there to be used.

VM: Socially, among the group at that early stage when there were just a few people, were you socialising between yourselves, out of hours, at the weekends?

PY: That was largely Tolbert's creation. Tolbert very early in the process became an administrator of the group and everybody was delighted to let him do it. He was very good at it. He thought we really should see each other outside the laboratory so there were picnics, and thing up in Tilden Park and so on and so forth, occasionally. The group did interact on the campus.

PY: You were all very young at the time.

PY: As I look back on it now, we were damn young!

VM: But largely unmarried, were you, and without domestic responsibilities?

PY: I was, I may have been the only one in that original group who was married. That was also one of my father's dicta: "Thou shalt not marry until thou hast thy PhD".

VM: Did you follow that?

PY: Oh yes. I got my PhD in June of 1945 and my wife and I were married on Bastille Day in 1945...

VM: Congratulations!

PY: ...and we're still married!

VM: You've just celebrated an anniversary.

The other question I wanted to ask you: did you see the dawn of the occupancy of ORL by Calvin's group? Did they take the building while you were there?

PY: Yes, they did. This was an interesting occurrence because as the need for the analytical laboratory had decreased, I began to get other assignments (*at the Rad. Lab.*) and one of them was to work with a man who was in the Chemistry Department at UC Davis who was down at Berkeley. He was working on a device to move, I think, uranium oxide along a tube so that it could be fluorinated and they were using what was then new in this kind of operation, it was one of these devices that just vibrated everything. You tuned it properly and the uranium oxide would go moving along. So we used to...this was all done in ORL; this was before I worked for Melvin. While that was going on, I had an assignment to Melvin's group; this was before he

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asked me to join the Bio-Organic Group. That was to use some of the small hoods in Gilman Laboratory and build an apparatus for synthesising some fluorinated compounds that he wanted. I designed the apparatus and it was all built out of stainless steel and so on and so forth, and I still have a place on my thumb where I burned off the top of a thumb with HF. I was working on this for a while and that's really where Calvin got to know me. It was after that, as that project began to wind down, that the Bio-Organic Group became crystallised and started up.

VM: And how does that relate to ORL?

PY: It doesn't relate to ORL. My work for Melvin was done in what later became his office in the Old Chemistry Building, the place with the fireplace. At the same time, I was working with this guy from Davis over in ORL. A few months later, when the Bio-Organic Group had been established, that space (*in ORL*) became available to Melvin. Here I was, back in space that I knew from a different effort, working on different things. This was about the same time that we were in Donner.

VM: I know, of course, that you have been in the round building because I have seen you on the movie on the 1989 reunion. You no doubt know the philosophy of that round building.

PY: Yes.

VM: What do you think? Successful do you think?

PY: I don't think one has to guess at its success. You just look at the output. It was completely consistent with the way Melvin ran his own science. It was lots of conversation possible, people being thrown together, a building laid out so that there were many intersections of trajectories and you couldn't get from here to there without running into somebody else.

VM: You liked it?

PY: That's a fairly interesting...I'm not saying I like that, but it was a very effective device.

VM: Have you maintained much contact with the Berkeley group?

PY: None. No, when I left Berkeley the people with whom I maintained contact over the years were a small group of people who, like myself, became its alumni and not necessarily of the group itself. I kept in touch with Dick Lemmon for a variety of reasons — our parents both being federal judges, Dick got interested in American Chemical Society affairs, he and I were both on the Board of Directors of the American Chemical Society at the same time. I kept in touch with Bert on a very occasional basis. I knew where Bert was and what he was doing. I lost track of Jim Reid after he came back here to the NIH and I wasn't aware until several years afterwards that he had passed away.

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VM: Melvin? Did you see Melvin?

PY: I saw Melvin occasionally. I would run into him, mostly at ACS meetings. You know, we'd have a five minute chat and that was it. One of the people with whom I kept in touch, I visited only once after he left Berkeley, and he has since been knighted, I guess, and that was Ted Abraham who became the head of the Sir William Dunn School of Pathology (*at Oxford*). Ted had arrived about a year before I left, I think, and we corresponded in a desultory fashion over the years. In the middle sixties I had occasion to go to Britain on my way to an international meeting in Dresden of all places and I walked by this place and I saw School of Pathology. I walked in and said "Is Professor Abraham here?" People sort of looked at me: "Why yes, he is." So I said "Could I see him?" And that was it.

VM: We wrote to him but he hadn't had a chance to reply before we left England. So I hope still to see him. What happened to you, you wanted an academic career and you got one?

PY: I wanted an academic career and I got one. I led a very simple life, a very simple life in comparison with some of my colleagues who became what I would call "academic vagabonds". That's a marvellous way of advancing one's career but I was extremely fortunate that I didn't have to do that. There came a point in my career where I accelerated its advancement by getting offers from here and there and so on and so forth, and I was fortunate in that the people who were successive heads of the Chemistry Department at Berkeley (*Editor: this presumably should be Illinois*) were always willing either to match the offers or sometimes meet them half way and sometimes go beyond them. I arrived in Urbana in August of 1948 and I left Urbana, not knowing I was leaving, in October of 1985 to come here. In the interim, I worked my way up through the ranks and I was not one of these people to whom research was the be-all and end-all. I kept the work on hot atom chemistry going for about five or six years, I did isotope effects for most of the rest of the time, and I never had a research group that was larger than four people, including one or two postdocs., and I never wanted more. I felt that with more I wouldn't know what was going on and I felt that I had to know what was going on if I was going to give guidance and train people and assist their education.

So my avocation became university politics and for many years I was chairman of the Committee on Committees of the Academic Senate, which is the kingmaker's role, and I was also the University's representative to the Illinois Board of Higher Education which was where the dirty politics in a system as complex as Illinois as we have five university systems in the state. I enjoyed that tremendously. In 1977 I joined (*Jack*) Corbalee, who was then president of the University. He wanted someone who came out of the faculty to help him solve his problems and I became Vice President for academic affairs, never having been a department head, a school head, a dean, a vice chancellor or anything. I went from professor to vice president, and it was more fun than a barrel of monkeys. Jack Corbalee left the presidency about three years later and his successor, Stanley Eichenberry, came into office and we

tried very hard to get along with each other and there were just a lot of ways in which we couldn't. So one day I said "I think it's time for me to go back to being a

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chemist”. He said “that isn’t the way I’ve got it set up”. By this time, John Corbalee had left to become the head of the MacArthur Foundation in Chicago. He said “how about your moving into Jack Corbalee’s chair in the School of Education, the College of Education?” I said “Stan, can you see anybody on this campus taking *me* seriously as a member of the faculty of the College of Education?” He said “oh”. So he said “I’ll talk to Ron Brady and maybe we can move the money around and put you back in chemistry”. So what happens? I go back to chemistry in the most unusual of all situations. I came on a line that was created for me in the budget. I was untouchable. I didn’t have to do anything. My wife used to say “Peter, you are not being paid to be a professor of chemistry. You are being paid to be not vice-president!” She was right. I had all through these years as vice-president — all five of them — I’d had postdocs. working with me, thanks first to the Atomic Energy Commission and later to NIH. When I went back, I decided I’ve got to get back up to speed in my research so that I know as much as the postdocs. know. I was really fooling them. I knew how to ask questions, you know. I’d had 35 years of asking tough questions and I could do that to cover a lot of things I didn’t know. The fact that they didn’t know them either was part of the game. But anyhow, so I went back to chemistry and I taught my classes and got myself all revved up in my research again and was enjoying life and fulfilling one of my lifelong obligations, which was to study Attic Greek. When I was in high school I took four years of Latin. My father was a superb linguist and I have always had an appreciation for language and I realised very early in my college career that there was a deficit in my mind, and possibly even in my character, that I had never given myself the opportunity to learn Greek. I always swore that whenever I retired, or had time, I would learn Greek. When I stepped down from being vice president was the perfect time. I went to the head of the Classics Department and I said “would you mind very much if I sat in on your beginning courses in Greek?” He said “absolutely not. You’ll be a good role model”. It was fun. I sat in Greek courses for three years.

Then an old colleague of mine from Chemistry was called down here to head the Education Directorate...

VM: “Here” I should say for the tape, is Washington.

PY: ...of the National Science Foundation. He came down in 1944 (*sic! 1984 seems more probable*) , this was (*indecipherable*) Shakashiri, who is an inorganic chemist. He was enjoying a very interesting career down here and he needed some political-administrative help. So he asked me to come down. I came down in 1985 for a year, stayed another year. After I had been here nearly three years, I figured, “look, I can do the world much more good staying right here than going back to Urbana and doing chemistry again”. So, I retired from the university and stayed here. That’s where I have been ever since.

VM: Last, last question: what did being in Calvin’s group do for you?

PY: What did it do for me in what way?

VM: Professionally.

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PY: Professionally, very little because I moved in circles where Calvin was known, especially later on as the person who had unriddled photosynthesis. But his research interests, even though he got his degree with George Glockler at Iowa...no, at Minnesota, I beg your pardon...Glockler was at Iowa, Melvin came from Minnesota...even though I had been associated with Melvin, I was not associated with him in the areas that were important to the career that I was building. Gerhard Rollefson as a sworn and acknowledged physical chemist was far better known to my colleagues at Urbana than Melvin was. No, my profit from the years with Melvin was that he gave me an invaluable opportunity to become seasoned. I could experiment, try out things in an absolutely fail-safe situation, I was part of a group that was made up of very bright, very gung-ho young people who were working on interesting things and, even though I wasn't working in the main currents of that work, I was aware of it, I saw the intellectual and professional activity that was necessary to keep it moving. I learned what you had to do to be a successful researcher and administrator of research. That has stood me in excellent stead, all of my life.

VM: That's a legacy worth having.

PY: Absolutely, and I'm very grateful for it.

VM: At this point it remains only for me to thank you because you have told me that you have to leave very shortly.

PY: I should leave shortly.

VM: Thanks indeed for your time and for telling us what you have.