

## Chapter 32

### R. CLINTON FULLER

Amherst, Massachusetts

July 24th, 1996

**VM = Vivian Moses; CF = Clint Fuller; SM = Sheila Moses**

**VM:** We are talking to Clint Fuller on the 24th of July 1996 in Amherst at the University of Massachusetts.

Clint, can we start the discussion by your telling us what life was like for you before the Calvin adventure and how you came to participate in it?

**CF:** Well, what life was like very early is really not relevant to my scientific career because in my other always wanted me to be a doctor, a real doctor, and I struggled all through my youth and adolescence with that held over my head. Luckily when World War II arrived I was drafted directly out of high school and at the time I went into the fleet to become a medical technician in the Navy. But at the time there was a programme where, if you passed a certain examination, the Navy sent you back to college and that was part of our so-called “V-12” (V for Victory, thanks to the British) programme and I was assigned to Brown University in Providence, Rhode Island, and had a great teacher there on the way to Medical School, I thought. The great teacher talked me out of it. It was George Kidder, who ended up here at Amherst that was a good friend of Melvin’s. He had almost an earlier influence on me than Melvin did. So when the war was over we were dismissed and I didn’t graduate by that time and followed my professor to Amherst, Mass., where I got both a Bachelor’s and a Master’s degree from Amherst College.

**VM:** In Chemistry?

**CF:** In Biology. I was really a cell biochemist.

**SM:** What year was that?

**CF:** I got my degree in 1948. And, incidentally, I was here in ’46 right after the war. It was a town where — I met my wife at college, at Brown University — we were married here in Amherst. It essentially took me 23 years to get across town where I

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am now Professor Emeritus at the University of Massachusetts. Between that, George Kidder wanted me to go to the very top lab. in the country to get my Ph.D. and he sent me to Stanford where Ed Tatum (of Beadle and Tatum, [*incomprehensible*]) had just gone from Yale and I joined that group at Stanford and spent four years there getting a degree in what was then called “One Gene, One Enzyme.” Molecular genetics had not been formed. I was very fortunate and had a wonderful experience and that is how I got associated with Calvin’s lab. The story is one of serendipity — being the right person at the right place at the right time. It was the spring of 1952, the Korean war was finishing, we had inflation, things were rationed, jobs were terribly hard to get. We had very good relations with our professor Ed Tatum, later a Nobel Prize winner himself, and he was in charge of Sigma Xi, a lecture programme at Stanford, and he invited Melvin Calvin down from Berkeley in 1952 to talk about what was then very exciting, the *Path of Carbon in Photosynthesis*.

Well, before the evening lecture Ed Tatum had his students over to his house to meet Calvin and to have a few drinks and a barbecue. And Calvin, again in a very gracious — I had never met the man but, of course, I had heard of him — in a very gracious way, came around and talked to us individually and introduced himself to me and said, “Well, young man, I understand from Ed Tatum that you are about to finish your degree. What do you plan to do with the rest of your life?” (That was a direct quote from Melvin. In retrospect I can remember.) And I said, “Well, Professor Calvin, I really don’t know but I’m looking for a job.” Mind you, this was May and I got my Ph.D. at the end of May and my fellowship ended and things were tough: wife and two of my four children and...Melvin said, “Oh, that’s interesting. You’re a microbiologist are you not?” I would have said “yes” to anything at that point so I agreed. Van Neil was on my Ph.D. committee so I had (*incomprehensible*). He said, “We’re having some trouble with contamination in our algal cultures up in Berkeley. I wonder if you would be interested in coming in to help straighten us out?” Those were his words. I assumed that was sort of a probe for some sort of a position; I didn’t know. And I said, “Well, I would certainly be interested. I’m very excited about what you are doing.” And I said all the right things. And Melvin said (this was on a Friday), “Can you come up and we will talk about it on Monday in Berkeley?” And I said, “I certainly can.” And I went up to Berkeley and we had a great talk in Melvin’s office, with Marilyn Taylor taking notes in the office, which I thought was interesting. A new experience for me. And Melvin said that they were really very much interested in getting the culturing situation straightened out and “if I was a good microbiologist, and if you had worked with Tatum you must be, and we have a number of projects which were completely dependent on having algae”. Now mind you, this was about four years after the *Path of Carbon in Photosynthesis No. 1* came out, published in *Science* where, essentially using  $C^{11}$  work, the early products of  $CO_2$  fixation were all malic acid, oxalic acid, the reverse of the Krebs cycle. That was published. And, of course, as we know, that was completely wrong until it went full circle back to some of Arnon’s work and so forth. So he did need some help because some of his cultures of algae were 50% yeast. That accounts for the Krebs cycle. So, in any event, to make a long story short, he offered me a job on the spot. I will never forget it — \$6000 a year!

**VM:** You mean he did that without even consulting Andy, who was running the...?

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**CF:** I met absolutely nobody else in the lab. that day. When I came up to see the lab. and look for housing and all that sort of stuff it was all very rapid. I was on the job in three or four weeks. Then I met everybody.

**VM:** You hadn't seen the set-up? You had simply talked with Melvin?

**CF:** He had taken me through the lab. and I met people but it didn't mean anything — we didn't spend any time at it. But I did have long talks with Andy and it was quite clear my job was to get going on these cultures and to straighten things out. And the future was wide open. He didn't, as Dick Goldsby has said, it was pretty much up to me. And that was my Ph.D. experience and that looked good to me.

**VM:** Can I ask a couple of questions at this point?

**CF:** Sure.

**VM:** One of them is that Melvin offered you a job without making it a short-term job?

**CF:** No. He made it quite clear that I wasn't coming in as a post-doc., I was being appointed as a member of his staff and I would initially get a year's appointment at \$6,000 a year.

**VM:** It could be renewable or....

**CF:** Something like that; I don't remember the details. Of course, he didn't have a contract for me to sign or anything but he said, "yes, I would like to hire you, and I'm going to proceed with this" and Marilyn was taking everything down.

**VM:** Who had been running the algal culture set-up beforehand?

**CF:** A technician by the name of Louisa Norris. Her husband also worked in the lab. In straightening out the cultures, it wasn't necessarily Louisa's fault that they were partially yeast. It was just that the microbial technique was not too good and I pointed out that we did we would plate out cultures and isolate single colonies, pick single colonies, these are the *Scenedesmus* or the *Chlorella* and grow those in small flasks. We made some changes in the shaking apparatus that grew the algae and things were fine. We had pure cultures. That's when the C<sup>14</sup> work was at its peak.

**VM:** And Louisa continued to work with you?

**CF:** Yes. She worked as my assistant, research assistant, essentially. She was so delighted to have help. She was a wonderful person, very nice, and there was never any competition or she would be doing something wrong. Calvin wanted a trained senior person on his staff that knew the biological story.

**VM:** So, when you started, your obligation was to clean up the cultures and run them effectively and the research was to be whatever you made it?

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**CF:** Pretty much so. Except we had a visitor from South America. His name was A.O.M. Stoppani, from Buenos Aires, and he was one of many international people who came through the lab., and Calvin wanted him to come up with a project while he was there. Well, I had had some photosynthetic prokaryotic experience because I had spent a lot of time with Van Neil because he was on my committee and I knew a little about photosynthetic bacteria. And I said, before Arnon got involved in this, I said, “Melvin, we ought to have some work now you’ve done higher plant leaves and then *Scenedesmus* and *Chlorella*. Let’s take a look at some prokaryotic bacteria. Andre was interested in this, this was his training, in microbiology. And so that was my first project. Stoppani was there for a year, and we published a paper on the *Path of Carbon in CO<sub>2</sub> Fixation in Rhodospseudomonas spheroides*. Calvin was impressed because it did begin to show some of the same compounds but with a different emphasis on amounts. It showed a lot of what Calvin first saw in yeast. In those days it depended on how you grew it but if you grew them photosynthetically, they were showing the early ribulose diphosphate and PGA ions and so forth, but a lot of malic acid, a lot of oxalic acid and so forth, so that there were a lot of products there that weren’t there in the algae.

**VM:** But you also had to learn all the local technology, chromatograms...

**CF:** Oh, yes. There was Andy and Al Bassham and they were just great, pitched in. And we did that all in the Old Radiation Lab. (ORL) and many of our colleagues, several — Roger Stanier, Stan Carson was there — a number of people in that era took chromatograms out in the upstairs attic in ORL, breathing benzene and saturated phenol papers and so forth, and there have been several deaths from liver cancer that...

**VM:** And you think some of those originated from that?

**CF:** It was an era when, not just in that lab., but a lot of other people. Stanier died of this, Stan Carson, an assistant of Van Neil. A lot of other people have. I’ve been lucky, I guess. In any event, so the project with bacteria was a start for me because I spent my career with photosynthetic bacteria after that and it was absolutely wonderful. Dr. Stoppani came to visit me and went back as head of the Biochemistry Department at the Medical School at the University of Buenos Aires and went on to be an elected member of the Academy of Sciences and then was President of the Argentinean National Academy of Sciences when he retired just a few years ago. I visited there with him. So that Calvin connection was...people out of Calvin’s lab. just became distinguished; they were distinguished when he got them. Calvin was amazing at selecting his people — he selected winners.

**VM:** At the time you went there, there were the two separate locations, in Donner and in ORL. Who were your colleagues, who were your senior colleagues in ORL?

**CF:** Andy Benson; Al Bassham wasn’t really senior as much but had been there in Calvin’s lab. as a grad...

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**VM:** He wasn't a post-doc., he was a staff member.

**CF:** He was a staff member. Andy Benson. A fellow by the name of Bradley.

**VM:** Dan Bradley.

**CF:** Dan Bradley. He was on the staff; stayed there quite a while. Those was the core in ORL. Of course, over in Donner the people at that time were Bert Tolbert, who headed that sort of group, Dick Lemmon. But we never had any real scientific interaction. That is, they never taught me the technology or...

**VM:** But you knew the people well enough.

**CF:** Absolutely. We met at the Friday morning meetings and Calvin was always there and one of the people had to speak. It was there that Calvin first scared me, at the very first couple of meetings I went to, in that he would just hammer at the speakers and so forth and only in retrospect do I realise he was doing, as we've heard, I think he was kind of trying to get the very best out of you. He couldn't care less what you thought of him. He wanted to dig out the best and his technique was to beat on you and make you go over and over, and in utter exasperation I used to, ugh, almost dread it. But he had a method and it was terrific. In my early days in that lab. there Calvin did expect you to work all the time. Now I had a wife and two kids, a third one on the way, just bought a house, I wasn't a post-doc. but was on the staff: didn't make any difference. I perhaps disagree with Goldsby a little bit on that about the senior people. One of the examples would be Sunday morning at eight o'clock the telephone would ring at home and it would be Melvin for me. My wife would usually answer the phone because I was usually asleep and perhaps had had a little too much to drink the night before and partied up a bit. Melvin would simply say, "Clint, I've just been reading over your notebooks and I want you to come down to the lab. right away." And there went Sunday morning. And very probably he would call somebody else who was working with me, maybe Stoppani, and we would be there and go over the books and it was incredible.

**VM:** So, in your experience, he went over the experimental material in sufficient detail so that he could absolutely talk nuts and bolts about it.

**CF:** He was on top of everybody's work. It was incredible, the amount of material he could absorb.

**VM:** So your main base in ORL, then, was that on the upper floor where the algal cultures were?

**CF:** Yes.

**VM:** And at the point when you were there, what was the way in which the algal cultures were grown?

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**CF:** They were grown on his shakers that were designed by Calvin and made by his glassblowers, which were water-cooled shakers with Lucite bottoms in the tanks. They were mechanical shakers with...

**VM:** These were flat-bottomed flasks, rather shallow?

**CF:** Flat-bottomed flasks with ground glass tops and you could bubble air or nitrogen or CO<sub>2</sub> through and when I got there that was it, that didn't change. It got cleaned up and we kept things like sodium azide in the water simply to lower the contamination and process and treat it microbiologically. So that's where the algae were grown and harvested.

**VM:** You would be interested to know that one of those flasks survives. It is in a closet in Berkeley. I'm not sure of the fittings but the glass vessel is there. By the time I got there several years after you started, they were also growing them in continuous culture with fluorescent tubes and vertical tubes. That wasn't your development?

**CF:** No. The steady-state apparatus was just being developed at that time by Al Bassham who really worked on that and developed the whole thing. And, of course, that was the famous story of the fisherman came out of the development of that steady-state growth apparatus with inserted tubes and large 25-gallon bottles.

**VM:** Is this the time to explore the fisherman?

**CF:** Well, yes. In 1954, I believe it was, Al Bassham had been working very hard to develop a so-called steady-state apparatus where the algae would be exposed to CO<sub>2</sub> continuously, which was generated in a completely sealed apparatus. And then we changed environmental conditions. If you deprive them of CO<sub>2</sub> and look at the transients, what happened, what would pile up? It was the time for the hunt for the C<sub>2</sub> acceptor. The steady-state apparatus essentially allowed you to make these environmental variables which would show what was happening to carbon when you took out key pieces. And it was very complex and very important and it was really the breakthrough where the path of carbon was once and for all — that apparatus did it. But it was complicated. It was one piece of glass machinery. You had places to inject bicarbonate in tremendous amounts because we would use up to, it seems to me, 5-10 millicuries of C<sup>14</sup> because it had to be circulating gas at very high activity because they (*the algae*) were only taking in a small amount during the exposure. But anyway, it was very complicated and Melvin wanted a sketch of it drawn up to put in a publication. It was the *Path of Carbon in Photosynthesis*, I believe 22 or 23, whereas the one that was all yeast was number 1.

**VM:** It was neither 22 or 23 because I wrote both of those. It was probably 21, I think.

**CF:** 21, OK. That much had happened since 1948. But anyway, Alice Holtham, the secretary, artist, general house mother in ORL — she was sort of Marilyn's duplicate but she was everything from secretary, artist, manager, just a wonderful person — and she drew up this apparatus, very, very carefully.

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**VM:** So somebody sketched it for her to do a proper drawing?

**CF:** Yes, that's right. And Melvin followed it along and Alice would have it and so forth. And it got to be so complex and everyone said and I said and she would show it to us in the lab. and say, "Does this look OK? Melvin hasn't seen it yet" She had it down pretty well but we said, now look, when this nice big picture gets reduced down to one column in the *Journal of the American Chemical Society* nobody with a magnifying glass is going to be able to read it to see what it is all about. It is sort of useless. But Melvin was adamant, he wanted it published. I suggested (Andy was in on this, too, because he knew Alice pretty well and could get this), let's do a little doctoring for the fun of it. Alice was a little hesitant about this, I think. So the lovely main tank with the bubbles of CO<sub>2</sub> coming into it, right on top of the tank, we had Alice sketch a little stick figure of a fisherman with a fishing pole and a long string with a hook going down and a goldfish, right where all the CO<sub>2</sub> bubbles were. It was wonderful. People in the lab. got a kick out of it. But when Melvin came over to look at it he set it down on the round white table and everyone was around talking. I don't know how many people in the lab. knew about it but Melvin didn't. He very carefully traced the whole thing out, looked all through, went right over the fisherman, never flinched, never saw it, I am convinced now. So it was submitted to JACS and the first thing we knew everything was fine and it was published that way. I think it was a first for the JACS to have somebody fudge with one of their figures, not even a reviewer picked it up. (*Laughter*)

**VM:** Do you know what Melvin's reaction was when he must have realised or somebody showed him?

**CF:** He was delighted.

**VM:** Was he?

**CF:** Yes, he was absolutely delighted. I think he would have been a little upset if one of the reviewers had said, "Don't fool around with that," or something. Back at his reunion in 1991 (*Editor: actually 1989*), or whenever it was, in Berkeley I showed that slide and it brought down the house again and Melvin was the one most pleased to see it.

**VM:** I would have thought that would have generated a tradition of jokes in papers but I don't recall that there were any others.

**CF:** I recall another one; it was great. When we landed on the moon in America (*sic!*) there was an article published in *Science* called the "Composition of the Moon" and they gave all the metallic substances in kinds of rocks by sound transmission, as they did in those days. And the very last column in the table was Gruyere in sound transmission. Right there. Which, of course, is Swiss cheese and the moon is made of Swiss cheese and that got by; it's a similar kind of thing.

**VM:** Yes. I think the very dry jokes are the ones you want, the ones that slip by, you hope, slip by...

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**CF:** I was sort of worried if Calvin knew I had participated in this. But that goes to another little incident in the lab. like this when...everybody knew in the lab. that Calvin was down the pipe for the Nobel Prize. That was right at the top of his agenda. He knew he could do it, we knew he deserved it, the brilliant man that he was. He hadn't really accomplished what he wanted to accomplish and that's why to complete the Path of Carbon was so important. That is what he was banking on. He would come in for the results every morning with Marilyn, at ten o'clock, with Marilyn around the table at ten o'clock. I set up a little flag on a ring stand, tall ring stand. It was a pennant with one bell on it and a little label on it said, "This is the Onebell Prize which is much more valuable than the Nobel Prize". We had a little plot in the lab. At first we wanted to put it out on the white table so he would see it in the morning but we decided that was a little direct so we had it sitting off to the side, but there, and every day we'd raise that pennant to half mast or up close to the top or drop it down to the bottom depending on how close it was to the Nobel Prize. But we said we would give him the Onebell Prize anyway as something to shoot for. He finally saw it and sort of smiled but never reacted very much.

**VM:** I wonder whether after that he came in and surreptitiously looked to see whether...

**CF:** (*Laughter*) I bet he did.

**VM:** ...and where he would have thought how you would assess his chances on that particular day. You think it was very much on his mind and he was...

**CF:** He never talked about it at all but you can see afterwards that half his perspective book is on the Nobel Prize, the ceremony, his family, and he really was...he never talked about it directly but he was very sensitive. He would talk about other people's Nobel Prizes because he was surrounded by them in Berkeley — they were getting them *en masse* at that point.

**VM:** I am sure that the time when you were there in the early '50s was a little early because the cycle had not been resolved. By the time I was there, three or four years later, it had essentially been worked out and I remember there was a measure of tension each year around November time as the results...until finally fruition and joy...

**CF:** Yes, 1960, right (*Editor: actually 1961*).

**VM:** ...and it all worked out.

**CF:** And President Kennedy.

**VM:** Well; so you were involved...we talked about your science as far as your involvement with Stoppani and you were working on the *Rhodospirillum* with him. And then what; I mean, how did it develop?



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**CF:** Then we got into...this is an interesting collaboration with Dan Arnon. And I think we know the story of Dan's relationship to Calvin and vice versa.

**VM:** Well, I don't know to what extent you can actually illuminate it. We know there is antagonism, of course, between them. We have not been able to discover how it started.

**CF:** I don't know. My guess is that it was straight personalities. They were both, in very different ways, extremely driven people with high focus and they came head on and I really can't answer your question except that they were not on speaking or communicating terms. But they didn't mind if we...he didn't mind if we... And that was the time when Arnon isolated the chloroplast that could do photosynthesis for the first time. My next project was one that has been described by Andy Benson and involving what went on as a result of the beginning of the steady-state experiments and so forth. I had done some training in enzymology as part of my Ph.D. thesis and knew my way around enzymes a little bit and that was the time when Andy had finally convinced Calvin — and believe me it was blackboard sessions, Calvin's driving — that  $C_5 + C_1 = C_6$  divided by 2 =  $C_3$ .

**VM:** Can I pursue this a bit because...

**CF:** That was finally accepted.

**VM:** Right. You were there at the time there were arguments about pluses and minuses, 3s and 4s...

**CF:** Transketolase, transaldolase all the movings and  $C_2$ s.

**VM:** Can you remember how it started? Can you remember...

**CF:** Yes. Andy Benson. He should have shared that Nobel Prize. Andy was enough of a biochemist...that was the time that a number of biochemists around the country (enzymologists) — Bernie Horecker at the National Institutes of Health...

**VM:** Racker...

**CF:** ...Efraim Racker — were working on transketolase and transaldolase,  $C_7$ ,  $C_5$ ,  $C_2$ , moving around. Andy knew about this. I knew about it. And so Calvin would often, when he left the round table, would bring his staff together in the little glassed-in office there where there was a blackboard which there wasn't out around the round table in that lab. He'd spend a lot of time (Marilyn would go on)...he'd spend some time with us and that's where he would really — it was worse than the Friday sessions — he would really tear us apart. And talking about transketolase and transaldolase with Calvin didn't mean anything to him, or he wasn't quite sure how the  $C_2$ s got in I repeated a minute ago. Andy finally said, I remember in exasperation, when he told about transketolase and there were  $C_7$ s and  $C_5$ s, and Andy popping his fist back equally to Calvin and it got very hot(?), not personally but very.....and said,

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“Melvin, don’t you realise that  $C_5 + C_1 = C_6$ , divided by 2 =  $C_3$  and that gives you 2 molecules of phosphoglyceric acid, only one of which will be labelled. And you know Martin Gibbs’ work on asymmetric labelling.” You could just see Calvin — it fell into place and at that point he disappeared, went back to his office. Two to three hours later he came back with the whole mechanism of the carboxylation of ribulose worked out and how it was an aldo-ketol transformation and so forth. That was the genius of the man. He could put it together like we couldn’t.

**VM:** When Andy said  $C_5 + C_1$ , was it clear...did you all understand what he meant by  $C_5$ ?

**CF:** Oh yes. Absolutely. We had seen ribulose diphosphate piling up and we knew...

**VM:** You knew it had to be ribulose diphosphate?

**CF:** Yes. But Calvin had the mechanism — we weren’t sure about the mechanism and how you would do it and why you would get asymmetric labelling and all that sort of thing but Calvin had it all right down.

**VM;** What about the other playing around with these numbers in all the intermediate rearrangements in sedoheptulose and the xylulose and the erythrose?

**CF:** From there on it fell right together and it was primarily Andy and Calvin that put it together. The whole Path of Carbon is published here.

**VM:** Oh, yes, I realise that. I came in too late to have heard much of the early discussion about that. It was all pretty well accepted. But this was Andy and Calvin between them working out the permutations of transaldolase and transketolase and what they could do with it?

**CF:** Andy had to help Calvin with this, yes. Calvin would often come over to our glassed-in cage. I had a desk with Andy in there and that was the case. But this all started with collaboration with Arnon and enzymology. When Arnon had gotten this chloroplast reaction, we knew there was a ribulose diphosphate carboxylase enzyme. Calvin had worked this out. Arnon had been isolating chloroplasts and they were special chloroplasts and only Arnon could do this properly and so forth. And that bugged Calvin, I’m sure. From higher plants using spinach and Calvin had not done any higher plant work at that time, I don’t think. So, by gosh, my contribution here was if you did all the photosynthesis then our new enzyme, which we had just submitted for publication (Rod Quayle, myself, Benson and Calvin), is going to be in that chloroplast. Could we go down to Dan Arnon’s and get him to prepare chloroplasts with him? Feed them hot  $CO_2$  and see...just a cell-free preparation, enzymatic reaction, put in the substrate, ribulose diphosphate which we eluted off chromatograms and turn on the lights.

**VM:** And you would have expected ribulose diphosphate...?

**CF:** I would have expected PGA.

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**VM:** But you would have expected RuDP to get into the chloroplasts?

**CF:** Into the chloroplasts, yes.

**VM:** Whereas, of course, it wouldn't get into the cell.

**CF:** Exactly.

**VM:** So *he* wanted to do that experiment?

**CF:** No, *we* wanted to do that experiment.

**VM:** Oh, that was not his suggestion that you work with Arnon?

**CF:** No. But he said, "Well, if you want to waste your time, yes." We told him about it and that we wanted to do it. That's a direct quote. And then he turned around and walked away — typical. So, there were two other post-docs. who were good friends of mine. One of them was one of Van Neil's students working with Arnon, Marybelle Allen, and Bob Whatley from Oxford.

**VM:** Yes, I've been in touch with him; we'll be seeing him.

**CF:** Well, remind Bob of our visit, Andy's and my visit down there. It was marvellous. We had the ribulose diphosphate in little vials, eluted off chromatograms, and we had hot CO<sub>2</sub> sealed up with a little syringe ready to shoot it in. And we went down to Arnon's lab. to do this experiment. and Arnon had, you know, the white towels laid out all over the bench. Marybelle had the mortar and pestle and the acid-cleaned sand, and Andy and I were down there with the stuff ready to go. And Marybelle and Bob, the four of us, lined up when Arnon, in a pristine white coat and so forth, marched in with a tray full of spinach that had been kept in the cold room to be nice and crisp. And the procedure started. It was ceremonial, absolutely incredible. Marybelle and Bob dumped the leaves in the mortar and pestle, someone dumped the sand in, Arnon grabbed the pestle and ground the chloroplasts (*Editor: this should be "spinach"*), they went to the right centrifuge with the right speed with the right head on it, spun down the cell debris, they got beautiful dark green colours — pure chloroplasts. We suspended this in a nice buffer, a weak buffer because you don't want to foul up chromatography, and covered everything up and it was done in the dark because this was an enzymatic reaction — we had a substrate and so forth — we shot it in over a period of time, took out samples and shot them with alcohol and killed it, took the killed things back up the hill and ran the chromatograms. Well, Melvin knew about this all right, that we had done it. "When are you going to develop those chromatograms, Clint and Andy?" I was working very close with Andy on this and: "Melvin, we know we only fixed 300 counts." You know, we are doing a cell-free preparation and it was in the early days of cell biology, believe me. And we said, "It's going to take two weeks, Melvin."

**VM:** For the chromatogram to sit on film.

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**CF:** Yep. So one day when Calvin came in Andy said, “Yep, it’s been two weeks. Let’s go up and develop the chromatogram.” So Andy and I and Melvin went up to the smelly room and Andy (*indecipherable*) took it out and clipped it onto the things and dipped it into the developer. And you know the white background on the film and you pull it up and down and keep looking and, in about a couple of minutes in the developer, up he pulled and there in the lower right-hand corner was a single spot just where phosphoglyceric acid showed. You’ve seen pictures of that chromatogram — famous. Melvin cheered before we even counted it, anything, and dashed out — we were afraid he was going to expose the film (*indecipherable*) but we had it in the fixer by then — and came back down to the lab. later with a short note prepared for a submission to the Journal of the American Chemical Society: Quayle — no, not Quayle; Benson, Fuller and Calvin, I believe, something like that. Maybe some other authors; I just don’t remember because it never got published that way. Not a mention of Arnon in whose lab. we had done the experiment).

**VM:** Go on. What happened? Did you guys agree to that?

**CF:** Well, we were sick. Al Bassham was involved with Andy and I. Al and I went over and had a long talk with Bert Tolbert, “What the hell do we do, Bert? Calvin wants to publish this without even recognising Arnon.” So we called up Marybelle and Bob and said this was going to happen and they said, “Dan has done the same thing without mentioning Calvin.” That’s a true story. And so...

**VM:** But did Dan have...? Dan hadn’t run chromatograms?

**CF:** No. We ran the chromatograms but he had put our names on it.

**VM:** Oh, I see.

**CF:** But it was just a draft; both of them were just drafts. Calvin gave us the written stuff with a blank space for tables so we could fill in the data and told Andy, Al and me to do that. We felt awful. I was scared to death. However, Bert was very good; he said, “You better go down and talk it over with...and have Marybelle and so forth talk it over with Dan and I’ll talk it over with Calvin“. He did. Calvin just showed up the next day and said, “I think I’ve found a better place to publish it and we’ll take our time and it came out as one of the *Paths of Carbon*.

**VM:** Again without mentioning Arnon?

**CF:** Yeah, but he separated it out and put it as a separate piece of a large paper when...No, it was a paper. This involved the beginning of the departure of me from that lab. I am probably skipping over something. What he did was submit it without our knowledge, Andy’s and mine, an abstract to the American Society of Plant Physiology was meeting in Gainesville, Florida the spring of ’54. He had entitled that “The Enzymatic Carboxylation of Ribulose Diphosphate with Carboxylase to form Phosphoglyceric Acid,” not mentioning too much about chloroplasts but using some steady-state data. Just an abstract, OK. And Andy and I saw this and we were both pretty annoyed because our names were on it and we hadn’t even seen it. Calvin and I

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had had some brief discussions before because I essentially wanted to get on with my own career. I didn't want to become a member of Calvin's staff and I don't think he did, too. I was very roughshod and young and naive and pretty... Well, I didn't want to take that kind of environment without blowing it myself. I would react to Calvin like I shouldn't react to Calvin and so forth.

We pretty much had decided he would help me find a job. The next step in this, as a result of this paper, he said, "Clint, I can't possibly go as a plant physiologist to this meeting in Gainesville. Would you like to go and present this paper?" And I said I would be delighted to; both of us were. I was looking for a job and it was a good place for me to look. And there was nothing difficult about this. It was marvellous how Calvin could be very helpful and compassionate and I felt great about it. He said, "Don't your folks live...(this was the real humanist in Calvin)...don't your folks live in the Northeast somewhere?" And I said they lived in Providence, Rhode Island. And he said, "Why don't you just hop up from Florida to go to New England to visit on the way back to the lab?" He said "I know some people at Brookhaven National Laboratory and we can send you up there. Martin Gibbs is there and is doing all this nice asymmetric work and I'd like to have you talk to him and see what is going on." So on my way up I stopped to see Marty. It was the year of one of the hurricanes, the '54 hurricane, one of the worst hurricanes they ever had, and I was marooned at Brookhaven and couldn't get home across the sand(?) to my parents. So I spent the weekend with Marty and had a great time. I spent the next day, on Monday, there and Marty had the head of the Biology Department call me in (whom he had talked to, apparently, on the phone). Marty wanted me to come to Brookhaven and they offered me a job and I got back to Berkeley, talked it over with Calvin and he said, "Great, Clint. You can launch your own career in microbial photosynthesis and I think that's wonderful."

**VM:** And you wanted to come back to the East Coast, as well, did you? Or that wasn't so important?

**CF:** I don't know. Sure it was important. I mean, we loved California, we really did...I don't know: it was a chance for me to go out on my own and do what I thought I could have done with the wonderful training I had had from George Kidder at Amherst and Ed Tatum at Stanford and Melvin Calvin at Berkeley. And Melvin, more than other people in a practical sense was after my Ph.D. when the pressures were off, really focused my career, which I spent up until 1986 purely in photosynthesis and microbial photosynthesis and published 150 papers in this area and got to be President of the International Society of Photosynthetic Prokaryotes, held the triennial meeting here at Amherst a number of years ago in 1991 with the biggest Russian delegation which ever came and eventually led, I think, to my being awarded an honorary degree which I am accepting next September in Moscow.

**VM:** Well, there are other things I would like to talk about now that we are talking about your career after you left Berkeley, let's complete it..

**CF:** That's how I left Berkeley to go back.

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**VM:** OK, let's complete it. So you went to work with Marty Gibbs for a while at Brookhaven.

**CF:** Actually I had an independent appointment there but published a couple of papers with Marty. I was right next door to him. Those were the wonderful days. The Atomic Energy Commission, now the DOE, was running the lab.; they set me up with a technician, money for a post-doc. and all the equipment I could want. Calvin was very helpful in getting people interested to come to work with me and I had summer visitors, very distinguished ones — Brookhaven was a big programme — I was surrounded by people like Dan Koshland and Arthur Kornberg in the labs. upstairs.

**VM:** They were there at the time?

**CF:** Yes. Arthur Kornberg worked with Dan Koshland every summer there. I got to know him very well. He's going into the phosphate polymer business. That's full circle. He is very much interested in polyesters which are cellular accumulations with DNA and RNA phosphates. It's a long story but I talked to him recently on the phone about common work and so forth.

**VM:** When we talked at lunch you mentioned that Marty Gibbs' observation on asymmetric labelling in glucose was very important. Could you amplify why you thought that was the case?

**CF:** Well, if you were looking for a  $C_2$  acceptor, a single one, which was Calvin's main drive and push, and you picked  $CO_2$  on the end to...we were looking at things like glycine, glycolic acid, you'd end up with (*indecipherable*)  $C_3$  in the carboxyl group of phosphoglyceric acid. If that was put together you would find  $C_4$  into a  $C_6$  and  $C_3$  equally labelled.

**VM:** That was their idea, wasn't it?

**CF:** Exactly. However, Marty Gibbs came along and found that at very early times, working with *Scenedesmus* and other things.... (this was when I was working not in Marty's lab. but next to him. He finished it then; he was working on it before I went there and that was why I was interested in getting there, I think)...that it was Bennett (*or Dennett; spelling not clear*) and Bernie Horecker was there, who was a great man on sugar degradations and transketolase, and he showed clearly that in early times that you recycle things and find  $C_6$ , let's say, and 60% of the carbon would be in  $C_3$  and 30% in  $C_4$  and the rest of it scattered around. It was definitely not symmetrically labelled. But if you only labelled... if you labelled the ene-diol section 2 position of ribulose diphosphate and added carbon to that (of course the ene-diol reaction which Calvin came up with a brilliant mechanism of) then you'd find only the  $C_3$  of the product labelled and the  $C_4$  would not be labelled at zero time. So that's why it was important. OK? Is that clear?

**VM:** Yes, OK. There is another point that I wanted to ask you because you've mentioned it several times about people calling Melvin by his first name. When I got there in '56

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no one was doing it, nobody at all. Andy, of course, had gone by then. You had gone by then. But you distinctly remember that you and Andy called him by his first name at that time?

**CF:** Oh, yes. And everyone in the lab. was, too.

**VM:** Everyone in the lab. was?

**CF:** I think so. Well, maybe people from Europe weren't. Jacques Mayaudon, I remember, always called him Professor Calvin. He was 40 years old and a relatively young guy and in American universities I always called my Ph.D. mentor by his first name and my undergraduate advisors. It is somewhat of a shock to me that times have changed and particularly our European colleagues. I like being called by my first name but I finally gave up because in the American university, after you get to be 65 years old, your undergraduates don't feel comfortable calling you by your first name.

**VM:** It is simply the fact that not very long after you left it was no longer the practice to do so. It must have ended with the people, your generation of people, who left at that time. It got resurrected many years later...

**CF:** In the 60s, yeah.

**VM:** ...when Mel Klein and I decided that this had gone on long enough and we must call him Melvin. But we noticed that at that time that we were the people who had not been his students and those who had, or close to his students, like Al and Dick and the others really felt uncomfortable calling him by his first name although now they do so. But it took them a long time and they weren't prepared...

**CF:** I guess I was brought up with it with Ed Tatum, who is just as distinguished as Melvin.

**VM:** So the other point I wanted to ask you, and I don't know whether you were there at the time and whether there is anything much you can add, but it has never really been clear to us why Andy left. Do you know why Andy left?

**CF:** The same reason I left. Andy — I know this for a fact because Andy and I had long, deep and heartfelt discussions about it — Melvin just for some reason wanted his staff to be his staff in a classical sense and he got some very brilliant people. It depends a lot on personality. Now Al Bassham, for instance, has a personality that just loved that. He loved the permanence, the exciting science he was doing, he was compromising with nothing.

**VM:** I must stop you there because the tape's about to run (*out*).

*Tape turned over*

**CF:** A lot of it was personality, too. I have compared Andy's and Al's personality and my own. My own was (*indecipherable*) even more extreme, perhaps. Andy was a

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brilliant, creative guy. He was interested in breadth, much broader than Calvin's interest in breadth, but he wasn't quite as good as Calvin and, therefore, he didn't have the group of 40 Ph.D.s working at it, and so forth. But he'd like to try, what I would call in a positive way, 'flighty' kinds of experiments on strange situations. Melvin didn't like that — "where was that leading Andy?", you know. And Andy liked to be independent, intellectually. He had some personal problems, which are really not relevant, but when one has personal problems it means a move sometimes is called for. That just was an additive factor, I'm sure. He had an offer at La Jolla which was just Andy's world. They were a bunch of dreamers down there and marine biologists were into all sorts of strange things. Andy got into the mango plants and the underwater business and he just leapt at it. It was about a year after I left, I think. He belonged independent, as I did, very cordially.

**VM:** Well, it happened again to other people and I was one of those people also who left for exactly those reasons and some others did. When you commented about Melvin's attitude not being the same as Andy's in terms of chasing off in all sorts of directions did Melvin actually try and stop Andy doing that or did he just wash his hands of him? What was his reaction when Andy did it? After all Andy was a grand man and the two...

**CF:** I think that deserves our attention (?). I never saw what I considered an important disagreement between Melvin and Andy that wasn't science. You know, that was Calvin's technique. I knew Dan Bradley also left for the same reason about the same time, as a matter of fact. I'd be dishonest if I said exactly why.

**VM:** I suppose at that time the carbon cycle and, therefore, Melvin's reputation, was not cut and dried and firmly established.

**CF:** That's right.

**VM:** It was on the border and Melvin, perhaps in the light of what you said earlier, was really very keen to get it wrapped up.

**CF:** I think he was, I know he was pushing very hard.

**VM:** And he wasn't terribly sympathetic with the idea of doing other things. Because once it got wrapped up, and there was a big final discussion in '58 which was the last of the big arguments, around Otto Kandler and the labelling pattern. After that it clearly got very different and people then shot off — including Melvin — Melvin also shot off in all directions after that.

**CF:** Absolutely.

**VM:** But I see the point that some years earlier, when things were not so resolved, he was less keen to dilute effort.

**CF:** But he never held it against any of us. He just was so helpful and I just have so much respect for the man over the years. There is a marvellous story, getting back to



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Amherst, and maybe this would be a good place to end. Calvin visited Amherst several times before even Dick (*Goldsby*) was here. But before that, I'm trying to think exactly when it was. I was at Dartmouth; it was the middle '60s.

**VM:** Had you gone there? OK but we must come back to your career because we didn't finish it. You were at Dartmouth.

**CF:** We finished it at Calvin.

**VM:** Right; you were in Dartmouth.

**CF:** I was at Dartmouth Medical School sometime in the middle '60s; trying to remember the sixties. Anyway, Calvin had been invited to give, I believe, a Sigma Xi lecture at Amherst College. George Kidder was there at the time; he knew I was close associate of Calvin and asked me to come down, which I did. So the only connection with Amherst was Kidder. Kidder probably initiated the lecture before Blankenship and Dick Goldsby were around. I came down and he gave the lecture. It was very interesting because it was held in Johnson Chapel, which is a famous place. And Calvin always wanted to come back and give his other lectures, which he did, in Johnson Chapel. It is an old 18th century chapel on the Amherst campus, beautiful, with the white boxes, you know, all the colonial-type thing. He gave the lecture there and there were several members of the , there because it was called...the title of the lecture was called "Chemical Evolution and the Origin of Life", it was that period of Calvin's time. So Calvin gave his wonderful talk. If you've heard it you know how everything just fell into place and life, DNA, life was there. It was the early days of the DNA revolution, , of course and so...At the end of the lecture, during the question period, one of the members of the clerics stood up and the President of Amherst College, who was chairing the session, pointed to him and said, "Father so and so" and the gentleman of the cloth said, "Professor Calvin, you've given a wonderful lecture and it's marvellous but as a man of God tell me, what have you left for God to do?" And Calvin, in an instant flash: "God sent me here to tell you about it." (*Laughter*)

**VM:** And how did your cleric react?

**CF:** There was just this deadly silence. He stunned everybody and then tremendous laughter and cheers. Ego, ego, ego.

**VM:** To backtrack, I'm sorry, let's come back to your career. We left you with Marty Gibbs...not with Marty Gibbs but with the company with Marty Gibbs...

**CF:** Then I had the beginning of my English experience. I had a...I got a National Science Foundation...Brookhaven National Lab. was run by a bunch of universities and they had a sabbatical system. I had been there six years and so I applied for a National Science Foundation so-called Senior Fellowship (I was 30 something at the time), Senior Fellowship award which would pay for you and your family to go across the ocean in the big boats in those days (I was 40, I guess, and child care wasn't as well known) but anyway I had known Hans Kornberg, of course, at Berkeley, and Rod

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Quayle at Berkeley, they were both at Oxford and they somehow or other talked Krebs into sponsoring my fellowship. So I arrived at Oxford *en famille* on the boat, *SS United States* at...dear, my geography fails me...

**VM:** Southampton, not Dover, nor for those...

**CF:** Southampton, and was met by my little red Hillman Minx station wagon, and the driver jammed four kids in the back seat and it came with a luggage rack and off we went to Oxford. We had a wonderful experience. We lived in Queen's College Playing Fields which was the old leper hospital outside the walls of Oxford, a 13th century building that had been made over into a residence. And we had a wonderful year with Krebs and Krebs' lab. We did a grand tour of Europe with all our children. Had a wonderful English story to end the grand tour. When we went back from Norway, from Bergen, Norway, we took the boat to England and one of my kids got sick when we were in a lovely place called Gol in Norway and we suddenly discovered he was covered with spots. We didn't know what it was except we remembered that in school in Oxford (he was in a pre-school or something — he was four years old), there had been a lot of chicken pox. And we had other kids and thought, oh my God. Anyway we got in the car and drove to Bergen where we had a reservation and we stayed there for three or four days while he got over the problem. But he was a mess, a scabby mess. But we had to get back and he was no longer contagious and no longer had a fever so we made reservations on the boat to go to Newcastle, overnight, I think it was as I recall. And we got on the boat which was loaded with English tourists going back to Norway (*Editor: presumably this should be "going back to England"*) and we were standing in line, and as you might image, you don't know my wife, but we have four very, very Norwegian blond, blue-eyed kids.

**VM:** Is your wife Norwegian?

**CF:** No, but I meant that we were in Norway at the time and they looked very Scandinavian. This very nice chap was in line with my wife — she had two kids, I had two kids in another line, something like that was the way we worked it, but she had Jonathan along with her with his scabby appearance. And this chap said, "Oh, are you from Norway going back to England?" And she said, "Oh, no, we're Americans and we're just on our way back to England because I have a child who got sick." And the chap looked at him. And my wife said, "Well, he's fine now but did have an awful bout with small pox." And the chap just looked at him, looked at her, looked at him. My wife, of course, had completely misspoken. She had meant chicken pox. And he said, "My, I didn't know they had that in Norway." And she said, "Oh, no. He got it in England." (*Laughter*) There was sort of an empty spot in line. Carol said, "This man was so strange." When she told me what she had said, I said "Oh my God" and dashed over and told him it was just chicken pox and he said, "Well, I thought it must be something else!"

So we got back to Krebs' lab. and spent the rest of the year there. I came back to the United States (*indecipherable*) but while I was still in Oxford I got a telegram from a chap in Hanover, New Hampshire, who signed the telegram "Dean of Dartmouth

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Medical School.” My first reaction was that Dartmouth doesn’t have a medical school. But he asked if I would be interested in becoming Chairman of the Microbiology Department. You see, I would become a microbiologist again. And I said, “Of course.” I was looking for an academic position so he flew me back from Oxford and interviewed me and one thing led to another and he offered me the chairmanship at Dartmouth Medical School, which was in crisis because it was being disaccredited by the AMA and they had to shape up and they hired six new department heads, which I was one of...

**VM:** That was in the early '60s?

**CF:** That was the year after Krebs, '61 or '62.

**VM:** How long did you stay there?

**CF:** I stayed there through 1967 when I had another sabbatical. I went to the University of California at Riverside to do a sabbatical and during that period of time...yes, during that period of time, I got another approach while I was at Riverside — would I be interested in coming to the University of Tennessee in Oak Ridge where they were setting up a new graduate school for biomedical sciences at Oak Ridge National Laboratory, which the University would have but this would be a graduate school because the then chairman of the Atomic Energy Commission, Glenn Seaborg, had chided the people on the Atomic Energy Commission, including Alvin Weinberg, who was head of the Oak Ridge National Lab., that you people at the unex of science, you use it all but you don’t produce any of your own. And that led to the University setting up this graduate school to produce Ph.D.s at Oak Ridge, which I was the Director of for five years or so and the next step out of the blue I was offered...that was the best of all worlds, except for living conditions in Oak Ridge. I had the best of my National Lab. advantage, a faculty of 100 distinguished people to start a new school with and so forth. But living in the Southeast, in red neck country of Tennessee was not for my children and me, and I got an offer as the Chairman of the Biochemistry Department, a new Biochemistry Department in building here at the U. Mass.

**VM:** When was that?

**SM:** '73?

**CF:** No, it was 1971-72.

**VM:** And you stayed here for the rest of your career until you retired, you said, in 199...?

**CF:** 1991. And now I have a full-time active research lab. in polymer science and engineering that originated back in Melvin’s influence again, splendid, in Berkeley.

**VM:** One last topic to finish this up. This question of ORL. Everybody, you know the argument, everybody who lived in ORL thought it was a special place.

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CF: It was.

VM: Why was it a special place?

CF: You know atomic energy had its initial cyclotrons there and so forth and we were very well aware of that because we could read the background. But that, in itself, is sort of special when you know the whole Lawrence story and it meant a lot to us in those days. It had its origin in atomic energy and that was a very big thing — the bomb — to our generation and the generation of the young people who were there were very much World War II products and, you know, we survived by not going to Japan, and the bomb and all the mixed feelings people have about that way to end the war. I had to say it, I wasn't too mixed...

VM: Do you think the building, do you think the structure of the building, was an important factor in the way the group operated?

CF: Yes. It was so cramped, everyone was thrown together and the design of the Calvin Building, of course, arose from that and, I guess, from the set-up in LSB, too. And the discipline of Calvin's lab. was the building, the physical building fitted his *modus vivendi*, which became our *modus vivendi* whether we liked it or not if we were there for the long term or not. And just getting together in the lab. where you had a bench coming out and we were working and Calvin would come in with Marilyn and you were right there, the table was right there, and you would drop everything if you could and you would surround...and that would be it. Our offices, either at our bench or our visitors' were at the benches, and Andy and I and Al had a shared office in the glassed-in area and we were stumbling all over each other. And the people there — I might to ask — it was a mixture, an international mixture — and everyone knew that building, from Japan, to Germany to England, they knew what it was for, they came knowing what it was and they fit into the spirit of the structure, almost. I sound a bit mystic but I think we all came away feeling, it's unanimous, we all came away feeling that way and I know down in the pits where we stored the chromatograms, that awful place — we used to go down there — and then just how horrible it was. Up in that chromatography room and the background counts from the old...were frightening. But also the spirit of adventure, too, that was part of it.

VM: Yes, it was partly that. Clearly, the people and the youthfulness of the people and the sense of unity in the group.

CF: And the building was all part of that because it was a tough time in the '40s and '50's. It was after the war and nothing like you people suffered, but nevertheless jobs were difficult to get.

VM: So, last, last thing, how well do you think the new Round Building recreated it in a modern sense?

CF: Well, my one visit both there to Melvin's office and to the whole three stories of the round structure — I don't think it ever can really replace...It works, it's Melvin's system, it works beautifully.

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**VM:** It worked while he was there. It doesn't work any longer.

**CF:** Well, right. That's because it was designed around him. But, I think Melvin perhaps made a mistake there. He couldn't redesign the intimacy and the spirit of adventure and the things you had to do to put up with that damn building sometimes. I don't know, just standing out on the steps with our deerstalkers on brings back a lot of memories. Those steps were important.

**VM:** OK, well I think you have other things to do...

**CF:** It's your pleasure but I think we can all think of things but they can't all be there.

**VM:** We are most grateful to you for a very entertaining hour and again helping to complete the story.

**CF:** Well, I am most anxious to see what comes out this. And I certainly said all the things...I got the God story in and that's so typically Calvin.

**VM:** OK. Thanks a lot.

**CF:** Wonderful to see you and now that I'm free to travel again...I have another colleague, a very close recent colleague...I have many colleagues in England but someday I hope to see Rod Quayle again. I saw him in Atlanta; he came to a meeting in Atlanta a number of years ago as an invited speaker and Otto Kandler was there at the same time, so I see these people.

**VM:** Well, he's alive and well, and we've seen Rod and we're going to see Malcolm Thain who's another guy with a deerstalker hat.

**CF:** Say you talked to me.

**VM:** And remember your promise: you are going to send us a picture of you in the deerstalker.

**CF:** I've got it down on my piece of paper and that's pretty good.

**VM:** OK. Thanks a lot.