Chapter 21

HENRY RAPOPORT

Berkeley, California

July 3rd, 1996

VM = Vivian Moses; HR = Henry Rapoport

VM: Talking to Henry Rapoport in Berkeley on July 3rd, 1996.

Henry, before you joined up, however it was you joined up with Melvin, what were you doing and how did it happen that you got together?

HR: We came to Berkeley in September of 1946, Sonia and I, and at the time the veterans were coming back from the war, there was a G.I. Bill of Rights, the place was absolutely loaded and the only place that we had for accommodations that first night, in fact that first week, was at Melvin Calvin's. He took us in and gave us a couch and we lived there for a couple of weeks until we found a place to live.

VM: Had you known him before?

HR: I had never known him before and knew nothing absolutely about him or anything like that. That was my first acquaintanceship. And from then it was merely as colleagues in the Department of Chemistry with no particular close contact because his area was primarily physical organic and mine was synthetic organic. And the only way our paths crossed was, I would say in the late '40s or early '50s, when we began to think about doing alkaloid biosynthesis and using carbon-14 CO_2 and the ideal situation would, of course, be to do it in collaboration with Melvin's facilities in ORL and that is actually the first contact. And our first laboratory experiments and our first laboratory set-ups for doing alkaloid biosynthesis were in ORL in the early '50s in the facilities under the jurisdiction of Melvin.

VM: Who were the people at work in the lab.?

HR: There was me; there was a guy by the name of Don Baker who is now at Zeneca Agricultural Chemicals and...

VM: That is in England, presumably?

HR: No, Zeneca is here now, in Richmond. Zeneca took over what had years ago been Southrup (*spelling?*); so it's Zeneca Ag. So Don Baker was one of the first guys with me. Melvin Look was another one and we built up the apparatus there. Dick Lemmon was working in the lab. with Melvin at the time and another guy whose name escapes me...

VM: Al Bassham?

HR: Al Bassham. So Dick Lemmon and Al Bassham were with him and the guy who went down to San Diego...

VM: Murray Goodman — Andy Benson.

HR: Andy Benson. Andy Benson, Bassham and Dick Lemmon were essentially the people working with Melvin and we went in and got a little corner of the place to do some work with exposing plants to radioactive CO_2. After that Frank Stermitz came along and he worked with me. He is now at Colorado State; he is a professor there in organic chemistry. That is how we got involved in biosynthesis and that's how we got involved with the Rad. Lab.

VM: Before that had you had any experience with the use of C^{14}?

HR: Never. It was all acquired right here on site with Melvin's group. Essentially Bassham was the guy that taught us how to do work with radioactive CO_2.

VM: Did you do any organic syntheses with C^{14}?

HR: We did a couple of labelling experiments but nothing important. Our role with C^{14} was primarily through carbon dioxide. We were doing kinetic exposures to see how the material moves through the plant. That was different. Most of the rest of the field, not here at Berkeley but the rest of the world, were using radioactive candidate precursors and feeding them and seeing how they could be incorporated. We were doing some of that but primarily we were doing kinetics of CO_2 incorporation to see how it moves from place to place. We then began to do other types of chemistry within the Radiation Lab. and within Calvin's group but that was only after we moved into the Round House. Then in the Round House we had a couple of alcoves and people there were doing a mix of organic synthesis or bio-organic chemistry and biosynthesis.

VM: Do you still have people in the Round House?

HR: No. I think the primary person I had over there was Lagarias...

VM: I didn't know him.

HR: ...who was a graduate student with me and who is now a professor in Biochemistry at Davis.

VM: Didn't Ning come back to work...

HR: Ning came back to work with me briefly but that was sort of an informal basis. But Lagarias, Clark Lagarias, was the major one and he did very well. He was doing work with plant pigments, chlorophylls and the bio-pigments and stuff like that, which was certainly related but it did not involve radioactivity.

VM: I know that's running ahead many years, but you kept people there until when?

HR: I kept people there until...The last guy was Donsky and he left there, I would say...I kept people there until Pimentel died.

VM: Until Pimentel died?

HR: I had a couple of people there during Pimentel's period. Now when did Pimentel die?

VM: I don't know but clearly that was after the period when Melvin resigned from the directorate.

HR: That's right. Because still with Pimentel we were involved actively in the Round House. But then when Pimentel died and Kim took over, first, I think interim and then we moved out.

VM: Was there any need for you to stay in the Round House so long? Presumably, although the activity was a novelty when you first went in and you needed to learn.....

HR: I think the feeling was, and I think Melvin had this feeling also, that it would be a good influence to have somebody, minimal, but some activity in the group which was primarily synthetic. And so we were essentially the synthetic arm. People could come to us, or to the people working with me, and ask some questions about synthesis and so forth and we would go to the Friday morning seminars and listen and listen from the standpoint of is this coherent and is this acceptable from the synthetic standpoint? So I agree, the work we were doing did not necessarily involve radioactivity. Much of the work going on in the Round House did not involve radioactivity. Ours was not as biological as the rest of it but I think that the reason why we stayed there and why Melvin agreed to have us there was because he wanted a synthetic influence, a synthetic smell of some sort around.

VM: In the early days in ORL...

HR: It was $C^{14.}$

VM: ... it was C^{14}; you absolutely mainlined with the other people there, working with that?

HR: Absolutely. And it couldn't have been done except for those facilities. In other words, the plan that we used, the strategy that we used, for biosynthesis would not have been possible at other places because here the facilities were unique for doing CO_2 exposures and kinetic studies. It could have been set up someplace else but it was very convenient because they were right here.

VM: At that time when you first started, you said around maybe 1950 or something...

HR: The early '50s.

VM: ...was there any radioactive work going on anywhere else in Chemistry?

HR: Yes, there was some radioactive work going on making labelled compounds but it was being done independently of Calvin. The one I remember was Dauben. He was doing some work taking Grignard reagents and carboxylating them with radioactive carbon-14 to make radioactive acids and radioactive acetate (?) and stuff like that. So (*indecipherable*) were radioactive reagents were being made to a limited extent but that wasn't going on in Calvin's place. If it was it was incidental and it wasn't then done with me.

VM: Were there safety precautions? Was the concern about radioactivity at that time very great or not?

HR: Very little.

VM: Did the Chemistry Department worry about people doing radioactive work?

HR: Not a bit. Let me tell you one experiment that we did with radioactivity that didn't involve biosynthesis and that is we were looking for morphine metabolites. And so we took morphine and took off the methyl group and made more morphine and then we put back the methyl group with radioactive methyl iodide and this was all stuff we did through Calvin's group and through the Radiation Lab. And then the question was to use this material to see what the human metabolites were because there was certainly very little known about them. And we got a group of volunteers, graduate students and post-docs., and they assembled in my office and we took our shots of morphine. We had someone from the Medical School who was collaborating with us and he gave us each a shot of radioactive morphine — that is probably the purest morphine that was ever given to humans. And then we sat around and had a seminar and then we took them over — the prize was to go over to the Faculty Club and have a free lunch. Two of the six couldn't handle it and they threw up!

VM: Before of after lunch?

HR: As they were sitting around concentrating, I think, and smiling! And then they collected the urine for two days and we did isolation following radioactivity to see what metabolites we could find.

VM: This was not a trace amount of morphine; this was...

HR: This was in millicuries.

VM: In terms of radioactivity; but in terms of chemical quantity ...

HR: Oh, it was a usual shot of 5 milligrams, 10 milligrams or whatever it was.

VM: So it had the usual pharmacological effect?

HR: But the idea was that no one got very excited about this. And we just went right ahead and did it.

VM: You didn't have to ask permission of anybody?

HR: I don't think there were any people working on this. If there were it was after the fact, just to fill out files. But low activity carbon-14 we paid very little attention to. We did get monitored; in other words they would check us once in a while. Since we weren't working with tritium and the whole attitude at that time was much less hysterical than it is now, it was perfectly OK. It was one of the prime experiments we did with radioactivity and had nothing to do with biosynthesis, if you wish. It was biodegradation.

VM: When you were working on the biosynthesis of alkaloids you worked using poppy plants?

HR: Yes.

VM: In a chamber that you'd had constructed?

HR: Exactly...

VM: ...for the purpose. I remember only very vaguely...

HR: It was a Lucite chamber; it was about 3 ft. high and about 6 ft. long and about 2 ft. wide and we had plants in there, we had a controlled atmosphere and we had cylinders that would lead in the CO_2. Again, all of this was based on a design that Bassham and Benson had already worked out. Instead of having ordinary plants in there we put in opium poppies. We had a greenhouse on the roof of Latimer which we didn't get until '63. Prior to that we just had hit or miss poppy plants around.

VM: That Lucite chamber was late, was it?

HR: No, that Lucite chamber was fairly early.

VM: Where did you have it?

HR: In ORL.

VM: It was in the building?

HR: In ORL.

VM: Was there any problem about growing opium poppies?

HR: Yes — well, it wasn't a problem. We had to go through, what is it, the Drug Enforcement Agency or the Narcotics, Tobacco and something like that and they gave us permission to do that. It was only a question that the plants were under security and so forth. The amount of narcotic that one could have gotten from these small plants we were working with, you could get more from a poppy seed bagel! (*Laughter*) In any case, we went through it. But the things were very loose in terms of security type of things and I don't think they were necessary — nobody was stealing anything, nobody was breaking in. It was a completely different atmosphere. It changed when the dope generation came in in the middle and late '60s. And then we had people wandering around the building and all sorts of things, stealing journals, tearing out pages from the chemical journals on how to make these various things. Prior to the dope generation we didn't have any of that. I would say prior to '68, '65, nothing. It was handled quite openly and sensibly.

VM: I remember from my own experience, and presumably it had happened before, that your colleagues sited in ORL were very much part of the group of people who worked there. There was no obvious distinction made.

HR: None at all and I think that they were welcome there because it brought in an element that had to be used occasionally and here was expertise right at hand. And we liked the biological exposure and it was a very nice space.

VM: Were they ever drawn away from your particular work into collaborations in other directions or did you not encourage that?

HR: I wouldn't say that but I would say that certainly there were...Lagarias was very much influenced, when he went to Davis and set up his own research programme, to make it more and more biological when he had really begun here as a synthetic organic chemist and I think that was the influence of growing up in the Round House.

VM: At the time when you joined the Round House, and presumably then got to know the Benson, Calvin, Bassham and Lemmon and all the other people, there must have already been a group of 20-30 people between there and Donner.

HR: That's true. I remember the seminars over in Donner.

VM: In the Donner Library?

HR: In the Donner Library.

VM: Most of these people were chemists by training...

HR: Initially.

VM: Initially...and yet here they were working up to their eyebrows in biochemistry. Were you also feeling your way into biochemistry, as it were?

HR: The exposure, seeing the possibility for chemical applications, yes.

VM: And you went to those seminars, I remember you used to go, didn't you.

HR: Oh, yes.

VM: What did you think of what they were doing? What did you think about the way Calvin led his seminars?

HR: I thought the seminars could have been a little more focused, if you wish. The seminars — you could have spent the time better in bed. But in any case I don't know that that distinguishes those seminars from other seminars. They got to be pretty diffuse, I thought, maybe too much so. There was a difference then — at the beginning the research was focused and the people in the place were all working on very closely focused subjects and targets. As it became more and more diffuse, it sort of became a duty type of thing rather than something that you look forward or down to.

VM: But in the early days it was very good?

HR: In the early days it was good. It was focused and you came to hear the latest stuff that was going on and there were a lot of discussions and arguments, certainly over in Donner.

VM: And you used to join in?

HR: I used to join in, yeah. Over in the Round House that began to dissipate, and more and more it was more like a dog and pony show. I guess that's necessary as it becomes bigger and more diffuse. At Donner everybody was working, essentially, for the same thing.

VM: And also, of course, everybody was older and had been spending that much more time together and the thing becomes, in a sense, formalised and it is difficult to break the mould, I think.

HR: And you're working over here and you're listening to that, that's interesting, how about that, but you're not going to get that excited.

VM: Did you ever become involved in the Calvin level of photosynthesis in opium poppies?

HR: Only to the extent that we established, using that technique, that amongst the half a dozen or so closely related opium poppies, which was the first one formed and which

was the last one formed. And that was done...and the result was contrary to what was considered the dogma at that time as to what it was. Morphine was considered the early product and then methylated to the others and it was just the other way around. The initial product was thebaine which then got converted to codeine which then was converted by the plant to morphine. And it would have been extremely difficult to establish this without C^{14}-CO_2 kinetic studies, which was possible here.

VM: So this was done by what subsequently has become conventional radioactivity kinetics: (*indecipherable*) material progressively down a chain of compounds.

HR: Yes, exactly.

VM: Were you using chromatographic techniques to separate? I don't remember that.

HR: We were using them but not primarily. The separation of compounds had all been worked out for years since, so much was known about the opium poppies. We were looking for unknowns but we didn't find any. Everything had been worked out years ago. So we knew what we were looking for — it made life a lot easier. So we used chromatographic techniques but they were not nearly as sophisticated as they had to be for the CO_2 fixation.

VM: So, you only had to separate a relatively small number of compounds...

HR: A small number of compounds and known compounds so we had our controls, we had our standards. So you are looking for what you know and it is a lot easier when you know what you are looking for than when you don't.

VM: How did you become involved in opium poppies and alkaloids?

HR: Because we were doing morphine synthesis and morphine modification. I started doing that as a post-doc. at NIH. So when I left NIH and came to Berkeley, my mentor at that time said, "Look, you're going to have to have some research problems to begin with and I'm not going to be working on this much longer; I'm getting ready to leave. Why don't you take these problems?" And I thought that was extremely nice of him. So when I came to Berkeley, the first thing I did was work on morphine analogues, morphine derivatives, trying to get compounds that would separate analgesia from the respiratory effect and other depressing effects. And then, as we started working on that, the point came up, you know, what about the metabolism? And what about the biosynthesis? So that came in through the opium alkaloids.

VM: At this time, presumably, your major base was in Chemistry.

HR: My major base was overwhelmingly in Chemistry. That was just 10% of it.

VM: But you considered — and you pointed out how Melvin considered it valuable to have synthetic people around — and obviously your people learned technology techniques at the beginning. As you went on, what was your continuing interest in having a small part of your group in a different building?

HR: Well, I think that there was some advantage in having the exposure, a different type of equipment used, different types of attitudes; for example you mentioned chromatography, a type of thing that we would not do. We would do all column chromatography rather than paper. So, it was an exposure that helped. But I would say there wasn't any strong guiding force for it. We had two benches there, two good benches, so we figured it was good to keep up the connection. If Melvin had come along and said, "Look, we need this space and after all you are just peripheral to what we are doing", I wouldn't have been happy but I would have agreed with him.

VM: Now, did your group (the people in Chemistry and the people in that building) meet as a "your group"?

HR: No. It was interesting. Practically none of the people from the Round House and from Calvin's group ever came to my group meetings.

VM: But your own people from the Round House came?

HR: My own people came from the Round House and occasionally some of my people went over to their seminars when a subject was going on. But I think the synthetic organic chemist is a much greater person with greater interests and can handle more than the one who gets into a great depth in a biological subject. He goes to a synthetic organic chemistry seminar and he doesn't know what end is up. I don't think that's unusual and I think that is what Melvin did on purpose when he looked for the people for his group; he looked primarily for an organic chemistry background. You take a look at all of those people, they came from an chemistry background. That was on purpose.

VM: Until the mid-'50s when he began to recruit biologists.

HR: Right. But in the early days when I first joined up they were all chemists.

VM: And you think that was because it was close to his own way of thinking?

HR: Exactly. And I think that was because he valued people and he felt that was the most valuable background. You had very good fundamentals there from where you could move and it is much more difficult to pick those up in reverse.

VM: And as an organic chemist you presumably agreed!

HR: And I think he was right, yes. The same thing we see in the pharmaceutical industry today. They like to hire organic chemists because they feel a good organic chemist can learn the biology that he needs to be a medicinal chemist but the other way around is impossible because we have such a complex jargon that you are not going to learn it.

VM: Well, you are not the only subject that has a complex jargon.

HR: I think ours is probably among the more complex.

VM: One of the worst, yes. The genetics is pretty bad as well these days.

HR: I think it is probably amongst the worst out there.

VM: One of the worst, yes, but genetics is pretty bad as well these days.

HR: That's right, genetics. But genetics at that point didn't come into it much.

VM: Did you perceive — you were watching this activity; I am thinking particularly of photosynthesis since that in a sense was the most coherent activity that Calvin's group had in the early years — did you see Calvin very much as the driving force in the originating of ideas or did you see other people making significant contributions?

HR: Oh, I saw other people. Benson and Bassham were very significant contributors and very much involved in the discussions. Calvin was very definitely a driving force but not necessarily the only source for ideas. He was there to say, you know, lets get going, lets do it, why wasn't it done? But the question of exactly how to do it and the sophistication and the intricacies, I felt Bassham and Benson were right on top of it.

VM: Well, of course, they were working with it day after day.

HR: And they were right there at the bench.

VM: That's right. Do you think Calvin's style of scientific management was an effective one?

HR: Well, when you have people like them it is. It reminds me of a famous story by the manager of the New York Yankees who was asked, "How come you are so successful? You keep winning pennants all the time." He says, "Well you have to know a lot about baseball and all the theory about baseball also, and it helps if you have Joe DiMaggio in centre field." So he had a couple of DiMaggios, there is no question about that; I always thought so. And that is one thing that shouldn't be overlooked about Berkeley. The quality of the graduate students is excellent and that makes a lot of the faculty look very good. Now, I'm not saying that the faculty wouldn't be good otherwise but these people, you get the very cream and they come in and they are excellent and that makes a difference. So if you don't want to lose another ball game get good ball players.

VM: But there are interesting things about that group particularly, perhaps, for that time. There was only one academic leader to the group. That was Melvin, himself. All the guys, Benson and Bassham and so on, were none of them academics; none of them had academic positions.

HR: None of them had academic appointments.

VM: And they were all, in a sense, research directors or whatever you want to call them but under Calvin's guidance — not guidance, but under his direction. Everybody else were transients — post-docs. and students — but focused, in a way, I guess by this hierarchy.

HR: In a way, yes. That's a British system. It reminds me when I visited Cambridge. Todd was in charge and he had two or three guys working with him as his, essentially, assistants in the same way Calvin did. Any one of those guys would have been a professor in America. He had guys like Johnson working with him and I forget the names at the moment. I was very impressed. I said, man, he has some good people working there. And they were all under him. Everything that comes out is Todd and somebody (?). In a way it was that sort of a system. He had very good people who probably at other places would have been faculty.

VM: Those who left did, indeed, became faculty.

HR: So it was a British system.

VM: So if one were to compare that with your group, which was not a small group, as I understand it....

HR: Oh, it was a large group.

VM: Your people were all post-docs. and students, were they?

HR: No, no. At that time they were mostly graduate students. It is only now that I am emeritus that they are all post-docs. The average, the ratio that I have maintained through the years was two graduate students to one post-doc. So we had a ratio of 2:1. But the post-docs. did not take any position either in terms of management or anything else that was superior to the graduate students.

VM: There was you — and the rest.

HR: That's right. There was a very, very flat pyramid. There were no lieutenants. In Melvin's case there were lieutenants. He was a general but he had a lot of captains.

VM: Well, in the early days he was a very hands-on general because he spent a lot of time...Presumably you were an even more hands-on general because you dealt directly with all your troops.

HR: Right.

VM: Do you think that in the set-up you had with such a high proportion of graduate students, it was more difficult to get a coherent programme in the sense of graduate students needing to get (*indecipherable*) for their theses?

HR: Well, the objective is not to get a coherent programme — coherent only in a very large sense. Synthetic organic chemistry with heterocycles! That doesn't confine it

very much. So what we really were looking for was a series of problems and projects in which we maybe had half a dozen different projects related only in the sense that in synthetic organic chemistry we use a lot of the same thinking and so forth, but the targets were very different. That wasn't true in the early days in Melvin's set-up but later on it became that way.

VM: Indeed. Do you think Melvin could, in fact, have done what he did in a university department, just as a professor, had he not had this Radiation Lab. link?

HR: Oh, I think the Radiation Lab. made a tremendous difference. I don't think it would have been possible without the Radiation Lab. I think the Radiation Lab., in terms of support, financial and space made a tremendous difference. And equipment and facilities made a tremendous difference. The whole idea of handling radioactivity here is so much easier than doing it someplace else. The whole idea of doing it with an administration that wants that sort of thing done and wants more of it done, I think. Maybe it could have been done someplace else but it would have been longer and I don't think it would have been done as well. So, yes, it's a combination that was very important, a unique combination. Calvin at Yale couldn't have done this and somebody else at Berkeley couldn't have done it. It takes somebody with that combination that was necessary.

VM: But not just because of the ability to use radioactivity in the way that they did. The ability also to get a group of people focused in that sort of way.

HR: With the support.

VM: With the support.

HR: And that's important. You can tell somebody like Bassham or Benson and say, "Look, here it is. You've got a job. You're not just a post-doc. You're not just getting starvation wages for a post-doc.; you're going to move up." And the Radiation Lab. is paying good salaries so these people were doing reasonably well. They weren't getting the prestige that they would have gotten in a faculty position but they were getting the material benefits from it. And they were getting the publications and so forth to their reputations. Because of the Radiation Lab. he had something attractive to offer to the researcher he wouldn't have had someplace else when a post-doc. comes in for a couple of years and then goes. It couldn't have happened with people like that. All through the years we've had post-docs. for a couple of years and that's it. The post-doc. who is going to stay for more than two or three years has got to have a very good reason. Commonly it's considered bad for the post-doc. and bad for the institution to have sort of an institutionalised post-doc. That's completely different than what you see in the Round House, in the Rad. Lab., where essentially you have institutionalised post-docs. because they're not post-docs. anymore. They become scientists or whatever and that's not possible in a department. So I would say that that set-up was absolutely necessary — could not have been done otherwise.

VM: Another very unusual feature, I think, is the very great cohesion among all the members of that group through the years — this reunion thing and the way so many

of them keep in touch with one another. You had a large group for a long time. Do they do the same?

HR: Not as a group. Through individuals and so forth and I have a lot of contact with people but we don't have any group assemblies.

VM: Why do you think they do it? Do you think it is this memory of golden years?

HR: I don't know. I think that the people that do it are the nucleus people that we were talking about. So we are talking about a half dozen nuclear people. You're not talking about the other hundred people.

VM: Oh, several hundred people.

HR: Several hundred people. So I think it a tight nucleus but the tight nucleus, don't forget, didn't spend two years or three years, they spent close to twenty years.

VM: A lifetime, some of them. Marilyn has been with Calvin for 48 years.

HR: So that's the difference and that comes back again. He is running a research institute type of thing within a university department: extremely unusual because where else do you get personnel that stay that long? A graduate student stays, now he stays five years; then he stayed three or four years, a post-doc. stays two years, some of them stay one year — so it's a completely different type of pass-through that you get with this nuclear core that we're talking about. They spent ten years plus with Melvin.

VM: So, do you think that type of organisation is still unusual? It certainly must have been unusual ...

HR: I would say it was very unusual then and it is unusual now at universities. American universities do not have research institute-types of set-ups. It is only when you get a national lab. connected with it for something like that that you begin to see those. I don't say Berkeley has it because of that but I don't know that, well, I don't know — there are a number of people in the Chemistry Department that have large groups, and the Materials Science Department. But again, it's a group, it's not an institution on its own. Very unique, then and now. It is almost like the Soviet system of the Academy labs. and that type of thing.

VM: Yes. But in a university context, nevertheless.

HR: Nevertheless.

VM: Never broke his link with the Chemistry Department. Very important to maintain that link.

HR: Absolutely, and that's what makes it even more unique.

VM: People have commented a lot about the influence of the building, itself — that wooden building, the open quality of it, the lack of (*indecipherable*)...

HR: ORL.

VM: ORL.

HR: ORL was a fantastic place to work, I thought.

VM: Tell me why you thought so.

HR: You went in there and you run into everybody and there it was. There was an urgency about the place that you just sort of felt. In a way it reminded me of a lot of the labs. in Old Chem. The fact that they were old labs. didn't make any difference. There was an openness and an interaction about it. That I didn't feel the same thing in the Round House. In spite of the architectural attempt to create it, I don't think it did.

VM: Were you part of that architectural attempt?

HR: Was I part of it?

VM: Were you part of the discussion group?

HR: No one asked me.

VM: No one asked you at all? Did you volunteer?

HR: I volunteered my views but obviously no one paid any attention to them.

VM: What did you volunteer?

HR: I didn't like the idea of the Round House. I thought that that was artificial — that you are not going to create interaction by making it very inconvenient to operate. I didn't like the idea of the spokes, either. I felt it would be much better to have multiple labs., like 4-person or 6-person labs. and get away from the general noise and the general tumult.

VM: That's what you felt in advance of the building.

HR: In advance of the building.

VM: Having seen it in operation — not now; now it's been seriously degraded compared to what it was — in its heyday, was it as bad as you felt it would be?

HR: It was bad. Maybe not as bad as I felt it would be, but it was bad. There are a lot of people who have to work in an area of noise. And I just like it to be quiet with controlled noise and that's why I like to have rooms. Now, Latimer is too segregated; we have 2-man rooms .

VM: Latimer, perhaps I should say for posterity who will listen, is the Chemistry building that we are now sitting in.

HR: Right. I think that's too segregated; we have 2-person labs. I think 4 or 6 is a very nice unit. People can interact and now they interact because they are right there and they hear each other's noise so they have to do something about it. I think when it is too big it doesn't work.

VM: Have you ever worked in that building?

HR: No. I've been to seminars in that building and I've had people working in that building.

VM: What have they felt?

HR: Well, they weren't particularly happy about it.

VM: Because I worked in that building for eight years and I had exactly the same hesitancy about the noise. There was remarkably little noise and in fact it was difficult to hear people talking to you from any distance away. Not because of the background noise but I don't know...

HR: One thing I remember about that building is at the beginning when they were putting up the roof they had problems with the tile because they hadn't figured on the fact that it was a smaller diameter at the top than at the bottom. That's the only thing I remember about the actual construction.

VM: I presume there were little men somewhere custom-baking these tiles, gradually tapered tiles.

HR: I knew the architect very well, Goodman. In fact he knew my family back in Harbin.

VM: In Harbin! You were born...?

HR: No, I wasn't, but my family lived there for a number of years and he knew the family.

VM: They came from Russia to America through the Far East?

HR: Right. So it was interesting coming across Goodman and talking to him. You see Rapoport — do you have people in Harbin?

VM: Did you ever discuss with Goodman what he thought of the building?

HR: No. I figured I'm not going to...Melvin was set on that. It had to be a round building.

VM: Yes, well, I was part of that, I have to say enthusiastic.

HR: I remember Dick Lemmon walking around with the plans. He was very much involved with the plans. And who was the guy who was essentially the chief.....

VM: The Building Manager, Paul Hayes. He was the Building Manager for the...as it were, the Lab. representative

HR: What is the term I'm thinking of: "gabai", the guy that runs the (*synagogue*)?

VM: The beadle or the sexton.

HR: Yeah, well all right, well, the gabai. He ran the show. He did everything. He was invaluable. No matter what happened. If the projector didn't work — Paul Hayes.

VM: He clearly felt that was to be his métier to do that . He was also a chemist by training. I have no idea how good a chemist...

HR: I didn't know that.

VM: ...but his chemical training gave him a degree of understanding and an ability to talk technology and techniques to people. So that although he was the business manager or the administrator of the building you could bring him a technical problem and he would know what you were talking about and that was very valuable because (*indecipherable*).

HR: No, I didn't know that. But he was extremely useful. You couldn't operate without him. You have to have somebody around who takes the responsibility like that and gets it done. He was reliable.

VM: So, it's been an interesting experience, the whole...

HR: Yes, I thought it was although it began to deteriorate. The period '50-'60 was a lot more interesting, exciting than the period '60-'80.

VM: But that's so often the case. The glory of the early years of any venture...

HR: And the intensity.

VM: The intensity, and the fact that people were younger and later they get older and they get staid or tired or whatever.

HR: Well, but I wonder whether that isn't what you've described now an institutional problem and not necessarily...because when you have pass-through people, that doesn't happen. And I think the permanent staff type of thing, and essentially it was a permanent staff where you look anything more than five years as a permanent staff.

VM: There is also the permanency of the leader. The fact that the man or woman is very good at one time of their lives and does great work and receives recognition doesn't

necessarily mean they are going to go on doing this. And yet it becomes very difficult to displace them from those positions.

HR: Oh yeah; there's no question about that.

VM: I am not putting Calvin in that category although I don't think he did anything after 1961 to compare...

HR: I agree completely.

VM: Well, you know, that's the luck of the draw. Nevertheless, the man and other people in that position are simply irreplaceable. Nobody can push them out in favour of other people. They are too well established and that's the nature of institutionalism, I guess.

HR: That's a major problem; we don't know how to handle that.

VM: That's right. Well, I think one of the ways of handling it, I think, is the way you described for your group and that is to say not let it become institutionalised and not always have this transient turning up of population. It produces a different effect, except inasmuch as you, personally, become older the group doesn't become older.

HR: That's true. The group doesn't become older because the average age in the group ten years ago is the same as it was thirty years ago and the same as it is now.

VM: And perhaps that helps to rejuvenate you and to keep you young.

HR: Oh, there is no question about that has an effect on you. Walking around campus has an effect on you. And, I think, a very good effect, very important because you see all of these people and you can't be that much different.

If there was an inhomogeneous unit within the unit (*i.e. within Calvin's group*), it was my presence in the sense that we had different targets, that we had different ways of going about things. Certainly, to Melvin's credit, he felt it was good to have that there even though it might not have buttressed some of his other projects as much as having people working directly on them. I think he liked the idea of what one would use as the very popular word now, the "diversity". We were the ethnic differences there.

VM: Well, there were two other ethnic minorities — there was Tinoco and there was Hearst.

HR: There was Tinoco and there was Hearst but they were later.

VM: They were later and they were not so permanent or so embedded as your group was, your presence. So you represent, really, the best, the most stable...

HR: In the end I think Hearst; Tinoco never took but in the end Hearst did and Hearst, I think, became much more involved, much more so than I was. But in the early days that wasn't true. And again I think I was an example. Hearst, Tinoco, Rapoport;

Hearst, Tinoco, physical, biophysical; Rapoport, synthetic organic, bringing in quite different things into this group which really was a major effort with him but where exposure was a good one.

VM: But interestingly, all still members of the Chemistry Department. No member of Biochemistry was ever brought in or of Molecular Biology...

HR: Well, Park was brought in. He was part of Botany.

VM: Park was brought in as a young man. We're going to see Park next week and get his story. But I think that he was not brought in in that sense but I don't know the details. He wasn't an established professor the way that you were when you came in.

HR: I think he was an established professor in Botany, I'm pretty sure. And I remember the meetings we used to have in the Directors' Room at the Faculty Club. We used to have the senior management at a Thursday lunch. Park was going to those and I think he was a professor or some sort of a professor in Botany at the time.

VM: Later on but not in 1958 when he first came — as far as I remember.

OK; I think we can...