Chapter 35

ROBERT RABSON

Rockville, Maryland

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VM = Vivian Moses; RR = Robert Rabson

VM: This is a conversation with Bob Rabson in Rockville, Maryland on July 30th, 1996.

As you know, I wanted to talk to you because of what you would have seen of Calvin's lab. from AEC Headquarters perspective, but before we talk about that, what was your own background because knowing what your own scientific training was will enable me to get a better idea of what you were looking at in his lab.

RR: OK. I got my Bachelor of Science degree and also my PhD degree at Cornell University working on plant physiology. While I was not working directly on photosynthesis, I was working on biochemistry of amino acids and various things like that. In any case, I did my postdoc., after I finished my PhD, with Ed Tolbert (Nathan Edward Tolbert) at Oak Ridge National Laboratory. There I did do some work on photosynthesis. As a matter of fact, we found some things that later on we understood better. For example, one of the things I was doing was checking radioactive labelled CO_2 and how it was incorporated into plants in a matter of seconds as well as minutes. One of the things we did with corn, was we found that there was malic acid that was one of the products. But at the time, the C_4 fixation was not yet understood. While we saw it, we didn't understand it. Later on people did. But we did a whole variety of work on photosynthetic systems. After I left the two-year postdoc., I got a job on the faculty at the University of Houston. I was the only plant science person in the whole department, as a matter of fact (*in*) the whole university. Nevertheless, I did go back to Oak Ridge for the summers, as well as going up to Beltsville, Maryland and working at USDA on different kinds of projects. They were not photosynthesis projects but they were plant science projects. And the during my fifth year at the University of Houston I was asked to come up to a Washington on a two-year rotation to serve the Atomic Energy Commission. Well, I was attracted to that and I decided, after getting permission, to go. Somewhat after the first year of serving in the Atomic Energy Commission, I was asked if I wouldn't stay on more permanently and I had to make a decision between going back to the university and staying on with AEC. I did enjoy the job with AEC. So, I made the decision not to go

back and I informed the people at the university that I wouldn't come back. So I stayed on. That was in 1963 that I started the job at AEC.

During the next few years I had interactions with people in many different laboratories, both the national laboratories and university laboratories. Furthermore, in those days we did support projects outside the country, so I remember going down to Peru a number of times, Costa Rica a number of times, Brazil — and visiting the people who had the project and seeing what was being done, and so on and so forth.

VM: This was all in biological sciences, was it — as far as you were concerned?

RR: Yes. All those projects that I visited were projects having to do with either plants, in one or two cases entomology and so on but mostly plants.

VM: When did you first know Calvin? You had presumably known about his work when you were in the university.

RR: Definitely. I think it was maybe one or two years after I came (*to the AEC*) that I did pay a visit to Berkeley and meet him, that would have been in the mid-sixties sometime.

VM: What was your role in the AEC office?

RR: I was in the Office of Health and Environmental Research and my principal responsibilities had to do with plant science and, to some degree, with things like entomology. It was relating nuclear things with agricultural and other applications.

VM: Which had been part of the original mission of the AEC, wasn't it, to do that?

RR: As a matter of fact, that's one of the things that the objectives were in supporting the outfit out at Berkeley in photosynthesis. The whole idea was energy transformation and the use of radioisotopes to better understand (*these processes*). Anyway, we had some contacts with people in USDA who were using radioactivity to induce sterility and then control insects by releasing sterile insects into the wild and overwhelm the natural population. That turned out to be a quite interesting and effective way of controlling insects that were nuisance insects.

VM: Did you use a panel system for judging grant applications? What was the internal mechanism in your office for deciding who to fund, how much to fund them, and directing things in areas you thought of interest?

RR: Yes. In lots of the units of AEC and subsequent Energy Research and Development, and then the Department of Energy, lots of the units only used mail reviewers but as a unit...you see I had another unit that was started in 1979 and this was not in the Office of Health and Environmental Research (*OHER*): it was in Basic Energy Sciences. The project, the programme rather, was the only biological programme in basic energy sciences. You see, there was a tendency to want to introduce biological sciences into basic energy sciences. And so they went through a procedure and a decision was

finally made that there would be a programme and then, after the programme was identified, they asked me if I would be the director. I said "OK". What they did was transfer something like four million dollars of the projects that I was working on in OHER over to Basic Energy Sciences. Ever since that time, the programme has been growing, so it's about 28 million dollars now. Also, at the beginning I only had myself and my secretary to handle things. We did use mail reviews and we established panels on an annual basis.

VM: So before then, in the early part of Calvin's lab, there had been no panels?

RR: Well, I wouldn't say there were no panels because there were quite a few instances of where people from the outside were called in to review a project. I remember that we would do that in taking the projects that we were supporting at Brookhaven and we would bring a number of people together who were experts in plant science and related things and have them listen to what they were doing, what they've accomplished, every three years. So that there was an outside review, people who were not associated with the Department of Energy/AEC, whatever. In any case, we did have — and do have — even though I have left, a system of using outside reviewers who were familiar with the technical aspects of the science to look at the projects, not only the ones that were going on but also the new things. During the panel meeting, once a year, we would review both renewal proposals and new proposals at the same time.

VM: In a unit like Calvin's, which had been supported by AEC for many years on an ongoing basis, there was, presumably, no likelihood that the funding would suddenly be terminated because the panel didn't like something. Nevertheless, you had to exercise proper control over where your funding was going. So how did you balance the AEC's appreciation of what the group was doing with what their ongoing research was, and the reputation: did you try to steer what they were doing? Did you comment back and say whatever, something you didn't like?

RR: If, by chance, they had come forth with a project that just seemed to repetitious and not particularly important, then we might raise a question. On the other hand, we did encourage that people do innovative things; that is, it's still being encouraged and it is, we feel, very important that with a programme that is not tremendously large that we do get involved in projects that are unique in many ways and innovative. It's not to say that every single project is that way but quite a few of them are. The other thing that we have done is run workshops where we would pick a topic that covered an area that was important but did not get enough attention...

VM: ...from the scientific community?

RR: Yes, from the scientific community. So we ran a workshop in, for example, plant biochemistry. This was some years ago, to bring out what needs to be done in plant biochemistry. We also ran a workshop on phytoremediation, that is, the use of plants to clean up things. There was so little work that was going on in that (*area*) and the work that was going on was not on trying to understand the basic functions of plants that relate to this. We tried to encourage people to start to do more basic science in

order to understand what plants can do and how to manipulate, and so on and so forth.

VM: So Calvin's activities and those of his group in photosynthesis and what followed were very much the sort of thing you are interested in.

RR: Exactly. We, quite frankly, always sought people who really had great capabilities and a tendency to innovate in their research. We were delighted to have those people. The other thing that we recognised is that when you do innovative experiments, you may not get the results that are very meaningful right away because you would have to do many, many other experiments. So, in analysing the progress made, sometimes the people would not have tremendously convincing results but we appreciated the fact that in doing highly innovative work this may take time. We did not, at least in our group, cut back the budget because they didn't publish anything very exciting in the last few years. If they told us what they were doing and where they were going and it sounded exciting, we'd still support it.

VM: I have two questions at this point. One of the is: was this pattern of funding, and the attitudes you have described, influenced by the experience of the AEC in the Manhattan Project, the idea of big science and long-term developments and that you can't rush things?

RR: Well, I can't answer that in a very positive way, or negative way, because I just don't know. The point is that the AEC, back in those days, had lots of technical people in contrast to what's going on in government these days where the people who are in charge are oftentimes not technical people anymore. As a result, there's a certain lack of understanding as to how research proceeds, and this, that and the other thing. This is a little disappointing. Back in those days, in AEC days, the people were indeed quite technically oriented and they had objectives which were in many cases understanding basic scientific kinds of activities. They were not at all criticised for not doing something which was immediately related to radiation or something like that. One really fine example of that was Alex Hollander's laboratory down at Oak Ridge. He conducted — that is, the people in his lab. conducted some of the most outstanding genetics of that day and it sort of led into molecular biology. He really had the tendency to look for investigators who would do innovative things. I really felt that he was one of the people who really made enormous contributions to biology by the way he ran the lab., the kind of people he had in.

VM: OK; the second question was about the mechanism of Calvin's group in actually communicating with AEC office in funding terms. From the inmates point of view, we used to write up the year's work and project the future year's work, and so on. And I presume, I didn't see this, but I presume that this was accompanied by a budgetary statement of what their expenditure would be expected to be in the following year.

RR: That's correct.

VM: Then, what happened to that? Did you...was the relationship such that they knew pretty well what to ask for and what would be acceptable or did you have endless discussion about how much here and how much there? How did it go?

RR: This was highly variable because nobody knew, really early on, exactly what the budget would be. If it went through Congress, and sometimes the budget would be incremented and sometimes reduced, and we would encourage people to submit budgets of at least a prior year level and, if they came forth saying that we want to do this in he future and it's going to require this and that, if there was any hope that the budget would come through, we encouraged them to submit that expansion of the programme. Or otherwise, if we were convinced that this was an important thing to do and we knew new money isn't going to be coming, we suggested to them that maybe if they had something there which really wasn't that important they would transfer the money. These are complex things, and with each laboratory it's a different story.

VM: But it was a gentle relationship which relied on personal contact between your office and your colleagues and them...

RR: Yes.

VM: ...and you were in a position, both sides, to talk back and forth and work things out.

RR: Yeah. We definitely wanted to have these interchanges so we knew what they were doing and they would have an idea of what kind of resources we had available to go ahead.

VM: It's really running ahead of the period that we want to consider, because the detailed time that we are discussing, we're researching, is going to finish is 1963 which is the time when Calvin's group moved into that round building, you may remember.

RR: Right.

VM: We have to stop somewhere and that's a place to stop. To look forward, later in the sixties there was, of course, a gradual move away from complete AEC funding of Calvin's group and people then began to get grants from all over. I gather that is a procedure which has increased. Does that mean that AEC or, I guess, it's EPA, DOE, whoever the present body is, the present successor body...

RR: DOE.

VM: Is it DOE?

RR: Now.

VM: Yeah...does DOE now use the same approach as AEC did in your beginning days or has it also changed?

RR: It's changed considerably. One of the things that's changed, I have already mentioned. The people who are managing are not all familiar with technology although, I must say, that people in Basic Energy Sciences, for example, who run the different divisions are all technical types and do keep in touch with the people at the laboratories. But at the higher elevation, some of those people just don't understand what the importance of certain kinds of research is and they don't give it much in the way of the resources.

Let me give you an example. A number of years ago we held a workshop on carbohydrate structure and we had people from many different places and we had them from other agencies at the same time, about 25 people, but the whole idea (and there were some people from industry), the whole idea was to lay out what is needed in the way of carbohydrate structure procedures. One of the recommendations that the workshop people came up with was that there ought to be centres developed. Since I supported this, I went around afterwards showing people that this was an important thing and we didn't get a dollar to go ahead with this until there was another thing that came up and one of the Senators from a state wanted us to do something at one of his universities and wanted *us* to do it. He said that he would proceed to put the necessary money into it, in our budget. But one very good thing that he said, he said this would be only three years, and if that group is not competitive, no more funding.

VM: The atmosphere changed and it sounds as if it became politicised as well.

RR: Yeah. The thing that happened is that we got the money for that particular project but at the same time they gave us some extra money and that's what we used to start up the Complex Carbohydrate Center. What we did, we put an announcement in the *Federal Register*, asking if there are interested people to send in proposals. We got six proposals and we reviewed it with outsiders and the people from the outside felt that there were two excellent proposals and then we site-visited with the people and had them give us additional opinions. Then we had to make a decision, either one or the other. We made the decision to pick it up at the University of Georgia. That particular project has worked extremely well.

It turned out later that the National Institutes of Health also contributed some money, not into the same budget that we had but an additional. They have gotten money from the outside, from different agencies, too. They have had literally dozens and dozens and dozens of people and institutions come in to work with them. They have provided means of gaining structural information about complex carbohydrates; they have provided courses for people to come and take for two, three, four weeks, people from industry to learn how to analyse things better and, of course, they have done research, too. I can show you a book that represents the responses of people to the Complex Carbohydrate Center and why they think it's important. In any case, we did not get recognition from the Department Of Energy, or anybody else, but we did find the money and I think it's recognised now.

VM: Have...I think, perhaps, this is the last thing I'd like to ask: have procedures become more bureaucratised than they were in the early days?

RR: Yes, quite indeed.

VM: Form-filling and things of that sort?

RR: Yeah. And the way the government is operated now, there is much more emphasis on the *way* the particular programme operates than what it achieves.

VM: I won't ask you to pass comment, but it's an intriguing thought to wonder how easy Calvin would have found it to get going had he started now compared to starting in 1945 when he did.

RR: Well, I think that there are agencies that do look for people doing innovative things that would produce information that indeed would have an impact. So I don't think that he would be impaired. He may not in today's time be able to start out with as much of a group that he had, but with time, it could build up.

VM: That's encouraging at least.

RR: I think this is very important. We have expressed that many times and other people who run programmes, both in DOE and in other agencies, have also expressed the importance of getting more innovative kind of things going. So, I'm not sure that every good scientist these days is being picked up because many of them are in the same area and there's already in many agencies lots and lots of money going into that particular area and they just can't...

VM: ...there has to be a limit.

RR: Yes.

VM: I want to thank you for spending time with us. It's a very valuable piece of illumination about the environment in which the group grew up and functioned. It's only by talking people like you who were at the other end of it that we can get that feel.

RR: It's my pleasure to meet with you and give you as much information as I can.

VM: Thank you very much.