

Chapter 9

NING GIN PON

Berkeley, California

May 29th, 1996

VM = Vivian Moses; NP = Ning Pon; SM = Sheila Moses

VM: This is May 29th, 1996, talking to Ning Pon in Berkeley.

So Ning, let's start with how you came to be part of this group in the first place.

NP: Initially, in 1954, I was working in Choh-Hao Li's lab., the Hormone Research Lab....

VM: Where was that? Here?

NP: Yes, down in Life Sciences Building, and there was a fellow by the name of Ian Harris, from Wales, actually. He had gone back to work at the MRC place, at that time the new Addenbrookes Hospital, I believe, with Francis Crick, Kendrew and those people. He said, "Oh, you shouldn't stay around, working for Choh-Hao Li; there's no future." (I had different ideas about this but he had said there's no future.) So, I said, "OK, fix me up to be going back to the College of Chemistry, from which I actually graduated as an undergraduate here."

VM: In what capacity were you working with Li?

NP: As a lab. tech, or maybe a senior lab. tech. by then. In any case, the project there looked really quite good because I had already three or four publications in the wind, actually, even though I may not be senior author; but still, it was very promising, I thought. But, nevertheless, Ian Harris said it would be more fruitful if I went back to Chemistry. Actually, I was doing chemistry in Choh-Hao Li's lab., doing protein-amino acid sequencing, that kind of stuff. Anyway, to make a long story short: somehow he arranged for me to interview Professor Calvin and I think, although I have never verified this, I think it was through Ed Bennett. Somehow the two, Ian Harris and Ed Bennett, must have talked about this guy (*Calvin*), that I was looking for someone to work for in Chemistry.

VM: As a graduate student?

NP: As a graduate student. Because I had been thinking of becoming a graduate student in some kind of comparative biochemistry with Choh-Hao Li. We did meet some way, somehow. I think Professor Calvin said, "Oh yes, we'll need someone like you because you have expertise in protein chemistry."

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VM: Had you known him (*Calvin*) when you were an undergraduate?

NP: Yes, I had met him. As a matter of fact he was a rather rotund sort of a guy then, I would say over 250 pounds probably, and somewhere during that time — I'm not sure at what stage — he became the president — what do you call it? — head of the Northern California Section of the ACS (*American Chemical Society*); they put out a little booklet called *Vortex*. He was there, presiding over a talk that John Gofman was about ready to give.

VM: John Gofman was in Donner at the time, wasn't he?

NP: He was in Donner. He said "I'm going to show you samples...pictures...ultracentrifuge pictures of lipoproteins. This was how, by the way, that the density of lipoproteins was first established. One talks about high-density lipoproteins, low-density... It's done by an analytical ultracentrifuge. The samples were actually from Calvin. Just about that time I think he was having a heart attack. I wasn't quite sure.

VM: These were Calvin's medical samples?

NP: Yeah, that's right! This guy (*Calvin*) is the primary source of the pictures I'm going to show you. I couldn't believe this.

VM: He said that openly, at this meeting?

NP: Yes, oh yes. I can't keep track of the chronology, somehow, when I eventually got to Calvin's group I asked him whether he had a Model E analytical ultracentrifuge. He said "Yes". As a matter of fact, he had, in the basement of this old building. "You mean the Old Chemistry Building?" "Yes." It was absolutely like a dungeon down there.

VM: Which old building are you referring to?

NP: The old chem. building.

VM: You don't mean the Old Radiation Lab.?

NP: No, the Old Radiation Lab. was an old wooden shack, whereas Calvin was in the old chem. building.

VM: Calvin had had his heart attack by then, hadn't he?

NP: That was my impression. I might have been mistaken about that. Somehow, he had had it. First of all, I got the feeling that when I knew him as an undergraduate — he was really time excited about undergraduate students. He would go into the lab. (you have to remember I was an undergraduate) and he would watch the students, he would be running a lab. course in organic chemistry, probably at the very lowest level, and he would look at the crystals: "Ah. What beautiful crystals!". He really related to undergraduates. In contrast, by the time you came, he couldn't stand them.

VM: He'd had enough, perhaps.

NP: I don't know whether or not it had anything to do with the fact that he was fat and rotund. Everyone sort of attributes the fact that jovial people are usually fat people or vice versa. I noticed that his personality had really changed considerably

VM: By the time you interviewed for the graduate student?

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NP: Yes, by the time I interviewed. No, I guess, he wasn't rotund and he had already had his heart attack. He said, "Oh, you have a background in protein chemistry and we could use a guy like you". I said "Oh". I didn't even know what photosynthesis was — by the way, I still don't, but that's beside the point. And I said "Well, what's this interesting thing?" And he said "Well..." He had already studied, in '54, by then Alex Wilson had gone through, so that must mean they had...which is called *Path XXI* — that is the paper that got the Nobel Prize, *Path XXI* in the JACS. Anyway, he said "Here's another avenue we have not looked at, what happened to the path of carbon in protein". So he brought up the idea that some woman in Russia, Boichenko (large woman), found out that actually CO₂ is assimilated much faster into proteins if you use blue light (I think I've gotten the story straight), whereas in red light CO₂ goes into the carbohydrate assimilation. You can correct me if I'm wrong; I think that's the case. So the next thing I knew I was down some theatre place looking for blue and red filters. They supplied the huge lanterns that you project on the stage, with different lights. I went down and got all these wonderful bluish filters, red filters, all kinds of things. Actually, I even ran the spectrum: just to be sure that we'd isolated the right colour we would put a copper sulphate solution of some kind just to narrow down the band, so to speak.

VM: Did you have a clear direction, a clear aim when you started as a graduate student?

NP: Oh, no. He was typically Calvin, or typically any research director (notwithstanding you: I don't know how you are!); that is, what he normally does is to spell out maybe three different projects and by then he tried to say, since you have an expertise in protein I would like for you to aim in that direction.

VM: And maybe you could do the other two as well!

NP: Well, actually not. He managed to slip in the others in the process but, in the course of all this, I actually did some work. I said to him, here's something very interesting, the assimilation of C¹⁴-labelled amino acids into *Chlorella* does not reflect the same specific activity and the activity might be in the protein once it is incorporated. You might have a pool of amino acids and yet, when this pool is assimilated you would expect that it would have the same specific activity as in (*protein*). It turned out that it wasn't that. I said that was interesting: does blue light affect it or red light affect?. It was at that stage, he (*Calvin*) said there's a guy by the name of Jacques Mayaudon who is working on carboxydismutase at the time

VM: He was here, in the lab.:

NP: Yeah. I remember him (*Jacques*) very distinctly because he managed somehow to put a rotor into the centrifuge without centring it and managed to destroy the centrifuge. We spent hours literally describing how to call this enzyme, by the way. In a special office with Andy Benson. "And what should we call it?"

VM: You are running ahead of me a bit. When you started working, you were working on the incorporation of hot CO₂ into proteins? With the different lightings?

NP: Not always hot CO₂, but hot CO₂, and hot amino acids. We have two different ways of getting the proteins into *Chlorella*. (*Editor: this must be in error — for "getting the proteins into Chlorella" read "getting the amino acid into Chlorella".*)

VM: In the typical photosynthesis lollipop type of set-up?

NP: Exactly.

VM: What was your analytical method for looking at incorporation into the protein?

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NP: You actually run chromatography, paper chromatography, see the way of (*?distribution*) the spots and we had a way of actually determining how much amino acid you had without looking at the radioactivity. We used fluorodinitrobenzene technique, the Sanger reagent.

VM: But the chromatography is for small molecules. How did you look for protein?

NP: First of all, we thought of the total protein. Then later we then simply hydrolysed that total protein without regard to what proteins were considering. Obviously, as the thing might have devolved, I might be looking at Fraction I protein, which is the carboxydismutase, which is the most prevalent. So why should you spend all your effort trying to look at small ones when the biggest one is there waiting for you to examine it.

VM: Where did you work when you first went into the Group?

NP: It was in the Bio-Organic Group, so that was in fact the shack.

VM: In ORL?

NP: In ORL, the Old Radiation Lab.

VM: Do you remember the room, the set-up?

NP: It was a huge room, with lots of benches. I am trying to think who might have been opposite me. It could have been Rod Quayle or Malcolm Thain — you know him?

VM: Oh yes. I know where he is and I've spoken to him.

NP: Is he still at Tropical Products?

VM: No, he's retired now.

NP: Retired? OK. So, anyway, they could have been opposite me. I vaguely remember probably being sandwiched between the hood, which was towards my back, and there I am, facing who might have been Malcolm Thain or Rod Quayle or these old-gang guys such as Clint Fuller.

VM: This was in the big room with the big white table.

NP: Yes. The big white table is our meeting place. Whenever any big official comes, everybody circles around there: Lord Todd came in, Khorana came in — they're all the Nobel prize winners, you know. The reason why I keep saying that is because I refuse to write my thesis, as you probably remember. Calvin was saying "Damn it, Ning,

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you've got enough for three theses and why don't you sit down and write your thesis?" "None of them look 'unique'", I said. Every time one of the Nobel Prize winners (*came through the lab.*), the first thing they would ask me: "Why don't you write your thesis?" That started all the way from Hevesy, whom I didn't know, by the way — I didn't know who he was or that he had won a Nobel Prize — to as far out as Szent-Györgyi. And all these people, they always said "Ning, write your thesis."

VM: And then you *did* write your thesis.

NP: Eventually yes, but only because, let's face it, I finally got together this paper that had to do with the effect of the activation of the enzyme with CO₂ and magnesium.

VM: Before I interrupted you, you were talking about Jacques Mayaudon.

NP: Yeah, OK. Jacques Mayaudon was the first guy, as far as I know, he may not have been the first guy but Malcolm Thain was working on it too, you know.

VM: On the enzyme?

NP: Yes, on the enzyme. I think Malcolm — I don't know if he told you...

VM: We haven't seen him yet.

NP: Well, he actually was purifying ribulose diphosphate, as it was called then, and he wanted to purify it by using brucine salts. I think that had to do with the fact that when you do that, if you crystallise it, you do it the proper way you can separate the two different isomers, apparently; I don't know why he had two different isomers because in the natural form there is only one isomer. So he must have done some synthetic thing about it. Whatever it was, you can talk to him; he might be remembering that.

In the meantime, they, Jacques Mayaudon, Clint Fuller, Al Bassham, all the big guns said "OK, we don't have ribulose diphosphate; you can't buy it". So, they simply took a lot of algae and kept running chromatography all the time and choosing this diphosphate spot on the chromatogram. You know where that is. Then you keep eluting, and you keep eluting tons of stuff, so we take the diphosphate region (*on the chromatogram*) and then we isolate from *Chlorella*, I think it was *Chlorella* they first used sonication to break it open and with a cell-free preparation and showed that cell-free preparation will convert that particular diphosphate in the presence of CO₂ to PGA.

VM: That's what they had done?

NP: Yes.

VM: Before you got involved?

NP: Yes. Again there was a paper published as a preliminary communication in JACS.

And Calvin says to me, "You know this guy (Jacques Mayaudon) is writing a full-size paper, probably in...an enzymology paper.

VM: There's *Enzymologia*, there could have been...

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NP: Could have been; it was that. “These Belgians would probably use a European journal.” But Calvin said he didn’t still quite trust (Mayaudon) and said “Why don’t you work on it? It would only be six months work.” (*Laughter*) Why do you smile?

VM: “Why don’t you do what”, did he say?

NP: Check on Jacques.

VM: In the sense of rerunning what he did?

NP: Essentially that and other things. At that moment, Jacques managed also to destroy the centrifuge and that really convinced Calvin. So he said to me “You’d better take charge, or something”. So, Jacques left and there I am, stuck with the problem. Because I had all intentions of returning back to the assimilation of C¹⁴ labelled things into proteins, because it was still getting kind of interesting.

VM: Before we lose that track: did you ever return to it?

NP: No.

VM: No; I thought perhaps not.

NP: I became a carboxydismutase expert and inherited that. But, mind you, I looked at it from the sense of serendipity that it was an interesting enzyme and I wasn’t an enzymologist at all.

VM: This change of direction for you was about ’55ish?

NP: I would say about that. I wasn’t really that long on the protein thing; it couldn’t have been more than half a year or a year at most. You have to remember that graduate students in chemistry, in spite of the fact that I was here to begin with as an undergraduate and took some graduate courses, I still had to take a lot of courses. So, to me there’s this battle of having to take oral qualifying exams, etc. At that stage, as a matter of fact, I kind of remember the College of Chemistry changed its policy. It used to be that the person who directed your research is the chairperson of the oral qualifying exam. But at that stage they said “no”, you can’t have that. So, Calvin headed off to Europe and there I was standing alone with no help; I had to fend for myself for the first time with this change in policy.

VM: You, of course, were formally Calvin’s student, he was your registered supervisor. In practice, presumably, you interacted with lots of people?

NP: My other professor, who was on the thesis committee, was P.K. Stumpf and Henry Rapoport. Neither of the two read the thesis (*laughter*); they simply signed it. This is the most ridiculous thesis I’ve ever written. I mean there’s only one. No one read it.

VM: Calvin didn’t read it either?

NP: I don’t think so.

VM: It could have been blank pages!

NP: Yes, it could have been blank pages! That was six years later. He figured by then, with four more publications in the wind, why should he bother reading it?

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VM: Back to your beginnings with carboxydismutase in '55. So what did you do?

NP: It turned out that Jacques Mayaudon had used as source material, in spite of the fact that the initial work involved *Chlorella* as the source material, the fact that you can't grow a lot of *Chlorella* to begin with and then you have to sonically oscillate it to break it open — Jacques, for whatever reason, decided...he saw a plant that must have been growing all over California, like a weed, and he decided that this looked like an interesting plant to use. It was *Tetragonia expansum* (New Zealand spinach) which really isn't a spinach, by the way, but still — I call it a California weed and one finds it all over the coast (Point Reyes, the top of any old cliff, along the shores). Tastes terrible. This plant was great for clearing carboxydismutase. The reason that you use that instead of *Spinacea* is because it probably grows year round; spinach doesn't do that, it tends to "bolt" in the summertime. I just followed that; I just followed through.

VM: Did you have to go to the cliff tops to get it?

NP: Oh no; you can go to the store and buy it anyway. It's sold as New Zealand spinach.

VM: As an edible crop?

NP: Oh yes.

VM: Even though you don't like it.

NP: Oh well, any time you work with it long enough, you wouldn't like, would you? If you worked with chickens, you would eat any chickens after a while!

VM: OK: so you started with New Zealand spinach?

NP: I used New Zealand spinach all the way through. When finally the thesis was put together, I think it even said that it was derived from New Zealand spinach. You see, Calvin maintained that you could write a thesis as long as it was from a different source. I said to him that a million other people were working on carboxydismutase (not a million: there's Horecker, Racker, etc.) but they use spinach, American spinach. I said that I didn't have anything unique about this New Zealand spinach other than it's from New Zealand. "That's it", he said; "that's good enough, it's a different source and therefore, you can write a thesis on it."

VM: Formally, he's right.

NP: Yes. I said that there was nothing exciting about it.

VM: Anyway, so you started. What did you do?

NP: There was no direction from Professor Calvin, actually; you know how he is. For one thing, even when you wanted to tell him something exciting, by the time you arranged an appointment and all that — he comes into the lab. and he sees the first person, right, and that first person is the one that's going to get the attention! (*Laughter*) The only way you could beat him is by being near the door, that's one way. The other way, if I recall correctly — and I was telling all the Germans [*in the lab.*] particularly like Hans Ullrich and Ullrich Heber who kept asking how in the blazes could you get to see this guy and have any fulfilment, scientific fulfilment? I told them that the way you do it is to make the appointment on such a day that no one is around, meaning either a weekend

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or late at night or early in the morning. I used all the different ways. I got to speak with him half an hour to one hour at a time, usually in the Old Chem. Building.

VM: So would you go over there with your stuff because there was a time when he would often come into the lab and sit down?

NP: Not on the weekends; he doesn't go to the lab. Occasionally he does. One time he came in just about Christmas, and he looked around and said "Where's everybody?" I said "Well, you know, I think this is a holiday!" (*Laughter*) Then he got upset because there was a Coke bottle somewhere. I knew that he had this antibody against Coke bottles just sitting there. Apparently he was very disturbed by that: there's a Coke bottle and it bothers him no end. He said "What is this doing here?" (*Laughter*) I said: "There are people who drink Coke and they like soda-pop and stuff." (*Laughter*) He said, "Where's everybody?" and I said "It's Christmas." (*More laughter*) And then he said "Why are you here". I told him that I had to change the solvent.

I remember even changing a solvent for you, come to think of it. One time you told me, and I got so horribly drunk and I said that I had to go to the lab. because Vivian's told me that I have to remove the big paper and I was sicker than hell because I had been drinking something with Jerry Crowley or somebody...

VM: That was much later.

NP: But you asked me: "Was I coming in the lab.?" and I said "Yeah".

VM: That was by the time I had become domesticated with children and I wasn't allowed out so much.

NP: I don't blame you just because I'm sick — someone smuggled in a bottle of wormwood type thing, absinthe I think. That is an alkaloid; I was sick for three days, "walking death" I think it is called.

VM: It didn't stop you doing experiments, did it?

NP: At that moment, it did. I was courting Lynn (*DuBois, later Ning's wife*) and she made me a very delicious dinner and I just couldn't even look at it from two miles away.

VM: But all of that is much later (*actually in the early 1960s*): you have jumped about. Please come back to '55ish.

NP: I can't remember 1955.

VM: Yes you can, that's when you started with the carboxydismutase.

NP: You asked me if there was any direction? There really isn't. It's like the ant that bumps into the wall, or something. I feel it was a random walk situation, you just keep bouncing over and once in a while you hit something, especially if you are already experienced in the lab..

VM: In that first period, when you started, were you interacting with other people, aside from Calvin?

NP: In truth, don't forget that at that time I am still taking things (*courses*) like thermodynamics and quantum chemistry.

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VM: But in the lab., with the postdocs at the time and the senior people and people like Al?

NP: There was Kazuo Shibata I remember very...and a few Israeli people, Yehuda Hirshberg and Ernest Fischer (but I think he came after Yehuda Hirshberg). Hirshberg as I recall had a heart problem.

SM: Yes: he even died.

VM: Who were the people in the lab. at the time who were experienced in enzymology? Anybody?

NP: Not in enzymology *per se* but they were experienced scientists, OK? So there'd be guys like Rod Quayle — he may not be an enzymologist but I felt that he was very experienced. And Malcolm Thain, although he was an organic chemist, still had a good intuition for it. Andy Benson; again, they were more in the higher level. Al Bassham, Clint Fuller, perhaps ...but other than that.

VM: So in the beginning there was no really experienced enzymologist in the lab. that you could interact with?

NP: It's hard to say that's the case. But, you see, those people had a paper on the cell-free preparation that did something and so to me they were experienced; I didn't have even that. At least I had some means of doing it. I think the thing was the how did I get — I have to ask the question — how did I get the ribulose diphosphate? I didn't isolate it from paper so I must have made it, too. Several papers were published by the Horecker-Racker bunch (two different groups) in which they used spinach extract to make ribulose diphosphate.

VM: You did that?

NP: So I did that. I didn't do it initially, I think Malcolm (*Thain*) did or Rod Quayle did. That's why Malcolm was doing this brucine salt crystallisation thing.

VM: Rod didn't mention that but he was certainly working on the problem.

NP: I think these two guys, probably, were essentially the source of the ribulose diphosphate that I started with. Later on, because you had to do it, you just simply followed articles.

VM: Talking of these two guys (*Quayle and Thain*) and the famous picture of the four (*Editor: actually five of them*) of them with the deerstalker hats (*Calvin, Quayle, Thain, Fuller and Rich Norris*), why weren't you in that picture with the deerstalker hats?

NP: Because I was just a graduate student.

VM: You weren't exalted enough to have a hat!

NP: Malcolm Thain, Rod Quayle, Clint Fuller and Bassham. Right?

VM: No, Melvin; I think there were only four of them.

NP: You sure?

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VM: Yes. We'll look at the picture some time and check it.

NP: I remember the deerstalker hats. Someone went to a great effort to get the...

SM: It was Rod.

NP: It was Rod?

VM: Yes.

NP: He was at Leeds, wasn't he?

VM: He later went to Sheffield but he hadn't been there then. He told us the story (*of how he got the hats*). OK; so you were working away...

NP: See, all I saw was this interesting group of people. There was always Clint Fuller arguing with Al Bassham. Al Bassham is a staunch Republican and Clint Fuller was a staunch Democrat. They were arguing all the time, literally, by where Alice Smith, the dishwasher was. You have to remember this group of people, you had a dishwasher, you had a guy by the name of...there was a glassblower.

VM: I can remember the glassblower Bill Hart, but he came later.

NP: I think there was a different glassblower who did beautiful animals and little glass things, and even a machinist of some kind.

VM: And there was the carpenter, Ralph Norman.

NP: Yeah, right, I'd forgotten about him. We had a whole little group of people that was equivalent to the lab. that was up on The Hill, that could do all that sort of thing. When you can't do it all, you walk across this kind of a lobby-like thing and there's a big machine shop (*in ORL*) that apparently which made things for the whole lab.; I'm talking about The Hill as well. And then we had this dungeon where the counting was going on; that's where I first met Albert Szent-Györgyi, by the way.

VM: In that counting room in ORL, underneath the building?

NP: I was counting something and Calvin says I want you to meet some guy. And the guy says "Why don't you write your thesis?". And I said to myself "Who is this guy?" This is Szent-Györgyi.

VM: You should have been on your knees.

NP: I should have been bowing to him several times. He was the guy that I recall: wasn't he ascorbic acid? Yeah, he got the Nobel Prize for ascorbic acid. Somebody had asked him, or I don't know whether he told me the story, he told me several stories in the presence of Calvin...(indecipherable)...how Warburg treated him.

VM: How did Warburg treat him?

NP: There are apparently many different echelons and when you are the lowest echelon you are in the basement. All that sort of thing. But, in the meantime, somewhere as time went along, he finally found this ascorbic acid and someone asked him "Where was this?". He thought it was a sugar and he would call it "Godnose". All sugars have to end in -ose, right?, so "Godnose" is probably the best name.

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VM: Before too long, however, a real enzymologist came into the lab. in '56, Bob Rabin.

NP: you mean.

VM: Yeah, "Let's face it". (*Editor: Rabin was well-known for this expression*)

NP: I don't know how he came into the project.

VM: He came into the lab. as a Rockefeller Fellow...

NP: I thought you came at the same time.

VM: ...and we came at the same time. He was an enzymologist and I wasn't. Did you begin to work with him pretty soon?

NP: I thought it was almost immediately. That seems to me very strange because he had the choice of doing almost anything himself. I guess he must have interviewed me in some way.

VM: We can go back to Bob and ask him about this.

SM: He said right from the very beginning he worked with Ning and he enjoyed the collaboration.

NP: He was a fun guy, actually.

VM: I think he probably saw it as an enzymological problem in which he...

NP: You will recall... Of course, there were all these things going on with the Germans and the Germans were so horrible, I think Mr. Kandler particularly, that I remember. I don't know, if that ever came up in conversation with any of these guys you talked to, but I talked to Calvin. I knew he was going to talk to Kandler about this article he was going to publish in the *Archives of Biochemistry and Biophysics*, negating all this PGA stuff. Calvin just muttered to me "I will never have another German in my lab.". (*Laughter*) (*Indecipherable*) ...he's referring to Kandler. It was my understanding that Kandler was not even going to give him an acknowledgement that he was going to work in that lab.

VM: That I don't know. We have to talk to Kandler and maybe find out.

NP: I might look it up in the *Archives*; probably it has an acknowledgement.

VM: But it was in *Archives of Biochemistry*, was it?

NP: Yes, that's where I thought it was. And then there was this famous paper that "Let's face it" and myself and you...

VM: I'm not sure that I was part of it.

NP: Hamamelonic acid?

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VM: No, I don't think I was on that one. So what did you do with Bob (*Rabin*)?

NP: What did I do with Bob?

VM: You were a graduate student and you were presumably having to keep an eye on your thesis and what was going to go into your thesis.

NP: I just went along and assumed it was all going to be in my thesis. I didn't ask Bob whether it was going to be allowed or not. I figured that I was in charge.

VM: OK. So, what did you and Bob start working on?

NP: There seems to be an interim here when Bob hadn't come, before you arrived.

VM: Bob and I came at almost exactly the same time.

NP: I don't mean that. There's an interim from my enzymological experience. I am trying to think — Power Sogo, have you met him?

VM: Yes, I remember him.

NP: Calvin had apparently been going around the country, in his usual fashion, visiting different labs, and at Washington University he bumped into Barry Commoner. He also bumped into...maybe it was in conjunction with Barry Commoner, a guy named Wiessman or Epstein — somebody who was a physicist or a geologist; apparently he designed an electron spin resonance apparatus.

You see, at that point, there was no such thing available. There were NMRs there but the NMR experience is a different matter, by the way: I could tell you a few stories about on one, too.

VM: We have time — we have to hear them.

NP: Well, anyway, the ESR. He came back and said "Oh, Barry Commoner is doing this and that on mitochondria" or something. He said "It could be great because we'll have the same kinds of free radicals or unpaired electron spins in photosynthetic systems". Of course, Barry Commoner was also thinking about that too, so here's this hurry-up thing about getting one (*an ESR apparatus*). So he thought that Barry Commoner felt that he had the sole access to an electron spin resonance apparatus. Calvin in the meantime, I heard, went over to Physics, to Professor Alan Portis, and asked who do you have who could build a thing like an electron spin resonance apparatus. He said "Power Sogo", because apparently he had built an NMR machine. So he came in and sat down near where the ESR machine was —he sat there and night and day was poring through the tube manuals, in those days we used things like electronic vacuum tubes, so he was poring away and he was designing things and it was amazing: I have never seen anybody just sitting there and using a tube manual and working out a whole circuitry. Next thing I know, he has the old soldering iron and is ordering all kinds of things from Oakland, wherever that might be: 3rd Street, down there. Things are rolling in, and pretty soon there is this instrument. Calvin said "You've got some stuff that should go through this machine". I said "Well, I could make some chloroplast stuff". So we did that and we had the first paper on the chloroplasts, electron spin...with my name of all things on it!

VM: Who did you publish that paper with, do you remember?

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NP: Power Sogo, myself and Calvin. The three of us I think; perhaps there was a fourth one.

VM: That was before Bob (*Rabin*) came, was it?

NP: I think so. (*Indecipherable*) is interesting, too: how that came to be was that Calvin says “I have now worked out the path of carbon in photosynthesis. Now we would like to look at the path of oxygen in photosynthesis”. We thought the guy went crazy, right there. We said how is he going to do that? Oxygen-17 has a nuclear moment with a spin. I said “What are you going to do? Are you going to grow big orange trees/”, because it required that much source material to spot oxygen-17 in this apparatus. First of all, you have to get this machine. Apparently Varian or...Was it called Varian?

VM: I think it was Varian at that time but I’m not absolutely sure.

NP: They already made a nuclear magnetic resonance apparatus, a huge thing, and had so much power requirement that literally I think they were talking about having a substation, some kind of electrical substation, because when you turn it on all the lights in Berkeley might dim just a bit! (*Laughter*) The amount of power that must have gone through it was so immense that it heated up everything so that the wiring was really tubing with water flowing through it to keep it cool. It was the most amazing thing. Of course, the magnet was huge, it was at least 12 inches in diameter, and was actually optically flat, the variation, the difference between (*the flat part to*) the unflat part was 1 part in 10^8 or something like that. It was just like polishing a mirror for the Hubble telescope. The weight of it was immense.

How did he first get the money for it? I wasn’t really involved but there was this guy named Rick LaForce: you remember him at all?

VM: I remember the name.

NP: Well, he himself has a story, this guy — first of all he was involved with a horrible triangle, two males with one female and there is poisoning of somebody at Cal Tech — I remember reading this in the paper. He was an ex-con; he was not supposed to do all kind of weird things. So he was involved in all that kind of stuff. That aside, he was a brilliant physicist, nevertheless. Calvin got him from Cal Tech to work on a certain project related to photosynthesis, the photoactivated separation of charge, so he was supposed to work on that. But he (*LaForce*) made films of things on which you can generate photoactivated separation of charge. He was the spokesperson, if you will, to convince Ernest Orlando Lawrence to supply the money to buy this big machine. They went into this one room and there was this guy Rick LaForce, Calvin, and I think Al Bassham was there and Andy Benson; could have been Clint Fuller too, I don’t remember the whole crew, I was kind of an outsider at that time. The only reason why I know about some of these things is because Rick LaForce was always stealing my stuff. He worked by night; he stole my things.

VM: Did you work in the daytime then?

NP: Yeah. I knew that he stole it so I would always go back, I think it was upstairs or some place, and I’d steal it back. We just kept borrowing but we understood each other! Anyway, somehow they managed to convince Ernest Orlando Lawrence that it was worth doing this path of oxygen in photosynthesis. That’s how we ended up with this big NMR machine.

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VM: Was Power (*Sogo*) running it?

NP: I am trying to think who...

VM: There was also a girl called Mary Singleton. (*Editor: She is now the president of the local section of the Northern California Section of the American Chemical Society .)* Didn't she work...?

NP: No, she worked with Gaylord Androes.

VM: That was later. But he was an NMR...

NP: I'm trying to think of all these people. Let me see. I know Power was involved — maybe he did run it, come to think of it. He may have been the very first NMR guy (*in the lab.*). Then you had Gaylord Androes and Ron Ruby.

VM: Eventually there was Mel Klein but that was much later.

NP: That's a crew in itself. There was also Erminio Lombardi, the big opera fan (*who worked in the NMR lab.*). He, in fact, was a spear carrier for La Scala — that's what he said and I believed him — a supernumerary, and he used to sing in (*the lab.*). It was like being in the shower. When your sample goes into the NMR machine to make the thing more homogeneous you spin the sample with an air blast supplied by a compressor, so Erminio liked the sound, it was like being in a shower, so he would sing every aria that Verdi could have put out, even the female parts, he would sing from the Barber of Seville "uno voce poco fa".

VM: I don't remember Erminio singing, but if I may briefly tell a story: We once went on a trip to the south-west with him and with Luise Stange, and on various occasions we all slept in the same room in the motel, and he snored. If you woke him up in the night and said "stop snoring", he didn't understand you, because he didn't speak English very well. He told us to say "girate?", and he would roll over. He would wake up in the middle of the night, we would shake him and say "girate" and he would spin over...

NP: ...like the NMR machine. What happened to Luise Stange?

VM: We are in touch with her.

NP: She ever married?

VM: No.

NP: She was such a nice woman.

VM: She still is.

NP: I loved her. Tell he I loved her.

VM: That's fine. She'll be delighted. So this NMR machine...

NP: So I've gone on a tangent again as usual.

VM: No, that's fine.

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NP: Well anyway; so the NMR was acquired. Of course we didn't have enough material to put oxygen-17...You see, oxygen-17-labelled water is not that available and the only place it could have been available either was Norway or Israel...

VM: ...in the Weizmann (*Institute*).

NP: In fact, I think, we were aiming at the Weizmann but was simply enriched, it was not even that pure. If you could put pure O¹⁷-labelled water in it, that would probably be enough, but if you have to look at some substrate that contained it, it would be impossible. You couldn't build a (*farm?*) big enough to jam everything into this machine. It really was, to me, a means of acquiring a nice beautiful machine, not the thing that was originally intended. It couldn't do it anyway.

VM: Were you involved later on in the oxygen stuff, in the O¹⁸ and the fluorine-18?

NP: No.

VM: Do you remember that it went on?

NP: No.

VM: The business of using O¹⁸ as a tracer and then bombarding it, do you remember that they had a tantalum strip, and that bombarded it in the cyclotron with protons and there was a woman called Ignored Fineman-Fogelström - (*Editor: this may have been Fogelström-Fineman*), I think, from Sweden who did that? You don't remember?

NP: No. The name impresses me but on the other hand I do not remember her.

VM: I can vaguely remember what she looked like. But you weren't involved in that?

NP: No.

VM: Did you do much in terms of NMR?

NP: No, I only had that one ESR paper. From there I figured that if anything is needed let Power ask me. At that point he had so many demands anyway...of other things that needed...

(*Brief discussion of ESR*)

VM: When you were working with Bob (*Rabin*), what were you doing with Bob, what was your objective?

NP: I told Bob that I had discovered that if you treat carboxydismutase with these different chemicals, or with CO₂ and magnesium, and what activated it, we decided to make a more methodical, more systematic analysis of this question. We really didn't do such a great job, in retrospect, in spite of the fact that at the time we thought it was a great discovery.

VM: In what sense don't you think it was a good job?

NP: There still was the question that Otto Kandler already pulled out a few reservations about PGA being the primary product, etc. The question he would pose to you, if this

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carboxydismutase was really the CO₂ fixation enzyme, all these lovely plants and trees are growing on, how is it that it works so slowly?

You remember that Otto worked with Martin Gibbs so it's the Kandler-Gibbs antagonism, if you will, of why this or that. If you will allow me, we'll go back just a bit. The first question, aside from the fact that the enzyme is not capable of doing the thing that a real live plant can do, I think they were first considering the fact that if you give CO₂ to a plant, then if you follow through the way Calvin and Sam (Aronoff) and all these guys Benson, etc. said if you if you cleave it (*i.e. the hexose*) in half with the *Lactobacillus casei* enzyme, or something, you can then consider the top half the same as the bottom half. I don't know if you remember what happened there. It turns out that the top half was asymmetrically labelled compared with the bottom half. Subsequently, we had explanations for all of that. That was the beginning of the questioning of whether or not 3-phosphoglyceric acid was the primary product.

Then, they started to zeroing on the enzyme I was working on. Now, I didn't feel that I needed to protect it. There's an enzyme sitting in a test tube, it works OK, it didn't quite account for why a plant worked that well, sitting there you would require so much carbon dioxide to make this plant work in that test tube. You take 5% CO₂ whereas atmospheric CO₂ is 0.03%. Right then and there it tells you that it won't work very well in 0.03%. On top of which there's a thing called "the K_M", the binding constant (that's really a misnomer; let's just call it the K_M), and the K_M for CO₂ in the test of *in vitro* is of the order of 0.001 M (at least, of bicarbonate) whereas in the plant it is of the order of micromolar. So then and there it tells you that this enzyme (*carboxydismutase*) in the test tube is not the same as in the plant.

VM: Sure, but that's a common biochemical problem.

NP: Yes, well. At that time, in spite of the fact that I had to work with this enzyme for a long time, I did not regard myself as an enzymologist. I didn't have really...occasionally I did ask P. K. Stumpf (he is an enzymologist) why do we have...? Well, they are different when you pull something out of the environment. You remember we had papers together. We had a question about this thing, too.

VM: Exactly, that was about...

NP: Even then, it was slow.

VM: You remember? We tried to pull the spinach apart...

NP: That made a kind of magnificent paper, frankly.

VM: A small story with that. It was actually the first paper ever submitted to the *Journal of Molecular Biology*, and I wanted it on page 1 of volume 1. But Paul Doty, whose paper was dated later than ours, actually got the first slot. I think we didn't appear before page 21, but we were the second paper in the first issue.

NP: Because of the way we could not make the connection between the enzyme that is in the test tube and the enzyme in the plant, that's when Kandler said, "well you know, there's something not quite right about the whole thing".

VM: What did you know about that enzyme in the early days, in terms of what it did?

NP: Other than the fact that it converted something to something else, do you mean?

VM: Did you know what the substrate and the products were?

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NP: Yes. We knew that. Malcolm Thain had supplied me with the substrate. It was a way of checking out what the path was.

VM: That was clean?

NP: Yes, that was clean, even though the substrate wasn't that pure, the product was certainly quite clear. There was no doubt about that.

VM: Did you know much of the purity of the enzyme itself?

NP: In a sense. That's when I approached Calvin to ask whether — you see, I think at that point Jacques Mayaudon had somehow run an ultracentrifuge; oh, he had Schachman do it. He didn't know how to do it — he let Schachman do it. Somebody in Schachman's lab., or somebody in the Donner, you see the Donner Group had ultracentrifuges.

VM: This was Howard Schachman who was in what was then the Biochemistry Department...

NP: ...and who is now in Stanley Hall.

VM: But then it was the Biochemistry Department, before Molecular Biology had been formed as a department.

NP: That's true; that has a history in itself. I think someone died of something that was like the Ebola (*virus*) over there. I wouldn't say it was Ebola but there was someone during my time, either when I was working with Calvin or just preceding that. Someone had died on the 4th floor of Stanley Hall. No one wanted to say much about it. They clamped down access to the 4th floor or higher. I found out later that in fact this guy was messing around with some kind of a bone, skull, non human primate.

VM: But that doesn't really impact on our story!

NP: No, it doesn't. It just impacts on mine! Anyway, the Kandler story. I don't know how — oh, he decided to come and show Calvin what was wrong. That's it..

VM: As I understood, it, but this will depend on what other people say. Kandler was with Gibbs on a Rockefeller (*Fellowship*) for a year, and after six months either Gibbs or the Rockefeller people decided it would be a good idea if Kandler came to Berkeley and help resolve this problem.

NP: Yes. I think that's the way it's put!

VM: That's the way it's put. We'll see what the others say.

NP: The three Germans, the Metzners, the Simons and the Kanders were all (*in the lab.*) at about the same time.

VM: Absolutely, all at the same time.

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NP: One was a *Schutzstaffel* guy, wasn't he?, because he claimed to have known one Heinrich Himmler or was it Hermann Goering? No, Heinrich Himmler; that's a story in itself, my God!

SM: Simon was the good guy.

NP: Simon was the good guy. Actually, Kandler was kind of interesting, too. He had a Henry J' you remember that? He had a Henry J car. The Henry J's got to be the smallest American car at the time and this massive person went in ... It consumed a huge amount of oil. They were discussing ("they" referring to Simon and Kandler) putting a 55 gallon oil drum as supply with a drip feed to have a constant supply. Those were the days when oil was cheap. I could just see them. He'd put oil into the tank (he didn't even bother to put it where you'd normally put oil); he'd put it in the tank and shake it!!

VM: I remember there were various magic mixtures that you poured into cars in those days

NP: STP...

VM: X7 and things that were supposed seal...

NP: No telling what BP came up with.

NP: That was an interesting crop. They all spoke German. And that was most interesting. I said this bothers me.

VM: Did you speak German?

NP: No, it bothered me. So I had my friend from the Hormone Research Lab. come visit me. I spoke Chinese with those guys. They told me right away that this doesn't sound right. It didn't cure them completely but momentarily, it seemed to have done something; There were all these different people there at the time, so I can't quite get the chronology working. There was also Chris van Sumere; now when did he come?

VM: He was also there at about that time, in about '56. Bob and I came in '56, Chris came then, Simon, Metzner and Kandler came part-way through that year. As I remember, it was the year Al was in Oxford and remember Al had a glassed-in office in one corner of the big lab., and throughout that six months Calvin and Kandler were to be seen arguing in that office. You couldn't often hear what they were saying there was lots of noise. And furious scribbling on the blackboard.

NP: Bob ("Uncle Bob") managed to resolve the problem with the hamamelonic acid thing.

VM: Tell me more.

NP: Bob knows more carbohydrate chemistry than I or, at least, he knew enough to know someone who knew more, guys like Robin Ferrier or someone...

VM: Robin Ferrier was much later.

NP: OK, someone. Then we realised that when you add cyanide to carbonyls that one forms a cyanohydrin addition product. From there came the story that immediately negated Kandler's famous *Archives (of Biochemistry)* paper.

VM: Had they been using cyanide to kill the plants?

NP: Radioactive cyanide.

VM: I see and then, of course, it gave completely the wrong picture. There was also a problem (but I don't remember; maybe you do) with what Metzner had done?

NP: I lost touch with what exactly Metzner did. Was it something about cooling?

VM: It was something about using hot methanol and getting methyl phosphate and this was an artefact of something.

NP: No, no; that was with Inia Tyszkiewicz.

VM: Was it now?

NP: Yeah. That paper with Inia and Ulrich Heber...his wife was Meta?

VM: It might be; I'll look it up.

NP: ...Because we had a paper with them, the one where they isolate under non-aqueous conditions chloroplasts....

VM: I don't remember.

SM: There's an interesting Germanic construction to that sentence. (*Laughter*)

NP: So that one — they were going to put my name on it. I said that I didn't want it, not because I suspected it. It's just that I hadn't done any of the work. They said "Well you're a good friend" and I said "But friends...(indecipherable)...really scientifically associated. Then Calvin came up to me and said "Why did you take your name off it; did you suspect there's something wrong? I said "No, I just didn't contribute". It turned out that the cold methanol killing and that got phosphorylated with P³² and that was the one with Inia's name on it, a PNAS article. I think Metzner had something to do with the way you killed the plants: you get a different CO₂ product other than PGA. Isn't that right? We) had suspected("we" referring to "Uncle Bob" [Rabin] and myself) that it was an active form of CO₂ that he's captured but I don't think it has anything to do with this hamamelonic acid.

VM: Another person who was there at the time, and I can't remember whether he worked with you, was Duncan Shaw.

NP: He didn't work with me. As a matter of fact, he was one of these outsiders, like Ulrich Heber, I think. There's the other Hans; wasn't that Hans Heber, too?

VM: There was Hans Ullrich and Ulrich Heber.

NP: Ulrich Heber, you're right. That's when I was approached: "How do you got Calvin to pay any attention to you?" I said "Well, the first thing you notice is that the guy (*Calvin*) talks to anybody that he sees first in the lab. So," I elaborated, "if you see him at the weekend, that would be fine". But they were all in the meantime gallivanting around the Sierras on the weekends. So I said "in that case, you're out of luck".

VM: You were not a Sierra gallivanter?

NP: I was a Sierra gallivanter but I chose to do it in the right time.

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SM: What does that mean?

NP: Well, when I don't need to see him.

SM: You didn't join in on these lab. trips, then?

NP: At first, I didn't. I said why should I go with this bunch of guys I see everyday, barbecuing who knows what, so I went with one Karl Lonberg-Holm. He and I were somewhat loners, and we said we can go out there but we don't have to stay with those guys. They are packing away 30 pounds or more and I said "what am I doing here?" I had been in the Army, and I said to myself that I would never put a pack on my back again!! And they were carrying packs bigger than what I had in the Army. Karl said "don't worry, we'll go out to some place" and the next thing I knew I was out in Lassen National Park, as far away from the rest of the group as we could.

VM: Not talking about photosynthetic matters?

NP: Or anything. Not having the same camp songs and what have you. There we were by ourselves.

SM: And they used to sing camp songs?

NP: I have no idea, I wasn't part of that.

VM: There were good social occasions, weren't there?

NP: There were probably good occasions, anyway. I wanted to be a semi-hermit, that's all. That was my own choosing.

VM: I don't just mean the mountain social occasions but here in Berkeley.

NP: We had all kinds of nice things.

VM: You were always in on those things, weren't you?

NP: It was almost inescapable. If Calvin says you go, you go.

VM: It was not unpleasant.

NP: No — I don't deny that.

VM: When did you turn from being a morning bird to a night bird?

NP: When I returned from retirement.

VM: Oh, I see. Come on. I recall....

NP: I wasn't a night bird then, was I?

VM: Oh yes, you were. I remember by the time we got into LSB and you were in that room with Naomi Levy, down in the corner...

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NP: Oh! Naomi Levy, yes.

VM: ...you were there half the night playing records.

NP: That was home for me. I even had a liquor cabinet! I offered Calvin a drink, and he said "Oh, what are you doing?" and I said "I'm having a little drink. So, would you like to have some?" "Oh, no thank you" and then he pointed to one of those high stools and I had a dime, Scotch-taped there. He said "What's that?" "That's the dime you keep telling me to get off of." So he sat down on it.

VM: "Get off the dime" was one of his expressions" wasn't it? By 1960-61 you were a night owl in that room down in LSB.

NP: I didn't know any females...

VM: So you stayed in ORL until the whole group had to move out and go to LSB?

NP: I didn't even realise that I was here when you dug the first hole here. I was talking to Marie Alberti and we looked at the dates, because she had access to all these pictures. So, I said "I was still here; How come you're not in the picture?" She said "you and I were the only ones that were working; all the rest were digging holes in the ground".

VM: Bob was only here for a year at that point and then he left in '57. What did you do after that? Did you continue working on the problem?

NP: I believe so. We did all these other things, for example, I worked with Rod Park.

VM: That was later. That was already in the in the sixties.

NP: I think it must be with Ulrich Heber's stuff. And you left about the same time?

VM: I left a year after Bob. We did a paper with Al and Ozzie, I think.

NP: A multiple author thing. I must have done something, but I don't remember now.

VM: When did you actually finish your thesis?

NP: '60.

VM: But you didn't leave then, did you?

NP: Oh, no. Calvin says...I had used the argument and said "Look..." He said "Why don't you write your thesis?" (*Indecipherable*) If I don't write my thesis he could just pay me the graduate student wages, which is half that of a postdoc. He said "Oh God; what can I do?" He knew that I had an argument against everything that he wanted me eventually to get this thing done. Finally I became a postdoc. and he said "You need to be an investigator" or something. There was a clearance requirement. Were you...?

VM: When I became a citizen I had to sign some papers.

NP: I always thought I had it when I was a graduate student but I guess not because he said that he had to get me a Q clearance. So he picked up the phone and called up The Hill and said "Can this guy be a postdoc right now, even though he doesn't have a

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clearance?” They said “what is his name?”. And whoever he’s talking to says “he’s cleared”. I said “What? How is this possible?” Then I thought about it. My brother, he had been working on The Hill, he had a QBW clearance.

VM: What’s a QBW? Very good?

NP: Super high. My brother asked me questions and I said “You are not by any chance working on the trigger of the hydrogen bomb, are you?” He just muttered and walked away. Later on he said “I have to head off to the Pacific” (I was living with my brother at the time) and I said “what for?” He was going to Eniwetok and Bikini. So he was checking out the trigger. So, I was cleared due to the fact that he was working on some supersecret thing. Funny stories arose at the time he was getting his clearance. I heard one of the foreigners — to take the stamps that were in Alice Holtham’s thing (*desk?*) and one said SECRET, or something, and he stamped his notebook. Were you there at the time?

VM: No. But go on, go on: tell me the story.

NP: There was a police guy that comes around (*through the lab.*) every (*night?*) and he saw this book. This guy couldn’t get his notebook back.

VM: That was Hans Kornberg.

NP: Hans Kornberg? Oh my God. I thought it was some other book.

VM: Hans tells that story and we’re going to see him on the way back.

NP: I thought it was Alex Wilson. So it was Hans Kornberg. It was a foreign guy. At least that seems to fit some pattern. It wasn’t hearsay.

VM: The security was not a great burden in the lab., was it, it wasn’t very obvious?

NP: Oh yes. Even in this building (*the discussion was taking place in the Round House*), when I first came about ten years ago, there was a guy who would come around. I would say “good morning” and the guy wouldn’t say a word. At least, he wouldn’t carry a conversation with me; he would ask me for my badge.

VM: But it wasn’t onerous in the sense that it didn’t stop anybody doing anything?

NP: No, no. I thought it was pretty strange that you would have a person coming around anyway. You see, I’m trying to think. When you start remembering all these people (*foreigners?*)...When did Jan Anderson (*from Australia*)...

VM: I think she was here when we first arrived.

NP: She almost burned down the building, eh?

VM: I don’t know about that.

NP: There was a guy who had a lot of shoes. The impression I got... A lot of shoes. He would take his shoes off and more shoes, like osmosis, keep sneaking under the bench, under my desk. You see there were two desks facing each other and then the partition.

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VM: You mean multiple pairs of shoes?

NP: Yes, different pairs of shoes each time. The shoes were coming in on my side, you see. It could have been Malcolm (Thain) but he was not that kind of a guy. (*Indecipherable section of some seconds*). Jan wasn't very careful so I was getting the fire extinguisher out. Because there was a monkey frame; you understand what that is?

VM: A chemical rack.

NP: I had my bench here, a hood behind me, and I'm facing her bench, and she has this rack, monkey rack. She was doing something, refluxing some very flammable (?) stuff (because in those days it was called "inflammable"; now it is called "flammable"), and I was sure she was going to do some horrible thing. Sure enough, she dropped some sodium in the water around the condenser and then, of course, the thing burst into big flames, and the flames reached the (sensor) that sends off to the Berkeley Fire Department, not the lab. one. They start racing in and already by then I had gotten the fire extinguisher and put it out. I had to keep the firemen from pouring more water onto the sodium, sodium and water ignite into a flame. It was a constant battle to keep the water away while I was...

VM: Was the building seriously affected?

NP: No.

VM: Was the building seriously affected? Was it damaged?

NP: Well, there was water all over the place where these guys were coming in. Jan was next to that door, with the steps outside, and she was out of that thing as fast as that. I was there, like a nut, trying to take care of this flame.

VM: And you put it out?

NP: I did. Later on, Calvin realised that I must have had something to do with it because he said "You shouldn't have (*indecipherable*)..."

VM: You'd have lost a lot of stuff if you'd let that place burn down.

NP: Yeah. It would have been horrible. I figure that was fun thing.

SM: And the shoes have no part in this thing?.

NP: No, the shoes had long gone by the time; I don't remember what happened to the shoes. It could have been something else. I'm mixing up things. Karl Lonberg was one down on the other edge near the door.

VM: Was Karl Lonberg in ORL?

NP: Yes. Have you interviewed him?

VM: No. I found him, but I haven't interviewed him. He's a farmer outside Ithaca, New York, but we're not going to get to him this year.

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NP: He was doing some interesting experiments, the were Britton Chance experiments, the kind where you pulse it with the radioactive ATP and then you look for the oscillation. He was one of the first ones to see it actually, before Britton Chance.

VM: He was in ORL, was he?

NP: Yes, right by the door. There was the big white table and his bench was...Also, there was a guy by the name of Dan Bradley. Oh that might have been it, I'm trying to recall. Those were the shoes.

VM: Dan Bradley, I'm afraid, died some years ago.

NP: Cancer?

VM: I don't know.

NP: He was a very bright guy. At that time he indicated that this whole business of information theory was going to take over and how entropy had to do with this thing. I said "What this guy was talking about?" I felt like an idiot in his presence.

VM: Then you spent how long as a postdoc.?

NP: Three years.

VM: Working on he same thing, still?

NP: The worms. A worm runner.

VM: You were a worm runner?

NP: Yes, I was a worm runner. No, I wasn't a worm runner *per se*, but a member of the worm running team.

SM: These worms were Planaria?

NP: There's this protein guy again, you can look at protein. Somebody just looking at macromolecules. Ed Bennett was the nucleic acid guy and I was simply to look at the worm extract (*indecipherable*) and take the cell-free extract of the worms and run it through an sizing column, like a Sephadex column. I managed to show several discrete peaks, so the question was do these several discrete peaks, or one, or all, have something to do the with the transfer of information (*in the worms*)? So I had this thing all waiting for the "trained" worms — which never to come, I guess. The question of how do you define what a worm, how trained is a worm? The psychologist has a thing called "response" and a worm could either scrunch up like a little shrunked up worm or that could be a response, or it could just simply stop and didn't shrink, and looked around. That could be the response.

VM: Didn't they have mazes? Didn't they have T-junctions and things in the tube and the worm could choose which way to go?

NP: No, it wasn't that elaborate as far as I know,. You had a deep well on one side and another deep well on the other side and you have a trough, with water, and you have an electrode on the two ends, and you put the planaria in — one at that time — (it wasn't kind of dumb and it didn't realise it needs to turn back to get to a deep well and it would

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always seek out (*indecipherable*) face in that direction, so the worm would travel across there. Then at a 3-second interval, during that 3-second interval, there was a 2-second light, at which point you zap it with electricity and it stops and responds. At first, it really didn't like it, of course, as one might imagine, and it really scrunches up horribly. Later on, with more light and more of this kind of regimen, it stops and looks around, and that might still be called a response. The requirement is that you have to have 23 out of 25 times that this worm will do that; 23 out of 25 times that worm is regarded as "trained". This includes all these funny kinds of response: scrunched up to stopping to look at the passing traffic. Then that worm, it was said, you cut him in half transversely and you can talk to Ed Bennett about that and you'll probably get the whole story. But we never got that stuff. It was hard to train them.

What did Calvin do? He gets the postdoc. from McConnell's lab...

VM: That was Alan Jacobson.

NP: ...and he got the technician (*Rita (?) Jacobson*) and he literally even got the water from that lab. Where was the lab.? Michigan or some place? And it was all published in *Playboy*, right?

VM: Was it in *Playboy*?

NP: Yes, in *Playboy*. McConnell's interview. It was irrelevant but it used up time. So that was an abortive period.

VM: But in the end you did leave, in about '63?

NP: '63.

VM: And you went to Riverside?

NP: That's right. I ended up writing this damn article on expressions of the pentose phosphate cycle because Al and (*indecipherable*) was there and Al in a smart way backed out on it. And he said "We need a commitment to do this" and I said "Well, you finish it".

VM: So you did.

NP: Well, only after...The editors of *Comparative Biochemistry*. Well anyway, one guy came out and said "When are you going to write it?". I said "It's being written"; so I finished that article in Riverside.

VM: You went to Riverside as what?

NP: As an assistant professor in Biochemistry.

VM: You stayed there how long?

NP: Oh, God! More than 20 years.

VM: Then you came back here?

NP: Yes.

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VM: You formally retired from Riverside?

NP: Yes.

VM: When you came back here you joined Henry Rapoport's group? Or have I got that wrong?

NP: I believe that's the case. I am trying to think...I didn't do anything for a while. I vaguely remember that I must have...That's right, because Henry Rapoport wanted to have a collaboration with me while I was in Riverside. He said, being an organic chemist, if he can supply a body, if I would teach him the enzymology, it would be great. That never did happen. In the meantime, I had all kinds of opium poppies growing in Riverside. Now, this was all on the up and by the way, because (*indecipherable*) allowed Henry Rapoport to set up with some kind of licensing agreement to make it workable. You go through what is called a California State Advisory Panel who immediately applies first of all to the Federal Narcotics Bureau to give you sanction to work on the stuff. So everything was on the up and up. I got this big pile of opium poppies and the State Advisory Panel was quite touchy about this whole thing, they wanted me to count every lousy capsule and make sure it's all accounted for and make sure that no one knows about it and it'd got to be in a locked greenhouse, all that sort of thing. We had a little tiny greenhouse in the Old radiation Lab. — you remember that? Outside of the basement counting room?

VM: Very vaguely.

NP: As a matter of fact, I think Jan Anderson asked me once or several times to escort her home some way, because there were some seedy elements from Telegraph Avenue walking around the greenhouse; they knew that there was something rather important growing in there, although they were being grown with $C^{14}O_2$.

VM: So they would have got a double... And what have you been doing here since you came back? In outline.

NP: We were supposed to look at the enzymology in which we convert a thing called reticuline, which is a benzoisoquinoline, to salutaridin which is a morphinan, and morphinan then is a precursor to these things like morphine, etc. We got someplace, but not to any great degree.

VM: Are you still doing that?

NP: No. Rapoport ran out of money, and not to pursue the biosynthesis of morphine. At that stage, I thought that I wanted to learn something about DNA and fortunately someone from France working on *Agrobacterium* had given a beautiful seminar. It was from somewhere in Paris. It was such a gorgeous seminar. Everything is now geared to DNA and at this moment I am back in enzymology, topoisomerase, which is an enzyme that breaks DNA. That's where I am now. We have been putting out papers.

VM: Great.

NP: Well, I don't know if it's great but it keeps me out of mischief!

VM: Which is where you ought to be.

SM: And where he's always been as far as we know.

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(Indecipherable exchange)

VM: On that cryptic remark, I guess we might as well close as the tape is coming to an end. Any more stories to finish up with?

NP: There are all sorts of stories but I'm not sure I remember

VM: Well, you do a think and maybe we'll come back and get some more stories.
(Irrelevant exchange) We will be in town for several weeks yet.