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University of California
Berkeley, California

THE CALVIN LAB: BIO-ORGANIC CHEMISTRY GROUP AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, 1945-1963

Volume I

Interviews conducted by Vivian and Sheila Moses
December 1995 - September 1997

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the Nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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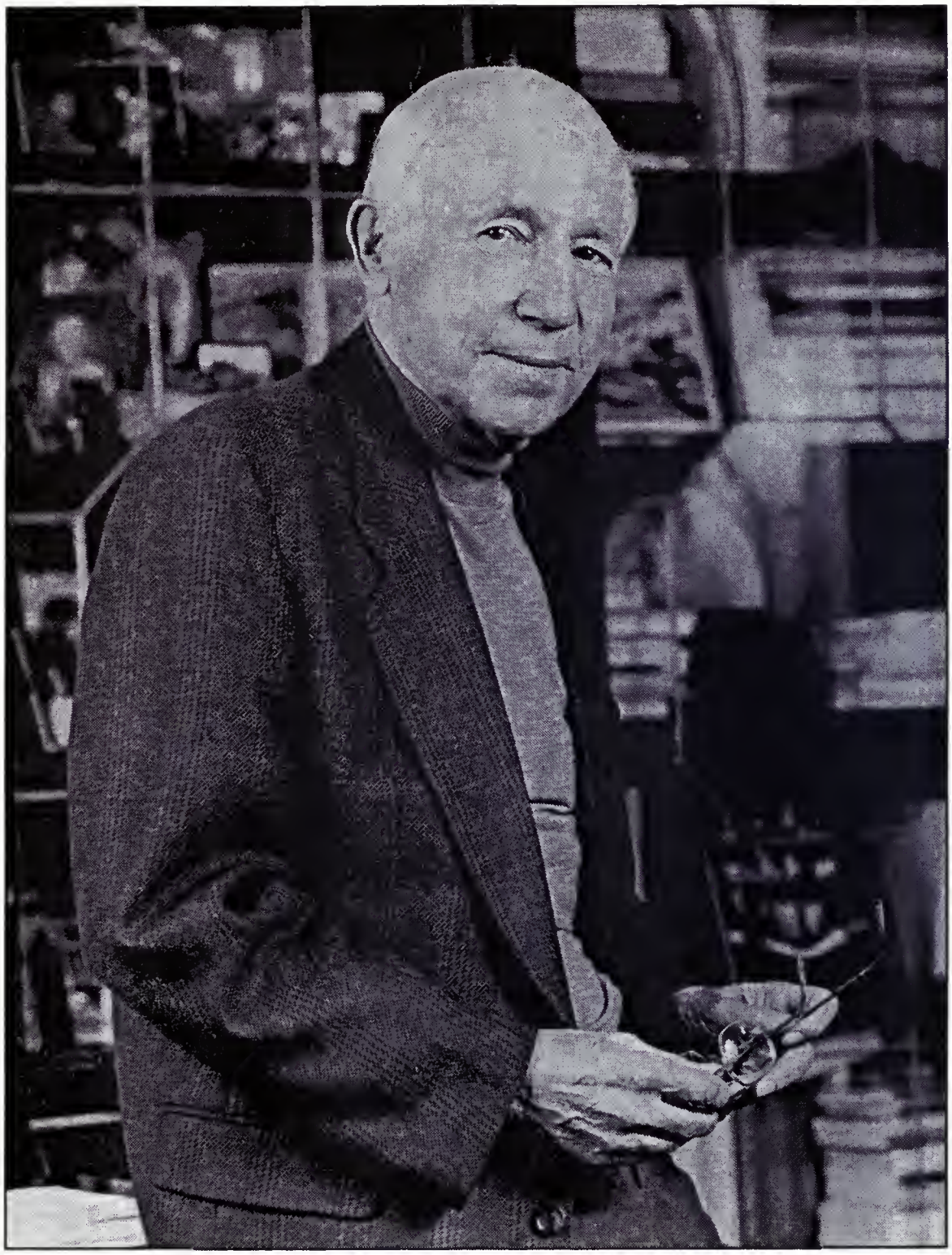

Melvin Calvin, 1991.

Photo courtesy Lawrence Berkeley Laboratory.

Cataloguing information

THE CALVIN LAB: BIO-ORGANIC CHEMISTRY GROUP AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, 1945-1963, 2000, 1008 pp. Two volumes.

Graduate students, postdocs, staff and observers of the Melvin Calvin bio-organic chemistry group at UC Berkeley from 1945-1963 are interviewed on the methods, management, community and culture of the Calvin lab; the Donner Laboratory-based group's work on organic reaction mechanisms and radioactive synthesis; work on photosynthesis in the Old Radiation Laboratory (ORL); years after 1959 in the Life Sciences Building, and the move to the Round House (Melvin Calvin Laboratory); recollections of Sam Ruben, Martin Kamen, UC Radiation Laboratory, E. O. and John Lawrence.

Interviews with Edward P. Abraham (b. 1913); Marie Alberti (b. 1937); Samuel Aronoff (b. 1915); S. Alan Barker (b. 1926); James A. Bassham (b. 1922); Edward L. Bennett (b. 1921); Andrew A. Benson (b. 1917); Ulrich Blass (b. 1924); Bob B. Buchanan (b. 1937); J. Grant Buchanan (b. 1926); Melvin Calvin (1911-1997); Gus D. Dorough (b. 1922); H. Montague Frey (b. 1929); R. Clinton Fuller (b. 1925); Martin Gibbs (b. 1922); Richard A. Goldsby (b. 1934); Murray Goodman (b. 1928); Carol (Quarck) Grisebach (b. 1919); Osmund Holm-Hansen (b. 1928); Ann M. Hughes (b. 1917); Martin D. Kamen (b. 1913); Otto Kandler (b. 1920); Lorel (Daus) Kay (b. 1926); Hans L. Kornberg (b. 1928); Alice (Holtham) Lauber (b. 1927); Richard M. Lemmon (b. 1919); Karel Louwrier (b. 1933); Peter Massini (b. 1921); Jacques Mayaudon (b. 1924); Richard L. Meier (b. 1920); Helmut Metzner (b. 1925); Gerard Milhaud (b. 1922); Vivian Moses (b. 1928); Roderic B. Park (b. 1932); Anthone Phipps (b. 1898); Ning G. Pon (b. 1925); Petronella Y.F. (van der Meulen) Prins (b. 1930); J. Rodney Quayle (b. 1926); B. Robert Rabin (b. 1927); Robert Rabson (b. 1926); Henry Rapoport (b. 1918); Duncan F. Shaw (b. 1931); Helmut Simon (b. 1927); Luise M.C. Stange (b. 1926); William Stepka (b. 1917); M. Marilyn Taylor (b. 1924); E. Malcolm Thain (b. 1925); Anne G. (Harris) Tolbert (b. 1930); Bert M. Tolbert (b. 1921); Nathan E. Tolbert (b. 1919); Edwige Tyszkiewicz; Christiann F. van Sumere (b. 1929); F. Robert Whatley (b. 1924); Lise (Schou) Wilkinson (b. 1924); Alexander T. Wilson (b. 1930); Peter E. Yankwich (b. 1923).

Interviewed 1995-1997 by Vivian Moses and Sheila Moses.

Introduction by Vivian Moses, Professor in Biotechnology, King's College, London.

ACKNOWLEDGEMENTS

The Regional Oral History Office, on behalf of future researchers, wishes to thank Sheila and Vivian Moses, Marilyn Taylor, and members of the Calvin Laboratory for their time and effort in producing these oral histories. The office is grateful to the following organizations for their donations.

The Bancroft Library, University of California, Berkeley
The Chemical Heritage Foundation, Philadelphia
The College of Chemistry, University of California, Berkeley
The Royal Society and Gresham College, London

Berkeleyan
January 15-21, 1997

Obituaries

Chemistry Nobelist Melvin Calvin Dead at 85

Nobelist **Melvin Calvin**, chemistry professor and a leading scientist at the Lawrence Berkeley National Laboratory, died Wednesday, Jan. 8, at Alta Bates Hospital in Berkeley following years of declining health. He was 85.

Labeled "Mr. Photosynthesis" by Time magazine in 1961, Calvin was awarded the Nobel Prize that year for using radioactive carbon-14 to show steps by which plants turn carbon dioxide and water into sugar during photosynthesis. Today this process is known as the "Calvin Cycle" in photosynthesis.

"For many years, Melvin was a vital personality on the Berkeley campus who contributed greatly to science," said Chancellor Tien. "It is a sad occasion to lose such a colleague."

Calvin retired in 1980 but continued his research until recently.

His findings sparked the U.S. Department of Energy's interest in solar energy as a source of power.

"Melvin's work was the cause of this agency starting its solar photochemical energy conversion research," said Allan Laufer, team leader with the Department of Energy Office of Basic Energy Sciences. "He showed converting energy from the sun into useful forms was scientifically possible. He was a very influential man."

"Since his appointment at Berkeley in 1937, Melvin influenced many areas of chemistry. It goes without saying that he became one of our most illustrious colleagues," said Paul Bartlett, chair of the chemistry department.

Fellow Berkeley Nobelist Glenn T. Seaborg said Calvin was a lifelong friend. "I have known him for 60 years." said Seaborg. "He was a great scientist and an extraordinary human being."

Calvin also did work on organic geochemistry, chemical evolution, chemical carcinogenesis and analysis of moon rocks.

"He was a very, very curious man," said his daughter, Elin Sowle. "He taught me to be curious and he taught everyone around him to be curious."

Born in St. Paul, Minn. in 1911, Calvin received his BS from the Michigan College of Mining and Technology in 1931 and his PhD in chemistry from the University of Minnesota in 1935.

After two years as a postdoctoral fellow in England, Calvin joined the Berkeley faculty as an instructor, becoming a professor in 1947. He formed the Bio-Organic Chemistry Group of the Lawrence Radiation Laboratory in 1946.

He became director of the Laboratory of Chemical Biodynamics in 1960, an innovative interdisciplinary laboratory with projects ranging from solar energy to brain chemistry. In 1980 the laboratory was renamed the Melvin Calvin Laboratory in his honor.

Author of more than 500 scientific papers and seven books and the recipient of numerous tributes and honors, Calvin also served on the President's Science Advisory Committee under Presidents Kennedy and Johnson and chaired the Committee on Science and Public Policy at the National Academy of Sciences. He received the National Medal of Science in 1989.

Calvin is survived by two daughters, Elin Sowle of Berkeley and Karole Campbell of Inverness; a son, Noel Calvin of Palo Alto; a sister, Sandra Davis of Los Angeles; six grandchildren and two great grandchildren. His wife, Genevieve, died in 1987.

A memorial service will be held on campus on a date to be announced. The family requests that donations be made to the Melvin Calvin Memorial Fund, College of Chemistry, University of California, Berkeley, CA 94720.

Nobelist Calvin Dies

See obituary, page 2.

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VOLUME II

INTRODUCTION

Vivian Moses

London

April 6th, 1998

Towards the end of 1945 something remarkable happened in Berkeley. To understand what it meant, we need to go back into history — a long way.

As with all stories of human activity, it is difficult to know where to start: with the birth of the hero, the dawn of recorded history, the emergence of modern man as a distinct species, the origin of the universe...? In our case, perhaps, the late 18th century will be sufficient. By then, Joseph Priestley in England had put a bell jar over a living plant together with a burning candle. He found that after the candle had gone out, the plant would somehow recharge the air in such a way that, if relighted, the candle would again burn. Chemistry was beginning to develop and, in Holland not long after, Jan Ingenhousz identified the gas released by the plant as oxygen, generated in quantities proportional to the amount of light the plant had received. Soon after, Nicholas de Saussure in Switzerland showed that an increase in the dry weight of a plant somehow depended on the presence of carbon dioxide as well as light. Clearly, carbon dioxide was being used to make more plant substance...but how?

The mystery remained for more 150 years. It soon became clear that, supplied with both carbon dioxide and light, plants would often increase their reserves of starch or sugars but the chemistry was quite obscure. Indeed, by the 1930s, when our story begins in earnest, the latest theory was quite unsupported by evidence. It went like this: starch is composed chemically of many molecules of the sugar glucose. Each glucose molecule comprises six carbon atoms, six oxygen atoms and twelve atoms of hydrogen: its "empirical formula" is appropriately written as "$C_6H_{12}O_6$". It was known that carbon dioxide, on its way to starch, is, in chemical terms, "reduced": hydrogen atoms are added to it and oxygen atoms are released. (That, presumably, was the basis of Priestley's observation with the plant and the reviving candle flame.) There was one well-known chemical (formaldehyde) which had just the right combination of atoms compared with carbon dioxide: two more hydrogens and one fewer oxygen, written as CH_2O. Just a little simple arithmetic would solve the problem: six molecules of formaldehyde, each one of them CH_2O, would somehow combine to give one molecule of glucose which was $C_6H_{12}O_6$. The facts that nobody could find any formaldehyde in plants carrying out photosynthesis, that formaldehyde added to plants failed to generate more starch and, indeed, that formaldehyde is a powerful poison for living things and had long been used as a preservative, did not put paid to the theory. Nobody had any better ideas.

Enter the radioisotopes

The trouble was that, once carbon dioxide entered a plant, the atoms of which it was composed could no longer be distinguished from the atoms already there: the plant, too, is made of carbon and oxygen (as well as other elements). That made it impossible to trace what happened to the incoming carbon dioxide in order to work out a route to glucose and starch. The picture changed with the discovery in 1934 of a type of carbon which was radioactive: chemically it actually was carbon and did all the things that carbon does. But this

type (or variety — "isotope" is the technical term) of carbon had a different atomic structure which made it unstable: individual atoms tended to disintegrate and emit high-speed particles which could be measured with the right sort of instruments. These "radioactive" carbon atoms each had a weight of 11 units compared with 12 units for commonplace, stable carbon atoms; the radioactive version was called "carbon-11" (or C^{11}) as distinct from the more usual carbon-12 (C^{12}). Carbon-11 offered a means of tracing the path of carbon in photosynthesis as it underwent a series of chemical changes on its way from carbon dioxide to sugars and starch: using carbon dioxide containing C^{11} as the starting material, one might be able to find out which chemical substances became radioactive and had therefore been made from the carbon dioxide absorbed by the plant.

But there was a problem. Atoms of carbon-11 in nature are very rare if, indeed, they exist at all. They have to be made in a cyclotron by bombarding boron atoms (in the form of boron nitride) with high energy deuterons; only a proportion of the boron atoms interact with the deuterons and are converted to carbon-11. It takes time to build up enough carbon-11 in the bombarded target.

But as soon as carbon-11 atoms are made, they begin to disintegrate. You cannot know or predict when any *particular* atom will disintegrate, but in any *population* of carbon-11 atoms, half of them will decay in about twenty minutes, half the remainder in the following twenty minutes and half of *that* remainder in the next twenty minutes; because half the atoms in any population of C^{11} decay in this twenty-minute period, that time span is called the *half-life*. Some seven or eight half-lives after removing the newly-produced carbon-11 from the cyclotron, the quantity remaining is only 0.8% or 0.4% of what one started with; effectively, experiments using carbon-11 have to be completed within about two-and-a-half hours of getting hold of it, a very severe limitation. Different radioactive isotopes have different half-lives, each characteristic for the particular isotope.

The short half-life notwithstanding, just before World War II attempts were made in Berkeley to use carbon-11 for exploring the path of carbon in photosynthesis. The people who did the experiments were Sam Ruben, an instructor in the Chemistry Department, and Martin Kamen, then the only actual full-time employee of the Radiation Laboratory (of which Ernest Lawrence was the director and an interested observer of the photosynthesis work). Later they were joined by Andy Benson. The Crocker Laboratory which housed the cyclotron used to make the carbon-11 was only yards from the "Rat House", the chemistry building in which the experiments were performed. Though the experiments were difficult and no doubt frustrating because of the short half-life of carbon-11, progress was made and some analysis was successful — but it was a painful business. Lawrence, a particle physicist of great experience, reasoned on theoretical grounds that there ought to be yet another isotope of carbon, this one weighing 14 units, which should be radioactive but with a much longer half-life, long enough to make it readily useable. He got Ruben and Kamen to look for it in certain materials which were being used as radiation shields around the cyclotron and Kamen[1] tells the story of how, one night after a very long vigil, carbon-14 was eventually found. It had a half-life of 5,600 years, long enough for any biochemist to complete his experiments. It was finding carbon-14 that would eventually make all the difference.

Though they did not have very much of it, the photosynthesis investigators immediately recognised the value of the new isotope and began to use it in their explorations. But they did not have time to get far because, on 7 December 1941, Japan bombed Pearl Harbor and the United States entered the war. That event radically changed priorities everywhere in America, nowhere more so than the University of California Radiation Laboratory (UCRL). Photosynthesis research was put aside as people turned their attention to more pressing matters.

World War II and its aftermath

The events of the war left their unmistakable imprints on photosynthesis research. An almost entirely new set of people became involved in the path of carbon studies: only Andy Benson from the pre-war team eventually resumed work in this area. Sam Ruben sadly died in a laboratory accident in Berkeley in 1943. For a while Martin Kamen continued to work at UCRL on other things but, as he eloquently described in his book[1], bizarre political factors forced him out and, as a result, he had no ready access to the radioisotopes needed to unravel the path of carbon in photosynthesis. Andy Benson was a conscientious objector and spent much of the war years in forestry and other non-combative activities. In the event, none of the pre-war photosynthesis team was in Berkeley when the war came to an end.

As a result of the nuclear weapons developments in the Manhattan Project, a wartime activity in which the UCRL played a major role, Ernest Lawrence found himself the director of what had become an immensely rich and powerful organisation. He had lost his interest neither in photosynthesis nor in the opportunity for its elucidation with carbon-14, with which there had scarcely been any chance to make much progress before wartime pressures became irresistible. His brother, John Lawrence, was director of the Donner laboratory in Berkeley, involved in medical research. There was one other factor which contributed to Lawrence's decision in November 1945: his habit of lunching in the Faculty Club where a number of younger members of the faculty also took their midday meals — one of them was Melvin Calvin. Lawrence perceived an opportunity and knew he had the means to make it work.

Calvin himself recounted[2, 3] how, one day late in November 1945 while walking back to their offices after lunch, Lawrence suggested to him "that it was time to do something 'useful'... and thus expand our interests beyond the uranium-plutonium fission extraction procedures". Lawrence's proposal seems to have been that Calvin should take charge essentially of the world supply (as it then was) of carbon-14, controlled by Lawrence, and use it for two things: the study of organic reaction mechanisms and the synthesis of radioactive compounds for medical research and therapy by John Lawrence and his colleagues, and also for Calvin himself to continue the work on the path of carbon in photosynthesis.

The Manhattan Project from which Lawrence was emerging was "big science" in its biggest form. The resources which he now made available for the chemistry of C^{14} and the photosynthesis work were minuscule by comparison but it enabled Calvin and his colleagues to develop a multidisciplinary research activity which was wholly remarkable and extraordinary both for its period and for a long while thereafter.

Calvin and the Bio-Organic Chemistry Group

In addition to getting the C^{14} and the funding, Calvin was also given building space. For their work on organic reaction mechanisms and radioactive syntheses, some of his group occupied part of the third floor of the Donner Laboratory on Gayley Road, the eastern boundary of the lower level university campus. The space was conventional: a long corridor running the length of the building had offices and laboratories opening off it on both sides. A little later, Calvin's group was offered almost the whole of the Old Radiation Laboratory (ORL), a wooden building next to Chemistry, erected in 1885; it had housed Lawrence's 37-inch cyclotron which not long before had been removed. The building, at most two hundred yards from the Donner, was modified ("refurbished" would perhaps be an exaggeration!) to meet the needs of the photosynthesis work and, for the following fourteen years or so, ORL became the focus of the photosynthesis ferment.

In 1959, that building was demolished to make way for Latimer Hall, the new chemistry building, and the people in it were relocated for an indefinite period some hundreds of yards away to the basement of the Life Sciences Building. It was not until 1963, with the opening of the circular, purpose-built Laboratory of Chemical Biodynamics (now the Melvin Calvin Laboratory and known affectionately as "The Round House") that the two branches of Calvin's group, until then called Bio-organic Chemistry, were finally united under one roof.

As many of our interviewees attested, there was a special atmosphere and flavour about ORL. Perhaps it was the relatively open design of the working space with a minimum of walls and doors. Perhaps it was the fact that, being old and wooden, people felt they could (and were able) to modify it without difficulty to suit whatever need might arise. But mainly, I suspect, it was the intensity and integrity of a group of young people, some more-or less-permanent and mainly American, others there for a year or two or three from all corners of the globe, working towards a common goal: the elucidation of the path of carbon in photosynthesis.

There seems to have been nothing like it anywhere else and the internal organisation of the group was also remarkable. The managerial hierarchy was clear and simple, in no way lending itself to internal conflict and competition. Everybody was so young! At the time of founding, Calvin, by far the oldest, was not yet thirty-five. Within two or three years he had recruited a small group of colleagues, all in their mid-twenties and all American; their status was both more-or-less permanent and yet indeterminate. Few if any had a defined limit to their employment and, as it turned out, some stayed for the rest of their lives. Others left at various times when the need arose for a change or in order to develop their own research independence. But at no time was any one of them a candidate for replacing Calvin as director.

In addition to support personnel (secretaries, technicians, craftsmen and others), much of the research was actually carried out by a comparatively large number of postdoctoral visitors (many from overseas, who came for a year or two with every intention of leaving) and a smaller contingent of graduate students, who would also make their own ways out into the world. So the permanent staff had no cause to compete among themselves: none of them would supplant Calvin as director and all were so well supported that any limits to their achievements resided entirely within their own capabilities. The post-docs. and students were in any case not competing within the group: they were temporary. And, as so many of his colleagues have remarked, Calvin was a good director. It sounds ideal — and for me and others it was!

Through the 1950s

The idyll, at least in ORL, lasted as long as the building. By 1959, the primary questions relating to the path of carbon had essentially been answered and in 1961 the merit of the programme was recognised by the award of the Nobel Prize to Calvin. With the demolition of ORL in 1959, things inevitably changed: the focus on photosynthesis was already slipping away and for close on five years the former inhabitants of ORL lived, as they saw it, in temporary exile in the Life Sciences Building. Planning for the Round House, the new prospective home for the Calvin group, had begun at the close of the 1950s; by November 1963 the building was completed.

That is essentially where our story ends except for an occasional backwards look at the old building and the new by some people who went on to work in the Round House and others who had long since left and returned only for brief visits. Inevitably things were different when ninety people occupied a single building, after Calvin had been honoured in

Sweden, when the photosynthesis focus had gone and work was spreading over an ever widening range of topics. Moreover, as it slowly and painfully grew increasingly clear through the 1960s that the seemingly unending source of liberal funding from the Atomic Energy Commission and its successor bodies was not going to last for ever, life became tougher and the competition for funding a real fact of life. But what happened to the group after 1963 is another story which we are happy to leave to someone else.

The science and the people

All the group's scientific advances were, of course, published at the time they were made (some of them even before, as one or two of our respondents have commented!). But nobody has ever written about the scientists themselves or the lab. as a society in which people worked and enjoyed themselves so much while they were doing so that some of them seemed never to go home.

Together with my wife Sheila, who is not a scientist, I first came to Berkeley in 1956 to join Calvin on a one-year post-doctoral fellowship; at his invitation it stretched to two. It was then time to return to Britain (and the conditions of the visa required that I did so) although Calvin invited me to join what had clearly become his permanent staff. But I was unsure and he suggested that I think about it for six months and let him know. The winter of 1958/9 in Britain seemed academically dark; the "new" universities were not yet established and the sort of job I might like to have was rare. Sheila and I decided that I should accept Calvin's offer and we immigrated to the US and Berkeley in October 1960.

In the spring of 1968, while on sabbatical leave in Oxford, I suddenly realised that the Calvin group embodied a fascinating story which had never been told, of how a group of individuals can unite in the pursuit of scientific knowledge. It may be that what brought this home to me was reading Jim Watson's *The Double Helix*. I had never before read a book about life among the sorts of scientists I knew and, for all its drama, I appreciated the value of what Watson had done. Why not do something like it for the Bio-organic Chemistry Group? Our story seemed every bit as good.

At first I thought of telling the tale in the form of a film. In 1968 all the players were still living and surely all would remember events with great clarity. That year Calvin was also taking his sabbatical in Oxford and whether I suggested it to him then, or after we had both returned to Berkeley, I am not sure. But he was not enthusiastic. Perhaps it was because I put it to him in the form of "you get the money and then *we* will make the film". But before long another opportunity arose.

In 1970/1, Sheila, who is an editor, was working with Paul Baum, a psychologist in Berkeley. She was interviewing him in order to write a story about his practice. We met his wife Willa, who directs the Oral History Program at the Bancroft Library. In the course of our conversation, it turned out that no scientists had yet been interviewed. The idea developed that Sheila might join Willa's group and start by interviewing Calvin: I could help her over any difficulties she might have with technical matters.

However, soon thereafter I was offered a Chair of Microbiology at the University of London and we decided to migrate once more and return to our roots in England. All thoughts of making films or undertaking oral histories of the Calvin group, or any other, disappeared and did not re-emerge for twenty-four years.

California and science history

As these things do, the idea resurfaced in a roundabout and unexpected way. In May 1994, I spent two weeks in California on behalf of the Science Museum in London, seeing if I could locate interesting pieces of equipment which had played a part in significant research programmes and which were in danger of being thrown away. Talking to a number of famous scientists who had by then retired from full-time activity, I found a treasure trove in their offices. Most of them had already had to move out of their large offices and larger laboratories. Many of their valued relics had already gone — into storerooms if they were lucky or into garbage trucks if they were not. They understood that before long they, too, would be gone and that most probably nobody would even recognise what they still had in their possession. The prospect of relative immortality in a glass case in London was attractive. Naturally, I visited Calvin's lab. in Berkeley to see what might still be there, perhaps items I had used myself (and I found some; one major item, even, which I had actually invented) and, of course, we got talking about the good old days.

Perhaps that started me thinking about science history in which I myself had played a part. In February of the following year, I lunched with a colleague at the University of the West of England outside Bristol and the conversation turned to oral history. In a flash, but for the moment forgetting what we had been thinking more than twenty years earlier, it came to me: why not do a history of Calvin's lab.? Many participants were still living but clearly time was not unlimited.

A few weeks later I was once more in Berkeley and was able to sound peoples' willingness to talk on tape about the early history of the Group. There was unanimous encouragement to go ahead and do it. I went to see Willa Baum to ask what she thought of the idea and she reminded me of what we had planned in 1970/1. Jack Lesch and later Roger Hahn of the Office for History of Science and Technology on the Berkeley Campus were equally encouraging; Sheila and I were offered office space for a period if we were to come to Berkeley to pursue the idea. Applications were accordingly made to a number of possible sources for funding to meet the expenses for the project to be undertaken in 1996/7.

The history of the Bio-Organic Chemistry Group

At the end of 1995 we planned to spend a few days with family in Los Angeles on our way back to London from a holiday in Australia and New Zealand. Marilyn Taylor, Calvin's secretary since 1948 and the keeper of his records, had already begun to work closely with us and she urged us to use the opportunity to come up to Berkeley and interview Calvin then and there; his memory was failing and she feared that, if we left it until the summer of 1996 as we had planned, he would be unable to contribute. It was a wise suggestion; even in 1995 he had forgotten many of the details and most of the reasons for his decisions. Luckily, his own oral history[2] of fifteen years earlier had covered much of the ground but we very much wanted him to be part of our own project. Having read both his 1980 interview and his autobiography[3] before we started, we knew that events were presented very much from his own viewpoint. But we needed also to find out what his colleagues thought and how they might remember events.

By the spring of 1995 we were fortunate in having secured funding from the Royal Society and Gresham College in London, from the College of Chemistry in Berkeley and from the Chemical Heritage Foundation in Philadelphia. We spent the second half of May and all of June and July of 1996 in the United States, eight weeks interviewing former members of the Calvin group up and down the West Coast and a further fourteen days talking to people across the country and on the East Coast. By the time we returned to London we had recorded on tape lengthy conversations with four people in Britain and thirty-two in the US. By September 1997 we had reached twenty more people in Britain, France, Switzerland,

Germany, Holland and Belgium and brought the interview programme to a close; we concluded with almost sixty hours of recordings, amounting to nearly half a million words.

Two colleagues have been of immense value to the project. Alice Lauber (née Holtham), who was once the secretary in ORL and who now lives in Seattle, not only gave us her own reminiscences but transcribed about a quarter of the recordings. Marilyn Taylor has been an integral part of the project from its inception and, indeed, from long before that. For half a century she has kept meticulous records, papers and photographs, of Calvin's activities and those of this colleagues; she helped us find all the ones of interest. She transcribed the other forty-two recordings and has worked constantly with us to bring this project to fruition. It may sound trite to write that without her we would never have done it but it is nevertheless true.

Interviews with non-group members

The scientific work of the Bio-Organic Chemistry Group was at the frontier of research, nowhere more so than in photosynthesis. Competition with other laboratories was often furious and relations between people in them and some in ORL not always entirely smooth. We therefore thought it would be interesting to record the views of three of these outside observers: what did Calvin's lab. and its efforts look like from outside?

We were also very fortunate in being able to talk to Martin Kamen. It was his work with Sam Ruben which laid the foundations of the photosynthesis studies in ORL, both because of their discoveries of C^{14} and their early work on the path of carbon using C^{11}. Nothing could be more relevant as an introduction to the studies in this field of Calvin and his colleagues.

About the transcripts

The transcripts are presented in the chronological order in which the interviews were recorded. As nearly as possible, they are verbatim. We had to make a choice and decided not to edit them except for "ers", "ahs" and "umms". In a few places, individual words and occasionally whole phrases could not be deciphered from the recordings: we have noted these as (*indecipherable*). Some people made statements which we or Marilyn Taylor know to be wrong; we have provided (*Editor's notes)* giving the correct information to the best of our knowledge. For readers who would like to refresh their memories about the science, we have included in our bibliography to this Introduction two helpful citations[4, 5] from the era of our study.

And from here?

Our intention has always been somehow to distil all the information in these interviews into a book of readable length. That remains our intention. The spirit is willing; the flesh...?

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4. J.A. Bassham and M. Calvin. *The Path of Carbon in Photosynthesis*. Englewood Cliffs, N.J.: Prentice-Hall, Inc. (1957).

5. Melvin Calvin and J.A. Bassham. *The Photosynthesis of carbon Compounds*. New York: W.A. Benjamin, Inc. (1962).

THE CAST

1.	Melvin Calvin	Professor of Chemistry; Director (US), 1945-80
2.	J. Grant Buchanan	Post-doctoral Visitor (UK), 1951-52
3.	J. Rodney Quayle	Post-doctoral Visitor (UK), 1953-54
4.	B. Robert Rabin	Post-doctoral Visitor (UK), 1956-57; 1963-64
5.	M. Marilyn Taylor	Secretary (US), 1948-97
6.	Richard M. Lemmon	Graduate Student and Senior Scientist (US), 1946-88 (approx.)
7.	James A. Bassham	Graduate Student and Senior Scientist (US), 1946-88 (approx.)
8.	Edward L. Bennett	Senior Scientist (US), 1949-88 (approx.)
9.	Ning G. Pon	Graduate Student (US), 1954-62
10.	Martin D. Kamen	Never a member of the Bio-Organic Chemistry Group (US)
11.	Osmund Holm-Hansen	Senior Scientist (US), 1955-58
12.	Andrew A. Benson	Senior Scientist (US), 1946-55
13.	Alexander T. Wilson	Graduate Student (New Zealand), 1951-54
14.	Murray Goodman	Graduate Student (US), 1949-52
15.	Ann M. Hughes	Research Scientist (US), 1952-79
16.	Richard L. Meier	Never a member of the Bio-Organic Chemistry Group (US)
17.	Vivian Moses	Post-doctoral Visitor (UK), 1956-58; Senior Scientist, 1960-71
18.	Marie Alberti	Research Scientist (US), 1959-present
19.	Bob B. Buchanan	Never a member of the Bio-Organic Chemistry Group (US)
20.	Lorel (Daus) Kay	Research Scientist (US), 1949-54
21.	Henry Rapoport	Professor of Chemistry (now Emeritus) and Associate Member of the Bio-Organic Chemistry Group (US), 1946-date uncertain
22.	Gus D. Dorough	Graduate Student (US), 1946-47
23.	Alice (Holtham) Lauber	Secretary (US), 1950-55
24.	Samuel Aronoff	Graduate Student (US), 1937-42; Post-doctoral Visitor, 1942-43; 1946-47
25.	Roderic B. Park	Senior Scientist (US), 1958-date uncertain; Professor of Botany, 196?-date uncertain

26.	Anthone Phipps	Support Staff (US), 1949-70
27.	Bert M. Tolbert	Senior Scientist (US), 1946-57
28.	Anne G. (Harris) Tolbert	Graduate Student (US), 1951-53
29.	Nathan E. Tolbert	Post-doctoral Visitor (US), 1950
30.	Martin Gibbs	Never a member of the Bio-Organic Chemistry Group (US)
31.	Richard A. Goldsby	Graduate Student (US), 1958-61
32.	R. Clinton Fuller	Senior Scientist (US), 1952-55
33.	Sir Hans L. Kornberg	Post-doctoral Visitor (UK), 1954
34.	William Stepka	Graduate Student (US), 1948-52
35.	Robert Rabson	Never a member of the Bio-Organic Chemistry Group (US)
36.	Peter E. Yankwich	Senior Scientist (US), 1945-48
37.	E. Malcolm Thain	Post-doctoral Visitor (UK), 1954-55
38.	Duncan F. Shaw	Post-doctoral Visitor (UK), 1956-57
39.	H. Montague Frey	Post-doctoral Visitor (UK), 1955-56
40.	Carol (Quarck) Grisebach	Research Scientist (US), 1954-55
41.	Helmut Simon	Post-doctoral Visitor (Germany), 1955-56
42.	Otto Kandler	Post-doctoral Visitor (Germany), 1956-57
43.	Karel Louwrier	Post-doctoral Visitor (Netherlands), 1958-59
44.	S. Alan Barker	Post-doctoral Visitor (UK), 1955-56
45.	F. Robert Whatley	Never a member of the Bio-Organic Chemistry Group (UK)
46.	Gérard Milhaud	Post-doctoral Visitor (France), 1951-52
47.	Edwige Tyszkiewicz	Post-doctoral Visitor (France), 1959-61
48.	Peter Massini	Post-doctoral Visitor (Switzerland), 1951-52
49.	Ulrich Blass	Post-doctoral Visitor (Switzerland), 1956-57
50.	Luise M.C. Stange	Post-doctoral Visitor (Germany), 1957-58; 1960
51.	Petronella Y.F. (van der Meulen) Prins	Post-doctoral Visitor (Netherlands), 1956-57
52.	Jacques Mayaudon	Post-doctoral Visitor (Belgium), 1954-55
53.	Christiaan F. van Sumere	Post-doctoral Visitor (Belgium), 1957
54.	Lise (Schou) Wilkinson	Graduate Student (UK), 1949-50
55.	Sir Edward P. Abraham	Post-doctoral Visitor (UK), 1948
56.	Helmut Metzner	Post-doctoral Visitor (Germany), 1956-57

Chapter 1

MELVIN CALVIN

Berkeley (California)

November 29th, 1995

VM = Vivian Moses; MC = Melvin Calvin; MT = Marilyn Taylor; JO = John Otvos

SM = Sheila Moses

VM: I thought we might start by thinking about how the whole photosynthesis, how the biochemistry of photosynthesis started. Because you (*Calvin*) were really a physical-organic chemist at the beginning and you decided to go into what was essentially biochemistry.

MC: I don't know whether or not I made a conscious decision, but it happened.

VM: Did you see it as going into biochemistry, or for you, was it just more chemistry?

MC: I didn't really think about that at all. I just did what I had to do. I didn't plan to go in this way or that way; I just went. Does that make sense to you?

VM: But you chose to start with looking at the fixation of carbon dioxide. How did you tumble to that? What prompted you to get ...?

MC: I had CO_2 I had radioactive carbon, carbon-14.

VM: And because of your earlier interest in pigments, it fitted together with that.

MC: No, I had lots of it so I had to do something with it. That was all there was to it.

VM: But why did you decide to do that instead of doing something else with it?

MC: Because that was an obvious thing to do. That's was the main sink for CO_2 in the world. So, I thought, well we'll do that. There wasn't anything subtle about it.

VM: You then began to develop this group of people.

MC: Well, I didn't develop a group of people. They came.

VM: They must have come to something. They must have known where to come.

MC: I guess so, I didn't really look for them, if that's what you mean. I didn't organise it. They came. You don't understand what I'm saying?

VM: I'm trying to think what it was like on Day 1, you know. You decide that you are going to do this. It was, as you say, the obvious thing to do. You had a lab. You had any experience?

MC: The Old Radiation Laboratory; it was an old wooden building.

VM: Yes, I remember that.

MC: I had the algae cultures in one corner of it, it one corner of the lab. I did that for many years, I don't know, how many years (5-l0 years, maybe more), I lived in that building. It was an old wooden building, until they tore it down.

VM: Do you remember what it was like at the beginning, when that lab didn't have anything in it and you had to decide what was to go into it.

MC: No, I don't remember that. I just put stuff down where I had to put it down, that's all.

VM: You, by yourself. Were there other people?

MC: There were other people--graduate students, post-docs.

VM: Do you remember who they were at that time?

MC: We can look it up.

MT: Not really, not that far back. But Andy was there, Andy Benson.

MC: I guess he was, but he wasn't crucial to the action. He was there, but I don't remember him doing something that made a difference.

VM: So there were original. graduate students and postdocs who were in with you from the very beginning.

MC: I guess so.

VM: We can try and look them up.

MT: Al was one of your graduate students, Al Bassham. When he came back from the war, he came back to get his Ph.D. He had a BS degree at Berkeley and then he went into the Navy. So he came back and somehow he made the connection with you. He was one of your original graduate students. And you had Sam Aronoff; yes, he was a chemist. You knew him from your war work; he worked with you on the chelates. And then there were some BS chemists like Tom Goodale and people like that...

MC: I don't remember.

MT: ...and Vicki Lynch was there.

MC: Yes, I remember her.

MT: She was a microbiologist; how she got there I don't know.

MC: I've no idea.

VM: I'll get back to some of them. Maybe they can fill it in from the other side of the picture. So, you started with this group of people.

MC: No, I started by myself.

VM: Literally by yourself?

MC: Yes. Then, we added people to the group.

VM: Had you ever handled biological material, living things, at that stage? So, you did it from the ground up.

MC: Yes, I had the algae culture in the corner and that was the heart of it. There were four black-bottom vessels in a shaker in a thermostat. And we kept them going all the time and harvested them when we needed them. That went on for years; I don't know how many years, but a long time.

VM: Presumably you were aware of the work that Sam Ruben and Martin Kamen had done.

MC: Yes, I was well aware of that. But they had moved away, for some reason. I don't know where they went, but they weren't there.

VM: But you'd seen the sort of set-up that they had had presumably.

MC: I didn't copy that. It was all done in the Old Radiation which is this building here, old wooden building, and I don't recall how we got there. I just went there. I think Ernest gave us the building.

VM: Some of the things that struck me...

MC: The cyclotron room was next door; it wasn't there but the empty room was there.

VM: But, you never had an office in that building, did you? Your office was always in chemistry.

MC: Oh, but I had an office there. Why?

VM: Well, I was wondering you see: when you went into that place, in the morning or whenever you went it, and you used to go in there every day as I remember, you didn't go and hide yourself in an office.

MC: No, certainly not, the office was out in the open anyhow. It was all glass lined. There was no closed-in area at all. How shall I tell you that?

VM: Well, I remember it. So, you used to go in there and just talk to people, as I remember.

MC: Yes.

VM: And what did you do; you grabbed the first guy you saw?

MC: I don't remember that. I usually had some particular part of it going in my head and that's what I would pursue. Something like that. There wasn't any profound thought about it.

VM: But you didn't do any lab work yourself...

MC: Oh yes I did.

VM: ...with your own hands there?

MC: Yes, it was possible to do that and I did occasionally something. I don't know that anybody liked it, but I did it.

VM: I remember that you used to spend a lot of time actually sitting with people at their desks, going through all the stuff. When did the big white table start, do you remember, where one laid out all the chromatograms? Who thought of that one?

MC:: That was later. That wasn't at the beginning.

MT: That was when we got chromatography. Remember, at first they didn't have chromatography and when it was necessary to have a place to spread out the chromatograms, that's when Mr. Norman made the table.

VM: Mr. Norman made the table?

MT: Mr. Norman made the table.

VM: And that became the social focus of the whole group, didn't it? That's where the coffee was.

MC: It wasn't done deliberately. It was an accident, I guess.

VM: But that's the way it worked out, and I remember that led, then, to your concept in the Round Building (*Melvin Calvin Laboratory, MCL*) of having a focus in the middle so that people would concentrate around it.

MC: Well, I guess so. I think there was some kind of relation. But I didn't think about it that way.

VM: The other thing that occurred to me is the seminar, the Friday morning seminar programme. Did that start from the very beginning?

MC: Very close. I can't tell you exactly but it was close to the beginning of the work in the ORL?

VM: How did you think of this God-awful time of 8:00 o'clock on Friday morning?

MC: How did I think of it?

VM: Well, you always got up very early, didn't you?

MT: Vivian, he taught a class at eight o'clock three days a week, so what's another day?

VM: I remember that. Some of us weren't used to thinking at all at eight o'clock in the morning.

MC: I was there all the time.

MT: I think that teaching the class at 8:00 o'clock, three days a week, was habit.

MC: It woke me up!

MT: What's one more day a week, on Friday morning!

VM: So those started from the very beginning and was this a planned organisation or did you just sit around and chat when you first started?

MC: I'm sorry; I don't understand what you mean.

VM: Well, did you decide who was going to speak a long time ahead? How did you decide who was going to give the seminar?

MC: Just that day.

VM: Oh, really?

MC: Yes: I looked around and asked somebody to talk

VM: Just at the drop of a hat, like that?

MC: Yes.

VM: By the time I knew it, they were told the day before.

MC: Oh, really.

VM: Yes, and then, these kids used to stay up all night getting their stuff ready.

MC: Well, I don't remember that but I would look round and ask somebody to talk. That was simple. There was nothing planned about it. I just wanted to get people whom I didn't hear about, whom I didn't see every day or whom I didn't talk to, to get up and talk.

VM: And you always sat at the same position, didn't you, at the right-hand side of the table, and this long table became part of the scene.

MT: Originally, when I first came, they had the seminars in the Donner Library, you remember that?

MC: No.

MT: Yes, that was on the second floor. There was nowhere in ORL basically for a place like this. But there were chairs and they came into the Donner Library but you still sat in the same position and they had chairs sort of like the seminar room in Gilman Hall.

MC: I don't remember that, but I'm sure that's true. I can't remember the details of that. I don't visualise it at all, but I dare say you are right.

VM: After that, we used to go into the Faculty Club before the Round House was built. There was a Faculty Club room that we used to squeeze into: it was a bit small.

MT: It was the Lewis-Latimer Room.

VM: I don't remember the name of the room but there was a room somewhere that we used to use.

MC: I don't remember that. You are probably right. I don't know how you remember all these things.

VM: I was there!

MC: Well, so was I!! But, that doesn't mean much — to me anyhow.

VM: When you started — I read the thing (*oral history*) you did with Norberg and there's a lot of information there about how you came to have an establishment in Donner.

MC: That was John Lawrence's idea, and I had the top floor, at least part of the top floor of Donner, for some years (I don't how many years, but for quite a while). I don't remember why that was so. I think John wanted it, and I wanted it. I didn't have any other place. So that's how it happened.

VM: The people in Donner were largely concerned with the synthetic work, weren't they?

MC: Almost all of them.

VM: Did you ever see a distinct separation between that group and the photosynthesis group?

MC: I don't know what you mean by that.

VM: Well, did you regard the whole thing, the Donner and the ORL people, as one, as part of one thing.

MC: I think so.

VM: Was there a lot of interaction between them?

MC: I can't answer that. I think so.

MT: Sometimes, Vivian, the people in Donner would make the (*synthetic radioactive*) compounds that the people in ORL would use. So, without Bert and the people who were doing the synthetic work, when you needed something over here (*in ORL*) radioactive, they made it there and then you could use it in your photosynthesis studies. So it was really an integrated operation.

MC: I guess so. I can't remember how it worked, but that sounds reasonable to me.

VM: So people moved freely back and forth between the two groups.

MC: More or less but there really weren't very many movements because they were different kinds of people. Some of them were analytical biochemists and the others were synthetic organic chemists and there wasn't much moving around.

VM: They were all one group when it came to seminars and activities like that.

MC: Yes, but they were different talents.

VM: Everybody knew everybody else, that sort of thing. When did you begin to attract this enormous range of people coming from other places, the foreigners, the others from inside the US?

MC: I don't know; they just came. I didn't make any effort about that.

VM: At the beginning, as I understand it, there really wasn't anybody else working on photosynthesis, or on this aspect of photosynthesis, at all when you started.

MC: In the world, you mean?

VM: In the world.

MC: I think you are probably right, but I can't be sure of this.

VM: How did you begin to publicise what you were doing?

MC: I published papers.

VM: Did you go to conferences?

MC: Yes, but the papers that I published were the important part. The conferences were important, but they weren't the heart of it. The heart of it were the papers.

VM: So people would then approach you and want to come?

MC: Yes.

VM: And how did you begin to respond? What did you say to these people when they said "I want to come" and they probably said "find me money", didn't they? They usually said that.

MC: Yes, of course.

VM: So how were you able to respond to that at the beginning?

MC: Well, I had the money. I don't know where I got it; Ernest, I guess. Lawrence. So, I just asked him and he gave it to me.

VM: That's a very favourable situation (*laughter*) — it doesn't happen any more like that, I'm afraid.

MC: Well, I don't remember really.

VM: So, you were able to accommodate these people from the very early stages?

MT: You probably remember, Vivian, from your own experience that Dr. Calvin would tell you to try and get a fellowship of your own; particularly for foreign people the Atomic Energy Commission money wasn't so easy to get for foreign people. So, you (*Calvin*) would say to foreigners: "get your own money" and then if you need a supplement, we can help you. Or, if you come for a year on your own we can take care of you for the second year. A lot of the people from Britain and France would come on their own money for a year and then we could take care of them after that.

VM: Were the original people mostly Americans and did the foreigners begin to come later?

MC: You are asking a question I just can't answer. I just don't know. It seems to me they were there all the time.

VM: They were certainly there for a long time.

MT: When I came in 1948, I remember that there were two foreigners then, an English person (*in Donner*) and I think there was one in ORL but I don't remember who they were. And the others were American.

VM: There was a guy called Ted Abraham.

MT: He's the first one I remember. He's a "Sir" now.

VM: He's in Oxford and he had to do with cephalosporins later on; that's how he made his name later on. He must be retired now but I think he's still in Oxford. I must go and talk to him as well about the early days.

MT: He was in Donner, he was not in ORL, and he was there about six months.

MC: Who are you talking about?

MT: Ted Abraham.

MC: I don't remember him very well; the name is familiar.

MT: You (*Calvin*) visited him when you were Eastman Professor in Oxford in 1967-68 you had social and chemical contact with him again.

VM: When you first started publishing in this field, in the path of carbon field, did you get responses from the botanists. Here were you a chemist, it seemed invading their field, did they respond? Did you get reactions from them?

MC: I don't remember, but I think there was some kind of reaction. I just don't remember.

VM: At the time that you started, really nothing at all was known, was it. Did you have any even vague ideas yourself at that time about what might be the mechanism?

MC: No, not the slightest.

VM: You weren't guessing?

MC: No. Very early on I found PGA (phosphoglyceric acid) and I had to figure out where it came from, and that's how it got started.

VM: I remember you told that story about sitting in the car outside the freezer store (at Grove and Cedar Sts. in Berkeley) and it came to you.

MC: Yes, I remember that.

VM: It's a true story, is it?

MC: Yes. My wife was in there, doing something, I don't know what.

VM: And you were on a red zone apparently, and a bit nervous I guess.

MC: How did you know that?

VM: You told it, I think, when you got your Nobel Prize. You did a press conference and I remember you telling that story. These are the sort of stories that will make good reading.

MC: I remember sitting there, I don't remember doing anything, just sitting there, while she was inside doing whatever had to be done. I didn't go in, but that's all I can remember about it.

VM: When you started you had a completely open mind as to what might be happening in photosynthesis.

MC: I didn't know what was going on. That was the point. There was no preconceived notion about what might be there. There wasn't any evidence, there wasn't anything.

VM: The only piece of work that I know which was relevant to what you did and even preceded your own work was a paper which I think might have been a theoretical paper in about 1943 by a guy called Zilversmit who described the kinetic sequence that he would expect from a line of metabolites, one following the other (the word escapes me for the moment) — a line of intermediates — and he had a theoretical paper in which he said that the initial radioactivity should be 100% in the first one and then it should decline. Just the way you, in fact, did the PGA identification. Did you know about that?

MC: Not that I can recall. But I may have known it. I don't recall it.

VM: It's exactly what you actually did.

MC: Yes, but I don't remember it. I don't remember it as a fact. I believe you!

VM: It was only two or three years before you did it so you might very well have been aware at the time.

MC: Very likely, but I can't tell you more about it than that.

VM: In terms of actually running the lab., clearly you were absolutely immersed in the science of it; did you also have a big burden running it administratively? Did it take a lot of your time?

MC: I don't think so. I just didn't do it.

VM: It had to be done.

MC: Well, not much.

MT: It was done by others: Bert (*Tolbert*) did it. You set it up so that the Donner office was the administrative office and took care of the budget. The person over there was Bert and then later Dick Lemmon. They took care of the budget, they took care of the personnel, they took care of all that kind of stuff so you did not have to do it.

MC: I wouldn't do it!

MT: You got somebody to do it for you, and it was done. So you could spend your whole time on science.

MC: I did! I didn't like that kind of work!

MT: But, unfortunately it has to be done whether you like it or not. A group can't go without somebody doing that kind of work.

VM: Was all your research essentially at that time in these two groups? Did you have additional activity in the Chemistry Department as well?

MT: I had to teach.

VM: But not research in the Chemistry building at that point.

MC: I don't think so.

MT: You actually had Janet Splitter: remember Janet Splitter, who worked all those years on the stilbene problem down in basement of Old Chemistry. And you had one of your graduate students, Gilbert Seely, he wasn't in the photosynthesis or anything, he was a (*indecipherable*) Chemistry grad. student working on chemical problems down in the basement of Old Chemistry. You always had somebody down there.

MC: I hear you, and you are undoubtedly right, but I don't remember it.

MT: You had (*Gustav*) Utzinger, a Swiss man, who came on the Rockefeller money, and he was down there working on what I called really "chemical" problems as opposed to anything in ORL or the Donner. So there were always one or two that you had.

MC: I guess you're right. I don't really remember that, Marilyn, but I guess you're right.

VM: And these people really played very little part then in the ORL activities?

MT: That's correct, but they were still part of the group from the point of seminars, and this kind of thing. They were working on what I called strictly organic chemical problems.

VM: Then there were the other people who were not part of your own group who also occupied space in ORL. Rapoport's group, for example; Rapoport had one or more people...

MC: Not much.

VM: ...but he was there. He had some representation in the group.

MC: Not that I can remember. It made no impression on me.

VM: I'm trying to think of the name of the guy. There was a guy called Mel Look. Wasn't he one of Rapoport's people?

MT: Yes, he was one of Rap's. people — and Clark Lagarias: you remember him? A grad. student. doing organic chemistry related somewhat to photosynthesis.

VM: Not really. There were one or two people there.

MT: Yes, mostly graduate students who were doing organic chemistry related somewhat to photosynthesis.

VM: There weren't any others at that time. I think the others all came rather later after the initial photosynthesis people, after the initial photosynthesis period. People like Tinoco and Rod Park...

MC: They were much later.

VM: Much later, weren't they? I have to limit his effort to something otherwise I won't live long enough, so I am going to limit it to the path of carbon period, from the beginning to about '56, '57, just at the time I came and wrote the last two of the papers.

While you were teaching in Chemistry, did you have a lot of teaching to do?

MC: No, very little.

VM: You used to teach, I remember, was it a freshman chemistry, or

MC: No, a sophomore, course, a sophomore organic course first year organic.

VM: And you did that for years and years?

MC: Yes.

VM: Did you enjoy doing that?

MC: Not particularly. I did it, though.

VM: You had to, I guess.

MC: I earned my living!!

VM: You didn't have to run lab. classes or anything like that, did you?

MC: No.

VM: Or grade papers?

MC: No, I had graduate students and teaching assistants to do that.

VM: So you were able to spend all your time essentially thinking about research, reading and actually being in the building

MC: I had to prepare the lectures, which wasn't very hard to do.

VM: Was it the sort of course that didn't change very much from year to year, was it an introductory course?

MC: Yes, an introductory course. It was a sophomore course in organic chemistry and I did that for many years, but that's all I can tell you.

VM: Can we come on, as the photosynthesis began to develop and you began to get information, the first big thing, apart from PGA, that seems to me happened was your shift away from ion exchange chromatography to paper chromatography.

MC: I never used ion exchange very much.

VM: But you had to use it at the beginning?

MC: At the beginning, yes.

VM: How did it happen, how did you tumble to the paper chromatography possibility?

MC: You get a picture of the whole thing all at once.

VM: I know. But how did you realise this was the thing to do?

MC: I can't answer that. I don't really know. It seems to me that was obvious, so obvious that I didn't think about it even.

VM: Somebody has to take the first step and actually do something about it, produce a tank and paper and solvents and all the rest of the stuff. Somebody has to decide that today were are going to this; we haven't done it before. You don't remember how it started? What about the radioautography? I think that was rather an original thing at the time.

MC: **Yes.** All the work was done with radiocarbon. So, I could always get a picture of what I had even though I didn't know what they were, what the spots were. I got paper after paper with spots on them, and the problem was, what are the spots?

VM: I first met you in 1955 when you came to London to give a talk; I came (*to Berkeley*) the following year, and I remember you showing this slide, you were talking to the Chemical Society or perhaps the Chemical Engineers it was, and you had this radiochromatogram on the screen and I remember you saying to get the spots is easy, it's putting the names on them that takes the ten years! You and your colleagues spent a lot of time doing that.

MC: That's all we did. That was the $64 question.

VM: Then, of course, to find out what was happening carbon by carbon inside these compounds.

MC: That was another matter. First you had to find out what they (*the compounds*) were before you could do anything else.

VM: How did you do this? Inspired guesses? Where do you start, with a mess of spots on a piece of film?

MC: I'm trying to remember, and I'm not sure I can.

VM: I guess you guys stood around and argued about the toss what this or what that might be.

MC: I guess that's what must have happened, although there wasn't much of an argument. I said "that was it".

VM: Yes, but you could have been wrong.

MC: I was.

VM: Some of the time.

MC: I don't remember how it worked, exactly in detail, except that I did a lot of identification, mostly from the position of the spots (*on the film*). A few of them are known and that was enough to be benchmarks for this position. Then, I could guess at the others and eventually sort them out. That's how it happened as far as I can tell.

JO: Finally what you had to do though, wasn't it, to synthesise these compounds containing carbon-14, pure compounds, and see how they migrated?

MC: That was the very last thing.

JO: That was the confirmation.

MC: I didn't do very much of that.

JO: But that was the final confirmation.

MC: I didn't do much of that.

VM: You then embarked on this big program of taking the compounds to pieces and analysing them.

MC: That was a big job.

VM: And that's really an organic chemistry job to start with, isn't it? Do you remember who were the people who developed the methods for that?

MC: I did!!

VM: Who worked with you to do it?

MC: You can look in the history and see. I can't remember. I can remember PGA and I can remember the chemistry I did there to hydrolyse the phosphate off and then chop it up. The first thing you get is the CO_2 off the end, and then you get the other two. I remember doing that myself, personally. But that's all I can remember.

VM: Presumably the other guys developed methods for all the other sugars and then you put it together.

MC: Yes.

VM: At some stage, you, someone else, must have begun to realise that the thing was cyclic in its nature.

MC: I hear you, and I'm trying to remember how that happened. Well, it's just reasonable. You had to have something regenerated in order to keep the thing (*the cycle*) going. That was the fact, that's where the idea came from. It didn't start from CO_2, it started from something else: it starts from PGA — well, it starts from ribulose diphosphate which then picked up the CO_2 to make two PGAs. The recognition of that was a major, major step. To realise that was the first reaction from the CO_2.

VM: I suppose that became obvious once you'd identified PGA as the initial compound. It had to be something...

MC: Yes, but you had to find the ribulose because where did the PGA come from?

VM: You may not remember this, but I'll tell you something that I remember. You remember we used to go out to beer on Friday afternoons?

MC: No, but go ahead.

VM: We did. We used to finish early and we used to go to Laval's, up on Euclid. You didn't always come. We tried to drag you, but you weren't too keen. But sometimes we got you there. We used to go up and drink beer, the whole gang. One Friday afternoon, when it was getting close to beer time, you walked into ORL, into one of the big labs. there, and Duncan Shaw — do you remember Duncan Shaw? He was an ex-fighter pilot from Britain — he and I were playing on the blackboard with schemes. We produced the "dephlogisticated soot cycle" (*laughter*), and it started out with carbon dioxide polymerase to make polycarbon dioxide. You walked in through the door and, in your usual style, you said "What's that?" We took an eraser and started take it off, and you said "Hold it! Hold it! There may be something in it!". (*laughter*). But then we then got you out for beer pretty quickly after that because there wasn't anything in it. Those were the days when there was still argument about just how the cycle went. I remember that there were a lot of arguments about how all the wiggles went.

MC: It's surprising that you remember all that.

VM: Well, It was an important part of my life.

MC: But I don't remember it.

VM: You'd done so much of it that you don't remember the bits. I remember there was a time that there was a lot of argument going on between you and, what's the guy's name? Martin Gibbs was it? I don't remember what the details of this argument were.

MC: It was pretty violent.

VM: What was the nature of the argument?

MC: I don't remember but it was not very pleasant; that's all I can remember.

VM: Presumably they were contesting data or the significance of data. It lasted some months.

MC: More than that.

VM: It gradually died down and I think there was plain sailing after that.

MC: How do you remember all that?

VM: I don't know how I remember it. It just is there, you know.

MC: I hear you, I hear you. But the only reason it's coming to me is because you are telling me about it. That brings it up, awakens it in my head.

VM: I obviously don't remember everything either and in the course of this conversation you say things, and Marilyn says things, which remind me of stuff that I have forgotten. You don't remember everything. But, I have been doing a fair amount of thinking about this (*oral history*) and I have also been reading some of the papers, and it brings it back. You know, we have, like everyone else who was here, we have photographs all over the place, mementoes, those little plastic things, so it is all part of our lives. That's why I think this project is really so interesting and why I'm looking forward to meeting the other guys and seeing whether they all remember the same thing, for example. I don't know how much of what you say is going to be remembered by the other guys. We'll find out; I'll come back and tell you.

MC: I'd like to hear it.

VM: I have just started. It was a nice idea to start talking to you because, after all, you were the originator of the whole thing and we happened to be in California. We are on our way back from Australia and New Zealand, visiting our family in Los Angeles, and Marilyn thought it would be a nice idea if we came up now and talked to you. So, we did that. We hope to come back next spring and get down and do the hard work on this.

MC: By that time you will know more about it.

VM: I will have refreshed my memory more and there are a few people in England that I can get to easily in the meantime; and then we can come back and do as many as we can while we are here, including, I hope, I'd like to get to people like Sam Aronoff.

MC: I don't know where he is.

MT: He's in Canada but I don't know... He was at Simon Fraser University, Dean of Science there. So now probably the best thing that might work is to write them.

VM: Well, we can try. And then there's Alice Holtham, isn't there?

MT: I have already talked to her about this. I will see her in Seattle in March and I gave you her name and address.

VM: And is she willing to divulge?

MT: I'm sure she would be happy to participate.

MC: I don't remember her.

MT: She was the secretary in ORL, she used to do the drawing and type the papers over there. She's the one who put the fisherman in *Path XXI.*

VM: You remember the fish and the fisherman? Well, I have to remind you. This was Alex Wilson, wasn't it?

MT: Yes.

VM: Alex Wilson's big paper. The way I heard it from somebody or other recently, there was a drawing of this whole business (*apparatus*) with a tank in it. Apparently the JACS (*Journal of the American Chemical Society*) didn't like the size of the figure and they said it was too big and would occupy too much space and they would have to compress it. In order to get their own back on the publishers, they drew a little picture of a fisherman sitting on the edge of the tank (*in the apparatus*)

MC: I remember that.

VM: And when it was reproduced in the *Journal* it was so small, it's there, but it's so small that you need a microscope to see it (*laughter*). I gather that Alice Holtham was one of the people responsible...

MT: She was one of the protagonists on that.

VM: So, you know, stuff like that makes life entertaining. But you didn't know about that?

MC: No.

VM: In your discussion with Norberg (*oral history, University of California at Berkeley, 1976-80*) you commented there that you thought that interesting though the Massini experiment was with the light-dark, you thought that the critical thing was the Wilson experiment where he turned off the carbon dioxide. You remember there were these two experiments to prove the cycle. Massini did an experiment in which he started with the algae and the light and then he turned the light off and he got an accumulation of... What did he get an accumulation of?

MC: PGA.

VM: He got an accumulation of PGA, that's right and the ribulose went down. He turned the light on and it went back again. And Wilson did the experiment of dropping the carbon dioxide concentration and seeing these kinetic waves around the cycle. You don't remember?

MC: It doesn't matter; you remember.

VM: Well, I remember that but what I was going to ask you is that you commented to Norberg that you thought the Wilson experiment was the more significant of the two and I wondered why you felt that.

MC: I have no idea; I don't even remember what it was.

VM: I didn't see this, but apparently Wilson had two large vessels of carbon dioxide and he switched from one to the other, so he could drop the concentration from 1% or

something to essentially zero just by turning a tap; it was a flush-through system of some sort. He followed the radioactivity in the intermediates and he saw these interactive waves of concentration as the shock went around the cycle one way and went backwards around the cycle the other way. You don't remember that? You should read that paper; it's a good paper!

I think perhaps we might have a little break after my next question. Do you remember what it was like when the whole group finally realised that you had proven the cycle? You had got the complete system. How did you celebrate? Did you have a party?

MC: No.

VM: No party?

MC: There were parties all the time.

VM: So there was no special reason for a party.

MC: Not that I can recall, but that doesn't mean much.

VM: I remember in about 1958 there was the Brussels World Fair. Do you remember we built that enormous display of the photosynthesis cycle with the coloured lights? Well, it stood in the back end of the Round House, near the back door.

MT: Yes, it was finally taken away. We had the panels inside the Calvin Lab. for years.

VM: There were these panels with the cycle on it, with all the labelled carbon atoms, and as it originally worked there was a series of coloured lights which went around showing the progressive labelling of the cycle components. And I remember the way it was designed, it was going to drop a sugar cube out of it (*laughter*). But for some reason either they wouldn't produce the sugar or the thing didn't work at the end, so it didn't actually produce much sugar. You don't remember this thing? There must be pictures.

MT: Oh yes, I think there are some...

MC: If I see the pictures, I guess I'll remember.

VM: That was really the final seal of approval that this cycle actually existed.

MT: Paul Hayes was the one engineered the panels and got them built on the Hill and got them taken to Brussels, got them installed over there for the World Fair and everything. He baby-sat them for a while over too.

VM: So can we take a break?

(*After a break*)

MT: Professor Calvin asked me: do I remember when the definite cycle occurred? The best way would be to go back into the literature; just check it, you know, with the literature. Or go back and look in some of the other biographical information that I sent you.

MC: Well, there's a biography...

MT: Vivian has all that.

MC: The autobiography that I wrote.

MT: He has all that.

VM: I have that and I don't have all the papers but I have references to all the papers and over the course of the next few months I'll actually go through all the stuff and remind myself of all of these things. I think the idea of how it became a cycle is a very interesting one; I realise it's difficult to cast your mind back over that period and work out how it happened but I...

MC: I have no idea; I can reason it but I can't visualise it. I don't remember.

VM: Do you have any sense, during the whole business, of whether there were many false starts, when you had to backtrack?

MC: Yes, always, always: lot's of them. I can't remember what they were, but there were lots of mistakes which had to be corrected — or didn't have to be corrected but had to be ignored.

VM: So you went down blind alleys and then realised after some period that this wasn't going to the right place and so...

MC: I guess that's the way it worked. You're asking me now something which is not there, not in my head.

VM: Well, I remember one or two blind alleys, rather later than that. You remember Helmut Metzner and methyl phosphate? Do you remember that one?

MC: I remember Metzner, faintly.

VM: You don't remember methyl phosphate?

MC: What about it?

VM: Well, he came up with this new spot. You must remember how exciting new spots always were! Our lives were governed by spots. And this was going to be the answer to something or other, I don't remember now. It wasn't my project and I don't remember myself exactly what it is.

It turned out that the methyl phosphate (I think it was phosphate-labelled) came from the methanol that he had been using to kill the algae with but until that was realised, of course, it was very exciting. There was virtually a whole new theory developing on that!

MC: I can believe it.

VM: Well, I think that's what happened: that the excitement at that time was such that people were actually dreaming up things day by day. There were these ideas just floating around all over the place and people were talking to one another.

MC: Where did you work, in which building?

VM: I started working in ORL.

MC: You did.

VM: When I first came you suggested I work on deuterated algae and I worked with Ozzie Holm-Hansen to start with.

MC: Yes, I remember.

VM: You said that would be a good way for me to learn the ropes and it certainly was: a few weeks of that stuff with Ozzie and I knew how to do it. And then I worked on a whole series of other things including I did the last two papers in *The Path of Carbon*. You remember we found traces of the carboxylic acid. We did some experiments: we'd improved the chromatography and there were some new spots which looked as if it might be the dicarboxylic acid we were looking for but, of course, it fell to pieces very easily and we did some tests on it. I remember there was an organic chemist who came through the lab. at that time and I think his name was Angel. Does it ring any bells? From Australia, I think he was, quite an old man, elderly at any rate. Older, I think, than either of us was at the time. And I remember you talking to him and asking him what he thought of the breakdown patterns we were seeing and whether this was consistent with the dicarboxylic acid.

And then I also found the erythrose, erythrose phosphate, and I think that was the last paper until Andy did one much later, number 24 I think it was, which was ten years or twelve years after that. There was the cleaning up at the end but there was all the excitement of finding things — and the cycle was wrapped up but there were always bits round the edge which were not entirely clear and we spent a lot of time working on those. So I did that and then I did a number of other things. We began to develop the idea of how the whole thing fitted together in the cell, what the organisation was inside the cell and we began to take cells to pieces and see what the bits did.

MC: I don't know how you remember all that.

VM: I don't know *how* I remember it; it's just part of it.

MT: Well, part of it, Vivian, was that it was such an exciting period. In other words, when you're excited about something, personally or technically or whatever, you're going to remember it. And it was exciting, even for a non-chemist like myself, you could walk through there and feel how exciting it was.

VM: One of the most exciting episodes that I remember was a guy called Ian Morris, whom you may remember — do you?

MC: The name is familiar but I don't remember him.

VM: He came here in about 1966...

MC: Australian, wasn't he?

VM: No, English. He came in '66 as I remember, about ten years after I'd come for the first time. He came from the same department in London as I came and underwent exactly the same shock of seeing a place like this that I had. I had come from this stuffy English department into the excitement and ferment of the lab. here and Ian

Morris did exactly the same. I just watched him: I watched him react the way I'd reacted ten years earlier.

MT: And he's now living in the States.

VM: Well, he unfortunately isn't with us any more.

MT: I did not know that.

VM: He became the director, I think it's called the Bigelow lab. in Maine, the Bigelow Marine lab. or some such name as that, and eventually became very fat, I think, and eventually he had a heart attack and it killed him.

MT: I remember about you first coming; I was expecting a woman! (*Laughter*)

VM: So many people were, I'm afraid.

MT: It was a shock when Vivian walked in! (*More laughter*) Looking back on it, and becoming more familiar with British names as I've gotten older, I'm not surprised but at the time... And there was no way from the piece of paper you can tell.

VM: It was a name that was fairly common just around the time when I was born and I have come across a number of people called that but it has really died out certainly as a boys' name since then but there are a few of us carrying the flag. I'm stuck with it — there's not much I can do any more.

SM: I don't think that you can possibly have any idea of how exciting it was for newcomers to your group. It was so dynamic. This is an experience that must have befallen many people who have worked for you over the years.

MC: I have no recollection.

SM: This comes from your personality and your way of doing things.

MC: I suppose; I don't think in those terms at all so when you say it I understand it, but that's all.

SM: But it's a great thing to have been able to do, to inspire people in this way.

MT: We're probably down to the third generation now, when you think about it. There are people who have come through, graduate students who have established there own little biodynamic groups or whatever they have wanted to call them. It's way of life that scientifically gets transferred around. Murray Goodman will tell you that. If you talk to Murray, he's very articulate on that subject. It's true. You learn how to do these things and as a scientific parent you transfer that to the next generation.

SM: I think it may have, literally as a scientific parent, gone as far as Kevin (*our son*) who is terribly excited about his work. He is a molecular geneticist and he is excited about his work in the way that Vivian has been and the way that you (*Calvin*) have been. I think it all rubs off from you, going down as it were. So this must be true for many other people.

VM: When you started the group, you really had little idea of what it would turn into, did you?

MC: None at all. As a matter of fact, there wasn't any group, it was me. Then we added people here and there and pretty soon we had a group. (*To Marilyn*:) Were you in ORL?

MT: No, I was always in Donner where the administrative offices were.

MC: I remember that, but all the work was in ORL.

MT: The synthetic work and the animal work, and things like Charlie Heidelberger, was done in Donner. Your heart was in ORL but you actually had another group of people doing the radioactive work...

MC: There was a lot of synthesis.

MT: ...synthesis and animal work. People would synthesise the compounds and people like Charlie Heidelberger would do animal studies with them, and Ed Bennett did animal studies and Gerard (*Milhaud*) did animal studies. They were over in the Donner; that's where the animals were. So, ORL was the heart, but you had to have a little bit of assistance from Donner or you couldn't have gotten some of the things done that you got done in ORL like having the radioactive compounds

MC: I think you're right. I don't remember but you're right.

MT: I know because I was there.

VM: How did you come to get ORL?

MC: Ernest gave it to me.

VM: He was just able to do that?

MC: Oh yes!

VM: He was "king", was he?

MC: Oh yes!

VM: Did you go in before the (*37"*) cyclotron was taken out?

MC: There was a cyclotron next door (*the 60" cyclotron in the Crocker Laboratory*).

MT: The 37" cyclotron had gone, though.

MC: It had gone by that time?

MT: Yes.

MC: But the room was empty.

MT: That's right.

VM: So you moved into an empty building, essentially, and brought all your stuff or whatever in there.

MC: Well, gradually built it up.

VM: By the time I got there (*1956*) you were occupying the whole building essentially.

MC: I didn't ever more into the (*special room*) where the cyclotron was; there was a room there and I never really worked there. I could go in there, but I never really worked there. Most of the work was in the next door lab.

MT: You had a machine shop in there, too, you remember, which was very handy: a machine shop and a glass shop, which stayed in ORL until it was destroyed. We had the rest of the building, except for the shops, which was very fortunate situation.

VM: Were you aware during the early heyday of the group just how remarkable it was?

MC: No, I hadn't the slightest idea.

VM: Really. It didn't occur to you that not everybody operated in this manner? Let me tell you what was so exciting about the thing. First of all, there was a large group of people, all working in the same direction. That was unusual in biology at that time. I know it must have happened on the Manhattan Project and things like that but in biological research you didn't find that in the late forties and early fifties. Secondly, you were obviously the major contributor. There was a guy there who was the guiding stimulus. I hadn't come across this before. No doubt there had been other people in that position, but I hadn't seen it. The third thing was that there was a lot of money, not just money dropping around but what money buys: you wanted something, you got it. There wasn't any question: you cannot do this experiment because we cannot afford to buy equipment/supplies.

MC: Ernest gave me the money.

VM: I know but it was a very fortunate situation that it worked like that. Had you been stuck for money it would be have been much more difficult.

MC: I suppose.

VM: Then there was the idea, which was new for me at the time, that people were not reserved unto themselves. Everybody was interested in what everybody else was doing. They used to join in. You had collaborations, breaking and reforming.

MC: I had a seminar with everybody there; Friday mornings, eight o'clock.

MT: They all remember that!

VM: They were very scared of you.

MC: Really? Why?

VM: I'll tell you why. Because you would interrupt them...

MC: Oh yes, but that was my job.

MT: You patterned that after Gilbert Lewis — you had a good teacher.

VM: But some of the people were not used to it they would start, and you would get them in the first sentence...

MT: That was as far as they got.

VM: ...and some of the younger graduate students who hadn't seen much of this before were not ready for it. Some of them responded well enough but there were occasions when they were intimidated by this. It was a very vigorous discussion that went on.

MC: I can believe that because I was interested in what was going on and that was the only way to find out.

MT: Later on, in the Round House, when people got bigger and you had the formal senior staff and everything, the speaker would get like a week's notice. It first was a day, first it was 24 hours, then schedules were made up and you would get a week's notice and there would be a printed form used to tell exactly what speaker and what day. That was a late period in the evolution (*of the seminar format*).

VM: I don't know whether you were aware of them even at the time: there were some interesting hierarchical things that began to develop as time went on. In the beginning the only person who had a place at the seminar table was you. You always sat at that same right-hand corner. Everybody else, just sat around, whoever came in next. In the course of time it got formalised. The senior staff sat at the table and the junior people sat in the chairs around.

MT: The grad. students were in the back of the room!

VM: There was a lectern where you stood with your stuff: it all became high tech. with buttons to press for the slides.

MT: That was in the Round Building; that was once we got into the Calvin lab.

VM: In the beginning it wasn't like that. In the beginning, I don't even know whether there were slides; there were just blackboards and waving arms. And you brought the stuff and laid it out on the table for everyone to see.

MC: The chromatograms.

VM: That's right. It became much more formalised as time went on, inevitably, I think.

MT: (*Indecipherable*)

MC: You are bringing up things that have been buried for years.

VM: That's one of the things I wanted to do. I realised that you would not have thought about many of these things and I wanted to try and bring them up.

MC: Well, you're doing it.

MT: One of the things that is interesting is from Professor Calvin's 69th birthday. We had a big party over in the Round Building, with cake and everything, and Al and I had sent letters to people saying would you write a letter to Professor Calvin and tell him what your experience was and so forth. Those letters were fantastic. They would be a source of information for you, Vivian.

MC: Where are they?

MT: Right here. If you read those letters, you will get the enthusiasm and how it changed people's scientific lives and what they remembered. Your going over to Laval's was in your letter, Vivian.

VM: Well: you can see how important that was!

MT: You should go back to Laval's; the beer is still good. That was an interesting thing because people recollected what affected them. It showed more of what the lab was like, not so much what the science was like, but what living in that lab was like.

VM: That's really what I want to do. The science has all been written; I don't want to write that all over again, that's old hat. But the lab., apart from these sorts of records, doesn't exist in a form which is accessible to anybody outside the circle.

MC: That's true.

VM: I think that's a pity, because it was a rather unusual place and it would be nice to...

MC: I suppose; if you can do it.

VM: If I can do it. Well all I can do to start with is to try to collect some information. How exactly to handle it I don't know. Sheila and I started talking about it and the first thing was that clearly we had different ideas of what might be done. I'm inclined to write it in an almost a story form.

MC: What's the matter with that?

VM: Well, nothing, if I can do it: I've never done anything like that before.

MC: Well, that all right.

VM: Yes, there's always a first time. I'll try; I think I'' try. But, there might be other things to write about as well. There might be actual lessons to be learned as well as a story. I don't know — this is the first morning and I have got a lot of tapes to fill in before I've got the whole story. And now there's all this collection of letters; we'll have to have a look at those.

MT: Well, you can borrow them. They are all Xerox copies of the letters. Professor Calvin has the originals of these letters in his house. But I Xeroxed them all and I have them in the office here.

VM: Perhaps we can have a look at them during this trip and see how they are. Are there hundreds and hundreds?

MT: Oh yes. We got a fantastic response.

MC: To what?

MT: Well, Al and I decided that since you were having a big party in the Round House to celebrate your 69th birthday. There was a huge cake over there in the Round House and Ed McMillan came and Don McLaughlin, people who were involved in creating

the building, Bob Connick and everything. And we asked people to write letters to say what had being in the Calvin Lab meant to them, what was their experience. Some of them were funny; everybody had their own ideas. John (*Otvos*) wrote one. These all came in and before you (*Professor Calvin*) took them home (and I don't know where they are up there), I Xeroxed them all...

MC: Oh that's good.

MT: ... and I had them bound up. They're probably lost if they're up at the house; I don't know where they are.

MC: I have no idea.

MT: These are Xeroxes. Some of the letters came in a little late, so they're stuck in out of order, etc. It was a very interesting response, a very good response. People loved what they did here. They were excited about it. They were excited about being in Berkeley. Where else could you go to breathe tear gas when you walked to the bank among other things.

VM: That was post-photosynthesis.

MT: Yes, but it wasn't post-lab. There were a lot of people — this is not just about photosynthesis I'm talking about. There was a lot of excitement in Berkeley then — there still is!!

JO: I have a short addendum about your discussions about the seminars and the questions. Somebody told me this when I first arrived, just what you said about how people used to sit around the table, and occasionally when Professor Calvin was late, he would come dashing in and he would glance at the screen and before he hit the chair he was asking questions. (*Laughter*)

VM: You were a great question asker. Something else occurred to me just in the last few minutes. When did you know that ORL was going to be demolished?

MC: I don't really know.

VM: Presumably you had fair warning that something was going to happen.

MC: I think so.

MT: Dick Lemmon probably would be your best source for that. The ORL was destroyed in 1958 and they had to find space for us in the Life Sciences Building. It was probably at least one year. You see, the planning for Latimer had been done and we knew it was going to come here, so it was probably at least a year before because I do remember that Dr. Calvin and Ed McMillan stood out there and watched it go and I did, too, watch it come down. We watched the big ball hit it, demolish it.

VM: You can't remember, can you, what your thinking was at the time the building was going? What it would do to the operation?

MC: No.

MT: It spread it out even further.

MC: It stopped it.

VM: That must have been a matter of concern to you at the time.

MC: I suppose it was.

VM: The thing that occurs to me, just at this moment, is that the whole of that operation in ORL was actually very tied up with the nature of the building. The open plan of the building was something that was an essential aspect of the way everybody interacted. And you commented in your Norberg responses, somewhere, to this open plan aspect. Did you not learn something of this from your time in Manchester with Polanyi? Did he have an open plan lab. there?

MC: I don't recall.

SM: You have talked about the fact that the lab. in Polanyi's group, there were doors between the labs. and also doors opened onto the corridors.

MC: Yes.

SM: This meant that people could move in many directions and encounter each other.

MC: I can remember that.

VM: ORL had very few doors. It didn't have none: it had one on the outside, which is around somewhere?

MT: It's in the Smithsonian. (*Laughter*)

VM: Inside it had few doors and I remember Al had a glassed-in office but apart from that the place was pretty open. I think that encouraged people just to move around. There was very little sense of "my space" and "your space".

MC: Your desk was yours.

VM: Your desk, but not much more than that.

MT: I think the counting room was the only other room which was separate because you had to keep the counting room...

VM: That was underneath

MT: It was downstairs and you had to keep that separate.

VM: When the threat came to demolish that building there must have been some concern about what it's going to be like without it. I remember the basement down in LSB was a very different operation.

MC: We weren't there very long.

VM: Four years, five years.

MT: Five years between the time you moved down there and the time we walked into that round building.

MC: I hadn't realised that.

VM: Do you remember anything about the planning for the round building, the concept? Did you think it up, did someone else think it up?

MC: No, I did.

MT: And the senior staff, too. You had meetings with... and the campus architects...

MC: What was the name of that architect?

MT: Michael Goodman. First it was going to be like a half-circle and that didn't work out and then we had a full circle. It's all written out in some of the things I have given to Vivian already. It was Dr. Calvin's idea and the senior staff supplemented it. Paul Hayes was the one who bird-dogged it.

VM: You wanted to retain some of the aspects of ORL, some of the interactive aspects, and was it your concept, as far as you remember, to have this round building with people facing the middle?

MC: Yes.

VM: Do you think it worked?

MC: Yes it worked — it's still there.

VM: The building is there but did it work the way you hoped it would? You felt satisfied with the building the way it was put together?

MC: I didn't think about it. I think so but I didn't pay that much attention to it.

VM: Those of us in the building felt that the building worked well...

MC: It did.

VM: ...but it was different from ORL. It was a modern building with good facilities. ORL didn't have good facilities. It had its own charm, but good facilities wasn't one of them. It wasn't a well designed building.

MC: It wasn't designed.

VM: Somebody must have put it together.

MT; It was built in 1885 or something and it just grew.

VM: Was it as old as that?

MT: Yes. It was a "temporary" building.

VM: Are those temporary buildings still down (*in the centre of campus*)?

MT: If you walk down through the grove they've all been knocked down. It's beautiful grass and flowers and everything; it's just lovely. In fact, the class that I graduated

with at Berkeley is giving a memorial glade for people who died in the war down through that area. It is really, really nice. It's worth a walk.

JO: There's one more thing about the building (*the Round House*) being designed and thought up by Dr. Calvin. The evidence for that is that very shortly after our group (*i.e. Calvin's Latimer Hall group*) moved out of the Round House, in 1980 I guess it was, all sorts of conversation barriers were put up on the second floor and now you can't see from one end of the floor to the other end any more. So that area of congeniality disappeared very soon after he left the building.

VM: Got an explanation?

JO: The person in charge of the building (*Professor George Pimentel*) didn't have these same ideas and it went on according to somebody else's concepts.

MT: But the big white table came over to the Chemistry Department for the graduate student lounge on the fourth floor.

VM: I must go an have a look at it.

MT: Is that the round table?

MT: Yes.

VM: The round table or the square table?

MT: The round. The square white table was up on the third floor, the rectangular table actually; it's still up on the third floor in the round building. But if you want to call it the (*round*) coffee club table was on the second floor, the big white Formica top table which, after Professor Calvin moved over to here (*Latimer Hall*) and it didn't seem it would be useful over there, the graduate students over here (*in Chemistry*) it's in their lounge now.

MC: Where's that now?

MT: Fourth floor, 427 or something; it's in Latimer.

VM: What I was less familiar with myself at the time, and so I ask you: What was the atmosphere like in Donner compared with the atmosphere in ORL?

MC: There wasn't any atmosphere there.

VM: Why do you think that was the case?

MC: Well, there wasn't anybody living there.

MT: Don't say that!! I was there for 13 years, and Bert lived there and Dick lived there, and Ed Bennett lived there. It had no atmosphere, I have to agree with that. But part of the thing was that you didn't really care what went on in Donner, so you very seldom came over there. And so, you were always in ORL. If we wanted to see you, people went to ORL. You did not come into Donner.

MC: I didn't come very often.

MT: You rarely came and that was because it (*the Donner Laboratory*) was not your building, like ORL was really your building. John Lawrence controlled the rest of the building and also it had the same kind of architecture as Latimer — you are in little offices with doors opening. It's very difficult to mingle when you have to go from room to room and open the door. There was no real way to have a congenial group.

MC: Yes, but in ORL that wasn't the case.

MT: Donner was a typical lab building, like they are still building all over the campus — corridors, buildings, doors. ORL was a real opportunity which you were able to translate into that other building.

MT: Into the Round House.

MT: Into the Round House. There will never be another building like that.

MC: Why not?

MT: Because first of all it is too small and the university can't afford to put little buildings up any more. Secondly, people don't think that way. Very few people think the way you think. Donner had no atmosphere at all; it was just like Latimer.

VM: Marilyn, of course, is right. I think that the Round House is an expression of the unitary character of the group. It was built for a group, it was custom-built.

MC: Well I designed it.

VM: I know but what Marilyn is saying is that sort of opportunity doesn't exist any more to custom-build a building for a group, or at least it's very rare these days. It was rare then, I suppose.

MC: I expect you're right.

MT: It was pretty rare then.

VM: It was just very fortunate that you were in the right position at the right time to do it.

MC: I don't remember where the money came from. Some of it came from Kettering, didn't it?

MT: Not all of it. The State of California came through, Kettering came through with the last $300,000, the National Institutes of Health and the National Science Foundation built us the building; it was equipped by the AEC. But the actual funds for the building were about four different sources.

MC: I had to go find them

MT: You had to find them. You had to write a lot of proposals — no electric typewriters, no Xerox machines, no computers. I have copies of all those proposals.

VM: All in that office out there?

MT: Yes, it's a gold mine.

VM: Does the Smithsonian know about your office?

MT: No, and the Bancroft (*Library*) doesn't either. One of these days I guess they'll know but I did not send that stuff down there. It's just incredible: from about 1958 to 1960 you were writing proposals for the building, at the time that Glenn Seaborg was chancellor.

MC: For the Round House?

MT: For whatever building you were going to get; it turned out to be round. You had to get the money first. To get an architect you had to have money on hand So, we had to have some money in hand before you could even talk to someone like Michael Goodman to decide what kind of building you wanted. So all these different proposals were sent to different people and the Kettering people supplied the last little bit.

VM: I remember the day that you stuck the spade in and you turned the first sod. It was an wet day, wasn't it, as I remember?

MT: Overcast — it wasn't a very good day.

VM: And the ground was a bit soggy, muddy when you did it, but it was an exciting day, nevertheless, when it happened. Do you remember the day when we marched in...

MT: I remember that.

VM: ...or the day when (*Arne*) Tiselius, was it?, who opened the building formally?

MT: The dedication was very nice — a special seminar and the whole thing. But I was remembering last week, and I think I told John about it, that his (*Calvin's*) first meeting in his office (*in the Round House*) with all the furniture and everything was on November 22nd, 1963. He was talking with Howard Cary, the Cary spectrophotometer man, at the time when (*President*)Kennedy was shot. I went and I told them and I opened the door and said that "President Kennedy has been shot". I think you (*Calvin*) didn't believe me. But I had my radio on, and in an hour or so we all went home. That was your first formal meeting in your office in the Round House.

VM: You hadn't been in the building very long.

MT: No, I think I moved in two days before. Everything was still in packing boxes everywhere.

VM: I wasn't her (*in Berkeley*) at the time. I was in Israel on a month's project with the International Atomic Energy Commission. So, I'd marked all my stuff for shipment up from LSB (*Life Sciences Building*) to where it ought to go in the building and then I took off. I think I wasn't here when you actually moved into the building and started working

MT: It was pretty much about a month that people were moving in..

MC: Which building are you talking about?

MT: The Round House?

MC: It's been so long since I've been there that I have even forgotten it. I wasn't in there very long.

MT: '63-'80 — seventeen years.

MC: Really?

VM: Time flies, eh? I'm wondering whether perhaps we might not close for today and let me and Sheila talk it over and see what we'll think about for tomorrow. We have come through a lot of stuff quicker than we might have done. I am happy to go on chatting, but I can't think of more things which are relevant. I think it might be helpful if we thought it over. Do you?

SM: Whatever you want.

VM: I have actually run out of the immediate things I can say to prompt you (*Calvin*). I could go on talking but it's not going to be particularly relevant.

SM: Perhaps Marilyn can think of some things.

MT: One of the things you might want to think about in the long run, and you might have a separate section, is to describe how this (*the Calvin group*) was the first interdisciplinary lab. in the entire world, I believe.

VM: ORL or the Round House?

MT: ORL. The group; I don't think you can attach it to a building. I am talking about the laboratory in the sense of doing research. If you think of all the things that went on went on — chemical evolution, botany, psychology, planarias, educated mice — if you think of that concept and look at the things that we did, the things that the senior staff and the visitors did, everybody now talks about interdisciplinary labs., it trips off the tongue very easily, everybody has one, they think, but we were the first. I think that's a real interesting thing. I think as you talk to the people you are interviewing with this in mind you're going to get a lot of responses that might be part of the path of carbon in one sense but they are also part of the creation of a laboratory and an environment that was very, very special and unique.

VM: And it really started in those early days.

MT: In those early days. It would be interesting to speculate whether during the entire time of the group if everybody had been together, how it would have been. But (*until* 1963) we were always separate and there was the group over in Donner, there was a group in ORL, there was a group way down in the basement of Chemistry. We were all part of the same thing. But, we were also different.

VM: That's very good. It's down on tape and its accredited to you.

MT: I don't care about that; I think it's an interesting story.

VM: It gives me something actually which I hadn't thought of, an ultimate focus of where the story leads to, is the creation of this interdisciplinary group and indeed an interdisciplinary building to go with it.

MT: It was almost by accident, not accident because Professor Calvin was interested in all these things (*i.e. areas of science*). It took not only his ideas but it took receptive people who would go for this. A lot of people only want to stay in their little box. So he had people around him, or he picked people, who would do that.

JO: Some people who have tried to copy this arrangement haven't been so successful. And I have always thought that the reason is that what they have put together is a multidisciplinary laboratory rather than an interdisciplinary lab. and then they kick the habit.

VM: Those two comments from both of you, I think, are very helpful in providing a very good possible focus for where the thing is going to head. I say "the thing", whatever the thing is by the time I get to it.

MT: If you ask people, particularly people like Alex Wilson, Alice and Al (*Bassham*) and Dick (*Lemmon*) and Bert (*Tolbert*) and Ed (*Bennett*), but Ed wasn't involved, they could give you the nuts and bolts of how the lab. worked. But you ask Alex Wilson and some of the others that who have been gone a long time how unique this atmosphere was and what effect it had on their lives and so forth, I think you'll find interesting answers.

VM: Thank you — very helpful. So shall we give you a rest at this point?

MC: I think that would be helpful.

VM: OK — I'll turn it off...

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VM: This is an experiment. We haven't altogether decided what this is going to turn out to be, but we were thinking of two possible outcomes. One of them is to write almost a story account of life in the lab., but it depends on how much information we get and what sort looks like, and the other one is try and do something more academic with it. That's one of the things I'd explore with you this morning. Essentially, how this very wide interdisciplinary group grew out of a relatively small beginning, because it started with you and it has fanned out into a group of a hundred people, with lots of through-put. That's something which would be interesting to explore. Maybe we could do that in a formal sort of way, depending on what the information looks like. But it depends what we collect, you know. It's not like a scientific experiment where you know the form of the outcome. We really don't. It's quite new for us.

Perhaps we can start with that, and later on we can talk about some of the comments people made in these letters (*letter collection of 1969 for Calvin's 68th birthday*) and see how you remember them.

MC: I don't remember them

VM: Well, we'll remind you. I just had a quick look at the letters this morning and there are some quite interesting ones that people have sent.

MC: What is that book?

VM: This book is letters sent to you on the occasion of your 68th birthday; that was a party. You said yesterday that you were always having parties; well this is one of the

bigger parties. What I would like to explore with you is the way in which from the beginning of your work before the photosynthesis started, you gradually expanded out into so many different sorts of areas.

MC: I suppose; I don't remember.

VM: Do you remember how you started, on the pigments and the phthalocyanines?

MC: Yes, I guess so. Go ahead.

VM: You went from there at the end of the war, you had the availability of C^{14} and somebody told me — maybe you can remember this — that one of the sources was a lot of ammonium nitrate slurry which had been sitting around the reactors for a long time. That's all I know. Can you remember any more about that stuff?

MC: No. Just to get it and you extract the carbon.

VM: What was the ammonium nitrate slurry doing around the reactors? Who put it there? Did you put it there, or was it there for some other reason?

MC: I don't know. I really don't know.

VM: You had access to it?

MC: Yes.

VM: And you did the chemical extraction of the carbon from it? What was it, ammonium carbonate by that stage?

MC: I don't remember. I suppose so.

VM: You had this C^{14} and you started...

(Short break)

JO: I was under the impression that the ammonium nitrate was put there for this purpose, to see if this transformation took place with the neutrons.

VM: Do you know who put it there?

JO: No.

MT: Could it be Joe Hamilton, who ran the Crocker Lab. and who was a colleague of Ernest Lawrence? The Crocker was it's own environment. It may be in the book *("Isotopic Carbon".)*

VM: So you had this stuff and you then began the Donner (*Lab.*) development of the isotopic carbon work and the parallel work in photosynthesis, and you saw those very much, did you, as a unitary operation.

MC: What do you mean?

VM: You could see that the two activities were supporting and interacting with one another?

MC: Oh yes, that was me.

VM: That was you. But you didn't stop there. You began to acquire other interests beyond that. I remember chemical evolution which something that was going by the mid-fifties. Had you had a long-term interest in that question?

MC: Not really.

VM: How did you come to that one?

MC: I don't remember. I have no recollection of that. She's (*i.e. Marilyn Taylor*) got something.

MT: When you had your heart attack (*in 1949*) and you were in Kaiser Hospital. You read a book by George Gaylord Simpson on "The Meaning of Evolution" and that sort of twigged your interest in this kind of thing. And then, next door to ORL was the 60 inch cyclotron, so right across the alley was a place where you could go and check some of the ideas that you had. And you followed Oparin's work of the twenties as a general interest. You decided, with Andy (*Benson*) and Joe Hamilton and a couple of other people in Crocker, to do whatever it was you did. (Not being a chemist, I don't remember.) You did something in the cyclotron, a similar experiment to Stanley Miller's, but you didn't get amino acids. You didn't take that next step to get amino acids so that's why everybody thinks Stanley Miller started chemical evolution. Really you started it, but you never did the second experiment to get to the amino acids. You got carbon dioxide, formaldehyde and that kind of thing.

MC: I've no recollection of that.

MT: Andy worked on that, Joe Hamilton and Don Moore(?). This was a preliminary experiment, but you didn't really follow through experimentally for quite a while, until Dick Lemmon got interested in the project and then there was a long period of time when those kinds of experiments were done, with Dick and Cyril (*Ponnamperuma*).

MC: I remember the lab. The shakers were in the corner, in ORL, and that's where our algae were. And we kept them going all the time, harvesting new ones all the time. But that's all I can remember of all of that.

VM: Do you remember going to Russia to meet Oparin?

MC: No.

VM: Because I think you did that in about '57, was it?

MT: '57.

MC: It could easily be. But it didn't impress me very much.

MT: That was the first conference (*in 1957 in Moscow*) on the study of origin of life (I think we're now up to thirteen or something), but that was the first time that all you people like Sid Fox, Oparin, Stanley Miller and yourself and Oparin and all the other people in the world that were working on this (*i.e. chemical evolution*) got together. It was very special because it was in the Soviet Union. That was a difficult place to go

in 1957, lots of paperwork, the Russians were very hospitable and so forth. When you came back you wrote a piece for Chemical Engineering News called "Diary of a Meeting in Moscow"...

MC: I did? Well it's in there someplace.

MT: Yes. ...it told about the whole trip, the institutes you visited the whole discussion on the origin of life.

MC: Well, that's in there somewhere.

MT: It's in your reprints.

VM: Do you have any memory of what followed and the people involved? I remember there was Cyril Ponnamperuma — you remember him?

MC: Yes.

VM: He was quite a guy, wasn't he? Do you remember where he came from, how he came here? I've know him...

MC: She knows.

MT: He was a graduate student (*from Ceylon*).

VM: I knew him in London, before he came here (*to Berkeley*).

MT: He came here as a graduate student. He was an older graduate student because he'd worked before. He got into the Donner and he hooked up with Dick Lemmon and worked on this chemical evolution thing and that was his whole life, professionally and everything. And afterwards he kept going on that until the day he died (*December 1994: his laboratory at the University of Maryland was called "Laboratory of Chemical Evolution". He served for many years as a special adviser on scientific affairs to the Sri Lanka government*).

VM: There were some other people. I can only remember some of them. One of them was Geoff Eglinton; do you remember him? He worked in that area as well.

MT: I don't know whether he did or not, but I remember Geoff.

VM: I saw him recently. He is alive and kicking.

MC: Where is he?

VM: He's in Bristol, retired, but still working. He was a Professor of Geochemistry, I guess. Who else was there?

MT: Geoff actually worked on the organic geochemistry project (*and the analysis of lunar samples; this work was funded by NASA*). (*In the chemical evolution area*) Christof Palm work with Dick Lemmon. There was Leonard Spicer — I'll have to go back into the literature. There were quite a few people who worked on it: Anneliese Schimpl. If you look at the list of reprints on that subject you can pick up the various names. And there was another different group that worked on the organic geochemistry, on the

Apollo program, through the Space Sciences Lab.: Al Burlingame, (*William Van Hoeven, Jerry Han*) people like that.

VM: The next thing I can remember (the chemical evolution work was in the fifties) was in the sixties you began to become interested in genetic control. I remember you asking me a question which you thought I ought to know (and I should have done, but I didn't) about how bacteria control enzyme synthesis. I remember saying to you at the time, this was one of these conversations in the lab., and I said I would look it up and tell you. And about three weeks later I admitted I couldn't find it and I would do it. And that set me on ten years work, doing it myself, so that was another thing got that added to the lab. But it really started because you asked that question. Had you not asked that question my life would have been quite different. You remember that?

MC: No.

VM: We published some papers on that in the sixties.

MC: I can believe it.

VM: And then what happened next? Then there was the planaria: do you remember the planaria?

MC: Yes.

VM: What turned you on to planaria?

MC: They were simple organisms, animals, as opposed to plants. That's all I can tell you.

VM: You don't remember what you were hoping to get out of them?

MC: No.

MT: Ed (*Bennett*) could tell you that.

VM: There was a guy who came to do it. There was a lot of trouble. Do you remember the story how the planaria were learning, you could teach them, and then you chopped them in half and each bit remembered? Do you remember that?

MC: No, but that's interesting.

MT: There was a sister-brother or man-wife or something (I can't remember their names).

VM: There was a guy who came...

MT: ...from Indiana somewhere?

VM: ...from Duke? I can't remember what his name was (*McConnell*) but he came to work on this stuff here because you couldn't repeat their results, and when they came they couldn't reproduce it either. There was a guy: ginger-haired, losing his hair — Alan somebody or other (*Jacobson?*).

MC: I would have to look at the list. You know they published all this stuff in "The Worm Runners' Digest".

VM: Do you remember "The Worm Runners' Digest"? Those were great times with these very excited conversations about whether they would or whether they wouldn't.

MT: Jan Alvarez worked on that, too, Luis Alvarez's wife.

VM: In here? In the lab.?

MT: In Donner and also in the round building. Ed (*Bennett*) is the person who knows all about planaria.

VM: The next thing that I remember, this is all fitted together in the sense that you were sitting in the centre co-ordinating everything, and, secondly, the people involved were interacting with one another, because it was that type of building (*now referring to the, not ORL*).

MC: Was that in ORL?

VM: That was in the Round House.

MT: It started in Donner and then came to the round building; the planaria work.

MC: Never in ORL?

MT: No — ORL was gone by that time.

VM: The last thing that I remember you getting involved with, but by that time I was on the verge of leaving and there may have been things later, was the cancer work, which was the late sixties and early seventies. I remember that you went to a cancer conference, I don't know why you decided to go to this cancer conference, in '69 or '70, and you came back very excited, full of ideas about what you wanted to do. And that, I think, started it.

MT: Then people like Ercole Cavalieri came on a Damon Runyon Fellowship and he had to work on cancer. He was an organic chemist and so they got into the whole business of photochemistry. I think that was pointed out in the Norberg volume. And then you had grad. students like Joe Landolph, and Dave Warshawsky. There was a whole group of ten or fifteen people who came to the lab. to work on various aspects of the cancer problem.

MC: I didn't remember that.

MT: It all started from that meeting and also from the fact that when Ercole came he had to work on something to do with cancer; there was no way he could use that money without working on cancer.

VM: Then I really lost track because I was only an occasional visitor after that. Was there anything major that developed after the cancer initiative?

MT: Part of the cancer project was when they built the Cell Culture Lab. on the roof (*of the Round House*) and employed two new people for the senior staff — Jim (*Bartholomew*) and Mina (*Bissell*) — that activity was quite central for some years, until they all moved up on The Hill (*with its own building*). They were on the roof of the Round House for some years, ten years maybe.

VM: There weren't any other radical departures into new areas after the cancer, were there?

MT: I don't think so. Dick Lemmon always working in the area of hot atom chemistry and isotope effects; there was the work done by Mel Klein and Ken Sauer and Al was working on his metabolic metabolism studies, Al Bassham.

VM: I can understand, you know, how you got involved in the chemical things, because you are a chemist, but NMR: wasn't that a bit unfamiliar to you?

MC: Well, I learned it.

VM: Was it difficult?

MC: No.

VM: Really? Because that was a major activity. Wasn't Power Sogo the first guy who came into the lab to do that kind of work?

MC: Yes.

VM: I have no idea how you got into that. Do you remember how you became involved?

MC: It was a matter of structure determination. That's all I can tell you.

VM: It was a developing technology which was coming into chemistry and you recognised the value of it?

MC: I guess so.

VM: And there were other things: there was ESR, I remember at the time as well. Did that develop very much? I don't know what happened to that.

MC: No.

VM: NMR turned out to be the more powerful technique.

MC: Well it did, yes.

VM: What else was there at the time? There were other sorts of novel pieces of equipment around, but I am getting out of the depth of my memory.

MT: Some of the older equipment was automated, the chromatography equipment for example, and the algae culture became much more automated than it was originally. When we moved into the building (*i.e. the Round House*) all the equipment was updated so there were much more modern facilities.

VM: That activity of Dick Lemmon's, with hot atom chemistry, you remember he used to fire carbon ions into compounds and got interaction between them.

MC: I don't remember.

VM: You don't know how that came about, whether that one of your ideas or whether it was separate.

MC: I have no idea.

MT: Wasn't it kind of problem with the degradation of radioactive choline chloride? It started over in Donner with Dick, and when these compounds, which were made in Donner, were sent away, there were problems with degradation and there was some kind of isotope effect.

VM: Choline chloride, I remember, was one of these very radiosensitive compounds and that became an object of study as to why it should be radiosensitive. That was part of the whole isotope philosophy of looking at these things.

MT: When we got into the new building had a whole room with nothing but hot atom equipment— you remember that great big thing that Dick (*Lemmon*) and Wally (*Erwin*) and Irv Whittamore and Ben Gordon and Wally Erwin worked on. That's later.

MC: Over there?

MT: Yes — the Round House. They'd wanted that equipment before, but there had been no place to put it in Donner so when the building was built, that was part of the building and it went in there to continue that research. It was there for ever until it got taken out about five years ago.

VM: One of the things that which was unusual, to say the least, about that building (*ORL*) and the way the whole group developed in later years, was that it seemed perfectly natural to you and to the people in it that these (*interdisciplinary*) activities arose within the context of the whole envelope of the group. But I think for anybody looking from outside, this would be a totally unusual phenomenon. Each of these activities would have merited an group/institution of its own. And yet here they all were, welded together under one thing, including the name of the building. Remember the name that you gave to the Round House, to the laboratory?

MC: LCB.

VM: But do you know what the "CB" stands for?

MT: Laboratory of Chemical Biodynamics.

VM: You had dreamed up that name. I don't think that anyone had heard of a name like that before. It was a good way, in only three words, of describing a very wide thing. In fact I recall once calling someone and talking to them and giving them my address as the Laboratory of Chemical Biodynamics and he said, "what, no astronomy?"

JO: You have come up to a point where we haven't said anything yet about artificial photosynthesis.

VM: Oh yes, I'm sorry — go ahead.

JO: That started somehow when Helmut Tributsch came here and a paper was published about photosensitised photocurrents in semiconductors and their solutions. That grew and that's when I started, in 1976. Tributsch's experiment was done in 1970 and things were going along rather slowly, and I was told that I was brought here partly just to co-ordinate that particular subject and that's just ending now. We have had 25 years of that.

VM: Are you there?

JO: No. We have lots of interesting things to follow up..

VM: So it's good science.

JO: Yes.

VM: That reminds me that you were also involved comparatively recently with diesel trees.

MC: Yes, there are pictures of them there.

VM: I heard stores when I came through —

MC: That's one back there.

VM: Is that a diesel tree?

MT: (*Indecipherable.*)

VM: What happened? I gather the stuff worked all right.

MC: You got latex out of it, but that's all. I didn't pursue it beyond that.

VM: Could you use it in diesel engines?

MC: No. It had to be purified.

VM: Expensive?

MC: No. But there wasn't enough of it to make a significant impact.

MT: But the purification was expensive.

VM: I guess there's just too much oil around at the present time.

MT: Too much cheap oil. It got to the point with this research, where Dr. Calvin was involved and they were doing field studies and harvesting the stuff there were plantations of the hydrocarbon-producing plants which were being harvested and (*indecipherable*) and they got out a material from these plants which could have been used as a chemical feedstock. But the price of oil tumbled again and there was no economic justification. All that knowledge that was built up by a lot people over a 10-15 year period just disappeared.

VM: Well, it hasn't disappeared; it's there. It'll wake up again some time.

MT: It will wake up again sometime. They had the California Energy Commission interested, people all over the world (*India, Japan, African countries, Spain, etc.)* wrote and asked about it. Developing countries: there's not a problem with the cost the labour was so cheap.

VM: You see relics of these initiatives. When we drove in the other day there were all the wind farms (*out near Livermore*); these propellers.. There are a bit redundant these

days. They're not very pretty anyway; they are ecologically not terribly desirable. And I believe they have turned out to be much too expensive.

MT: PG&E is still forced to buy power from them. That's one of the reason our power is 50% higher than the state of Oregon. The windfarms were set up here and in Northern and Southern California and the utility companies have to purchase the power. When they (*the windmills*) went in, everyone said "we need power, we need power", so the government agreements were set. I was reading about this in the *Wall Street Journal* the other day and the discussion in California about the deregulation of electricity costs, so they (*utility companies*) are forced to buy the power (*from the windfarms*) even if they don't really need and of course that affects the price which is not (*indecipherable*) in the market. We could have another oil embargo and the power (*from the windfarms*) would look pretty good again.

VM: Another thing that we might talk about. In the sixties you joined Kennedy's Presidential Science Advisory Committee (*PSAC*). You must have been in a very interesting position on that committee because of the wide range of activities back here in Berkeley.

MC: I don't know. I suppose so.

VM: Did you feel that you were able to draw on many types of experience?

MC: I never thought about it that way. I just did what I did.

VM: I presume that the Committee didn't discuss the details of photosynthesis very much, broader issues than that.

MC: Yes, that's right.

VM: You by then had a fair experience of broad issues in this range of things. Your wartime activities as well, on the chelates. Are you in a position at this stage to almost philosophise about what it all adds up to? Do you want to hazard a guess?

MC: No — it didn't get very far.

VM: This is part of the exercise I'm doing now, it's such an interesting phenomenon that I think one must make sure that...

MC: ...it doesn't get lost.

VM: ... doesn't get lost. As the initiator of it...

MC: That's just the way I worked.

VM: So you had little idea at the beginning that it was going to develop in the way it did?

MC: No. I didn't think about things like that.

VM: You just went ahead and did it. But you thought, I'm sure you didn't think just of today and tomorrow. You must have been thinking of what's going to happen next week and next month.

MC: Not very much. I was more concerned about the algae and the shaker, would they live and would they produce what I wanted them to do, and so on, than I did about the long-term outcome of it.

VM: You mentioned yesterday that when you started the photosynthesis thing you really had no concept of how long it would take to solve the problem.

MC: No, of course not.

VM: You thought — once possibility was that it might be pretty quick.

MC: I didn't think about it that way. I never thought about time, how long it would take or anything like that. I don't recall ever thinking about things like that.

VM: After a little while it must have been pretty clear that it was not going to be a very easy problem, because as the problems developed and puzzles showed up and you were beginning to collect a lot of people to do the work. So that's an indication that it wasn't going to be over quickly, at that stage. I guess by the time, by about the end of the forties or so (*to Marilyn Taylor* — and you were already here by the end of the forties), the group was already fairly big.

MT: Probably 35-40 people.

MC: Really?

MT: There were two groups.

VM: It must have been one of the largest academic science groups here except, maybe, for The Hill itself. Did other people have groups of that size then?

MT: All the professors in Chemistry had their own groups, but they weren't this big.

VM: Even then, after just a few years, yours must have been one of the largest groups on the campus.

MC: I never thought of it that way; maybe it was.

MT: The other big group in Chemistry was up on The Hill with Glenn Seaborg. That group, of course, was never on the campus; it was always up on The Hill.

VM: Were there pressures to move your group up to The Hill?

MC: Not that I can remember. If there were, I didn't do anything about it.

MT: At the time when it became obvious that ORL was gone and you had to have a new building, you were offered space up on The Hill, somewhat near where Mina (*Bissel*) now is.

MC: Yes, but I wouldn't take it.

MT: You said you would not take it because you wanted to be on the campus. But the AEC at that time would have been perfectly willing to put building for you up there.

MC: But they put it up here.

MT: The AEC did not put any money...

MC: No, Kettering did that.

MT: I know but I'm saying the AEC would have given you the money for a building on The Hill somewhere near where Mina is. That was an option you didn't choose to exercise. Therefore, you had to go and get the money for the building.

MC: That's when I went to Kettering.

MT: No, Kettering was the end, the last $300,000. The State of California, the National Science Foundation and the National Institutes of Health and then Kettering. Kettering was the last little bit.

MC: That was the last $300,00.

MT: Yes, that was the last little shot. You had gotten all this other money before and then you were able to get money from Kettering.

VM: Why didn't you want to go up on The Hill?

MC: It was away from the campus.

VM: That was an important factor for you?

MC: It was to me, yes.

MT: you wouldn't have been able to interact with the Psychology Department, the Anatomy Department, the Botany Department, the Heaven knows what else you would have worked with near as easily on The Hill. Even though The Hill is most wonderful, it's isolated from the campus. I think being on the campus has a certain advantage scientifically for the interaction you are talking about.

VM: So your own ability to migrate easily from your office in Chemistry to your office in the round building was partly...

MC: Mostly it was from my office in (*Old*) Chemistry to ORL, the old wooden building next door where this building (*Latimer Hall*) is now.

VM: But that was destroyed and something had to happen.

MC: Then I went to the Round House.

VM: You went to LSB (*Life Science Building*)...

MT: That was five years later. *Then* you had to go between your office in Old Chemistry and the Life Sciences Building.

MC: Oh yes, I remember that — that was a terrible thing.

MT: You had that (*electric*) cart; that car was always where you weren't.

VM: That really must have been a severe jolt to your scientific lifestyle.

MC: Well, it was but it didn't last very long.

MT: Five years is a long time.

VM: I think that had a serious effect on the relationship between the two groups of people...

MT: It was very serious effect on the relationship between the group in Donner, up here, at this end of the campus, and then the people in LSB. It was very difficult to have communication: it was a long way up and down the campus and between the Old Chemistry Building: it was like a triangle.

VM: And everybody was getting a bit older and that hill was getting steeper every year. That's why you had a cart.

MT: The idea of the cart was wonderful. It was very practical, but he (*Calvin*) would drive the cart down to LSB...

VM: Oh, he drove the cart himself...

MT: Yes, he drove the cart down. And then his wife would call and she would pick him up in LSB and then the cart was at LSB. The next day, you would be up in your Old Chemistry office, and where's the cart? It was down there where you left it. There was one of the graduate students and one of the things he volunteered to do was to keep the cart in the proper place; it was John Eastman.

VM: Do you remember this car?

MC: No, not really.

MT: But it was so funny: John Eastman, when we had that thing (*i.e. symposium*) for Dr. Calvin in 1989. his little a speech (I don't know whether John remembers that or not) was about his responsibility for the cart. He was the cart man.

VM: I remember you in the cart, actually going up and down the hill. But it was a real pain having to commute .

MC: It didn't last very long.

MT: Five years!!! That's a long time. I remember it vividly. I had to walk from the Donner Office to the Life Sciences Office to your office (*in Old Chemistry*). I spent a lot of time walking. It was good for me, but it wasn't easy to keep up any kind of communication or relationship between the two groups under the circumstances.

MC: I hear you: I hadn't realised that.

JO: When was his heart attack, in sequence of all these things of moving...?

MT: In 1949, very early in my career at the lab.

MC: It only laid me out for a few days.

MT: I bow to differ with that. You were home for a long time...

MC: ...how long?

MT: At least two-three months. I drove up to your house every day, bringing the mail, taking dictation, for a long time.

MC: Really! What do you mean by a long time?

MT: At least two to three months. You would have people come up to the house, come up to see you. You would call up Al and tell him you wanted to see him about this and that and he would go up to the house and you would call whatever and there was a constant stream of people up there. But I was up in the house almost every day for quite a long time.

MC: I believe you but I just don't recall.

MT: Not only did I take dictation, I made coffee!!

VM: Down in LSB the accommodation was much less open, of course, than it had been in ORL and it was later in the Round House, but it wasn't totally enclosed. There were a couple of big labs, there, I remember, one of them roughly in the middle of the long corridor and then there were individual rooms — you were right: they were not very nice down there. In the big lab in LSB it was possible to recreate something of the atmosphere of (*ORL*).

MT: It was actually. I have a picture of Martha (*Kirk*) cutting hair in the big lab and that sort of thing. The parties around the table — there was that one big lab. where a lot of work went on and there were individual rooms...

VM: I think it was a great relief when we came up to this round building. And I remember the discussions that went out about what sort of building one ought to have and at that time there was overt discussion about the philosophy behind the group.

MC: Who built it, what was his name?

MT: Michael Goodman.

MC: He did what I asked him to do.

MT: Well, after discussion.

VM: I remember on many occasions, and I think Al must have been one of the prime people and Dick...

MT: And Paul Hayes, of course.

VM: ...discussing, since it was a clean sheet building, you know, at the beginning there was a possibility of suggesting anything, one didn't know how far it would get, but at least *suggesting* anything. And trying to recreate in the new building what people felt had been the best, particularly of ORL, I don't think anyone wanted to create what LSB was like, but recreating ORL in a modern building. The philosophy of putting all the individual work space in a big lab., in one half of the building, and all the service facilities which had to be in rooms because of noise, etc. in another part was excellent.

MC: That was the way it was in ORL.

VM: Yes, but it required an analytical understanding of ORL in order to be able to design the new building and that's what came out of it. That building (*the Round House*) was almost perfect: one or two mistakes, we made.

MT: ORL?

VM: No, the Round House. I remember that we forgot to put floor drains in the freezer room but apart from a few minor things like that it really worked very well indeed.

MC: It's still working.

VM: It's still working. There was a great fear at the beginning that it would be very noisy because everybody would be in the same lab. But it turned out not to be. The rooms were big enough so that the noise didn't carry and people just weren't that noisy so it was OK.

MC: The big labs were half-labs. (*half of the building*). The benches were on radii; I guess they still are. There was a sort of centre (*on the second floor*), where the blackboards were, and that's all there was to it.

MT: And then the staff offices all the way around the edge. And the rooms which had to be for themselves, like the isotope lab. and that kind of thing, had their own spot.

MC: Well, it's still that way, isn't it?

MT: The uses of some of the rooms have certainly. If you go through there now, if you look at rooms that used to have the isotope lab., where you did radioactive synthesis, that's used for something else. The function for some of these rooms has changed as the research has changed.

JO: I thing also one of the reasons that the sound problem didn't develop was because there are no two walls in that building that are parallel. There is no way that a sound wave can hit a wall twice and it just gets disbursed.

MT: Even the offices.

VM: I'm sure that's a factor. But also internally the rooms, soundwise, are very broken up with pieces of equipment so that you wouldn't get flat reflecting walls. But you said the idea was to have the benches on the radii and then an inner radius would have the common equipment that people were using, and then you get to the discussion centre in the middle. It worked pretty well.

MC: That was my design, with Mr. Goodman.

VM: I think, as I recall, they insisted on putting a red tile roof on it so that airline pilots flying over could make sure where they were.

MT: If you look around the University almost every building, including the new one right here next to us (*in Latimer Hall*), has a red tile roof. There must be a Regent some place...

VM: ...someone who owns a tile company.

MT: Cory Hall over there is one of the few that doesn't have a red tile roof.

VM: But I think the Round House was designed well.

MT: It's like a little cake, a cake with a (*red roof*) frosting.

VM: In fact, when you won the Nobel Prize, there was a party, and there was a cake (*in the shape of the building*). Do you remember the cake?

MT: I have a wonderful picture of that cake.

MC: Where was it, at my house?

MT: At your house.

MC: Not the Round House?

MT: No — the Round House wasn't even started at the time you got your Nobel Prize.

VM: There was a cake, in the shape of this Round House to be, and there were napkins (*with a picture of the building on them*), weren't there?

MT: That's right.

MC: There were what?

VM: Napkins.

MT: The cake was nice. I have a few of those. The cake was wonderful. Terry (*Taylor, her husband*) was taking pictures and I have a marvellous picture of the cake before it was cut into.

MC: Did it have candles on it?

MT: No — not the building, not for the Nobel Prize. It was in the shape of the building.

MC: Candles sticking in it, though.

MT: I don't remember that.

VM: I remember also, on the occasion when you got your Nobel Prize, there was the problem of your speech.

MC: What speech?

VM: That you had to make in Stockholm. I don't know whether you remember the way you used to make speeches. But, you had no script, and you used to sit on the edge of the table and talk, and then at some point you would realise the time was nearly out and you had hardly started, and so you would rush through the things (*which was usually the newer research results*)...

MC: Really?

VM: Oh yes, that was quite common ...and you knew that this had to be different for the Nobel Prize speech. So you did the speech and I remember that Paul Hayes and I were sitting with a stopwatch. You had to finish in 40 minutes otherwise all hell breaks loose. We got it for you and we put clues in the thing and I think Gen (*Calvin*) was going to sit in the projection office with a script and know when to tell the projectionist to change the slides so you didn't have to say the words and you would save a few seconds there. I wasn't there but I gather you finished in time, which was rare for your speeches.

MT: It was all organised and all the hot stuff didn't come at the end, boom, like that.

VM: Yes, you did tend to do that. The introductions to your speeches were very good but they went on a bit.

MT: They want on for a long time; I almost knew some of them by heart!

VM: People wanted the "meat" at the end and they got it very fast.

JO: I had some input later on. I programmed one of these little calculators to flash the digits progressively along the speech. I put it into a cycling pattern: the first two would flash, and then the next two. When he got within 15 minutes of the end of the speech, he could see like a thermometer when he was getting to the end. This was the old type that had a little paper in it, so it could print out, and part of the program was that when it got to be one minute from the end, the paper started to come rolling out of the calculator to catch his attention.

VM: Did it work?

JO: I don't think he used it very much. That wasn't his style.

VM: I can understand. It takes away the spontaneity, doesn't it, if somebody tells you how to talk and it's not your style.

MT: The most interesting experimental stuff (*i.e. information*) always came at the end and people didn't have time even to absorb it before it was all over. I think that was frustrating for them sometimes.

VM: They had to go away and read the papers after that.

MT: Or have a question and answer session.

VM: That reminds me of something else. All the papers, lots of people wrote papers, and I remember through the period when I was publishing with you, you read everything pretty carefully, didn't you...

MC: I tried to.

VM: ...and you would make your comments and corrections. There was quite a lot of debate, argument one even might call it, from time to time about what should go in and what should come out. That must have taken a lot of time, reading all that stuff.

MC: Well, that was my business. I didn't worry about the time.

JO: But there was a system to it. He didn't like to look at papers until they were practically perfect. So there was a time we came in and said "this is the last session" and the authors are here. And then he went through the paper word by word. When that was over, that was it! He never saw it again and it went out.

VM: There was a marvellous thing happened in our lives. The most important thing that happened to any of us was the development of these typewriters that you can alter things with. I remember that first one we had was the memory typewriter? The secretaries had a hell of a job, you can imagine, every time we made a correction they would have to start retyping the manuscript...

MT: ...and making more mistakes.

VM: Do you remember this typewriter?

MT: He wouldn't remember.

VM: It was a big thing.

MT: It took up one whole corner of my office. It was huge. It had two tapes, one on each side; oh dear.

VM: Now we have these little desktop things. They are very clever.

MT: I had one of the first electric typewriters at the Lawrence Berkeley Laboratory, the Lawrence Radiation Lab. When I came to work for Professor Calvin I had an old beater and I had been there about one year and I was upset with the typewriter. So Bert (*Tolbert*) put in an order for an electric typewriter and I had the second. And I have gone through every single thing since.

VM: That was pre-golfball?

MT: Oh yes.

MC: Pre what?

VM: These were little balls that bounced around, balls with the letters in typeface, and they bounced around in the right position.

MT: There were many different kinds of typefaces and symbols and that kind of thing.

VM: That was high technology, 30 years ago, 35 years ago

MT: I had one of those for about 20 years.

VM: (*To Calvin*) Did you ever learn to use a computer?

MC: No.

VM: Let me recommend it. It's a good way of wasting your time.

MC: It's too late now.

VM: Let me comment on one or two other things as we are getting on here. Let me tell you about some of the letters (*that were written to you in 1979*)There is a letter here from Kent State University written by Ed Gould. I don't remember Ed Gould (*to Marilyn Taylor*): could you remind us about him?

MT: He was a tall, dark-haired...

MC: He was here for a while, wasn't he?

MT: He was here for a year. Professor of Organic Chemist (*from Kent State University in Ohio*)...He came on his sabbatical and worked on the third floor (*of the Round House*). He was strictly an organic chemist; I really don't know exactly what problem he was working on.

VM: What he says in the letter is. I'll read you his birthday greeting. This was for your 68th birthday: Some of them are obvious and don't need any comment. He says: "Birthday greetings to Melvin Calvin: "Professor, scientist, scholar, institute director, author, editor (that's all plain sailing) — molecular architect". What do you suppose he means by "molecular architect"?

MC: Designing new molecules.

VM: You designed new molecules?

MC: Sure; that's what I was doing most of the time.

VM: (*continuing Ed Gould's message*): "Radical prober".

MC: I don't know what that means.

VM: Did you work with radicals?

MC: Oh yeah. That happens.

VM: "Mechanistic detective".

MC: (Laughs) Who wrote all that?

VM: Ed Gould. The next thing he calls you "A virtuoso of the isotopic trail...

MC: He's a poet.

VM: "Unraveller of the greening enigma, Nobel Laureate".

MC: What book is that?

VM: This book is the letters which were sent to you on your birthday. I just had a quick look that morning and I wanted one or two to talk to you about, and this one jumped out at me. "Savant". Are you a wise man, you reckon?

MC: No.

VM: "Master of porphyrin excitation, Carousel ringmaster, Critic" (you're certainly a critic), "Friendly but insistent inquisitor" How does that grab you? Do you see yourself as an inquisitor? "Sceptic, Quizmarshal".

MC: What does that mean?

MT: Seminars. You're sitting and quizzing.

VM: "Gadfly".

MC: That was my job.

VM: "Scientific statesman" (now we're getting up market a bit), "Interdisciplinary catalyst, Fountainhead of new ideas". And then he says essentially that it was a pleasure to know you.

MC: Who is this?

VM: Ed Gould, writing from Kent State University the Chemistry Department. Then there's another letter here from Peter Hammond, do you remember him?

MC: He's in England now.

MT: No, he's at Livermore; he never went back to England

VM: He (*Peter Hammond*) says "the multidisciplinary group (that's your group here) was an excellent tonic after the strict organic synthesis school of the University Chemical Laboratory, Cambridge". I guess that was Todd, wasn't it?

MC: He was a professor there.

VM: What sort of a guy was Todd?

MC: Oh, my. What do you mean what sort of a guy was he? He was an autocrat.

VM: He was not an interdisciplinarian?

MC: No.

VM: He was a straight disciplinarian! Was he an autocrat? But he was good, wasn't he?

MC: You did what he told you to do.

VM: But he was a good chemist?

MC: Yes. He got a Nobel Prize.

VM: But he never branched out into lots of things, the way you do.

MC: Not that I can recall, but that doesn't mean much.

VM: We had a guy called Neal something, who came from Todd's lab. in the fifties, who worked on pyridine nucleotides, nicotinamide adenine dinucleotide. You remember this guy, Neal somebody, a chemist, a young man? He went back to England, as we

did. I was a postdoc. here and went back to England and you suggested it might be nice for me to come back. I wasn't sure at that stage, but six months in England in 1959 convinced me. The same thing, apparently, happened to Peter Hammond. He went back to England and his wife apparently jolted him into coming back and he sent a letter to you saying that if there was a job going in Berkeley he would give his right arm for it. Is he one-armed now? Was there a job in Berkeley?

MT: No, but he got to Livermore and he stayed there the rest of his professional life. He's now retired.

VM: And then there was Barrie Hesp? Do you remember Barrie?

MC: Oh, yeah. Where did you dig up all those names?

VM: He came from John Barltrop, he was one of John's students.

MC: I remember John. Barltrop was in Cambridge.

VM: Oxford. He (*Barrie Hesp*) came here in the early sixties and he worked, I think, with a guy called John Turner, who came here from Liverpool and worked on porphyrin something; he was a chemist. He was very impressed with the interdisciplinary activity of (*indecipherable*) and how much his stay in the Round House influenced his later life. I think you will find many people were in that position Then he joined the pharmaceutical industry. He's in the States now. This (letter) was still written from ICI in England but later on he came to the States, I think also with ICI. Then there's another letter here, this isn't exactly a letter. This is from Lorel (*Daus*) Kay.

MC: I remember her.

VM: She talks about the pleasant memories, and then she summarises her reactions. This is the time you were approaching retirement as director of the Round House. She says "They're shelvin' Melvin!!!, No way!! I'll bet a rare isotope, He's just as active in the 1980s as he was in the 1950s; here are C-14 reasons why, she says:

C1, Ceaseless energy;

C2, catalytic activation of everybody else. That was absolutely true. You used to say things to people which got them moving in ways they hadn't necessarily thought of.

C3, consideration for his group — now, that's true.

C4, The Calvin Cycle, in every biology text now, including the one she teaches from.

C5, Chromatography.

C6, Counts/min.

C7, Chlorophyll;

C8, was Chlorella;

C9, was sedoheptulose and ribulose phosphates and PGA;

(*Tape turned over*)

C10, Curiosity and capacity for new ideas. I don't think you ever felt, did you, that there was something which was not in your bailiwick?

MC: Well, I did whatever I had to do.

VM: Whichever direction it went?

MC: Yes.

VM: C11, candour.

MC: What does that mean?

VM: It means you always spoke your mind. Did you?

MC: I think so.

VM: I think so, too.

C12, Creativity concerning creation of life.

MC: Well that was evolution.

VM: C13, she says, Calvin

Then, she says, C-14 reasons why you'll still be as much a Contributor as ever to the Chemical Biodynamics Lab. And she sends you happy birthday greetings.

The last one I have here is from Hans Kornberg; Marilyn put a little sticker: now "Sir" Hans Kornberg. Did you know he's "Sir Hand Kornberg"?

MC: No, I didn't.

MT: We have a lot of "Sirs" (*as alumni of the group*).

VM: There's Rod Quayle and George Radda and Hans Kornberg...

MT: ...and Ted Abraham.

MC: What does that mean?

MT: They have been knighted by the Queen.

MC: So what!!!

MT: It's nice to be called "Sir".

VM: It depends on your taste. Some people like it; I think Hans quite likes it. And Geoff, I think, Geoff Wilkinson. Do you remember Lise Schou? She is married to Geoff Wilkinson, who is a Nobel Laureate in chemistry. We had dinner at their house a few months ago. I contacted her about this (*project*) and she said we must get together. She is very helpful. I haven't talked to her yet in detail, but I will. I will remind you of something else before coming back to Hans: when you went on sabbatical in

Oxford as Eastman Professor and you lived in Eastman House, which is still there, Bob Rabin and I arranged a dinner in London for all the ex-members of your group, and that's the first time I met Lise Schou. It was in University College.

MT: I remember the house in Oxford. It was next to the playing field.

VM: That's right and I remember that Gen was very impressed with that book which was written by (*George*) Beadle's wife called "These Ruins are Inhabited". I have never read the book but she obviously read it. Did you ever read it?

MT: It book was written from a different point of view!!

SM: I remember that Gen wasn't very happy with Eastman House because it didn't have a fence around it. She felt it was like living in a goldfish bowl.

MC: That's right. It was.

VM: Subsequently, I think it has a hedge round it; I think we saw it not too long ago. Anyhow, to get back to Hans (*Kornberg*). He says: "My reluctance to discuss experimental findings, and even have them announced in seminars given by other members of the group elsewhere in the United States, before I could be reasonably certain of their accuracy, led Clint Fuller to stamp my notebook with an imposing purple SECRET stamp. Unbeknown to me, this now desecrated notebook was left on the bench and to my dismay, I was handed a security violation citation by an armed policeman the next day.

MC: Who was that?

VM: Hans Kornberg. So, were there armed policemen walking through the building?

MT: Yes. When we had classified material, in Donner the classified material was there for a long time — I forget when it finally left — there were policemen coming through and one of the things they checked for was whether the safe was closed, whether there were any documents on the desk and so forth. (*Policemen also walked through ORL.*) This was just a joke document, but the policeman wouldn't know that.

VM: There wasn't any classified material in ORL was there?

MT: No, but there had been (*in the early forties*). There was quite a bit in Crocker and actually a lot of the things we did in Donner when I first came to work we had to use sealing wax, and that kind of thing. So there was classified work going on there.

VM: This is not the end of the story. He says: "Luckily Melvin was not in Berkeley at the time".

MC: Who says that?

VM: Hans Kornberg. "And the inexorable progress of my file from security office to security office was neatly aborted by Bert Tolbert who tore out the offending page and with a disarming smile said that he saw no page marked SECRET in the book. Although the matter was thus ended, I also remember the chill horror that I felt when on my way back to England I opened a marvellous book on the beauties of California to find that the title page was stamped THIS DOCUMENT CONTAINS INFORMATION VITAL TO THE SECURITY OF THE UNITED STATES. I had

visions of spending the remainder of my fellowship in jail." But he didn't. And, instead you know, he became Master of Christ's College in Cambridge and I visited him there about 18 months ago when a microbiologist in London died who happened to be his cousin and there was a memorial meeting there. Hans was a very good Master of Christ's, I think, but now he has retired from that and from Cambridge and has gone to Boston University, whether permanently or on a temporary basis, I don't know.

JO: This is a little out of order, but can be fixed up. When you were talking about the fact that Professor Calvin always did what he wanted to do and never went from one thing to the other reminded me when he retired, formally, and became emeritus it was in the newspapers and the Daily Cal sent a reporter to interview him to get some impressions. She was a young girl, a freshman or sophomore, and she sat here (*in Calvin's office*), and I was in here, too, and the first thing she asked him was "Well, Professor Calvin, now that you have retired I suppose you'll now be able to do all those things that you always wanted to do!" He almost threw her out of the office.

VM: Do you ever remember doing anything you didn't want to do?

MC: Sometimes I had to, it was forced on me by the circumstance. I don't remember what they were, but I am sure there were occasions like that.

VM: But you never had onerous administrative jobs in the Chemistry Department. Were you ever Chairman of the department?

MC: No, no.

VM: You never want to be?

MC: No, no, no.

VM: So you avoided committees, did you?

MC: Yes.

VM: What about things like the Academic Senate, did you play much of a role in that?

MC: No, not much.

MT: You were on the Educational Policy Committee for some years, which was one of the more important committees on the campus; I don't remember whether or not you were chairman, but you were on that committee for at least three-five years. And that was quite a time-consuming thing. The committee meet frequently and it was an important committee for setting up (*educational*) policy for various types of things. And then, of course, you were always on promotional committees — that kind of thing.

MC: That was departmental.

MT: University-wide sometimes because sometimes Chemistry had input into Biochemistry and things like that. I remember the Educational Policy Committee as being a fairly onerous type of task, you can't slough off something like that: you have to go to the meetings, pay attention, write reports and that went on, I think, for at least three years.

MC: I don't remember that, but it's probably true.

VM: What about the directoral obligations on The Hill? I remember you were one of a panel of directors.

MC: As director of the Round House.

MT: You were an Associate Director of Lawrence Berkeley Lab. also.

MC: That was because (the group was a division of which was a division of Lawrence Berkeley Lab.). You had to go along to meetings and take part in all the discussion.

MC: I guess I did bit I don't recall very much about that. I didn't impress me very much.

VM: I can see why. Because you often ask one of "us" to go and replace you (*at the meetings*) when you were away somewhere and they were rather boring meetings so I can see why you wouldn't have been terribly interested. But I guess they all took time. You sought to avoid them.

MC: Of course.

VM: But you did like to travel around and meet other people and go to meetings.

MC: I did a lot of that.

VM: You enjoyed that?

MC: I don't know whether or not I enjoyed it, but I did a lot of it.

VM: You learned a lot of stuff from those places?

MC: Well, I was invited and I went. I didn't ever initiate the action that I can recall.

VM: I'm sorry to keep remembering these things, but I remember one occasion in the early sixties, about '62 or so, when it followed this thing that I mentioned earlier on about you asking me on enzyme control mechanisms. When I'd looked into this a bit, I realised that many of the interesting people who were working in this area were in Europe. I said why don't we get them here? And you said: "We'll see about getting some money". And you got some money — from NSF?

MT: From NSF. We had a group of seminars for two separate years.

VM: We had some discussion about who to invite. And we invited people mostly from Europe — there was an odd Russian and there was someone from Israel, Michael Sela from Israel. There were 12 of them and I was their host.

MC: All at once?

VM: No, they came successively — over two years?

MT: Three years?

VM: Something like that — and each one spent a week in Berkeley and they were guests of "us", of our group, but we also, of course, shared them with other interested people on the campus and they gave a scientific seminar and a public lecture and they communed around in the place generally, and we took them about. I remember the question arose about whether we should offer them any money. We paid all their expenses, but do we also offer them an honorarium? There was some discussion about how much you offer people like this, prominent people.

MC: I don't remember it.

VM: I remember how we resolved it. I said to you: "Suppose somebody invites you to Paris for a week and pays all your expenses and offers you $100...

MC: $100!!!

VM: This was the 1960s...would you go?" And you said "no"; "$200", "no"; "$500", signs of wavering. (Laughter) So we settled on $1000.

MC: Did I get it?

VM: You didn't get it, you didn't go; it was for these other people. We gave them $1000. And most of them came, not all of them. We invited Hinshelwood, but he didn't come. He was one of the few who turned us down. We had a good group of visitors on that occasion.

MT: I have all of those archives, too.

VM: Yes, I bet you do! We felt that the Americans were relatively easy to get here because the distances were not great and they travelled around all the time anyway. But the Europeans were more of a problem because they travelled less, especially in those days. You don't remember them coming?

MT: That was part of the (*indecipherable*) with the building opening. Remember (*on April 1st, 1964*) we had the dedication of the building when it was brand new...

MC: The Round House?

MT: The Round House...and the proposal was made, here we had this new institute and one of the nice things to do would be to have a series of special visitors presenting seminars. You applied to the National Science Foundation and convinced them that that would be a good idea and the invitations went out. It was initially for only one year but it was so successful you got a second year. So, for the two years after the building was opened we had these seminars, I think it was in the spring; they all came in the winter before and did their thing. It was very successful, I think. It did two things. It first of all brought people here to meet with others (their colleagues on the Berkeley campus) and also put the laboratory on the map as a scientific institute. If you create something, a new building with a lot of people in it, but nobody knows who it is or what it is. So, we got a lot of publicity through the university to have these people come.

VM: As Marilyn said, it put the Round House more on the map than it otherwise would have been and also faster. And it also, I think, emphasised this interdisciplinary attitude, because these people were over a fairly wide range of biological and chemical interests, and they were all guests of this group.

MC: You remember it, and I don't.

MT: You haven't asked this question yet, but you might in discussing the laboratory, its dual function as an Organised Research Unit of the University of California and its function as a division of Lawrence Berkeley Laboratory. We are a dual-functioning institute and that is very unusual.

VM: Did this dual-functionality of the group influence the way it developed, do you think?

MC: I don't know. I have no idea.

MT: The dual-functionality did not come into existence until 1960, which was when the building plans had gelled and the money for the building was available. When we were ORL and Donner, or Donner and LSB, we were only a division of Lawrence Berkeley Lab., the Rad. Lab...

VM: But always with a teaching function.

MT: With a teaching function but we were not a separate University entity. This University entity was created in 1960. Part of the reason for that was to get the money. People like Kettering could give money to the University but they couldn't give it to the Lawrence Berkeley Laboratory because that was AEC or whatever it was at the time *(i.e. a government laboratory)*. We had a dual thing: it was an administrative and also a financial avenue. The financial avenue continued for a long time. When people wanted to give money they could give it to the Laboratory of Chemical Biodynamics as opposed to trying to do anything through The Hill. That still continues: money still comes into the laboratory as an Organized Research Unit (*and is administered by the laboratory as a University institute*). You always wore hat you wanted to wear: sometimes it was better to be campus institute and other times a Hill-connected group. So you could whichever was best at the moment. It was very handy.

VM: You were always able to place graduate students into these activities.

MC: What do you mean?

VM: The graduate students that came to you essentially from chemistry.

MC: I would persuade them to do whatever they wanted to do, so to speak.

VM: There was no difficulty with putting them into the units which were all AEC units.

MC: No.

MT: That was where the support came from. We also had graduate students from biophysics, botany, psychology and biochemistry, so it wasn't just chemistry students.

VM: That means that you had...

MT: ...faculty ties to these other groups.

VM: I remember that you became Professor of Molecular Biology at one stage...

MC: I guess so.

VM: ...in addition to Chemistry and you gave a course in chemical evolution...

MC: I don't remember but maybe so...

VM: ...and I think it was the course either based on your book or on which you later wrote the book (the book *Chemical* Evolution which Calvin wrote at Oxford while Eastman Professor).

Why don't we take a break at this point, take a deep breath and think whether there are any other topics that we should bring up.

MC: Good, good.

After a break:

VM: Could you talk a little bit about all the support staff in the lab., the technicians and dishwashers and the secretaries and the carpenters and all the people who kept the thing actually going.

MC: Well, "she" (*i.e. Marilyn Taylor*) did.

VM: She was one of them, but there were lots of them.

MC: But she did everything.

VM: What does he mean?

MT: I don't know what he means.

VM: What do you mean that she did everything?

MC: I didn't do anything.

VM: We had all these people in the place, they must have come from somewhere.

MC: Well, she did it.

MT: No, no, I didn't do it. The senior staff in the lab. was responsible basically for the support staff. In other words, they took care of hiring them, and nurturing them, showing what they wanted to have done. The senior staff, really, under you, who set the tone for the laboratory. They were the ones who together would interview people for jobs. It wasn't any one person who would interview you for a position; for example, Martha Kirk was interviewed by several people, Ann Hughes was interviewed by several people. We got the carpenter from ORL, he just moved over, also the glassblower. And then we had a dishwasher.

VM: Yes, Alice.

SM: We were talking about this and the point about it was the fact that there were all these remarkable people who stayed here virtually forever and it was very much part of your group and these people were happy to be in it and felt that it was a home to them. This was the point.

MT: We felt like a family. Coming to work was like coming home. I think most people performed in that kind of a situation. When necessary, you did 150% or 200% or whatever because of the situation that we had in ORL, in Donner and, of course, eventually, in the Round House. Particularly for the office staff, it was very interesting: there were four of us that were there for 20 years, that's a very unusual thing in an office staff. There was myself, Lois Soule (you didn't know here), Beth Klingel and Gloria Goldberg. We all worked together as a group. What had to be done, was done. There was no "boss" or people working underneath someone. Having been over here in Chemistry now for almost 15 years, I can see that that was an unusual situation. I am sure John (Otvos) will agree with that, too. Very cohesive situation. The same thing existed with the people who assisted in the laboratory, people like Martha (*Kirk*) and Ann Hughes — we all worked as a team and felt that what we did was important.

VM: And there were all the people who were offering various sorts of technical support directly to the scientists: the carpenters, electronics people, those sorts of guys, whom you could absolutely rely on all the time. They were very helpful and they were very good and competent.

MC: I didn't hire them.

VM: But you knew them all, didn't you?

MC: Yes.

VM: And you learned some most amazing things. I remember from Mr. Norman, the carpenter (*from ORL*), he taught me how to carry a full cup of coffee along the corridor because it tends to spill. He said what you have to do is keep moving it up and down as you walk. (*Laughter*)

MT: I remember the glassblower (*Bill Hart*). If you needed some little thing personally, you could always go down to the glassblower for it (*first in ORL and then in the Round House*). To be able to have our own building, with these services — a machine shop, a glassblower...

MC: You're talking about ORL?

MT: no the Round House — was very unusual. We had our own "goodies" — electronics shop, computer support. That was very unusual, it made working there for the scientists much easier than if they had to go through the ordinary bureaucratic situation (*which existed elsewhere on campus.*)

VM: The secretarial help — Marilyn is being modest — in those days was even more important than it is now when everybody has their own computer. In those days, they didn't and the secretaries did the typing, did the drawings for the figures. Nowadays you do all this kind of thing on the keyboard. But then, you didn't. You needed to have the skills, reliability and a degree of imagination, particularly when you came to the drawings the scientists would only give approximate indications what they wanted and they expected other people to make a good job of it. And the journals always wanted the drawings to be very well. You my remember that we walked yesterday about Alex Wilson and his diagram with the fisherman on the tank. Do you remember that?

MC: I remember that, but I don't remember how it came about.

VM: It came about because...

MT: Alice Holtham drew it.

VM: ...the journal (*Journal of the American Chemical Society*) wanted to compress the picture into a very small space and these guys got their revenge on the journal by putting the little picture of a fisherman which was then was so small you could hardly see it. In fact, Sheila wanted to have a look at the fisherman on the edge of the tank. (*The drawings*) were all very skilfully done by the secretaries who drew (*the figures for the technical papers)*.

MT: The fact that these people had been going on for long periods of time meant that had expertise so the scientists did haven't to be so specific, particularly with the drawings. We had a wonderful girl who did our drawings for a long time, maybe more than over ten years (Evie Litton) who knew more about how to present these things for the journals, when it had to be drawn up, than the men did. She could interpret everything that they did and that wasn't easy. We always had one person who did that, even from the early days in ORL we had that kind of person who did the drawing and some of the typing. These people stayed a long time.

SM: I remember, when I did my homework in preparation for these sessions, reading that very early in your career, you (*Calvin*) did your own glassblowing, you made your own equipment. I think that was in 19?? You did that yourself.

MT: Maybe.

SM: But you probably have a particular appreciation for the way that other people do things because you had done it yourself.

MT: I suppose; I never thought about it that way, but that's probably true. I'm tired, I'm beginning to get tired.

VM: I think that's about all I can think of. Any more things you'd like to say at this point? Need a rest?

This is the first interview in our series. When we come back next spring and summer, we'll collect some more information and perhaps we will talk again. We are signing off for this one.

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Melvin Calvin

Date of birth April 8, 1911 Birthplace St. Paul, Minnesota

Father's full name Elias Calvin

Occupation Mechanic Birthplace Kalvaria, Lithuani

Mother's full name Rose (Hervitz) Calvin

Occupation Housewife Birthplace Russia (Georgian district

Your spouse Genevieve (Jemtegaard) Calvin — deceased

Occupation Probation officer/housewife Birthplace Washougal, Washington

Your children Elin (Calvin) Sowle, Karole (Calvin) Campbell, Noel M. Calvin

Where did you grow up? St. Paul, Minnesota and Deroit, Michigan

Present community Berkeley, California (since 1937)

Education PhD (University of Minnesota, Chemistry, 1935); BS (Michigan College of Mining & Technology, 1931)

Occupation(s) University Professor of Chemistry. Formerly (1960-1980) Director, Laboratory of Chemical Biodynamics, University of California, Berkeley

Areas of expertise Physical-organic-biological chemistry

Other interests or activities

Organizations in which you are active

Chapter 2

GRANT BUCHANAN

Bath

March 27th, 1996

VM = Professor Vivian Moses; GB = Grant Buchanan; SM = Sheila Moses

VM: This is a conversation with Grant Buchanan in Bath on March 27th, 1996.

Can I start by asking you how you came ever to be in Calvin's lab., what your background was and what was the chain of events which brought you there?

GB: In early 1951 I was a research student in Cambridge, England, working on vitamin B_{12} and I was starting to write my thesis and so on and I heard from A. R. Todd, who was my supervisor, that he'd had a letter from Calvin asking if he had somebody he could recommend to go to work with him and he asked if I would like to go. So I said I would and I applied for some money (I think it was through the US Public Health Service or some such organisation) which was unsuccessful, in fact. But then Calvin that said he would organise something and Todd said that he could come up with my fare and so I was going to work with Calvin. But also in that lab was Charles Dekker who was working in the area of nucleotides.

VM: In Todd's lab.?

GB: In Todd's lab. And he had an offer of a job at the University of California in Berkeley. He was married, he had two small children and he had to get over there. He also had a car, an English Austin A40, and he aimed to drive across from New York to San Francisco.

VM: He was going to take his car from England?

GB: Yes. So to cut a very long story short, that was how I actually got to California. We dropped his wife and two children off halfway over, and Chuck and I carried on and we arrived in Berkeley in mid-October in the lab. Calvin obviously felt I ought to have arrived on the first of October and he was a bit, I felt, upset but he was very friendly. I was going to stay in International House, which I had already organised, and he came up with an instant loan, as I recall of $100, to get me through until I got everything organised.

I would like to return to Cambridge. In the work I had been carrying out I had carried out a lot of paper chromatography. In fact, in the Cambridge lab. I was the first in the organic chemistry lab. to actually carry out paper chromatography which I had learned from Sanger and from Partridge. The other aspect of that work was I had really hardly any experience of making compounds and handling crystalline compounds except as an undergraduate. So I was really highly competent in a certain area and rather incompetent in other areas. When I'd got settled into the lab. (into ORL) I was given the problem of carrying out some degradations on sedoheptulose.

VM: Can I ask a question — when you say when you got settled in, what was it like arriving, when you came in through the front door? Had you already spent the night in Berkeley? Had you gone first to International House?

GB: No, we actually drove straight on to campus and I just went into the lab.

VM: Whom did you see, do you remember?

GB: Calvin, himself, was there and there was a sort of friendly atmosphere but I honestly can't remember who else I saw at that point.

VM: Do you remember what impression that building made on you when you first saw it?

GB: Well, it was light and airy, and there was possibly slightly an element of pre-fab about it. It was all wooden but I wouldn't like to...that's the overall impression I had of it.

VM: When you arrived and you walked into the front door and you saw Calvin very quickly, did he immediately start talking about what you were going to do?

GB: (Laughter) I honestly can't remember that. We had come from the north, from Sacramento, and we'd had a very heavy rain coming over the Sierra and we had travelled a long way, as you will gather and I can recall him almost instantly writing his cheque for $100, which was very useful.

VM: And significant in those days, wasn't it?

GB: Oh, it was, yes.

VM: This was 1950, was it?

GB: No, this was October '51.

VM: You then, presumably, settled into your room at International House?

GB: Yes.

VM: And then you came back to the lab., perhaps the next day was it?

GB: I honestly can't remember that. I had a nice open bench and the recollection I have is that in Cambridge the benches were all of wooden tops. These were black, shiny, plastic ones, obviously aimed to clear up radioactive stuff and so on.

VM: When you first began to talk to people about what you might do, did the inspiration come primarily from Calvin or did he send you around to talk to everybody else?

GB: No. It actually came from him. What happened is that they were interested in finding the order of the labelling in sedoheptulose and relatively recently Richtmeyer had come up with a proper structure for the anhydride of sedoheptulose which is a highly crystalline compound. It was a question of converting sedoheptulose phosphate into the free sugar and then acid on it would form the anhydride, and carrying out some periodate cleavage on the anhydride in order to get out each radioactive carbon atom. I realised quite early on that I would have to learn quite a lot in order to achieve this. It was really not the kind of chemistry I had ever done and I would have to go through a learning process in order to do this.

VM: Presumably, when you first got there you were not very familiar with the whole photosynthesis project as an organic chemist?

GB: Oh, I was, yes. I had read into it quite a lot. As an undergraduate in Cambridge I had done Part I Biochemistry and I actually knew quite a lot on the glycolytic cycle, and so on. I was actually quite well into that. Also, in the lab. in Cambridge I was on a floor where Charles Dekker and also, importantly, Khorana and Dan Brand, and I was into the chemistry of phosphates and sugars and I was actually quite into that. In fact, when had I applied for a fellowship to go to Calvin to raise some funds I had had some ideas on how to get hold of some of the reactive compounds. At the time I went they were talking of a two-carbon fragment which was going to react with carbon dioxide to form the PGA and I had some ideas of how to get hold of the reactive fragment. I was actually reasonably into the enzymes which were involved in the various transformations.

VM: As you can remember it, in the lab. in which you worked, who were on the neighbouring benches?

GB: Opposite, through the reagents, was Al Bassham. I'm not sure who was directly behind me, I think it was empty, and on the other side was Vicky Lynch.

VM: At the time when you came there was the big white table in use already for looking at radioactive films?

GB: Yes, I think so, yes.

VM: As a social centre did people congregate around this thing, drink their coffee and discuss the latest film, as it were?

GB: Yes, that's right, I have a recollection of that.

VM: How about the seminars?

GB: Ah!

VM: Yes, that rings a bell. What was it like when you were there?

GB: These were held in Donner at eight o'clock in the morning.

VM: On Fridays, was it?

GB: I can't remember the day of the week. All I know is that the people, in giving them, were really given a rough ride by Calvin. He was highly critical and wanted to know

the exact evidence and details. He was obviously quite wide awake at that time in the morning.

VM: How much notice, at the time when you were there, did people have that they were going to give a seminar at eight o'clock in the morning?

GB: Well, I can't recall that, actually. And oddly enough I can't recall if I ever gave one or not; which seems slightly odd. I gave a seminar in chemistry in the Chemistry Colloquia on the chemistry of vitamin B_{12}. I have a recollection of that but I can't instantly remember having given a seminar in Calvin's group.

VM: In the seminar group at that time can you hazard a guess as to roughly how many people participated?

GB: Well, 12-15, but I haven't an impression of a crowded room or anything of the sort.

VM: How did people sit, do you remember?

GB: No, no.

VM: Not the individuals but generally speaking, what was the layout—did you all sit around a table or in a circle — what sort of set-up was it?

GB: It was a flat room, and the recollection, and I haven't any strong recollection of this, I have to say, is of us in chairs facing forward rather than round in a circle. But I honestly can't remember.

VM: Please, carry on with what you then did when you got stuck in.

GB: In general chat around the lab. I found that there was a drawer, or drawers, containing old chromatograms and these were complete with x-ray films. And also I heard that there were some unknown spots on these chromatograms. This intrigued me. These were experiments, I have to say, carried out by other people and I should say, now, that I haven't ever carried out an experiment with radioactive carbon dioxide and a plant and a light source. I didn't ever use the lollipop set-up.

VM: But you saw it, presumably.

GB: Oh, yes. And I knew what happened. Anyway, there were these old chromatograms, and one of the unknown areas was in an area corresponding to phosphates. Also, I heard from Vicky Lynch that they had an enzyme which they used for stripping phosphates off sugars called "polidase-S".

VM: Yes, it was still in use many years later.

GB: She said that it contained invertase so that any sucrose phosphate there would have got converted into fructose and glucose. Anyway, I got hold of these old chromatograms which I assumed (I was not acting under cover or anything of the sort) were common property. Now, quite a number of them, I think, were actually done by Andy Benson or somebody, and obviously if he had been around I would have sorted it out with him. But these were in an open drawer and so, with Calvin's approval, I thought I would find out what was an unknown spot.

VM: Andy was away for the whole year that you were there, was he?

GB: I see in your list that there was somebody called Nordal. He was in Norway and Andy, I think, was spending a year with Nordal.

VM: And you were in the lab just a year, were you?

GB: Under a year.

VM: So you never saw Andy there at all?

GB: No.

VM: Have you met him since?

GB: Oh, I have, yes. So, I had a look at these unknown spots of which there were two of interest. I haven't got my lab. records here and I can't tell you the order in which it happened but the first one, I think, was a rather slow-moving area in the phosphate region. In order to find out if it had altered, if it had decomposed, I re-ran it on the usual system of two-dimensional chromatography and found it had, in part, decomposed.

Now, here is where an interesting thing happens. Around that time I got from the bookshop, I think on — what's the name of the main street in Berkeley?

VM: Shattuck Avenue? University Avenue? Telegraph Avenue?

GB: Shattuck, I think, the road to Oakland. And I got a book which was an account of a meeting held in early '51, in Baltimore I think it was. It was published by Johns Hopkins. It was called Volume I of "Phosphorus Metabolism." I have it upstairs, actually. I was reading the article by Leloir in this book and he was describing the chemistry of UDPG and he said that in the presence of ammonia it decomposed into a cyclic phosphate of glucose, and when I looked at what I had of the re-running of this unknown spot there was something carbon radioactive at a higher R_f which actually wasn't as high an R_f as a sugar and yet it was a higher R_f compared to a sugar phosphate. And I thought, no, I wonder if it is a cyclic phosphate. Again, I come to Cambridge in that I had actually isolated a phosphate component from B_{12} and I was well into R_f values and what happened when you got OHs and phosphates and influence on R_f value and I thought, now I wonder if it is the cyclic phosphate of glucose. And it was. So I went right into this area and got out from it a radioactive uridine. Oddly enough, in Cambridge I had had a spray reagent which was good for sugars, glycosides, and I was able to show that I'd got a radioactive uridine. Also, coming out of it, there was a radioactive inosine in that the polidase also has a deaminase in it — and all and all this was obviously a rather interesting area. From the sugars in the area after complete hydrolysis there was glucose, galactose, mannose and xylose. Obviously I was interested in the glucose/galactose thing in connection with Leloir's work but in the case of mannose it ought to have had a guanosine component in it which I actually didn't nail down. But it was very interesting in the light of Hassid's later work to have got xylose from there. So, this was, as far as I was concerned, far more interesting than trying to find the labelling in sedoheptulose.

VM: Can I interrupt you at that point to find out with whom you collaborated, if anybody, both inside the Calvin lab. and outside. You knew Hassid, presumably?

GB: No.

VM: Did you not meet him while you were there?

GB: Well, I saw him at seminars and things. But if I can continue on the thread of this: in order to sort out the structures, I was in correspondence with Leloir and he sent me a sample of UDPG. Unfortunately, while I had this correspondence with him, in Edinburgh I moved labs. in the early '70s and I had some very interesting letters with him and with Khorana and all sorts of interesting stuff and they are all lost. They got lost in a move and I've tried to find them, I just cannot.

VM: Leloir was in Argentina at that time, was he not?

GB: That's right. Anyway, the point was that he and his collaborators had discovered UDPG. That was his compound and so on. He actually found it because he was looking at a yeast which grew on galactose. So he found the co-enzyme for the interconversion of galactose and glucose, and this was a rather special yeast at that time. We had found it in an alga(e) and obviously, to me, and to Calvin when I told him, it had some rather more widespread use. I said that compounds of the kind of UDPG and also guanosine diphosphate mannose were concerned in the sugars themselves undergoing changes and then transfer in an active form into polysaccharides.

VM: This was the point that Harry Beavers liked a lot, didn't he, in his comments in the annual reviews where he compliments you on the imagination...

GB: No, this is Axelrod...

VM: Axelrod and Beavers. And there is this comment in that paper on the "imaginative use..." He puts it something like that.

GB: Actually, there is something by Leloir himself. Here is an account of a meeting held in Argentina on the biochemistry of the glycolytic linkage.

VM: That's 1972, is it?

GB: It was actually '71, published in '72. This was held, I think, in connection with Leloir's Nobel Prize. He has a preface to it and in it he says that the first experimental information on the donor role of the nucleotide sugars came from the work of Dutton and Storey who, in 1953, reported that the glucuronide donor was UDP-glucuronic acid. As an aside, I would say that I was actually in London and I was working in London at the Lister Institute and working with Jim Baddiley, and he and I in '53 had the idea of this glucuronic compound and we called round at the Chester Beatty Institute and tried to sell it to them as an idea. Dutton and Storey's paper came out shortly after that. So, anyway, to return to Leloir's preface: it was also suggested in the Calvin group that UDPG served as a glucose donor for sucrose phosphate at synthesis and he carries on and Cannon and co-workers also mention that "compounds of the UDPG type could be concerned in the transformation of sugars and their subsequent incorporation into polysaccharides." So he realised it at an early date.

VM: In the lab, back in the early '50s, how much were you talking to the other people about what you were doing, other people in the lab.? Calvin and the others?

GB: Well, a bit. Apart from putting chromatograms onto x-ray films I was really not learning any new techniques. I was actually using what I knew to solve problems. Consequently, I happened to be asking: "how do your carry out so and so and so and so". I was really getting on with it. I wouldn't say I was anti-social but I worked flexitime. I wasn't in early in the morning apart from eight o'clock seminars. I was in late at night and I was quite friendly with the night watchman.

VM: Were you the only one in late at night or were there others?

GB: I was largely on my own. But I wasn't, I think, anti-social. It was that I rather liked the peace and quiet at night.

VM: I am sure you weren't anti-social. When it came to things like having lunch, what did you do for lunch? Did you bring sandwiches...?

GB: No, no. It varied. Early on I went to the Faculty Club on campus. They had some rather nice lunches there.

VM: As part of a crowd — or by yourself?

GB: Do you know it is terrible, I should remember these things. But early on I went over there and I met Rapoport and Cason but...what did I do for lunch...? I think I had it in I-House, actually. It was quite a short stroll up to International House. I am certainly not a sandwich person.

VM: I wonder what sort of social life you do remember about the place? Did they have parties? Did you go out on weekend trips with people?

GB: No. I hadn't a car. The social life I had was largely in I-House. We used to go across to San Francisco in the evening. Also they had an annual I-House festival of dance and I was keen on Scottish country dancing. In fact, I actually learned how to do it properly. There was a Yorkshireman who was called John Bull, of all names (he worked with H.A. Barker when he wasn't repairing old cars), and I actually learned how to do it from him and actually some English country dancing as well for the I-House festival. But I didn't go off skiing or into the mountains.

VM: Not at all?

GB: No. Through friends in I-House I had an Easter trip to Yosemite, which was very enjoyable, and Christmas '51 I was in Los Angeles. I had a relation who I was going to see and unfortunately she was unwell and I couldn't actually see her. So far as socialising in the lab. was concerned, I recall going to Calvin's house on an occasion, an evening or whatever, but I wasn't heavily socialising in the lab.

Something I recall in the lab. as a focus of attention was the coke machine in that we had quite regularly an afternoon coke which cost 10 cents at that time — I don't know what it is now — and of course it was all in bottles. On the underside of it. it had the name of a particular plant at which the bottling was done and there was a game which I can't instantly recall: you paid an extra 10 cents into a kitty, as I recall, and the person who had the place which was furthest away got the kitty.

VM: Yes, there were a variety of names at the bottom of coke bottles.

GB: Yes, that's right. The other recollection I have of the coke machine was a scare one day where there was some escape of radioactivity. There was a lab. quite close to ORL which had things going on and somebody had come through from it and there were radioactive prints all up to the coke machine and then away from it again. And they had to find out what had happened and so on.

VM: The lab. bench you had, was it one of those with a writing desk at the end of it?

GB: Yes.

VM: So you didn't have an office somewhere else.

GB: No, I hadn't an office.

VM: Did you have people visiting you at your lab. bench and sitting down at your desk and peering at the results and things of this sort?

GB: Only Calvin and he wasn't in every day but he was interested.

Running in parallel with this unknown area which I mentioned, and this was really working on a hunch, and having heard that polidase-S has an invertase in it, I cut out the hexose phosphate area and had a closer look at that. There was a solvent which I had encountered in Cambridge. The early papers on paper chromatography of phosphates came from C.S. Hanes and Isherwood who were in the Department of Botany in Cambridge and they published, in 1949-50, around then, a paper in *Nature* on paper chromatography of phosphates. They had a solvent for the phosphates which included picric acid in it, of all things, and I tried this solvent on this hexose phosphate area. But this solvent had been slightly altered by somebody else to contain half the quantity of picric acid and I got a new band which I cut out and freed from picric acid, treated it with the phosphatase, a real phosphatase, and got sucrose from it.

VM: Yes, you report that and perhaps it was in this *Phosphorus Symposium*.

GB: Yes, it is actually in *Path XIX*.

VM: Yes, that's right, that is the one I was thinking of. There was in one of these other parts that I noticed..., the one with the kinetics in that you did (*Path XVII*), where you commented in your letter to me that Calvin had... Were you party to these discussions about the kinetics and the early attempts at formulating the cycle?

GB: I wasn't, no. In fact, what I remember was that Calvin and Al (*Bassham*) — not, let me say Calvin himself, was very much into kinetics and pool sizes and such ideas and he felt if he could feed in the numbers he could come out with what actually happened. And I am quite sure if he had a computer he would have done just that. These schemes with how the different carbons get labelling and so on and so forth, I was actually not involved in. In fact, this, I think, was what Alex Wilson and also, I think, Peter Massini, were into — carrying out actual photosynthetic experiments and the rates of incorporation of radioactivity into the various compounds.

VM: So in a sense this paper was really written in two bits: you did the sucrose one, the sugar phosphate bits, and other people did the kinetics.

GB: That's right, I would think by Calvin, himself. I honestly don't recall exactly. All I know is...[*looking at the papers to refresh memory*]...that I wrote up to about...

VM: Most of it, by the looks.

GB: Well, it was. I mean, the only way kinetics came into it so far as I was concerned was Calvin pointed out, with hindsight, that the getting of the right degree of label into the sucrose phosphate actually agreed with it coming ahead of sucrose. But I am really inherently an organic chemist and I was interested in compounds and how the compounds got there and I was actually not part of that ...

VM: ..."that" is the early formulation of the cycle?

GB: One of them, yes.

VM: Just so I don't get confused, that is *Path XVII.*

GB: Yes. If I could return to the sucrose phosphate. It had an unhappy ending so far as I was concerned. I returned from an Easter holiday in '52 in Yosemite and found a letter from the Draft Board saying that I had to report and have a physical examination.

VM: What was your status with the draft?

GB: Well, I was on an Immigration Visa through bad advice and for no other reason. When I'd been contemplating going to the States, when I was in Cambridge, I hadn't got the Fellowship I had applied for and Calvin had offered something and I hadn't any idea how much it would be. I thought I might have to take some teaching when I was there in order to make ends meet. A friend, I won't say who it was, who had spent some time at Sloan-Kettering in New York and was recently back from there said, "If you want to do that you have to have an Immigration Visa."

VM: You would have been in your mid-20's or so at that time?

GB: Yes, I was born in 1926. So as far as I was concerned, it was only a kind of visa, without realising any of its implications at all. So, anyway, I had a trip to the US Embassy in London and a medical and fingerprints and heaven only knows what, and I got this visa. I arrived in New York and as I went in I was told that I had to register at the Draft Board within 6 months, which was the first slightly ominous thing to hear, which I did on the stroke of 6 months. Shortly after Easter '52, I was informed. So I had this medical and an intelligence test and I was graded A-1 and was going to be called up within 3 weeks.

VM: Within 3 weeks!

GB: Yes. The Korean war was on. But it was a rather shattering blow. I was still, of course, liable for a call-up in the UK. I wasn't running away from anything. So, at this rather fraught time Calvin was very helpful. Actually I recall also going to the British Consulate in San Francisco who were very unhelpful and who felt it was all my fault and they were very unhelpful. Calvin wrote a letter to the Draft Board giving all the circumstances and as a consequence I was given leave to go on holiday to England (*looking at the* letter: I see the Calvin letter is the 7th of May 1952). I left the lab. in June with a couple of English friends and we drove eastwards — we drove south and then we went up into Canada and then we ended up in New York and I sailed home,

having sorted out the income tax, which is the final thing you have to do when leaving the States, on the Queen Mary early in August '52.

VM: When you say the Draft Board gave you permission to go on holiday in England: was it a qualified permission on the condition you came back?

GB: No, it was unqualified. I got the impression they had relented and this was as far as they could actually have it in print. I returned to the States for some job interviews and things in 1959 and I was slightly apprehensive that I was on some black list when I was going in but I haven't had any further problems so far as that is concerned. What annoyed me slightly was that some time afterwards I was in Cambridge and I saw Todd and he obviously knew about it and he thought I had been silly or messed it up —anyway, he was slightly grumpy about the whole episode. At any rate, that was what happened.

As it affected sucrose phosphate, as you can realise, I was sort of under some pressure at that time. The problem was, of course, that sucrose has got eight hydroxyl groups and the question is which one has the phosphate on it. This would only come from an acid hydrolysis of the phosphate to see which half the phosphate stayed with. And I carried out the acid hydrolysis and had in it a carrier of fructose-6-phosphate. I think I already had evidence that it was on the fructose end and I wanted to clinch that it was on the fructose-*6*-phosphate. So I ran the chromatogram and left it on x-ray film and off I went (home). As you recall, it requires a few weeks in order to get an x-ray spot so Al actually did the spraying after 3 weeks or however long it was. It looked as if it wasn't the fructose-6-phosphate. It wasn't a terribly clean picture but it really looked as if it wasn't the 6-phosphate so I thought, well, perhaps it was the 1-phosphate, which was the other known one. When I wrote it up after I got home I opted for that but it was a rather messy picture and I think, if I had actually not had to run away and scamper home, I would have got something slightly more clear. Anyway, it turns out, of course, that it *is* the 6-phosphate of fructose and later on I actually synthesised that compound when I was in Newcastle and it is actually in the Sigma catalogue from our route.

VM: And that is this paper here in *Carbohydrate Research* '72.

GB: That's right. But what I feel, well: this *Path XVII* came out as Volume 2 of the *Phosphorus Metabolism* which again took place, I think, in Baltimore, or wherever it was, through the Johns Hopkins Press. Now, Calvin went there and he would actually have been putting forward our paper. I have no idea what he actually said at that meeting, I really haven't. Here I would like to say of this whole work I carried out in Berkeley: I recall in the lab. one day a chap, Russ Bean [??] from Hassid's group coming in, and he had one of the Calvin-type chromatograms and he was having a look at the biosynthesis of a compound called fluoridicide, which is a galactoside of glycerol. It is from a seaweed and his chromatogram obviously had a whole heap of salt in it which was altering the R_fs in the phosphate region. And I said to him: "If you look in there you will find fluoridine diphosphate galactose which is going to end up as fluoridicide."

Now, I think, after I left, Calvin was inherently not interested in this thing in that nobody took it up after I left. Andy had returned; I had an impression from Alice (*Holtham*) who I saws in London, that Andy had been slightly put out that I had done all this. I'm really not sure. But the upshot is that neither Andy nor anybody else actually followed up this work either on the sucrose phosphate or the nucleotides, and

the people who really got into it was Hassid's group. And they came into it as a direct consequence of me, I would say.

VM: I guess Hassid's group was actually a dedicated carbohydrate group, wasn't it?

GB: Yes. But you see also, Hassid had got off on a wrong track on the biosynthesis of sucrose. He reckoned it came from glucose-1-phosphate. He had an active group. There are several names, Neifeld {??} and a whole crowd of them, some of whom are at NIH now, and I think, as I saw it, they found a rich source of these compounds from mung bean and they really did it properly. Now, as I left them, I do think that Calvin, if he had felt like it...he had this link to Leloir (through me) and as you probably realise it is a huge area and all polysaccharides come through this route.

VM: I think it was just a little bit too early in the life of the lab. in the sense they were still very dedicated to unravelling the primary thing of photosynthesis and it wasn't until 2-3-4 years later that they really began to take a greater and greater interest in the spin-off activities.

GB: Yes. I would go along with that.

VM: (We have probably ten minutes left on this one.) When you left there and came back to Britain, briefly what happened to you in the next 40 years?

GB: Oh!

VM: Well, in a few minutes. And if you could also tell me what contact you subsequently had with people from there and, if you like, what affect it had on your life.

GB: Well. After coming home, I was in London and I had some regular correspondence with Calvin over the writing up of the papers: *Path XVII*, of course, had already happened. That was still in '52 and he was very keen to write up everything. *Paths XVIII* and *XIX* originally came out as these reports for the Atomic Energy Commission or whatever it was.

VM: They were called "UCRL Reports".

GB: That's right. And in fact, I have some of them, I hope, still in Edinburgh. So I had some active correspondence over those. Originally, *Path XIX* had got several authors on it. In particular, I felt that Vicky Lynch had been extremely helpful over the actions of polidase-S. I ended up as sole author. Either he felt it was well done or else he felt he would rather not have his name attached to it, I'm really not quite sure. Anyway, I was rather pleased about it.

VM: Well, it is a good paper. He did see the manuscript before it went out?

GB: Oh, yes.

VM: I notice you thank Al Bassham and him as well as Vicky Lynch. And then you went to Lister?

GB: Yes. I was with Jim Baddiley. I was hired to work on the synthesis of coenzyme A with Malcolm Thain. This is how I met Malcolm Thain. It was really through my connection that he and, I think, also Rod Quayle, went to Calvin. I had known Rod in Cambridge. He was doing his second Ph.D. in Cambridge.

VM: His second? Does he really have two Ph.D.s?

GB: I think he does.

VM: I'll ask him when I see him.

GB: I'm really not absolutely sure about that. He had started off with Hughes of Hughes and Ingold in North Wales and he has gradually gone from a physical organic to organic to...how he ended up.

So, I was in London for 2-1/2 years. I actually met Sheila in London — we were in the same tennis club and we met up there. Jim (*Baddiley*) got the Chair in Newcastle and I went up with him and worked on nucleotides with him and we sorted out the structures of cytidine diphosphate, ribitol and glycerol and got into dichoic acids and antigens and that kind of chemistry, and I also got into some sugar chemistry. I got a Readership in 1965...

VM: In Newcastle, was that?

GB: In Newcastle, and went to Heriot Watt as Professor of Organic Chemistry in '69 and was eventually Head of Department in Chemistry in the last four years I was there. I was largely involved in synthetic organic chemistry of a class of compounds, C-nucleosides, and I also worked on their biosynthesis.

VM: What are "C-nucleosides"?

GB: In which the ribose is linked to carbon rather than to nitrogen. These are actually naturally-occurring compounds. Some of them are antibiotics and antiviral agents.

VM: Nothing to do with nucleic acid?

GB: Well, in a way, they have that kind of look about them. But also I got into some carbon NMR in relation to biosynthesis in that, of course, you don't have to have it radioactive now which is rather easier in chemical time, anyway.

VM: (We've got probably just a few minutes left.) Do you feel you can look back and say what this nine months actually did for you?

GB: Well, yes. It was a very enjoyable experience. Calvin, himself, was an extraordinary man. As I said in the write-up I gave to Calvin's volume, I appreciate it that he gave me my own head and didn't say I ought to have been continuing to carve out sedoheptulose. I always have liked solving unknowns and problems and things rather than hacking my way through something.

VM: So you had plenty of opportunity of doing that.

GB: And I had that, yes.

VM: And what sort of contact have you kept with the people.

GB: Well, I saw Andy — he was in the UK in the '60's and he came to see us in Newcastle and he actually stayed with us in Newcastle. I felt if I had seen him at the time I was in Berkeley I would have enjoyed it.

VM Have you seen Calvin at all, ever?

GB: No, I haven't.

VM: Why not?

GB: That's wrong. He was in England for the Centenary Lecture of the Chemical Society in 1956 and he came to Newcastle and he gave his lecture, actually, at Newcastle.

VM: Yes, I remember that.

GB: And I saw him then.

VM: But that's been a long time, that's 40 years ago

GB: Yes. I wrote to him. He was in Oxford in the '60s. I wrote to him and asked him if he could come up to Newcastle and he wrote and said he couldn't, unfortunately. But at the time we were in Berkeley in '84 Al, I think, was on holiday and I didn't see him. I would have liked to have seen him. But I haven't seen Calvin himself since '56.

But turning again to this phosphate thing, as I pointed out in the letter I wrote you, even in his Nobel lecture he had the wrong structure for sucrose phosphate and it was quite clear it was wrong, you know.

VM: That Nobel lecture was put together very fast, actually, and one of the main difficulties was that apparently protocol demanded that it absolutely must not over-run time and Calvin was awful when it came to over-running. I was one of the ones who helped organise him and make sure he didn't and his wife was sitting up in the projection box almost conducting what he was saying. But anyhow, there really wasn't a lot of time, I think, the time he would have liked for his lecture.

I think we have just about got to the end so I would very much like to thank you for all your reminiscences. One thing I forgot to do is to bring a camera so either I'll come back and take a picture of you some time or if you happen to have one or can find one...

GB: Actually, I have had a recent ...

VM: I think we might as well turn it off now.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name JOHN GRANT BUCHANAN

Date of birth 26 SEPTEMBER 1926 Birthplace DUMBARTON, SCOTLAND

Father's full name ROBERT DOWNIE BUCHANAN

Occupation GLUE MANUFACTURER Birthplace DALREOCH, DUMBARTON

Mother's full name MARY (MOLLY) HOBSON WILSON

Occupation HOUSEWIFE, LATER GLUE MANUFACTURER Birthplace BUCKTON VALE (LANCASHIR

Your spouse SHEILA ELENA LUGG

Occupation HOUSEWIFE Birthplace LONDON

Your children ANDREW GRANT (1959) JOHN ROBERT (1962)
NEIL DAVID (1965)

Where did you grow up? DUMBARTON

Present community BATH

Education DUMBARTON ACADEMY (1932-36) GLASGOW ACADEMY (1936-44)
CAMBRIDGE UNIVERSITY (1944-47 and 47-51)

Occupation(s) PROFESSOR OF ORGANIC CHEMISTRY
(Heriot-Watt University, Emeritus 1991) Professorial Fellow BATH UNIVERSI (1991.—

Areas of expertise ORGANIC CHEMISTRY, particularly
CARBOHYDRATE CHEMISTRY.

Other interests or activities Golf, gardening, listening to music.

Organizations in which you are active None at present.

Chapter 3

JOHN RODNEY (ROD) QUAYLE

Compton Dando, England

May 11th, 1996

VM = Vivian Moses; RQ = J. Rodney Quayle; SM = Sheila Moses

VM: This is a conversation between Rod Quayle and Vivian Moses on the 11th of May, 1996, at Rod's home in Compton Dando in Bristol.

I wonder whether we can start by how you ever came to be in Calvin's lab. in the first place?

RQ: I graduated in chemistry in Bangor and I did my Ph.D. in physical organic chemistry under E.D. Hughes. Although at the time my interest I felt, really, was more in biochemistry but at that time to get a biochemistry degree was really a rather difficult thing to do because there were only two or three universities that gave one. So I did my Ph.D. degree on physical organic chemistry with E.D. Hughes and then thought I should go to Cambridge to work in Todd's lab. because here was an organic chemist who was actually working on the chemistry of biological systems and I thought this would be a nice place to go to and I got a University of Wales Fellowship to go there. So I worked for four years in Cambridge under Alexander Todd and the problem there was the structure of some blood pigments in aphids. It was one of Todd's sidelines. His main line was nucleic acids but he did have several sidelines of which these insect blood pigments were one of them.

So I joined a research group of about three of us who then worked on the chemistry of these blood pigments: they were hydroxy quininoid compounds. It still was basically chemistry and I still felt that what I really wanted to do was to actually see or be able to work...how biological systems actually worked rather than determining structure or synthesising compounds. And I suppose that at that time one got to know, to hear about the work of Calvin and this interdisciplinary group that he had in Berkeley that was working on how photosynthesis worked. And although we knew about it in conversation, I suppose it was the fact that a close colleague of mine, Grant Buchanan who was working on the next bench to me, decided to go to Berkeley to work for a year with Calvin.

VM: Had you read Calvin's papers at that point?

RQ: No.

VM: It was conversation?

RQ: It was conversation, the sort of scientific conversation you might have about what was happening where. Buchanan went to work on some sugar phosphate chemistry, mainly, and was very enthusiastic about the group that was working there and the fact that so many different kinds of scientists seemed to work together. So I thought, well, this might be the sort of place which would bring together what I felt I would like to do. I talked with Todd about it and he said well, if that is how you feel, I know Calvin quite well and I'll write to him, which he did. I had a letter back from Calvin saying he was interested in this; could I send him some details of me, which I did, and a little later Calvin wrote back saying he'd be very pleased for me to work there. He was particularly interested in the fact that I was a chemist who was an expert in natural pigments because at that time Calvin felt that the primary act of carbon fixation was somehow connected with the epoxide ring of carotenoids.

VM: This would have been in '53 or '54?

RQ: That was '53. And hence carotenoids were going to be the answer and, as I was a pigment man, this could be very appropriate. I got a Fulbright travel grant and Calvin fixed up funding through the Atomic Energy Commission and I arrived in Berkeley in September/October 1953.

VM: Were you married at the time?

RQ: I was married at the time. My wife was a teacher. She was a zoology graduate; she was teaching at the time at Ely and she came with me.

VM: How did you get there?

RQ: We sailed by the *Queen Mary*, as Fulbright scholars did at that time. There was a dock strike in New York which broke out when we were half way across and the *Queen Mary* was diverted to the only other deep-water harbour that would take the Queens, which was in Halifax in Nova Scotia. We travelled to the States by special train from Nova Scotia to New York. Then train across from New York via Chicago and the California Zephyr through to Berkeley.

VM: Did anybody meet you at the station?

RQ: No. No, we spent virtually our last money for a taxi up to the flat we were told would wait for us and we arrived there and I turned up at the lab. the next morning.

VM: Where was the flat?

RQ: Bonita Avenue.

VM: What number? We lived on Bonita Avenue.

RQ: Well now that I would have to give you but I couldn't give it to you offhand. Yes, Bonita Avenue, strangely enough. And we changed our flats three times during the two years that we were there.

VM: So you arrived at the lab. on Day 1 of your adventure. What was it like?

RQ: Well, I walked up there, directed to the Old Radiation Lab., and I walked in through the door. I was worrying — I can remember this very well because we hadn't been married that long, my wife and I — and one of the few new clothes that had been bought for the wedding was a rather expensive sports coat in a particular kind of thorn-proof tweed that I'd had from Cambridge, from Buttress's in Cambridge. This was my best jacket. I walked in through the door and I was greeted by the sight of several people in the main lab. and Calvin, as I found out, was standing on the table in the middle with a camera on a tripod; Paul Hayes had got a molecular model on the table and Calvin, standing on the table, was levelling up a camera to take a photograph — it was thioctic acid he was photographing — and as I came in the door, I met Alice Holtham, the secretary, was near the door, and I introduced myself so she walked into the lab. to Calvin and said: "Here is Dr. Quayle from Cambridge." Calvin stopped photographing, looked at me, and shouted, "Hey, he's got a jacket just like mine!" (*Laughter*) My first impression, as you can imagine, coming from the austere hierarchy of Professor Todd's lab. into this atmosphere where the Professor was standing on a table photographing and shouted at a visitor about his jacket, was something I was quite unprepared for. In Cambridge there was a chain of command; you were under day-to-day supervision by one of the younger staff who would then report to Todd, who would then come and see you once a month, and there was no way he would be standing on a table shouting at newcomers.

VM: So how did you feel? Did you feel, "My God, what have I got myself into? This is going to be good!"

RQ: No, I was just amazed and surprised as, of course, we had travelled to the new world for the first time, life was full of surprises and here was another one — a totally different kind of academic atmosphere. When he had finished his photographs he said, "Come along to my office and we'll chat about what you are going to do." I then followed him to his office in the Chemistry Department and we sat down and immediately addressed me by my first name: "What are you going to work on?" So I said, well, you did mention in your letter that I would be looking at the role of carotenoids in photosynthesis, so that's the area I thought I would be working on. He looked slightly puzzled and after a few minutes' reflection said, "Oh yes, I did mention that, yes, well you could work on that if you wanted, you know, we're still quite interested, but I don't think it is involved in the primary act; we think it's something much closer to a sugar phosphate." So he said, "If you really want to work on carotenoids you can but go and talk with Andy Benson and Al Bassham who are more concerned with the carboxylation reaction which I don't think is going by the carotenoids." And that was it. He was obviously busy.

VM: You hadn't met these guys yet?

RQ: No. And so back I went to the lab. and was surrounded by these friendly, eager people who wanted to talk and we started from there.

VM: This was all on Day 1 was it?

RQ: All on Day 1. I mean, there were some things to attend to. Andy Benson said, "Have you got your luggage?" I said "No, it's down on the station somewhere." So he said, "Well, we'll get that up," and we went in his car. It was Andy Benson and his family who were tremendously helpful. He was the sort of deputy head of the lab., and he just took us aboard, both of us; his wife had us in and they solved all kinds of practical problems of newcomers coming in from outside.

VM: As you spent time in the lab. and saw how everyone was contributing, how did you see the role of Andy versus Melvin, or complementary to Melvin?

RQ: Well, there was no doubt that Calvin was the source, the bubbling source, of ideas. They came out of him — whenever he came into the lab. he'd got some new idea which was going to be revolutionary. He would come tearing into the lab. with this new idea which you'd have to stop and listen to and he'd pull those finger joints...

VM: Yes, that's right — I'd forgotten that.

RQ: ...clickety click. If he felt you weren't quite, you know, keeping up with him he would sort of look at you and click, click, click. It was most off-putting. And then he'd bubble forth: it was this compound, that compound, and "You understand, Rod, do you, you're following me?" And then he would go away and, Andy, who would have listened to all this, said "Oh, that's his latest theory, is it? Well it's nonsense, it won't work because of this or that." So there was, between Andy and Calvin, there was a sort of tension, in a way. It probably was a creative tension, I think, but Andy could see reasons why something wouldn't work and he would know very well that in two days' time there would be another rush of ideas that would come in. Andy was a very good practical person, you know. He knew how the whole lab. worked, what you could do with radioactivity and how to cope with it. I think it was a pretty creative tension between the two.

VM: Did they get on well together on a personal level?

RQ: As far as you could tell. It was something that I felt was the working arrangement between the brilliant chap at the top and somebody who had seen it all happen before, perhaps sometimes a bit frustrated that perhaps not enough credit was coming his way, I don't know. There was a tension there

VM: They settled you in the lab. and they gave you a bench, presumably?

RQ: They gave me a bench and I was really left to talk to Al Bassham, Andy Benson, Clint Fuller, who spent considerable time talking about it, and Calvin, who popped in and out, said, "Find out about thioctic acid, very interesting, this is where the energy is really coming — from the cleavage and shutting of this ring. While you're sort of talking about this carboxylation reaction, have a word with Paul Hayes, who is doing some spectroscopic work."

VM: He was doing experimental work at that time?

RQ: He was doing directed spectroscopic work. Calvin said, "We want some more model compounds to study the spectra of. Maybe you could make some thiozolidenes, that's what we need. Have a word with Paul; he knows what structures we want." So for the first month, I suppose, I just did some straightforward synthetic organic chemistry making some model compounds for spectral work.

VM: This was in ORL?

RQ: This was in ORL.

VM: Can you remember, you were in the big room?

RQ: I was in the room with the big table with the white top upon which chromatograms were spread.

VM: Who were your neighbours?

RQ: On one side of the bench was Dan Bradley, with a noisy calculating machine on the bench, and on the other side was Al Bassham, through the grid on the other side was Al Bassham, so I was sandwiched between the two.

VM: Did Al at that point have his glassed-in office in the corner of one of those rooms? That must have come later.

RQ: No. Andy Benson and Clint Fuller had an office — they shared an office which was glass partitioned off from the rest.

VM: It would have been that office that Al moved into when Andy had left?

RQ: Presumably.

VM: And people congregated around this big white table, did they?

RQ: Yes, yes, it was the focal point. Everything was laid there; chromatograms were laid out there, Calvin was in and out and would pass judgement on these as he dashed in and out. Coffee was taken in the lab. The two Negro ladies, the glass washers, Altha (*Van*) and Alice (*name?*), used to make the coffee which we drank in the lab. I don't know how much radioactivity was taken in at the same time. The precautions were, by today's standards, minimal.

VM: So what specific project did you actually start working on when you got organised?

RQ: Well when I finally got organised it was obviously clear from the *Path of Carbon XXI*, it was quite clear from those beautiful, bouncing pools in the experiments that were done, which I think one of the great classics in dynamic biochemistry was the way they studied the pool sizes on changing the conditions with switching the light off or replacing CO_2 with nitrogen. I thought that was just an amazing piece of work. And it was quite clear from that that there was some compound in the diphosphate area that was probably the acceptor. I mean everything pointed to it. When you suddenly cut the CO_2 off, a compound suddenly increased in size judging by the radioactive area and it was in the diphosphate area and logic told you that that must be the acceptor or close to it. And so talking with the others it seemed that what we ought to do was to repeat the experiment in a lollipop-type apparatus but with no tracer, flush the system at steady state with nitrogen so that you bumped up the size of the pool, kill it with alcohol, run it as a stripe on chromatographs with markers of a radioactive diphosphate area at either side, cut a strip across and elute it and whatever was in there should be the acceptor which, if you then did an incubation with the eluate and $^{14}CO_2$, you ought to get fixation. And that's what we did and it worked. Then the next part was to confirm, well, what is it in the diphosphate area that is doing the fixation? If it's a diphosphate we ought to be able to separate it and at that time you separated it by ion exchange chromatography. So the next thing was to set up a separation procedure on ion exchange columns and then make an enzyme extract to do a long-term incubation and then separate out the compounds on the ion exchange resin.

At that time in order to make a diphosphate from a monophosphate, if you wanted to use a biological system for doing it, then you would purify or semi-purify a kinase system. And I remember we thought we would use a crudish extract, get a kinase activity there and the kinase extract will probably give us a mixture of, you know, hexose diphosphates, there will be di- and tri-pyridine nucleotide phosphate, and hopefully the acceptor, and we know how to separate those but we need to find out how to follow the kinase activity. And the way that we biochemists did it at that time was to work in a Warburg apparatus with bicarbonate buffer and as the kinase activity proceeded you got mole for mole acid equivalent in a bicarbonate buffer and you measured the CO_2. Well I had never seen a Warburg apparatus. And when I reported to Calvin that the biochemists seem to run these kinase experiments manometrically with a Warburg he said, "OK, that's what we'll do then." I said, "Well, I have never handled a Warburg, I don't what they're..." He said, "Well learn! We're here to learn!" And so I did.

VM: Was there one in the lab.?

RQ: No, there wasn't one in the lab. — I had to talk to the biochemists, and I knew some of them anyway, and we got together a manometric apparatus and with trial and error we learned how to do it. We set it up in a little dark room on the ground floor as the only place there was room to put another shaking incubator in and it was a room in which Dan Bradley had got a stroboscope attached underneath a water bath and he was doing stroboscopic work on some part of the physical chemistry. It was the weirdest place to work because every now and again he would turn the lights off and switch his stroboscope on and jump up and down to coincide with the frequency of his flashes.

VM: He jumped up and down?

RQ: Yes, he would jump up and down so that, to your eye when he'd got it right, he appeared to float in the air. He synchronised his jumping to the flashing and you have this weird figure of Bradley 3 ft. off the floor.

VM: You only saw him, of course, when the light was on and when it was off...

RQ: ...he bumped on the floor. It was pitch dark and he had a wonderful illusion going which provided a lot of fun in the lab.

VM: Yes, people used to watch this, did they?

RQ: Fortunately, the approach for this carboxylation reaction was a good one and I think we had Andy Benson, Clint Fuller, myself and Melvin on the publication. It was really one of those discoveries that came simultaneously with virtually the same thing from another lab. and if you look in the publications in 1954 in the *Communications* to the *Journal of the American Chemical Society* you will find that the note we published was followed by one from Horecker's lab. with Weisbach, Horecker and Smyrniotis, in which they used a spinach preparation but they worked from ribose-5-phosphate and ATP. We were just that bit further along with having got hold of ribulose diphosphate, whereas Horecker was presuming that was what he was making.

VM: He hadn't laid his hands on that yet?

RQ: He hadn't laid his hands on the sort of Holy Grail compound which was RuDP (= *ribulose diphosphate*), so we were just that little bit ahead but in all fairness the two labs. were side by side on that.

VM: Did you know about Horecker's work and how close they were?

RQ: Strange to say, Horecker's note was sent to Calvin for refereeing and it came into the lab. at the time that we were publishing and Calvin came into the lab. with this manuscript, looking concerned. But yes, we knew about it in that it did come in for refereeing and the two papers were published together.

VM: But you'd essentially done your work by that time.

RQ: We had done it and we were home and dry on that. Then after that came the whole business of isolating enough RuDP chemically to actually do some proper chemistry and biochemistry. It is one thing just eluting something off a paper chromatogram in microgram amounts and another thing to get it in substrate chemical quantities. We then moved on to large-scale incubations with partially purified extracts and kinase to make RuDP enzymatically, purify on ion exchange, precipitate as a barium salt and then do some chemistry and biochemistry.

VM: And you got sizeable amounts did you, workable amounts?

RQ: Oh yes, we were working with something like 150 mg. of barium salt so we were working on enough to determine the liability of the phosphate groups; we could identify unequivocally what sugar it was, we could identify unequivocally where the phosphates were and towards the end of my 2 years we were trying to find any spectroscopic evidence for the form in which the sugar phosphate worked — the most likely form seemed to be ene-diol, and we were trying very hard to see if we could get any spectroscopic evidence for a proportion of ene-diol in solution. But I left at the time where we really hadn't got any strong evidence.

VM: I remember two or three years later people were going great guns — Bob Rabin, in particular, was looking at possible enzymatic reactions for carboxylation. Were you already thinking of these at the time, when you were working on the ene-diol possibility?

RQ: Oh yes, certainly when we looked at ribulose diphosphate as a substrate we drew out on the blackboard the different chemical forms that it might take in solution and wondered whether a cyclic phosphate would be there. We wondered whether the carbonyl group would migrate to another part. We wondered about ene-diol. We wondered about acetal configurations. But the one we kept coming back to was the carboxylation of an ene-diol form a bit like PEP *(= phosphoenolpyruvate)* carboxylate as the model. That was the one we felt... The weakness of ribulose diphosphate was, to us, that due to the fact that it was a pentose-1,5-diphosphate it couldn't have a ring. Hence, it was a sugar phosphate with a free carbonyl, no possibility of furanose or pyranose rings, and hence you could do things with it that you couldn't do with a hexose.

VM: As it turned out, presumably what you learned there about the carbon cycle and radio tracer work was the springboard from which you developed all your C_1 stuff later on.

RQ: Yes. I arrived at Berkeley really as an organic chemist. I could not have written down the TCA cycle; I couldn't even have written down the glycolytic pathway. My

ignorance was as profound as that. All I knew was that I really was fascinated by how a cell works its chemistry — that was what really interested me. So when I arrived I started to read the biochemistry textbooks and I worked my way through Needham's *Dynamic Biochemistry*, Baldwin's *Dynamic Biochemistry*, I worked my way round the TCA cycle, up and down the glycolytic cycle, pentose phosphate cycle — they were all absolutely new to me. And, of course, they were the *lingua franca* of the lab. Everybody spoke in terms of these cycles and sequences; so with a few months of that I felt I'd got hold of the main parts of central intermediate metabolism and certainly learned what makes a cycle and what you have to do to establish it. And so by the time I finished there in two years I considered myself almost a biochemist then.

VM: When you went back to Oxford, just to look forward because it's a suitable time to do so, I remember, but I may not remember this correctly, but did you and Hans (*Kornberg*) agree that you would do the C_1 and he would do the C_2? (*This is an error: Kornberg did the C_2, not the C_3.*)

RQ: At Oxford, yes. When Hans and I — he came to the lab. as a Commonwealth Fund Fellow in the summer of 1954.

VM: Is that when you first met him?

RQ: That is when I first met him, and that, in itself, was quite interesting because I was working in the lab. one summer afternoon. Calvin rang up from his office and said, "Rod, will you come down to my office; I've got Kornberg here. Kornberg is coming to work in the lab. for two or three months and I'd like him to work with you. So can you come down?" Now, to me, Kornberg meant Arthur Kornberg. I was amazed and horrified at the thought that Arthur Kornberg...; well I thought it was weird that we hadn't heard that Arthur Kornberg was coming here for a visit. And what was even more weird is that Calvin should think he should work with me when I was only just learning some of this intermediate metabolism. Anyway, with some trepidation I went down to him and there was the young Hans Kornberg, whom I hadn't heard of and whom I had never met because he'd come from Sheffield working on urease in cats and had been for a year as a Commonwealth Fund Fellow with Racker and hence was working in pentose phosphates, although I didn't know that. And so Hans and I collaborated in which we did a lot of inhibitor work on the photosynthesis enzymes, you know, what inhibitors worked and what were the consequences in cell-free systems, and so on.

So that was the start of an association with Hans and it was renewed when we met again in England. I was working in the Colonial Products Laboratory, which is the job I went back to from Berkeley. He was in the MRC unit with Krebs. And as a result of that meeting I was brought into contact with Krebs who ultimately offered a post in his MRC unit, which I joined. At that time, of course, Hans was working on the biosynthesis of cell constituents from C_2 compounds and I helped him for about the first year doing some isotopic degradations that needed doing. In the meantime, between us, we sort of carved up — he would concentrate on biosynthesis from C_2s and I would go down to reduced C_1s, really.

VM: And that actually carried you through much of your experimental work?

RQ: And that actually opened up into something that kept me busy for a very long time.

VM: But to come back to Berkeley and the early '50s when you were there — the seminars, the Friday seminars?

RQ: Yes, they were, to an English person coming in from the formal atmosphere of seminars in Cambridge where people did have to give seminars but they were well prepared, delivered at tea time — the Calvin seminars were terrifying. Eight o'clock in the morning with, at that time, no warning. It was Calvin's thesis that anybody should be able to tell you what they're doing. You know, if you are working at the bench and if somebody asks you what you are doing, you should be able to tell them! So it was nothing more or less than Calvin coming in at eight in the morning, and he'd look round and he would say, "(so and so) I haven't heard from you recently, tell us what you're up to". And it was as unprepared as that. Nobody knew when they were going to be dropped on. Once you started with a seminar, Calvin could tear you to pieces. He got lost in the science, totally divorced from any personal feelings, and he would shred you. If the science was bad he would be so carried up with it that he would shred you to bits if he thought your science was sloppy. And I have seen someone — I certainly saw one (a female research student) actually reduced to tears because Calvin **would not let go** about some sloppy work until in the end she just burst out crying. And then Calvin would look very upset — what has he done...

VM: Did I do that?

RQ: ...you know, what happened? So they were terrifying and they were unpopular, not just because of the fact you had no warning but because you were continually needing to present some figures. It's one thing talking in general terms about what you are doing but in a seminar audience people want to know the evidence and you can't carry all your manometric data or whatever in your head, you need to have it with you.

VM: So did you come in with sheaves of paper?

RQ: Well, not really, because you wouldn't know what the hell to bring. And so after about a year and, I think, prompted too by the extreme distress of some of the more sensitive people, like this girl, suffered, I think people like Andy Benson managed to persuade Calvin that it would be better, and more profitable for us all, if we had a day's notice so that people could bring sense, as it were. So you would get a day's notice; the day before he would come along and say, "You, tomorrow."

VM: That second phase lasted a long time. What happened then was that people would go into total purdah for 24 hours preparing themselves for this, because it was decided at the Thursday lunches (and when I became part of the management team I was there) and we got fed up with that because it went on, because it took people out of circulation and we began to make programmes for months on end. I suppose some of the excitement then got lost as people really had prepared and prepared and prepared, but we got a more organised set of events.

RQ: Well, I was fully behind Calvin when he said "Anybody should be able to tell what you are trying to do and how far you've got." You don't need a week or two to prepare yourself to say it but you need some figures and a day is long enough. I was quite happy once the day rule had come. Eight o'clock in the morning to an English scientist was a shock because Calvin, of course, started work at about six and by eight o'clock all cylinders were firing and tuned and he'd got all his ideas, and it came as a surprise. But you got used to it and, having worked for a year later in my career in Germany, the German labs. started at eight as well and perhaps it was the English labs. which were, perhaps, too laid back in that regard.

VM: Were you there when Andy left?

RQ: No.

VM: By the time you left, which was in the summer of '55?

RQ: It was in the early summer of '55.

VM: Was there any sign of Andy leaving at that point?

RQ: We weren't surprised — no, as a matter of fact I'm wrong. I think he had gone to Penn State, now you mention it, because we did call in on him on our way home. Yes, I think he'd left, if I'm not mixing up calling in on him on another of our visits to the States. No, I think he must have left.

VM: So that you're not conscious now, at any rate, of any great upheaval at the time when he left?

RQ: Yes, as you mention it, I think I can remember a sort of tension between him and Calvin probably had surfaced to the point where...I'm sure, now I think about it.

VM: What about parties in the lab., the social side of things, what comes to mind?

RQ: Well, it was a very sociable organisation, it really was. We were a very mixed crowd; we were from all different countries; we were truly a multidisciplinary team with botanists, physicists, chemists, mathematicians — we were all there. Several of us were quite young postdocs and it was very, very sociable. People invited you to their houses for evenings, for dinners. We used to go up to places like Yosemite or down to Death Valley as a group in the lab. These would be organised at very short notice and Friday night would see two or three cars going up to Yosemite and back on Sunday night.

VM: Had you a car at the time?

RQ: We never had a car. So we were always carried about in other cars.

SM: Did you find that the social life in that group, the way that things worked, coloured the way in which you worked with your own groups later?

RQ: Yes, I think so. We certainly, I think as a result of that, those of us who had been in that group, I think we did get used to the fact that research groups needed a kind of cohesion in the form of, certainly, seminars of an informal kind.

(*Tape turned over*)

RQ: I think — it brought home to us, in way we'd never been exposed to in England — was the fact that a really dynamic kind of research environment was engendered so much by the people at the top being actually part of it on a day-to-day basis. The idea of a senior scientist doing his research through some deputies who were doing the supervision just seemed to us, after that experience, quite a strange way of doing things. Of course, organic chemists tend to have huge research groups compared to biologists (you see some research groups of ten or twelve) and biologists, by and large worked with much smaller groups because the senior people are part of the

activity and I think you learned that in Berkeley very, very quickly, to one's advantage.

VM: Have you worked in any other American labs. apart from Calvin's?

RQ: Yes, I spent a summer in Seattle as a Walker Ames Professor and the host lab. belonged to an ex-postdoc. of mine, Mary Lindstrom, and she, at that time, was an Assistant Professor. She is now a full Professor at Cal Tech. And there it was the same sort of thing. She ran a very tight research group which knew each other very well, they socialised with each other and she, as the research head of it, was there with them all the time. And I spent a summer there.

VM: I wonder whether in Britain nowadays, at least biochemistry labs. are now more like the American level you first met in Calvin's lab. than they were once upon a time.

RQ: Well, I came back to this country, and when I joined Krebs' MRC unit, as distinct from my previous time in England as an organic chemist, I was now in a biochemical atmosphere and the people that I knew and worked with and was associated with worked in small, highly active groups. I mean, Krebs was in the lab. every day. He had his own technicians. Krebs designed the experiments for them; he'd come down during the day to see how the results were coming out. It was the same sort of system. I think that there is a cultural difference between, perhaps, the English and the Americans, but more importantly, I think there was a discipline difference between the chemists and the biologists — a big cultural difference between the two whether they be Americans or English.

VM: You may not have felt this so much, coming from Todd's lab., but when I went to Calvin a year or two later, after you, one of the things which struck me, apart from all these things which you've mentioned, was the lavishness of the support. They had equipment which I had never dreamed of. Did you find this same sort of thing when you got there?

RQ: Yes. The equipment was not the limiting factor on what you were trying to do because they had it. If there was a good reason why such and such an instrument should be tried, they'd have it.

VM: And they would make it, if necessary.

RQ: If necessary they'd make it. Or they would buy something that was near to it and alter it. And I was very impressed at the way new pieces of equipment, like gas analysers, would come in in their boxes and within minutes the people like Al Bassham would have the cases off while they modified them inside to do the job they wanted. And that, to me, was amazing because to a chemist these instruments were boxes of tricks which you called skilled technicians in to modify and these people could do it themselves and expected to. No, there was never any lack of equipment at all.

I suppose I've been fortunate in that respect because when I came back to England we were part of the MRC and Krebs' unit was well supported. When there was a good case for something we got it, so I think I was lucky in that regard.

VM: So you left that group in the early summer of '55. Did you make a long trip back to England or a quick one?

RQ: We took a long trip back. We went back by train up the Oregon coast. The lab. saw us off on the station — the whole lab. turned out. They presented me with a huge Stetson hat, which I still possess, and they also made it, when the train turned in, they made it as if we were a newly-married couple. They showered us with confetti and we got aboard the train and sat up coach class up to Portland, Oregon, with some matrons looking at us, looking at me, anyway, in a very poor light as this is not a good way to be treating your young wife, sitting up on a coach class train. And we stayed with Howard Mason, the oxygenase man whom I know. We went on up to Vancouver, stayed with Khorana, whom we knew from Cambridge days, and then across Canada, ultimately to New York where we stayed with Paul Srere who was working in Racker's lab.

VM: Yes, I didn't realise that Paul had been in Racker's lab. I've known him in recent years.

RQ: So, we got to know Paul quite well.

VM: There is, I remember, a picture of five of you in deerstalker hats. Where did they come from? How did that happen?

RQ: They came from Lilywhite's in London and it was part of the fascination, I think, that some of the American's had for the English way of life. One of the peculiarities to the Americans of one part of the English way of life was the fact that some gentry wore deerstalker hats and I offered to send to Lilywhite's for a supply for those who would like to wear them. I've forgotten now how many people said they would like a deerstalker but Calvin certainly wanted to be in on it so an order was sent off for deerstalkers which arrived in Berkeley.

VM: You are on the steps: there's Calvin, you, Malcolm Thain, Clint (*Fuller*) and there was a fifth, was there? I can't remember now. (*It was Rich Norris.*)

RQ: I'd have to look the photo up; I've still got it.

VM: Where's the hat?

RQ: Oh, the hat. Well, my hat did quite good service but it did shrink in the rain, as those things tended to. And mine shrunk to a point where I couldn't wear it anymore.

SM: And you don't have it?

RQ: And I don't have it.

VM: I was hoping to take a photograph of you in that hat.

RQ: I have the Stetson that was given to me and that hasn't shrunk.

VM: I'll take a picture of that if I may.

SM: Was there any connection with Conan Doyle as far as deerstalkers?

RQ: Sherlock Holmes. Yes, they knew all about Sherlock Holmes. There were all kinds of things like English tea and marmalade and stuff.

SM: They did have a very warm feeling towards us, I remember. They really did. And I remember once being in the Co-op, which I am sure you were familiar with in Berkeley, and some ladies were looking hopefully at the fish paste in order to make sandwiches for tea for the cricket club in Berkeley. Did you have anything to do with anything like that while you were there?

RQ: No, I don't think so, no. But English people, you are quite right, I thought, were regarded with a lot of affection there and they certainly seemed to be welcome in places.

One of the overwhelming impressions on me was the fact, in the States, you came into a lab. and anything was possible. Coming from the division of academic departments in England, Departments of Organic Chemistry, Departments of Physical Chemistry, Departments of Chemistry, all very separate, and you were labelled as being one in that discipline. It was very rigid. And suddenly to come into a lab. where you were a scientist: you happened to be a chemist, but the chap next to you was a botanist and the other chap next to you was a physicist, and if you hadn't ever seen a Warburg manometer in your life before, if it was necessary to use it, you learned. Calvin, himself, would learn from the man who cleaned the lavatories. If the man who cleaned the lavatories had by any chance a good idea, Calvin would have it out from him without a trace of embarrassment. You wouldn't get a top English professor learning anything, well, from a junior research assistant for that matter, never mind the chap who cleaned the lavatories. It was: "We are all scientists! We are here to learn! If you have never heard about how to do that, learn it! That's what we're here for!" It was that, I think, great freedom on how you would tackle a problem, which in this case did demand different sciences coming together. And the fact was, of course, he could cope with it. He was such a polymath, himself, he thought everyone else would be naturally a scientist rather than one kind of scientist. And that, I think, came as a tremendous eye-opener.

VM: There is something that struck me very much when I first went there and I don't know whether it is specifically Calvin or whether this is more generally an American phenomenon, but compared with the English lab. which I left in 1956 there was much, much more to-ing and fro-ing between the individual researchers. Everybody seemed interested in what everybody else was doing and they formed collaborations all the time. Whereas in England beforehand people kept to themselves more and didn't, as it were, muscle in. Did you find that sort of thing?

RQ: Yes. That was certainly true. There was a much more dynamic kind of atmosphere.

VM: In fact, 10 years after I got there, and I think it was literally so in '66, a chap called Ian Morris, who was an algologist from University College, one of Phil Syrett's students originally, who came there as a postdoc. and he came from the same place as I had and I observed him reacting in the way I had reacted 10 years earlier. This business of sitting and talking and somebody pops in and says, "Did you know so and so and have you thought of this?" It just hadn't happened to him in England just as it hadn't happened to me. That was part of it. I don't know whether this was specifically the result of Calvin or whether Americans just tend to do that and always have more than the British have.

RQ: Well, I think they are more, you see it now (*with*) Americans: anything is possible. If you really want to do something, do it. It's all possible.

VM: Don't look for reasons why not.

RQ: Don't look for reasons why not. I remember when I turned up in Oxford and when I was working out what I might do research in, I'd might go to talk to D.D. Woods, who was the Professor of Microbiology. And DD would give you 20 reasons why you shouldn't try. He was a very good scientist and his prognostications of difficulty were well founded but he would sit there and: this mightn't work and that mightn't work and have you thought why you shouldn't do this because that will happen. You go into somebody, say, in the Berkeley set-up there and he would listen to you and say, "OK, we'll try it tomorrow at nine o'clock."

SM: It is a very different sort of ethos. It's somehow a very English thing, this derivation of strength through adversity. It is good for you to be having a hard time, in a sense, and if you come out of this with something then it is really remarkable.

RQ: Yes, I suppose. But, well, it's optimistic. It may work, so let's do it.

SM: Do you think that any of this was coloured by, the difference, was coloured by the immediate post-war feeling in England? Because it still felt very much like that in the early '50s. Whereas, coming to Berkeley, there had clearly never been one (*i.e. a war*); it was very different.

RQ: It was very different. Yes, there was a big difference in the whole atmosphere. We came from post-war England, where we were still on rationing, into this great paradise out there where food was flowing out of the shops, equipment out of the labs. and money and so on. The whole thing was, from drab post-war England with your one and sixpenny worth of meat a week, quite something.

VM: You got used to it, didn't you. It didn't take long.

RQ: Well, you had to. I mean, for all Calvin's (I think he is a wonderful character) — he did not tolerate fools gladly. He'd tear you to bits if you came up with some sloppy science; well, I don't think he would want you there for very long. He'd have you out as soon as he could. He just could not bear somebody (*not following him*)... he got very bothered if he felt people didn't know what he was talking about, you know: knuckle pulling: "Are you following me?" Click, click, click. I don't know if he carried on doing it.

VM: Yes, he did and you are right. I'd forgotten but you are right. Now that you mention it, I certainly remember. I think he's stopped now, but still...

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name JOHN RODNEY QUAYLE

Date of birth 18-11-26 Birthplace HOYLAKE, CHESHIRE, UK

Father's full name JOHN MARTIN QUAYLE

Occupation PHARMACIST Birthplace ISLE OF MAN

Mother's full name MARY DORIS QUAYLE (née THORP)

Occupation HOUSEWIFE Birthplace MANCHESTER, UK

Your spouse YVONNE MABEL

Occupation TEACHER (RETIRED) Birthplace BIRDWELL, YORKSHIRE, UK

Your children RUPERT JOHN

SUSAN CLAIRE

Where did you grow up? GILCAIN, CLWYD, NORTH WALES, UK

Present community COMPTON DANDO, BRISTOL and BATH, UK

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UNIVERSITY COLLEGE OF NORTH WALES, BANGOR. CAMBRIDGE UNIVERSITY

Occupation(s) PROFESSOR OF MICROBIOLOGY, SHEFFIELD UNIVERSITY 1965-83

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Areas of expertise CHEMICAL MICROBIOLOGY

Other interests or activities

Organizations in which you are active ROYAL SOCIETY, LONDON

SOCIETY OF GENERAL MICROBIOLOGY, UK.

Chapter 4

BRIAN ROBERT (BOB) RABIN
(with Sheila Rabin)

Potters Bar (Hertfordshire)

April 8th, 1996

VM = Vivian Moses; SM = Sheila Moses; BR = Bob Rabin; SR = Sheila Rabin

VM: ...talking to Bob Rabin in Potters Bar of April the 8th, 1996.

Bob, can we start by my asking you, how did you come to go to Calvin's lab. in the first place?

BR: I obtained a Rockefeller Fellowship and Dr. (*Gerard*) Pomerat, who was really running the Rockefeller programme from New York at the time, was, I think, a very great friend of Melvin's and I think he, above everybody, persuaded me to go to Berkeley.

VM: Did you know anything about the place before you went?

BR: I think we knew a little bit about the path of carbon in photosynthesis and had read some of the reviews of some of the work that Melvin's group had done and it all seemed very exciting, although, of course, by the time I got there the path of carbon had been virtually traced, at least they thought it had been.

VM: So, before you went, had you had any correspondence with Calvin about what you were going to do when you got there?

BR: No. Calvin passed through the lab. in London and spoke to me very briefly. It was very interesting, the conversation, but he didn't say anything at all about what he would want anybody to do. He just said that I would be welcome there.

VM: So, you arrived with a clean sheet, as it were.

BR: More or less, yes. In fact, I knew nothing at all about plants and rather little about photosynthesis.

VM: So, how did you travel?

BR: We travelled across the Atlantic by boat, on the SS United States. It was the most horrendous trip that anybody could have. We got into the end of a hurricane. The first we knew about this was when they came into the cabins and took everything that was movable out. My wife (*Sheila*) is a very poor traveller, and I really felt for her, but there was nothing I could do. When I went up to dinner, the dining room was almost empty. Sheila, of course, remained in the cabin, completely horizontal. The ship was tossing around like you had never seen. In fact, so much so that the screws would actually come out of the water and you could hear the engines speeding up. It was absolutely horrible. That's the worst experience I can remember.

VM: But eventually you got to the other side.

BR: We got to New York, and when we visited the Rockefeller to make the arrangements for onward travel, Dr. Pomerat took one look at Sheila and he said well you'd better go by air, which, of course, in those days was not very usual. So they booked us on the Red Carpet Service after spending a few days in New York at Rockefeller's expense.

VM: So you arrived where, in San Francisco?

BR: We arrived in San Francisco. We took a taxi as I remember, to a hotel in Berkeley, the Claremont, and we stayed in the Claremont one night and then we phoned the Calvins and they came and were horrified that we were in this very expensive hotel. That actually didn't worry us because Pomerat was going to pay from the Rockefeller Foundation.

VM: So, came the first morning when you went to the lab., presumably.

BR: No, it was not quite like that. Mrs. Calvin came along to the hotel and whipped us out and we stayed with the Calvins for a few days while we found a place to stay.

VM: So, you didn't go to the lab. for several days.

BR: I think I went to the lab., I don't think the first day we were there, more the second or perhaps the third day.

VM: When you finally got there, could you remember what it was like when you walked in? Who did you see? What did you talk about?

BR: One doesn't remember exactly what one talked about. I do remember it was fairly shambolic affair, a sort of wooden structure, very temporary looking. But everybody was very friendly and Calvin, I think, had to go off to Washington for a few days so it was not possible to talk to him. I think it was Dick Lemmon who was the first person I really talked to. I don't think Dick really wanted to talk about the scientific aspects or what Calvin might have wanted me to do. So, I never really had a programme. In fact, the program evolved, really, because at the time there was great excitement because it looked as if the Calvin cycle, as it was then called, might not be entirely correct. As you may remember, the methods that the early workers used for stopping the algal reactions was to drop it into hot alcohol. At the time, people had decided they would try other methods of killing the algae before they analysed the algal contents for radioactivity. They used cyanide and they got a very different pattern of carbon fixation and kinetically some of the compounds were very peculiar indeed. Everybody was very concerned about this because they wondered really whether the

Calvin cycle was correct and if there weren't some artefacts that originated from the hot alcohol treatment.

I remember talking through the data with some of the German workers who were in the lab. at the time who had done these experiments. What immediately occurred to me was that they had forgotten some of their elementary chemistry in their great hurry to produce something new. What they had forgotten was that a five-carbon sugar like ribulose diphosphate has a naked ketone group which would react very readily with cyanide and, in fact, it was not a cyclic sugar in the normal sense. I talked to one or two people, I think Ning Pon particularly, and we wondered whether half of this wasn't due to cyanide reacting with the ribulose diphosphate. So, I think it was actually before I talked to Calvin in detail we had already started off to do some experiments on this. Then Melvin Calvin organised some radioactive cyanide, and we presented this idea to him and said we thought this might be an artefact...

VM: We is you and Ning Pon?

BR: Yes, we had gone through it. In fact, Ning had gone through the literature, actually, and found some branched-chain materials like hamamelose, and so on, which other people were thinking were intermediates in the carbon fixation but could clearly originate from the attack by cyanide. What puzzled everybody at the time was that if this was so, you would expect to see an acid at some stage but prior to that you would see a cyanide derivative. Why an acid was never seen from the hydrolysis of the cyanide was very simple really. It was that it lactonised very readily.

VM: This was the famous hamamelonic acid.

BR: I don't think this was the correct description of it. Because hamamelonic acid has to be from hamamelose and this isn't hamamelose this is an adduct from cyanide which is hydrolysed. When we talked about this to, I think, Helmut Metzner and Helmut Simon they thought this couldn't be because you never saw the acid component. Ning and I argued very furiously that the acid wouldn't exist really because it would lactonise very rapidly. At any rate, it was then possible to test all this out because Melvin had organised some radioactive cyanide and we did the experiments with cyanide and got exactly the same pattern of fixation as when you used radioactive carbon dioxide. Well, it wasn't exactly the same, but it was close enough to indicate that there was a very distinct possibility that it was an artefact. So, that took really quite a lot of time, because, as you know, everything was done on chromatograms and everything took a long time to do.

VM: Can we go back a bit to the beginning. You say you really developed this project with Ning before you ever got to talk to Melvin about it in detail.

BR: Yes, I think that's true. I think Ning may recall this as well. But that's my recollection of it, that we chatted about this and it kind of emerged.

VM: So, by the time Melvin and you and he (Ning) actually got together, it was agreed that that was what you were going to do.

BR: Exactly; I think he (Calvin) was very enthusiastic about it. And, of course, he added an awful lot to it because he knew much more than we did about the chemistry of these sugars and the possibility of an attack of cyanide on the keto-sugar. Most sugars, if they are in a ring structure, are not that readily attacked by cyanide. It was just an oddity, really, and it was just unfortunate that cyanide was used.

VM: Where was your bench? You had your own bench, presumably?

BR: Yes. Ning and I were opposite each other but we used to share the facilities., We were at one end of the lab., as I remember. We used to use other people's space. When we used the radioactive cyanide we went into a different area completely because that was considered to be relatively dangerous although the amounts of cyanide used weren't that great.

VM: Had you worked in a lab. like that, that style of people interacting, before you went there?

BR: No, no.

VM: How did it strike you when you saw it, when you found yourself in it?

BR: I think it was so friendly that it would have appealed to anybody. Unless you wanted to do something totally of your own, which had nothing to do with the rest of the lab., then that might have been difficult, although I don't think actually Melvin would have objected because he got so enthusiastic about everything that was worthwhile. He was a great enthusiast and tremendous at encouraging people. I think it would have appealed to almost anybody. Of course, the lab. was full of such very good people. It was really the cream of some of the European laboratories who worked there and were working there at the time. There were always visitors coming through the lab. It was a very vibrant place. The people discussed things together and then there was the eight o'clock seminar, which didn't greatly appeal to the Englishmen, but most of them appeared, sometimes a little late, but we were always there. What impressed me was that Melvin always led the discussion and always had something to add to it. He was one of the very few scientists who could talk physical chemistry to physical chemists and biochemistry to biochemists, and I think those sorts of people are very rare indeed.

VM: So, if you worked with Ning, as I remember Ning was not an early riser, but he didn't go to bed early, either. So, did your day actually overlap much with his?

BR: In the middle of the day we worked together. The morning I set things up and it was very good because I could always leave early for Ning to complete the experiments. As you know, most experiments fail for reasons which are either obscure or due to the stupidity of the experimentalist. So, it was fine, really, and if he wanted a day off it was all right, and if I wanted a few days off, that was fine as well. And nobody interfered with us.

VM: So, essentially the whole of the experimental work that you did there you did in association with Ning.

BR: Yes.

VM: Tell me about the life in the lab. as you remember it.

BR: Well, we went up to the mountains skiing, as I remember, and it was very, very friendly. We'd never skied before but there were plenty of people to help you. I seem to remember also that we went camping for the first time that we had ever seriously gone camping, and, again, there were lots of people to lend you things and help you out, although really you didn't need any tent, you could sleep out in the open, up in

Yosemite. We spent a lot of time travelling around America. Nobody ever suggested that we should spend more time in the lab. than we did. We were never interfered with. I think people made reports and they discussed it with other people in the lab. — Melvin was always walking around, talking to people, almost every day. He would do a tour of the lab, as you remember yourself. He never ever implied or suggested that one should spend more time working. If you were going off somewhere on a trip, he just said "Hope you have a good time".

VM: Did you actually work long days there?

BR: Sometimes and sometimes we had long weekends. It was an excellent atmosphere. People worked when it was required so to do. Sometimes the experiments that Ning and I were doing were extended and sometimes he stayed and sometimes I stayed.

VM: I guess you lived close enough to come back in the evening.

BR: Oh, indeed yes. We lived in a flat in Berkeley, I remember this very well, run by a very odd Hungarian called Voitov.

VM: I remember that name (*laughter*!!)

BR: It was fine, really. I think Melvin's greatest asset was Gen because she really looked after people and made them comfortable. I think we would have had great difficulty finding a suitable place if it hadn't been for her. She drove Sheila around until they found somewhere satisfactory and I think she showed her where to go shopping, and so on. Remember we had come from a Europe which had hardly emerged from rationing, hadn't emerged from rationing actually. We were not used to seeing food of this sort whatever.

VM: This was your first trip to America?

BR: This was our first trip to America. I think she (Gen) was a tremendous person, really. She helped everybody.

VM: Apart from the very unusual character of that particular lab., coming from the English atmosphere of 1956 into America of 1956, were you struck by different styles in the way the scientific establishment worked, the way the laboratories worked?

BR: It was completely different. All the work I had done (*in England*), I had done myself, really. I don't think I ever had anybody to supervise it. One could always go and talk to people and you were always welcome in London. But it was never on any sort of formal basis. I think what Melvin did was always made his presence felt; he was always there and he was always a source of information and was always very happy to discuss problems with people. I might say he was one of the few people who always had something constructive to offer. I think what was my great impression was the sheer extent of his scientific abilities. I have never met anybody who had the same knowledge as he had of science in general, whether it was from the physical aspect or the biological. I think he was really a kind of one-off, really.

VM: Were you used to a lab. as well supplied as that one?

BR: No — no, no. In London we made our own thermostatted water bath because we had run out of money. There was nothing like that in Melvin's lab. Everything was available. If you wanted radioactive cyanide, Melvin obtained it. Radioactive C^{14}-

cyanide was a very odd material to get hold of. You or I would find great difficulty in making it ourselves and that was true of everything. There was no shortage of resources.

VM: Did you find yourself talking about all of these problems with lots of other people besides Ning with whom you were working directly?

BR: I think mainly with Ning and Melvin. We used to chat to the other people, but I think not in the same sort of way. Of course, I think it was a little bit difficult with Helmut Metzner who had done the original work with cyanide. We tried to keep him informed of exactly we had been doing. I think that clearly they had made a blunder in interpretation. I think it was a very understandable blunder and I think it was a mistake that anybody could reasonably have made. It really wasn't that predictable that a five-carbon sugar diphosphate would be attacked by cyanide as it happened. Obviously, that makes it rather difficult. But I must say that Melvin was fine, once we had shown Helmut the information he accepted it because he is a very good scientist in his own right and he just needed to be convinced by the facts.

VM: That was the only experimental problem you worked on while you were there?

BR: Yes. This took quite a long time, really. Remember that there was no experience with handling radioactive cyanide in any event, so that all had to be learned. Everything else had been done by dropping the algae into hot alcohol. I don't think anybody had used any other killing techniques prior to Helmut Metzner. I think Metzner was absolutely right that you needed to apply new killing techniques to the problem in any event. I don't think anybody would say that what was then called the "Calvin cycle" had ever really been proved in reality. I think people knew PGA (*3-phosphoglyceric acid*) was formed fairly rapidly, but nobody had any kinetic information, real kinetic information.

VM: There was a paper by, Wilson was one of the authors, in about '54 or '55 in which the cycle in the form that it was finally adopted was first put forward and there were concentration kinetics of one sort or another, turning the light on and turning the CO_2 on and off, things of that sort.

BR: I don't think anybody had ever shown that any of the intermediates were kinetically competent for the whole cyclic process. That's an enormous task and you don't wait to do that before you publish the system. I think what emerged stood up very well in real senses. The first reaction, of course, of ribulose diphosphate with CO_2 is still a very enigmatic process. You can get oxygen opposed to carbon dioxide, and so on, and there are an awful lot of complexities in the whole thing. I think that what was the great triumph that it was more or less the outline of it has survived. It's proven, it's in all the textbooks and it's generally accepted as one of the prime mechanisms, not the only one but certainly one of them.

VM: I guess your main contribution for that visit, as distinct from the later one, was to resolve the cyanide.

BR: The work I did was almost entirely that. The other thing that one learned a little bit about electron spin resonance and NMR because there were physical chemists in the lab. who you chatted to.

VM: Like who?

BR: I'm just trying to remember.

VM: There was a fellow called Power Sogo.

BR: Yes, he was there and...there was one other person. You get confused with the two visits. Remember that I was there in '63-'64 as well. Then it's hard to remember who exactly was associated with what.

VM: On your first visit you arrived in September '56 and how long did you stay?

BR: We left in July (*of 1957*).

VM: Did you motor back across America?

BR: We did indeed. We did a very extended tour, eight weeks.

VM: Then some years later you decided to come back.

BR: Yes...

VM: Tell me about that. Why did you decide, how different did you find it, what did you come back to do — things like that.

BR: Melvin invited me back because he wanted an enzymologist, although Ning was still there. Ning was a very capable enzymologist and doing all sorts of very interesting things. At any rate, Melvin invited me back and so I accepted and University College agreed that I could go for a year's leave of absence.

VM: What were you? A Lecturer at UC (*University College London*) at that point?

BR: Yes. That was a great convenience for me.

VM: Can I backtrack just a minute? Before you went (*to Berkeley*) the first time, what was your position at UC?

BR: Assistant Lecturer. I had just been appointed actually.

VM: So they gave you a leave of absence then?

BR: Yes.

VM: Back to '63. When you came back the second time, then, obviously you knew something of the set-up even though it would have changed somewhat. But did you have then a clear idea of what you were going to do?

BR: Yes. I wanted to do some enzymology and Melvin told me to go around and talk to various people in the lab. to see where was a sensible place to fit in. I mean I got on very well with Pat Trown who was very interested in how the carboxylating enzyme worked and so that was essentially what I worked on almost entirely with Pat Trown.

VM: Where were you when you arrived whenever it was in 1963. Which building was then in use, where did you go?

BR: The Round House.

VM: You went into the Round House.

BR: I thought this was the most bizarre structure that you could think of. There were all sorts of jokes around about Melvin going up and down the middle, you know, and when he was away in Washington his ghost was scurrying up and down (*the staircase*), things of that sort. There were all sorts of witty people. It actually worked extremely well because people met for coffee in the middle, you may remember, and they talked to each other. It was a strange atmosphere. What I thought, it was really a bit too open for my taste. I don't like that open sort of laboratory although many people have followed that pattern.

VM: Had you known anything about the new building before you saw it?

BR: No, no.

VM: But you knew it was there.

BR: I knew there was a new building, because we knew that Melvin had raised the money to put this together, but there were no reports of it. Nobody had come back and told us what it was like. Again, you see, it was so superbly well equipped It was extremely well managed, actually, in other ways, in terms of the availability of materials and there were always people to do the ordering; you really didn't have to do very much yourself except say what you wanted. So, Melvin was very lucky: he had some excellent back-up. I think (*Dick*) Lemmon, particularly, was mainly responsible for the organisation and he seemed to be the man who sorted out all the problems. I guess if they became very difficult they got referred to Melvin. But, again, Melvin spent a lot of time talking to people but nothing like as much as he had previously. But, then everything had gotten bigger and I think Melvin spent much more time in Washington, advising the President.

VM: By then he was on the President's Science Advisory Committee and he was going to Washington quite often. Do you think that there was a successful carrying forward of the style of working in the old building (*the Old Radiation Laboratory*) into the new one? Do you think they did a good job of designing a building, since they had the opportunity of a new one, to incorporate what everybody felt had been learned from the style of working in the old one?

BR: I don't think anybody could ever recapture the way the old building worked. There was something completely different about it.

VM: The spontaneity of having developed that way.

BR: I think it (*the new building*) was too artificial, in a way. Here was a building which was deliberately round in order to force people together. It doesn't ever work out like that.

VM: You first went into it when the building was very new: I can't remember exactly when it opened, but something around the time when you arrived, so you must have been in on the first days. Then you came back, at least for some sort of a visit, several years later, I can't remember for how long you were there, a month or two. How do you think the building had settled down in that time?

BR: I think it was a very efficient mechanism for producing scientific information. I think the old building was a source of originality. I think the new building was a source of exploitation of what was known. There's a very big difference. Why they were so different I honestly can't tell you. But I think that the first time I was there (*in 1956-7*) it was about ideas, even if the idea was to show that the cycle was really not entirely correct. The second time I was there it was much more modern science. People were thinking about electron spin resonance and computers and how to put data together and how to analyse data better. I don't think there was the same level of originality.

VM: But that would presumably have happened anyway.

BR: Probably.

VM: Not just a building-related phenomenon.

BR: I think, you see, there was not much left to do in photosynthesis in the way that it had been done previously. Melvin was much more interested at the time really in the light reaction itself and how that functioned and the physics of it. But I don't think the technology was available really to tackle that at that stage (*in 1963-4*). It has come along since, but that, I think, was the main thrust of his interest. The other people who were doing the more classical biochemistry were left to their own devices, was my impression, which was very different from the first time I was in Berkeley.

VM: There were a number of significant differences. For one thing, the group was united. Remember when you were first there, there was a group in Donner which was rather separate. Then it had become much bigger and it had branched out into a number of areas simultaneously. There was not the cohesion, I think, of a unitary group that we experienced in the '56-'57 period, in the new building.

BR: No. I think that what was missing, really, was the presence on a day-to-day basis of Melvin, in all truth. When I was first there (*in Berkeley*), the overwhelming impression I had was the dynamism of the whole outfit was Melvin Calvin. People had enormous respect for his abilities and his talents. The second time I was there (*in 1963-4, in the new building*), Melvin just wasn't there sufficiently. He had so many other things to do and, of course, it was much more diverse. There was much more physical chemistry which was a little bit different from what the reputation of the lab. really rested on.

VM: Those eight o'clock seminars, do you think they served a useful function for everybody or just for Melvin?

BR: I think they made people get up early, which I suppose, is valuable. I don't think...I don't know, really. I think you can have seminars at any time of the day, truthfully. Eight o'clock happened to suit the style of Melvin Calvin and fine, and people accepted that. But I think the most important thing was when Melvin walked around the lab. and talked to people individually because I think you learned much more — I mean, I learned an enormous amount just talking to Melvin because he had this inane *(?; innate?)* depth knowledge of physical chemistry and organic chemistry which very few biologists had. You couldn't do that in a seminar.

VM: At this point Sheila, Bob's wife, is joining in because she has some very vivid memories as well of what it was like, especially the contrast between the first and the second visits to Berkeley.

SR: For me this was quite a trip and my first experiences in Berkeley were to be taken out with Mrs. Calvin — which I called her, "Mrs. Calvin", although everybody said "Gen". I felt that this was not proper or right, being English — and going to the University CO-OP supermarket and going in to buy things and coming out with nothing. Because I had gone with a bag to pack my shopping only to find that everything was done for you (*and groceries were placed*) into paper sacks. I noticed that Mrs. Calvin was buying things like eight tins of, maybe, fruit for a dollar and I just could not come to terms with this because up until I got married meat was still on rationing in England and so the opulence of everything was just overwhelming for me.

As far as the lab. was concerned, I found this to be so friendly and everybody was on first-name terms which is so foreign at that time in England. Everything (*in England*) was formal and starchy: Professor this and Doctor that, in Bob's lab. in University College, although everybody was very friendly, you still approached them on a formal basis. Professor Baldwin and Mrs. Baldwin, they were like the king and queen of the Biochemistry Department at that time, and one was in fear of them. Going to see meet Professor Calvin and his wife, and staying with them for some time before we got our flat, it was so different. It was like being with friends and they encouraged you to use their first names but I still found this difficult.

Going into the lab., everybody was so friendly. They were so nice, they were so helpful and you just got into the swing of things. Taking us away on trips, camping, skiing, introducing us to people to buy cars, service our cars. At one point our car broke down for a major trip and the guy said "borrow my car and when you come back yours will be ready". This, to us, was so different. First of all, Bob and I didn't even have a car before we left to go to California — our real first car was the one we had in California.

VM: How did you see the social life in the lab.?

SR: It was very good. Everybody wanted to mix and we all did. It was just so nice. I shall never forget it. It was one of my very first impressions was how friendly everybody was, especially Calvin, who was very, very nice to people who had no knowledge of science at all. He would spend hours with you telling you how things worked but, he never tolerated a scientific person who didn't know. He just looked at them with disdain and scorn and would walk away; he just had no patience at all with them.

VM: What sort of difference did you spot from the time you first went in '56 and came back again in '63? Was it a different group? Clearly you must have known some of the people.

SR: Yes, it was a different group. There were not the foreign people, the Europeans there. There was definitely nowhere near that sort of type of lab. It was nice, still. Of course we had children, all of us had children at that point. We were all more constrained, although we did go away quite a lot camping with the kids and enjoyed that very much. I can remember going down to San Diego and seeing Ozzie (*Holm-Hansen*), who was at the Scripps Institution of Oceanography.

VM: "Ozzie", I should say, is Ozzie Holm-Hansen.

SM: And he is still at Scripps.

SR: There was not the sort of camaraderie we had on the first visit, of meeting all these new people. Maybe because it was so different for us then but I can't remember meeting anybody so different as we did then; we made so many new friends then.

VM: I suppose there were lots of us who were coming out of Europe at that time (1956-1957) in our various ways, being mind-blown by the first experience in America. We were all that much younger. We were all unencumbered and perhaps we mucked in and socialised more freely and more completely than we did later on.

SR: Maybe we did. But even going back in '63-'64 it was still quite different to England. They still had much more than we had and they could still afford much more than we had. But when you look now at the differences, it's not so great. In fact, shopping here is probably, in my opinion, is much better here (*in England*).

VM: You also have some memories of what you did at Christmas time out in the relatively warm climate of California?

SR: Yes, I do. I distinctly remember going to Stinson Beach, the whole lab. I think went, and we had this barbecue there. That was certainly something new for us. I don't think we had ever been on a beach barbecue in England at Christmas (at any other time). Also, remember that you and Sheila and Bob and I went to the vineyards in the Napa Valley and this was also sometime in December, and I have pictures of us sitting amongst the vines, having our picnic, which was quite different. Also, when we went to San Diego, that was around January time, and I can remember Bob going to visit some laboratory near Hollywood, or somewhere you went, and I sat on the beach with the children (*this was in 1963*), it was about 77 degrees Centigrade (*this must surely be Fahrenheit!)*, there wasn't an American in sight. There was just me and the two kids on the beach, and this other woman. She came across to me and said "Hello" and I said "hello". She said "you're from England" and I said "yes". As it turned out, she had come from Muswell Hill (*a London suburb*), which I thought was quite something. There were no Americans — -it was too cold for them.

VM: I remember the first Christmas we were there in '56. Sheila and I drove to Los Angeles to see some people and came back to Berkeley on Christmas day in our convertible, our bright red convertible, with the roof down. That was an unbelievable concept to be doing that on Christmas day.

There's a story I vaguely remember about gin.

BR: Yes. That's correct. Pat Trown was a great man for fermentation and he had a whole lot of juniper berries and he had these in a great pot. I remember, actually, helping him to distil this one time and produce some white-coloured liquid which was extremely potent. We poured this into the eggnog which you people thought was completely alcohol-free. I guess that was all terribly illegal and bad.

VM: You actually distilled your own gin?

BR: We did, indeed. I guess that's a violation of all sorts of federal laws.

VM: I'm sure of it. Perhaps we'll have to put this bit in anonymously.

BR: I think you'd better be very careful what you do with this! It was really very interesting material. First, it tasted exactly like gin, it was made properly. Pat had looked it all up. He had gotten literature on how gin was made, he had the juniper

berries. I think he had actually used them in a fermentation with something else. I never, ever got to know what the brew was. I think it was, in some ways, more like our first visit. It was a ridiculous thing, really, to make gin in the lab. and pour it into the eggnog but it was done. It was the sort of crazy thing that would have happened the first time (*in ORL*).

VM: You haven't finished the gin story yet.

BR: Well, the gin story was very interesting because I think it's not proper for anybody to put a still up in the lab. in order to make alcoholic beverages.

VM: Melvin knew about this, did he?

BR: No, Melvin didn't know a thing about this actually. He must have been a little bit suspicious because he came in one day sniffing and said he smelled something very peculiar. I have forgotten what Pat Trown said but I remember Pat had a whole fermentation going and a proper still. The stuff that came out was really pretty good. Pat was a very good experimentalist and was good at making gin.

VM: Was there a sizeable quantity of this stuff?

BR: There was enough to lace egg nog and produce the desired qualities in the people who drank it. They didn't know they were drinking Pat Trown's gin!

VM: If I may bring you back from the gin to the science that you did when you were there on the second visit: you mentioned earlier that you had come back as an enzymologist to work on enzymology. What did you do then and how did that work out? You said that your agreement with Melvin was that you would work on the carboxydismutase.

BR: We did, I think, some interesting experiments. I think at this time it was very limited what you could do, actually, in the way of understanding enzyme action. We used some inhibitors and we produced a mechanism for the reaction which I doubt, really, would be acceptable today. We thought that one of the thiol groups in the enzyme was involved in the reaction and reacted with the ribulose diphosphate. And, indeed, that may well still be the mechanism. I think probably the mechanism that we produced, which, I think, Melvin thought was really quite good, and he had a considerable hand in formulating the ideas, explained it, whether it was the actual truth I really don't know. I think it is such an extremely complex enzyme which, of course, can accept oxygen in place of carbon dioxide, that it was really a bit arrogant to think that with the technology that was available then you could produce a chemical mechanism. But we produced one that you could present and people could argue about. And they used to get very hot under the collar because nobody ever accepted anybody's formulation of any enzyme reaction at the time.

VM: It was published?

BR: Oh, yes. We published some papers on it.

VM: Was there a strong reaction?

BR: You know, it was the usual thing. Some people said that this is a lot of horse manure and other people said this was a piece of great brilliance. It was actually neither one nor the other. It was not complete horse manure nor was it great brilliance. I think it was a carefully planned set of experiments and they could be used to demonstrate the

possible mechanism. There were an awful number of imponderables and all sorts of things we didn't understand. We had no idea of what the structure was, we had no idea of what groups there were at the catalytic site. It was interesting, we did a lot of kinetic experiments. I think we found out a few interesting facts.

VM: This must have been the very tail end of the path of carbon. I can't offhand think of anything which could have come later, or much later, than that.

BR: Well, there was nothing left to do, really, in the path of carbon at the time. The lab. was, in any case, going much more physical. But there were always very good biologists in the lab. This was the other strength. There was always an expert in the areas that you might need to know something about but you didn't have any detailed knowledge yourself. You could always go and talk to somebody about things. That was the great strength (*of this lab., even in 1963*).

VM: Thank you, Bob, for that illuminating set of reminiscences which somehow, or other, I can't tell you how, will be incorporated into whatever Sheila and I write up.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Brian Robert Rabin

Date of birth 4 Nov. 1927 Birthplace London

Father's full name Emanuel Rabin

Occupation accountant Birthplace London

Mother's full name Sophia Rabin

Occupation housewife Birthplace Poland

Your spouse Sheila Patricia Rabin

Occupation retired Birthplace London

Your children Paul Robert Rabin

Carol Rabin

Where did you grow up? England

Present community

Education Latymer's School, Edmonton

University College London

Occupation(s) retired

Areas of expertise Enzymology

Cancer therapy

Other interests or activities

Commercialization of university research.

Organizations in which you are active

Cogent Ltd, London Biotech Advisory Ltd.

Chapter 5

MARILYN TAYLOR

Berkeley (California)

May 23rd, 1996

VM = Vivian Moses; MT = Marilyn Taylor; SM = Sheila Moses

VM: This conversation is with Marilyn on May 23rd, 1996 in Berkeley.

Since you probably know more about this whole business than anybody else anywhere ever, let's start with how you got involved in the first place. How did you join the group?

MT: I was working at Cutter Lab., part-time, after I graduated from college and I saw an advertisement in the paper for a secretary at what was called the Radiation Lab. at that time I put my application in and several months later I got a telephone call from the then personnel manager who was named Mr. William Bigelow — I don't know whether you remember him or not?

VM: Vaguely.

MT: Vaguely. They set me up for an interview, I filled out voluminous papers, including clearance papers. In order to work there you had to have a Q-clearance, so all the clearance stuff went through before they even interviewed me. If I hadn't been "suitable" that way, they would never have bothered with me.

VM: Can I interrupt you at this point to tell you — which year was this?

MT: 1948.

VM: OK. Thirteen years later, when we were in LSB (*Life Sciences Building*), I was once visited by the security guys who were running a check on you and they asked me whether I could give them whatever they wanted, I don't remember what it was now. I pointed out that I wasn't a citizen, which I wasn't at that stage, and they said that didn't matter. I was very touched to be asked. You got through, I take it?

MT: Yes. As an aside, it was only two years ago that I gave up my clearance.

VM: They are still running it, are they?

MT: Yes, they do it every, I think, eight years now and there was just no point in my continuing to have a clearance. So it was a rather long-lived clearance. I got through the clearance procedure and went to the personnel office, and Mr. Bigelow talked to me. He explained there was this job with Professor Calvin and his group, he explained what the group was — it was a small group of chemists located in two different places — in the Donner and in the Old Radiation Lab. and that Professor Calvin had had very excellent secretaries, the only unfortunate thing was they all got pregnant! They were coming and going and he be very excited about them and they were very good and then, bang, they would go.

I went and interviewed the four senior staff people (*of the group*) at that time: there was Bert Tolbert, Peter Yankwich, Jim Reid and Charlie Heidelberger. They all interviewed me and I seemed suitable to them. At that time, of course, Bert asked all these pertinent questions like: when was I going to get married and, if I got married, when was I going to have children?

VM: This was all before that had happened.

MT: Yes. All before this had happened. You couldn't ask those questions now. It would not be proper. After they had decided that I would be "suitable", then Dr. Calvin interviewed me. He bounced into the office. He was much more rotund in those days. We talked a bit and he asked what kind of work I had done, which was technical secretarial. And, you know, he said "OK, you'll do."

VM: Did you know any chemistry?

MT: I had chemistry at college, Chemistry 1A/B and Chemistry 8 and then I had been working in an engineering group for some years. I knew enough chemistry. I overlapped with the girl who was there, Betty — can't remember her last name, her husband was a zoologist (*it was Betty Cohen*). In about a week she clued me into everything and off she went and I came.

VM: How big an organisation was it at that time?

MT: Let's see. There were four senior staff in Donner, Martha Kirk was there, two other women who were technicians — well, they actually had BS degrees and they called them research associates, we had one graduate student. In ORL, there was Andy (*Benson*), who was in charge, Al Bassham, Sam Aronoff, not Bill Stepka, two or three other people — Tom Goodale, Vicki Lynch (Vicki Haas, actually), the dishwashers and people like that. That was about the total group. There wasn't any other faculty associated with the group at that time. Calvin was it.

VM: How close were the two groups of people?

MT: There was very good co-ordination back and forth. In such things as counting (*of radioactivity*), for instance, the Donner had the best counting room so that's why a lot of the counting was brought over and done in Donner. Peter Yankwich was the one who was most involved in developing the counting techniques, both in ORL and in Donner. Bert and his people would synthesise the compounds that were then used with the algae and also in the animal studies, and various other things. The work in ORL would have been very difficult if they hadn't had such close in-house support for making the radioactive compounds which didn't exist then. There were very few sources of that kind of thing.

VM: Although they were separated by a couple of hundred yards, they considered themselves a unitary group?

MT: Oh yes. Of course, we always had parties together. You remember this in ORL, we had all the parties there and the people from Donner would come over. The Donner people were not as intimately associated with the photosynthesis work, or even as intimately associated with Calvin, but they were still a very important part of what was going on. When Professor (*Ernest*) Lawrence asked him (*Calvin*) to develop the group, he (*Lawrence*) was interested in animal studies as well as the plants because of Dr. John Lawrence (*Ernest Lawrence's brother, who headed a medical research group in the Donner Laboratory*). That worked into the Donner end of it and we were much more closely associated with Donner at the beginning, when I first went to work there, than later on.

VM: Calvin was very concerned, wasn't he, with the early isotope work and was an of the authors of the book ("*Isotopic Carbon*")?

MT: That book was one of my first projects. That had just been written and the final typing had been done when I showed up.

VM: It wasn't a case that Calvin was much more oriented on the photosynthesis, he was spread across both of the things?

MT: He was but his real interest...At that time, actually, paper chromatography hadn't come into the work and the efforts which were going on in ORL were very laborious and things weren't happening near as fast. So, therefore, sometimes things were happening more quickly in Donner with the animal studies that Martha (*Kirk and Charles Heidelberger*) was involved in than the work in ORL with all the chemistry that had to go on. When (*paper*) chromatography came in it opened up a whole new world for them.

VM: Calvin always liked to be at the forefront of new developments.

MT: Oh yes, I think that he was. The whole chromatographic technique really got them going.

VM: So, you started off in Donner?

MT: Yes, I was there from 1948 to 1963.

VM '63? That's when the (*round*) building opened?

MT: Yes. Actually, I moved into my Latimer (*Hall — the Chemistry Department*) office for about six months before the building opened because Calvin was given that space in Latimer (*on the sixth floor*) and it seemed prudent to occupy it with labs. That's why I was in that office for about six months before we moved into the building.

VM: So, from your point of view you saw mostly what went on day to day in Donner rather than what went on day to day in ORL.

MT: Because Calvin was located in (*the*) Old Chemistry (*Building*), I was back and forth all the time.

VM: You were his personal secretary?

MT: Yes, and the group secretary?

VM: There wasn't space for you in the Chemistry building at that time?

MT: No. Actually in a way I think it was... I never worked next to him until we went into that short period in Latimer and then over to the new building. In a way I think it was advantageous not to be next to each other because I think you are more efficient. In other words, he would get his dictation stuff together — and he was very good at that, he was always excellent at dictation and very well organised — so he would organise himself and would say "OK, come on over now". We would do it and I would go away. All this other stuff would go on — all of his students would come in or he would go back to ORL or something like that. When you are sitting next to each other there is a constant interruption and you may not be as efficient in some ways.

VM: When was it in the life of the group that it spread out to encompass so many overseas people? By the time I got there in '56 it was well away.

MT: We had our first one (*in Donner*) in 1948: that was Ted (*Edward*) Abraham (*from Oxford*). Dr. Calvin at that time had money from the Rockefeller Foundation which was one of the ways we could get foreign people in because of the clearance (*problems*). I would say really starting from '49 on we had foreign people. In '51 Peter Massini came (*from Switzerland*), there were all sorts of people. We just kept going and going and going. Of course, there was a lot of foreign money, too, overseas money could bring people in. Usually (*people came*) for a year and then Dr. Calvin would say "if you stay for a year we can take care of you (*for another year*)"; the usual routine. When these people (*the foreign visitors*) started coming and then went back home to wherever that was, they would say "gee, you have to come to California, to this place".

VM: The word just spread.

MT: The word just spread. We would have many, many more applicants, even applicants with money, than we could fit in, particularly before the building was created.

VM: I remember that by the time that I came, he (*Calvin*) was already adopting the view that it wasn't enough just to have money, you had to get the money competitively because he didn't know who people were and he had no way of judging. Did he start that very early?

MT: Yes. Very early on. The actual AEC (*Atomic Energy Commission*) budget wasn't all that great and we didn't have, at that time, very many US postdocs. You had the graduate students — there was always a group of those, and you had the senior staff and then you had people like Martha (*Kirk*) and Ann Hughes but you didn't have an awful lot of US postdocs. They started coming later.

VM: They didn't exist?

MT: They just didn't exist; I don't think anywhere they existed. The foreign people came in, and then the word spread, and then the US people came. He was always very careful to have competitive fellowship types, with the possibility of support (*from here*) for a second year or a third year or whatever.

VM: As far as the budget to run the lab. as a whole is concerned, that was all arranged by him?

MT: Yes: he would talk to Professor Lawrence, or Ed(*win*) McMillan (*Lawrence's successor as director of LBL)* or whatever and the (*budget request*) went through The Hill. At that time, things were great. You always problems if you wanted some huge piece of equipment, you had to justify it. Basically, our budget was never a real problem.

VM: So there was not the competitive atmosphere in budgeting that came in later.

MT: No. you didn't spend your whole time writing grant proposals. In fact, one of Bert's jobs, and that was also true for Dick Lemmon when he took over, was as budget officer. Later Paul Hayes took on some of that (*work*) when we had the building. They always knew how much money they needed for people, pretty much, but it was the equipment and so forth (*that was negotiable*). It was their (*Bert's and Dick's*) jobs to write the reports, get it to The Hill (*and have it sent on to Washington for final approval*). If there was any arguing or discussion, that was their function.

VM: As I recall, it was a fairly simple format that you had to say how you'd fulfilled last year's expectations...

MT: List of publications.

VM: ...what you were doing this year and what your aspirations next year were. You had this routine of shifting stuff up, year by year (*between the budgetary categories*).

MT: They still do that. It was much simpler in the earlier days. There didn't have to have quite so much justification. A lot of it was that if you were doing work, and if the people on The Hill thought you were doing good work, then it sort of automatically was taken care of.

VM: Was there a feeling, both in the group, and perhaps among others, that it was a well-funded, unusually well-funded, or was that not really the case? Were others like that, too?

MT: I think practically everything at the Radiation Lab. at that time was well funded. That was the result of the times we were living in.

VM: Did it give rise to trouble? Did people get envious?

MT: I think that people were always somewhat envious of Dr. Calvin because of his enthusiasm and the fact that he didn't stick to one little field — he was here, he was there, he was every place. We really did have pretty good funding. Some of the groups in Donner, who were working in medical research, at that time didn't have quite as good funding as we did because the NIH hadn't started rolling out the money yet.

VM: In the early days, particularly before chromatography took hold, did they ever think that this problem was going to be too difficult and they wouldn't be able to do it?

MT: I don't think I was ever involved in discussions like that.

VM: Did you get the feeling?

MT: No, I felt they'd figure it out. They were working hard. They had gotten some breakthroughs before chromatography so the they were not discouraged. But there's no question in science that some of these techniques just push you far ahead. Not just that group. Chromatography opened up things for everybody. That was one of the Nobel Prizes that was given (*for a technique*) that had a fantastic practical application, every place.

VM: When you first started, ORL was fully occupied, was it, by his group?

MT: No. Upstairs there was a remnant of a group of engineers (Paul Warrington was up there) — left over from the war days. In the back end (*of the second floor*), where they put the algae shaker, that was where the Medical Department was, that's where you went and got your lab. tests (*before you were employed*). That moved out fairly early after I started. And then, of course, there was the machine shop and the glass shop (*and the carpentry shop)* at that other end of the building downstairs (*i.e., all on the main floor*).

VM: You mean not just for the Calvin group?

MT: No. The machine shop was actually a Radiation Lab. facility — people in Crocker used it, people in Donner used it, I think even some people from Chemistry used it. That was what I call a lab.-wide facility. It was a great thing to have there.

VM: Eventually, the Calvin group occupied all the building except maybe for some of those machine shops.

MT: Yes, they occupied the downstairs. Probably within two years after I got there (1950) they had the whole building except for the shops.

VM: It was, of course, this open building, the doors were never shut, what doors there were, were never shut. Donner was very different, was it?

MT: Donner was like Latimer, like a hotel, with the doors opening (*into the corridors*), so therefore, you couldn't personally have the same kind of interaction that you did in ORL. The Latimer is that way; most of the campus buildings are that way. It's very hard, you have to make a real effort, to get together and talk about stuff. Whereas in ORL, you were all there together and that was also true when the Calvin Lab. was built.

VM: Do you think that the people in Donner failed to mix as well as the ORLers because of this?

MT: Yes, I think so. We had a very tiny little room for coffee. (*When I first started working in the Calvin group*) we used to have it in the hyperbaric chamber out behind the building that was connected with a corridor which was where experiments on the Air Force were done during the war. There was this big chamber and we would huddle around (*it with our coffee cups*), there were mice (*in their cages!*) on all the counter tops. We all made an effort to go down and do that (*i.e. have coffee*) but it wasn't the same. You had to walk a long way (*down three flights of stairs*), and it wasn't a very pleasant area (*for socialising*).

VM: I haven't heard of this hyperbaric chamber.

MT: Maybe I have the wrong word You should talk to Bert about that; he's the one who would really know the history. It was an enclosed chamber, inside this big room, called the Donner Annex, and there some experiments done there during the war for oxygen deprivation in aviators, or something like that. There was this great round thing (*chamber*) in the middle of this room, and these counters around the edges. We crowded in there. That was knocked down — I can't remember — maybe at the end of the fifties, maybe earlier, when they put the Donner expansion on.

VM: You said there were mice in there at the time you were coffee?

MT: Yes, sure. Safety has changed!

VM: Oh yes. You can't drink coffee in mice rooms.

MT: If we hadn't had that — then we moved up to some other little room (*on the third floor of Donner*), there were chemicals all over the place. None of us seem to be any the worse for wear for this.

VM: The group occupied only a fairly small proportion of the Donner building?

MT: Yes, the third floor, one side. And then the big lab. at the end, across the corridor (*from the other space*). It was all synthetic (*organic chemistry*) work. Martha's (*Kirk*) animal work and Ann Hughes' animal work was done in the same area, in other words, the animals were right there.

VM: But, of course, the Donner people all went to the Friday seminars.

MT: The seminars originally were in Donner, in the Library. The ORL people would come over for that. Everyone would all be there at eight o'clock in the morning

VM: What shifted it out of the Donner Library?

MT: I think when they (*the people from ORL*) moved to LSB (*the Life Sciences Building*).

VM: At one time they were held in the Faculty Club.

MT: Yes, that was when they went to LSB: in the Lewis-Latimer Room. That's the room they can pout a partition down the middle and they moved from the Donner to the Lewis-Latimer Room.

VM: The Donner people, of course, never moved out of Donner until they went to the Round House; they occupied that space all the time.

MT: Yes, they did. The Donner people just went from Donner to the new building.

VM: Coming back, if I may, to the time around when you came. The way you described it there must have been something like a dozen people or so in each of the two sites.

MT: Maybe 30 or 35 total.

VM: It was as big as that even then.

MT: Well, if you took in the graduate students and stuff like that (*office people, dishwashers, etc.*), the BS people level people who came and went, some stayed a while, some didn't. A sort of a floating population there...

VM: You saw it, as you did see it, from the centre as Calvin's secretary. Presumably there were many more inquiries from people wanting to come than actually finally turned up.

MT: I never it but I have a big book in the office of all the statistics through 1980 of how many people were there, what countries they came from and all this kind of thing. I used to have great big folders full (*of correspondence*) from people who had asked to come and never made it. In general, I think, the inquiries were from very excellent people. All the people were eager to do something and some were fortunate enough to get money.

VM: One of the things that has been very striking among the people we have so far talked to (not many yet) is how relaxed the whole thing was and, in particular, how unstuffy Calvin was.

MT: He was not stuffy.

VM: Not in the least.

MT: No.

VM: Particularly people who had come from foreign countries were not used to seeing (*someone of his stature who was so relaxed*). Was Calvin unusual in that, or was that the style of everybody around here at the time?

MT: I think it was pretty relaxed. I don't know about too many of the other Chemistry professors. But certainly people like Luis Alvarez had that same attitude, and Segre was pretty relaxed. So, I think these people did have an attitude of friendship toward their group. Because they were in an organisation like the Radiation. Lab. that protected them from so much of the outside difficulties of the world, they could be that way. These groups were always very close-knit. The Alvarez group was very close-knit, the Segre group was close-knit. It was hard for me to judge the Chemistry groups. There weren't really any big groups at that time (like the Rapoport group which evolved later on). There were almost like individual professors with two or three grad. students and a couple of postdocs. Bigger groups in chemistry came along later. Calvin was always busy, he was never what you would call a relaxed person from the point of being laid back, he was always pushing forward. But it was very informal.

VM: From the time when you knew him, did he have this business of getting up very early in he morning and going to bed very early at night?

MT: Always. He used to teach eight o'clock classes, they used to be on Saturdays even. He has always been an early morning person. He'd would pop into ORL at ten minutes of eight before his class...

VM: Just to make sure.

MT: Yes.

VM: And he was fat in the early days?

MT: I have some interesting pictures of him.

VM: And he smoked?

MT: Oh yes. In the Old Chemistry Building, not his second office, but the first one which was on the court yard there with the atrium, there were wooden floors and he used to put his cigarette down and grind it out.

VM: He smoked heavily?

MT: Very heavily. And he was very rotund.

VM: And he had a heart attack?

MT: Yes.

VM: About when was that?

MT: In 1949, when he was 38 years old.

VM: Where did it happen?

MT: I don't remember now. I think it was at home; I think I would have remembered had it happened at the office. It was quite severe, very severe. He off for several months.

VM: Was it a touch and go situation?

MT: Pretty much. Very severe heart attack. But I think if he hadn't had the heart attack then, and if he had had his first heart attack when he was 45, that would probably have been it. If he hadn't had the heart attack at age 38 he would have kept on with the same bad habits that he had with the smoking and eating.

VM: He spent time in hospital, presumably?

MT: He was in Kaiser.

VM: Did he work when he was in the hospital?

MT: Not in hospital. When he went home, he was told not to come back to the office for several months. He had people come up to the house and I would go up every day with the mail and he would answer the mail and I would bring up whoever he wanted to see that day. I would make coffee and they would talk, and we would all go back down again. He was working but he was at home and he was only doing it so many hours a day and then Gen would say "go now; time is up".

VM: He lost a lot of weight at that time?

MT: Yes, and he kept it off. He went on a very strict diet from the point of view of his cholesterol and even now, when he could afford it, he says "I can't have any ice cream". He is thin as rail; it would do him good to have a bowl of ice cream, maybe.

VM: There was a recurrence, wasn't there, much less severe — in the sixties?

MT: He has had several in the sixties. There was one in the late sixties or early seventies...

VM: When he was off for three months or so?

MT: Yes...and then he has had several bouts of congestive heart failure and various other things. I think he really did pay attention to his diet and various other health matters after the (*initial*) heart attack.

VM: He has always maintained a great deal of liveliness in spite of whatever physical problems he may have had.

MT: It's only in the last two or three years that he has sort of diminished his psychological energy.

VM: To get back to the ORL group in the late forties, early fifties. Calvin at that time presumably was the way he was later. He would take an intense interest day by day in what was going on in the lab., looking at data, etc.

MT: Every day.

VM: But he never did any experiments himself, did he?

MT: From what I remember in writing that book ("*Following the Trail of Light*") he was doing experiments early on, probably in '47 — maybe in '48, I don't know — but that sort of ended. Other people were coming in, things were going too fast, he had his teaching, as the group got bigger he had to pay some attention administratively to the group. I would say — and you could check this with Al (*Bassham*) or Andy (*Benson*) — probably not much after '48 or '49 at the outside, maybe even earlier, he just didn't do experiments himself.

VM: You say "his administrative responsibilities": he never was actually much of administrator in the lab. himself.

MT: No, but he was appointed to various university committees which required his attention. One was the Educational Policy Committee. In addition to his Rad. Lab. stuff, he did have university responsibilities. If you are put on these things you have to do some work and that took a little time. Plus, he started travelling, giving talks and that took him away from the lab.

VM: When did that occur?

MT: He had been going to things like ACS (*American Chemical Society*) meetings throughout his career since he was at Berkeley. But I think one of his first international trips was to England in '49. From then on, it just burgeoned. He was always going. He took a sabbatical in '50 and was in Europe for five months.

VM: Where did he go?

MT: It was Norway, mostly: he spoke in England, he spoke in Germany (*he also went to Italy*).

VM: He was getting better known, I guess, by then.

MT: The first path of carbon paper came out in '48. In the meantime, he had all these other interests, the chelate work (which just came out all of a sudden as a result of declassification at the end of the war). And all the synthetic (*organic chemistry and reaction mechanism*) papers were coming out from the Donner. His name was becoming very well known in many fields.

VM: Was he talking about all those things or was he talking mostly about photosynthesis?

MT: If you look at the list of his speeches, he was talking about (*everything*). When he got into the thioctic acid later on, he even talked about the stilbene work he did with Janet Splitter — he was talking about everything. Of course, his favourite thing was the photosynthesis. He also enjoyed the deuterium work that you guys did, he thought that was fun. He had things that he liked the best to talk about.

VM: When people came into the lab., when this turnover of postdocs and graduate students came in the lab., did he actually think ahead of time about what they were going to do? Or did they shop around?

MT: Most people, as you know, in their (*initial*) letters (*of inquiry*) said we've done so and so and we'd like to use this knowledge in the field of so and so. Most of them did not come in and say "I want to work only on this problem". So they would come in, and they would have a talk, and Andy and Al and everybody would be in there, and there would be some interesting problem discussed, and he (*Calvin*) would say "why don't you try that?" And off they'd go!

VM: He wasn't dogmatic about what they should do?

MT: No. He didn't say "you have to work on this". There were some things that were so exciting that he was always happy to have more than one pair of hands working on it. But the people that came with preconceived ideas about what they were going to do — they wanted only "this" — didn't have as good a time. There were some who did that: (*Gustav*) Utzinger was one. He only wanted to work on whatever he was working on down in Old Chemistry, the organic chemistry problems. I don't mean that he didn't get something out of it (*his visit*), and because he came on his own private money, there was no reason he couldn't do that within the scope of the whole group. In the ORL group, the photosynthesis thing was going so fast that there was always something new and exciting, particularly when you go down a little further toward the late fifties/early sixties and you get into ESR, NMR — all that kind of thing broadened the whole program.

VM: There were plenty of rumours, about which I am very vague, about differences of opinion between Calvin's group and other groups, the Arnon group was one.

MT: That's why Nate Tolbert wanted you to talk to Bob Buchanan.

VM: How did you see it from your point of view? What do you remember of it?

MT: I certainly remember hearing about it but I couldn't scientifically judge anything, because I'm not a scientist. I think that Dr. Calvin sometimes rushed into print a little sooner than he might have if he hadn't had someone breathing on him in a way. I think Arnon was as unique a character in his way as Calvin in his, and there was also Martin Gibbs. So, they wouldn't all love each other. There was definitely conflict with Arnon.

VM: These conflicts were not just technical ones — were they personally antagonistic?

MT: I don't know. I can't answer that question.

VM: You never saw them together?

MT: I never saw them together. Al (*Bassham*) can tell you more about that.

VM: I remember when Otto Kandler came, there was intense argument, wasn't there, for many months?

MT: Yes. Again, I wasn't into it from the scientific point of view. But you could see it personally in the lab. Just walking into the lab. and they would be going at it, verbalising.

VM: Did you get the impression that for all their arguments they disliked one another?

MT: I don't think so. I don't think Kandler disliked him. I don't know what Calvin felt for Otto Kandler. They didn't have that much contact after Kandler left. Calvin kept up close contact with some of the people (*who had been here*), but I don't think Otto was one of them.

VM: Wasn't there a time when Trudi Kandler and Gen (*Calvin*) decided to try and make peace between them?

MT: I don't know.

VM: I'll have to talk to some of the others and see whether they remember that. One of the things that was very striking when we (*first*) came (*in 1956*) was the social atmosphere in the lab.

MT: It was wonderful.

VM: And it was like that from the beginning?

MT: Yes, much more so in ORL than in Donner, again because of the physical closeness of the people. There was always a lot of social contact, lot of parties, weekend skiing, the camping, the beach parties, party, party, party. Very, very good. That even maintains itself today, surprisingly enough, in the round building, with the various groups in there now — they are always doing things together.

VM: And it was from the beginning like that?

MT: It was always that way from the beginning. I've always felt that was one of the wonderful things about that group was the social interaction among everybody and on an equal plane.

SM: Were Calvins part of these group social activities?

MT: On some occasions. I think they went on one ski trip that I know of. The local things like the beach parties at Stinson, they would go, the camping I don't think they ever went. But the were always asked. Partly after Calvin had his heart attack, he may not have wanted to do that kind of thing so much.

VM: When you joined, the use of ORL and Donner had already been in existence for two or three years, I guess. Did they think it was going on forever or did they see the possibility that the building was a temporary structure which wouldn't last?

MT: You mean ORL?

VM: In particular, ORL.

MT: I think they thought it was their home but that's just my own opinion. The problem with the building was the fact that it was so old that the facility was not adequate for the kind of science that was going on in that building. About 1958 when they were starting out Latimer Hall they had to knock it (*ORL*) down because they had to make room for the big new Chemistry complex. I think until maybe the late fifties it just looked like it was our home, for the photosynthesis people not the Donner people. I think it really came as a personal psychological blow to Calvin when they had to knock it down and the group had to move down to the Life Science Building even though they thought they would have adequate space in Latimer Hall. That turned out not to be true, and that's when Calvin went for his own building. There would not have been room in what Calvin was assigned in Latimer Hall for the ORL people and the Donner people and the whole thing; it would just not have been enough room.

VM: Let me try and get the sequence clear. At some stage, it became clear that the Chemistry Department was going to build Latimer Hall. Can you remember when that was?

MT: They knocked ORL down in 1958 so it must have been in the mid-fifties.

VM: So they (*Calvin and his colleagues*) knew some time ahead of time that things were going to happen. What did they do in response to hearing that the building was going to go?

MT: I do not remember or I just was not involved in this. It had to be because they got the money for this new building (*Latimer Hall*) and it involved not only ORL, it involved also Crocker: they had to knock down Crocker as well as ORL in order to make room for this Chemistry complex. There was a lot of scurrying around. And also the Anthropology Building went down to make room for Campbell Hall. There was all this building on campus in the mid- to late-fifties which required something like ORL to go. Of course, there was no justification even to try and keep it because it was such an old building. I do think that Dr. Calvin (it was about 1960, maybe even earlier, in the late fifties) saw the need for a building of his own because there was not going to be enough space in Latimer Hall for his activities.

VM: The original thing is that he presumably agreed to a temporary residence in Life Sciences because he had to go somewhere and originally he thought he was going to get back into Latimer?

MT: No — I think at that point, when they went down to LSB, he knew he would have a certain amount of space in Latimer, no matter what, as a chemistry professor but it was not going to be adequate for the photosynthesis and the rest of the group. About that time (*the beginning of the sixties*), he started writing building proposals to various people (I have a whole shelf of building proposals!).

VM: That was when the group was already down in LSB.

MT: He actually got the money for the building before he got his Nobel Prize (*in 1961*). Some people say that because he won the Nobel Prize, he then got his building. It didn't work out that way. The building was being designed and all that kind of thing when he got his prize.

VM: So had he not got a building of some sort, they might even have had to stay there?

MT: In LSB?

VM: Something like that. It's probably unpredictable as to what would have been the consequence. But the group was in LSB for about five years, as I remember. I suppose they must have known fairly early on... Well, you tell me: how did he set about finding money for a building?

MT: First of all, he discussed it with the people on The Hill and the AEC. I think they would have funded it but he would have had to go up to The Hill, and he absolutely didn't want to do that because, by that time, we had people from psychology, biophysics, biochemistry, molecular biology in his group. When it became clear that he did not want to move the group up The Hill, I think he discussed it with (*Glenn*) Seaborg, who was then Chancellor (*of the Berkeley Campus*) and they suggested there that were certain avenues he could apply to and the State (*of California*) would provide a certain percentage (*of the construction costs*), the National Institutes of Health and the National Science Foundation. So those three people were...we didn't have too much to do with the State but these other two were (*indecipherable*). He also wrote at that time to all sorts of other foundations — I can't remember, there were so many things going on. This was before computers, before Xeroxes; it was laboriously typed out over and over again. It came down that the NIH would give a certain amount of money, NSF would give a certain amount of money, and the State; and then there was this $300,000 that the Kettering (*Foundation*) people (*provided*). The AEC equipped the building. So, in a sense, it was joint thing — there was AEC money in the building in the form of equipment.

VM: The design of the building, the, must have begun reasonably early because you have to go out for money on the basis of something; you can't just wave your arms around.

MT: I don't remember the date of the first architects meeting but the senior staff got together and the campus people hired Michael Goodman to be the executive architect.

VM: That was their choice?

MT: That was their choice, that was the campus choice. They had certain people they worked with and he was also on the faculty of the School of Architecture.

So Michael Goodman was the architect and then they had Florence (*Porter*), his assistant, was the liaison between Michael and the people (*i.e., university, senior staff, contractors, and the AEC*). The senior staff got together (*to decide*) what kind of building would they like. What do we want in this building? The whole thing was the kind of space that they'd had in ORL. Even people who had worked in Donner saw how good that was; it wasn't that they wanted to work in a building that was just like a hotel which was just like the facilities they had. With that kind of philosophy, (*the evolution of the design*) was interesting. First of all that was half a circle...

VM: I remember that that was Al Bassham's original design.

MT: ...and then that didn't look too good and it seemed to be a full circle, with the various kinds of room for chromatography, and stuff in the cores. It turned out to be a very practical plan, actually.

VM: As it worked out, the building had very few faults.

MT: It turned out remarkably well and even when they (*later*) had to put the temporary trailers (*for cancer research*) up on the roof it didn't affect it (*the building*) that much. I remember there were these planning meetings with the architect, planning meetings with the campus people, and all the senior staff participated including Paul Hayes. It's too bad Paul didn't keep a record (*of the history of the building design*). He did, actually, have a lot but I think it's been destroyed now, or sent away to the archives, of how the building was created because there was a lot of discussion back and forth.

The building came out very well. The philosophy was there, the facilities were very good (state of the art at the time [*in 1963*]), a lot of good work has been done in that building just as enthusiastic work as was done in ORL.

VM: Can we talk a bit about Paul Hayes, because he's no longer here, unfortunately to talk for himself? Was he originally a scientist in the group?

MT: Yes, he had a degree in chemistry from somewhere in Texas. He was hired as a BS chemist to work in ORL. I think he took over after Tom Goodale was working on the path of carbon when chromatography first started out; he also worked with John Barltrop on the thioctic acid problem. So he was a chemist and he did do chemical experiments. Because of his personality and his background, particularly as we got into the building design, somebody needed to be able to spend full-time, basically, with the architects and the campus people (*and the contractors*) and the people on The Hill. Paul seemed to be able to be that person. He had enough chemistry to know what was important but he got along well with that kind of person and it didn't take him away from any really interesting research projects that he might have had to give up, or not work so hard on — like, say, Al and Dick — it was good to have someone like Paul who could be their liaison and take over the day to day headache of that kind of thing. He did this very well.

VM: He was really the guy who coordinated, from the group's point of view, the new building and how they were going to fit into it.

MT: He did a very good job. The whole project ran very smoothly. Of course, there sure was lot of frustration, but basically it went very well. But we were doing this at a good time when there was money. When there's money and you're not having to scrimp too much, things go along a lot better.,

VM: How did Michael Goodman view this building, do you remember?

MT: He thought it was funny. At beginning, he sort of laughed and thought it was a crazy building. But, I think, he liked it, when it was all over. It looks like a little cake with a little frosting on the top. No room is square and there is hardly any rectangular space in that building. I think he liked it a lot: he would sort of joke about it but I think he did like it.

VM: I have to say, looking at it nowadays, that compared with some of its near neighbours, it looks rather an attractive building.

MT: I think it's the last little building ever built on that (*the Berkeley*) campus. Calvin later wanted to extend it upwards but they didn't make the foundations originally strong enough so we couldn't do that. You just can't afford little buildings any more. Like the houses in the Berkeley/Oakland hills that burned in the (*1991*) fire; they can't afford a little house on it *(i.e., each individual property*) any more like the original ones that were burned down; it's the same thing. Looking back on it, when we first moved in and for many years after, I think it was a charming building. It was a fun place to work.

The enthusiasm was there, the (*first*) day I walked in and it certainly lasted through the time Calvin was there in the round building. People were very enthusiastic in that group, even the people in Donner who sometimes felt a little isolated from what was going on, particularly when the other group went to LSB. We (*the Donner people*) didn't feel we were as much a part of it (*the Calvin group as a whole*) but that was just because of the physical distance. But everybody (*in the Round House*) was enthusiastic about what was going on. With the things happening in photosynthesis, it was very exciting but there were also things happening in animal biochemistry and in Dick Lemmon's radiation chemistry and the chemical evolution experiments. There was just exciting science going on (*in the Round House*), in all different directions.

VM: The photosynthesis was, of course, the anchor by which, at least, the ORL contingent functioned, but lots of stuff spun out from that. There were people who were very much a central part of that group who were not actually working on photosynthesis, or not directly. Anything that they did reflected back because there was so much back and forth communication which went on all the time.

MT: It was amazing that that happened. It even happened, actually, when we got into the new building. The third floor was supposed to be mostly photosynthesis — it turned out to be a lot of other things in addition — that reflected back not only on photosynthesis but, if you want to call it, structural biology now — the whole idea, the different things they were doing.

VM: Nevertheless, there was a difference in atmosphere, wasn't there. I wasn't there at the time when the move actually took place out of ORL: I left with it functioning and came back to find everybody buried in the basement of LSB. Obviously, things were different. Good though the Round House has been, it never quite recaptured that atmosphere (*of ORL*).

MT: No, it didn't.

VM: Maybe that's simply because it was just later. People were older and science had moved on and they were thinking different things, as well as the structure of the building.

MT: Calvin made a comment one time, about it used to be he would go into ORL, day or night, the graduate students would be there, they would be working, and the postdocs; everybody would be working. "Now", he said, "it's like a business — it's a 9:00 to 5:00 or 8:00 to 5:00 or something." That was ten years or more ago that he said that.

SM: Some of the other people we interviewed, they made the point that during that later period he was less around because he had to be in Washington or he had to be in other places. It was his presence at the time in ORL that made the real difference.

MT: I think that made a tremendous difference. I think that as he progressed, or whatever you want to call it, and got involved in things in the National Academy of Sciences, the President's Science Advisory Committee, all sorts of other things that he was involved in, he was gone at least half of the time for some years. Therefore, those people who came through during that period of time didn't get the nurturing from Calvin personally that they had gotten earlier on (*i.e., that earlier people had received*). That was just one of the reflections of fame, if you want to call it that. People tend to get involved: they are asked to do something, they feel they need to serve and so one service leads to another service and pretty soon it just snowballs.

VM: I think there were a number of factors which were important, as you say. One of them was that when the group started, Calvin was very much the leader and the senior man. But as the years went on, everybody got older and they (*the senior staff*) were all becoming pretty well known, the leaders of the various groups, and themselves being invited to international conferences. So although Calvin was still the leader he couldn't quite be the leader in the same way as he had been a lot earlier. The fact that he was being involved in things more and more remote from the daily activities of the lab., just changed the character. So perhaps that was one of the contributing factors for the Round House simply being different from the earlier days of ORL. It was a younger period altogether for everybody.

MT: I think that is true of organisations. In other words, as the organisation gets bigger and more important then the leaders are asked to do this and that and they just don't have the physical time to spend. There was nothing wrong with the building itself. Everything was very conducive, I am talking about the Round House now, to interaction. The building itself contributed to interaction. But he was gone, a lot. There's no question about it.

VM: It's surprising, thinking back, how well, even by modern standards, ORL functioned as a building. Because it was actually a very crummy building as a building, very old fashioned in the way it was put together. And yet I don't recall at the time that there was any great difficulty in working in it. I think that everything seemed to work well enough, even in this funny old building.

MT: You had good equipment. In other words, even though the building was old, you weren't suffering with really old equipment. When something was needed, you were able to buy it, you were able to set things up in funny spaces, but, nevertheless, the equipment you had, there was never a problem. You could always get stuff, or have it made. Even though you may not have physical glamour, you had everything you needed to do your work, plus all the people.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Margaret Marilyn Taylor

Date of birth March 27, 1924 Birthplace San Francisco, California

Father's full name Archie Roy Mack

Occupation Educator Birthplace Topeka, Kansas

Mother's full name Margret May (Brown) Mack

Occupation Housewife, teacher Birthplace Belleville, Kansas

Your spouse Terrence Henry Machell Taylor — deceased

Occupation Engineer Birthplace Vancouver, BC, CANADA

Your children Terrence Douglas Taylor, Alan Henry Taylor, Gary Richard Taylor, Lynn Louise Taylor

Where did you grow up? Berkeley, California

Present community Berkeley, California

Education B.A. University of California, Berkeley 1946

Occupation(s) Administrative Assistant to Professor Melvin Calvin, Lawrence Berkeley Laboratory, since 1948

Areas of expertise

Other interests or activities

Organizations in which you are active Lawrence Hall of Science, University of Docent Coordinator. National Docent Symposium Council, Vice-Chair

Chapter 6

RICHARD M. (DICK) LEMMON

Berkeley (California)

May 24th, 1996

VM = Vivian Moses; DL: Dick Lemmon; SM = Sheila Moses

VM: This is a conversation with Dick Lemmon on the 24th of May, 1996 in Berkeley.

Dick, how did you come to join the group of people that later became the Bio-Organic Group, or maybe they were always called the Bio-Organic Chemistry Group?

DL: I had spent the war years — I'm a little hesitant to say very pleasantly — working on the Cal Tech campus on a war project on the chemistry of rocket propellants. While I was there, I got to know very well a Jack Miller, who was a Berkeley chemistry BS and who was very impressed by a new young member of the faculty at Berkeley, whose name was Melvin Calvin. He said that if I wanted to go on to a PhD that he couldn't give me a higher recommendation. He said, "don't stay at Cal Tech, go to Berkeley, where there not only is there this fantastic young chemist Melvin Calvin", but he had some other reasons, oh yes, his other reason for going to Berkeley was that Berkeley was on the cutting edge of nuclear chemistry, creation of new elements and their use as tracers, and all this kind of thing. For those two reasons he said I ought to go to Berkeley. I followed his advice.

VM: Were you in Pauling's department at Cal Tech?

DL: Pauling was the overall director of this war project that I worked on. A collaborator of his who worked on protein structure with Calvin (*should this read "Pauling"?)* was the immediate director of the group that I worked with. One of the main things we were doing was using column chromatography as an analytical tool. What was in captured German and Japanese rocket propellants was better than what was in ours? The answer to that was "practically everything".

VM: When was this, that you first contacted Calvin?

DL: : It would have been in March of 1946.

VM: What sort of group and set-up did he have at that time?

DL : Practically nobody, not even Al Bassham then. The group had just been formed and Andy Benson was the main person in the group. I'm not sure about Bert (*Tolbert*) —

whether Bert was there or not, it was a month or two earlier or later. But I think Andy and Bert were about it. There was Dorothy Johnson, a young Berkeley BS chemist, who also joined the group about mid-'46. That's all. The group was only three or four people. Oh — Peter Yankwich might have been a member then. Perhaps all of the authors of "Isotopic Carbon" were members of the group at that time. There might have been half a dozen people in that group when I joined as a graduate student.

VM: When you first came was there the clear division of activity between the photosynthesis and the synthetic isotope work that later was so clear?

DL: The group really was formed, E.O. Lawrence's charge to Calvin, was to find ways to utilise carbon-14, the newly-available radioisotope of carbon. I don't think that Calvin had the photosynthesis idea any sooner than he had the idea of the general isotope work. When I talked with him as a prospective graduate student he proposed photosynthesis to me, and I have always regretted that I didn't do it, but photosynthesis was a complicated business, as I saw it then. Another thing he proposed to me was to use the carbon-14 to see how a compound, pyruvic acid or the ester ethyl pyruvate in particular, this compound has two carbonyl groups, adjacent, and one of those carbonyl groups comes off as CO. Which one? It was a very obvious system to work with, it looked like the synthesis would be easy (it wasn't). It turned out to be quite do-able, to put carbon-14 in one of the carbon atoms and see which carbonyl came off. That worked out beautifully and became part of my thesis.

VM: Did you have any biological background?

DL: Not really. I can't remember any course in biology of any sort that I ever took.

VM: So, photosynthesis would have been a bit mysterious for you.

DL: That's perhaps another reason why I didn't take (*that suggestion*).

VM: So, you joined this small group as a graduate student, working on the ethyl pyruvate release of carbon monoxide, and where were you physically?

DL: When I joined the group at that time, its only space was in the Donner Laboratory, on the third floor, and that continued to be the space for research other than photosynthesis. As I recall, the photosynthesis people moved into the Old Radiation Laboratory about mid-1947.

VM: As late as that?

DL: As late as that (it might have been early 1947). They moved over from Donner to take up the photosynthesis in the Old Radiation Lab.

VM: The first time you met Calvin: can you remember meeting him?

DL: Pretty specifically.

VM: Tell me.

DL: Well, he was obviously very energetic, which I expected, he was very smart, which I also expected, he was very overweight, which I had not expected. But, he was very good, I think, at interviewing a prospective graduate student. He was easy to talk to; although he obviously very bright he was not formidable. I felt at ease talking to him.

VM: Was he very informal then? I always remember him as being informal...,

DL: Yes, he was quite informal. Very easy to talk to.

VM: This was in his office in chemistry?

DL: This was in his office in the old brick chemistry building. Was that still in existence when you came to Berkeley?

VM: Yes, I remember it, the one with the turret (*actually a cupola*) still kept as a momento. I was there before that fell down.

DL: Makes you an old-timer.

VM: Otherwise I wouldn't be doing this!

SM: That's where the fireplace came from (*the one which was in Calvin's office in the Round House*).

DL: That's where he had his fireplace which had been G.N. Lewis' fireplace and that's why he (*Calvin*) was particularly fond of it.

VM: So, you settled in in the Donner Lab. in one of those rooms on the third floor. Were there other graduate students there at the time?

DL: I think Gus Dorough was there, some of the time at least. Other than Gus, I think I was the only graduate student. Al Bassham (*James A. Bassham*) joined the group: I joined it in April and I would say Al perhaps two months later, very close. But I can't specifically remember Al working in Donner. So, I think he joined just as the move (*of the photosynthesis people*) to the Old Radiation Lab. began.

VM: What sort of group feeling was there at that time, because for someone like myself who knew it only much later when it was very much more developed.

DL: We really felt that we were on the ground floor of a very exciting era where isotopically labelled compounds were going to solve all the problems in biology and chemistry. If you wanted to know what happened to CO_2 in photosynthesis, you just put in the radioactive stuff, follow the compounds it made. Essentially that idea was quite correct, although it was much more complicated than people knew at the start. We knew that we were on, as I say, the ground floor of an exciting era in chemistry and biology; we knew that we had as director of the group somebody really exceptional. Calvin was already invited to give lectures all over the country, even though he was, what was Melvin then? He was still in his 30's.

VM: He was born in 1911; yes, he would have been...

DL: Yes, in his thirties... and all these invitations and prizes that he was already getting, like the Local Section of the (*American*) Chemical Society had made him their chairman, and a lot of things told us for sure that Calvin was going to be a big name in chemistry.

VM: You were conscious, were you, that you really had a monopoly on carbon-14 at the beginning.

DL: Yes, indeed we did. It was first produced here in the 184-inch cyclotron, right after the war, and the director of the Radiation Laboratory, Ernest Lawrence, had invited Calvin to use it and he obviously was going to hold back on requests from the rest of the country to let Calvin be the first into the use of this important material.

VM: Somebody mentioned, but I can't remember who, that one of the earliest sources of C^{14} was from bags of ammonium nitrate which were placed around the cyclotron. You must know that story.

DL: Yes, that's right. It was a reaction of protons bombarding the nitrogen atoms which then became carbon-14. That's why the ammonium nitrate, a heavily nitrogen-containing compound.

VM: Was it actually placed there as a shield as a shield? Is that why it was there?

DL: No, it was there as the reactant to get carbon-14. The nitrogen in the ammonium nitrate (nitrogen -14), proton in, neutron out, the same way essentially, gave you carbon-14.

VM: So it was put there deliberately for that purpose?

DL: Deliberately as a reactant. They had shielding, of course, but that was lead bricks.

VM: You guys worked it (*the carbon-14*) up from that?

DL: You know, I'm not sure about this. All I remember was getting the carbon-14 as barium carbonate. After the irradiation of the ammonium nitrate and the formation of the carbon-14 it was recovered as CO_2 which bubbled into barium hydroxide which became barium carbonate. This is pretty simple chemistry. Some technician up on The Hill was doing this and we first got it as barium carbonate.

VM: But later on, presumably, you got it from the nuclear reactors, not from the cyclotron.

DL: This was in 1946-47 and, by — I would say by the early fifties, it was commercially available. We did supply a few people, I guess were friends of Calvin, and helped them out with occasional small samples of our own carbon-14.

VM: It must have been five-seven years before there really began to be a commercial supply.

DL: Yes, I would say the early fifties.

VM: What sort of group activity did you have internally? When did the seminars start?

DL: The seminars, I think, were already an ongoing institution when I joined and they were held in the Donner Laboratory's library which was a floor just below our main laboratory.

Which brings up — maybe I can diverge a little bit, with a story about that. Up in our laboratory we had the job, always, of removing the last traces of carbon-14 from any glass vessel that had been used. The way we did it in those days was to have a bath of warm dichromic acid-sulphuric acid, concentrated. It was a wicked, wicked brew. There were a lot of precautions taken to try to be sure that this concentrated dichromic

acid stayed in its place in this bath. One night it began to drip down onto the library below us. Fortunately, the drip was small in amount and no books were damaged down below. The flooring was damaged, that kind of thing, but we had daytime nightmares thinking of what could have been the case if that whole big pot of stuff had failed. It was a stainless steel pot. Even so, the dichromic acid somehow managed to eat through that...

VM: Actually corroded it? The pot was leaking?

DL: Yes, the pot was leaking.

VM: Not only was it this strong acid, oxidising acid, but it also had the residues of your C^{14} in it?

DL: That would have been, from the standpoint of the health hazard, very, very minor because in those days we had "tut, tut, tut, tut, tut, tut, tut" (*i.e. very little*) radioactivity, and that was enough for your experiment and what you had to do. What it took to damage a living organism was thousands, if not millions of times that.

VM: So the acid rather than the radioactivity...

DL: The acid; the radioactivity was of no consequence. Today, it would be. If say there's radioactivity in anything, 95% of the American public wants to run in the opposite direction. When you tell them that they have carbon-14 in their own bodies, they don't believe it.

DL: So were you sitting there, one day, having a seminar, with this stuff coming through the ceiling?

DL: No. This happened in the early morning hours, something like 3 or 4 or 5 a.m. The first person who got to work that day found a slow drip of this awful stuff down in the library below.

VM: Were the seminars always at eight o'clock in the morning on Fridays?

DL: Yes. In the early years they were., I don't think it was you, Vivian, but I remember somebody saying that in England this would be regarded as totally uncivilised.

VM: I think all of us would have said it.

DL: I was one who thought that was a good time for the seminar.

VM: I mean all the English would have said it.

DL: The English; yes, I dare say.

VM: How quickly did the group grow?

DL: Let us say there were six, roughly half a dozen in 1946. I would say by 1950, another four years, it had grown to be 20-25. I have no firm handle on this; this is just a general recollection of how it was I don't think you'd find that seriously wrong. Something like 20 or 25. By 1960 we were maybe 40...

VM: ...or even more than that.

DL: Even more than that.

VM: I remember that by the time we got into the Round House...

DL: Maybe you're right.

VM: ...there were actually 90 people or so, living in that building. Some of them were really attached more to Rapoport and other faculty. Nevertheless, in the sense of being members of the building community, there were about 90 people.

DL: I still don't think I'm too wrong about saying 20 in 1950.

VM: How did they begin to come? What drew them? Was Calvin's work becoming publicised.

DL: The main thing was people who were interested in photosynthesis saw paper after paper and could see that the head of the laboratory doing this was one Melvin Calvin. Perhaps even more came from their professors of whatever institution they came from. They knew Calvin personally, or knew him very well from the literature, and knew he was a very important guy. So, if you wanted to work for your PhD with somebody who is a real live wire, I recommend Melvin Calvin.

VM: Did they begin come from abroad as well as from inside the US?

DL: Oh sure.

VM: Quickly? Early in the day?

DL: I remember a man by the name of Hirschberg who came to us from Israel. He was here in the early 1950's.

VM: We met him here in 1956, when we first came. But I don't know whether he hadn't perhaps been here earlier, before then.

DL: Well, I can't tell you. I said early fifties: so if he came he came '52 to '56; something close to that. There were, let's see: I'm, sure there were others, other than Hirschberg. I remember a man from Brazil, though at least for the moment I have forgotten his name (*Henrich Hauptmann*); he was a very early member of the group. He was a professor of chemistry at one of the major universities in Brazil. As I recall, he was a very good chemist. He did some work in mechanisms, it wasn't in photosynthesis.

VM: Did people come because of the development of C^{14} technology, did they come to learn that?

DL: That was a major reason. That reminds me of one of our earliest foreign visitors, Professor Hans Schmid from the University of Zürich in Switzerland. He was attracted to Calvin's group specifically by the availability of carbon-14. He was a very good organic chemist and not so interested in things biological. I remember his saying — he wondered if the customs people at the airport would catch him as he brought the first carbon-14, research carbon-14, into Switzerland and he did not declare it! (*Laughter)*

VM: They wouldn't have known what to do with it!

DL: That is the second foreigner that I remember. Hans came in 1948 or 1949.

VM: So people began to come for a period, either as graduate students or as postdocs. for a year or two, quite early on in the life of the group?

DL: Quite early on, definitely. I am sure there's another one or two that I am not thinking of at the moment but who were here, let's say, in the forties.

VM: The style, presumably, gradually progressed to having a central core of people who were permanent scientific residents of the group and the floating population...

DL: ...sure, of graduate students and postdocs and visiting faculty.

VM: And you became one of the permanent people when you completed your PhD?

DL: No, I spent a year in Zürich as a postdoctoral fellow. Toward the middle of that year, I began to think what am I going to do when the year's up? I wrote Melvin a letter saying that if he had something worthwhile for me to do in Berkeley while I was seeking a permanent job, and he wrote and said "yes" . I came back and nothing was said from that day to this about when I was leaving. (*Laughter*) I did a little looking around, for some kind of a job in industry or academia, and I don't remember making any particular progress whatever. But nothing was said about my leaving, so I just didn't leave.

VM: When was the year that you spent in Zürich, so I can reconstruct (*the sequence*).

DL: I was there in the academic year '50-'51.

VM: By that time, what was the position of people like Yankwich and Charlie Heidelberger; was Kritchevsky there?

DL: Kritchevsky was here in '49 — no '50. He came here while I was in Zürich because he had come from Zürich,.

VM: Was he a temporary visitor?

DL: He was in his second postdoc. assignment; the first was in Zürich.

VM: Who were the permanent people at that time?

DL: The authors of "Isotopic Carbon": Calvin, Reid, Tolbert, Yankwich, Heidelberger. So they were all here as permanent people.

VM: So they were AEC employees except for Calvin who was faculty?

DL: That's right. Which reminds me, I wanted to say sometime here — although it's obvious to you, it might not be to some others — that when the new space became available to Calvin in the Old Radiation Lab. and everybody but Calvin thought it looked like a cow barn and you couldn't make a lab out of it, he was right, you could make a good lab out of it. Then all the photosynthesis people moved into that lab. All the rest of the group, who were doing things other than photosynthesis, were in Donner. There was a connection, however. It came about this way: one of the main lines of research in our Donner section became radiation chemistry because it turned

out, quite unexpectedly, that one of the compounds that we had synthesised for somebody's tracer use some place else was a compound called choline chloride which was decomposing under the effects of its own carbon-14 β-particle emission. That led to a lot of work on choline chloride — why was this compound so unstable? A lot of the mechanism was worked out that the real reason for that instability is still not known. But this got us into the area of radiation chemistry. One of the things that Cyril Ponnamperuma (*one of Calvin's graduate students*) was doing when he came was the radiation chemistry of adenine. So, in addition to putting carbon-14 into compounds of (*biological*) interest, there was this radiation chemistry. There was still organic chemistry mechanism work going on. For instance, Pete Yankwich's work on the decarboxylation of malonic acid which first showed up with the rates of reaction of the carbon-14 compounds were not just the same as the carbon-12. Later, it was found in the photosynthesis work that indeed the algae or plants prefer carbon-12 compounds to carbon-14 compounds.

VM: I remember that. That was me and Ozzie (*Holm-Hansen*) and Chris Van Sumere.

DL: How about one John Weigl?

VM: John Weigl is dead now.

DL: He's dead now but I'm talking about when you were saying... He got his PhD in '49.

VM: I never knew him.

DL: W-E-I-G-L.

VM: I know who he is, yes.

DL: He was here...he left after he got his PhD. But as I remember, I thought that John was the first one to show this (*isotope effect*) in algae; I could be mistaken.

VM: Maybe we just repeated what he did and didn't even know about it. That's happened before in history!

DL: I would have thought that Calvin would have put you in contact with him — he could have called up on the phone...

VM: We certainly did a paper like that in which we showed that (*effect*)...as you happened to mention it.

DL: Charlie Heidelberger, in the early fifties, was working on a rather complex synthesis, I think it was (*carbon-14 labelled*) benzanthracene *(Editor's note: It was in the late forties; Charlie Heidelberger left the group in 1948*) which had a very theoretical role in cancer and Heidelberger went on to a career in cancer research (*at the University of Wisconsin and later*) at USC (*University of Southern California*). You know that he's gone: he's dead.

VM: When the photoynthesisers moved into ORL, what was the feeling among those who didn't move? Were you glad not to go into this shack or did you feel that you were missing out? Was there a feeling of loss?

DL: I think that those of us who were not doing photosynthesis, we all had something else going and wanted to see it through. We might have said when I'm through with ethyl

pyruvate, or whatever, I'll maybe do some work in photosynthesis, which, incidentally, I did. Part of my thesis was the role of pyruvate in photosynthesis.

VM: Was it? Was that published?

DL: I don't think it was and I don't know why it wasn't. I think it's because Melvin got very interested in something else and so did I in something else. We just never pursued it. The pyruvate became acetate mostly and it wasn't very profound work.

VM: But pyruvate is the very centre of metabolic biochemistry; you can't get a more central compound than that.

Was there a feeling that the group was splitting?

DL: No. We were all together and we'd meet once a week in the seminars. The people doing photosynthesis contributed ideas to the people over in Donner and vice versa. It's an interesting question that you ask and I have never thought of this before. My impression was that there wasn't any feeling that we had been split apart. After all, we had the same director as well as the same seminars.

VM: That's true. And the distance between you was only 150 yards.

DL: Yes, good exercise.

VM: Was there a lot of toing and froing, people moving between the two labs.?

DL: I'm sure, using pieces of equipment that were available one place and not the other. There was a lot of such.

VM: There was no sense of estrangement between the two groups.

DL: No and there was a very good reason for that. Melvin himself didn't reflect a dichotomy. He seemingly was just about as interested, maybe not quite, but still very interested in the work in Donner and by the mid-1950s I think he was smelling a Nobel Prize in photosynthesis so he must have been more interested in photosynthesis, but he did not neglect Donner.

VM: I remember that when I first came in 1956 it was very noticeable how much time he spent in ORL and how close he got to the people there. He would sit down with people at their desks and want to see the raw data and talk about it. Did he do that in Donner too?

DL: Not that much.

VM: So he did some?

DL: Yes. He showed up often enough that people would say "Oh, my God. I was supposed to have this done and there he is! Asking me what I've done and I haven't done it yet."

VM: Where were the secretaries?

DL: They were also in Donner, in a room just down the hall. We had about four laboratory rooms, all in the same group together, and at the end of that hall was the

administrative of the lab. which was Bert Tolbert's office and Marilyn Taylor, and, later on, about 1960, we had yet a second secretary. (Marilyn shouldn't be called a secretary, she's an administrative assistant and, of course, a damn good one.) Those two or three were in this administrative office in the same hall as the research for the group went on in Donner.

VM: When we first arrived there was a secretary called Dea Lee Harrison.

DL: Oh, there was one in ORL.

VM: I couldn't remember — I thought she was in ORL.

DL: You're quite right.

VM: She was a sort of local secretary, was she, for the people to work with in ORL?

DL: Yes. The budget was kept in Donner, the most important documents of all were in Donner. The personnel files were in Donner.

VM: So the whole administrative centre of the group...

DL: ...was in Donner right up to the time of the occupancy of the Round House.

VM: Was Bert (*Tolbert*) the chief administrator in the early days?

DL: Yes, he was.

VM: And then you took over from him later?

DL: I took over that job when Bert left.

VM: Was there actually a lot of administration because those of us who worked in the scientific end were really not very conscious of it, because we didn't do it? There was no grant getting, it all seemed to resolve itself around Thursday lunchtime meetings.

DL: I would say that I spent for many years maybe a third of my time on administration and two-thirds on research. I think that is about what it was for Bert, but you'll have to ask him.

VM: The funding for the group in those early days came overwhelmingly from The Hill, presumably, from the Radiation Lab.?

DL: That was a way point: it came from the AEC.

VM: But through The Hill?

DL: Yes, through The Hill.

VM: The group was answerable to people on The Hill?

DL: It's curious to reflect back on those days and think how it is now. Once a year we had to write up our research proposals and nobody suspected that some of it, or any of it, wouldn't be funded. It all got funded, year after year. The AEC staff in Washington regarded their primary job as to explain to the Congress why this was important work

and should be supported. Nobody in Washington ever thought of saying "oh you shouldn't do it that way, you should do it this way". By and large, the people on the AEC staff in Washington were not so gifted at research work as they were at administration. Now, of course, it's very hard to fight for your grants, very difficult. Of course, the names of Lawrence and Calvin had a big part of this. Anything those two men wanted, I wouldn't say that the AEC necessarily got it for them, but they went to bat immediately without any question.

VM: Was there much debate about the size of the annual budget or was it something that you put a figure forward and they accepted it?

DL: It was based very much on recent past experience. We knew that per person of a given category we needed approximately so much money and we saw last year that it had taken x-dollars to run the lab. and buy the equipment. So, for this year, let's make it x +25%. It was informal as that.

VM: Year after year, x plus 25%?

DL: Yes — no, I don't say it was every year. The one year we might say that we have two new full-time PhDs and we had better make it 30%. Or, we didn't hire anybody new this year, but all we need is for inflation, so add another 5% and send it through.

VM: Out of curiosity, when did that begin to change?

DL: I would say it began to change about 1965-67. I guess the reason I say that...I remember that Melvin spent a year at Oxford in '69.

VM: No, 1967-68; we were there the same year.

DL: You must know that. It was about the time he wrote his book "Chemical Evolution", which I should mention is another major research effort that went on in the Donner Laboratory simultaneously with the photosynthesis in the old building. Now, I've forgotten what I was going to say.

VM: About the change in the budgetary mood.

DL: Oh, yes. I remember that when Melvin came back from his year at Oxford, I don't know how the subject came up, but he mentioned some press conference that he had and some British newspaper man said "Well, I suppose to fellow scientists this business on photosynthesis is interesting to you guys, but why should I want to have my tax moneys going to you having a pleasant time in the laboratory/" I think Melvin's reaction to this was a bit of a surprise because until that moment, at least, science was so much looked up to by the public on both sides of the Atlantic, particularly in the United States, that they got their budgets with the greatest of ease., Now, suddenly, here's a voice saying "Well, is the public really getting its money's worth for all this work that you are doing?" Then, after that, we began to hear that from American voices in Congress and elsewhere. So, for me, the awareness of this new reaction toward science (*began in*) the late sixties.

VM: Interesting to think how differently the group might have developed had that mood been prevalent much earlier.

DL: If we had had really to fight for our budget, it would have been a devil of a lot harder to get that budget for photosynthesis (*that we did up until the mid-sixties*), let alone

for organic reaction mechanisms. Radiation chemistry might not have been so bad because that was tied into radiobiology which was the accumulation of knowledge that might help us survive when the bomb goes off. Then you need to know what's happening to all these metabolic compounds. That might not have been stopped. But photosynthesis would have been harder, I'm almost certain.

VM: I think one of the great benefits of photosynthesis research in those early years was that there were the resources, in effect, one way and another, to chase all the leads that came up. One didn't have to pick and choose and the horizon, I remember this from the photosynthesis point of view and no doubt it was in parallel on the isotopic work, any good idea somehow would be followed. Maybe people couldn't do two things at once, so they would have to make a personal decision about what to do. If it was good, the resources would be made available somehow and you didn't have agonising appraisals about which way to do.

DL: I don't know why it has occurred to me, but I would like to mention a German postdoc. that we had here many years ago, I have forgotten who it was at the moment, but I remember very well his praise of the Berkeley Campus. He said if you have a question about **anything** in biology, physics, chemistry, you name it, someone would say go talk to professor (*so and so*), he's an expert. There's an expert on this campus on every subject that he could think of. I hadn't thought of it that way before. I knew that Calvin was a big expert but the rest of the faculty, so what? The rest of the faculty had a lot of very talented people.

VM: It's a big institution and there are a lot of people so that there are enormous numbers of specialities around this campus, more than you would find in many others. It has, for a long time been a big campus, so people felt like that.

Coming back to Donner, in particular, by the time the group recombined in the round building in '63, there was already a fair amount of biological work in Donner.

DL: Are you talking about in the Calvin group?

VM: In the Calvin group, yes, with Martha Kirk and Ann Hughes, in particular, and I think Karl Lonberg started there.

DL: And there was Ed Bennett's brain biochemistry, which I am sure Ed will talk about.

VM: How did that happen, that that sort of work arose in Donner as yet another activity.

DL: My impression was that again it was Melvin Calvin reading all sorts of scientific literature, and he read a paper by a guy back at the University of Michigan who claimed to have trained worms to grow in either dark passages or light passages, whatever you train them to do. Then when the worms had been trained to prefer a light passage rather than a dark passage, you ground them up and fed them to untrained worms.

SM: That was planaria, wasn't it?

DL: That was?

SM: Planaria.

DL: Planaria, you're right. What was the name of the man who...?

VM: The man's name, I think, was McConnell; can't remember his first name. There was another guy called Allan Jacobson who was his associate who actually came here and spent some time in the lab., together with a man named Bill something from Duke (*Bill Byrne*). There was a lot of hassle and they could never repeat it (*their experiment*), do you remember? Not here, anyway.

DL: I remember when this McConnell gave a seminar here in Berkeley. I resented it as he was talking about: "Here, you spend multimillions of dollars on these huge pieces of equipment and my research", he said, "this flask only costs 15 cents" and the worms were free!. He was emphasising how he was doing this great research, at least as important, as the work up on The Hill, or any place else on the Berkeley Campus, for a budget of nothing.

VM: But I remember that long before then, must be by the mid-fifties, people were working on isotopes in animals. Wasn't Ann Hughes working on deuterium effects in mice, for example?

DL: Yes, she was indeed.

VM: Was that a deliberate decision in order to expand the isotope work into biology or did it just arise one day?

DL: I think it was an outgrowth— we did a lot of work with carbon-14 and somebody seeing that there were opportunities with tritium and with phosphorus-32 in biological work. As I recall, it sort of flowed naturally out of that.

VM: We'll be talking to Ann so I'll ask her how she got moving on that.

DL: I'm sorry that Martha isn't still here to be interviewed.

VM: That's right. For the record, that's Martha Kirk who must had died 10, 15 years ago.

DL: It's more than a decade now: it's 12, 14 years ago.

VM: Can we talk a bit about the social scene in the lab. in those early days? By the time I came, the party season was very well established. Was it always like that?

DL: Yes. I can remember a Christmas party when there were only half a dozen of us around the Christmas tree. In the next year or two we started exchanging presents, as long as the presents did not cost over twenty-five cents. Dinners at each others homes were quite frequent. There were very few exceptions by our feeling (by "our" I mean the group in general) that their co-workers were interesting and pleasant people to be with. The exceptions, I can only think of a couple and I will not say who they were, were so kind of gross that it was just normal whatever.

VM: We remember that part of the social thing was the fact that the lab was populated almost 24 hours a day. There were people around; I guess they were graduate students rather than older people with family commitments, but that was always the case, was it?

DL: Yes. It is not the case, now, for several reasons. One is, I guess, the principle one is the safety factor. This reminds me, I want to digress just briefly.

VM: Please, digress as much as you like.

DL: This is a safety story out of the Donner Laboratory. Whoever was in charge of safety up on The Hill used to give plaques, framed glass-enclosed certification, that the laboratory had gone for the last 24, 36 months without a single reportable accident. One of those plaques was put up on the wall of the laboratory and Toni Phipps, who in those days did the laboratory glass cleaning — later on she was the stockroom manager — was reaching to get something from a shelf and this plaque fell down and missed her head by a matter of inches. We all congratulated Toni on her near-miss. We got to thinking about this and Toni also was saying, she said she wished she had been hit on the head by this as it would have been a wonderful safety report to have put in — here we had an accident because we had been issued a safe-work plaque. That's the story.

VM: Were there really no accidents at all, apart from your story of the chromic acid coming through the ceiling? No serious mishaps at all that you recall throughout the history of the group?

DL: I remember the most serious accident that I remember was that one of our graduate students; he worked on chemical evolution with me and I should remember his name but I don't at the moment. He was doing a synthesis at the hood nearest my office and suddenly there was a bang, an explosion. This man looked around, and his glasses were encased with chemicals which had blown into his face; he was wearing safety glasses.

I am reminded of another story from the Donner Laboratory, with respect to Marilyn Taylor, who, as you all know, was the group secretary and/or administrative assistant for an incredible number of years and recently, incidentally, was awarded a Berkeley Campus prize or award for her tremendously effective and long service.

VM: Can we interrupt you there? We talked to her and she said nothing about this prize. What was the prize?

DL: This was given to Marilyn— I guess it was just a year ago at commencement time in the Chemistry Department. It's a very high honour. The highest honour the campus gives to the staff, the highest staff award. "To Marilyn Taylor for...."; I never did see the thing, so I can't give you the direct wording. For very long, very devoted and very effective service to the Berkeley Campus.

VM: Very appropriate. I'm very glad; that's very good.

SM: She's a living archive.

DL: I have not been able to remember names which she is going to come up with like that.

VM: I interrupted you; you were about to tell a story.

(*Brief technical discussion about the recording.*)

DL: This story involves Marilyn Taylor. One day she was in the Donner office; there were three of us also in there at the time. That was important because we had to say to each other: "Are you sure that she said it, the way I heard it" And everybody agrees it was right.

She was involved in getting a book that Professor Calvin wanted. She wanted to "borrow" that book. So she dialled the library number and when the phone was picked up at the other end we distinctly heard Marilyn say "Is this the University bar room service?" (*Laughter*). We all cracked up. Marilyn had to put down the phone and dial again later because there was so much laughter in the office. End of story.

VM: I noticed just now, for the first time since we began talking, or perhaps for the first time, that you refer to *Professor* Calvin. It reminds me that in this entirely informal group the only bit of formality was the way we all addressed him. It was not until very late in the day that any of the scientists called him by his first name.

DL: I think that was generally the situation. Here's John Lawrence, who was the director of the division in the Donner Laboratory, I think everybody addressed him as Dr. or Professor Lawrence. The reason I bring up this name is that even though he had an extremely famous brother (Ernest Lawrence), John Lawrence was not that famous and still I think those were the titles that were used. After all, the graduate students more or less had to do that.

VM: Well they did. But the staff members who, after all, had been working with him for many years, continued to do that. I remember talking to Mel Klein about this in the sixties. Calvin's wife would always refer to him as Melvin.

DL: Yes.

VM: Always — maybe not to the graduate students, but to us, certainly. And yet we never called him Melvin. One day Mel and I decided that we would. From then on, we called him Melvin. He never batted an eyelid. It wasn't as if he noticed!! Maybe he never had noticed!!

DL: I don't know when I started saying Melvin, but it was some time more or less way back. But I remember Joel Hildebrand, who we got to know very late in his life, an extremely distinguished chemist, I remember his saying to me one day "Oh, call me Joel", like that; he was almost annoyed to be called Dr. Hildebrand.

VM: Did you call him Joel?

DL: Yes, from then on I never said anything else. He obviously meant it. You and I have had this same kind of a problem. A graduate student who would say "Dr. Lemmon" for a while, and I would say "Oh, call me Dick". Some of the graduate students, at least, were reluctant to abandon the formality, thinking it wasn't quite proper.

SM: A lot would depend where they came from because formality...

DL: The Europeans are far more formal than the Americans.

SM: Or they were.

VM: I don't want to mention names at this point, but in the course of our stay here there were a couple of German postdocs. and, although we were all on first name bases, the Germans always called one another "Herr Doktor", or at least "Doktor", and we raised an eyebrow at this. We said to the one with whom we were more friendly: "Why do you call your German colleague in this way?" He said, "he's older than I am and I can't refer to him in a familiar way unless he does it first to me". We fortunately don't suffer from that.

DL: I remember our Swiss friends, the Schmids (*Hans and Kathi*), once got into quite an argument in front of Marguerite (*Dick Lemmon's wife*) and me about which of the two of them first used "du", the informal form. Particularly, Kathi was very annoyed at her husband who said that you (*Kathi*) first said "du" to me, and she swore up and down that she did not. The same man, Hans Schmid, a professor of chemistry at Zürich, he like all able bodied Swiss men had to serve two weeks, three weeks, something like that, in the army every year. When they went on their military manoeuvres, Hans said here one day along came a graduate student of mine who outranked me militarily. The poor graduate student just didn't know what to do. If it had been the two of us by ourselves no doubt he would have said "Professor Schmid", but with other military colleagues around him, he was supposed to use an informal form — you do this or you do that. The Europeans can really have a problem and it is exacerbated. I remember the story about the young boy who comes to work as a minor-grade technician, you say "du" to him, informal; then a couple of years goes by and he becomes a graduate student, and to a graduate student you say "Sie"; then later on, when he becomes a close research colleague, your are back to "du" again. There's always a little uneasiness the first person who starts the new title.

Are we recording all this?

VM: Of course.

DL: Nothing to do with the lab.

SM: What it pertains to is the lack of at a personal level in the Calvin group. This is how it started because Melvin was certainly not somebody who stood on ceremony.

DL: No he didn't. I remember one time at a seminar (I don't know if you were there). It was being given by a young lady, I think her name was Doris Chin, an organic chemist (*Editor*: *Actually it was Peggy Kwong*) working on some problem in organic chemistry. Anyway, Melvin kept interrupting her as she would say something and, after one of these interruptions ,she suddenly looked right at him and said "Professor Calvin, will you let me finish what I am trying to say and then you can ask questions". He said, "Oh, all right". We were all surprised that he took a direct reproach very well. There was no sign in subsequent weeks or months that he was angry at her for saying, in effect, "shut up". It wasn't quite that blunt but it was close. She pointed her finger at him and said "Professor Calvin, let me finish".

VM: Was he always like that? Was he always attacking the seminarist immediately?

DL: Yes, absolutely. This wasn't supposed to be a formal occasion where you wait until the speaker was all though and then ask questions. We all understood that we could interrupt the speaker. I remember somebody, perhaps it was Melvin himself, saying that "when you get up to talk about your research, pretend that there's no audience but an old friend of yours who graduated in chemistry the same year you did, and he came in the lab. and said Hey, John, what are you doing?"

VM: People used to get very worked up, didn't they, about the prospect of having to give a Friday morning seminar, especially the younger members of the group.

DL: Yes and that's why we had to abandon the original system where we just gathered, and Melvin would look around, and he would say, "Hey, Vivian, tell us what you have been doing recently". He had no more notice than that. The young people kept

saying: "Yes, I can tell you, but I need to have my paper chromatograms; let me run upstairs and get them". So we would sit there for three or four minutes while somebody ran upstairs to get their data. Because of that awkwardness it was agreed that we would give the speaker notice a week in advance or a day in advance. It got to be more and more that way that the big lead time had to be given.

VM: When I first came in '56 people were getting a day's notice. The seminarist was decided at the Thursday lunch (*by the senior staff*).

DL: OK. Then it went to longer.

VM: Then it went longer because people were going into purdah as soon as somebody felt the finger pointing, they immediately stopped doing anything except crash (*preparing for the seminar*). They often stayed up all night getting this stuff done and they were all done in the morning.

DL: They couldn't have had very good command of what they were doing if they had to stay up all night. Again, as somebody said, if an old friend of yours walks into the lab. and says "Hey, what's your research? You aren't gong to say "Well, come back in a week and I'll tell you". You know what you're doing, for God sake.

VM: In practice, Melvin wanted a little more detail than this.

DL: He wanted a little more detail, of course; that was the sticking point.

VM: I wonder if we can close this session by talking a bit about the way in which the new building came to be there, the Round House, and what it meant for all of you to move over. I guess the whole thing was precipitated by the approaching demolition of ORL when they built the chemistry building.

DL: Indeed it was. We sent in applications (*for building money*) to the National Science Foundation, the AEC, of course, and there was one other, might have been the — I think it was NIH. There were three granting institutions combined in this which meant we had goings over by committees from all three of them.

One of the things I remember about this planning for the new building was Calvin's ideas of this big laboratory where people could see what other people are doing and all that sort of thing. When we first drew a Round House with half of that circle the open lab. and the other half with the things that had to be enclosed (like isotopic work, various instruments, the darkened room, whatever, administrative offices) — when we first put this in front of the University architect, Louis DeMonte was his name, he said like this: "Don't be ridiculous — a **round** building? Do you know what you have to do to build a round building? You run a pipe a couple of yards and you have to make a bend or an angle of 15° and then another angle?" It's impossible!" Well, all of us on the committee planning the building, Melvin, of course, was in charge things, we all said that we've got to work in this building and even if it costs a little more we want it done this way, a round building. So, finally, after a second or third meeting, this architect finally threw up his hands and let us go ahead on that basis. But it was quite a fight. Even though there is at least one partly-round building on the campus, one of the agricultural buildings (*Wellman Hall*) down at the other end of the campus is that way. What I particularly remember was the horror on the face of the University architect when that plan was first presented to him.

VM: What was the relation between him and Michael Goodman? Was he the actual architect?

DL: Michael Goodman was **the** architect for the building. Above him was Louis DeMonte who was the campus-wide architect. Anybody who was planning a building had to report to DeMonte and had to satisfy him. The Regents had given him the charge to have some kind of coherence on the campus. It was such a charge that never worked, obviously!

VM: I remember at the time of this planning that there were various ideas. I think it was Al who suggested at one point a semi-circle on a block and I think Michael Goodman didn't like that.

DL: That's right; it wasn't a round building at first, it was a semicircle on a block. That was even worse from DeMonte's standpoint than a totally round building.

VM: One of the problems that I seem to remember, but I may have this wrong, was the difficulty of sealing the semicircle to the block. I don't see why it should have been a difficulty but I think it was.

DL: I don't remember DeMonte bringing that up, but he may well have. It sounds reasonable.

VM: At the time, the thinking was, as I recall, that the big labs., as they finally evolved, would have this spoke-like structure with the work benches radiating at the edges and people would turn away from their benches, or whatever they did, and would face the middle. And the middle would be the discussion area where people would congregate.

DL: There was a big white space: you could put down your paper chromatograms.

VM: The big white table and the paper chromatograms was obviously a very strong focal point in the thinking of people. In Donner, you didn't quite have a big white table equivalent, did you?

DL: In Donner? No, we didn't; we didn't at all. Nothing like that.

VM: Was there a meeting room for you?

DL: We had the library at eight o'clock in the mornings — the library opened for business at nine. But that was all in Donner.

VM: It was not so cohesive, was it, as the (*ORL space*)?

DL: No. It was much better, of course, when we moved into the Round House.

VM: As far as I remember, we all contributed variously to the design of the building and the architects and the construction people actually did very well for us.

DL: Yes, I think they did. When we finally got over this major sticking point about the overall shape of the building. One of the problems that I think DeMonte had pointed out was, I guess, (*indecipherable*) the tile roof: the tiles had to be somehow wider at the bottom than at the top, they had to slope. If you haven't looked at the building with this in mind, you might do it some day and see how it was something of a problem to do that. It's the sort of thing that a non-architect would never think of.

VM: So did they have to have specially made tiles for the roof?

DL: I think so; I think they were specially built.

VM: Have they got some spares?

SM: Yes, they would have to be fan-shaped.

VM: That's right. What's your view of the success of the building as it was built? Do you think it fulfilled the hopes of the people who built it?

DL: Yes, I do. I think it was certainly better than what followed it because when George Pimentel became the director and saw all this space, this wide-open space, he started putting little cubby-holes (desk with a partition around it) (*everywhere*), going back to the idea of everybody in his little niche and people not interacting so much as they inevitably did in those big labs. I think the building was successful. We didn't expect that it would lead to six other Nobel Prizes in addition to Melvin's but I think it worked well.

VM: I would agree. I think that bearing in mind that you could never recreate ORL because it wasn't just the space, it was also the time that was characteristic of ORL, it was the place where the early group lived, and the group wasn't early any more in 1963. The whole character was beginning to change.

DL: I remember a specific example of that system being very good and that involves Karl Erismann from Bern, Switzerland, a professor of agricultural chemistry, something like that, a biologically-related chemist. He was the most repressed, introverted person who ever joined the Calvin Lab., the most introverted one. It was so hard to get a word out of that man. Somebody from the third floor, perhaps it was Al Bassham, got Karl to come down to a cup of coffee occasionally and people would ask him questions about Switzerland and about his research, etc. We visited Karl a year and half ago in Switzerland, in the Engadine where they were on vacation. Karl is not an inhibited guy any more, he's a completely different individual from this very repressed, very formal Swiss professor who joined our group many, many years ago. I am sure there were cases not so dramatic as with Erismann but I would cite that as evidence of that we brought him out of his shell, which made him a better scientist. He was brought out of his shell in a way that he could now more easily go to somebody and say "Hey, John, I know you're working with this infrared spectrometer; would you mind helping me, I have a little problem on it". Before, he wouldn't have thought of doing that.

VM: I think that was true, certainly at that period, for all the Europeans because all of them, even the English who are perhaps less uptight than others...

DL: ...but none of them so dramatically as Karl Erismann, in my view.

VM: The question that I have for you is: whereas the Europeans (and I presume the Japanese) found it a remarkable place in terms of social ease and interaction, what about incoming Americans: was it a novelty for them or was all American science like that?

DL: I would say not nearly so much. The wide open spaces, I can remember at least one American objecting to it for what I thought were very valid reasons. He said, here I'm

doing something that's very touchy, and I have to have the right number of micrometers, or something, and somebody comes along and says "Hey, how did you enjoy your hike last weekend?" He would be interrupted at just the wrong time. I remember suggesting to him, and he did follow this, put one of these (*lab.*) stools out by the sink, where your passageway is, that says "Please don't bother me until noon time", or whatever. I think that at least in part worked. That's the only objection I can remember to that system (*i.e., the open laboratories*).

VM: Clearly, there are pros and cons in any systems.

SM: When you came in, Dick, you quoted us from a magazine you were looking at where somebody had written about the kind of atmosphere, the kind of ethos in a new building which you thought that this was the first time that things had been expressed in this way. Do tell us, again, what it was that you told us then.

DL: The article in C&E News about a new laboratory building, I think it's called the Beckman Chemical Sciences Building, at UCSD (La Jolla). In this new building, somebody was praising its architecture in having wide open spaces which would provide interaction between scientists of different disciplines so they could learn from each other what each other was doing. To my own recollection, Calvin was the first one who had that idea. Even Calvin, I can't give him too much credit, because the Old Radiation Laboratory, which the group moved into, I described, and I think not unfairly, as looking like a cow barn. It was a big wide open spaces and the laboratory benches were put in there and, I think, maybe it was only after the fact that Calvin decided "Gee; this wasn't such a bad idea after all, to have the lab. benches in wide open spaces".

VM: I think all of us who lived in that building recognised the value of that sort of structure, fortuitous as it happened with ORL.

DL: If you wanted some tubing to go through the wall into the next room, you just drilled yourself a hole. You couldn't possibly do that in a normal building.

VM: As far as the name of that building, the first name of that thing, Chemical Biodynamics...

DL: Well, the "Bio-Organic Group" was the first name.

VM: How did that happen?

DL: That was the name that Melvin picked. Bio-Organic — he wanted to emphasise the applications of organic chemistry in particular to biological problems.

VM: When the lab. itself, the round building, was called the Laboratory of Chemical Biodynamics.

DL: That happened in part because the completion of that building our group got cohesive and put back together again in one place. Ernest Lawrence wanted to designate us, now, as a division of the Lawrence Laboratory. Am I right? I think it was. Ernest Lawrence died in 1958 and the (*Calvin*) laboratory was not yet under construction. It was under (*Dr. Edwin*) McMillan that the formal division came about. We were supposed to call ourselves a division so we picked the (*name*) Chemical Biodynamics, Melvin again picked it. Why we were not called the Bio-Organic Division, I don't

know. Somehow Calvin thought Chemical Biodynamics sounded more like photosynthesis, which was the main thrust of the lab.

VM: He was interested in the dynamic view of biology. So that's fair enough.

DL: I guess it was a superior name and that was the new division that got that name.

VM: Soon after we moved in there I had occasion to call someone and ask them to send me something and was giving them the address over the phone, and gave them this word, this "Laboratory of Chemical Biodynamics". I heard a sort of gasp at the other end and they said "You have forgotten the 'astro'". They thought we'd got the chemical and the bio and the physics (as represented by the dynamics) but we left out the astronomy. One last question, because I know that you have to leave soon. When ORL was going to be demolished, how was the space found in Life Sciences to accommodate the (*ORL*) people?

DL: Vivian, this is a question that you are going to have to ask Al Bassham. However, there is a campus-wide faculty/staff committee that was charged with this business of finding space if a new department was formed; there's a cancer research laboratory now at that end of the campus. It was obviously a very legitimate request on the part of Calvin to be provided with some suitable temporary space until the new building was built. That was the "suitable" space that they found.

VM: That must have reacted very badly on the relationship between that (*the ORL*) group and (*the*) Donner (*people*) because they were now much further apart.

DL: There was still going back and forth but I have no doubt it was less intense than when the other group was in the Old Radiation Lab.

VM: And Calvin had his little electric cart, you remember.

DL: I would have thought it was a good idea — Melvin had already had his severe heart attack — should have a little electric cart (*to go between his office in Old Chemistry and the Life Sciences Building*). It turned out that he didn't use it very much and just quit using it which must have meant that he got down to LSB less frequently than he used to when they were in the Old Radiation Lab.

VM: There was a very marked difference. The whole character (*of the group*) changed and it was only partly recovered when we went back into the Round House.,

DL: It sort of mystified me because I thought it was fun to ride in that little electric cart. I don't know what Melvin objected to.

SM: I know you're in a hurry and perhaps the answer will need to come some other time. But we are talking about the interaction of personalities in the group, particularly that of Melvin, of course, and how his personality changed the ethos — or originated it. Since he has not been someone who's very easy at relationships on a personal level, as opposed to the professional personal level, how much influence do you think that Gen might have had in the cohesion of the group socially?

DL: I think she was a very important effect on Melvin and on the group in general. I have heard it said by more than one person that Melvin was extremely lucky, both in his wife and his secretary. I never heard anybody say that that wasn't the case. They were both just magnificent supporters of Melvin's work. I don't suggest that he would not

have gotten the Nobel Prize without these ladies, but certainly they contributed a great deal to his professional successes. With respect to social things, like having dinner parties, without any doubt it was Gen that was always proposing these things and was sort of the life of the dinner parties at the Calvin's home while Melvin tended to sort of drift off in the corner by himself.

VM: Melvin on these occasions would go so far and then something would click and he would get fed up with the occasion and drift off.

DL: I often thought that I think Melvin would have remained a better director (*of the lab.)* if he had never gotten the Nobel Prize. Because he became somewhat stiffer, more formal and harder to communicate with, at least it seemed so to me, after that Nobel Prize than before. He tended to annoy people by seeming to be an expert on any subject that came up, however remote from science. If something came up about who's the better man, Dole or Clinton, if somebody said "oh, Dole", Melvin, if he disagreed, would just jump on this person very hard. He wasn't any more an expert on other matters than the rest of us, granting his tremendous superiority as a scientist. I think in that respect the Nobel Prize was not good for his group, however good it was for him.

SM: Do you think this was because he saw himself differently, or because, since he had to be absent a lot after that...

DL: I think he saw himself differently. He was in a rare, very select group of people now, whereas before he was a professor of chemistry. There are a few thousands of them but Nobel Laureates aren't so many.

VM: He was also mixing with a different crowd of people. He wasn't mixing almost entirely with scientists as he had been previously.

DL: That's quite right. He became a member of the Bohemian Club, and all that.

VM: And the President's Science Advisory Committee and he had a role...

DL: ...in all these assignments in Washington.

SM: He took himself seriously in the sense that he had to perform.

DL: It's just an impression. I don't know how true that really is. I remember somebody over in the Medical Center (*in San Francisco*), about a year or two, let's say '62-'63, a year or two after Calvin got his award and I don't know that he had Calvin in mind; he was a professor over there and I don't remember his name at the moment. He wrote an article in *Science* about how he felt the Nobel Prize was really hurting science in general, not helping it. There was this fierce, intense rivalry and people tried to politick, how about sending a letter on my behalf and all this kind of thing that was going on. After the person gained his award, a new building was built. Calvin's came before the award so it doesn't apply to him. But a new building is built and the person is a new director of this grand new research institute and he tended to disappear into his fancy office and was not so available any more to his students and research collaborators as he used to be.

VM: Not only was he (*Calvin*) not so available, he was almost unavailable. In the latter days, he was rarely in the working part of the lab. at all. If he was there, I have to say you usually had to look for the television cameras that were behind him.

DL: You might agree with me that maybe it was not for the best for his lab. and his group that he got a Nobel Prize.

VM: It's difficult to come to a decision either way. It's the passage of time, people mature, that's what happens to them. The group would have developed in some way or other, it couldn't have remained the way it had been in the glory days of the fifties. It must have changed in some way and this was one of them, one of the possibles.

DL: By the way, we must all remember, as you two I'm sure know, that the Round House, now the Calvin Laboratory, was not a result of the Nobel Prize. It was all funded and done and in the works when the announcement came from Stockholm.

VM: While we are talking about this new building, and I don't know whether I may have mentioned this in other interviews that we have already done, that Al Bassham had a glassed-in office, you may remember in ORL, in one corner of one of the labs., one of the few partitions in the building I'm bound to say. On the wall of this room he had a little story about the three stages in the life of a great man, the anonymous great man, not any particular one. The first stage where he does his great work under poor conditions (pink string and sealing wax); the second stage is where he is designing his new building; and the third stage is when he is showing the visitors around the new building. That turned out to be not totally untrue in this case.

So anyway, I wonder perhaps we ought not to leave it at this point.

DL: Yes; I would like to leave about now.

VM: Perhaps we can pick it up again when we have talked to some more people.

DL: You've exhausted me, Vivian.

VM: No, no. There's a lot more where that came from.

SM: It occurred to me, that when Ann (*Hughes*) mentioned that we were going to have dinner together at her house, with Ed and Millie Bennett, she was under the impression that we were going to do this sort of thing then. Then she learned that it would be a one-on-one interview. I am wondering whether in addition to the one-on-one interview, one might not see what sort of patterns, as it were.

VM: Let's try it. The worst that can happen is that it becomes unintelligible and we can't use it.

DL: The most important interviewee will be Marilyn Taylor. She knows everything about everything.

VM: We've already talked to Marilyn extensively already and I'm sure we'll come back to her as well. For the moment, let's call this a day then until the next time.

DL: OK.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Richard M. Lemmon

Date of birth 11/24/19 Birthplace California

Father's full name Dal M. Lemmon

Occupation Federal judge Birthplace Kansas

Mother's full name May Alice Lemmon

Occupation Housewife Birthplace California

Your spouse Marguerite H. Lemmon

Occupation Homemaker & Director Birthplace California

Your children Janet, Marilyn, Brian

Where did you grow up? Kensington, CA

Present community " "

Education B.A. Stanford Univ. 1941

Ph.D. Univ. of Calif., Berkeley, 1949

Occupation(s) Research chemist

Areas of expertise Organic chemistry

Radiation "

Chemical evolution

Other interests or activities Conservation

Private flying

Organizations in which you are active Regional parks association

Yosemite association

Chapter 7

JAMES ALAN (AL) BASSHAM

Berkeley, California

May 25th, 1996

VM = Vivian Moses; AB = Al Bassham; SM = Sheila Moses

VM: This is a discussion with Al Bassham in Berkeley on the 25th of May, 1996.

Al, you were saying just now that Sam Ruben was instrumental in your getting into this business *(i.e. photosynthesis)* in the first place.

AB: That's right; it's not of great import to the discovery, but it's important to me because it really influenced me to be Melvin's graduate student and work on the path of carbon in photosynthesis. When I was an undergraduate at Berkeley, a chemistry major in 1940, perhaps it was the spring of '41, the first year, as you may know, the chemistry professors, especially the younger members of the department, used to take laboratory sections and be the laboratory instructor. I had the good fortune to have Sam Ruben as one of mine. One day, instead of talking about the usual protocols for laboratory experiments, he said I would like to tell you a little bit about my research because it's kind of interesting. He said recently we've gotten access to carbon-14, a radioactive tracer, as a result of atomic experimentation going on here (*at Berkeley*), Oak Ridge and other places and we are now using it as a way of tracing what happens to carbon, whether it be in biological reactions or chemical reactions.

My particular project is to follow carbon dioxide during photosynthesis, where carbon dioxide is taken up to make sugar. He went on to talk a little bit about how they used this radioactive isotope and how they were analysing the products and so forth, and hoped to find out the pathway. That was all there was to that. Then the war intervened and I went into the Navy for three years. I came back as a graduate student at the University of California. By the way, they didn't want to accept me as a graduate student because they have a policy against taking people for graduate work who have done their undergraduate degree (*at Berkeley*). But they made an exception in my case, perhaps because I was a veteran and pleaded that it would be inconvenient for me to go elsewhere. So, I came back in for course work only and after one semester, why the dean called me in and asked me if I would like to work for a PhD, because he had heard something good (*about me*).

VM: You already had your bachelor's degree?

AB: I got my bachelor's degree during the war. I didn't quite have enough units when I left Berkeley but I finished all my requirements. While in the course of my Navy duty I got a chance to go to radar school, pre-radar school was at Harvard, this was following my getting my commission at Columbia (*University*), and the radar school at MIT. I later learned after a bit that you could apply for credits for the work that you did at Harvard done by the graduate.. the electric engineering school. So I got these credits and the University of California notified me that I was awarded a degree while I was out at sea in the Pacific. So I came back and, as I just said, re-enrolled as a graduate student. So anyway, I was allowed to become a PhD candidate at the end of the first semester and I wanted to go into some phase of organic chemistry. I got a list of professors to go to and Calvin was on the list, the first one on the list. So, he was the first one I went to see and he provided me with a list of his research projects. The first one he mentioned was the work with carbon-14, both with photosynthesis and also with some organic reaction mechanisms. And, of course, being with the big professor I listened politely to all of his research proposals. But I had already made up my mind as soon as I heard about carbon-14 in photosynthesis, because of my experience with Ruben. And so that's the one I told him I wanted to work on and that's how I got started in that project.

VM: Where did you actually meet him the first time?

AB: I met him in the old red brick building, the one that dates back to around 1900, when it was modelled after a German chemistry department.

VM: What was your impression? Had you heard of him?

AB: Frankly, I hadn't really heard of him. I had never had a course from him. He was a young professor and I had been gone in the war and I didn't know much about him. This was the first time I had actually encountered him, at this meeting.

VM: In the light of where you had been in the previous years you weren't very familiar with the university atmosphere and the way things were done.

AB: I wasn't. There were some other organic chemistry professors such as Jim Cason that I had taken courses from whom I liked very much. But I wasn't tempted to go to any of them after hearing about this possibility to work with carbon-14.

VM: What was the set-up when you made this arrangement with Calvin? He accepted you and you agreed to work on photosynthesis.

AB: You mean...

VM: Physically where were you, who was there, what did you do?

AB: Where we worked, of course, as you perhaps know, was in the Old Radiation Laboratory...

VM: From the beginning?

AB: Yes, from the beginning...the old wooden building. This was a building which, I guess, had been originally built as a temporary building for some engineering studies many years ago, but had been given to E.O. Lawrence, the inventory of the cyclotron, when he began to make himself famous with his cyclotron inventions back in the early thirties. He had had a 37-inch cyclotron there as well as a shop for doing all

kinds of machine work and a nice shop for doing glass work and a carpentry shop. These were all very important things as you will see to our group later on. They were originally put in there for E.O. Lawrence. Anyway, things was already starting to move to The Hill, to the 184-inch cyclotron and the other things that were there, but he still had something going on in our building. But space was being vacated and it was given to Professor Calvin to carry out the carbon-14 work which also, of course, was a part of the Radiation Laboratory — it was then called the University of California Radiation Laboratory, or UCRL. Initially, I think it was just a lab. on the (*west*) end (*of the building*) and an office or two. Shortly after I got there we got access to a central area where the cyclotron had been before it was moved away. We refurbished it a bit, but it was still a big open area with desks (*and benches*) around which led to a lot of the communication between the various people that worked there.

VM: Is that where the big white table was ultimately placed?

AB: And the big white table was ultimately put out in the middle of that.

VM: Who was there when you joined?

AB: Let's see if I can remember them all. Andy Benson, of course, as you know had done most of the initial experimental work and there was Sam Aronoff, a postdoc. who came from the plant physiology department, I believe, and later moved to other universities, I think, and ended up at Simon Fraser (*University*) in Canada. His role was to tell us about plants: we were all chemists, including Calvin, and most of us didn't know anything about plants at the start. There was another graduate student, named (John) Weigl who worked for a while and eventually went to Xerox. I never heard from him after that.

VM: He died about '82.

AB: Yes, I guess I knew that. Then, there was a technician — a fellow, a red-haired guy. What was his name? It may come back to me later — I can't remember right now.

VM: It wasn't Goodale was it?

AB: Oh yes, it was Tom Goodale; actually, the one I just referred to replaced Goodale. Goodale was the first technician we had but he didn't last long for one reason or another. I mentioned all the others, I think, except perhaps we had a dishwasher at the time, someone who took care of the lab. That was about it, when I first started there.

VM: Very small.

AB: Very small at the beginning. It grew after a bit. Later on other students came, that we'll be talking about later, like Wilson and Goodman and so forth.

VM: When did you actually join the group?

AB: I started as a graduate student in the fall of '46 so this would have been about January '47 I believe.

VM: Was the group in ORL already set up or did you get the sense that they had just moved in, or were they well-established in there?

AB: As I have described it, they had only a small part of the building. Even as I went in the space was getting larger because initially they just had the lab. on the (*west*) end and Andy Benson's office which was on one end of the building. I don't remember when I first moved in, I think we already were expanding into the other room and after a short time later we got space upstairs for the chromatography work and so forth. It's a little hard for me to remember precisely what we had when I moved in.

VM: What did you start working on when you got there?

AB: The first job that I was given was to chemically degrade very small amounts, and lightly labelled amounts, of succinic acid and malic acid. These were four-carbon carboxylic acids which Andy had identified as being minor products of photosynthesis with carbon-14. It's kind of an interesting story, too, in that although these turned out not to be involved in the carbon reduction cycle, later on people did discover a shuttle mechanism, called the C4 cycle, involving malic acid, not succinic acid, involving malic acid and it has some importance in certain plants. At the time, we thought it would be involved in the basic incorporation of CO_2 in all plants. So in order to find out whether the carbon label was migrating from the carboxyl group, where you would expect to find it first, in a carboxylation reaction, to the central carbon atoms, where it should be found if there's a regenerative cycle, it was necessary to degrade malic and succinic acids and determine the distribution of carbon-14. That was my first job.

VM: Did you work out the methods for that or were they already established?

AB: They had to be worked out with respect to the radioactivity in small amounts. People had chemically degraded (taken apart) acids before and there were classical methods in the German literature that I had to look up for doing degradations. But all this had to be done on a micro scale because there wasn't much radioactivity in these compounds and so you had to work with as little carrier, that is unlabeled compound, as you could get by with.

VM: This was pre-paper chromatography?

AB: This was pre-paper chromatography .

VM: How were these materials isolated?

AB: I believe they were isolated by ion exchange columns by Andy Benson.

VM: One you point you made, which I would like to ask you: you said it was the first job you were given. Now as an incoming graduate student, did you have a topic which was going to be your thesis topic which was your own and you could develop the way you liked or did you immediately join in with a group? I'm not quite clear about the...

AB: In this case, I really joined in with the group. As a graduate student I was involved in a number of publications but they were publications with a lot of other people working in the team. It wasn't, as more commonly the case, that I was given a specific topic of my own to work on. I guess you could say that my specific topic initially was the degradation of the carboxylic acid but that evolved as time went by into a broader scope of things. The title of my thesis still had...was something like "The Path of Carbon in Photosynthesis: The Carboxylic Acids".

VM: So, you had a sense of theme that went through your thesis research.

AB: Yes, it was a subdivision of the whole thing but it was a kind of a theme that I was involved in.

VM: You had your own bench...

AB: Yes, I did.

VM: ...presumably, in that room with the...?

AB: By the time I got there and got established, I had a bench in the big room. Initially, I guess, it was some space maybe in the little end lab. I don't remember exactly.

VM: The big white table, which appears so prominently in everybody's memories of the whole group, has always been referred to as the place where you look at the chromatograms. In the beginning, there weren't any chromatograms. When did the big white table come? Was it there when you got there?

AB: I don't think so. I think it did come after the chromatograms because the reason it was white...Our lab. benches were, at the time, generally lined with black Formica (and black was the colour we used) and we did develop after a year or two the need to look at the radioautographs of the paper chromatograms. At that point we needed a big white surface to set them on. That's when I think we put together a bunch of cabinets in which we had chemicals stored and then had Ralph Norman, the carpenter at the time, build us a top for it that would just fit over those cabinets and make a big white space made of Formica. (*Editor: The big white table was in existence by 1948 as it shows in photos of that year's Christmas Party.)*

VM: It never really was a table, it was always a...

AB: In was never a table at that time.

VM: The top surface, as I remember, something like: was either very hard plastic...

AB: It was white Formica, and we had experience with that, a good material unless you put alkali on it, in which case it blisters, and we used that for the table.

VM: The lab. benches, as I remember in ORL, were the ones with chemical racks, somehow closely associated.

AB: They did have, yes.

VM: And so presumably they were designed by chemists. The whole atmosphere was a chemical lab.

AB: Very much chemical, yes.

VM: So the biology...

AB: We were novices with plants. We had to learn everything about plants. People like Sam Aronoff helped us in getting algae cultures going, and so forth.

VM: It turned out, of course, very much a biochemical activity as it developed. Who was the biochemist? I guess you all became biochemists in the end.

AB: By and large the biochemical work was done by chemists who learned biochemistry, people like Rod Quayle who came and tried to isolate enzymes — did isolate enzymes, and do forth, who had never done that kind of stuff before. I am trying to remember who was more of a biochemical nature. Clint Fuller, perhaps, had a bit more biochemical background than some of the others. You'll have to check that with him; I think he did. Of course, Sam Aronoff was a biochemical plant physiologist. People like Calvin, Benson and myself and other graduate students were by and large untrained in biochemistry. Some years later, I taught, as an adjunct professor, biochemistry about five years and I always thought it was a little bit ironic that I was teaching biochemistry when I had never had a course in biochemistry (*laughter*).

VM: That's the nature of a pioneer, isn't it? You start something which other people haven't done.

When you first started, what sort of plant material was being used?

AB: I was going to say that we started with algae but that's not true. We did do experiments with leaves. What leaves did we use? — soybean we used, I think.

VM: Barley, perhaps?

AB: Barley was used? Yes, that's correct. What else? Well, eventually, after a time, a lady scientist named Vicki Lynch came in, who was also very much trained in plant work. She and others did a study of a number of different phyla of organisms, different kinds of plants and so forth, and tried to see if there was a universal pattern of carbon fixation in all of them, which there was.

VM: I was wondering how the use of the *Chlorella* and *Scenedesmus* started, since you were all chemists. It's true I can see that Sam Aronoff could have introduced you to the idea, but somebody must have made a decision: we are going to get a way from barley leaves, or whatever.

AB: I think that the algae, of course, were very appealing, for a chemist, because they could be treated almost like chemicals. They are not, of course, they are cells but you can pour suspensions of them, you can bubble gas through them, and you can do all kinds of thing with them. And, very important, you can kill them — you can stop the biochemical activity quickly. That's why they (*algae*) were used.

VM: Were you there at the beginning of the algal use?

AB: I may have been. I can't swear one way or another the actual moment they were introduced.

VM: I remember them, but this was ten years after that, as being, these large flat-bottom flasks in the shaker thing over lights. Was that the earliest form of continuous culture?

AB: That was, yes. The shakers going back and forth in the water bath. I think I may have been there from the beginning but I didn't have anything to do with setting it up. That would have been done by the people who knew more about plant physiology.

VM: Vicky Lynch, perhaps.

AB: This may even have gone back to Sam Aronoff's time. I'm not sure.

VM: Was there someone even at that time whose job it was to look after these cultures?

AB: There always was Vicky Lynch or somebody like that.

VM: The first one I remember, I think, was Pat Smith.

AB: That was much later.

VM: OK. So these algae were growing and at some stage, presumably, you began to concentrate (you collectively)...concentrate mostly on the algal tissue as a...

AB: I used them a lot in the early years of my work because they lent themselves to kinetic experiments, taking samples where you vary the length of time of photosynthesis from a second or two on up to several minutes.

VM: The "lollipop". Can you remember the beginning of the lollipop?

AB: Again, I'm not sure. I didn't invent it. My guess would be that it was someone like Andy Benson who would have invented that because he is very good at all experimental set-ups and he probably foresaw the need to make a thin vessel with a stopcock at the bottom and the top, and ways of bubbling gas through it. I probably can remember about the time it (*the first one*) was built but I couldn't put my finger on the precise date.

VM: Presumably that was a facet of using algae because it wouldn't have been much good...

AB: Oh absolutely. It (*the lollipop*) was used just for algae. In later years when we wanted to do leaf experiments we went to a lot of pains trying to make something similar to a lollipop with faces on it that you could detach and pull the leaf out quickly. It was never quite as easy as it was working with algae. With algae, as you know, you just put on a little pressure and you turn the stopcock and squirt out a sample.

VM: Who actually built the stuff? Was there a glassblower?

AB: Yes. As I said earlier on, there was a glass shop (*in ORL*); we inherited, as it were, the use of the glass shop. The shops were still used by people on The Hill. We inherited the use of the glass shop, the carpenter and his tolls and the machine shop. We could always just go in and talk to the people directly. We were in better shape than the people up on The Hill because they were right next door to us.

VM: I see; these were actually The Hill...

AB: For a while they were also used by people on The Hill but I'm sure that very soon they got their own up there. But for a time, why, they were very handy for us and that went on for several years before they eventually...

VM: So that was a major factor, perhaps, in the way that you...

AB: They were marvellous. I think that any scientist in a department would be absolutely thrilled at having the kind of access that we had to glassblowers, to machinists and to carpenters to do their work. We tried to maintain that as best we could in our budget over the years as we moved around, although, of course, we had to give up some of it;

in particular, with respect to the glassblower and the carpenter, we always have had them for a number of years afterwards in the lab.

VM: When you first joined, and there was this small group of people in ORL, presumably you had a very easy and close social relationship in the lab., you were always talking to one another.

AB: That's very true. As you know, when you there in he group later — it was even more true when we were smaller, of course, because there were fewer people to be involved, and everybody knew everyone else well — we did, I might add, to have more people, we combined our social events lots of times (birthdays and special things) with the group that was in Donner (Bert Tolbert, Dick Lemmon and those folks who were working on mechanisms of organic reactions using carbon-14 or doing animal studies as the case may be). When we needed to have enough people for a decent small party, we all got together.

VM: Did you feel very close, the two locations?

AB: Yes, we felt very close and Melvin Calvin, of course, did everything he could to maintain that cohesion. One of the other things that we might want to talk about a little bit more was the group seminars that we had regularly every week at eight o'clock in the morning, to the horror of the British delegates to our lab. (*laughter*) but it got everybody going. At these meetings, they weren't formal in the sense that nowadays you have a seminar and you have a schedule of speakers and they know months ahead that they are going to talk. The way they worked was that we all got together without any knowledge of who would speak and Melvin would point to somebody and would say "Al, you tell us about what you are doing this morning". Of course, Melvin was pretty much on top of what everybody was doing so he had an idea who might have something interesting to say. But, when he pointed to you and said your name, you had to present your talk whether you had anything to say or not.

VM: And you had to do it without props, presumably.

AB: No slides, no props of any kind. If you thought you might be called on you could bring something along, but usually you didn't, because it was in another place, another building. They were held over in the Donner Lab., for instance, and we were in another building about a block away.

VM: When you first joined the group, the seminars were already going?

AB: Yes, I think he must have initiated those meetings from the very start. Marilyn, or someone like that, could tell you more about that.

VM: In the early days, you all sat around a table did you?

AB: I'm trying to remember the configuration of the first room we were in. I don't think so. I think it was more or less of a conventional small room with seats and the speaker up at the head. I don't think we had a table. That was something we introduced later on when we had the opportunity to configure the room.

VM: Melvin, of course, sat up front!

AB: Yes, oh yes.

VM: On the right-hand side.

AB: Sort of in your face, as it were. As soon as you said something he didn't agree with, he was on top of them immediately.

VM: He has always been like that?

AB: He has always been like that. But, people who worked there didn't take that too personally because they knew he was like that. But, I must say, in later years some of our visitors were very much upset, people were used to a more formal procedure. Being jumped on as soon as they said something that he thought was foolish, was a little hard for them.

VM: Presumably, soon after you joined, the group was in an expansionist phase?

AB: I think it was. I think we were expanding more or less continuously for almost the whole time, especially the first 10 or 20 years, and that's not too surprising. Because there were very few places in the world where one could go and learn about radioactive techniques. Of course, radioactive isotopes did diffuse to various parts of the world, and some of the early work was done in other countries, but we had a very good access to both the radioactivity and to substantial financial support to carry out experiments. Those weren't the days of writing grants to NSF or NIH or anything. We made a kind of a quarterly report to the AEC, which was the controlling body, in which we outlined what we were doing, what we had found, and so forth and they more or less took the reports and sent us and money — not unlimited, but generally enough to carry on the work and maybe expand a little bit.

VM: I think those were the days when the AEC was very keen to have non-military activities.

AB: That's probably one of the reasons.

VM: So, in the course of your PhD work which took you five years, or something of that sort?

AB: I didn't take that long, actually. I started in the fall of '46 and I got my degree in '49. It took me three years, basically.

VM: Did you not have to do courses?

AB: The first semester, that I talked about, I did courses. After that, I took a few courses but not many and mostly I just devoted myself to (*my experiments*). We all had to do teaching from time to time in the sections. I was supported initially by the veterans' programme, as a W.W.II veteran. That didn't last very long before I was offered a graduate student research stipend. That replaced the veterans.

VM: By the time you had finished your graduate work in '49, how far had things gone in the path of carbon?

AB: They had gone quite a ways. I don't know when the must definitive publications came out. I always think of *Path XXI* as being a definitive publication but it wasn't the first announcement of the carbon cycle by any means. Melvin, of course, was invited to speak and gave lots of talks here and there and some of them were later transcribed in

the proceedings. I think by 1949 I think we had a pretty good handle on the cycle, even though the most definitive article didn't appear until about 1953.

VM: That meant that since the whole activity became highly dependent on paper chromatography, that must have been introduced well before '49.

AB: Oh yes, absolutely.

VM: Before you arrived, or did you witness it?

AB: No, no. As I said earlier. when I arrived, Andy Benson, aided. I guess, by Tom Goodale and other people, was isolating things with ion exchange chromatography. Bill Stepka came up to our lab. shortly thereafter (I don't know the precise date — he'll be able to tell you that) and he had been working with amino acid separation by paper chromatography, following the methods invented by Martin and Synge in England. He already knew how to separate out alanine, aspartic acid, things like that. Very soon we discovered they were indeed labelled, at least after a minute or so of photosynthesis, or even maybe 30 seconds they would begin to get labelled. Things like alanine and aspartic acid.

VM: This was known from the (*ion exchange*) column work?

AB: The column work may have revealed the presence of labelled amino acids. Andy Benson would be a better authority for that than me. In any event, we use it to separate amino acids by paper chromatography which was much quicker and easier and we discovered others which I am sure he didn't find with the column work, things like serine and so forth. Andy, I think especially, and, I guess, Melvin Calvin and others recognised the power of this two-dimensional method but it wasn't very good for separating the phosphates. When they used the solvents that one uses with amino acids, you get just a big glob of stuff down in he corner; they didn't move far enough. So Andy Benson, really, I think, was the one who experimented with — tried different solvents until he got one that would separate the sugar phosphates. He can probably tell you the date of that better than I can or we could look at some of the publications. By the way, I brought most of the *Paths (of Carbon papers)*, I have them bound up out in he car; I imagine you have them all.

VM: I would very much welcome borrowing them while I am here (*i.e. in Berkeley*).

AB: I would be happy to loan them to you.

VM: Thank you very much; that would be very helpful. When paper chromatography was first introduced, you didn't have any dedicated facilities for it?

AB: That's right.

VM: As it's a smelly activity, where did you do it?

AB: Initially, in the lab., but, as you said, it was very unpleasant. It was particularly unpleasant because some of the early solvents we tried were even worse than the ones we used later and the ones we used later were pretty bad. I remember some of the early ones involved imines like lutidene; it really was awful. Somebody (Calvin, Benson, somebody) knew that there was space in the second story of the ORL which had been used for offices for, I guess, E.O. Lawrence and those people, but they had vacated them or were about to. They made arrangements to get hold of that space. It

was renovated and changed a little bit and we put the chromatography tanks up there. It was still smelly but you didn't spend too much time up there. In retrospect, I think we should have gotten money and installed a good vapour escape system but we didn't do it.

VM: There must have been a rapid investment, then, in chromatography equipment. There were the big boxes and there were also the stainless steel trays which were designed by you — not you personally; by the group?

AB: Yes, they were; I don't know now who designed them. They contracted out with some kitchen supply people or somebody like that. The first chromatography boxes were wood, I think they even had just paraffin linings, which wasn't very satisfactory with organic solvents. Then we got the idea of putting Formica linings (*in the boxes*). We had Ralph Norman (*the carpenter*) build some of those. They gradually evolved into the kind of boxes that we used later. Over the years, I guess, we went more and more to stainless steel and things like that.

VM: You remember the early ones which had sleeves that you fitted over the rails in the tank and the papers went over those? When you wanted to take the papers out you put stainless steel clips over them and you took them (*the papers*) out and hoped they didn't tear, while you were suffocating.

AB: Yes, that's true.

VM: You remember there was a Martian helmet that someone wore?

AB: Yes, that was me. That's one of my few contributions. it wasn't a very good one. I was horrified at all these organic vapours we were breathing so I tried to get some kind of helmet with an air supply to it that people could put over their head. It was so hot and uncomfortable that unfortunately most people didn't use it. But I thought people should at least have the option of being able to work with this stuff and not breathe the fumes.

VM: At the time when chromatography came in, you must also (I am using "you" again in the collective sense here), you must have decided to go to radioautography. All of these things were new, not just for you but just generally because they were new inventions. Do you remember the introduction of radioautography?

AB: I more or less remember it but I don't know the precise time or anything like that. I guess, I'm not sure, trying to remember: I just don't know how it came about. It was introduced very shortly after the use of paper chromatography. Obviously, we had the radioisotopes to detect and we wanted to find out where they were and this was a way to do it.

VM: Then there was that underground room in the basement, the dark room where you loaded the stuff up.

AB: That was my doing. The reason it was my doing was, as I told you earlier, that I was charged with degrading these chemical compounds and finding the extremely small amounts of radioactivity that were in the middle positions of the carboxylic acids. I soon discovered that I couldn't do this by regular counting with the old-fashioned lead-shielded counters because we had a cyclotron in Crocker Laboratory, right next door, which they used to turn on at night and other times, and gave a variable background which was higher than the amount of radioactivity than I was trying to

detect. So, I searched around for a place that would be more shielded. I don't know who mentioned to me that there was an underground area. At the time, when I first went down there, it was just dirt but there was some sort of foundation or other types of cement walls which, perhaps, had supported the cyclotron or some heavy equipment, I'm not quite sure what. Anyway, we put our counter devices down there. You know what these devices were. They had these lead shields and then they had a Geiger tube inside and there was a slide that went in that you could insert your little planchette on which you had mounted your chemical with radioactivity and slide it into there and turn it on. You could set it to count for long periods of time, or long periods of background, whichever you needed. That helped some but it still was pretty hard to work down there. So eventually we got money from somewhere (I didn't get it but the people —Melvin, I suppose, did) to have some work done. They excavated a bit, put a cement room in and a stairway leading down there which made it easier to get up and down.

VM: Before too long you were using those end-window Geiger tubes with mylar windows and gas flow. Where did they come from? Who invented them?

AB: I'm not sure. You see, initially we used split mica (*for the windows*). It was quite an art, I guess. I didn't know the art but there were those who could split mica and get very thin sheets of mica to put over the Geiger tubes. As you can imagine, these were not very uniform and were easily broken and didn't work very well. I don't know who found the mylar (I would always guess that it would be Andy Benson since he was very clever at such things; but it might have been somebody else) to put over the tubes and made it possible to count with that. I don't know when that happened. I think we were stuck with the mica for quite a long period of time but eventually we went to mylar

VM: You remember that agonising business of shielding the bit you wanted to count from all the neighbouring pieces of paper with the bits of card, putting these mylar end-window tubes on that. That was an excruciating activity.

AB: Of course, that led eventually, as you know, to the invention of the "monster" by you and, I guess, (*Karl*) Lonberg-Holm so we could do it automatically.

VM: That's right and I think I mentioned last year that the relic of that (*machine*) still exists down there in the warehouse in Emeryville; I went to see that.

AB: We eventually elaborated on that. Let's see: what did we do? We eventually rigged it so we could count both C^{14} and P^{32}. I don't remember how we did that.

VM: You had two counters in sequence...

AB: Oh yes, that's it: two in sequence.

VM: ...one had a thick window.

VM: It's your version of it, your rework of it, that's down in the warehouse. By '49ish, when you'd finished your graduate work, what happened to you then? How did you make the transition from being a graduate student to being a regular staff member?

AB: A lot of the work (*on the path of carbon in photosynthesis*) was still going on and it was extremely interesting. Not surprisingly, I wasn't anxious to leave and go somewhere else. It wasn't so much that I didn't think I could find a job somewhere

else but it was such an exciting business and it wasn't all completely wrapped up yet, so Melvin Calvin offered me a postdoctoral support at the lab. we had lots of those available from the AEC. I went to work as a postdoctoral fellow. Time went by and I stayed on. I got a National Science Foundation senior postdoctoral fellowship to go to Oxford for a year in '56; that's some years afterwards. When I came back from that, I was a sort of staff scientist rather than a postdoctoral.

VM: You had been a postdoc. up to that time?

AB: I'm not sure, you know, just when the change... It might have changed even before then because that would have been a period of quite a few years. Probably I was already a staff scientist before I went to Oxford. It gradually evolved.

VM: By the early to midfifties, this was going to be your permanent career. You could see it?

AB: That's what it turned out to be; I didn't know that at the time.

VM: You were in a stable position with research (*and support*) at that time. There were lots and lots of people, of course, who came through the lab. at that time and some of the papers have a very long lists of authors.

AB: Indeed.

VM: What was the practice in those days? You and Andy and Melvin, presumably, were the main co-ordinators of the direction in which things flowed.

AB: I think, generally speaking, Melvin determined the list of authors and who should appear. I don't know what rules he went by, exactly. Sometimes he went alphabetically, which is good for me! Other times, though, he went more by who he felt had made the biggest contribution. I want to say that I thought Melvin was always very generous in attributing credit to those who had done the work but I couldn't tell you precisely how he decided. I remember on some occasions making suggestions that others ought to be included on the list — I don't want to be specific now because I don't remember specific... Sometimes we would have graduate students who would do key bits of work or technicians who would do key bits of work. On degradations, for instance, like Lorel (*Daus*) Kay did some splendid work on the sugar degradations and they obviously ought to be, I thought (and, I think, Melvin though too) included on the list of authors. I don't know whether that answers; that's about the best I can do.

VM: In the early fifties, before Andy left (Andy left in about '55, I think)...

AB: Is that right? I don't know the precise date.

VM: ...you and Andy and Melvin were really the people who were the ongoing core of the photosynthesis activity. So, presumably, the three of you, however you arranged it, were the main co-ordinators and directors of the way people were going. Each new batch of graduate students coming in, or postdocs., presumably talked to the three of you in order to work out what to do.

AB: I think Melvin mostly decided who was going to do what and then they would talk to us and we would get more specific. Sometimes his ideas would be more general. People came and talked, at that time, to Melvin and he put them in some direction or

sent them to one of us to work on some general area. Maybe we would be more specific in what they would actually do. With the graduate students, he pretty much determined more precisely what they would do. I am sure he's the one who got Alex Wilson going on doing the transient studies that he did with carbon dioxide high and low and so forth.

VM: He was always the one formally responsible for the graduate students, as a faculty member?

AB: Yes, in those days. Later on some of us had graduate students who were nominally with Melvin but he would simply assign them to us and we would give them a problem. We are talking about the early days now.

VM: In the early days, he was very closely associated with all the graduate students, much more so than later on.

AB: Yes.

VM: I also remember, and I am sure this must have been the case from the very beginning, that he was absolutely concerned on a day to day basis with every piece of information in the lab.

AB: Absolutely. The thing we always used to say was that Melvin would talk to a student or somebody and outline six months worth of work and came in the next morning and ask them what results they had gotten!! That's a bit of an exaggeration but it was kind of like that.

VM: He wanted to see every smudge on every chromatogram, every kink on every curve.

AB: Absolutely. He could make interpretations that nobody else would even think of, some of them right and some of them wrong, but he had a fertile imaginative mind for working out what something might mean.

VM: But he was always amenable to being contradicted, wasn't he? If you had a better argument, he didn't pull rank.

AB: Yes. Some of us had to eventually, sometimes we had eventually to set the record straight, as it were, because he, like all geniuses, he had lots of ideas and some of them were brilliantly correct but there were a lot of others that weren't.

VM: He was a very, very stimulating man and I think all of us recognised that. Later on, you became interested in expanding out from the original carbon cycle work in order to the implications of it. When did that begin to take shape?

AB: It kind of evolved over time but I would say after about '55 or so, '56. I began to do very elaborate kinetic experiments with the help of Martha Kirk, who was a very talented scientist herself although a technician, and we would do these careful kinetic studies from the very shortest times and we got interested in the flow of carbon out from the cycle into secondary products such as sugars, amino acids, carboxylic acids and things like that. That led inevitably into consideration of secondary pathways. Also, of course, there came along after a time the C4 pathway which we kind of missed because we didn't work that much with C4 plants. We had to investigate this and look into what was happening there. Actually, Andy and Melvin published a C4 pathway as the path of carbon in photosynthesis, as a speculation. Some of the work

that I had done with degradation showed that that wasn't right because the carbon wasn't spreading quickly enough to the centre carbons of the malic and succinic acids. Having tried it once and rejected it, we were perhaps slower than some were to embrace a new C4 pathway. It became clear, eventually, from the work of Kortchak in Hawaii and Hatch and Slack in Australia, and so forth, that there was indeed a C4 pathway. We had to take that into consideration.

VM: What influence did your year at Oxford have on what you did later? You spent the year with Krebs in '56-'57.

AB: That's true. Probably: somewhere on what we were just talking about — the other dark metabolic reactions in plant cells that come after photosynthesis. Most of my time in Oxford was really spent trying to develop a method for dealing with very small amounts of things again, based on my work done earlier. It didn't really come to anything too great. It was a nice experience and I enjoyed it very much and my association with Professor Krebs but I don't know that it had any profound influence on what I did, really.

(*Brief gap in the recording*)

VM: It adds life to the picture of the place: do you remember when, etc. When somebody dropped something, when somebody did something bizarre, whatever it was. We had lots of parties, I remember. There were always excuses for parties

AB: Oh yes.

VM: Birthdays and Christmas.

AB: We always had a nice Christmas party.

VM: These trips up to the mountains and to the deserts and so on.

AB: That goes way back to my earliest days (*in the lab.*). Soon after I got there, I learned that people like Andy Benson and Bert Tolbert were going off to the mountains and they, of course, invited me to come along. I had lived in a mountainous area myself in the past and enjoyed the outdoors although I had never really done backpacking before. So, they introduced me to backpacking and mountain climbing. We had quite a few people in the lab. who liked to do that same sort of thing. We used to sometimes initiate people, visitors, foreigners on that, and most of them enjoyed it — some of them found it a bit strenuous. One of the social events was to go off to the mountains and climb a peak somewhere. We did sort of rock climbing, although not fanatical rock climbing. By that I mean sixth-class climbing up cliffs. We used ropes and we climbed slightly difficult peaks. Dick Lemmon participated in that and a number of other people. I think this was a social occasion., I think Melvin Calvin always looked a little askance at this; he was afraid that we would get hurt and he would lose somebody that he needed in the lab. He was always relieved when we came back and dragged in on Monday morning, all still intact.

VM: Presumably you were talking path of carbon all the way up and all the way back.

AB: No. (*Great laughter*)

VM: Did people talk about this sort of stuff when they left the lab.? I seem to remember that we talked about almost nothing else.

AB: I don't know. Perhaps you talked about it more than I did. We were talking about it much outside the lab.

VM: Maybe you had more of it than I did. What about working all hours of the day and night: were you one of those who did that?

AB: When the occasion required it. One of the things about Andy is that he always seemed to get going on the big experiments, in those early days, about four o'clock in the afternoon. This was somewhat to the consternation of some of the rest of us. That meant if we were doing a study with algae and we killed them about four in the afternoon we had to get them worked up and concentrated and extracted and reduced to something that we could put on a paper chromatogram and the chromatogram started. Sometimes it would be pretty late at night before we got out of the laboratory. We could only leave when we had the chromatographic box closed up, the solvent in and things running.

VM: Were there lots of experiments where the concept required many people to collaborate very closely?

AB: Yes, well, I don't know what you mean by many. It required teams. If you were going to do the kind of thing I just mentioned, where you harvest algae, centrifuge them down, resuspend them and do all these things, you really need several people to do it — draw the samples, control the stopcocks, do the timing, killing, and so forth. There was a stage where we usually had a small team of people working together, not a great many people, but three or four maybe.

VM: Was the pattern generally of the scientific work in the lab. one in which many people worked on more than one major project, some of them they would do by themselves and some they would do in association with other people?

AB: Yes, I think that's a fair statement.

VM: You found yourself doing that sort of thing as well?

AB: Yes.

VM: About the events which led up to the new building eventually. I guess the reason why ORL had to go was because the Chemistry Department wanted to build a new building.

AB: That's right. The Chemistry Department was housed in a very old building, as I mentioned earlier on, a brick building, and it was really quite inadequate for modern chemistry courses. It needed to be replaced. Even before that they needed to find some more space. This could be done right next to the building in the place where we were in which was itself a "temporary" building, about 50 years old, was located. It came down to the point where we simply had to get out of that building (*i.e., ORL*) and with much consternation, because we had had a very productive time there and we hated to leave it, we hated to give up the shops that I mentioned, all those things were terrible to lose. The best that the University could do for us at the time was to find us temporary quarters in the Life Science Building, in the bottom floor of the Life Science Building. They were quite nice laboratories but they were in no way comparable in terms of what we were doing to where we had been. But, we made the

best of it and we went down there. I guess in the meantime Melvin...I don't know when he got his Nobel Prize relative to...

VM: '61.

AB: ...relative to the move that we made; you'll have to look that up somewhere. His doing that (*i.e., getting the Prize*) made it more amenable for us to get funds from various sources to build a the new building. We almost right away started to develop plans, I guess, for the new building.

VM: When the first move to LSB took place, what were you expecting would ultimately happen? Did you expect to get space in the new chemistry building that was being built?

AB: No, I don't remember that being the case. Maybe it was. If so, it would have been probably insufficient space, in our view. Again, Marilyn or someone may be able to tell you better about this. I don't remember specifically.

VM: It must have been a time, then, of considerable uncertainty...

AB: It was.

VM: ...partly because you didn't know how long you would have to stay in this space and what the outcome would be. And, of course, there was the separation from the people still in Donner.

AB: That's right. That was very sad, too. They were much further away. I am sure that almost immediately Melvin was hoping to somehow find a way to get us all back together in adequate space somewhere. I'm sure he was beginning to talk to all kinds of people, potential donors and agencies, that might be able to help us find a building. I'm afraid I'm not going to be able to help you much on the details of that.

VM: During that four or five years in LSB, how did you find the atmosphere of the group, particularly the photosynthesis group, because that was all that was down there? How did it change? How did it compare with what it had been in ORL?

AB: There are two things to keep in mind here. One is the transfer into less desirable quarters and the other thing is, of course, that the excitement was mostly over by then in the photosynthesis field and, in fact, Melvin was thinking of other things by and large because he saw more opportunities in other types of research. So, I was kind of a hold-over who continued to work on plant physiology. It wasn't ever as exciting a time as was during the mapping of the path of carbon in photosynthesis. That kind of excitement left. That's not to say, though, that some of Melvin's ideas didn't stimulate other excitements in the laboratory but it wasn't the same thing as it had been in ORL. But he, I think, always hoped somehow to regenerate that kind of an atmosphere by having a big laboratory that would serve some of the same functions as the original space had done. He could see that it wasn't happening so much down in the Life Science Building.

VM: I suppose one of the things was that there wasn't so much of a single focus.

AB: That's right. As I said — we no longer focused specifically on the (*path of carbon*).

VM: When it came to the design of the new building, I remember, I'm not sure whether I am correct, that it was your idea to have a semi-circle on a block.

AB: I don't think that was my idea. It might have been Melvin's. I'm not sure whose idea it really was. What I do know is that when we kind of proposed that, the architects were sort of aghast and then they came up with the idea of making something architecturally more appealing by making the building a complete circle.

VM: I think they thought a semi-circle/block combination was too fussy for a building of that size.

AB: I think that's right.

VM: So, this building took several years before it was authorised and then before it was built. Do you think that the way it worked out was a good realisation of the sort of philosophy that everybody had felt underlay the ORL situation?

AB: I think it was. But we were faced with the fact that the group was growing all the time and with the fact that there were more (*scientific*) interests coming in and that, ultimately, the University would be introducing different team leaders in the form of more junior professors with their own ideas. To my mind, it was going to inevitably become impossible to maintain the kind of spirit that we had originally with a small group. You just can't take a small group and really expand it to a large laboratory and quite maintain (*that type of spirit*). Although I think we went fair a fair distance in that direction in the early years in the new lab. It gradually kind of dissipated as time went by.

VM: I guess that one of the things that happened was that the senior staff people were more advanced, more developed in their own careers, and each one of them had collected a group of some size.

AB: They had to be. If you were going to continue to command support and respect in the scientific community, you couldn't go on just being an assistant to Melvin Calvin your whole life. All of us, at one point or another in our careers, had to make a decision as to whether we even wanted to stay in Berkeley or go somewhere else and teach or do some other job. I am sure everybody who worked there had offers of other things. People stayed for various reasons. Of course, they love the area, but one important reason was that the new lab. would offer them opportunities to have groups of their own and students of their own and carry on something scientifically meaningful under their direction. That was necessary, I think, if people were going to stay.

VM: And, in fact, you were able to do that right the way through until you retired.

AB: Pretty much so, although after Melvin retired it was quite a changed scene. But I still had good graduate students and things to interest me right up until the time I retired.

VM: Al, I think that's a marvellous collection of reminiscences so far. As you know, we are going down to Southern California and to other places and see some of our former colleagues next week and when we come back perhaps we'll have the opportunity of bringing things up for discussion again. Thanks a lot.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name James A. Bassham

Date of birth Nov. 26, 1922 Birthplace Sacramento, CA. USA

Father's full name James Calvin Bassham

Occupation Miner, Forester Birthplace French Gulch, CA, USA

Mother's full name Helen Alma Baker

Occupation School teacher Birthplace Healdsburg, CA USA

Your spouse Leslie A. Bassham

Occupation Librarian, Homemaker Birthplace Pasadena, CA USA

Your children Eric (38) Chemist, Glen (37) Electronic Engineer, Helen (35) Admin. Asst, Frank (32) Biotechnician, Susan 27 Molec. biologist

Where did you grow up? Trinity Center (pop 200), CA USA

Present community El Cerrito, CA USA

Education B.S. (chem) Univ. of Calif. Berkeley (1945), PhD Org Chem U.C.B 1949
note: U.S. Naval Reserve WW II, Korean - Ret. CAPT USNR

Occupation(s) Research Chemist, Sometimes Professor of Biochem. U.C.B. 1947-1986 (inc), Visiting Sr. Post Doc - OXFORD, UK. 1956-? Retired 1986

Areas of expertise Photosynthesis - biochem. of carbon fixation

Other interests or activities local civic affairs (parks), gardening, music composition.

Organizations in which you are active various conservation orgs

Chapter 8

EDWARD L. BENNETT

Berkeley, California

May 28th, 1996

VM = Vivian Moses; EB = Ed Bennett; SM = Sheila Moses

VM: This is May 28th, 1996 and we're talking to Ed. Bennett in Berkeley. When did you first join Calvin and his group?

EB: 1949.

VM: And how did it happen?

EB: Well, I was at Cal Tech at that time and I went to one of these ACS meetings and heard him talk about photosynthesis. This sounded like an interesting place to go. So my professor at Cal Tech, Carl Niemann, wrote Calvin and there was an ACS meeting up here, so Calvin said "come on over and be interviewed". I think I was mostly interviewed by Bert, if I recall. Subsequently, I was offered a job but I wasn't offered a job in photosynthesis. I was offered a job in more the animal biochemistry segment.

VM: What was your previous experience and training at that point?

EB: At Cal Tech I had done a certain amount of organic...During the war I spent time in Florida in the great swamps (terrible) and then at Cal Tech my degree was mostly making fluorinated phenylalanines.

VM: Of course, you were a chemist?

EB: Yes, a chemist.

VM: Not a bio of any sort.

EB: Not a bio, no. I never had a course in biology. My work involved a little bit of what was then called biochemistry because they were enzymatically-resolved into D- and L-forms which was something Niemann had worked a lot on using papain.

VM: And you had just finished: what? Your masters degree?

EB: Ph.D.

VM: You had finished your PhD. when you came?

EB: Yes.

VM: What animal work was suggested?

EB: I think what I started doing was actually making radioactive aza-adenine, aza-guanine, using carbon-14. I spent close to two years making those darn things. Of course, then, carbon-14 was real valuable. You tried to save every little bit and recover it, and so on and so forth. So I may have done some...I don't know if I ever got around to doing any experiments with them or not. Then, I got a fellowship to Europe after two years and went to Kalckar's lab. in Copenhagen. That was an interesting year. I was kind of naive, I guess, but fortunately I got to come back (*to the Calvin group*).

VM: When you were first hired by Bert in 1949 was that an open-ended commitment?

EB: I suppose it was. It wasn't anything really spelled out. I guess the other kind of biology-sort of thing that I had done before coming here was working with James Bonner. At Cal Tech you had a major and a minor and the minor, in my case, was in plant physiology. We went out to the desert one weekend and got a lot of plants and extracted them and tested them for plant inhibitors. I found one and identified it, I guess, and unfortunately neither with that nor with the phenylalanines was there any chance of (fluorophenylalanines) of doing any work with them subsequently. I've seen references in the literature to the fluorophenylalanines being used for various things.

VM: When you got here, when you got to the group, who was there?

EB: I think Martha (*Kirk*) was there at that time, if I remember correctly, obviously Bert (*Tolbert*), Pat Adams, I believe.

VM: You are the first one we have talked to who has mentioned Pat Adams. I don't remember Pat Adams.

EB: She was tall, slender, blonde.

VM: What did she do?

EB: She was in the synthetic group. The group at that time had three parts. There was the photosynthesis part that was over in ORL...

VM: And they had already gone to ORL by the time you arrived?

EB: Yes, they were over there. And then there was the part down in about the three or four rooms down at the end of the (*third floor*) hall in Donner. One side was doing carbon-14 syntheses and developing things on that; the other side, as I recall, Martha with Bert, were doing metabolism experiments in humans and in rats. I don't know what the years were; one would have to look those things up but I remember it going on.

VM: Which side of the corridor did you join in?

EB: I wasn't in the carbon-14 synthesis side. My work was supposed to be leading to more biochemical stuff.

VM: Who were the carbon-14 synthesizers?

EB: The people I remember there; Dick (*Lemmon*) knows dates and so on better than I do. There was Pat Adams; of course, Rosemarie Ostwald was involved.

VM: Was she one of them as well?

EB: She was involved up there. Marilyn can fill you in the years. There was a fellow named Bob Self who died relatively early. Then there was Eugene Jorgensen, who was somewhat afterward and who was unfortunately murdered over in San Rafael, very mysteriously, kind of tragically actually, because he was murdered and his son was under a certain amount of suspicion and I think his son actually died in an automobile accident or something like that. Gene and I used to play tennis together quite often. (*After he left the Calvin group*) he went over to San Francisco (*UCSF*).

VM: Did this C^{14} synthetic work go on for a long time?

EB: I think it went on, probably if I were to pick a year, I would say '55 or so. It gradually got phased out as more commercial places came in (*making labelled compounds*). I remember that Dick (*Lemmon*) got involved, as he probably mentioned, in this radioactive decomposition stuff (*with choline chloride*). I remember I pulled out a few compounds that I'd had around; they ran them on chromatograms and so on and so forth.

VM: How did you work at that time? Were you working by yourself or together with other people?

EB: I had a couple of people who worked with me. There was this gal named Barbara Krueckel, who was not much good. Ultimately I worked a lot with Ann Hughes. We worked on the fly business (*Drosophila*), the D_2O in flies. Then this business, I remember we got a big laugh out of it. With the D_2O we were talking about mice and I guess it was at a AAAS meeting I talked about mouse eggs. Everyone laughed, but in a way that's true. They have eggs...

SM: Just like people.

EB: ...just like people, Yeah. That wasn't exactly what I meant. So I worked with her on that. I guess it was much later, after we moved into the new place (*the Round House*), a lot of other things (I worked with Ann [*Orme*]) took place. This business of memory transfer, that was started over in Donner, actually. That was an example of something that so frequently happens. Calvin reads something and he came around and kind of threw out the idea and: "Does anyone want to work on it?". Usually, most of these ideas didn't go anywhere, I don't think he ever held it against a person but ,if you did work on it, he'd certainly support it. That (*the memory transfer work*) started there. We got the two people from Jacobson and Jacobson, who were students with McConnell and they came out. I remember we had a place set up downstairs in Donner (*on the first floor*); it started there. When we moved over to the Round House, I remember what room they went to there.

VM: That was a man called Allan Jacobson, wasn't it?

EB: There was Allan and someone by the name of R (well, we could look them up).

VM: What was McConnell's first name?

EB: James McConnell. It's kind of interesting. I kind of had this in the back of my head that somehow or other he he'd been involved in the Unabomber thing. When newspaper articles came out a few weeks ago I was almost right. It turned out that, I think, the first Unabomber bomb sent went to his home and a student opened it up and was injured. So, it was presumably aimed at McConnell but it didn't get him. I remember telling Mark about it that I thought.....and I haven't talked to Mark since. But that memory was right on that.

VM: That's Mark Rosenzweig (*in the Psychology Department at Berkeley*).

EB: I think that was about '63 when that incident happened. It was the first one up in Michigan.

VM: Coming back to the early days in Donner: did the people at that end of the corridor in Donner feel close to the ORL people?

EB: Reasonably so. Actually closer than to some of the people in the LBL (*Editor: This is clearly in error. He means "LCB", the Laboratory of Chemical Biodynamics, Round House*) feel to those on another floor now. We all met together once a week for the infamous Friday or Thursday morning briefings and they were held down in what was then the library (in the Donner Library), still is the library, but didn't have all the stacks in it. We used that for the room.

VM: These were the seminars?

EB: Not the seminar room but the room in the library.

VM: These were the seminars you were talking about that were held on Friday mornings.

EB: The Friday morning "show and tell". At that time, early on, you didn't have the advance notice. You kind of came down with your notebook and were quivering, shall I say, just hoping that he (*Calvin*) wouldn't look around and see you. Sometimes I think two people got attacked! That way the people from our place and LBL — LRL (*Editor: this is an error for ORL*) got to know each other. Also, of course, one used the paper chromatography facilities over in old lab. We were going across to that lab. quite frequently.

VM: So everybody knew everybody pretty well.

EB: Yes, I would say that everybody knew everybody. Not like now.

VM: What did you do with chromatography? I don't remember your using chromatography?

EB: Oodles of stuff. We studied metabolism of radioactive adenine in mice; I guess Barbara Krueckel was a co-author on that; (that was after I came back from Copenhagen) determining the half-life of various adenine in the DNA and RNA of various tissues, finding that in intestines in had a curve like *that*, in liver then it went like *that*. That sort of thing. There wasn't a lot of work on that at that time. I don't ever think I did very much with azaguanine. And then I worked with a fellow that you may have known when you came, Dr. John Weaver who was an MD.

VM: I remember the name, not the person.

EB: I don't know where we got it, but we worked with stilbamidine, and did some studies on its metabolism. Those were all back in the early fifties.

VM: For these you used the same sort of chromatographic techniques as the photosynthesises.

EB: Certainly with the adenine, for example, you would want to know what it was metabolised to, how many compounds it went to in the urine and also to check out the purity of your stuff. We did oodles of chromatograms.

VM: Radioautography and counting, and all of that stuff.

EB: With the adenine, for example, it very quickly went ADP and ATP and all those kind of things, and a certain amount would get metabolised to other degradation products.

VM: As a group, the people who were working on the animal metabolism, did you have as sharp a focus as perhaps the photosynthesis people had?

EB: The reason for it, of course, was that we were interested in cancer and there was actually another person, who hasn't been mentioned, was (*Dr.*) Max(*well*) Gordon (*Editor: He came to the Calvin Lab for postdoctoral studies*).

VM: I didn't know him at all.

EB: I just don't remember what years he was there and he finally went from our place who. He subsequently went on to Smith, Kline & French (*Smith, Klein, Beckman*) and he was interested in some of the aza compounds. He was there (*in the lab.*) for a couple of years.

VM: So the interest in cancer really goes back a very long way.

EB: Oh yes, because Charlie Heidelberger was working on that kind of problem there, too. That goes back, might say, before I came. I guess in a way, Calvin suggested: "Well, let's do something connected with cancer research". I remember that I made radioactive adenine and radioactive guanine, probably traces of them still exist, and made this radioactive azaguanine and azadenine, which nowadays would be a cinch but then you had to worry about the yield and all that sort of stuff.

VM: What was the relationship between the Calvin group in Donner and the rest of Donner, with John Lawrence's' group?

EB: It was moderately close. Bert obviously worked a lot; he worked with a fellow named called Nat or Nate Berlin (*Dr. Nathaniel Berlin, a medical doctor*) who went back to NIH and did a lot of work on blood cell turnover. I don't know who was involved there but obviously his metabolism studies with Martha, he worked on a lot of that to develop this machine, the instrument with the hood on. People were given a shot of this or that and then the excretion was determined.

VM: Tell me, I don't know anything about that one.

EB: This was probably, nowadays they wouldn't want to know. Bert and Martha were involved in these metabolism studies, using both mice and rats, and ultimately

humans. Bert designed and they built this machine that measured the CO_2 output, radioactive CO_2 output, actually both outputs I think continuously.

VM: What was the machine? I don't know about it.

EB: It was nothing more than an ionisation chamber, I guess.

VM: You moved your hand round your head to suggest...

EB: You put a hood on, air came in and air came out. With a rat you put it in a cage, I guess, like a desiccator, but more like a cage; air came in, air went out, flow meters on it so you knew it was 500 cc a minute or whatever it was. So you could determine how much was metabolised from its rate. I don't know who it was that provided the patients, whether it was John Weaver or Nate Berlin, but they came from the medical side of the Donner.

VM: And they went in this hood as well?

EB: What the patients? Yeah.

VM: I see.

EB: As I recall...Bert can tell you better. But, say with severe arthritis and presumably normal people, that sort of thing. Then, I think, subsequently this machine, or the progeny from it, were converted into commercial instruments. This was probably the first, or among the first, of metabolism machines.

VM: What about the way in which the people in Donner interacted with Chemistry? Did you have anything much to do with the Chemistry Department as a department?

EB: I don't recall that I did.

VM: You had graduate students, Melvin's graduate students, presumably?

EB: Occasionally, yes. I didn't get many but I had some. Actually, in thinking of the timetable, I guess it was '54 that Calvin brought Rosenzweig and Krech (*Professors Mark Rosenzweig and David Krech of the Department of Psychology, University of California, Berkeley*) over to meet me. He had talked to them, or those two individuals had talked to Calvin, over at the Faculty Club. Subsequent to that time, most, but not all of my efforts, were spent on this brain chemistry research. I guess our first publication was in '54 and so it must have started about then.

VM: If I can backtrack just a bit: as I remember, Calvin's original agreement with *(Ernest O.)* Lawrence that he and his people would make radiochemicals which John Lawrence could use in his group in Donner and, in addition, to which he would do the photosynthesis stuff. You were really part of the expression of the collaboration with the Donner group in a sense, would you say, or am I pushing it?

EB: It was pretty tangential, because I don't think anything I ever made were they considering using.

VM: Did they participate in your research at all?

EB: Outside of...I think John Weaver was the only one I ever collaborated with. Berlin and probably Weaver, and maybe Will Siri, were people that Bert collaborated with, I could think of. And, of course, they had people working with them, those three individuals.

VM: This was all supported by the AEC at the time, under the auspices of their remit to support non-military use of atomic energy?

EB: Yes.

VM: When you later moved into the brain studies, did you continue to use isotopes or something which would...?

EB: No, I don't think we ever used any (*isotopes*) in it, actually.

VM: Did you begin to fall outside the formal AEC guidelines in that sense?

EB: Probably. That was what made the lab. so interesting. We certainly wrote about what we were doing and it wasn't until much later, I think, in fact. way after we moved into the Round House, that there was much problem with funding that part of the research.

VM: What direction did your work with Rosenzweig and Krech take?

EB: More serendipity than anything else. They came over, I guess it was mostly Krech that had the idea that cholinesterase — at that time people did not really distinguish between acetyl cholinesterase and cholinesterase; it was all cholinesterase — and I guess it was primarily Krech that had the idea that different levels of cholinesterase would lead to different behaviour in the rats, different problem-solving abilities. He had worked for years using the Krech hypothesis maze: this is a maze that you can't solve because it is always jiggered so there's no solution, it's random. Rats will adopt a hypothesis and they'll adopt a hypothesis, either I can solve this on the basis of visual cues (if light was right the last time, light will be right the next) or can be spatial (if the right side was right this time, then the right side will be right next). So, you can divide them that way.

They had divided a few of the rats into maze-bright and maze-dull animals. Initially we had, I don't know — perhaps less than 20 animals. At that time, (*Professor*) Joe Neilands, who was right across in the biochemistry building, had a machine called a pH-stat that this very strange character named Cannon, I think, had developed. Joe let us use that to measure the enzymatic activity in little areas of the brain. It turned out that those (*rats*) which were maze-bright, I think (could be turned around), had higher levels of cholinesterase in the visual and occipital cortex than did the maze-dull. We published that in *Science* and went on from there. I think really, in retrospect, it was probably a strain difference involved as much as anything. Anyhow, that's what we did.

Actually, in terms of support that at time — oh dear, I had her name; she married Roderick... What the hell was her name?

VM: What did she do?

EB: She was an assistant on all this and she was involved in the assays. We assayed oodles and oodles of animals and somewhere along the way, early on, Krech had the idea that maybe if these animals exercised more, shall I say, used their brains more

(whatever word you want), that this would change the brain parameters. We got involved in that and we had what became very well known as the EC and IC and the SC animals. Indeed, we found that there was a difference in the activity of the cholinesterase. By that time we knew more about cholinesterase and knew that it was acetyl cholinesterase and cholinesterase, and that these two responded somewhat differently. We worked and worked and worked on that.

Subsequently, when we were putting a lot of this data together, we noticed that one group (*of rats*) had lower activity per milligrams, but had more milligrams, so had higher total activity. Then came the idea that the cortex actually changed in its parameters with the environment. About that time Marian Diamond joined us. She was involved, of course, in the anatomical work which she still has carried the theme of the enriched and impoverished (*environments)* very, very successfully since. Unfortunately, she split from us but she has gotten a hell of a lot of mileage out of that idea, applied to humans. A story I like to tell, one night I was listening to the radio in our bedroom, as I often do, and kind of fell asleep, or turned it on, and heard her talking at the Commonwealth Club (*in San Francisco*). I didn't have to hear very many sentences before I knew who it was. Then I fell asleep, it's old hat, and subsequently I heard the applause and oodles and oodles and oodles of questions. The next week there was this guy Smith, who was then chairman of General Motors, got to the end of his talk, (*sound of slow clapping*) little bit of applause, a few questions. Marian, she just retired as director of the Lawrence Hall of Science after five years. We had a very fine retirement party up there with all the big shots — Seaborg, and the Chancellor and all of that — and in April they had a dinner for her as the Alumna of the Year. She has really gotten a lot of honours for both her research and her teaching.

VM: How long did you continue to work with her?

EB: Probably ten years without going back, many years. We really had quite a productive cycle then, went from the mid-(*fifties*) probably into the seventies, I guess.

VM: You mentioned earlier on that you had spent some time with Kalckar's group in Copenhagen, that was after you had been here for...

EB: ...two years...

VM: ...two years and you went for one year to him?

EB: Then I got interested in adenine metabolism over there, as I recall.

VM: And that's how you came to do that kind of stuff when you came back?

EB: Yes, I believe so. I believe I was interested somewhat before I went but I did more of it over there.

VM: At which stage in your career was it clear that you had become a permanent member of the group? Or was it never clear?

EB: It was never really clear! It just kind of went on. I don't think there was any time that I got a piece of paper saying "You are now a (*permanent member*)". It was only after many years.

VM: Is that generally the way the group tended to operate, do you think?

EB: I would guess so, but you should ask Dick (*Lemmon*) and ask Al (*Bassham*), but I think it was pretty informal in those days. There weren't all the rules and regulations in either our lab or in LBL. Nowadays, you know, after you have been there so many years if you aren't made some sort of status position, why you are probably going to have to go. Along the way, on this psychology stuff, we started writing and getting our own grants so we were partially funded by NSF or NIH, and I can't remember the gal that did a lot of the work early on — Hilda Karlsson. She married Tom Roderick so I was always mad at Tom because he took her away! They are *back at Bar Harbor now. He has had a very successful career. Then Marie (Hebert/Alberti*) came. Somewhere along the way I guess I was involved in hiring Hiromi (*Morimoto*).

VM: Both of whom have been with the group since the (*late*) fifties.

EB: I remember about Hiromi that Bert was very concerned about hiring a male. He didn't think as a technician that that would work.

VM: Why not?

EB: I don't know. I guess he didn't realise that the Japanese were God damn good. Probably later than that, Hiromi did a lot of work with bungarotoxin that we were involved with. I don't know whether or not the Ferchmins (*Pedro and Vesna*)were here when you were here. They were in the Round House so that is really post the period you are talking about. But then Marie is a superb person, I would say.

VM: And she's still in the group.

EB: She's working mostly for Hearst (*Prof. John Hearst*) now, I'm not quite sure.

VM: Is Hiromi still there?

EB: Hiromi is up on the Hill in that isotope thing (*National Tritium Labeling Facility*). I think that he is probably a mainstay up there. He is real competent, real hardworking, probably couldn't ever get him to write a paper; maybe you can now. Marie is one, you know that some people nowadays they've got to have a job description; if something isn't in the job description, then they might not want to do it. Marie always would do anything that you would ask her. If wanted some literature looked up, fine; if you wanted some stuff put into order, she would put it in great order. She and I probably killed and dissected more rats than anyone would ever want to know, probably way over 10,000. She dissected them and did a superb job. Again, I don't know when it happened but I remember once when we were looking for grant support, we were having NSF and NIH teams coming — those days they came to see what you were doing, rather than this business of reading and then having questions and misinterpreting what you said, not giving you a grant. So Mark told me who was coming to one of them, and I told Mark, "you can forget it". There was a guy from the midwest who was coming that we had some discussions with, shall I say. So, they came to watch us do a dissection. The man looked at it: "Terrible, all sorts of white matter, not uniform at all". About a week later the other team came, looked at her doing dissections: "Beautiful". She hadn't changed her technique between week one and week two; and then we did a lot, in terms of chemistry there, we kind of abandoned the acetyl — I guess Hiromi did oodles of acetyl cholinesterase assays, that's what he did oodles of.

Hiromi worked out a method for doing DNA and RNA analyses which, I think, was better than any in use at that time and may still be one of the best available, using

CTAB but the methods used at that time using a spectrophotometer and colour reactions was just no good for brain; it gave all sorts of false answers. So then we did studies on RNA to DNA ratio and found that the ratio of RNA to DNA was higher in certain brain areas of the EC animals. Even though the difference was small, if we were given 12-14 pairs of animals, divided into group A and group B, randomised, we could without fail told you which group was which, it was that reliable. What it meant, we started to think about things to do, but we never got to it. Probably now nowadays with what you can do with RNA and DNA we could have found (*something interesting*). Ann Orme worked for me, too. I guess she started working in Donner.

VM: Did you gradually move away from working with other people in the Calvin group more and more to work with Krech and Rosenzweig?

EB: Yes, I would say that probably by 1956, I don't think I did much of any work that was traditional of what the Calvin group was working on.

VM: Did Calvin retain an interest?

EB: Yes, very much so.

VM: Contributed?

EB: Yes. He got interested in this worm (*planaria*) business, enough in that to get a trailer put up on The Hill, where Ann (*Hughes*) was. She was a firm believer in it, perhaps still is (I don't know).

VM: Ann Hughes?

EB: Ann Hughes.

VM: We'll be talking to her and find out.

EB: You'll find that she was a firm believer, unfortunately. She went back to work with Ungar. Ungar sent us some extracts which never really worked, always excuses. Then David Samuel (*Editor: From the Weizmann Institute in Israel*), again in the Round House, got involved in it — this is all Round House — and Bill Byrne was involved in it. So that era was shortly after we moved into the Round House.

VM: David Samuel was a bit later; David Samuel was '65-'66.

EB: So that worm stuff started over in Donner and was carried over into the Round House.

VM: What was the view of the people in Donner about the approaching new building? You had not had to make this move....

EB: We had to move down to LSB (Life Sciences Building).

VM: Did you me to LSB?

EB: Yes, I did.

VM: Why did you have to leave Donner to go there?

EB: Probably because they wanted our space. I don't know who else moved, but certainly I remember I moved down there.

VM: Not everybody moved. There was always a presence in Donner until the round building, wasn't there?

EB: Bert was gone, I guess. I don't know whether Dick stayed in Donner until we moved or not. I know why I had to move because we were doing the brain analyses upstairs in ORL so that's why that work had to move. I guess my office and stuff was still in Donner but the brain analyses were upstairs in ORL, in kind of a loft room up there, so they moved down to Donner (*Editor: Error — should be LSB*) in one of the rooms. The thing I remember about that one time in Donner. LBL (*Editor: Error? Should it be ORL?*) had these kind of frosted glass pane doors and one time I opened the door and who did I hit, Henry Mahler?

VM: Who?

EB: Henry Mahler.

(*Brief discussion about LBL vs. ORL*)

EB: That was perhaps the most noteworthy....

VM: Was he badly damaged?

EB: No, fortunately. He could have been.

VM: So, when the designs began for the new building, what was your interest in securing...presumably you were a contributor.

EB: We all had our input.

VM: What was your input? What were you looking for in the new building?

EB: I guess I was mostly interested in the room that was kind of "my" room, shall I say, that was going to be my rooms on the first floor. As you go around to the left there is an office and then there are two rooms, the next two rooms. Initially both of them were assigned to me and then, I think I didn't use the first one of the two too much and gradually had to give that up. Then the second room, that's where Hiromi did a lot of work and that's where our snake venom work was done, and then, of course, I had a couple of lab. benches upstairs and had a couple of graduate students. I was kind of involved with a guy named Simpson (*Lance Simpson*), I think, and also with a fellow who went to England and has done real well and is now up in Davis (*Michael Hanley*).For a while there, that again was in the Round House, we had a really good group going, with a graduate student and with Vesna and Pedro (*Ferchmin*). When they left we got another person is as a postdoc. but he didn't accomplish a hell of a lot. That program kind of lost its momentum. It was a good programme. We did a lot of things then and we are still equal to the time.

VM: What was your view about the proposed design for the Round House with these big open labs? Did you like the idea?

EB: Yes. I liked it then. I think it has kind of gone downhill since then. It's been cut up a lot more, a lot more stuff (*equipment, desks, etc.*) put into it. I think on the whole that

lab. has adapted pretty well when you consider we have been in there 33 years and it is still pretty functional without a lot of major changes. I think the idea of having an open lab. where people could interact was really great.

VM: You found from your own experience and from your people that it worked like that, as it should have done?

EB: Yea, yeah. That I liked, I know that some of the others don't do it that way, I had an office there on the second floor on the south side, one that looked out into the lab. — I never pulled the blinds down the whole time I was there. What I liked about it: You could sit in your office and see if someone was out there, if you wanted to go out and talk to them, know where people were and what was going on. Some people go into their offices and pull down the blinds, there was all this talk about having windows or not having them (*in the offices*). I thought it was great being in a glass house.

VM: Everybody we have talked to so far has been very (*enthusiastic about the building*).

EB: Some of the newer people in the building have the blinds pulled down all the time.

VM: Perhaps you needed to belong to the earlier generation of people who had grown up with the idea. How about the social side of the lab.? Have you always been a participant in gatherings and parties?

EB: Some. You know as one gets married with more responsibilities you become less of a participant. But certainly Martha and, to a slightly lesser extent but not a lot, Ann Hughes did a lot. Martha used to have a trip or two every summer up to Wrights Lake, which 10 or 15 people would go up; they used to organise ski trips up to Yosemite and a moderate number of hikes up to the mountains. I don't think there are so many of those now.

Of course, as the lab has gotten bigger it's harder to do. There are 70-80 people in there now. Some of them I probably wouldn't recognise if I saw them. When we were in the lab., one knew everybody. Absolutely. There was nobody in the lab. that you didn't know their name. Another way that the lab. has changed a lot, in a different context, is — Calvin was always visible. He was visible in ORL because, again, that was open and, as you probably know, he was always out there looking at a chromatogram. That was the idea he carried over to the new building. You might say he wasn't as visible over in Donner but, since we had meetings once a week, he was visible then.

VM: When he came to Donner did he do the same thing as he did in ORL, actually talk to people in detail about their work?

EB: Yeah, I think so. I think he knew pretty well what was going on. Certainly you felt like if you wanted to talk to him about your work, I think sometimes, I don't remember, but I think sometimes would call us over to his office, about something, and say "Hey, Ed, I'd like to know a little more about what's going on, what you are doing". Not in a threatening sense, just in an interest sense.

VM: Did these social activities go on from the very beginning? When you joined were they doing trips up to the mountains?

EB: Some of us, yes. Certainly, in part, because a few of us had rather similar interests — Bert, Al, Dick and myself. I don't know if Dick mentioned but recently he got

interested in the registers off the various peaks in the Sierra. He went down to the Bancroft Library where they retrieve the registers from a lot of these places and maybe put new ones (*in place*). Dick's very organised, you know. He knew that on August 6, 1949 he had been on a trip to this place or that place, Mt. this and Mt. that, so he found the registers, when he could, and he Xeroxed the pages from them. Some of the pages had Al and Dick's name, maybe mine on, maybe another one with Andy's (*Benson*). He had about half dozen or more of these. He just did this recently.

VM: I can guess what they are, but I have never seen them. These are registers kept on the peaks?

EB: They vary. More formally they were maybe a notebook, something like what you have or even smaller. Some of the more formal places would have a metal box that they were in. So, when a person got up to a 14,000 foot peak they would inscribe their name and the date and maybe some comments — "Beautiful view today" or "Hell, I don't know why I did this" or "A storm is coming". Some of them were less formal, nothing more than Prince Albert cans with pieces of paper in them.

VM: It shows you that we never got to the top of one of these peaks!!

EB: You should have. I remember some of the Swiss fellows that came over that went up there with Dick, Bert and myself, the Schmids, Kathi and Hans Schmid.

VM: How about the wives? Did they go along too? Your wife? Dick's wife?

EB: I guess most of the trips I was referring to were those climbing trips were before we were married, but not all of them. I think one of them afterwards; I'm pretty sure that Millie went on that one, up in the area of Mts. Williamson, Barnard and Kindall (that was probably before we were married). Bert was intent on climbing all of the 14,000 foot peaks in California and those three were three you could get in one weekend. The notable thing about that is going up Shepherd Canyon and I swore that if I went up that once I would never do it again because you go up over a ridge and down some, then you go up this long, long canyon in the sun, the stream is way down there and there's no water coming in the side streams. We got up to where we could camp overnight by a little stream; there was a little bit of water there, enough to cook with. The next morning it was all dried up. We then went and found a camping place on some bare rocks. Andy and Al, I think, went quite often to the White Mountain because there was some research going on up there. Al may have been involved in that as part of his navy stuff: I don't know, you'd have to ask him. That would have been before the 1950s or so.

VM: Then there was the lab. picnics — Tilden Park?

EB: Sometimes in Tilden. Later on, when Calvin got his ranch, there were a couple of picnics up there. They were often in Tilden. Nowadays we seem to have them more at the back of the lab. They had a few of them recently, during the last five years or so, up where the Kerr Campus is now, back in a field up there.

VM: Were those centrally catered or did everybody bring stuff?

EB: People either brought stuff and then somebody would take responsibility for getting the main things.

VM: They were usually one-day things or afternoon things?

EB: Afternoon. They still have so-called picnics or barbecues outside the lab. in the back there now. Vangie (*Peterson*) is a great one for organising things like that. If she had her way she would probably have a picnic every month.

VM: And then I remember Christmas parties.

EB: Yeah.

VM: Presents? Were there presents?

EB: At the early ones there were. And I recall there was some poetry or something that went along with them. They were all over in ORL.

VM: Who were the poets?

EB: You know, if I gave a present to you, I'd want to make up something about it.

VM: I remember that Dick was a bit of a poet at one time. Did you write poetry too?

EB: Hell, no. Again, there are so many people, they (*the Christmas parties*) kind of disintegrated once Pimentel came, partly because he liked to have loud, loud music. Even now they have music. They try to put 60 people into that seminar room, 60 people start talking, I give up on things like that.

VM: It gets more difficult as you get older to hear all these things.

VM: Can I stop the tape? It's almost at the end.

(Tape turned over)

SM: I'm wondering what your first impression personally was of Melvin was, when you first met him.

EB: Well, I guess one was very excited, you know. I went to this ACS talk and heard this discussion of all the work going on in photosynthesis; at that time it was pretty well developed and it sounded like something that would be a heck of a lot of fun to work on. I don't think I ever felt that he was unapproachable or anything like that. I was elated when I got a job up here, jobs weren't than easy at that time. I had one other offer, from Lederle. I had the choice of going here or going back to Lederle; there was no competition and I recommended the Lederle job to a good friend of mine, a fellow by the name of John Brockman, who took it and was very successful there. He worked with Tom Jukes for many years; unfortunately, he's gone the past year or two. If I hadn't gotten this job I might well have ended up at Lederle, I don't know.

Another place I was interviewed for was this Baxter Laboratories, which, at that time, was a small outfit, now it's a huge outfit.

VM: From what you have been of the Calvin group over the years and what you knew of other groups before you came and of your time in Copenhagen, is it a very different outfit from other places?

EB: I think it was, particularly up until the time that Calvin phased out.

VM: Could you epitomise why you think...

EB: Kalckar's lab. is the only other lab. that I had close experience with. That was a relatively small lab. Kalckar worked right in the lab., he was around all the time. It was great if you could understand what he was saying but apparently his English and Danish weren't too different. Finally, I didn't get along that well with him because I had different ideas of what I wanted to do than he seemed to have, even though we had spelled them out initially. It was a small lab. with 3-4 other people, couple of lab. technicians, and, if you removed all the walls, it would have fit into this place — in an old Danish building and, of course, much, much less in the way of equipment. He obviously was a very brilliant person but he was a kind of hard person to understand just what he was thinking. I enjoyed my year there, not so much because of the lab. (I didn't mind the lab.), certainly worked hard there. But I liked Copenhagen and all the things that it offered. It was a great city.

SM: So the dynamic of the group in the Round House, and before the Round House, was different from any previous experience you had had or any other group that you knew of in the matter of working.

EB: As a graduate student at Cal Tech, maybe there were, my lab. experience after leaving Reed College in 1943, I went to Cal Tech to join the war effort, shall we say. There was a group of us who were working on the analysis of war gases; I worked with Niemann and Swift. Niemann was very well known for his analytical procedures — inorganic analysis — so our job then was to phase down what he had designed for macro, down to where you can analyse 3 or 4 milligrams of some unknown war gas which he had perhaps collected on a carbon filter, or collected a drop or two. So we worked out an analytical scheme which I think was perhaps pretty good, I think. It was used for a long time. We developed a method of putting the sample, mixing it up and putting it in a little tube and heating it, as I recall. I remember Joe Neilands, some years later, said he still used that in his laboratory here (*at Berkeley*) for people to analyse stuff; he thought it was great. That was before all the days of modern instrumentation.

There were half-dozen of us working on that and a few of us got sent off after a year and a half to Florida, to a place called Bushnell. What we did there was to analyse bubbler after bubbler of samples that were collected out in the field: in other words, they would shoot off a mustard bomb and then they'd collect a bubbler for the first half hour, the next half hour, and so on, and bring them to the different stations; you would have to analyse them. My job was mostly to titrate, I guess it was with bromine these things, and you used an indicator. Finally, we noticed that the first samples were easy but the later samples got harder and harder; the end point wasn't as sharp. Then it was realised that what was happening: the first samples had one sulphur predominantly and then the polysulphur ones began to come off which was one of the things which led to the idea that the mustard was very persistent. In a way it wasn't that persistent; it was the impurities that people were discovering, detected six hours after the bomb. In humidity like that it was all gone. So, I spent too God damn long down there.

VM: When you were at Cal Tech, was that in Pauling's department?

EB: Yeah, yeah.

VM: So you knew Pauling well?

EB: I wouldn't say I knew him well. I managed to survive two floodings of his office.

VM: You flooded his office twice?

EB: We had a big electrophoresis device, about this big around, and electrodes here, put your sample in I forget in where. The tubing broke or something happened in the middle of the night; it went down to his office. Some time later, a similar sort of thing happened. But he didn't kick me out.

VM: You knew him personally, saw him in seminars, etc., stuff like that?

EB: Yeah, yeah.

VM: Very different from Calvin?

EB: I would say quite different.

VM: They were both remarkable men. How would you differentiate?

EB: Well. I didn't interact with him, of course, like I did with Calvin, so it's a little harder to tell. But obviously he was a man with lots of ideas and some were right and some were wrong.

VM: That's true of Calvin as well.

EB: That's true of Calvin as well but I think maybe more of Pauling's were right, I don't know. It's hard to know.

VM: Did he bubble out, the way Calvin does (did)?

EB: Yes, I think he threw out ideas, yeah.

VM: Receptive to criticism?

EB: I don't know. I never criticised him.

VM: Do you find Calvin receptive to criticism?

EB: Well, I suppose so. It would depend who it is coming from. I think if it were more suggestions, he would probably be receptive, but if you — I've never ever tried it — point blank said "You are all wrong in this". Now, (*Mina*) Bissell would be a good one (*of whom to ask that question*); she was one of the few people in our group who would ever tell Calvin to his face that what he was saying wasn't right or something ought to be different.

VM: But if you gave him a reasoned argument, he would...

EB: Yeah. I don't think I ever had much reason to differ with him on things. There was a time when one went over papers somewhat with him. He would make suggestions and that sort of thing; there wouldn't be any problem. Later on, of course, he would look at them less carefully.

VM: In the early days, when presumably he had more time, did he write papers or was it people like yourself who always wrote the papers?

EB: No, he wrote a lot of his papers, I think. Yeah. Certainly he would talk. Lots of times he would give a seminar, kind of a lab. seminar before a talk, and he would go and give the talk, and putting the two together those would become at least a paper, maybe not quite as formal a paper as some others, but he would test out ideas. Marilyn would know better but I think early on he personally probably wrote quite a few papers. Some of the papers certainly had his name alone on them and I think he was always generous about co-authorship. I don't know of anyone that complained "Hey, I did half that work and I didn't get my name on the paper". I never heard of anyone making that complaint.

VM: Not at all, On the contrary. We have heard one or two cases when he felt his name....

EB: If anything, I think he would lean the other way and your name would get put on a paper. That was probably less prevalent then than it is now. I think nowadays the system seems to be put everyone that even washed the dishes on a paper, almost; if they type the paper up, put them on, almost too much.

VM: Going back in time, again, what did you do about actually writing them? Everything was written longhand, or did you all have typewriters?

EB: We had typewriters. I tended to write papers longhand and type them; I could type.

VM: You would type them yourself?

EB: I typed the first draft. Then, of course, we had people that would type the final draft either in our lab., or when I was working with Mark and Krech, why they had secretaries (*in the Psychology Department*). A lot of the papers that had my name on with Mark's I probably didn't write that many words on; he was good at writing and this worked very well. He would write a draft and I would make suggestions on it, we'd work it over together. The chemistry part I would usually have to write. The more general part, he was a much better writer than I was.

VM: I was thinking of the days before word processors and memory typewriters and photocopiers, it was more difficult, wasn't it?

EB: I guess we had photocopiers, fairly early. There was one in Donner, not in our lab., but one in the library.

VM: In the old days?

EB: Yes. I remember there was an early Xerox down there, before it was a household name; it would be interesting to know when it was. I asked the broker about, would that be a good company to buy; he didn't know anything about it. I didn't buy any a — that was probably a mistake; that was when it was 10 and it probably went to 100 or more. That was before it was a common word like Kleenex or Coke.

VM: Generally speaking, did you have good equipment?

EB: I think we had pretty good... For better or worse, I wasn't like Mel (*Melvin P. Klein*); I never wanted a lot of high tech. equipment but we managed to buy a couple of pH-stats of our own, after using Neilands', bought from Cannon; he came in and worked on it. It seemed like a sophisticated piece of equipment at that time. We had a Beckman (*spectrophotometer*) that had a sample changer on it that went back and

forth. In fact, I think I may have made a suggestion that they make something like that. When Hiromi was first doing (*this work*), taking a reading here, then pulling it there, pulling it there, running it down, then doing it again. We then got this one which was on a recorder, and you could get the slopes. That was tremendous. Nowadays that wouldn't be considered high tech. at all. Then, the Beckman was a work horse at one time.

VM: What did you do about library facilities? Donner was adequate for your needs and you didn't need to go much elsewhere?

EB: Donner and things down at the other end of the campus for things that were more psychology. There was never any problem with the library. Donner was pretty good in those days. It's only in more recent years that they've gotten this policy of journals on more than one place on the campus, probably on too many places. I maybe overstating it but they are so expensive. It used to be that you could find JBC (*Editor: the Journal of Biological Chemistry*) in Donner, the Main Library and might even have found it down in the LSB library, several places at least.

VM: So in those early days, you really didn't need to go down to the other end of the campus much.

EB: I did, because I was working with Krech and Rosenzweig. People would go down to LSB quite a lot, I think, and use that (*Biology*) library. That wasn't that afar.

VM: Did other people in the group form associations in other departments, the way you did with Rosenzweig and Krech (in Psychology)?

EB: Some did. I don't think, I don't know how many did, you'd have to ask them. I think it was certainly encouraged. Al may have formed some with the people down in the photosynthesis area; I think he had a joint appointment for a while. When it was in terms of the time frame that you are talking about, I wouldn't know.

VM: So you felt by the time you had been in the pace a few years and were well established that you were pretty much an independent operator, you did whatever you thought fit? And he encouraged you?

EB: Yes, we wrote grants, and Calvin had to sign-off or write a letter supporting it. There was no problem. It wasn't quite the...Most of these grants went through Campus rather than through The Hill because there was less red tape involved at that time. Subsequently, there got to be some problems. Again, the time frame is somewhat different, but it seems to me that one of the things that we used to do that has been subsequently lost was there was usually a certain amount, a pot, of money for postdocs. They were kind of passed around to different people from year after year. In other words, let's say Dick had a person the last two years and I hadn't had anybody, then Dick wouldn't get someone and I'd get someone, or Al would get someone. The other thing you were asking about, equipment: there was usually a general sharing of ideas of what kind of equipment was needed and what the budget was going to be, it wasn't like now, I think: there are more unilateral decisions made to buy us $60,000 piece of this or that which may or may not do more than gather dust. I know there's more than one piece up there (*in the attic*) that hasn't done much more than gather dust over the years. Somebody thought it was real cute, certainly a few years ago.

One of the things, again, post-moving into the Round House, that I always felt was kind of a decline, was when we to give up the downstairs storeroom. Then, everything

was all parcelled around upstairs (*and in the halls*). Up until that time, everything was downstairs; if you wanted a beaker, you went down and a beaker, if you wanted this or that, and we had storekeeper; and we lost a certain amount of cohesiveness when that happened. That was partly to make room and partly because of budget, and so on.

Then, of course, with this Tiger Team; I don't know if anyone has told you about the Tiger Team recently...

VM: No, I don't know about the Tiger Team.

EB: Yes, you probably don't. A few years ago the Department of Energy, of course, was coming under great criticism for all their contamination. So the then, guess it was Watkins, the ex-Navy guy (*who was head of DoE*) decided he would take charge. So he had these Tiger Teams that went out to various places, including ours, and how much money it cost the lab. and how many days to clean it all up? Some of it was necessary, of course, but they had to throw away oodles and oodles of stuff. Talking about Marie (*Alberti*), she was kind of cast in the middle of that. One day they would come along and tell you that these things should be on a shelf separate from these things, or they could be on the shelf with these things, and then they would come around and say no, they can't be on the same shelf, they have to be on a different shelf. I think I never saw Marie come as close to breaking as with that. She worked real hard on it (other people did to) but her efficiency, she was the one that got the brunt of it, and, fortunately, she has come with us on —Millie has organised a number of trips to benefit Mono Lake and fortunately one of these trips came up so she got away from it all, near the end.

SM: During what period was this?

EB: This was in the last few years.

SM: That recently?

EB: But now they expanded the Health Safety group up in the lab. (*LBL*) to 110-120 people up there (a huge number) and they have developed all these protocols of how you gotta do stuff and, if you have something coming out of the system and if your procedure says to mix A and B, and you have it written down, you are probably all right. But, I may be exaggerating a little bit but not too much, but if your protocol doesn't say you mix A and B and then you go ahead and mix them, you may be in trouble. And, of course, the storage business: of course, some of that's legit, but it seems like lately they've slacked off. I don't want to say anything to Marie but her desk is somehow the messiest lately. They came into your office and you had to make sure that you didn't have anything up high, like those books that might fall on your head. That wouldn't be safe up there. Mel (*Klein*) managed to somehow escape the... The problems with his office — I guess he is just moving now, or has moved his stuff, because today when I was poking my nose in there, I think I saw Marie and someone else looking into what was Mel's office. I didn't look but maybe they are painting it or something, I don't know.

VM: In the days when you started and for many years after that, presumably safety and things of that sort were differently regarded?

EB: Differently regarded, yeah. Nowadays if you should throw one atom of carbon-14 down the sink you probably would have problems.

VM: But there probably weren't too many accidents, were there?

EB: No.

VM: Nobody died?

EB: There was a lot of — a few months ago there was a lot of concern about experiments that may have been done unknowingly to people without their permission at LBL, with radioactive isotopes. I don't think carbon-14 ever came out, but if it did, I can see Bert maybe having problems. I think they were more interested in those with the heavier isotopes. In retrospect, it's easy to say, "Well, you shouldn't have given that person 10 microcuries of plutonium", but you didn't know at that time.

VM: After you came back from Copenhagen in '54 you actually spent the whole of the rest of your experimental life in the lab.? Did you ever go away again?

EB: No.

VM: And now you've retired? When did you retire?

EB: I don't know. In terms of pay, about 8-10 years ago. But then I had part support from down in Tolman until two or three years ago.

VM: Do you still retain an office in the building?

EB: Yes, I'm getting ready to move out of it, though. I have taken out some stuff, and as I was telling Sheila we have been involved in cleaning out the downstairs, so when we get that cleaned out I'm going to bring this stuff from the office home. Then I'm going to hear some more static.

VM: Did you ever think of moving to one of the academic departments?

EB: The opportunity never came along and I never looked very hard. This was nice, you know, until 10-15 years ago. You could get a grant and, between that and the support you got from the Department of Energy, you could run a pretty nice program.

VM: Aside from grad. students, have you done any teaching, or much teaching?

EB: A little bit. Occasionally down in Psychology there were a couple of semesters when I taught a "chemistry" course or whatever you want to call it, for psychologists: talking about receptors and things like that there. That was probably 20 years ago.

VM: You haven't missed not having a lot of teaching?

EB: No. I kept busy enough without it.

VM: You know, that's excellent. If one can continue to be active all the way through, that's great.

Well, shall we call it a day?

EB: Yeah, I gotta go.

VM: Thank you very much for coming and thank you telling us so much.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name EDWARD L. BENNETT

Date of birth NOV 20, 1921 Birthplace HOOD RIVER, OREGON

Father's full name LEITH SMITH BENNETT

Occupation BUSINESSMAN Birthplace NEW YORK STATE

Mother's full name SADIE MILDRED NOYES

Occupation HOUSEWIFE/LIBRARIAN Birthplace ASTORIA, OREGON

Your spouse MILDRED J. GNTSCH BENNETT

Occupation NUTRIONAL SCIENTIST (HOUSEWIFE Birthplace BALTIMORE, MD

Your children ANITA, REID, KEITH

Where did you grow up? HOOD RIVER, OREGON

Present community BERKELEY, CA

Education B.A - REED COLLEGE, PORTLAND, OREGON
PH.D. CAL TECH, PASADENA

Occupation(s) NEUROSCIENTIST - RESEARCH

Areas of expertise LEARNING & MEMORY - ANIMAL MODELS

Other interests or activities CONSERVATION/ENVIRONMENT
HIKING, SKIING, GARDENING.

Organizations in which you are active SIERRA CLUB - SF BAY CHAPTER
BERKELEY WATERFRONT COMM UNTIL DEC - 1995 (8+ YEARS)

Chapter 9

NING GIN PON

Berkeley, California

May 29th, 1996

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

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

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

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

VM: Where was that? Here?

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

VM: In what capacity were you working with Li?

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

VM: As a graduate student?

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

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

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

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

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

VM: These were Calvin's medical samples?

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

VM: He said that openly, at this meeting?

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

VM: Which old building are you referring to?

NP: The old chem. building.

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

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

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

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

VM: He'd had enough, perhaps.

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

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

NP: Yes, by the time I interviewed. No, I guess, he wasn't rotund and he had already had his heart attack. He said, "Oh, you have a background in protein chemistry and we could use a guy like you". I said "Oh". I didn't even know what photosynthesis was — by the way, I still don't, but that's beside the point. And I said "Well, what's this interesting thing?" And he said "Well..." He had already studied, in '54, by then Alex Wilson had gone through, so that must mean they had...which is called *Path XXI* — that is the paper that got the Nobel Prize, *Path XXI* in the JACS. Anyway, he said "Here's another avenue we have not looked at, what happened to the path of carbon in protein". So he brought up the idea that some woman in Russia, Boichenko (large woman), found out that actually CO_2 is assimilated much faster into proteins if you use blue light (I think I've gotten the story straight), whereas in red light CO_2 goes into the carbohydrate assimilation. You can correct me if I'm wrong; I think that's the case. So the next thing I knew I was down some theatre place looking for blue and red filters. They supplied the huge lanterns that you project on the stage, with different lights. I went down and got all these wonderful bluish filters, red filters, all kinds of things. Actually, I even ran the spectrum: just to be sure that we'd isolated the right colour we would put a copper sulphate solution of some kind just to narrow down the band, so to speak.

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

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

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

NP: Well, actually not. He managed to slip in the others in the process but, in the course of all this, I actually did some work. I said to him, here's something very interesting, the assimilation of C^{14}-labelled amino acids into *Chlorella* does not reflect the same specific activity and the activity might be in the protein once it is incorporated. You might have a pool of amino acids and yet, when this pool is assimilated you would expect that it would have the same specific activity as in (*protein*). It turned out that it wasn't that. I said that was interesting: does blue light affect it or red light affect?. It was at that stage, he (*Calvin*) said there's a guy by the name of Jacques Mayaudon who is working on carboxydismutase at the time

VM: He was here, in the lab.:

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

VM: You are running ahead of me a bit. When you started working, you were working on the incorporation of hot CO_2 into proteins? With the different lightings?

NP: Not always hot CO_2, but hot CO_2 and hot amino acids. We have two different ways of getting the proteins into *Chlorella*. (*Editor: this must be in error — for "getting the proteins into Chlorella" read "getting the amino acid into Chlorella".*)

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

NP: Exactly.

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

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

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

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

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

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

VM: In ORL?

NP: In ORL, the Old Radiation Lab.

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

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

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

NP: Is he still at Tropical Products?

VM: No, he's retired now.

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

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

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

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

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

NP: Eventually yes, but only because, let's face it, I finally got together this paper that had to do with the effect of the activation of the enzyme with CO_2 and magnesium.

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

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

VM: On the enzyme?

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

VM: We haven't seen him yet.

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

In the meantime, they, Jacques Mayaudon, Clint Fuller, Al Bassham, all the big guns said "OK, we don't have ribulose diphosphate; you can't buy it". So, they simply took a lot of algae and kept running chromatography all the time and choosing this diphosphate spot on the chromatogram. You know where that is. Then you keep eluting, and you keep eluting tons of stuff, so we take the diphosphate region (*on the chromatogram*) and then we isolate from *Chlorella* , I think it was *Chlorella* they first used sonication to break it open and with a cell-free preparation and showed that cell-free preparation will convert that particular diphosphate in the presence of CO_2 to PGA.

VM: That's what they had done?

NP: Yes.

VM: Before you got involved?

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

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

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

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

VM: "Why don't you do what", did he say?

NP: Check on Jacques.

VM: In the sense of rerunning what he did?

NP: Essentially that and other things. At that moment, Jacques managed also to destroy the centrifuge and that really convinced Calvin. So he said to me "You'd better take charge, or something". So, Jacques left and there I am, stuck with the problem. Because I had all intentions of returning back to the assimilation of C^{14} labelled things into proteins, because it was still getting kind of interesting.

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

NP: No.

VM: No; I thought perhaps not.

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

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

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

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

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

VM: Calvin didn't read it either?

NP: I don't think so.

VM: It could have been blank pages!

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

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

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

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

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

VM: As an edible crop?

NP: Oh yes.

VM: Even though you don't like it.

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

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

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

VM: Formally, he's right.

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

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

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

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

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

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

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

VM: That was much later.

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

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

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

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

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

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

NP: I can't remember 1955.

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

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

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

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

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

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

SM: Yes: he even died.

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

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

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

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

VM: You did that?

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

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

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

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

NP: Because I was just a graduate student.

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

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

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

NP: You sure?

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

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

SM: It was Rod.

NP: It was Rod?

VM: Yes.

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

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

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

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

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

VM: And there was the carpenter, Ralph Norman.

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

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

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

VM: You should have been on your knees.

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

VM: How did Warburg treat him?

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

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

NP: you mean.

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

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

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

NP: I thought you came at the same time.

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

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

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

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

NP: He was a fun guy, actually.

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

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

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

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

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

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

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

NP: Hamamelonic acid?

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

NP: What did I do with Bob?

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

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

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

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

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

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

VM: Yes, I remember him.

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

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

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

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

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

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

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

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

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

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

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

VM: I remember the name.

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

VM: Did you work in the daytime then?

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

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

NP: I am trying to think who...

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

NP: No, she worked with Gaylord Androes.

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

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

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

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

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

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

VM: We are in touch with her.

NP: She ever married?

VM: No.

NP: She was such a nice woman.

VM: She still is.

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

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

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

VM: No, that's fine.

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

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

NP: In fact, I think, we were aiming at the Weizmann but was simply enriched, it was not even that pure. If you could put pure O^{17}-labelled water in it, that would probably be enough, but if you have to look at some substrate that contained it, it would be impossible. You couldn't build a (*farm?*) big enough to jam everything into this machine. It really was, to me, a means of acquiring a nice beautiful machine, not the thing that was originally intended. It couldn't do it anyway.

VM: Were you involved later on in the oxygen stuff, in the O^{18} and the fluorine-18?

NP: No.

VM: Do you remember that it went on?

NP: No.

VM: The business of using O^{18} as a tracer and then bombarding it, do you remember that they had a tantalum strip, and that bombarded it in the cyclotron with protons and there was a woman called Ignored Fineman-Fogelström - (*Editor: this may have been Fogelström-Fineman*), I think, from Sweden who did that? You don't remember?

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

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

NP: No.

VM: Did you do much in terms of NMR?

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

(*Brief discussion of ESR*)

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

NP: I told Bob that I had discovered that if you treat carboxydismutase with these different chemicals , or with CO_2 and magnesium, and what activated it, we decided to make a more methodical, more systematic analysis of this question. We really didn't do such a great job, in retrospect, in spite of the fact that at the time we thought it was a great discovery.

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

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

carboxydismutase was really the CO_2 fixation enzyme, all these lovely plants and trees are growing on, how is it that it works so slowly?

You remember that Otto worked with Martin Gibbs so it's the Kandler-Gibbs antagonism, if you will, of why this or that. If you will allow me, we'll go back just a bit. The first question, aside from the fact that the enzyme is not capable of doing the thing that a real live plant can do, I think they were first considering the fact that if you give CO_2 to a plant, then if you follow through the way Calvin and Sam (*Aronoff*) and all these guys Benson, etc. said if you if you cleave it (*i.e. the hexose*) in half with the *Lactobacillus casei* enzyme, or something, you can then consider the top half the same as the bottom half. I don't know if you remember what happened there. It turns out that the top half was asymmetrically labelled compared with the bottom half. Subsequently, we had explanations for all of that. That was the beginning of the questioning of whether or not 3-phosphoglyceric acid was the primary product.

Then, they started to zeroing on the enzyme I was working on. Now, I didn't feel that I needed to protect it. There's an enzyme sitting in a test tube, it works OK, it didn't quite account for why a plant worked that well, sitting there you would require so much carbon dioxide to make this plant work in that test tube. You take 5% CO_2 whereas atmospheric CO_2 is 0.03%. Right then and there it tells you that it won't work very well in 0.03%. On top of which there's a thing called "the K_M", the binding constant (that's really a misnomer; let's just call it the K_M), and the K_M for CO_2 in the test of *in vitro* is of the order of 0.001 *M* (at least, of bicarbonate) whereas in the plant it is of the order of micromolar. So than and there it tells you that this enzyme (*carboxydismutase*) in the test tube is not the same as in the plant.

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

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

VM: Exactly, that was about...

NP: Even then, it was slow.

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

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

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

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

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

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

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

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

VM: That was clean?

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

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

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

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

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

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

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

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

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

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

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

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

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

VM: Absolutely, all at the same time.

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

SM: Simon was the good guy.

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

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

NP: STP...

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

NP: No telling what BP came up with.

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

VM: Did you speak German?

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

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

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

VM: Tell me more.

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

VM: Robin Ferrier was much later.

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

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

NP: Radioactive cyanide.

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

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

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

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

VM: Was it now?

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

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

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

VM: I don't remember.

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

NP: So that one — they were going to put my name on it. I said that I didn't want it, not because I suspected it. It's just that I hadn't done any of the work. They said "Well you're a good friend" and I said "But friends...(*indecipherable*)...really scientifically associated. Then Calvin came up to me and said "Why did you take your name off it; did you suspect there's something wrong? I said "No, I just didn't contribute". It turned out that the cold methanol killing and that got phosphorylated with P^{32} and that was the one with Inia's name on it, a PNAS article. I think Metzner had something to do with the way you killed the plants: you get a different CO_2 product other than PGA. Isn't that right? We) had suspected("we" referring to "Uncle Bob" [*Rabin*] and myself) that it was an active form of CO_2 that he's captured but I don't think it has anything to do with this hamamelonic acid.

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

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

VM: There was Hans Ullrich and Ulrich Heber.

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

VM: You were not a Sierra gallivanter?

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

SM: What does that mean?

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

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

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

VM: Not talking about photosynthetic matters?

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

SM: And they used to sing camp songs?

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

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

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

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

NP: We had all kinds of nice things.

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

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

VM: It was not unpleasant.

NP: No — I don't deny that.

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

NP: When I returned from retirement.

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

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

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

NP: Oh! Naomi Levy, yes.

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

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

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

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

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

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

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

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

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

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

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

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

VM: When did you actually finish your thesis?

NP: '60.

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

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

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

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

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

VM: What's a QBW? Very good?

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

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

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

VM: That was Hans Kornberg.

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

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

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

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

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

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

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

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

NP: She almost burned down the building, eh?

VM: I don't know about that.

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

VM: You mean multiple pairs of shoes?

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

VM: A chemical rack.

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

VM: Was the building seriously affected?

NP: No.

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

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

VM: And you put it out?

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

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

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

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

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

VM: Was Karl Lonberg in ORL?

NP: Yes. Have you interviewed him?

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

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

VM: He was in ORL, was he?

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

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

NP: Cancer?

VM: I don't know.

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

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

NP: Three years.

VM: Working on he same thing, still?

NP: The worms. A worm runner.

VM: You were a worm runner?

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

SM: These worms were Planaria?

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

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

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

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

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

VM: That was Alan Jacobson.

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

VM: Was it in *Playboy*?

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

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

NP: '63.

VM: And you went to Riverside?

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

VM: So you did.

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

VM: You went to Riverside as what?

NP: As an assistant professor in Biochemistry.

VM: You stayed there how long?

NP: Oh, God! More than 20 years.

VM: Then you came back here?

NP: Yes.

VM: You formally retired from Riverside?

NP: Yes.

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

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

VM: Very vaguely.

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

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

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

VM: Are you still doing that?

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

VM: Great.

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

VM: Which is where you ought to be.

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

(*Indecipherable exchange*)

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

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

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

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Ning G. Pon

Date of birth Dec. 18, 1925 Birthplace Oakland, CA

Father's full name Fook Pon

Occupation Laundry/Cook Birthplace S.F. USA

Mother's full name Chin Shee

Occupation ? Birthplace China

Your spouse Marilyn J. Pon

Occupation - Birthplace Iowa

Your children Malcolm Kendrew Pon

Where did you grow up? Oakland

Present community Berkeley

Education Ph. D. Chem

Occupation(s) Scientist

Areas of expertise Enzymology

Other interests or activities

Organizations in which you are active

Chapter 10

MARTIN D. KAMEN

Montecito, California

May 31st, 1996

VM = Vivian Moses; MK = Martin D. Kamen

VM: This is a conversation with Martin Kamen on Mat 31st, 1996 in Montecito.

I wonder whether we can start the conversation by your telling me about some of the work that you did with Sam Ruben on photosynthesis before the war, I guess.

MK: He (*Ruben*) came over and he thought we could do something with carbon dioxide, with the route of carbon dioxide in photosynthesis. At first, he wasn't very clear about it but after a while we got started thinking about it. Then, I joined him full-time. I was only there for a while, part-time, at the beginning. As things developed, I became a full-time devotee.

VM: What was your status at the time?

MK: I was the Radiation Lab. chemist.

VM: You were actually an employee? You were a graduate, and did you have your PhD already?

MK: Oh, yes. I was the only paid employee in the Radiation Laboratory.

VM: Oh, really?

MK: I got $1200 a year. (*Laughter*)

VM: Could you live on it?

MK: Oh, yes. That was a lot of money.

VM: What was Sam's position at the time?

MK: He was an assistant professor — no he wasn't, he was an instructor.

VM: In which department was that?

MK: Chemistry.

VM: Your own background is chemistry, is it?

MK: Yes.

VM: How did the two of you dream up this idea of getting involved in a biological matter?

MK: It's a long story.

VM: Please tell it, if you've got time.

VM: Chaikoff (*I. L. Chaikoff, Department of Physiology, University of California, Berkeley*) came over one time with Lawrence (*Ernest O. Lawrence, Director of the Radiation Laboratory*) and said he had thought about using labelled $^{11}CO_2$ in looking at the fate of carbohydrates in rats. Sam said, later on, that it was his idea but Chaikoff had glommed onto it. Anyway, after a while we began thinking what was the reason we were working with rats. Sam said let's forget about that and work on photosynthesis instead. Because, after all, it's a cinch: you and me together can do this in a couple of weeks! That was after three years. I thought about that and maybe several hundred experiments later we still had not figured out where the CO_2 went. We didn't have enough time. Anyway, that's how it started.

VM: Where were you working at that time?

MK: I was a Radiation Lab chemist.

VM: Up on The Hill?

MK: No this was before The Hill. Down in that old shack where the 37-inch cyclotron was located.

VM: That was the building that Calvin's group later occupied?

MK: No. they had a building of their own, I think.

VM: That came much later. In the meantime, they worked in that shack which was called the Old Radiation Lab. So you were actually working in there yourself.

MK: Yes.

VM: This would have been which year, roughly?

MK: 1936-1940, 1941.

VM: What sort of plant material did you use?

MK: We were working with Zev Hassid (*Professor W. Z. Hassid, Department of Biochemistry in Berkeley*) and we used barley and some other plants and then went to algae finally because we figured out that it was easier to work with algae than with plants, for various reasons. First of all, you have millions of algae, and have only one plant. But anyway one or two complications with...(*indecipherable*)...and we weren't getting very reproducible results so we went to algae. We learned a lot about how to

handle the algae and that sort of thing. (I'll get my voice under control here in a moment.)

VM: The C^{11} only had a half-life of 20 minutes or so.

MK: Twenty-one minutes.

VM: So, you had to work pretty fast.

MK: Twenty-one minutes is the half-life. You can work seven or eight half-lives.

VM: That's still only a couple of hours.

MK: Well, if you have some definite things to do, you can do them in a few hours. Of course, we couldn't do the important things but we got as far as we could with the short-lived stuff.

VM: How did you make the ^{11}C? Bombarded it in a cyclotron?

MK: We bombarded boron oxide. I had developed a procedure where the CO_2 went out of the gas space. We had a target isolated from the cyclotron, the beam came through an aluminium window and then the CO_2 was collected with an aspirator and taken over to the lab. to work.

VM: So it was actually gaseous CO_2 in the container?

MK: Yes, most of the recoil activity comes off as CO_2 from boron oxide.

VM: How long a bombardment did you have to do to get enough stuff to work with?

MK: Ten minutes.

VM: Oh, as short as that?

MK: Well, the half-life is only 21 minutes, so you are already pretty far up on the saturation scale in ten minutes.

VM: Did you have to take this stuff in a great rush to the lab.?

MK: Well, if I walked to the laboratory it took me 2 minutes; if I ran, one minute and a half! So, I usually ran at the last minute so Sam thought I was really serious about things! But it didn't make much difference with a 21 minute half life.

VM: What was the nature of the experiments? What did you actually do at that time?

MK: We had a desiccator — we had plants in there — and we tried out to find out where the CO_2 went. Of course, after six or seven or more experiments we had no idea where it was. Because we didn't use short enough times. We thought maybe 8 or 10 seconds was enough but it turns out that it's even shorter than that. By the time it had gone 10 seconds already, it was distributed among all the products of photosynthesis. So, we didn't have any definitive notion of where it (*the activity*) went but we finally figured out that was in a compound which was heavily oxygenated and polar and had

hydroxyl groups in it. But we couldn't isolate it out of the product itself (*although*) we tried. It takes days and we didn't have that time.

VM: How did you try to isolate it, by traditional chemical means?

MK: I went to the library and looked up all the means there were. It's a long time ago and it's hard to remember. I tried about all the things I could think of that would bring different compounds out. Of course, we (*indecipherable*) everything so we had no idea what it was.

VM: It was before the days of ion exchange or anything like that.

MK: Oh, yes. Before the days of chromatography, that would have helped us a great deal. We had nothing at all except traditional methods which means precipitation, washing, taking hours to get things to the point where they had activity. That was our criteria: that after reprecipitation we still had activity.

VM: Everything had to be timed, presumably, because you had to keep track of the half-life of this isotope.

MK: We had a chart and we kept track...it wasn't difficult at all. We could only work about seven hours so we ran pretty fast.

VM: How many years did this go on?

MK: Three years. We did thousands of experiments, whenever the cyclotron was running, that is. I took care of that part of it. I devised the procedure for making the CO_2. I think Sam started by coming over and putting boron oxide in the target and then scraping it off. Of course, all the carbon came off as CO_2. I designed a target in which all the gas space was labelled, then using an aspirator we were able to get the activity off in a matter of a few seconds.

VM: Luckily, that was easy, otherwise, if you had to spend time on that it would have been hopeless.

MK: I suppose I should be dead by now from all the radiation I took but I seem to be doing all right.

VM: Later on, you found carbon-14.

MK: Yes.

VM: What set you looking for it?

MK: Well, Harold C. Urey was talking about how there wasn't any good tracer for any of the important biological elements: hydrogen, carbon, nitrogen and oxygen. (*Indecipherable*) So, Lawrence called me in and said "Find me something with a long half-life".

VM: Just like that: "Find me something"?

MK: He (*said*) "I know where there is a possibility of one". I went to carbon first and found C^{14} there. Of course, this has got a half-life much longer than it should have, 3,700 years (*Editor: actually 5,700 years*) and it takes an awful lot of carbon-14

before you see it at all so it's hard to find. But anyway, I made a protarget up which had all kinds of complications. The beam is several watts of energy and that's hard to dissipate in a vacuum, even with water cooling, so I made some protargets which stood up to the beam and got a carbon which had enough activity so we could see it. It was hard to make with deuterons; of course, with neutrons it was no trouble at all. While I was knocking myself out trying to make it with deuterons, it was being made in enormous quantities with the ammonium nitrate which was sitting around the (*cyclotron*). We had some ammonium nitrate there and I remembered.

VM: Can I adjust this (*the microphone*) a bit better so that, we can clip it on this side. Off we go.

MK: You live in England? How lucky you are.

VM: Where were we, I've forgotten, can you remember? Perhaps I'll replay it.

You were saying about the ammonium nitrate which was stacked around the cyclotron.

MK: I had that sitting there as a sort of forlorn hope. I didn't expect much from that at all.

VM: You put it there deliberately in the hope that something would happen.

MK: Yes. The neutrons come out in a diffused manner from this large machine and there's no focusing. So, we had a very diffuse beam. It turned out that there's a very high cross-section in this reaction, neutrons on nitrogen, so it came out that we had an awful lot of carbon that we didn't know about. I remember that after we had spent so much time trying to get the carbon-14 from the deuteron bombardment, because that's the way we did it, then I remember just passing some CO_2-free air through some of these tanks (*of ammonium nitrate*) and getting enough activity to really swamp anything we had before. The reaction with neutrons and nitrogen took over.

VM: What were you bombarding with neutrons?

MK: Ammonium nitrate in solution. We had water tanks also. But mostly I just had ammonium nitrate in solution, and there was, you know, we had to (*indecipherable*) where most neutrons are. These were 10 gallon tanks with acidified ammonium nitrate, which leaked, so after a while I was told to move these tanks. So I took what was left to the Rad. Lab. (*Old Radiation Laboratory*) and ran some air through them. We had enough activity there to keep us going for...we had microcuries. It would have taken us years to get that same kind of activity out of deuterons on carbon.

VM: Did you begin to use the C^{14} for studies on...?

MK: Yes, we had Andy Benson there at that time. He was "undesirable", he was proscribed by the Army (*Editor: he was a conscientious objector*) so he had time free. He was given the job of seeing where the C^{14} went.

VM: Even in those days when you had very little of it?

MK: Yes.

VM: I see. We're going to see Andy Benson next week.

MK: He can tell you about that.

VM: You discovered C^{14} in about 1940, I think, didn't you?

MK: It was in December of 1939 and I wrote up a thing and Sam and I published a note about it in 1940.

VM: I read an article quite recently describing you sitting up one night waiting for the thing to happen. I can't remember, somebody sent me a popular article in a magazine.

MK: That was the time I sat up for three nights and ran the cyclotron, trying to get this thing. It was too long a half-life by far; having it so long in this part of the periodic system. You know, (*indecipherable*) half life which is at least ten orders of magnitude longer than it should be. The whole thing is crazy anyway; nobody has ever understood why all this (*indecipherable*). It turned out that carbon had a much too long (*half life*); sulphur-35 is made exactly the same way, from neutrons on chlorine-35, the same reaction: it has a 87-day half life.

VM: Is that more or less expected?

MK: That's expected but this (*the half-life for carbon-14*) was not expected. I remember all the arm waving and posturing that went on among the theoretical physicists to try to explain this. But, of course, they couldn't explain it either and nobody could ever really explain it. It's fortuitous. Anyway, there it is.

VM: How long did you go on using the C^{14} that you had?

MK: I never had a chance to use it because the war broke out and we were told to do something else.

VM: I see. At the end of the war...

MK: I wasn't there. I was a security risk, all the time, and I was considered very undesirable. In fact, I was drummed out of Berkeley for that reason. I was really a liability to everybody concerned because I was very liberal in my viewpoint and the Army was very conservative. So if you spoke in a liberal fashion, they thought you were a security risk. I was a security risk all the time. I wouldn't dwell on that; that was 50 years ago.

VM: What did you do, during the war years?

MK: During the war years. I was in charge of making all the radioactivity that the cyclotron could do for various people like Don Yost, (*indecipherable*) and so on. I remember one time we made radioactive chlorine and it was thousands of curies. But I don't remember anyone putting a dosimeter onto that! I remember that there were dosimeters for a while but they were always off scale. We stopped worrying after a while.

VM: Did you have medical checks?

MK: Well, there were supposed to be some but they were really very few/

VM: Really, I suppose people didn't really understand the implications of what the damage could be.

MK: I am a living refutation of everything. I must have been bombarded with; I have more radiation than anybody ever took and never saw any result.

VM: And that's 60 years ago now, so you survived.

MK: Yes, I have children. Of course, the trouble may be in the second generation. If the children (*indecipherable*) marry someone else in the same generation, they may see some trouble but so far that hasn't happened.

VM: I'm glad to hear it! When you left Berkeley, did you come down here to Santa Barbara?

MK: No, after I left Berkeley I went to St. Louis and I ran (*indecipherable*) for a while and then from St. Louis I went on to other things.

VM: You wrote, of course, that book on radioactive methodology.

MK: I did that one summer.

VM: One summer, is that all it took you to write that?

MK: I was pretty full of stuff. I was the only living authority on it.

VM: And that went into more than one edition, as I remember.

MK: It went through three editions. I don't have a copy but I think it was three editions.

VM: I looked it up and the last one, as I remember, was about 1957. It got a bit out of hand, I suppose, after a while.

MK: They wanted me to keep on writing it, and I said "no". It's really a job to keep in touch with all of this stuff.

VM: Did you keep up with the photosynthesis work that was going on?

MK: No.

VM: You just went into other areas?

MK: Right. There was no point in trying to compete with a tribe like Calvin had; he had all the activity, he had all the money so I went on to something else.

VM: Did you have any contact with Calvin's lab. during those years?

MK: No.

VM: Are there other things you can tell us which would be relevant to what we're doing, which is really working around the Calvin story. But, I guess you cut contact with them. You know him, of course?

MK: Yes.

VM: But you were not a frequent visitor...

MK: No. I didn't see him at all because I didn't want to go near the Laboratory. I thought I had been badly treated by them and I wasn't going to go near them. In fact, I was badly treated by them but that's their story.

VM: Can we talk just for a few minutes about what Berkeley was like before the war?

MK: It was all built around Lawrence. He had terrific charisma. I think about it 50 years later it really wasn't there, but we thought it was there. He was a great leader of people. He wasn't much of a physicist. He was more interested in big science, which he invented. None of us were ever much interested in that: we wanted to do experiments and get some results from this. But always he wanted bigger things, bigger and better. So as soon as he got one cyclotron, he would build another one. That went on until he went on to something else. We never had a chance to do any experiments that had to do with science.

VM: Where did funding for research come from in those days?

MK: Research Corporation and various other sources. Lawrence wasn't around very much — he was out raising money.

VM: Did it come from private sources?

MK: Sometimes. Mostly from the Research Corporation and other places but I have no idea where it came from. He supported the laboratory by going out and raising money.

VM: Were you personally, and the people that you worked with, tight for money?

MK: We were always tight for money. We never had very much.

VM: Did you have other people working with you, graduate students?

MK: No. I wasn't allowed to have graduate students because I wasn't on the faculty in Chemistry. So Sam had them.

VM: It was probably before the days of postdocs., or more or less it was.

MK: I guess so, they called me a postdoc.

VM: But you were actually an employee of the Lab.

MK: But. I was still a postdoc; I had no tenured position.

VM: Berkeley must have been a rather peaceful place at the time, much smaller than it is now.

MK: Oh yes, much smaller. I get lost going around there now. I can't identify the buildings. Most of the ones I knew are gone.

VM: Were there many wide open spaces on the campus?

MK: Oh, yes. It was much more pleasant then than it is now.

VM: Had you yourself been a student at Berkeley?

MK: No. I came from Chicago.

VM: Did you get your PhD in Chicago?

MK: Yes.

VM: OK, I think that's perhaps as much as we can really expect from you after all this time. Unless you have any...

MK: Read the book (*Editor: Kamen's autobiography of the period "Bright Science; Dark Politics"*).

VM: "Read the book", indeed; I've read the book. But unless you have any other bits and pieces...

MK: I've written a sequel to it, which may or may not see the light of day, but it's being processed by somebody. I. Robinson in Oregon has the text and he says it will come out soon.

VM: What's is going to be called?

MK: The first book was radiant science and dark politics. Now it's all radiant science and no dark politics.

VM:. Thank you very much.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Martin David Kamen

Date of birth Aug 27, 1913 Birthplace Toronto, Canada

Father's full name Harry Kamen

Occupation Photographer Birthplace White Russia

Mother's full name Ursula Rabber

Occupation Birthplace Lithuania

Your spouse —

Occupation — Birthplace —

Your children David (son)

Where did you grow up? Chicago

Present community Casa Dorada, Monterey, CA

Education BS, Ph.D. (Chicago)

Occupation(s) academic

Areas of expertise Botanical microbiologist, nuclear chemistry

Other interests or activities Music

Organizations in which you are active —

Chapter 11

OSMUND (OZZIE) HOLM-HANSEN

La Jolla, California

June 3, 1996

VM = Vivian Moses; OH = Ozzie Holm-Hansen; SM = Sheila Moses

VM: This is a conversation with Ozzie Holm-Hansen in La Jolla on Monday, the third of June 1996. The first thing I have to ask you, then, is how come that you got involved in the Calvin group? How did you ever get to Berkeley in the first place?

OH: I got my PhD in Wisconsin in '54 in chemistry/biochemistry with Folke Skoog, got a Fulbright award for postdoctoral work to go to Norway to work with Brorud (*correct spelling?*) and during that time I shifted my allegiance a little bit and I worked down in the chemistry building at the University of Oslo. During my time there, one of the visitors who came through the lab. and gave a very interesting lecture was (*Arnold*) Nordal, the discoverer of sedoheptulose (*as an intermediate in the biochemistry of photosynthesis*). He had just come from Calvin's group. This was my first exposure to all the great things happening out in Berkeley. I talked to him quite a bit, did a lot more reading. Toward the end of the year I was planning to spend about a month travelling all through Scandinavia on a bicycle. One day I got a letter from Calvin, who had been in touch with Folke Skoog and who was looking for somebody with phycological interests and abilities and Skoog had given him my name. And he said "would you like to come out and join us?". So, I quickly packed my bags, cancelled my tickets for my month's stay Scandinavia and headed for Berkeley.

SM: Which year was that?

OH: 1955.

VM: So, you arrived there at what time of year in 1955?

OH: It was probably August 1955.

VM: You had never met Calvin before then, never seen him"

OH: Never seen him, never met him. We had had probably two or three exchanges of letters while I was in Norway. I remember the first time I met him we were standing down in the ORL which is no longer there — remember that great big table with the white top of some kind, where we used to spread out about twelve chromatograms? We were having coffee and he came in and I met him there. I remember the day well.

VM: By that time, you were, of course, already committed to him, you were there.

OH: Oh, sure. I had been working a couple of weeks.

VM: What impression did you get of him when you first met him?

OH: Not much the first time, because he never said too much, other than a warm welcome. You get the impression of a rather intense — of course, I was aware of the history of the guy, that he was eminently successful. I had read a lot of his books. At Wisconsin I had done a lot of work with chemistry and tried to purify salts. I knew his book on the theory of organic chemistry and all his chelate chemistry, so I was aware of the nature of the man I was meeting. He had a huge reputation and was very accomplished. You can't help but be impressed by that. At that time, he was in good shape. He was, I think, a lot slenderer than he had been some years previous. It was a nice friendly short, not much social chit chat, just a welcome to the lab. and that was about it.

VM: Before you got there (*to Berkeley*), you knew something of what the lab. did because of you talked to Nordal. Had you read up a lot of the papers and were you pretty familiar with the research?

OH: I was pretty familiar because I was interested in photosynthesis, I had always been interested in photosynthesis and photobiology.

VM: Was Andy Benson still there when you got there?

OH: He was just in the stage of leaving. He had actually left the lab. But I met Andy just about the time I came so we've been good friends ever since.

VM: When you first got to the lab. did you have a defined job to do, a defined set of responsibilities? How was it put to you?

OH: I did have the one responsibility of taking care of anything and everything to do with the phytoplankton, the biological collections, which included the succulent plants we were growing for sedoheptulose. I remember we had a little greenhouse in the back of ORL. So, I had to maintain and grow all the mass cultures, chemostats, all this stuff. That did not take too much time so the rest was just free essentially to do any kind of research that you wanted.

VM: By the time you got there the algal culture technology was up and running? People were doing it?

OH: It was in pretty good shape. We had the big mushroom flasks and the big batch cultures, the shakers. We added a lot of the tubes for the chemostat where we maintained the cells in exponential growth.

VM: Did they exist at the time that you arrived there?

OH: I don't think so. In fact, I think this was one of the impacts, if you want to be so generous as to call it impact, that I may have had during my time in that group. Before I came, they were basically chemists working the lab. , with the exception of a few people like Norris (*Louisa Norris, Rich Norris*) that preceded me. He was kind of a taxonomically oriented phytoplankton person and I came with a strong background

in some chemistry, biochemistry, physiology, but also a strong biological background. So I was aware of the overall functioning of cells, more than a lot of chemists, I think. I remember talking with Calvin very soon after I came about the nature of his experiments, when he talked about steady-state. By steady state they meant taking a culture of *Scenedesmus* or *Chlorella* in a little flask, or tube, and flushing it with nitrogen for sometime to eliminate all the CO_2 and then shooting in CO_2. I don't think you can visualise that at all as a steady state. Things are happening. I remember when I mentioned to Calvin that this might affect the pathway of incorporation of C^{14} he pooh-poohed it completely and said that that would never be. So we had a dichotomy of thinking. I think I had a better intuitive feel for metabolism of cells and the importance of all the environmental parameters: temperature, light, pH, nutrient concentrations, etc.

VM: And, of course, those were some of the things that we actually worked on in the years that you were in Berkeley, some of the factors...

OH: You remember when I left, Al Bassham had this beautiful lollipop. You remember the old lollipop experiment just with a simple glass vessel? By the time I left, he did this all by himself, with his people. I did not contribute to his instrumentation there. By the time I left, he had the lollipop very nicely instrumented so he could control almost everything, all the environmental parameters.

VM: Eventually, we will come to when you left, and so on. At the beginning, then, you were responsible for the algal cultures and the rest of the plant material. Who did you work with at that time?

OH: Who did I work with?

VM: Was there a technician who did the routine work on it?

OH: There was one person, Althea Vann, and she left sometime after I came in there, some months after I came. And then I had another technician by the name of Pat Smith...

VM: Pat Smith

OH: ...Patricia Smith

VM: The redhead.

OH: The redhead. She stayed for a couple of years. Then, Paul Hayes was there and he helped a lot. He helped a lot with the instrumentation, electronics, etc.

VM: But it was these ladies, Althea Vann and Pat Smith, who did the routine...?

OH: They did all the routine culture work, media preparation, things like this.

VM: In that first year, when you got there, who were your friends in the lab. ? (*Laughter*) Well, I know I'm digging.

OH: ...part of my marriage from 40 years ago! I think I remember nearly all my friends, but what year — if they were there in my first or second year, I'd have a little trouble being sure.

VM: But I can help you because I was there your second year. So, if I don't know them...

OH: There was Duncan Shaw...

VM: He was coincidental with me.

OH: ...second year, Utz Blass. You were second year or first?

VM: Second.

OH: Kojiro, from Japan. he German girl Stange...

VM: She was later still, in your third year, I think.

OH: Maybe in the first year I guess I just worked hard. (*Indecipherable*) We started all the camping trips and socialising later.

VM: You mean that you didn't go camping before you spotted my great enthusiasm?

OH: Not too much, no. I was new in Berkeley and was enjoying the life on Telegraph Avenue. We lived just two blocks away from Telegraph.

VM: Whereabouts did you live when you first arrived?

OH: Two blocks off Telegraph...

SM: Channing?

OH: Channing. That's right, Channing.

VM: Then you moved to Spruce, 1218. That's while we were there. We have friends now who live at 1216.

SM: Or even 1220.

VM: Or even 1220.

OH: That's right. The first night we were there we had a big party. We had the big Belgian...

VM: Chris van Sumere.

OH: ...who was very loud and the landlord almost threw us out the next morning. He said we had too noisy friends!

VM: Maybe you can remember the occasion. There was an occasion of a dinner party in which you and he (*Chris van Sumere*) and I all came in tuxedos. You had this lovely white tuxedo with a red cummerbund.

OH: That all happened a long time ago. Don't tell me wife or she'll...

VM: Do you remember there was such an occasion? In fact, it was for Chris' son's christening.

SM: We had him believe that in the west, as it were, on such occasions everybody dressed like that.

VM: So we did. I think we had the right clothes and we were the only ones dressed...

OH: That has faded from my memory.

VM: What sort of research work did you start when you got there?

OH: I spent quite a bit of time, probably a month or so, digesting everything that had been written in the lab. I just went through and read everything I could. I looked at the cultures and did some thinking. I started to do quite a bit of work with the blue-green algae which hadn't been investigated very much at the time; I was interested from my Wisconsin days. I actually did quite a bit of work with the blue-greens during my three years in Berkeley. They aren't too easy to work with as they are not unicellular. Well, there are some unicellular ones but we had filamentous ones, *Nostoc* and *Anabœna*. The reason I got involved with that — there was a spot below alanine on the two-dimensional chromatograms which sometimes got very hot, which we did not know (*anything about*). In fact, that's the one which that turned out to be a carbamyl phosphate. That was Pekka Linko, must have been there during my first year.

VM: He was, yes.

OH: OK. So I spent most of the first year with Pekka Linko, that's right, trying to find what that unknown radioactive spot was. It was carbamyl phosphate. The reaction is very intense, it's very strong; you get a lot of incorporation of C^{14} via carbamic phosphate synthetase reaction in blue-greens.

VM: That came out of the regular photosynthesis type experiments, lollipops, and all the rest of it?

OH: Yes. That one was also carried on in the dark to some extent. The reaction doesn't drop off nearly as fast as the RuDP carboxylation reaction.

VM: As a plant physiologist you were probably one of the first ones to extend the whole chromatographic thing away from the photosynthesis, as such, into dark reactions and other sorts of things.

OH: I wouldn't be that generous to what I did. There's a huge history on dark fixation and if you look through the old literature — Calvin published quite a bit on dark fixation, mostly in the tricarboxylic acids, organic acids. There was a lot known.

VM: As the time went on into the later period; how long were you there, actually?

OH: Three years, '55-'58.

VM: You left in '58? That must have been soon after we went, in the summer of 1958.

OH: I went back to Wisconsin.

VM: What did you do in the later two years, when you say the first year you had your nose to the grindstone, and it was only when my gang turned up that you branched out socially.

OH: I did less work and more play.

VM: We produced a lot of papers, as it happened, in those years.

OH: I did quite a lot of work with "Kishino" on the effects of salts. This was one of the questions that Calvin and I had about the importance of nutrient concentrations and getting away from the distilled water.

VM: I'm, not sure I know who this guy is.

OH: Kishino , the Japanese?

VM: Is that his first name or his last name?

OH: Nishida.

VM: Oh, yes, yes indeed.

SM: Kojiro Nishida.

OH: This was one of the points of disagreement between Calvin and myself. The importance of the nutrient solution in which the cells are suspended. Nishida and I published one little paper on it, on the different paths of incorporation of C^{14}, depending on whether or not the cells were in distilled water or in some kind of nutrient solution. Then I did a lot of work with Duncan Shaw with the dark fixation, mostly of blue-greens. I don't like to think about those days because we were pretty stupid and I think the DoE, the whole history of the....Maybe I shouldn't say this.

VM: No, no. Go ahead; it's all old history.

OH: I've been involved with the DOE on many other types of investigations, such as working out at Eniwietok where they had about 50-60 atom blasts. I think DOE showed a horrible disdain and lack of interest in protecting humans from radiation. I remember that Duncan and I — this is something that I would never do again — we were working in the dark room. In fact, a lot of our experiments we did in the dark room. We would have a boiling bath, we would have one of these...in fact, I still have one of these little jars over here...with 1 or 2 ml of liquid suspension We would dump, then, usually, 400 microcuries of C^{14}. That's a lot of radioactivity. For field work in the Antarctic or general oceanograph work I simply use somewhere between 1 and a maximum of 5 microcuries. Here we were using 400 in one small volume and then we would dump it into boiling methanol, which means we suddenly get a huge cloud of radioactive C^{14} which you a breathing?

VM: In a hood?

OH: No, in the darkroom.

VM: With no hood in the darkroom?

OH: No hood.

VM: No decent ventilation?

OH: I don't remember any ventilation in that darkroom in ORL.

VM: Was it dark so you couldn't see what you were doing?

OH: We had a little dim red light somewhere in the corner.

VM: That wasn't terribly good?

OH: At the time no one was particular. Everyone knew, we used to spend a lot of time arguing after hours about the radioactive safety labels, safety limits, how much radiation you, as a worker, was allowed, and how that compared with workers in Russia. In Russia they had three different levels, remember, depending on the importance of the person: Ordinary workers could get a huge amount of radioactivity, and very important personnel would get probably 1% of that. In our country we only had one (*level*). The DOE, back in those days fully recognised the problems, but always the bottom line was the financial. The bottom line is that if you make everything safe from the standpoint of health of the individuals then everything gets so expensive you can't afford it.

VM: And that was true, you felt, even in Calvin's lab. ? Did you try to institute safety measures?

OH: No, I didn't, because at the time, I guess you are young and you don't worry about those things. I would not work under those conditions today, though.

VM: Did you have a monitor with you, a Geiger counter monitor?

OH: Not in the hood, no. We wore little (*radiation*) badges. They are mostly for protection by the agency against possible litigation in the future. We still use those things but I don't have much confidence in them.

VM: You didn't have a clicking Geiger counter to warn you if anything got too hot?

OH: We did in the office. You remember you and I would be sitting up on the second floor of ORL looking down on the 37-inch cyclotron. (*Editor: It was the Crocker 60-inch cyclotron.*) Sometimes in the late afternoon, it came to 4:30 or 5, instead of going click, click, click, it would sound like a machine gun. That's when we picked up our coats and decided to head out and go somewhere else.

VM: As far as I know, none of us who were in that lab. in those years have suffered. Obviously, Martin Kamen made a point that he's sixty years beyond us.

OH: That's true. That's one of the things that I don't, one of the reasons that I don't worry about it. Because CO_2 — after all, you breathe out 4% CO_2 so it's a readily exchangeable atom. I'd be much more concerned if I had been working with cobalt-60 or radioactive calcium.

As a matter of fact, we did — I just remembered that one — A very interesting piece of work which I did which was never published. I worked with cobalt gamma radiation (cobalt-60). Was that while you were there?

VM: I don't remember.

OH: It might have been in my first or second year. They had a separate room, up in...I forget where it was; maybe up in Donner...which they had maybe the country's

biggest source of radioactive cobalt. You put the sample in and then go in the other room and operate the controls. I irradiated *Chlorella* with huge amounts of gamma radiation, quickly ran back to ORL and checked the standard experiments, the path of carbon in photosynthesis. It was amazing, neither the rate of photosynthesis nor the path of carbon in photosynthesis was affected for hours. Those cells grew, they became giant cells, they were alive weeks later, about ten times the volume and double the yield...the diameter.

VM: It had been a genetic effect.

OH: Sure. The ionising radiation had knocked out the mitotic (*mechanism*) and damaged the DNA, of course, which we know a lot about now. It was interesting to me. In fact, this has carried over into a lot of my present work. I do a lot of work in the photoregions with UV radiation and that only extends, of course, down to 280 nm. We are very much concerned about damage to DNA and photodynamic action of free radicals. This was a very good illustration that strong ionising radiation could knock out the ability of a cell to divide and not have any discernible effect on the path of carbon (*in photosynthesis*), in the rate of photosynthesis or the path of carbon, for hours to days.

VM: I'm not terribly surprised, because DNA is much more sensitive to hits because it's a template rather than the actual enzyme molecule.

OH: One of the biggest areas of research nowadays, since about 1987, has been the biological effects of UV radiation because of the ozone hole. If you look at the big bulk of that work, probably about 95% is being done with looking at short-term photosynthetic rates. I have done a lot of this myself where you measure rates of photosynthesis with or without UV-B or UV-A for anywhere from 4 hours to 12 hours.

VM: How do you measure it these days?

OH: Just CO_2 incorporation.

VM: Hot CO_2? Radioactive?

OH: Yes, C^{14}, sure. Nowadays we use liquid scintillation counters. Back in the Calvin lab. days we had to use Geiger-Mueller tubes and I also used the crystal scintillation counter, the predecessor of the liquid scintillation counters. In fact, that's kind of interesting: we had to go to Crocker Lab. , and they had a big crystal of sodium bromide, or something, and we put a sample into the crystal and count. Now, we have scintillation counters all over the place and when you tell students you were doing work on this problem before scintillation counters, in fact if you are telling them you were working in oceanography before there were any CTDs.

VM: What are CTDs?

OH: Conductivity/temperature/depth, it's a standard. Now they have disposable CTDs: you just drop something over and up a thin copper wire you get a complete profile of the salinity, conductivity and temperature with depth.

VM: All of those things in our lifetime; who uses carbon paper nowadays? (*Laughter*)

OH: I wasn't sorry to see that one go. But this work with the gamma radiation was interesting. Unfortunately, I never published it. I refer to it and I have to refer to one of the old quarterly ONR reports, which is impossible for anyone to get hold of, I think.

VM: I was thinking recently that one of the things we used to do while we were there, we used to go for lunch somewhere and I was trying to remember where it was. It was some sort of cafeteria somewhere on the south side of the campus. Do you remember where it was? We used to go and line up and get something there.

OH: Are you talking about the old hamburger joint off Telegraph?

VM: I had an idea it was a campus facility of some sort, but I'm not really sure. Part of the student union, perhaps? It doesn't ring any bells for you?

OH: No.

VM: I remember standing in line once with you and Utz Blass arguing about how to pronounce the name of those animals which look like horses and have black and white stripes. I said "zebra" and you said "zeebra". We asked Utz and he said "Of course, it's "tsebra".

OH: It wasn't on the menu, I trust.

VM: No, it wasn't on the menu, but there was some reason why we were arguing about that particular word.

OH: Don't remember that.

VM: You remember the seminars, however, on Friday mornings?

OH: I do. One point I wanted to make earlier; I remember — well, you were involved with this, too. We worked with Ning Pon isolating various cell organelles and looking at compartmentalisation of reactions and enzymes within the cell, which is still the most interesting problem. In fact, one of the reasons I was interested in that, and it's still important in a lot of work, is interpreting the data from C^{14} incorporation experiments. If it's newly-incorporated C^{14} it is deposited in the chloroplasts as a storage product and the respiration is in the mitochondria and you liberate the cold CO_2, then it's hard to determine the net rate of photosynthesis. Trying to correct for respiration is still a problem for which we have no easy answer in biology.,

VM: We did two papers on that. We did that one that you just mentioned. We also did another one in which we tried to look at the dynamics, kinetics, of hot CO_2 in and out from respiration and photosynthesis in dark and light. You remember: that was in the *Journal of Molecular Biology*?

OH: With the big lollipop set up. That's when you and I worked late one evening.

VM: Probably, yes.

OH: In fact, that night...I think about that set-up because it's a period when we were blessed with beautiful facilities. I remember for that set-up we had the C^{14} by ionisation counting counter, the oxygen was O^{18} circulating gas phase, something

which I never had the access to that kind of equipment since. I remember one interesting thing (you might want to delete this from the tape). Remember, we were measuring CO_2 with the infrared gas analysis and there's a little intake in that thing. We were using CO_2 for cooling. We had the IR down below the level where we were using the dry ice. We had the CO_2 flowing down, like the fog coming over the San Francisco hills, and it was affecting the CO_2.

VM: I don't remember that; maybe I've wiped it.

OH: I remember it very distinctly. I have always remembered that CO_2 flowing down.

VM: You couldn't actually see it, could you?

OH: Yes.

VM: You could see the (CO_2) fog?

OH: Yes.

VM: We used to live in an office, didn't we, on the second floor under the roof of ORL.

OH: That's right.

VM: I don't remember now whether it had a pitched roof, did it, in the office?

OH: I think it did, yes. It was overlooking Crocker Lab.

VM: It was remarkably untidy, I remember. There were three desks.

OH: Your desk! Mine was....I think mine might have been even better than yours.

VM: I think I have a photograph somewhere. Who was the third person who shared the office? Do you remember?

OH: I don't remember.

VM: Was it Nishida?

OH: I think there were rotators in there. Do you remember who occupied, who was in charge of the Crocker Lab. at that time?

VM: No, I don't.

OH: Hamilton.

VM: Hamilton?

OH: Doctor, MD. He had his own operation there. He was the one who died of thyroid cancer. I think that's a very classic case. His family, as I recall, took it to court and he was declared "death from accidental..." He died because of some job-related fooling around with radioactive iodine.

VM: And we were, of course, pretty close to that thing.

OH: Oh yes.

VM: When we used to go down into that underground counting room, the basement room, where we had all the Geiger counters, did the background rate shoot up there in the late afternoon like it did in the office?

OH: I don't think so. I think it was shielded.

VM: There was a lot of concrete round it.

OH: Yes.

VM: You were the guy who taught me the Calvin technology when I first arrived because you were a year ahead of me. We worked, I think, on the second floor and we had a bench in a big lab. outside the office, didn't we, as far as I remember. For general work. Obviously, we used the big facilities whenever we needed them but I think we had our work benches (everybody had their own work benches), as far as I remember this was on the second floor. I can't remember what the lab. was like; can you remember it, what the room was like up there?

OH: I remember we had all the apparatus. We had all the shakers and temperature-controlled growth facilities up there.

VM: Was the upstairs divided into rooms or was it a big open space?

OH: Upstairs, that's where we had the chromatography room with all the tanks where we used to walk around with the hood. We did have good precautions for the phenol and the propionic acid. We put on a helmet with air being circulated, compressed air.

VM: Did you use that? I think I never did.

OH: Of course. Phenol is nasty stuff.

VM: I think I've survived it; that's been a long time.

SM: You used to come home smelling of it.

VM: I remember one of the things that we worked on in the early days was on D_2O, growing *Chlorella* in D_2O.

OH: In fact, that effect was inhibiting the rate of sterilised mice, or something.

VM: We tried to grow it (*Chlorella*) in pure D_2O and we had great trouble and we never succeeded in getting more than about 60 or 70% as I remember. I think somebody else later on did.

So you were there...What made you leave, in the end? You were there only three years on a job which you presumably you could have held you indefinitely if you'd wanted to.

OH: Not really, no. I always visualised this as a postdoctoral training period, not a permanent thing. I had the offer to go back to Wisconsin as an assistant professor, where I had a very nice laboratory, so that was very enticing to me.

VM: When you were hired in Berkeley in Calvin's lab. , was there any discussion between you and Calvin as to how long you would stay?

OH: No. I assumed it would be of limited duration, two or three years.

VM: He never said that.

OH: I don't think we discussed that, no. The initial correspondence was such that — I don't think I saved the letters — but I went there with the full intent of just trying to do a good job, learning as much as I could, getting a good foundation in that work, and then moving on to a teaching faculty position. I have always liked to move around, change one's area of research, I think it's very stimulating. From my experience, as I looked at people who had stuck at research in one lab. in a non-teaching capacity, and a lot of the Calvin's group were researchers without any direct contact with the faculty at Berkeley, I think there are diminishing returns after some years.

VM: Why?

OH: For one thing, to get back to one of the points we mentioned earlier, the Calvin group had existed for such a long time and was so good for such a long time because it had a strong, very strong leader. I think if you are a good scientist I think you decide on what you want to work on yourself, you want to approach a problem yourself, you want to think for yourself. Once you are a member of a big group, like working in industry where they give you monthly work assignments in your mail box, you lose that individuality and imagination. In the long run, it's a great period for learning and broadening yourself but I don't think it's in the best interests of a potentially productive scientist to be a kind of a hired hand or to be subservient to a distinguished person for more than some three or four years.

VM: Did you feel — clearly you did. You felt that Calvin was a great influence in what you did?

OH: Of course. I learned a lot from him.

VM: You learned a lot but did you feel that you could have done whatever you wanted to or did you feel constrained?

OH: No, no. That's one of the good things about Calvin. He never negated or expressed dissatisfaction; he was very critical, so you always tried to do your best job. I don't think he ever said "don't do this or don't do that". He was always very receptive to any ideas.

VM: Anything you wanted to do, any bright idea that you developed or thought up, and you decided you would like to run with it, you could have done it?

OH: You could have done it, right. I think that's a mark of a person....I'm not surprised. I think any good scientist would have this inclination.

VM: One of the characteristics about him was that, once the main path of carbon was resolved, I think he was not after any particular target anymore and he was willing, maybe even before then, he was willing to entertain lots of other ideas on how the subject could develop. Maybe in the early days, and I'd have to talk to other people about this, maybe he was more single minded about what he was looking for,

certainly not towards the end. Did you miss the fact that you didn't have any of your own graduate students in Berkeley?

OH: No.

VM: Did you miss that?

OH: No, because that wasn't my role there. We were strictly hired researchers and as such when Calvin got other visitors like Nishida from Japan: I remember Nishida's English was very poor and Calvin saw him once, I think, and then handed him over to me to take care of and then the next time I saw him, I think, was at his farewell party! (*Laughter*) We never had any students but we got visitors to work with, to help start on their research projects.

VM: You didn't miss the absence of students?

OH: No. I had just come from my PhD at Wisconsin with a year of (*postdoctoral*) research (*in Norway*) and I wasn't in the mood for trying to teach or educate graduate students.

VM: You wanted to spend all day every day doing research.

OH: I was still in the 100% learning mode myself.

VM: What about the social life as it developed in the later period, in the second and third years that you were in ORL: the parties, the camping trips, and all the activities outside the lab.? We remember you as being *very* involved in all of that. Do you remember it like that?

OH: I do, I do. I look back upon those days a lot. Now I tend to work here in the lab. about six days a week, including almost every evening. I remember working in the evenings a few times with you when something really demanded extra effort. Most of the time we did a lot of fun things.

VM: You used to play a lot of sports.

OH: A lot of sport, a lot of camping, a lot of social life.

VM: How about the parties in the lab. , do you remember anything in particular about them?

OH: Not particularly, except that we always...we had the usual Christmas party. We had very good attendance at Christmas parties, we always had nice farewell functions when people left, Calvin used to have social functions at his house. I think, in looking back upon it, it's probably about the best functioning large group I have ever seen, very diverse personalities, different ages from senior research people to very young students.

VM: How do you account for it?

OH: I draw the analogy to this other group I was involved with for 22 years, the Fuching (*Editor: spelling correct*?) Research Group which was world famous.

(*Interruption*)

OH: The Fuching group, the leader of that was originally John Strickland who was a brilliant guy who came from England during the second world war, trained in chemistry. After the war he went to Nanaimo and he and Tim Parsons revolutionised biological oceanography through their expertise in biology, chemistry and biochemistry. He died when he was 49. We had a big group of about 40-45 people and we were left with about 7-8 senior people, like myself, so we ran it for one another for 18 years but never had one person who was **the** boss. We rotated chairmen so it was always musical chairs. We rotated the chairmanship and all the other functions of running a group. Eventually, the thing kind of just fell apart, even though we were all world known scientists with a lot of graduate students. It fell down from the conflict and problems — interpersonalities and it also fell down socially. Calvin's place, I don't think you ever had the possibility of this happening. I think everyone intuitively recognised Calvin as the dominant person and everyone had direct lines to him. You might not like everyone in the group but you never let it interfere with the overall functioning of the group.

VM: Were you ever aware of him not communicating with people except in the Nishida type of case where he felt there was a language difficulty. Did you feel — don't mention names if you don't want to — that there were people that he didn't like or that there were personal conflicts in the lab. ?

OH: He threw out one student, I forget the guy's name, who had done something bad, he just threw him out and told him never to show up again. Don't remember the person's name. Let me tell you one thing that bothered me a little bit about that time. It had to do with radioactive isotopes and you'll be talking with the important people so you can correct me or correct any mistakes.

VM: Get their point of view, anyway.

OH: Right. We had a graduate student in there by the name of Karl Knut Lonberg-Holm who was very bright and you'll be seeing him, I trust.

VM: I'm afraid we won't be seeing him this year but I am in touch with him.

OH: This was at the height of the Cold War and the Russians were setting off bomb blasts in Siberia. He took it upon himself to make wipe tests on automobiles in Berkeley and report the data to the local public broadcast radio in an effort to inform the people of what was happening worldwide. The story I got is that he was called into Calvin's office and Calvin was acting on behalf of higher-ups in Washington. He essentially gave him the order either cease and desist from any such activity or get out of my lab. He could not do both. This was one thing that I thought was very bad on Calvin's part. It's another illustration of however strong and powerful you are in the world of science, you are subservient to the people handing out the money in Washington. He had a huge lab. and he needed the money. That's one reason why he was so productive. Most of us write proposals, spend half of our time nowadays writing proposals. There, none of us worried about money. Calvin got all the money for all the equipment we needed, all the salaries, all the support facilities. He could not jeopardise that even for a good scientific public relations reason. I found that very disturbing. I thought he should have been big enough to tell the AEC that there was no interaction between what the graduate student did on the outside and what was done in the lab.

VM: Unfortunately, I think it's too late to ask him about that.

OH: Oh I think so.

VM: I don't think he would remember.

SM: He doesn't remember most things, unfortunately.

OH: Sure. But even today, pressures of all kinds, political pressures — I see it around Scripps (*Scripps Institution of Oceanography*) all the time. However high you go, the Chancellor's ear is going to be sensitive to the demands of the Regents who are very economically successful business people. It's a continuous circle, like the food chain of one person biting the other.

SM: I think, apart from anything else, until his (*Calvin's*) daughter, Elin, was arrested in the student revolts, I think it was in '68, (*Editor: it was 1964*), I don't think he really had any sensation of political awareness. He just did what was convenient for him and really seemed to have no political opinion. That hit him.

VM: Can I correct that? Those revolts were actually in '64 over the Goldwater (*Republican convention in San Francisco*). Rather earlier.

OH: I guess with that....I thought you might ask me my general impression of Calvin. You asked me my initial impression.

VM: Please; go ahead.

OH: I'll give you my summary after having spent three years there and I left with the firm, firm realisation that he was a brilliant man when it came to chemistry, a very hard worker, a man who was dedicated and, I think, focused on only two main issues in his life: his science, which was very encompassing (hard core science) and his family. He was a very devoted family man and very hard-working imaginative scientist. But you could never get him to think, or discourse, or show any enjoyment of any other activities which many of us enjoyed...sports, relaxation, social life, opera, music, baseball. I remember having lunch with him at these weekly organisation meetings over in the Faculty Club; the only thing he ever showed any interest in was science and then family. He is a complete blank to me in terms of all the other interests which many of us have in life.

VM: He did change a little after he got his Nobel Prize and was drawn into the Washington circuit. There he learned about another style of life and about other sorts of people and he became involved in politics, not party politics, of the Washington advisory groups. He changed a bit.

OH: I'm not sure that's a worthwhile goal.

SM: He probably learned how to handle it, rather than to have opinions.

OH: A couple of other impressions that I have of Calvin...one of the things I learned from was never to go far beyond the knowledge that you are sure of. He was absolutely, what's the word?, not tough, but he would cut anyone down. He was certainly smart enough and this was certainly true in the Friday seminars we had. There might be a professor from Germany speaking on chemistry but if he ever made a little mistake in his interpretation, then Calvin was really ruthless in chopping people down. I don't think he showed any compassion or any understanding of human nature or sensitivity.

He'd just whack them in terms of faulty thinking. So, you learned to be very careful with everything you said in terms of argument or discussion.

VM: But he also came out with some pretty way-out ideas which you could criticise to him.

OH: Oh sure. He liked that pretty well. (*Indecipherable in terms of meaning*) One of the good things about him was that he was very receptive to all kinds of ideas. This was exploring things. I remember the one time that Ning and maybe you were with a group, and Duncan Shaw was certainly in the group, we were sketching on the board the soot cycle — were you there?

VM: Yes, yes.

OH: ...which was a spoof on the old carbon cycle. I was very embarrassed and thought he would give us hell. I think he read it...

VM: We were about to erase it and he said we shouldn't., there might be something in it.

OH: I thought many times in the past that that's indicative of the man. He could, his imagination was very good and he did not immediately dismiss anything like that.

VM: Would I be correct in summarising your view that the success and the character of the lab. depended clearly on the leader...

OH: Of course.

VM: ...on the character and the intellect and the dedication of the leader, but also on the support?

OH: Well, sure. I think a group like that...he was always in the lab. and he'd come around at seven o'clock in the morning, he worked long hours. Another facet of his life, he always knew. I think, what everyone was doing. He would come around and talk to you and ask for the minutest detail. I remember on the experiments on the culturing stuff, if he ever saw a rubber tube rubbing up against the moving mechanical parts, you knew you would catch hell. I think he really got the best out of everyone through kind of fear of being chewed out. But he kept track of what you were doing. This is certainly one of the reasons why he was so successful in getting good work out of people. He showed an interest in you and he was there to talk to you and you often learned something from talking from him.

VM: Do you think that another important factor was the nature of the problem itself, it was such a nice clearly defined exciting problem? You knew where you were going and what you were trying to do.

OH: That's particularly true when he first started, for The Path. When he started there were all kinds of conflicting ideas. In fact, it led to people like Fager from Chicago quitting science, or quitting that kind of science, and becoming an oceanographer because they got on the wrong path. Once the PGA got realised that that was the first stable product, then the problems became much more diffuse. This is a very good point, Vivian. Having been an oceanographer for about 35 years, oceanography is so different. Nine out of ten times I hear an oceanographic lecture I keep thinking, what is the nature of the problem, what is this person trying to get an answer to? Most people don't have a good question or a good problem. In this case, there was a very

sharply described problem, namely, the first stable product of photosynthesis and what happened to it, the path after that.

VM: That provided an opportunity to develop the momentum for a group which could then run and become more diffuse without falling apart.

OH: One thing I wanted to say before, about the structure of the group and why it was so successful. Not only did you have a very strong man at the top who was thoroughly familiar with everybody and with the details of the day-to-day work, but you also had a small group of permanent people, like Al Bassham, Dick Lemmon and Paul Hayes, who maintained the lab. in terms of everyday functioning; they kept all the machinery running so Calvin was separated (*from that*) and he was the intellectual head. All the other details of running a lab. were essentially taken care of by this second stratum. Again, to go back to one of the other questions of why I left the lab. , I don't think the second stratum, it's hard to be your own imaginative scientist and being in the second stratum.

VM: I think it was at the time when you left. I think later on it became much easier because people had developed their own careers and their own reputations and because Calvin was much less in control later on because he was spread more and more thinly.

OH: But then the nature of...the work in the group, was much, much broader. Rod Park came, after I left (we overlapped for a couple of weeks or so), and he expanded the work into the biophysical structure of chloroplasts.

VM: Rod actually came before you left, did he? Was he nominally your replacement?

OH: Yes.

VM: I don't know where he is now, I must see if I can look him up.

OH: He's a big shot on the Berkeley Campus.

VM: No, I think he's left Berkeley.

OH: He's left?

VM: Yes, I think he's gone somewhere else.

OH: He might have retired, but he's a big shot sailor. He likes sailing big vessels.

VM: Marilyn will know where he is.

OH: I bumped into him here about ten years ago and wasn't he dean or something?

VM: He was. I had lunch with him in Berkeley two or three years ago, four years ago, but I think he has moved on. I'll find him.

OH: By the time you left, was there any talk yet of a new building?

OH: Oh sure.

VM: What did you feel about the role of ORL itself in shaping the group? Do you think the structure of the building was a factor?

OH: (*Laughs*). It was an old building. Wasn't it Al Bassham who had a little sign in his office about a young person who was doing Nobel Prize winning work in an old wooden laboratory, and they become famous, gets lots of money, building big new labs., and then they never do much after that.

VM: The last bit was that the third stage was that he showed visitors around his new lab. Al Bassham did have such a thing in his office.

OH: ORL was very good, I think, for producing work. You were in close proximity to people and it worked well. Everyone in the group, that included chemists, physicists, biologists, biochemists, were together essentially in one little old building. That's one of the objectives of Calvin's whole philosophy was to have people working closely together, increased communications.

VM: It's an interesting thing, I feel, of this question of the space in which you actually do your work and whether the space as it were comes ready-made or whether you have to build the space yourself, as they did in ORL. It was a very personalised building. They had arranged things very much to suit themselves in that building. It was very flexible: you could knock things down.

OH: Sure, this is one of the advantages of an old building — and old houses: you can move walls around. You can't do that in these modern buildings.

VM: When they built the new building...you've been in the round building, have you?

OH: I've been in it, never worked in it.

VM: What did you think of it when you visited it? Obviously, this was an attempt in a modern building to recreate the spirit of ORL. Did it leave any impression on you?

OH: I was there just for the two-day ceremonies up there four or five years ago. It was so big that I got no feel for it, really, about how well it might function in the same way that we visualised ORL. It would be very hard to maintain some of the attributes of ORL, which was much smaller.

VM: That's something that we will have to think about and we ask everybody what their impressions were of the new building. They vary, as you would expect. Do you think that given the structure of the earlier group that it could have lasted indefinitely, or do you think that sort of structure, with a great man and the growing group, has a limited life of necessity and sooner or later it must break up into something else?

OH: Like everything else in life, I think there's a productive period and then a demise and ultimately a disintegration. I can't think of any group that has really gone on for a long time. You get institutes, of course, where the directors die and new ones come in. But for a group, I visualise that there's a birth, a young developing period, a highly productive period, and then change.

VM: Do you think that the Calvin group went on for a good long time in its productive phase compared with others?

OH: I think it would probably be about the best I know about. After all, it lasted from the mid-forties to...I guess it's still going on.

VM: Well, it's no longer his lab.

OH: It lasted until the time Calvin retired.

VM: That's a period of forty years near enough.

OH: Oh sure. That reflects that one dynamic man who was recognised as the authority.

VM: It did change very much in character by the end of that forty-year period. It was no longer funded from a single source. People were already having to get their own grants, etc.

OH: I guess if you want to get another lab. , I guess something like the Salk Institute here (*in La Jolla*) might be comparable. It's one man, Jonas Salk, who was famous and built up a huge institute.

VM: But he populated it with stars, didn't he?

OH: Yes. He brought in stars.

VM: Calvin didn't do that. Whether he wanted to, or didn't want to, he didn't. He did it with much younger people.

OH: There's a big difference. Salk really became involved with art and philosophy and integrating religion and everything else into one functional system.

VM: Of course, Salk did that..

OH: Salk thought he was the spokesman for God.

VM: As far as I remember, Salk developed that lab. after he had his Nobel Prize. And, of course, Calvin did it long before he got the Prize. Can we finish by....

OH: Having been at Scripps Institution of Oceanography for 33 years and living and experiencing some of the politics of UCSD (*University of California at San Diego*) and SIO, I look back upon the time up in Calvin's group and I marvel at the good relations between however many we had, 30, 40, 50 people in the group which included a lot of social functions, camping trips, cross-country hiking trips and all, and in all that time I don't remember any bad personality problems anywhere in the lab. which interfered with any aspect of the work. Which is very rare. Everywhere I go now I see the personality conflicts and political gains and things having very adverse effects on the organisation of the institution, on productivity, how you spend your day, how happy people are. But thinking back on my three years in Berkeley I don't recall ever feeling frustrated or being annoyed or feeling that my work was suffering because of any personality problems with anyone in he lab. This is a most remarkable thing.

VM: If I may say so, I think it reflects the lack of hierarchy. I think there was the "boss" and there was everybody else. And as far as everybody else was concerned, there were the visitors who were not there for very long and there was a small group of people who were the permanent ones but there was no competition between them and Calvin. They were never in line for his job, as it were, and, therefore, they were not jockeying for promotion or position. I think that sort of absence of competition for status inside the group must have been important.

OH: One little chapter, which we have not mentioned on is: we also had Otto Kandler there during that time and we had the German couple, man and wife...

VM: Metzners (*Helmut and Barbara*).

OH: Metzners. Remember there was a lot of controversy for a short while on the possible C_1 product. In fact I think Calvin published a paper on that which he had to withdraw. Or he should have: I don't know whether he ever did it but it turned out to be a chromatographic artefact. I imagine you will be talking with some of these people.

VM: We would like to talk to Kandler and to Metzner, if we can get to them, and we are going to talk to Martin Gibbs (where Kandler had been) before we go back to England.

OH: Another interesting chapter was that during that period we had some pretty heated discussions with Calvin in his office about these results. Even though it contradicted what he felt was right, he again showed a very good open minded, scientific approach where he listened to it and did not tell us to just forget about it. It was a good open scientific discussion.

VM: I suppose as a result of the Kandler involvement, there was one meeting where all of us did gather in his office to thrash it out once and for all.

OH: I remember him coming up to me after a meeting and putting his hand on my shoulder and saying something about "it was a good meeting and I'm glad we got together". It continued on for a bit but again this was typical of Calvin. He's a true scientist. He wanted to get the answer and I don't think he cared whether he had been right or wrong. This is an important point. If you look at all of his publications, he made a lot of errors in interpretation. But they are not stupid errors. They were errors which were based on the best thinking of the time on the data available. And there's no harm in that. In fact, it's a good way of science progressing. He was not afraid of making an error, which a lot of people, these days, are afraid of making errors.

VM: I think the worst you can say is that he sometimes went into print a little prematurely.

OH: Oh, sure.

VM: I think he felt that there was competition from other people working in the same area and I remember at the time that there was concern that other people shouldn't get there first. But that's natural in scientific work. By and large it worked very well, I must say.

OH: During this episode with Otto (*Kandler*) and the Metzners, and even though it was against what he thought, he was very open minded and really showed that he was interested in the science and did not care about personalities. He took no personal involvement with this.

I can give you one example in oceanography where some person about 20 years ago was trying to prove that these little yellow-green cells in deep ocean water were alive. I was talking to him and he said if they proved not to be alive, he would be so depressed he would probably kill himself or something. He got emotionally involved. Which is not the true scientific... As a scientist, you should try to find a scientific answer; you don't want to get emotionally involved. In this I thought Calvin was very

good. He was always trying to get *the* right answer whether or not he was right or wrong.

VM: But it was important to him, like for any scientist, that *he* wanted to get the right answer. He didn't want the next guy down the block to get the right answer. It was important that they came from him.

OH: I don't know how much interaction he had with (*Daniel*) Arnon.

VM: There were people whose publications concerned him and, if you read the literature and read some of the reports that people have written, there was obviously concern about being beaten by others the need to get there first. But that's commonplace.

When you left Berkeley, you went to Wisconsin as an assistant professor?

OH: Right.

VM: How long did you stay there?

OH: Four and a half years, five years.

VM: So that takes you to about '63?

OH: Yes.

VM: Then you came here to Scripps?

OH: Then I came out to here.

VM: And you have been here, as you say, 33 years.

OH: Yes, more or less.

VM: What relations have you maintained with the Calvin group, with the people in the Calvin group?

OH: Not too much. We corresponded and sent Calvin Christmas cards for many, many years. Some of the people we corresponded with and kept in contact with the Moses and a few people.

VM: And, of course, Andy's in the building.

OH: I keep close contact with Andy and I see people like (*Paul*) Saltman who had some involvement with the group at one time. No — Paul never did. He was one of the people I used to read about, working on succulent plant metabolism. We have Murray Goodman and I see him once in a while down here.

VM: We're going to talk to Murray at the end of this week. OK; thank you very much. It's very much what we hoped you would tell us.

OH: Do you want me to add something about what I have done since then?

VM: Yes, please do.

OH: Off the tape?

VM: Record it; I think it's interesting for people to know what happened to those who were in that lab.

OH: OK. I'd like to say one person I met at Berkeley at the swimming pool was Dr. Ellsworth Dougherty who was head of the, absolutely a brilliant guy, he was head of the laboratory for gnotobiology up in Strawberry Canyon; he was an MD/PhD, absolute genius. When I went back to Wisconsin he had a grant to go down to the Antarctic to investigate terrestrial and freshwater life in the Antarctic and he invited me to participate. I went down there, that was in '59. On the way I stopped in Berkeley and saw Calvin and told him I was going down to the Antarctic. He really expressed great envy, I think.

VM: Envy?

OH: Envy. He realised that, he said something to the effect that he wished he could down and do some biological investigations on the forms of life that were endemic, that survived in these harsh environments. We were not looking at the ocean, we were looking at the terrestrial and freshwater streams. He showed a real interest, particularly with regard to possible photosynthetic pigments effect at low temperature and of light, and everything. I went to the Antarctic (this '59/'60) and I got very much involved. I had never been interested in ecology *per se*. Back in Wisconsin I was in the physiological-biochemical area under Skoog and the other major component of the botany department there were the plant ecologists under people like Clark who has a very famous ecology section. But there was a big barrier between the two and so there was hardly any conversation. We never took courses in any other discipline.

But then when I went to the Antarctic, I became very much interested in the effects of low temperature and phase changes for the transition between liquid water and the crystalline state. In the Antarctic all the terrestrial life: mosses, liverworts, a few mites and things, but the plant life is basically algae and liverworts and mosses. It's usually in crevices which are very protected from the harsh environment. Sometimes you have to look down about five inches in the rocky stubble before you find a layer of algae which have enough protection to survive.

VM: Excuse me: this is life on a rocky substratum?

OH: Very often you will find.. round the (*indecipherable*) there's a lot of rubble and small stones. If you just walk, you won't see anything. If you get down on your hands and knees and start pulling away and uncovering a few layers of rock, you find layers of green algae and mosses.

VM: Is there anything on the ice itself, on the ice sheets?

OH: There are snow algae, sometimes. Most of the life is microscopic and you have to get down on your hands and knees and look for it. It was pretty obvious that if you are looking in a little niche of a dark volcanic rock (I have some over there on the window sill), you are exposed to repeated cycles of freeze-thaw in terms of minutes, many times during the day. I became very much interested in the survival mechanisms of water relations and went back to Wisconsin and changed my major interest. I spent a couple of years just studying effects of freezing and thawing in repeated cycles and of freeze drying and removing...and the degree to which you removed the bound water of cells and the effect of viability. I got interested in

exobiology at the time. In fact, I have hundreds of samples of freeze-dried algae from the Antarctic in these little tubes (should have one here somewhere); yeah, these little tubes which are sealed in high vacuum and which are probably good for decades or centuries, maybe. If anything is going to survive geologic time, it's that. At the time in Wisconsin I was interested in space travel and if you are ever going to send a man about 100 light years away, you will probably have to free dry him. My research had changed a lot. During my trips I gave some lectures here at Scripps and I was invited to stay and I have been here ever since. So I became an oceanographer in contrast to my formal training at Wisconsin. You might even call me an ecologist now. But I'm afraid I don't think too much like an ecologist at the present time. I'm still interested basically in the response of organisms to environmental stress and how they adapt. The last three or four years I have been spending half my time on the study of the effects of ultraviolet radiation, particularly under the ozone hole in the Antarctic.

VM: Last question: What did those three years in Calvin's lab. do for you?

OH: Probably gave me a very good springboard to a good academic life. Far more important is the intellectual development and seeing good science at work and the functioning of a great mind and having it kind of infused in you. I think that carries through everything I do now to some extent.

VM: Had you met an exciting situation like that before?

OH: I had met famous people. In Norway I had worked with Brorud who was one of the most famous biological oceanographers (phytoplankton). That's a different kind of science, very descriptive, some physiology of the coccolith formation, but it doesn't have any dynamic approach where you really intertwine chelate chemistry and physics and biology. This (*the Calvin lab.*) was a qualitatively new chapter in my education, I think, which made a great impression on me...

VM: I think as it did to everybody else.

OH: ...and it affected my thinking of science and it affected the approach I had to thinking about problems and how you attack them.

VM: I think we'll close it there. Thank you very much indeed.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name OSMUND HOLM-HANSEN

Date of birth 9/9/28 Birthplace NORWAY

Father's full name OSMUND LUTZÖWE HOLM-HANSEN

Occupation ENGINEER Birthplace NORWAY

Mother's full name BERGLIOT PEDERSEN

Occupation HOUSEWIFE Birthplace NORWAY

Your spouse TANYA SPRAGER

Occupation Housewife + ART Birthplace SANTA MONICA, CA

Your children OSMUND HOLM-HANSEN III

Where did you grow up? CONNECTICUT + NORWAY

Present community LA JOLLA, CA

Education Ph.D.

Occupation(s) RESEARCH BIOLOGIST
(UNIV. OF CALIF., SAN DIEGO)

Areas of expertise PLANT PHYSIOLOGY,
BIOLOGICAL + CHEMICAL OCEANOGRAPHY

Other interests or activities SPORTS + TRAVELING

Organizations in which you are active O

Chapter 12

ANDREW (ANDY) A. BENSON

La Jolla, California

June 4, 1996

VM = Vivian Moses; AB = Andy Benson; SM = Sheila Moses

VM: This is a conversation with Andy Benson in La Jolla on Tuesday, the 4th of June, 1996.

Andy, can we start from the very beginning, before you ever got to Berkeley because when we were talking to Martin Kamen last week he said that you worked with him on photosynthesis long before the Calvin group.

AB: What you are saying is pure typical of you and Melvin and most people who are interested in photosynthesis. None of them realised what went on before 1946 and it's a darn shame. Most of it's my fault and also bad luck...Here's a (*newspaper copy*) of the story of the disaster that happened just after I left Berkeley; that was in the summer of 1943.

VM: And this is Sam Ruben's accident?

AB: Yeah. You will understand it because you know the chemistry.

VM: What happened with you personally? How were you involved with Sam and Martin? What did you do?

AB: That's an interesting story...

VM: Please tell us.

AB: ...and you'd better know it. I came to Berkeley — my father took me over there to see what was with chemistry — and so we were ushered into the office of Wendell Latimer. You remember that name?

VM: I remember the name and he has a Hall, of course.

AB: He died so they could make a Hall out of the space. He was a brilliant man and extremely influential in American chemistry. (*Telephone bell...*) I was interested in chemistry; that was 1934.

VM: What stage in your life was that?

AB: I had just graduated from high school. So, I arrived in Berkeley in September of '35 and enrolled in chemistry. As I was explaining here earlier, the first step in chemistry was to give an examination for all the six or seven hundred people who signed up for Chem. 1A. I think that was a good system. They would grade all these people and put the top 20 in Hildebrand's class and the next 20 in, maybe, Seaborg's class or Libby's or somebody else and they went through the whole faculty of chemistry in the lab. sections of Chem. 1A. The first semester I had Hildebrand once or twice a week for half an hour or so. But it was a great group of people and they were the top guys anyplace. The second semester I had Latimer and Latimer's son was also in one of these (*sections*).

VM: These were lab. classes?

AB: Yes. There would be 20 students and a professor on an informal basis. It was fantastic. My advisor, as a freshman, was Ronald Olson who was a professor of anthropology. He didn't know chemistry from nothing but that didn't make any difference. I became interested in anthropology and I took a few courses. The Anthro. Building, as you may remember, was right next to ORL. For some reason or other Olson was the expert in the department on the Indians of the Pacific Northwest. Since 1968 we (*Gerard Milhaud, myself and other collaborators*) have been working with spawning salmon in British Columbia and our headquarters is in a little Indian village of Alert Bay (*on Vancouver Island*) with just the Indians who made Frans Boas famous as the founder of modern anthropology. So it was just fortuitous and a typical example of the advantages of Berkeley for a simple student at that time. There was just no problem of communication between students and the top guys in any field. So I appreciated that immensely.

Just this last year, in 1995, there were three students from La Jolla High School who were named the top three in California in the Westinghouse science talent search and one of them was named Frans Boas.

VM: Was he related?

AB: Almost. He must have been a great, great grandson, or something like that and he didn't know much about these Indians and he had never been up there. Last year I took him up there and he got to meet the people who knew all about Frans Boas. Then he toddled off to Harvard to start his college work. He was quite thrilled with the experience, too. This was typical of Berkeley; it stimulated a lot of interest.

VM: How big was the freshman chemistry class when you were in it?

AB: Six hundred and fifty or something. It was not small. When I graduated, I applied to different schools for teaching assistants and I was accepted in about three places: Johns Hopkins, Princeton and Cal Tech. Naturally, I went to Cal Tech. I didn't know why I was accepted at Cal Tech but immediately you fell in with the top guys in science, today actually. One of them was one of the top mountain climbers of that period, climbing K2 in the Himalayas and things like that, and the other was a top ski guy on Mt. Rainier. Gee whiz, you really have exposure to that kind of stuff that later was important with Bert (*Tolbert*) and Dick (*Lemmon*) and Hans Ostwald in ORL. I did a lot of climbing in Yosemite and the Sierras, everyplace, rock climbing and ski camping.

VM: You went to Cal Tech with a bachelor's degree, not with a Ph.D.?

AB: The real smart people at Cal Tech are the undergraduates; the graduate students are second class. The undergraduates are all hand-picked by Cal Tech faculty. They cruise around the country and interview every applicant personally and that determines who is selected as undergraduates. It gives a pretty high class, energetic group of students. The exposure to these students as a TA (*teaching assistant*) at Cal Tech was a good experience, to realise how good people are naturally.

VM: Which year did you go? You said you went to Berkeley in '34.

AB: I graduated in '39. I went to Cal Tech in '39 on a hot as hell day, it was 106 or something like that, and we had to take the prelim. exam in chemistry which was, as you can imagine, pretty brutal. I didn't study physical chem. because I figured I knew more physical chem. than they did and I passed it but I think Pauling came over to me and said "Yes, you did all right but not good enough for somebody from Berkeley". They made me take it a second time. Next time I cracked the books so it was all right.

I did my thesis in synthetic organic, the study of the structure of sphingosine which was not established at that time. My professor, Carl Nieman, was interested in that and he had four people in our laboratory working on sphingosine structure. I went to work on the relationship of the position of the two hydroxyls in the amino group with periodate. By the time my thesis was written I knew a lot about periodate oxidation. , And as you are aware, that was instrumental in the way Al and I succeeded in degrading compounds. (You can close that door, if you want.) That paid off. The rest of it (*the research work for my PhD*) was synthesis of thyroxin analogues. I knew a lot about fluorine chemistry, through the fluorinated thyroxins. When I later came to Berkeley, Melvin was concerned with fluorinating TTA (trithenon...ketone (?)). I had something to do with that; it nothing to do with any photosynthesis.

VM: So all this time you really had no contact with biological concepts, it was all chemistry?

AB: My minor was in animal physiology and my first paper was in neurosciences.

VM: Where was that?

AB: Cal Tech.

VM: When did you finish at...?

AB: I finished in '42. I did my work at the Cal Tech marine station for some ungodly reason on peripheral inhibition in the scallop muscle. My teacher, who was C.H.E. Wiersma, who was one of the founders of that kind of neurosciences, this was a great experience for me. It makes a nice calling card around here (*Scripps*) where there's a lot of neuroscience types because Wiersma's a sort of god in that business. Anyway, I came up with my thesis exam, PhD, and made a little story about the synthesis and all that and the faculty is free to ask idiot questions. They started asking me about the equation for radioactive decay — had nothing to do with my thesis.

VM: You hadn't used any radioactive materials in it?

AB: No, not in the least, and I didn't know why. I suspect Pauling asked the question. Then, years later, it dawned on me what was going on. Latimer had sent me to Cal

Tech and I did all right, and then Pauling, who I was on good terms with; I took his courses. One evening I was up in the lab. with either a five-litre flask full of boiling methanol sitting on the edge of a crockery sink, which was pretty hazardous, and in walks Pauling to tell me that I didn't too well in the quantum mechanics exam and I'd better stick with organic. That didn't bother Pauling at all. Then I realised that these questions about radioactive decay had to do with Latimer's request to Pauling for somebody to be on the Berkeley faculty who knew some organic who would collaborate with Sam Ruben. I didn't realise this at the time or until a few years ago. This was a planned operation between these two guys who had very high respect for each other. Don Yost at Cal Tech was more of Latimer's type of chemist but Pauling had been in Berkeley as a student or a postdoc. with Lewis and so it was understandable.

That's how I showed up (*in Berkeley again*). And it was sort of presumed that...they gave me a lab./office about the size of this room in the Rat House.

VM: In the Rat House! Which was the Rat House?

AB: You don't know the Rat House? Where have you been?

VM: I'm not sure I remember which the Rat House was. Which was the Rat House?

AB: The Rat House was probably erased...was Lewis Hall built when you were there?

VM: Lewis is still there.

AB: I know. Was it there when you were there?

VM: As far as I remember, but I'm not sure.

AB: Before Lewis Hall was the Rat House.

VM: It was an old building, I take it?

AB: it was done in 1915 and was originally a shingle-covered building. It was very well constructed by modern standards, all wood and totally unadorned, and it had, I think, two classrooms. Sam Ruben had a lab. downstairs and an office upstairs; I don't think he was ever in his office. I had this little lab./office where (*indecipherable*) and all of the carbon-11 experiments were downstairs where the counters were.

VM: Had Latimer offered you a job at that point?

AB: Hildebrand did. He was the Dean at the time I was offered the job, at $2200 a year.

VM: That was '42, '43, something like that?

AB: '42.

VM: Was it explicit that you were going to work with Sam?

AB: No

VM: They left it up to you to find each other?

AB: Yes. I don't know who assigned me the space. But that was fine because Sam was the most wonderful guy in the world.

VM: What sort of job did they say they offered you? A job to do what?

AB: I was in instructor; I was the low man on the totem pole in the department. The term "instructor" doesn't have any connotation; nowadays it's very impressive. But Bill Gwinn and other people that (I don't know any that you know); Dauben came later and most of the guys came. I'm trying to think of any that came with me as instructors; there were about three.

VM: And you had to teach the students, of course?

AB: Yeah; I taught organic. Do you remember Randall, of Lewis and Randall thermodynamics? Randall had distilled too much mercury and it affected his brain and he was a little bit strange, but he had been a good collaborator with Lewis. He was teaching Chem. 105, (*indecipherable*) inorganic. Latimer called me in one day and he said that the students were rebelling because Randall insisted that they buy his book on thermodynamics which had nothing to do with (*indecipherable*) inorganic. Would I please take over Randall's lab. section and try to teach the poor kids something.

VM: You hadn't written a book at that stage.

AB: I got along fine with the students. Then I taught Chem. 101, advanced organic synthesis and the freshman lab. Of course, being just an instructor, I had a lab. section of students from the lower categories of 20 students. Most of them were not too great students but two of them became very impressive professors.

VM: Who were they?

AB: One was (*indecipherable*) Moss who worked with Martin Kamen at Washington University and she and Martin were the first ones to discover nitrogen fixation in photosynthetic bacteria. The other one was — his name doesn't come to me — but he was a very well-liked and distinguished professor of plant biochemistry at Davis and he just retired.

VM: Plant biochemistry? Wasn't Paul Stumpf?

AB: No, no. (My computer will take a little while to line up and I'll give you his name.) He worked mostly on anthocyanins and that kind of chemistry but he was very good as a student, he got an A in my section. I've still got my class book so I know who was there.

VM: Did you know about Sam before you went to Berkeley?

AB: I knew about the discovery of C^{14}.

VM: Did you know about the C^{11} and the photosynthesis work?

AB: Not much, I was an organiker but it didn't take me long to learn.

VM: When C^{14} was discovered, was that of great interest at the time? It was before my time.

AB: Of course.

VM: Was it well publicised at the time?

AB: Yes. There was a big article in *Life Magazine* about what they were doing. The real original publication was certainly a breakthrough, a breakthrough that Lawrence had sought for quite a few years. Martin has explained it well in his book. It was uncertain whether it would be a long-lived thing or not, but in the lecture that Martin gave here (which was not too well thought of because it was rather disjointed) he did draw on the board a diagram of the synthesis of C^{14} by neutron capture by nitrogen. I don't remember a discussion of that in any of the papers describing the discovery of C^{14}. I could get Martin to enlarge on that before it is too late.

SM: This was a recent lecture, was it?

AB: Yes. Murray (*Goodman*) was there and he could give you some feeling that the audience, and Martin too, were quite disappointed in the lecture.

VM: Was Martin still in Berkeley at that time, when you got there? Working with Sam?

AB: Yes. We used to have get togethers in planning experiments. Martin and I and Sam would be in this classroom for 40 students, scribbling on the board. I didn't have much to say because I didn't know much about it. They pulled no punches by telling each other they were absolute idiots, or this idea was stupid, or something like that. But they were very, very good friends and respected each other.

VM: Was it just the three of you working in this area together?

AB: We were the only faculty members who... There were some students, Charlie Rice and Mary Belle Allen, you remember that name?

VM: Oh yes.

AB: Mary Belle, she was a graduate student in chemistry and they finally didn't accept her, something like that, I don't remember, and Sam used to call her "Madame Curie" and she didn't like that too well. She was finishing off some of the microbial work that had been started and I don't remember what she did but she published some stuff. You asked me a question.

VM: Well, the last one was whether Martin was still there?

AB: Oh yes, he was still there, but...I remember I and my wife had been at the Piggly Wiggly grocery store and we (*met*) Martin and he had been kicked out of the lab. as an insecurity risk. He was working for the local shipyard in Richmond, checking the correctness of some kind of welding or something like that.

SM: When was that?

AB: '42.

VM: Had this mood developed rapidly once the US entered the war?

AB: It was a result of the un-American activities nonsense.

VM: That was already active in '42?

AB: Yes. He had been accused of transferring some radiation secrets to the Russians. Actually, he was giving them some music manuscripts of some quartets or something like that (Martin could tell you and [*indecipherable*] in his book). I don't know where my copy is; I bought about six copies of his book (*Radiant Science, Dark Politics*) but now they're all gone and I can't find one. You can buy it in a bookstore here. If you don't have one you should...absolutely. Because it said a lot of things about Miss Kittredge.

VM: (Could you not cover the mike, I don't know how well it...don't put your hand over it, I'm not sure.) Miss Kittredge?

AB: You don't know who Miss Kittredge was?

VM: No.

AB: You could find out in Martin's book. She was the secretary to the Dean (of the College of Chemistry). The Dean was usually Latimer but it had been Lewis. Lewis wasn't doing any teaching; he just ran the seminar on Thursday afternoon, then he'd pursue his (*indecipherable*). She wrote all the letters. These guys were too busy being scientists and they signed the letters, which was usually OK. But Miss Kittredge didn't like Sam too well, I don't know why, and everybody was afraid of her because she didn't have a sense of humour and she was really loyal to the Dean and to the College of Chemistry but she managed the lives of students (*and faculty*) as a consequence.

VM: So, when Martin left the lab., you continued working with Sam without him, presumably?

AB: Martin was still in town and he was welcome to come to the lab. but probably he was no longer affiliated with the Radiation Laboratory.

VM: He was actually working literally in the lab.?

AB: His work was always with the cyclotron. It was in ORL; you know where the white table was?

VM: Yes.

AB: That's where the (*37-inch*) cyclotron was. It was there when we moved in. The main lab. where Murray worked, and me and Al and everybody, was just a grungy place covered with yellow powder on the floor, uranium salts. So we just had a cheap linoleum pasted on top of it.

VM: On top of the uranium salts?

AB: Yeah.

VM: It (*the radiation*) doesn't come through.

AB: No, it wouldn't come through. If you destroy a building like that now, someone would really scream, for no good reason, of course!

VM: Within about a year or so Sam was killed.

AB: Yeah, it took a little over a year which was a darn shame. He worked like hell, doing all his teaching and then Sam would start working (*in the lab.*) at 7:30 in the morning and go home at 2:00 a.m. I worked with Sam all the time. I hardly ever saw my wife, and for a newly-wed girl with no great background (*in science*), that was a miserable sentence, it was not good.

VM: What did you do when Sam died?

AB: I was not there.

VM: You were not there?

AB: No. If you read that (*Kamen's book*?), you'll understand it.

VM: So you weren't in Berkeley either?

AB: Not when Sam died, no. When I was at Cal Tech I had many meetings with Bob Emerson; do you know that name?

VM: Vaguely.

AB: The Emerson drop!

VM: Vaguely.

AB: It was a drop in the absorption (*indecipherable*) well under the spectrum. Emerson was the one who with that information led to the two light reactions in photosynthesis. Emerson was in Cal Tech, just a lovely guy, and I heard him give a seminar on this stuff. Of course, I didn't realise how monumental a discovery it was. Emerson was a very thoughtful and gentle guy, not like Sam. British — was he British? He was a descendant of Ralph Waldo Emerson, and he had a brother who was a professor in Berkeley. Bob Emerson became the guru, or leader, of a small group of us who felt that we should be conscientious objectors during the war and he was very supportive and helpful. It wasn't too long before Emerson was killed in the crash of a British turboprop...

VM: When was that?

AB: ...with the round, with the oval windows.

VM: Oh. That was the Comet; that was a jet.

AB: That was a jet?

VM: Yes, that was a jet.

AB: What do you call it?

VM: Comet.

SM: De Haviland Comet.

VM: That would have been in the mid-fifties, I think; can't remember exactly.

AB: Mid-fifties?

VM: It fell out of the sky because they cracked around the window frames.

AB: I didn't have any contact with him after I left Cal Tech.

VM: So you went back to Cal Tech from Berkeley at that time?

AB: You're getting ahead of us. So I was in Berkeley with Sam Ruben and this trouble with the draft boards and everything, they were after me because they were refusing me the proper 4F status which I could have succeeded had I expressed the willingness to work on defence contract.

VM: 4F was some protected category, specialist category?

AB: 4F was probably conscientious objector. I don't remember which is which but I could look it up.

VM: But you could have got exemption on specialist grounds?

AB: Everybody else was working on (*defence*) projects. By March '43, Sam was overloaded with his research project, defence research project described there. Latimer was the manager of all of this interaction with government research organisations. Sam was working on the movement of heavy gases, gas clouds. Meteorology and...Sporadically we would do carbon-11 experiments but not so many. Sam gave me all of the first C^{14}-barium carbonate that they had made.

VM: The C^{11} was made in the cyclotron? How did they make it?

AB: Yes, from boron.

VM: You didn't have much time to use it?

AB: We did maybe a dozen or two dozen carbon-11 experiments. I made phosgene with carbon-11; destroyed it...

VM: But you had not much time before the radiation decayed away?

AB: When you did an experiment?

VM: Yes.

AB: You could work for about two hours...

VM: ...from the time we got it out of the cyclotron to the time you had to have everything counted?

AB: About five half-lives so five times twenty is 200 minutes (*Editor: sic!*).

VM: What were you able to do in photosynthesis in that sort of time period?

AB: Well, we did an awful lot. That's what Sam's papers with Hassid (*W. Z. Hassid, Biochemistry Department in Berkeley*) and Martin Kamen were about, trying to isolate the products. But they couldn't do any separations, the kind that we normally have done. It was all precipitations and you get co-precipitation and adsorption problems that they never recovered from.

VM: Particularly, I guess, if you're trying to work with...

AB: Later on, when we had the big polemic with James Franck and all those idiots, physicists, in the midwest, Martin was advising them from Washington University, St. Louis, where he was doing very good work but he was giving them the advice based on what he and Sam had done five years, six years before. That was not good advice. It was just unfortunate that he didn't have any of the organic experience that I did.

VM: There were usable amounts of C^{14} available at that time?

AB: Yes. There wasn't very much and Sam gave me the lot.

VM: What did you do with it?

AB: We were looking for the path of carbon in photosynthesis. We never used that terminology but it was exactly what it was. The point of view which Sam and Martin had developed was that the product of photosynthesis is the addition of CO_2 to an acceptor to make a carboxylic acid. That is what it turned out to be but there are all kinds of ways that he could be misled by that. They figured it was not a photochemical reaction but an organic chemical reaction. Therefore, it would happen in the dark. So they were doing dark fixation of carbon-14 dioxide and they did a lot of experiments and started isolating the product. The isolation was based upon the partition coefficient between water and ether, or water and ethyl acetate, of the radioactive product. I altered it by making the methyl ester with diazomethane. This little office I had with a lab., didn't have any hood, it had an open window so you could open that up. I made concentrated diazomethane to make sure the thing got methylated and then measured the partition coefficient and that would tell you it was carboxylic acid for sure. Sam had never done any of that kind of chemistry. That's what I was doing.

VM: How much C^{14} did you actually have, did you remember? Was it millicuries? It wasn't curies...

AB: No; it was millimicrocuries.

VM: Oh; that small?

AB: Yes. If you will let me get...How far can I go with this cord (*i.e. the microphone lead*)?

VM: You can go quite some way with this cord.

(*Benson retrieves sealed vessels*)

VM: You don't have some of the original stuff still there?

AB: Oh yeah.

VM: You have! Good heavens!

AB: Here's some tritium water from 1939! I don't have any carbon-14.

VM: I'm not going to smell this!

AB: Well, don't open it. This is tritium water: 1.37×10^8 counts per minute per mole.

VM: That's been there for 50-odd years.

AB: Yes.

VM: So you're down about four half-lives for the tritium.

AB: This is irradiated H_2O^{18}; it's now dry — just leaked out, evaporated out, dried. That was the result of some experiment that they did. I don't have any C^{14} that I know about. We used it all and re-used it; you know, you catch it in alkali, in barium hydroxide.

VM: It was very precious at the time.

AB: That was all there was and they weren't making any more.

VM: What did you find out? I don't remember the early papers on photosynthesis?

AB: I found out that the partition coefficient of CO_2 fixation was, I think, 0.14, or something like that. That is about as far as I went. I wrote it up. By that time I was in the mountains, in the Sierras, in this civilian public service camp of conscientious objectors working for the Forest Service. It was nice place to be, totally isolated, we were fighting fires and making roads and things like that. I had this manuscript to send to JACS and I just never did. I got a nice letter from Sam (in here some place) in September 1943.

VM: Just before he died.

AB: Ten days before he died.

It was a very cordial letter, hoping that I was OK and that the family was OK and all that. He was saying what he was doing. He worked up by Mt. Shasta, they had some experimental site. I had visited the Dugway Proving Ground in Utah (you know about that?) and the veterinarian there got him interested in phosgene toxicity. So that's what they were working on. I had also done a lot of other experiments with radiosulphur and carbon-11 on the mechanisms of organic reactions, with Sam and his collaborators. One had to do with *(indecipherable)* green and methylene blue reaction mechanisms in the phosgene project. Various professors in Chemistry and Sam had been recruited to advise their students on how to solve these things and I was helping with that.

VM: While you were up in the mountains?

AB: Oh no, that was before I left.

VM: So during the period you were in...

AB: While I was up in the mountains I had this manuscript I was going to send in but I was too busy and it didn't seem like an important thing to do. As I look back on it, I should have sent it in. It would have made all the difference in the world for my future.

VM: During that period of what: two years, three years you were up there?

AB: Yeah, about a year and a half. Then I was transferred to Stanford University to work on antimalarial drugs where I learned a lot, too. After that, I was transferred to Cal Tech which was the headquarters of the antimalarial project, that was 1945-46. I went from Cal Tech to ORL, to Calvin's group in '46.

VM: How did this happen?

AB: Ernest Lawrence was always interested in photosynthesis for some reason or other. He and Sam and Martin spent a lot of effort making carbon-11 and looking at CO_2 fixation. It was one of the things that Ernest thought was important.

VM: And you knew Ernest and he knew you?

AB: Oh yes.

VM: Did you know Calvin from the days in Berkeley?

AB: I knew Calvin from the day he came as a postdoc. with Lewis; that would have been in 1937. (*Editor: Calvin came to Berkeley as an instructor; he was never a post doc. there.*). I knew Calvin because he sat over there in the seminar room and I sat back with...the students weren't even allowed in there so I would just sneak in on Thursday at four o'clock and he (*Lewis?)* didn't kick us out, we just had to shut up. The faculty sat around the table down at the bottom with Lewis with his big cigar at one end and as soon as the cigar got fired up, that was the beginning of the seminar.

VM: Tell me, did Calvin used to interrupt seminars in those days, the way he always did in our group?

AB: Bloody right. That was the prime memory I have of Melvin in '37 and '38 was the fact that he was a real good questioner. He had the best questions after every seminar on almost any topic. He was a master at that. You have to give him credit for that. Perhaps that's why he was so successful with Dow Chemical. You know he was associated with Dow Chemical.

VM: I knew that Calvin was a consultant for Dow.

AB: You are going to have to unravel this story. I was working for the Forest Service and fighting fires and working with aerial photogrammetry (mapping) — a lot of field work in the mountains of California and Nevada. Then I was transferred to the chemistry department at Stanford. There were two other postdocs working on the antimalarial project and both of them were outstanding chemists. The professor was student of Franklin's who invented ammonochemistry. Are you familiar with that?

VM: I'm not an organic chemist.

AB: Anyway. This guy was F.W. Bergström.

VM: What was his name?

AB: Bergström. All the reactions were done in liquid ammonia and this was all ammonochemistry. That was fun. One of the guys working with me (*at Stanford*) was Ted Norton (T. R. Norton) and he was an outstanding organic chemist from Northwestern. After the war he joined Dow Chemical in...what was that place that Dow had a lab.? And then he became almost a Vice-President or a top operator in Kalamazoo.

VM: Was it in Concord, Walnut Creek or something like that?

AB: Concord (*Editor: actually it was in Pittsburg*), and eventually at the Dow headquarters in Midland, Michigan, that's must have been where it was. It turned out that Ted had been the one who hired Melvin for Dow Chemical. Ted is or was living in Honolulu; I had a big talk with him a couple of months back. He had a miserable proliferation of cancer in the hips and everything; it's just awful. But he was telling me about his experiences in managing Melvin for Dow. He was impressed with a lot of it. He understood the picture pretty well.

VM: So. you went up to Berkeley in 1946?

AB: Yes. The war was over and Ernest decided they had to get back to photosynthesis. Martin had gone to Washington University with his good colleague who died of cancer.. So he (*Lawrence*) sought around in the Chemistry Department for some organiker who would carry on (*the photosynthesis work*) and Melvin was it. They put Melvin Calvin in their (*Rad. Lab.*) budget and that made it possible for Melvin to invite me to come to Berkeley to start the photosynthesis lab. Bert (*Tolbert*) had already been there and had been working on medical aspects of tracers. And that is described in...I'm sure you know about that. This had to do with John Lawrence's interest in chemotherapy and the mechanisms of all kinds of things.

VM: Melvin describes that in the oral history he did about 1980 which we have.

AB: I read some of that. and we'll discuss that later when this machine is not on.

VM: We've got a couple of minutes to go on this side (*of the tape*).

AB: Some of that is not correct; Melvin's memory is sort of clouded by what he would like to think rather than what it really was.

VM: Well, we're certainly interested to hear what your view of those events is.

AB: Melvin got me there and they said this is where our lab. is, and I designed the lab. and ordered all the pieces.

VM: So you were there at the beginning of ORL before ORL was used for that photosynthesis lab.?

AB: I (*indecipherable*) out of that big room, alongside where the cyclotron was. There was a little lab. there.

VM: Had the cyclotron been taken out ?

AB: No, it was there. I was scheduled to be moved down to UCLA.

VM: I see. So you started to build a photosynthesis lab. in the building while the cyclotron was still there?

AB: Oh yes, I think it was still in use but the 60-inch was already functional.

VM: The 60 was in Crocker *(Lab.)*, was it, and the 37-inch was in ORL?

AB: Yes.

VM: What did you plan to do, as it were? What was in your mind as to the way you were going to develop the project?

AB: I was continuing what I already started; I didn't have any more imagination that that. I started working like a (*indecipherable*). As soon as we got any kind of laboratory we were thinking of (*indecipherable*) every morning.

VM: What? Melvin, perhaps?

AB: No, no, no: Ed McMillan.

VM: Ed McMillan?

AB: Yeah. And I has realised that as far as chemistry was, I had been isolating stuff that I got with the partitioning and I got down to stuff to crystallise so I was trying to recrystallise it to constant specific activity and Ed would come in to kibbitz with me every morning. Little did I know that in the afternoon he was isolating neptunium.

VM: By this time you already had sizeable amounts of C^{14}, presumably, from Radiation Lab. sources, did you, or were you still working with these tiny quantities?

AB: At that time it came from Oak Ridge.

VM: In reasonable amounts?

AB: Yes, we had plenty.

VM: Did you have prior access — did other people have access?

AB: No.

VM: You had all of it, or most of it?

AB: I'm not sure of that. Probably other national labs. had had equivalent access.

(*Tape turned over)*

VM: OK. So we were talking about in the lab. in the ORL at the very beginning. Who was there working with you; who started; who were the earliest people?

AB: At first I think I was working a little bit over in Donner with Bert and — was Dick Lemmon there? With — who wrote the book on radioactive carbon with Calvin?

VM: Heidelberger and Yankwich and Reid.

AB: Yeah, Jim Reid. Yes, he was there. Heidelberger came later and Pete — ah! One of Same Ruben's graduate students along with Mary Belle was Pete Yankwich so I knew Pete very well back in '43.

VM: He's now in Washington.

AB: Yes: he's a big head honcho of American education.

VM: Yes. I'll try and get to see him...I'm not sure...

AB: You should; he's a very distinguished gentleman. One of the things you may not know is that Pete Yankwich's father was a federal district judge in southern California.

VM: I've never met Pete and I don't know anything about him at all.

AB: And Dick Lemmon's father was a federal district judge in northern California.

VM: That I did know. So, in the beginning you started off working in Donner presumably because ORL was not yet ready?

AB: We had to pick out the linoleum; it was sort of reddish stuff.

VM: So it was your job, among other things, to get the building ready?

AB: I've built many many labs. That's why I'm so horrified at the way the idiot lab. architects want to do things. They don't know from nothin'. A lot of the things about laboratories were devised in Germany 200 years ago and a lot of them haven't changed one bit, and they should. But neither the architects nor....one of our good friends is the head of a major lab. architectural firm in the world, he designed the Salk Institute labs. and he's been designing labs. for McMurdo (*Sound*) and the University of Riyadh — a million square fee of laboratory space (can you imagine that?) and NIH and every place. But those guys are human and not great.

VM: I guess you really weren't in a position to design a lab. You had a bit of an old building available and you did the best you could with it.

AB: I had had a lot of experience, built houses, I knew plumbing and wiring.

VM: Had you done that by that stage in your life?

AB: Yes. But Melvin had never lifted a finger, maybe a violin, but I doubt it! He didn't understand any of that. The people who knew were Bert and me. Or Bert and I?

VM: Bert and I: "Bert knew and I knew..."

SM: It's not very important!

AB: I've got to use the Queen's English here!

VM: That's all right: this is America!

SM: She is always "we"; there's no problem!

AB: Anyway, Bert and I were raised on farms and we understood how things went. I learned an awful lot of technology from Bert. He taught everybody. So you've got to give him all the credit you can in whatever you are doing.

VM: As you got into the building, who joined you, who were the first people to work in the building?

AB: Maybe (*Sam*) Aronoff was one of the first major ones that he (*Calvin*) brought in because they figured that Aronoff knew something about plants. My own interactions with Aronoff were on the edge of thorny. That wasn't because he was good it was just personal. In the last few decades we have gotten along fine. I remember we had some technicians, Gordon Hall and Tom Goodale — you've seen that name?

VM: I've seen the name, don't know the person.

AB: Tom was a character, sort of independent character, and he would look all over the place for something that was lost and finally it was on the shelf and he would say "Ah God: if it'd been a snake I would've bit it!". Which means it was awful close.

VM: Do you know where he is now?

AB: No.

VM: I haven't found him.

AB: Gordon Hall was working with a huge international contractor in Indonesia; he came by one time about 15 years ago. The same name as the people that manufacture water heaters. You don't care, anyhow.

VM: In the beginning, you started chemical isolations, presumably, of photosynthetic products?

AB: Yeah. Then the concern was whether photosynthesis was truly a light reaction or a dark reaction because it became clear after I isolated crystalline succinic acid that that was what I was trying to find with Sam Ruben, and that's just not very exciting but Melvin cooked up a cycle, the C4 cycle, so that was C4 photosynthesis but it didn't have any value in terms of what we were really after. This was disappointing when you look back on it but I developed a lot of techniques.

VM: Were you beginning to degrade products at that stage?

AB: Then Al (*Bassham*) came as a graduate student and I put him to work degrading malic acid. Then Al developed a skill in degrading things so I had to give him the fructose first and but then later sedoheptulose and ribulose and glycolate.

VM: Was the arrangement in the lab. that these students were formally Melvin's students?

AB: Yes. I wasn't on the faculty.

VM: You were a Radiation Lab. employee, presumably.

AB: Yes. The students were all Melvin's students but Melvin didn't have much to do with signing their projects, especially when Murray (*Goodman*) came — do you remember when Murray came?

VM: It must have been '51 or '52, but I haven't talked to Murray yet.

AB: I think it was earlier; You'd better nail it down. Anyway, Melvin had a coronary in '49, at the age of 37 (*Editor: actually 38*) — you know that?

VM: Yes, I knew that he had one.

AB: And his brother (*Editor: Calvin never had a brother*) and father had died at age 37, of coronaries.

VM: No, I didn't know that.

AB: It was a grim prospect; it scared the bejesus out of him...

SM: Luck he had...

AB: ...and Genevieve gave him the right kind of diet and it was lucky for Melvin that the guy in the next lab. (*Editor: in Donner, where the heart attack occurred*), and the guy who took care of him when he was struck with this coronary, was Jack Gofman. You know who that is?

VM: Yes.

AB: Jack was the only one in the world who knew anything about HDLs and LDLs at that time.

VM: Melvin was very fat then, wasn't he?

AB: Yes. He was built like Isaac Stern.

VM: And did he smoke as well?

AB: Constantly. Oh cripes — that was the worst part of working for that guy. It was just awful. He'd come in the morning when I was trying to write something up (I had a little desk in an office in ORL) and all around my chair were these cigarette butts, all over the floor. I didn't have enough sense to kick him in the rear end and tell him to get out of there.

VM: He chain-smoked these things, did he?

AB: It was the pits. My father never smoked, he was a doctor and he wouldn't allow anybody to smoke when they came into his office. My dad was six foot six and he could tell people "don't" and they didn't. So I was always prejudiced against smokers and here was Melvin, smoking like a sieve, and he was a likeable guy and I didn't mind his personality too much, but he was a (*indecipherable*) so I had to clean up after him every day. And then after he had his coronary and got so scared that he quite smoking, he never apologised once for having stunk me up and cluttered the floor to pick up after him for several years. I'm still bitter about this.; you shouldn't put it on your damned...

VM: It's not terribly sensitive, I don't think. Were you there when Melvin had his coronary?

AB: Yeah, sure. It was at an AEC review committee were there and they had to present their budget and the reasons for their existence. That was held in the little seminar room at the end of Donner hallway. I don't remember the details of him grabbing himself because the next thing I knew they had him lying down on a table, or couch someplace, in a the room next to Gofman's lab.

VM: Was he making a presentation at the time?

AB: I don't remember whether he was making a presentation or whether it was just after or just before it but it was just then. So that was the beginning of what must have been several months, two months in the hospital to level him out and get the heart back. That was a real shock. We had agreed to write a review for *Annual Reviews of Plant Physiology* or was it *Biochemistry* (you can look it up) and there I was stuck clutching the burlap, having to write the whole review, which I did, while Melvin was incommunicado in the hospital for a long time.

VM: Was there concern about whether he would survive at all at the beginning?

AB: It was pretty serious in those days. He realised how serious it was. He was scared stiff and so was Genevieve but she solved his problem.

VM: So no more smoking and no more eating!

AB: He lost 50 pounds immediately and he looked almost the way he is now. But I do have a photo of him, I wish I could put my finger on it, before the heart attack. He's in swim trunks, wading into the water in New York at Cold Spring Harbor. You could tell what shape he was in, total pale. But it's a superb picture and it will surface sometime; I certainly put it some useful place but I don't know where it was. I've looked for it a couple of times unsuccessfully.

VM: When you set up this lab. in '46, by then, presumably, you were beginning to use ion exchange separations.

AB: Not really. Ion exchange came a little later. That came by way of Dow Chemical. We wouldn't have known anything about ion exchange were it not for the fact that Melvin was consulting for Dow.

VM: Right at that early stage he was consulting?

AB: You can find out from Marilyn maybe when he started but I think it was certainly '48, maybe '49 that he started with Dow. Before that he had been working with Dow on his chelating business which may have started in '46, I don't know. He didn't communicate with me on any of this stuff. He would bring home resins later on from Dow.

VM: What were you using as a technology way back in the beginning of the ORL days?

AB: Just partition between organic solvents. I knew a lot about that from having taken chemistry in Berkeley. It was important for doing iodimetry kinetics and so forth.

VM: I remember the stories about the first isolation of PGA. You took that out, eventually, on a column didn't you? There is this story of Melvin in the red zone who suddenly realised what it was while the car was parked.

AB: That doesn't have anything to do with PGA. That had to do with the cyclic recarboxylate (*Editor: no doubt this means the primary CO_2 fixation reaction*). It had to do with the carboxydismutase involvement of ribulose diphosphate.

VM: That's what he dreamed up in the red zone when he was parked?

AB: I think so. But we had discussed dismutation reactions for ten years and this was nothing new. I was never very excited about that idea.

VM: Presumably this whole development of the carbon cycle came about interactively and in bits and pieces as you went along?

AB: Oh sure. We were in the dark, but Melvin never admitted that he was in the dark. He would publish papers and they were only half-truths. But one of his great attributes is that he never worries about being wrong. It doesn't bother him at all. He just goes ahead. You got my little paper on thioctic acid?

VM: I have got *your* paper on thioctic acid.

AB: Where have you been, Vivian? Make sure you get one. Anyway, Bob Buchanan, do you know him....

VM: We are going to see Bob Buchanan in a couple of weeks time.

AB: He's a top-notcher, absolute top-notcher. He is now the President of the (*American Society of Plant Physiology*). He decided that we should have a certain little page in the (*ASPP*) Newsletter of Plant Physiology dedicated to people writing... So he got me to write the first one, which you will get, and Melvin wrote the second one and Marty Gibbs wrote the third; I don't know who's on deck now.

VM: The Gibbs' one I've seen but I haven't seen yours and I haven't seen Melvin's.

AB: Melvin's was OK, but it's not anything you don't know. But the one I wrote about thioctic acid is something you know but it's from my point of view.

VM: Sure; I'd like to see that.

AB: I thought it was fun. Everyone seemed to enjoy the heck out of it and Buchanan said people in his class were really eating these things up.

VM: Sometime around then you had the introduction of paper chromatography.

AB: Yeah, Stepka came from Cornell. I don't think I knew much about paper chromatography until Bill came.

VM: Stepka had apparently been with Steward in Cornell.

AB: Steward had a postdoc. from England...what's his name?

VM: Dent.

AB: Dent and Stepka learned it all from Dent. As you well know, it was a very toxic kind of paper chromatography and didn't work very well with phosphate esters, especially with any salt. Maybe that's one of my major contributions in ORL was devising a functional paper chromatography method that works, and still works better than anything else.

VM: Can we think about that for a bit. You presumably were aware of the invention of paper chromatography, were you, in those early days...

AB: Oh yeah.

VM: ...but between you it didn't seem appropriate for you to use it at that time in photosynthesis. Why didn't you start using it, once you knew about it?

AB: We did. We started immediately.

VM: Before Stepka came?

AB: No, no, no.

VM: Stepka brought the idea to you?

AB: Yes.

VM: So you weren't aware of the Consden (*Martin*) and Synge paper chromatography which came out in, I think in '44?

AB: No, I don't think so. But, as you well know, paper chromatography is just a matter of partition between water and organic solvents and that's what I had been doing. The partition column came out with Bob Holley's separating RNA, aminoacyl RNAs.

VM: When Stepka brought this around, when? '48 or so, do you reckon? How did Stepka come to the group?

AB: He was a graduate student, if I'm not mistaken, in Soil Science — trying to think.

VM: But not in Chemistry?

AB: You're going to see Stepka; he'll tell you.

VM: From your point of view, did you bring Stepka in or did Stepka find you? How did you make the contact?

AB: I think he found us. I don't know...you'll have to find out from Bill how he was engaged because he was not a student in our place. But he may have been a graduate student in Plant Nutrition. I'm pretty sure of that. He was working with Overbeek; he'll tell you. He knew the English literature, what there was, very well but he wasn't enough of an organic chemist to devise a novel method, which I did.

VM: When he came in, presumably it was new to you guys and you had no set up for it, no equipment for it and you had to build something. How did you start, glass jars or did you build wooden tanks at the beginning? How did you get going to the extent...?

AB: Stepka only knew glass jars but I wasn't going to put up with this nonsense. So I made it the way I thought it ought be and it worked pretty well.

VM: Presumably you tried it out in glass jars and convinced yourself it would have some value.

AB: At first we had the glass troughs, glass weight rods. But since we were in the Radiation Lab. and we could have anything built we wanted to of stainless steel, by the best guys in the country, I got troughs that were masterpieces and you still can't buy anything that good.

VM: Did you design those troughs and the sleeves and the stainless steel clips the hold the...?

AB: Sure. Of course. I designed everything. Talking about design: when I was working in my little office with Sam (*Ruben*), we had to spread the samples from the dark fixation so that we could count them, count the C^{14} (there might be 100 counts or 200 counts, or something like that) with some reliability. All I had at the time was Libby's screen-wall counters. The wall was a screen with holes, some of the beta (*particles*) could sneak in and get measured inside the Geiger counter volume. The way Sam had been doing it, he had sort of wrapped the thing on a paper or something, or cellophane or something around the screen and then sealed up the counter. I said heck, we've got to have ground joints. So I made such a counter with ground joints, the first one, that you could take apart and put the sample in. The sample I mounted in glass cylinders, about that long, just the length of the screen. The counter had to be twice that long so you could put the sample over here for the background and slide it over the wall for the counter. First you have to evacuate it, fill it with counting gas and then count the bloody thing, and to put the sample inside of a cylinder required inventing a sample roller. So I had two rollers that rolled the cylinder and with heat flowing through this thing and you would dry the sample inside the cylinder so it would be thin and uniform, and then you'd put it into the counter. That was the precursor of the disc thing that I designed, the rotating disc where you could blow and get a nice sample spread over a disc for counting a thin organic sample. Bert advised me on a lot of this.

VM: Bert had experience of course with the use of....

AB: Yes. They invented a little cup for getting barium carbonate. And Sam knew all about self-absorption of barium carbonate. I don't remember if I used...I must have made barium carbonate discs, too. Maybe I did that with Sam, I've forgotten.

VM: When it came to the two-dimensional paper chromatography, did you start out with two dimensions?

AB: Yes.

VM: There was that little frame thing that held the paper while you blew air onto the spot and ran the liquid out of a (*pipette*).

AB: I invented the little folded stainless thing, you just hold the paper, let the paper go where it wanted to go.

VM: For drying of the solvent or putting the sample on .

AB: Putting the sample on.

VM: You invented that little arrangement with the blowers top and bottom.

AB: I put just one blower. I'm the guy that put the big Bunsen burner in the blower mount so you could get real heat. I could run circles on anybody in boiling water or evaporating anything. I still can. To do all these things, both with Sam and at Cal Tech as an organiker, all the evaporations were done in vacuum with a water aspirator in the big flask. We didn't have rotary evaporators which are still not as good as what I was doing. With five minutes a litre for water, I could do pretty well. So I don't think anybody could beat me boiling water!

VM: I remember those rotary evaporators were always giving trouble when you did it under high vac. or even on the water aspirator they would tend to bump.

AB: I figured all that out.

VM: How about those end-window Geiger counters that we used which had the mylar windows. How did they come about, the gas-flushing ones?

AB: I think Jim Reid and Bert had a lot to do with the design of those things. They had the thin mylar and so I guess they used what they had.

VM: This technique of using the radioautography: that must have come about the time you were introduced to chromatography.

AB: I did all that, yeah.

VM: Were you actually the inventor of that, I don't know what the history of radioautography was.

AB: I invented the drying racks and the boxes, you know you pull out the rack and you put all the papers in there, and they have to be sucked from the bottom or else they would flutter around, you can't dry them in the hood.

VM: Did you invent the concept of those big film radioautograms that went on the chromatogram?

AB: Yeah, sure.

VM: Had anybody used them before? I don't remember.

AB: I suppose.

VM: They were used for medical X-rays, weren't they, for chest X-rays?

AB: People before us would only have one-dimensional things and put a piece of film on the strip. But since I knew a lot about X-rays since I was that big...my dad had a portable X-ray machine so he could go to a farmhouse and make an X-ray of somebody who'd broken something and take it back to his office and develop it. This was a beautiful oak box with a Tesla coil inside, which weighed about 40 pounds, and you screw in a couple of things with brass balls and pull out copper wires and you hook this onto the X-ray tube which is held with a wooden oak clamp, and take the X-ray or else use the fluoroscope/fluorescent plate. I spent a lot of time watching my fingers with this unshielded X-ray tube.

VM: You remember when they had that stuff in shoe shops and you could look at your feet?

AB: I wish I had this thing. It would be worth a fortune now, I was just too busy...

VM: The one that my father had?

AB: It had big glass tubes. So I'm the first one to use, I bet you, the 14 x 17 (*inch*) X-ray film (*for radioautograms*).

VM: I've seen some of your original chromatograms up in Berkeley.

AB: I still have thousands.

VM: You have a museum full of stuff here, have you?

AB: More or less. I refer to them once in a while. These films and chromatograms include all the compounds and all the information. There's no lab. notebook. I never kept a lab. notebook. Why keep a lab. notebook? You ought to have everything on the paper chromatogram, the details of the experiment, when it is and who done it. The fact that everything in the algal extract is on the paper chromatogram, nothing else, either at the origin (maybe it blew away), at least they are all there and most of them are separated from polysaccharides to phospholipids and triglycerides. It's a fantastic separation technique and nobody really recognises the fact that it is a gradient elution. The composition of the solvent system changes with how far it has gone. I published some stuff on this once and nobody really realised the pH changes that occur.

VM: In fact you have chromatography of the solvent itself as part of the process.

AB: Yes. Part of it evaporates and the aqueous part is adsorbed on the water (*Editor: this should be "on the paper"*) so the solvent is more organic as it goes. This had been recognised slightly by some people but not to the extent it should have been. It runs circles around most other chromatographic techniques in spite of HPLC.

VM: And you have stacks of the chromatograms still around the place?

AB: Yes.

VM: You still using it?

AB: Yes. I dig them up once in a while when we need something special.

VM: Do you use paper chromatography as an analytical technique?

AB: Oh yes.

VM: You are all set up to use it?

AB: I have not been using it the last six months but my boxes are down below by the lab. that I've got. This was my lab., but these other jokers moved into it and I've got the drying rack in the hood, and so forth. That's me. I'm just counting some C^{14}-methanol. The first methanol that I used was made by Bert.

VM: C^{14}-methanol?

AB: Yes. I fed it to algae and they fixed the methanol into sucrose and every other normal intermediate so it was clear that methanol was readily metabolised by algae. When my colleague Arthur Nonamura, who worked with Al Bassham ten years ago, was trying to grow his alga which produced oil (isoprenoids) at the time when oil prices were high, he was trying to get these algae to grow faster,. And I suggested that he give them some methanol, they love it. I had done this with Bert's methanol a hundred years ago. Sure enough, the algae grew twice as fast. Then Arthur became a farmer outside of Phoenix with 1,000 acres of cotton, and he sprayed his cotton with 30% methanol and it produced a crop a month ahead of anybody else's around there. He made a lot of money as a consequence of not having to pay for all the water, insecticide and labour. We published this and this developed into something that will be worthwhile.

VM: If I can take you back 40 years to ORL...

AB: I'm not interested in what's 40 years ago. How are we going to do this experiment to understand how methanol works? I'm trying to get some radioactive benzoic acid, right now, and I asked John Foster how you make it. He said it's very simple. You start with pyridine, or piccolinic acid.

VM: You can't buy radioactive benzoic acid?

AB: You can, for $1,000 per millicurie. But it's made by neutron absorption by pyridine, or pyridine carboxylic acid, which gives you very hot stuff. Harwell charges an arm and a leg for anything. Those SOB's in your country: they got a closed contract with the Russians for their cheap barium carbonate and they charge $200 a millicurie for it instead of $4.00.

VM: That's business, isn't it?

AB: It's robbery.

VM: You don't have to buy it!

AB: You capitalist, you.

VM: That's what it's about. That's how they do it!

VM: When we were talking before lunch, you said to remind you to talk about Bersworth.

AB: Ah yes. About 1947 George Bersworth came rumbling into the lab. I don't know why he found Melvin but there must have been a reason: you could ask Melvin...

VM: I don't think you could ask Melvin.

AB: ...or you could ask (*Art*) Martell (*Editor: now at Texas A & M University*). Bersworth introduced me and Melvin to chelates. He came because Melvin had a metal chelate, cobalt chelate compound for his oxygen production during the war, that's why Bersworth came. I learned a lot from him. He came through four or five times, talking chelates, because he had invented the versenes, as he called them. Lately I have been dealing with a chemical company near Boston for which Bersworth had been a

consultant in designing of their chelate compounds. I was amazed to hear something about him. I don't know if he is any longer alive. He's the father of chelates.

VM: You also mentioned Philo Farnsworth. Who was Farnsworth?

AB: Ah, yes. That was a story about Farnsworth and Earnest Lawrence coming in every day with this complicated glass apparatus with a lot of wires dangling out.

VM: Who was Farnsworth?

AB: Philo Farnsworth invented television.

VM: Invented it?

AB: Yeah. Television was not a British invention, it was American.

VM: Oh dear!

AB: I can show you a US postage stamp with Farnsworth's picture on it. Another one with Tesla, another one with the inventor of the electronic vacuum tube. There were four electronic inventors in this country.

VM: And he came through the lab. with the complicated...? What was he doing there?

AB: A complicated thing; they was going into the glassblower's shop.

VM: To get it fixed?

AB: To get it readjusted, or something; change the position of some electrode. This was the forerunner of the trinitron three-colour television gun that SONY developed for (*indecipherable*) production.

VM: That was remodelled, or fixed up, in ORL?

AB: Yeah. I forget the glassblower's name; I've got a photo or slide or something.

VM: So, before too long, presumably, they got the 37-inch cyclotron out of the middle of the big lab. You had then essentially the whole building except for the shops which remained there?

AB: No, we didn't have the whole building, but at least the 37-inch room was ours.

VM: Where did that big white table come from?

AB: I built it, or organised it.

VM: It wasn't really a table, was it? Didn't it have sets of closets or drawers (*under the table top*)?

AB: Yes, it did have drawers in it for storage space but we wanted some big white thing to lay down the X-ray films. It needs to be white or you don't get the contrast that you want.

VM: So tell me: how did it begin to work out that you began to realise that there was a cyclic effect in the path of carbon?

AB: Anybody knows that there has to be a cyclic regeneration of an acceptor but it wasn't clear at first that ribulose di(*phosphate*) was the acceptor. I looked all over the lot for C_2 acceptors, so that's why we had labelled glycol and acetate and glycolate and all those possibilities — and we sort of gave up on that. About the time that we realised that I was doing the experiments where you withhold carbon dioxide and look to see what's happening, and it was the ribulose di(*phosphate*) spot that was piling up. That made it clearly the acceptor. But that didn't tell you how. Maybe that's the idea that Melvin got at the red light, in the red zone; I don't know. But we had been talking dismutations for a long time as a way of gaining free energy from a reaction. And it's still amazing that nature invented a process which organikers had never dreamt of. In all of the other enzyme reactions, there's an organic chemical model for it. It may be impossible to do in a biological thing but there's always a model reaction. But not for ribulose carboxylation.

VM: I guess nature has been going a lot longer than organic chemists, and given time the chemists would have come up with it.

AB: I don't know, but maybe some well-read organiker would discern an organic reaction that's analogous, maybe in ammonia chemistry or sulphur chemistry or something like that, but we didn't know about any and I still don't know. I talked with Stanley Miller every week or so about these things. Stanley was educated in the Berkeley Chem. Department and he is very familiar with what we were doing.

VM: He's down here now, in the Chemistry Department at UCSD. He's still there, is he?

I think the last things that really...a couple of things I want to talk to you about. You eventually left, when, in '55?

AB: Yeah. But we didn't do the PGA (*Editor*: *phosphoglyceric acid*) bit; you jumped.

VM: No, please tell me. I'm sorry.

AB: Because then we were fiddling with ion exchange resins and Melvin got the Dowex 1 resins from Dow and also there was some precursor of Dowex resins, what were they?

VM: The Amberlites?

AB: No, before that. It was a local product, made in Berkeley or El Cerrito or some place. Probably written up in our paper. Anyway, in the very short-time photosynthesis the main chunk of the radioactivity was much harder to elute from the anion resin than the activity from longer photosynthesis. Those phosphate esters came off much more easily than the one from short, short times.

VM: You knew they were phosphate esters.

AB: Yes, I don't know how, but we ran some controls. It was Melvin that recognised the fact that if it's harder to elute from a resin it means it is bound by two anionic groups, not only one. Therefore, it's bound to be PGA.

VM: Why was it bound to be PGA?

AB: Well, it could have been fructose diphosphate.

VM: You knew it was a sugar at that point?

AB: Yes.

VM: Or something like a sugar at that point?

AB: Yes.

VM: So you isolated that, I remember, didn't you, by mixing the hot stuff with a lot of cold.

AB: I hydrolysed the phosphate off and then made the pure fenasyl glycerate with fenasyl benzoyl chloride; it's written up. That's a derivative of the glyceric acid. And then co-crystallised that to a constant specific activity and that sort of nailed it down. In there (*Editor: in Benson's filing cabinet*) is a bottle of the original Kohlbaum glyceric acid that I used. Our chemistry storehouse was fantastic. They had a standing order for everything from Eastman Kodak and then they had 100 years of Kohlbaum and German preparations and preparations by faculty members and whatnot. It was a real museum piece. That kind of thing people don't keep any more.

VM: To tell you the truth, I just don't go into chemistry storerooms. I don't know what they keep in chemistry storerooms; they've probably cleaned out all the old stuff.

AB: Now they clean it out and order things like a cook, go down to the grocery store and buy what's necessary for dinner, and that's it!

VM: Yes, you're probably right. So I was opening up the question of your eventually leaving there. You tell me that I was going too fast and there were other things you wanted to discuss.

AB: I backed you off to PGA.

VM: Yes: do you want to back me off to anything else?

AB: I'll back you off to where I was stuck writing the *Annual Reviews of Plant Physiology* or *Biochemistry; Plant Physiology*, I guess, a review on CO_2 fixation. And that was an OK review; Melvin read it over and that's about it. At that time I don't think we knew what the carboxylation reaction was. After that, he went on a vacation trip to Norway with Genevieve and her mother, visiting their family and all that. A professor of chemistry in the Agricultural College (*of the University of Norway*) buttonholed him and begged him to send somebody over to set up an isotope laboratory. So I was awarded a Fulbright grant to go to Norway for a year. That's when (*Grant*) Buchanan came to the lab. and what's that other guy with the Italian name?

VM: Massini; Peter Massini.

AB: They did a good job.

VM: Buchanan worked on sucrose phosphate, I remember; that's what he told us.

AB: I was on the verge of identifying UDP glucose and if I had stayed there I would have recognised it before Leloir, but Leloir took over. Leloir was a very fine man, there's no question about that.

(*Tape* change)

VM: OK: Andy Benson tape 2.

The question I was about to ask you: when you did those really classic experiments of the kinetics, when you turn the lights on and off and dropped the CO_2, did you know what to expect, were you looking for confirmation of an idea, or was that rise and fall of ribulose and PGA unexpected?

AB: I don't think that was expected. We knew that something had to rise and fall. The best work was done by Shinichi Kawaguchi who came, maybe in 1949. He was a professor of kinetics actually in the University of Osaka. He became later president of the Japanese Chemical Society, a lovely guy. He was very meticulous and we did a time series of CO_2 fixations from 5 seconds up to 10-15 minutes. He and I made chromatograms of the whole series (I've got all those).They were beautifully done and the separations were excellent. At that time, we knew something about hydrolysis of the phosphate esters, so the phosphates were eluted and hydrolysed to recognise which ones they were so we could get a kinetic rate diagram for all of the intermediates of CO_2 fixation. It's wishful thinking but both Melvin and I thought we could learn something from this. The only thing that we can say we learned is that malic acid production is very fast, almost as fast as PGA. It makes me think that by some mechanism that the phosphopyruvate carboxylation is an important aspect of photosynthesis that still hasn't been clarified.

VM: Was it those sorts of experiments that gave you a handle on the concentration of compounds in the cell and that you could then use these to look at the variation when you altered the external conditions?

AB: Yeah. We could deduce from that what the concentrations in the cell were. But at that time I don't think we did that. That came later when Alex Wilson was working and Al and a number of people. But Alex did it on a grander scale than any of the others.

VM: We'll see Alex tomorrow and talk to him about that. There is, of course, that thing in Alex's paper, of the little fisherman on the...

AB: I have a good copy of that.

VM: Who did that? Alex himself?

AB: Alice Holtham drew it. But it was Alex's nonsense. This is akin to dipping the drinking milk straws into honey and sticking it to the ceiling. He was a total fox, that guy. He was just delighted that this sailed through the JACS and got published. It was a very small diagram in the journal, you know. You could just barely see this little fisherman.

VM: You need to know where to look for it, don't you?

AB: It's there. I showed it in my talk in Grenoble last year in January. We had a chloroplast-photosynthesis meeting in a ski resort town above Grenoble. I gave the

historical talk in that. I spent a lot of time about Alex Wilson, who was such a character.

VM: The other name that came up before lunch was John Weigl. You had something you wanted to say about John.

AB: Actually, it was about his wife, who gave me the title for an article I wrote, "The Green Secret".

VM: What was the article?

AB: Something about radioactive carbon in photosynthesis but we didn't know the cycle at that time, that was 1948. It was a reasonable article but it's hard to find in the journals nowadays because it was published in the Yearbook of the Grolier Society.

VM: One of the things that certainly struck me when I first became involved with this was, in a sense, how wise, or if you like to say how fortunate, you were that you were working on a problem where you get rid of the unused substrate. When you put the chromatographic products from the algae onto the paper, all the hot CO_2 which hadn't been used was lost, and it didn't foul up the rest of the experiments. If you tried virtually any other substrate...

AB: You have had to get rid of it.

VM: ...you'd have a great glob of the stuff sitting there which is likely to foul up anything else.

AB: Good point; write that down in your article.

VM: Well, it's on the tape now. Had it not been like that, you would have had a hell of a lot more trouble sorting stuff out because you were dealing with very faint compounds.

AB: The whole sequence is a bit of fortunate choices, like me being educated in Berkeley and knowing who these guys were, and going past the 60-inch cyclotron every day while it was being put together, when I'd go to class right past the Anthropology Museum because I knew something about that; then getting to work with Sam Ruben and Martin at the right time — nobody else had that opportunity. And then the first shot of radioactive carbon us.

VM: The combination of all those things really rather suggests that you cannot design deliberately organisations to be successful like that. They happen because things work out by properly chance.

AB: Yeah.

VM: Can you put your finger on what you think are the most important reasons why that group was so successful in what it did?

AB: Maybe because Melvin was so darn clearly oriented in which way he wanted to go and he refused to let anybody distract him with their own ideas; he only wanted to push them for what he could glean that would help *his* ideas, which is sort of disgusting but in other ways it was very successful and it's not very considerate. Not

to pay any attention to what a visitor wanted to tell us but needling the visitor for what you could get out of him.

VM: When visitors came into the lab., did he tell them what to do or rather suggest?

AB: He'd angle around to find out what they knew that would do him some good and then keep asking questions...

VM: In order to provoke them to go in the direction he wanted?

AB: ...in order to get them in the direction that he wanted. That was the secret of his success. He was excellent at asking questions!

VM: What about the building? People have very fond memories of that shack. Do you have fond memories of it?

AB: Oh, yes. I have memories of the Rat House, too.

VM: I can't share those with you because I didn't see it. But the ORL I certainly can.

AB: But the Rat House didn't include much of a group. It was mainly Sam and I who were involved. Bill Libby's lab. was right next (*to ours*), in the same room, practically and Barker I knew and Hassid and so forth (*in LSB*).

VM: What do you think was so special about ORL?

AB: Calvin was darn good. He'd get there before eight in the morning because he always had an eight o'clock lecture or something like that. He would always come in and ask "what's new?" Before he went home in the evening, 5:00, 5:30, he'd always ask "what's new?". I learned after a while not to tell him everything that was new because we had to have something in our pocket when we really have to show up with something new. Once in a while — I think Al understands that, too.

VM: The people that we have already talked to mention that the physical structure of the building, for all that it was an old crummy building, it was a pleasant place to work and it was socially very integrative, people were pushed together and they liked working together. They liked the central feature of the big white table.

AB: That's true. It wasn't so bad because it had new shiny floors and new benches and the best white porcelain lab. sinks in the world because everybody that had these grey old duriron filthy things and had lousy faucets. I had the best chrome-plated faucets in the world. Over every lab. bench was a line of two-lamp fluorescents, solid, so it was brightly lit and that makes a lot of difference. Most people don't realise that.

VM: Had you ever worked in a building before with big labs. like that, or was your experience in small labs.

AB: Cal Tech. They were big labs. but there was not the interaction with others. Every student had his own independent project; I knew what they were. The interaction there was with the postdocs. That's darn important. In ORL there were a lot of postdocs. and the experience for students was extremely valuable as you well know.

VM: There were probably more postdocs. through that place than there were students, weren't there?

AB: Yes. It was a real...Murray will attest to that.

VM: What do you think of the Round House that was built obviously much later, a decade nearly after you left?

AB: It was just too much stuff! It was spread out in too many directions and I don't feel that anything significant developed out of it.

VM: As a building, given the opportunity, the fact that ORL got demolished, that they were pushed down to LSB in not very satisfactory accommodation. And the opportunity arose..

AB: I don't think I could knowledgeably make a comment that. It may be OK but the people who worked there will know best, not I. I just saw it. There are these fancy EM and NMR, and all that stuff in different places around the perimeter. That was a nice idea. We spent most of our time around the big white table; there was enough room for everybody.

VM: Well the big white table was still there but, of course, the group had become much bigger and I think it grew to nearly ninety or so in the Round House.

AB: I think Melvin's Friday morning seminars were very effective in getting bashful people to get up and tell their story.

VM: As I started to say earlier, you left in '55, was it?

AB: The end of '54.

VM: Where did you go from there?

AB: I didn't know where I was going for a while, except that I had to go.

VM: Can you tell us why you had to go? OK.

(*Benson shakes his head*)

VM: Where did you go?

AB: To Penn State. I had a brother-in-law who was a Professor of Geophysics at Penn State and he and I were classmates at Cal Tech before that. He married my sister, and she came to my graduation. Millikan gave us our PhDs. They were settled in State College, Pennsylvania, and I got an offer to go there so I went. It turned out to be a very good opportunity because the chairman of my department, Agricultural and Biological Chemistry, was very generous. He gave me a decent lab. and I didn't have to do much teaching. They were real bright students because there was a very strong Chemistry Department there. I made a lot of good progress in lipid chemistry. Our article in B&BC 1000 is on the discovery of phosphatidyl glycerol which is the main phospholipid (*indecipherable*).

VM: How long did you stay here?

AB: Six years or seven.

VM: And then you came here?

AB: I decided I didn't want to stay forever in Pennsylvania; it was too hot in the summer. It was OK but we invented the sulpholipid and a lot of neutron activation chromatography which, I think, was clever, but nobody uses the (*technique*); stupid people. One of the guys next door has got a job in the University of Rhode Island Marine Center and they've got a nuclear reactor right there and he could do fantastic things. As long as you have a free nuclear reactor, you can run with it. Boy, it's just terrific. See that tube? It's yellow because of the radiation? You put a chromatogram in there and hot-up the P^{31}...

VM: That's right. You were doing this...

AB: ...to make P^{32}. So we got a way of quantitative analysis of phosphate compounds, phospholipids, doing kinetics of a lot of things.

VM: I remember your work on that.

AB: You just saw of the top and take the chromatogram out and put it on a film after the silica decays, aluminium or whatever with a short life.

VM: How long do you have to leave it in there?

AB: A few hours to overnight.

VM: And the paper doesn't deteriorate significantly?

AB: That's the limiting factor. I got the smart idea of rolling up, ironing mylar polyethylene sheet onto the paper and you can roll that up and activate that and the polyethylene holds the paper together. The only trouble is that the SOB, at Dow Corning?, who invented Mylar...found a long-lived...very dastardly interference from some radioactivity. It turned out to be...not scandium...a catalyst for the polymerisation of the mylar, and it was the secret part of the patent. I tried to get information. I got German Mylar, Japanese Mylar; it's all made by the same du Pont patent.

VM: And it interfered with your phosphorus?

AB: So that was impossible. You're right — the fragility of the paper...What we learned out of this was that the compounds whose spots are radioactive, but it's no longer the original compound, it is just recoiled phosphorus, so we got into hot atom chemistry for quite a while and did some important things that are now turning out to be pertinent to the origin of life kind of stuff.

VM: When you left Penn State did you come directly here?

AB: I came to UCLA. My classmate from Cal Tech, Jim Mead, was the director of a major lab. in the med. school there and he offered me a job at reasonable income.

VM: You weren't there very long, were you?

AB: Just a year. Then I had a choice of staying at UCLA in their biology-botany department and the med. school, or else coming here. Back in '54, when I was in ORL, two characters from Scripps here invited me to come down and try carbon

measurement of phytoplankton productivity. So I came down with some counters and we went off on the ship chasing algae in the ocean. That was in '54. The guy who invited me is being posted some place on Friday so we have a big party coming that.

VM: Who was that?

AB: His name is Bill Thomas. He is still going. He is studying the ice algae up in Yosemite Park; these are photosynthetic algae that live in the ice.

VM: Are you still doing experiments here?

AB: Yes. Not any good ones, but that's what this is all about, I guess. I bought $1,000 of radioactive methanol and I just dilute it. It came in one lambda of methanol in a Gregovsky ampoule and I had to transfer it to a couple of cc of water because I'm going to use it in water, anyhow. Dick Lemmon pointed out that it will be killed by radiolysis.

VM: Do you complain?

AB: People complain about paper chromatography being slow and tedious and all that and it's not. The amount of time you spend on a single analysis is very little. You just have to wait for a couple of days.

VM: Do you do lots of them at the same time, don't you?

AB: Yes. You can think about something else! One important part is making every minute count and not let anything hold you up, like going to dinner, so you can turn the chromatogram at the right time of the night. That way, you can get things dry in a hurry. We have speeded things up a lot. I can come up with a product maybe twice as fast as anybody else.

VM: In the 15 years after you invented that paper chromatography technology, we haven't improved it a great deal. We have a bit. We got some alternative solvents because the original solvents were not good from every point of view, we have improved the counting techniques somewhat, on paper we have an automatic counter and things of that sort.

AB: I didn't like that. You cut up the paper and then you ruin the whole advantage. You can't store these pieces.

VM: But we had a lab. notebooks; you didn't have a lab. notebook. You can store the film.

AB: No, we don't have a lab. notebook. Who needs a lab. notebook?

VM: I tell you that when you are faced with counting thousands of these spots, making the thing automatic was a revolution.

AB: Maybe.

VM: There was no way any of us were going to do it the other way.

AB: Did Alex Wilson do that or was that done after Alex?

VM: The automatic thing? I did that with Karl Lonberg. That was my contribution. And that machine, in a version that Al modified, is one of the few relics that remain in the AEC/Radiation Lab. storage warehouse in Emeryville. That's there.

SM: But it's mislabelled.

VM: It's no longer mislabelled, I hope because I told them built it and who designed it; I hope it is no longer mislabelled. They originally had it listed under the name of the technician who modified it. So I sent them a copy of the original paper and showed them that wasn't the case.

AB: The glassblower's name was Harry, somebody. That part's right.

VM: I'll ask other people around and see if anyone can remember the glassblower's name. Marilyn will probably remember.

AB: Oh, I've got it. You have never seen my slides? They are labelled like that, thousands of them.

VM: My God!

AB: You don't have to look at the pictures. You just read the titles and you know exactly what's on the film.

VM: "John Lawrence, '45"; God, it must be marvellous looking through some of these.

OK, well I think it remains only for me to thank you very much indeed for all the time you have spent with us. And shut the tape down.

DR. BENSON DID NOT WISH TO COMPLETE THIS FORM
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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name ANDREW ALM BENSON

Date of birth 24 SEPT. 1917 Birthplace MODESTO CA

Father's full name

Occupation Birthplace

Mother's full name

Occupation Birthplace

Your spouse married 1942 and 1971

Occupation Birthplace

Your children 2

Where did you grow up?

Present community La Jolla

Education BS (UC Berkeley) 1939 Ph.D (organic chemistry) (Cal Tech) 1942

Occupation(s) Professor of Biochemistry and of Plant Physiology (retired)

Areas of expertise Path of carbon in photosynthesis; tracer methodology, lipid metabolism, and others

Other interests or activities

Organizations in which you are active Nat. Acad. Sci, Am. Soc. Biol. Chem, Am. Soc. Chem, Am. Soc. Plant Physiol, Royal Norwegian Soc. Sci + Lett

Chapter 13

ALEXANDER T. WILSON

Tucson, Arizona

June 5th, 1996

VM = Vivian Moses; AW = Alex Wilson; SM = Sheila Moses

VM: This is a conversation with Alex Wilson in Tucson on Wednesday, June 5th, 1996. Can we start, Alex, with your early history and how, in the first place, you ever got to be in Calvin's lab.?

AW: From when I was educated in New Zealand?

VM: That's right and what brought you to Berkeley eventually.

AW: When I was 16 I left school and got a job in the Chemistry Division of DSIR and I went part-time for the first year and then I went full-time for the next couple of years to get my bachelor's degree, and I worked in the analytical section and the sprays and insecticides section of the New Zealand DSIR. Then, I did my master's degree on the chemistry of tutin and picrotoxin (*spelling?*) which is a poison found in a New Zealand plant. (Tutin gets into honey sometimes and kills people.) I then graduated with a masters degree and first class honours and I was awarded a Fulbright and I went to Berkeley to work with Melvin Calvin.

VM: You knew about him at that time.

AW: Yes. I was very interested in the kind of chromatography and all that sort of stuff. In the last part of my time at the DSIR I have been trained, there were just two people — myself and the person running the section in radiochemistry that started a carbon-14 lab., which was one of the first carbon-14 dating labs. in the world — so I was quite interested in radiochemistry. So, I went to Berkeley. I was pretty young, I was only about 20 and I found Berkeley a very interesting place, coming from a small place like New Zealand. In fact, I could go up the hill behind the University and see more people in the Bay Area than there were in all of New Zealand at the time. Berkeley was the biggest university in the world at the time, 26,000 students, and I was living at International House which was half Americans and half foreign students. I found the whole system incredibly interesting.

VM: But you weren't a biologist at all, were you, before you went there?

AW: I had done my masters degree in organic chemistry but I was very interested in physical chemistry as well, and all branches of chemistry, really. I didn't know much about anything else than chemistry. In fact, my thesis with Calvin was basically a physical-chemical kind of thesis, although it was on a biological subject. In fact, in my prelims. they started asking me biochemical questions — they had a couple of there — and I didn't know much biochemistry. It developed into a big slanging match between the chemists on my committee and the biochemists on the committee as to how much biochemistry somebody doing what I was doing was doing. They finally compromised by saying that I had to do first year of biochemistry.

VM: This is when you first started, when they were setting up your courses?

AW: No, no. When I went for my prelims., which was a fair way along in my course. I had picked up a bit of biochemistry but I had no systematic knowledge of biochemistry. I was just looking at the system as a kind of dynamic circular chemical system.

VM: Can I backtrack just a bit, to before you went to Berkeley. Did you expect to work specifically with Calvin specifically on and specifically on photosynthesis? Was that your objective?

AW: Yes I did, That was my objective, although when I accepted and got there I was told that I didn't have to work with Calvin, I could work with anybody. But I still was very interested. Calvin was a very interesting person.

VM: Had your communication been with Calvin?

AW: Yes.

VM: Where did you meet him for the first time?

AW: I didn't meet him until I got to Berkeley. I had a scholarship to go to England but I decided that I would sooner go and work on (*photosynthesis*). I thought it was a fascinating subject because you are doing some pretty interesting things with such pretty simple systems, like co-chromatography and all that sort of stuff.

VM: Do you remember the first time you came face to face with him?

AW: Yes. We just sort of chatted. I was very interested in how interdisciplinary he was and, in fact, that's something I learned from him, to be interdisciplinary, basically to take knowledge) from one subject, techniques from one subject, and exploit it in another subject. I am probably now even more interdisciplinary work than he was because I have wandered all the way from chemistry through biochemistry and all the way to earth science now I am actually working on trying to recover information from ice cores.

VM: Which has a meteorological bent to it?

AW: Well, paleo...and the greenhouse problem.

VM: When you first got there and had your first conversations with Calvin about what you were going to do...do you remember?

AW: He said " we don't know what you're going to do. Why don't you just work in the lab. and help the other people and eventually we'll find something, we'll stumble across something which will make a good thesis" .

VM: What happened then?

AW: That's exactly what happened?

VM: Who did you work with?

AW: I worked with Andy (Benson) and some of the other people there, Al Bassham, and I found it very interesting. I learned a lot, about a lot of things.

VM: Were you working as it were as their assistants in what they were doing?

AW: I was helping them with experiments. The mistake I made at Berkeley was that I put too much time into my lab. work. You see, most graduate students who were there spent all their first couple of semesters doing course work whereas I was working in the lab. every day. I was good at working in the lab. and I had a lot of lab. experience, a lot more than most people of my age, because I had done so much work in working for the Chemistry Division (*of DSIR*) in New Zealand. I was probably a pretty competent experimentalist.

VM: Did they give you credit for the courses you had taken in New Zealand or did you still have to do what all the Americans (*had to do*)?

AW: No. I was obviously not going to get any credit, so I decided rather than do it in organic chemistry I would take the degree in physical chemistry. Besides, Berkeley was very, very good at physical chemistry. The average organic chemist at Berkeley knew as much about thermodynamics as most physical chemists anywhere else did. It was a very, very strong school in thermodynamics.

VM: And the Fulbright fellowship was going to support you for the whole of your period while you were there?

AW: Well no: it provided me transportation from New Zealand and I had some other kind of grant, which was related to the Fulbright, which gave me a certain amount of money every year. And then I got a teaching assistantship which actually turned out to be a very good thing because it gave me enough teaching experience to get a job at a university later in my career. Although, at the time I wasn't too interested in going into academic work because I'd worked in a government lab. all my life.

VM: How much time did the teaching occupy?

AW: The teaching was kind of interesting. At Berkeley (*for freshman chemistry*) they had 25 rooms (*sections?*) with about 25 students in each. They gave them an aptitude test and they put the best students in Room 1, the next best students in Room 2, the next best students in Room 3. They put all the odd people in Room 25 — and I got Room 25!

VM: You were the bottom of the heap, were you?

AW: Since I was a foreigner and actually was quite good at talking to people who couldn't speak English very well, because I was living in International House, they gave me all

the people from Iraq and wherever. Most of them had already done some chemistry. When the exams came out, we came about fifth among all the things, so they gave one foreigner to look after all the other foreigners, which I thought was pretty astute. I would have spent, I don't know, I don't really remember, but it wasn't not all that much and I found it (*the teaching*) quite interesting.

VM: Aside from that, you worked essentially in ORL all the time.

AW: Yes, I would go to ORL and then go off to lectures. I had a chance to go to Alaska the first summer I was in Berkeley and I decided to work in the lab. and not go to Alaska; I regretted this later. But, I have since been to Greenland which is even more interesting.

VM: Did you have your own bench eventually in ORL?

AW: Yes, I had a bench all the time. In fact, I shared Andy's office with him.

VM: Did you; I see. In ORL?

AW: In ORL.

VM: At that time the big white table was there already?

AW: Yes.

VM: And you congregated around the table? Who were your contemporaries as you remember them at the time?

AW: There was Al Bassham and Andy, of course, and a little coloured girl whose name I don't remember who did the lab. looking after, there was a woman who grew algae, I have forgotten her name.

VM: It wasn't Althea Vann, was it? Was she a redheaded woman?

AW: No, she was a tall thin lady. I think maybe her husband was working there, too.

VM: In that case I don't know who it is (*Editor: perhaps Louisa Norris — Dr. Richard E. Norris was a postdoc. in the lab.*).

AW: They were working on trying to grow algae to get oxygen for submarines and things. Then there was — who is the lady who lives up (*in Northern California*)?

VM: Lorel (*Daus*) Kay?

AW: Lorel was sharing the other side of the bench with me. There was — who was the graduate student that died?

VM: Dan Bradley.

AW: Dan Bradley. Dan and I were about contemporaries on the PhD. And the guy you're staying with..?

VM: Murray Goodman.

AW: Murray Goodman; he had been doing a PhD for quite a while. When I was there, Professor Nordal, do you...

VM: I've never met him; I know his name though.

AW: He was a professor, a really old person, a professor of pharmacology from Norway, an expert in sedoheptulose and was extracting sedoheptulose out of avocados. He was quite a character. In fact, he used to give the girls a hard time. In fact, they wouldn't go up to the chromatography room unless I came with them! (*Laughter*) The first sexual harasser I'd met!

VM: He was there, presumably, for a fairly short period, was he?

AW: I suppose so. I was there for three years and people came and went.

VM: :As your period in the lab. developed and time passed, it became clear, presumably, what you were going to form your thesis on, or did it? How did your thesis work actually develop the way that it did?

AW: When I got there, I got there at a very good time, they were just doing the lollipop experiments where they were feeding carbon-14 bicarbonate to algae in the fume hood and then they were trying to do short experiments and controlled time experiments and chromatographing the result and counting the spots. As time went on, and then Peter Massini turned up and actually a Japanese person turned up who came from a big company in Japan; he was an expert in photometry and how to do absorption spectrum work through light-scattering materials. (*Editor: this was probably Kazuo Shibata*). It was kind of funny as he could understand me quite well I had a very strong New Zealand accent and everybody in the lab. would joke about my speech, you see, they couldn't understand me. In fact, I had a lot of trouble originally. The first week I was there I went down to the store and asked for a cork to go in a bottle and they said "the machine's upstairs" . They thought I said a coke! I had terrible trouble with New Zealand slang. For example, the first Monday I was there we were doing this aptitude test for the first-year students and we were sitting in a big room full of other graduate students, and we were grading the results. I made an error: I said " does anyone have a rubber?" (*Laughter*). There was kind of a deathly silence.

SM: We have had similar linguistic experiences!

AW: At any rate, they had trouble understanding me initially. I got better at it as time went on and I spoke a lot more clearly and I didn't drop the ends of my words like New Zealanders tend to do. But the funny thing was that this Japanese guy goes over to Calvin's office, which was over in the Old Chemistry Building, and Calvin called me up and said "Alex, come over and translate for me" because Calvin couldn't understand this Japanese guy and the Japanese guy couldn't understand Calvin. I sat there and I just translated!

SM: Presumably the Japanese guy thought he was talking English?

AW: I could understand him because I was used to talking to foreigners at International House and I could talk slowly and he could understand me and I could understand him.

VM: As your work developed in the lab., when did your thesis direction become clear to you and to others, I guess?

AW: It slowly became clear to everybody that in a cycle like a photosynthetic cycle the reservoirs aren't constant sizes, in other words, they fluctuated wildly, depending on how much CO_2 or how much light or whatever; how much everything was. So, I had a bent toward instrumentation so I acquired the idea that maybe we could follow this by labelling. Instead of doing a tracer experiment we could use the carbon-14 to label everything and then change something and see how the reservoirs change. I had to quantify the whole thing. I had to make sure I could run good chromatograms and I worked out that if you humidify, raise the humidity, on the paper for the phenol you got a much better chromatogram. I had to dry the paper so that I got — I couldn't dry them from one side because I would get all the radioactivity on one side, I had to dry them evenly, and I developed a technique for spraying a phosphatase on the spots so that I would turn the phosphates into sugars and then I could elute them easily. I was running a tremendous number of chromatograms so I had to automate the whole thing so it went a lot quicker and I could actually do the work. I would set up the experiment and then everybody in the lab. would help me for 30 minutes while I took all these measurements on the samples. I had to develop the whole technique of building this fancy piece of apparatus which would control everything and doing all the chromatograms and then making the Scott counters work properly so I could get good data, and I had to have internal standards. Just doing one enormous experiment which took a long time and then doing another one.

SM: When you say that everybody would come and help you while you were counting?

AW: Doing the experiment: I would set it up and then everyone would come and would pass me the tubes and I'd take the sample and...

SM: So this was indicative of how people worked together in the lab.?

AW: Yes. Everyone got along very well together. We used to go on trips to Yosemite and we went on another trip to Pacific Grove to see the Monarch butterflies. It was a really nice group of people.

SM: That would have been in the autumn, wouldn't it?

AW: Yeah. Actually, there were only two graduate students in the whole lab.. It was really a much more industrial kind of situation.

VM: The way you describe it, it sounds as if people were not very proprietary of what they were doing individually but it was very much a team effort.

AW: That's right. We used to have a meeting every Friday morning and discuss what everybody was doing. And then Calvin would come to work on Monday morning with some kind of crazy idea he had thought about over the weekend and I came to the conclusion that people don't have better ideas than anybody else, they just have more ideas! (*Laughter*). Which is a pretty important point, I think. He was always coming up with ideas, many of which he filtered out himself, and the other ideas we would have to filter them out. One in every hundred ideas turned out to be a good one.

VM: Presumably the rest of you were thinking up ideas also. It becomes a habit, doesn't it?

AW: There was a big battle in the lab. as to whether there was a two carboxylation cycle or one carboxylation cycle. Calvin was the last one who believed the path of carbon in photosynthesis.

VM: What did he think were the two carboxylation events?

AW: He got influenced by the original kinetics that were done where we were doing the lollipop experiments. He got misled because the ribulose reservoir was building up as we let the algae just sit there. When you pushed in the bicarbonate, it immediately turned into CO_2 and the ribulose diphosphate dumped into PGA. After the cycle had settled down it would go on normally. It looked like there were two carboxylations. Of course, he was trying to analyse the kinetics. In order to analyse the kinetics he had to assume a steady-state which it wasn't. So it was kind of interesting.

VM: The rest of you argued against him, argued with him, that that was not the case.

AW: Right.

VM: So, the group of you foresaw the carboxylation of ribulose diphosphate at an early stage. Can you remember who first conceived of that carboxylation?

AW: It originated, the fact that the kinetics didn't work out very well meant that we didn't understand the whole system. Then Peter Massini did an experiment, turning the light off and on and noticed that when you turned the light on, within 30 seconds the ribulose diphosphate went down and the PGA went up by about the same amount. So it looked as though these two were related to one another but that wasn't good enough evidence by itself. The concept of fluctuating reservoirs and the concept of using, say, the cutting off of CO_2 or lowering of the CO_2...You see, I didn't cut it off; I took it from about 1% to .03%.

VM: What did you have, two big vessels?

AW: Two big vessels full of CO_2 which had the same specific activity, one was running at 1% and the other was running 0.003%.

VM: There must have been a fair amount of radioactivity in these vessels.

AW: I'm glad I wasn't doing carbon-14 dating at this time. They were pretty big vessels, sort of...

VM: Two or three feet across?

AW: Right. And you had to have bladders in the air. It was a pretty tricky experiment to get it organised so it worked and you had to monitor the CO_2 concentration, which I did on a recorder, and I had an infrared gas analyser in the stream. It was probably one of the first of the really controlled experiments. It worked very well. We got very good results.

VM: This was the sort of experiment that you were describing earlier, where everybody had to chip in.

AW: I needed them at the very beginning because I needed to slow the system down. I had the whole thing cooled to 6 degrees. I had a big thing of ice and I was taking water off the bottom of the ice. You know that if you put water in a Dewar with ice at the top,

then the bottom of the Dewar is 4 degrees (that's the maximum temperature of water), I was taking that and putting it through my jacketed vessel, so I could slow it down to Peter Massini's experiment which was done at room temperature. I had to work really quickly, so I would take the samples as quickly as I could at the beginning and for this I needed some help, not for very long, just for the first 12 points or something; after that I could do it and it continued on for quite a while.

VM: Did you have to run this big experiment more than once?

AW: Yes, I ran it more than once and I ran it going both ways, going from 1% to low CO_2 and when that had equilibrated I took it back up to 1% again, and so the whole thing happened in reverse.

VM: That must have been a massive job with all the chromatograms you were running, spots you were counting.

AW: I had to develop evaporators for evaporating things on a massive scale, like this octopus deal where I had these little tubes, maybe 12 or something like that, all hung up with rubber tubing and being agitated so they wouldn't bump, to evaporate these things down. I had a system for transferring spots off a cut-off spot onto the origin of a new chromatogram.

VM: How did that work? I don't remember seeing one of those.

AW: Do you want me to show you a picture?

VM: Yes. Let me stop while you go and get it.

We are looking now at the paper: *The Photosynthesis Cycle: CO_2 Dependent Transients* and this is JACS, is it?

AW: It looks like it:.

VM: Volume 77. It shows the octopus. That's that; I can see that This was your elution apparatus? That's on page 5951 if we go and look at it again.

AW: I had several of those so I could elute several spots at once. The trick was to do the phosphatasing on the paper; so I would cut out the spots I wanted and then I would spray it with phosphatase, and then I would hang it in an atmosphere saturated with water and toluene, so the bugs wouldn't eat it, and that would turn all the phosphate esters into sugars, and then they were much easier to deal with. You can't elute the phosphate off quantitatively. I had to do everything quantitatively.

VM: It was actually quite a big gamble, wasn't it, that the experiments would really work, because there's a lot of elaboration in what you did and things might not have worked out. But, you know, I think it was not only well conceived but well executed and you were lucky. You might not have been as lucky.

AW: Mind you, I didn't have go get that far into it in order to find it wasn't...That's something else I learned: don't invest too much time until you know it's going to work.

SM: You designed this apparatus, did you have to construct it yourself, or did you have it built?

AW: I mostly constructed it myself but they had a workshop and they would make things that I wanted. It was a pretty impressive looking piece of apparatus.

VM: That formed the main thrust of your thesis, did it?

AW: The thesis that I presented, yes.

VM: This proved definitely the cycle existence and the transients in the cycle.

AW: It showed two things. It showed that the thing upstream from CO_2 was ribulose diphosphate or something that's very closely related to it. It might have been a product from it that had a very short lifetime or something that was very small and you couldn't pick it up on a chromatogram. And it showed that probably in cyclical biological systems the reservoirs can fluctuate up and down very quickly and you could see the wave go back and you could see the wave go forward if you look at these pictures.

VM: Yes, that's right. I remember these and I remember how important they were at the time. This is the one on page 5952. These successive waves going around the cycle, in both directions, and then they conflict in the middle.

AW: I wondered at the time if you couldn't use it for other things, like determining what compound picked up nitrogen in nitrogen fixation. I considered doing that when I was back in New Zealand but I never really got to it.

VM: The difficulty, of course, is that there's no radioisotope...

AW: I could have labelled the carbon compounds in, say, *Azotobacter* or something.

VM: But by then you were on to other things.

AW: I was doing other things.

VM: I don't think anybody did that experiment, that I recall. What about life in the lab. as you knew it? You worked, as you said, in the main room of ORL; you worked in one of the....

AW: No, I didn't. Actually, if you leave the room that had the big white table and you walk through, on your left there was a place where they washed the dishes. My bench was right there. There was a double bench there, room enough for four people, and my actual bench was right there. Who was the girl who lives in California?

VM: Lorel Kay.

AW: Lorel Kay worked next to me. If I looked through to the other side, my apparatus was on that wall. If you turned to the left there was an office. Andy had the desk towards the bay and I had the other one.

VM: There was free movement of people, of course, in and out of the building and all around the building all the time, wasn't there?, and you were running up and down the stairs to deal with the chromatograms and into the cellar to deal with the counting. Earlier on, you were telling us some entertaining stories of your life in Berkeley — what you did in International House with straws, for example. Well, why don't you...?

AW: There were a lot of interesting people at International House and International House was run rather autocratically. We used to think of things, while we were having our evening meal, we would think of all sorts of terrible things to do, one of which was to take the cover on the straw and dip it into some honey and shoot it up at the roof of the dining room which was a very, very high roof. Of course, it would go up and it would stick on the ceiling and just hang down. By the time I left there were hundreds and hundreds of straws up there. Not that I put them there; it was just that other people took my technology and...

VM: You did not experiments with straws did you?

AW: I thought it was going to work but I didn't want to be thrown out for doing it, you know. You can't get thrown out for thinking of something with intellectual input but you can get thrown out for physical input.

VM: You didn't have to persuade people very hard to try. What else did you do at the straw International House which was entertaining?

AW: We used to think of all sorts of things such as how to get from the boys side to the girls side using the freight elevator. We never actually did it, but we actually got accused of doing it. When e were called on the mat for doing it said "Did anyone ever do it?", but nobody ever did. We had a lot of theoretical pranks that we got up to.

VM: I don't know the structure of d International id House because I never lived there. There were two wings, were there, boys in one and girls in the other?

AW: There were different floors for boys and girls.

VM: How did they aim to keep you apart? Was it clever what with the staircases going to one set of floors....

AW: Yes it was, that's right.

VM: But the freight elevator notionally served all the floors?

AW: You could have done it with the freight elevator but I never actually did it with the freight elevator because I had no one to go to on the other side.

VM: That's hard luck, isn't it? Then you were telling us that you had interesting questions with your immigration papers.

AW: Miss Kittredge, who later became Mrs. Wilson, was a really interesting person.

VM: Not your Mrs. Wilson?

AW: No.

VM: She didn't marry you?

AW: No; the reason I remember her name is because it was the same as mine. But she used to run the Chemistry Department with an iron hand. She used to do the most incredible things. Once, actually every semester or every year or whatever, I had to get something from the University saying that I was a student in good standing. I

would go to Miss Kittredge and I would say: "Miss Kittredge, I need something for the immigration department". So she would get out an official piece of paper, type a letter, sign "K. Pitzer" on the bottom and give it to me. Which I was pretty impressed; it didn't take very long, just took a couple of minutes.

VM: And the immigration department was quite happy with this!

SM: Except that Calvin had suggested that you go and consult her on something...

AW: There was something else. I don't remember what it was, but I asked him if I could do something and he said "Alex, go and see Miss Kittredge. She's the one who's going to have to make up her mind eventually, anyway!"

VM: You told us also that you had an interesting run-in with the question of signing some security document.

AW: I came to Berkeley as a young person of about 20 and coming from a small country I really had trouble treating America very seriously. I entered in the middle of the McCarthy era and two things happened to me. One was, after I had been there for a very short time, somebody called me up and said that I would have to go and do an English test to show that I was proficient in English. I said to them, "I speak the Queen's English; what do you speak here?" They finally let me off.

You see, not too many New Zealand students had been to Berkeley; now they are much more sophisticated about what there is in the world. The other thing I had to do was sign the Loyalty Oath which said that I was not a communist, a member of the Communist Party, and that I promised I wouldn't overthrow the government of the United States with force. I always thought this was kind of strange because I came from another country. I read on the bottom of it that I was subject of Her Majesty Queen Elizabeth II and I wasn't too sure what plans she had for the United States. I felt that I really couldn't sign this. They let me get away with it, which is pretty incredible, really. I might have been the only person in the whole McCarthy era that didn't sign the Loyalty Oath.

VM: I suppose, I don't know what the rules were. I think that was a splendid thing to have done. (*Laughter*).

AW: I had a humorous bent at the time.

VM: Was the lab. atmosphere generally jokey at that time?

AW: Yes, it was.

VM: Light-hearted?

AW: It was light-hearted.

VM: But people worked hard and long hours, did they?

AW: I worked very long hours. They used to publish papers with lots of lots of peoples' names, everybody in the lab. on it, with Calvin at the end. Andy used to say this is what I call "isotopic dilution", which I thought was very funny.

VM: But Calvin contributed to the papers, didn't he; discussion and he used to look at the manuscripts?

AW: Yes. Actually, something very funny happened. Who was the English guy that studied...?

VM: Grant Buchanan.

AW: Grant Buchanan came and he lived at International House so I knew him quite well. He came, very English....

VM: Scottish, excuse me.

AW: OK, Scottish. I remember the first day he was there. Calvin has just written a paper. Calvin used to dictate these papers and Marilyn used to straighten out all the English. So, Calvin gave him the paper and asked him if he wouldn't mind reading it over the weekend and commenting on it. He went through and corrected all the grammar and Calvin didn't even know what a split infinitive was; and Calvin got pretty upset about this. I thought it was kind of interesting.

VM: Yes: kind of interesting. Generally speaking, I thought that Calvin was pretty good on manuscripts. I must say that I don't remember correcting his manuscripts, but he never used to play too much with my manuscripts.

AW: It's just that Calvin didn't worry about things like split infinitives!

VM: No, that's right. He was, of course, very keen to publish things and make sure they were published competitively early and that nobody else got in. When you were working on these kinetic problems was there any sense of other people in other labs. working on the same sort of thing?

AW: No, I don't think so. I think we were the only people in that business. I don't think anybody had even conceived of it. Maybe even today not too many people appreciate it.

VM: What did you think of the lab. as an institution, as a group of people, and the structure of it, led by Melvin and the way it was run? In the light of what you saw there and what you have seen later, how do you look back at that lab.?

AW: I think it was a very nice group of people to work with. Everyone was very friendly. As I say, there weren't that many graduate students. I think I was very fortunate to be in that particular environment. I kind of learned a lot of things, one of which I talked about before, which is good people don't have better ideas than other people, they just have more. The one thing was that people...I was quite impressed by how Calvin would, perhaps subconsciously, reorganise the order in which things were discovered to make it appear more logical. Because it was just too much trouble, probably, explaining to people what really did happen...

VM: ...in terms of the sequence of events.

AW: Yes, the sequence of events. The fact that a lot of things were discovered kind of accidentally.

VM: Such as? Got any examples?

AW: In a way the experiment that Peter Massini did. They weren't looking for that, they were looking for something else. Something which is very interesting, I found, later going into isotope geochemistry. We used to talk about what are now called the C4 plants and how they stored malic acid at night, and we all knew about that. Most isotopic geochemists, stable isotopic geochemists, knew that there were two kinds of plants, some which had C^{13}s of about 12 and others that had C^{13}s about 25. It was amazing that we didn't realise that there were different kinds of photosynthesis, or rather the C4 plants, I guess, just have a thing in front. It wasn't until the Australians...

VM: Hatch and Slack, wasn't it?

AW: He worked for a sugar company, therefore he was working on sugar cane, and he noticed that when he did a lollipop experiment, or the equivalent of it, he got a great mass of malic acid. That's kind of interesting because when I first came to Calvin there was somebody else in Chicago, who was a competitor of Calvin's

VM: Gaffron, I think.

AW: Gaffron; well, he was into the malic acid cycle. He was into the Krebs' type of acid cycle and not into the phosphate business. So in a way, it was kind of interesting how this thing should have been so obvious to everybody, and it took somebody else in Australia doing a completely similar experiment to discover C4 plants.

VM: I wonder whether that isn't a reflection of the very great dedication to solving a particular problem.

AW: Yeah, I think it might be.

VM: Maybe one gets a bit blinkered. Your eye is so fixed on the ball that you tend perhaps...

AW: In fact, we were really studying the path of carbon in photosynthesis in algae.

VM: Yes, and assuming that everything else was going to be the same.

AW: And yet when you live in the desert, like in this (*Editor: i.e. Arizona*), you can see that what the plants do, they take in CO_2 at night, when it's humid and they're not going to lose too much water, and they close up and make malic acid. In the day, they turn malic acid back into CO_2 and run up through the Calvin cycle.

VM: But still, I think you will agree that to tackle the problem as it was perceived in the mid-forties, of how do plants fix CO_2, to choose a system with which to do it, to choose a system that, in the end, the one that they chose you could work with, and to push the thing through was a very considerable...

AW: I am really more wondering why I, myself, didn't realise it. I knew there were two kinds of plants.

VM: It's difficult to think back, isn't it?

AW: Something can be staring you in the face and you don't see it...

VM: ...until suddenly it clicks and then you realise...You commented earlier on about people discovering things serendipitously, not necessarily by looking for them. Does that come directly out of your experience with this?

AW: I think that people discovered the...it should be pretty obvious to a physical chemist that if you have a cycle, the reservoirs should change up and down. But nobody ever thought about it. They thought that the reservoirs were fixed sizes.

VM: Even under the conditions in which you actually handled the material? Because you took the algae out of a growth culture of some sort and centrifuged it, and resuspended it in some other medium convenient for the purpose. You then put it in the vessel in which it hadn't been, shone lights on it, and it didn't dawn on all of you that things were happening inside the plant when you did that? And, in spite of all that, you felt, or some people felt that...

AW: You did want to make the plant happy but you didn't want to put too many nutrients into the suspended liquor otherwise it would ruin your chromatogram. You assumed that it was happy and then you squirted some carbon-14 bicarbonate and tried to find out what the first thing was and the second thing and the third thing. But it took a long time to figure out that if you did the kind of experiment that I did it gave you a chance of seeing what was upstream. That was going from a static system to a dynamic system.

VM: I think the idea that if you are looking for a simple linear sequence of events it really doesn't much matter what happens to the pool sizes. It's only when you get the complicated interactive events which, by the time you were working in it, had become all-important to the (*problem*), that the pool sizes can make or break your understanding of what goes on.

AW: We were really trying to see what was upstream and what was upstream of that, which was the triose phosphate.

VM: When you were doing this work, which would have been...you went there in 1951 and stayed until 1954?

AW: Yes.

VM: So you would have been doing this work...

AW: '52, '53.

VM: '52, '53. By that time did you have the breakdown of radioactive concentration in all the carbon atoms of all the sugars you were looking at? Was that part of it?

AW: People were doing it, though.

VM: But it wasn't all laid out yet; it wasn't all laid out ready?

AW: People knew that when you took the hexoses, the two middle atoms were hot, which meant that probably the two PGAs came together from a C3 to a C6. But no one had worried too much about what was upstream.

VM: The arguments that later developed about the labelling patterns in the pentoses and the heptoses, of how it couldn't be C1 it had to be C3, etc., was that after you did these kinetic experiments, do you remember?

AW: In parallel, probably.

VM: It was all part of the ongoing thinking of the lab. at that time?

AW: They were trying to figure out the path of carbon to see where the carbons were in the individual six- and seven-sugars and I was really on a different tack altogether. I was trying to do it some other way, by building a fancy piece of apparatus and stopping the CO_2 and seeing what built up.

VM: Was everybody talking all day, every day, about this? Were you aware of what everybody else was doing?

AW: Yes. We had regular seminars every Friday and people had to talk in sequence about what they were doing.

VM: Even that, any individual didn't talk except at fairly long intervals. If you waited to hear via the seminars what people were doing...

AW: People knew what everybody else was doing. Everybody talked about what they were doing all the time.

VM: Continuous topic of conversation?

AW: Yeah, right.

VM: To change the direction slightly, what's your memory of the social scene in the lab.? What did you do for relaxation? Did you tend to mix with other lab. people, did you have your own friends? What did you do?

AW: My social life was more related to International House. Besides, most of the people in the lab. were older than I was. I wasn't married and they were kind of married. I mostly associated with the other members of the group when we went on trips to Pacific Grove or Yosemite or somewhere like that.

VM: You were a mountaineer with them? Did you ski as well?

AW: Yes.

VM: Did you do that often? Did you take a lot of time out of the lab.?

AW: I used to work pretty hard. I used to work hard and play hard. I'd work most weekends and most evenings. International House wasn't that far from the lab.

VM: I think to have accomplished a PhD of the complexity that yours actually was within the period that you did, and to have done TA-ing and to have taken courses, you must have worked pretty concentratedly, over much of the time. What about social events inside the lab. — did you visit one another's houses, did Calvin invite people up to his home?

AW: Not very often. I don't remember socially interacting very much with the members in the lab.

VM: Did their wives and children come into the lab.? You knew who they were?

AW: Yes.

VM: How about the events inside the lab., the Christmas parties? If you were designing techniques for blowing straws up to the International House ceiling, you must presumably have contributed something of the sort to the ORL Christmas events and parties.

AW: I have a lot of recollections of International House, because it was pretty big and the people were more or less my own age. But most of the people in ORL were considerably older than I was and had growing children.

VM: You felt a social gap between you and them.

AW: I suppose so, although I did enjoy going to Pacific Grove and Yosemite and those places.

VM: (*This side of the tape is nearly finished so we'll run it out and open up on the other side.*)

About the ORL itself, people have very fond memories of that building. I think most people who worked in it talk about it with appreciation and they comment on the influence that the structure of the building itself had — no separate rooms, minimum of walls, minimum of dividers, people didn't work behind closed doors, and that sort of thing. What do you feel about that as a way of running a lab.? How do you think it affected the way the group ran?

AW: I think the big white table was a good thing because if anybody wanted to talk about their work they could bring out their chromatograms and spread them on the table and eventually everybody came around and talked about it. I remember, I had done some weird experiment and — who's the guy who stuttered?

VM: Grant Buchanan.

AW: Grant Buchanan; he saw something really interesting in my chromatograms and I gave him the spots. I had managed to break down something by accident which he had been trying to do for a long time. I just stumbled across a way of doing it. I had two extra spots and I didn't know what they were. So, everybody really did know what everybody else was doing. The big Round House was a much greater development than that. The complaints I heard about that was that noise travelled a lot and it was hard to get a nice quiet...see, I could go into the office which I shared with Andy and I would be quiet, although there was a big glass partition there.

VM: But other people didn't have offices, did they? Many people simply had desks at the end of their benches so they did have whatever room noise was going on in ORL.

AW: In the room with the big white table, that was arranged so there was a desk at the end of the bench. But in the lab. I was in, I just had a straight bench, maybe seven foot of bench or something. So I never had any place to sit around in other than the office.

VM: You could go and hide in the office whenever you wanted peace and quiet but the other people really couldn't. I don't remember that there was a great hubbub of noise; there was obviously conversation, particularly at coffee time, but presumably in your day, too, everybody took their coffee together, more or less, didn't they?, and they all congregated around the table.

AW: I was very impressed the way they made tea.

VM: How did they make tea?

AW: Coming from New Zealand...This was in the morning. They would put all the cups out, full of moderately hot water, and take a tea bag and dip it in and say "How many dips would you like?" I remember saying that the Queen would never have approved of this!

VM: Certainly not! For many years we used to import our tea — and even this time we brought tea with us, to be on the safe side. But you did see the round building?

AW: I never worked in it, but I saw it later.

VM: You know what happened. The old building got demolished and therefore they had to move into something new. This (*the Round House*) was an attempt to recreate the atmosphere (*of ORL*) in a modern sense. When you saw it, what did you think of this building?

AW: I thought it was a pretty interesting building. It means that you do interact. At least you know the people. If they are in separate labs...I never had too much to do with the group that was over in Donner. You know there was a group over there that synthesised chemicals and fed them into people and figured out how the hell they metabolised them. Interestingly enough, they are just doing that kind of work again using the IMS in the lab. that I'm in. I never had very much contact with them at all.

VM: Did you know them? Do you know who they were? Do you know their names?

AW: Well, yes, because I met them at seminars. Who was the guy who ran that?

VM: Bert Tolbert.

AW: What happened to him?

VM: He's in the University of (*Colorado at*) Boulder. Well, he's retired from the Chemistry Department.

AW: I knew Bert Tolbert, but I never really got intimately related with what they were doing because they were in a different location on the university.

VM: That's interesting because other people were not in your position. I think that's because you were a graduate student and the people who were older and had grown up before they divided so obviously knew them much better.

What happened to you as a result of your photosynthesis experience in Berkeley? Without spending hours — potted history.,

AW: After I left Berkeley...I really was there for three years, less a summer, and that summer I got into the course that van Niel was running. It was a really interesting course because he would only take people, like me, who didn't know any microbiology but were working in a microbiological field. He was a really interesting person. We went down to Pacific Grove and we rented a house. There were seven of us living in it and we kind of lived microbiology. He would run it three days a week — Monday, Wednesday, Friday — and ran it about 14 hours a day; it was really, really interesting.

VM: How long did it go on for? All summer?

AW: I think all summer, yea. Pretty interesting: I learned a lot of microbiology. He taught it from a historical perspective. We even did things like...I remember we did an experiment trying to see why no one had ever managed to ferment orange juice. You can ferment just about everything, but not orange juice. So we tried to ferment some orange juice. We went looking for crab, for those little crawlies...freshwater crayfish. We had a wonderful time.

After that I drove across the country and had a job at Standard (*Oil of*) Indiana as their radiotracer person. That was very interesting. I worked there for a couple of years and made a lot of money and organised a trip, four people, a Scot person who worked for Standard of Indiana and a Scot person who lived in Canada and a Dutch person who lived in Canada, and we bought a car and drove it down through South America and up through Africa. We sold the car and I went across country to New Zealand. I went back to the job I had originally except that it had changed meanwhile in the New Zealand Institute of Nuclear Science. I did the same kind of work.

VM: Same kind of biological work?

AW: Well — sort of. I was kind of interested in instrumentation and the relationship of one thing to another. I invented the paper chromatographic version of scintillation counting.

VM: I remember: in a dish of scintillation fluid?

AW: I was trying to study germination using tritiated water and I wanted to find out what compounds the water got into. So, I would run an ordinary chromatogram, just using the kind of technique that was in ORL, and then I got a very, very fast film, which actually wasn't too different from the film we were using, and I would put the chromatogram in the dish of terphenyl and toluene, which emitted the right kind of radiation, and put the film on top. I could pick up the tritium very well, and I did a whole lot of work and identified the compounds you got from germination. I kind of went into the tritium version of the path of carbon in photosynthesis.

VM: When was that?

AW: That was in Wellington.

VM: Which year was that, do you remember? It must have been not later than about '57 that you did that.

AW: Yeah, probably.

VM: And the reason was that in the early part of '58, I think, I did some tritium work in Berkeley using vast amounts of tritium to follow what we hoped would be the path of hydrogen in photosynthesis. I was aware, now that you reminded me, aware that you had done that. Somebody must have told me that you'd done that. But we didn't use your technique. We used the same technique as before, but we were using so much tritium that it came through anyway. I guess you were probably not using curie quantities of tritium. I used 5 curies of tritiated water in a glove box with Radiation Lab. people protecting me, and stuff of that sort.

AW: We could use similar quantities of radiation to carbon-14 except, of course, tritium has only got about a tenth of the energy of carbon-14 so we would probably have to use about ten times as much in terms of curies.

VM: So, that's back in New Zealand with germination problems. How long were you there?

AW: I left the Institute of Nuclear Science after being there for five or six years, in Lower Hutt, and went to the University of Wellington. I was there for about ten years in the Chemistry Department and then I was offered a position as Dean of Science and Professor of Chemistry at a new university they were setting up in Hamilton. It was quite a tricky job, setting up a university: I got interested in the economics of universities. The thing was, we got it off the ground with pioneering kind of people, but then the university got more and more complicated, as universities do, so I'm afraid I'm a pioneer rather than a runner. My marriage broke up, partly because of all the work I had to put in. And, I got involved in the Antarctic. We used to go down there in the summers which was kind of a fun thing to do. There were so many easy things for a chemist to do down there that I published a lot of stuff on the Antarctic.

VM: This was, by now, in the sixties, was it, or the seventies?

AW: The sixties.

VM: Did you ever meet Ozzie Holm-Hansen down there? He's an ex-Calvinist who's at Scripps and an Antarctic marine biologist.

AW: No, I was a land person. The people I ran around in Antarctica I learned a lot of geology from. I'm probably the last geologist to learn something completely in the field and not in the classroom. And my marriage broke up, I was offered a job, here in Tucson, as Director of Research for a big mining company which had decided that they weren't going to be able to smelt copper in the normal way. They had big plant; well, it was supposed to be a pilot plant but it ran 100 tons of copper per day using hydrometallurgy. In other words, basically you dissolve up the mineral chalcopyrite in a very acid, very salty (?) solution and then you electrodeposit it.

This problem was completely out of their knowledge. The metallurgists that they hired don't know anything about that sort of thing and they were running into all sorts of problems. So, on the advice of an Israeli consultant they had that I had negotiated with solar heated lakes (?) in Antarctica, I was offered the job, to try to straighten out some of their problems. They needed a general analytical, physical-chemist . We got the plant running. It was an interesting process but like the rest of my life it was 30 years ahead of its time! I worked for them for about eight years and then Pennzoil, which we were the mining subsidiary of, shut the whole thing down and I was given early retirement. So I went to the University of Arizona.

VM: As a faculty member?

AW: No, as an adjunct professor (well I suppose that's faculty), which meant I didn't have to do that much teaching; I can just do research. I decided that people like me ought to try pick some difficult problem and try to solve it, so I tried to develop, and in fact developed, sublimation technique which is pretty tricky experimentally, way beyond the expertise of glaciologists, but they needed something like this in order to recover the gases quantitatively from ice cores and measure the various isotopes and their quantities. That's worked out very well.

VM: And you're still there doing it?

AW: I'm still doing that.

VM: While you were talking, I was thinking of two last questions, I'd like to ask you. One of them going back, of course, to Calvin's group, one of them is: what your view is of the group as you knew it as a research organisation? Do you think it was an effective way of doing it?

AW: I think it was a very effective way of doing it. They had people of great diversity of expertise. Calvin himself was very broad based. He was originally an inorganic chemist (*Editor: incorrect; he was a physical-organic chemist*) and it's just exactly the kind of group to do it, although it actually required — I think was it Bill Stepka who brought in the chromatography?

VM: Yes, it was.

AW: OK — if chromatography hadn't have come in, they would have never gotten anywhere. So, they tried doing it with phosphate chromatography, ion exchange chromatography, but Bill Stepka came in — actually, this was just before my time — and brought paper chromatography and radioautography and that's really what they needed; that really broke it through.

VM: Would I be correct in concluding that it would be very difficult to design a group *de novo* to do what they did? There was so much happenstance that developed in the course of their work that you couldn't have sat down in 1945 and said "this is what we are going to do for the next ten years" .

AW: If they had tried to do it in 1940, before the invention of paper chromatography, they wouldn't have done it.

VM: Also, there wasn't any C^{14} in 1940; it was only just discovered.

AW: Right. You needed C^{14} and you needed paper chromatography.

VM: Well, the C^{14}, of course, Calvin had. He knew about C^{14}, and was offered it by Lawrence so that part was OK.

AW: Ruben had already started to work on carbon-14. He had mainly been working on carbon-11, 28 minutes half-life.

VM: And they had tiny quantities of C^{14}. The chromatography, as I'm sure you're right, was the big breakthrough which actually enabled them to do what they did.

AW: They came up with innovative things like co-chromatography and then I quantified it. It was really cheap stuff to do.

VM: But there is another point and that is: you say that had Stepka not brought it in. Stepka must have got there in something like '52, or '51, something like that.

AW: Probably before that because I was there in '52 and I don't think he was still there. He must have come in about '50.

VM: The invention of chromatography actually took place in '44...

AW: '46.

VM: '44; '44, I think, and that means it was known. Even if Stepka hadn't brought it in, one can argue that they would have realised.

AW: Oh, I think it would have come in eventually, but you'd had to know how to do it.

VM: Well, you send someone to somewhere else where somebody is doing it.

AW: When I was there, just as I came, they learned how to chromatograph organic phosphate because when I arrived they couldn't do that. All the phosphate esters stuck on the origin. You have to wash the paper with oxalic acid to get rid of all the heavy metals, and also there was (*indecipherable*).

VM: I remember them doing that.

AW: That was a big trick. I think Stepka just could chromatograph things like malic acid, but not the phosphates. Working with phosphates was a tricky thing. Acid-washing the paper and I was involved with spraying phosphatase on it once you got the phosphate ester separated. You also had to humidify the paper if you wanted to get them (*the compounds*) apart from one another.

VM: In your day of working with chromatography of phosphates, did you over-run the chromatograms so the solvent dripped off the bottom for a long time in order to spread the phosphates out?

AW: No.

VM: That must have come later. I vaguely thought that I must have invented that myself, but I can't remember whether or not I did, or whether somebody else did. Of course, it spreads things out and you get rid of a lot of the junk off the front end, stuff you don't want, and then you get a lot more further back.

AW: If I did the chromatography right, if I had well-washed paper and humidified it in the phenol, I could get nice, tight spots. I had to get the spots hopefully under the Scott counter, so you really had to know how to run good chromatograms.

VM: The over-running chromatography arose when we wanted to look for very minor components, which people couldn't find, like erythrose-4-phosphate and carboxylation products.

AW: You mean you ran it on a line, one-dimensional.

VM: No, no. We ran it two dimensionally, but we ran it about twice as long or three times as long so that the phosphate area was spread out over the whole paper.

AW: Were the spots really big?

VM: The spots were bigger but, if you kept your fingers crossed, there was enough discrimination; they didn't merge.

The last thing I wanted to ask you was about actually producing your thesis because you are the first former graduate student in that lab. I have talked to. As you say, there weren't many.

AW: I had a really good deal. They produced my thesis as a ORL report.

VM: You had to write it...

AW: Yes.

VM: ...the secretaries typed it for you...

AW: As an ORL report.

VM: ...and produced the figures...

AW: Yeah.

VM: ...and that was it.

AW: Then I could rebind a copy of it as a thesis. So I got my thesis typed for nothing!

VM: Very well placed compared with many graduate students.

Well, that's as far as I can think unless you can have any more stories...

AW: The only story I have, which to me is hearsay, I wasn't there...I thought Calvin was a very good lecturer. He went to this lecture, I believe in England, and he gave this lecture when he was very good at giving lectures, and he wrote on the board this is one possibility and this is another possibility, and, in fact, the people back in Berkeley are working on it at this very moment. And somebody walks in with a telegram and he opens the telegram and crosses one of the possibilities off! I thought that was very clever. I always wanted to do that myself. (*Laughter*)

SM: He'd arranged that?

AW: Oh yeah. It was obvious that he had arranged that, it was obvious to the audience. The audience were just taken in, you know.

VM: But they liked it.

AW: But they liked it, even the English. (*Laughter*)

VM: I think that's splendid. Well, OK; on that note, let me just thank you very much for your time. It has been very well worth while our coming to see you and I was very

pleased to meet you after all these years of simply knowing your name on a paper and as an inventor of scintillation for tritium chromatograms.

AW: Thank you.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Alexander Thomas WILSON

Date of birth 8 Feb 1930 Birthplace Wellington New Zealand

Father's full name Percy Fredrick Wilson

Occupation ~~Carpenter~~ Carpenter Birthplace Wellington NZ

Mother's full name Dorothy Evans

Occupation Office Worker Birthplace Wellington NZ

Your ex spouse ~~[illegible]~~ Joyce Wilson

Occupation 2ndary School Teacher Birthplace Wellington NZ

Your children Bruce & Eric Wilson

Where did you grow up? Wellington New Zealand

Present community Tucson AZ USA

Education B.Sc MSc Ph.D. D Sc

Occupation(s) University Prof

Areas of expertise Chemistry

Other interests or activities interdisciplinary

Organizations in which you are active

Chapter 14

MURRAY GOODMAN
(with Zelda Goodman)

La Jolla, California

June 7th, 1996

VM = Vivian Moses; MG = Murray Goodman; ZG = Zelda Goodman;
SM = Sheila Moses

VM: This is a conversation with Murray and Zelda Goodman in La Jolla June 7th, 1996.

About your time in Berkeley in the "good old days", how did you come to choose to go there?

MG: I was an undergraduate at Brooklyn College in New York and I had taken an organic chemistry class with a Professor Lewis Sattler and had done very well. As a result of that, he asked if I were interested in going to graduate school; my answer was "indeed, yes, I wanted to go on to graduate school!" He said that he had just read some interesting papers on the use of radioactivity as tracers in biochemical research and he would like me to look at them. I did, and these were early papers of Melvin Calvin and dealt with tracers in photosynthesis, primarily the C^{14}, and I found this to be very exciting. I indicated that I wanted very much to do research of that sort and among other places I applied to the University of California at Berkeley. Dr. Sattler wrote to Professor Calvin and, lo and behold, I was accepted and that, together with other acceptances, was analysed by me, Professor Sattler and my family and I decided I wanted to go to Berkeley and study with Melvin Calvin.

VM: Which year was this?

MG: 1949.

VM: So, you went to Berkeley. You'd never met him before you went there?

MG: I met him on arrival.

VM: What was it like, meeting Melvin for the first time?

MG: He was a very active, dynamic man, a chain smoker; he weighed close to 200 pounds, agitated and dynamic, excited and fascinating, all rolled into one human being.

VM: Can you remember where you actually first came face-to-face with him — in which room, what did he say to you, what did you say to him, or is that too far back?

MG: I know where we met. We met in the Old Radiation Laboratory, this rather ramshackle World War I temporary building which was still going quite full tilt in 1949. I came in on a Monday morning and he'd been there for some time; I introduced myself, he was delighted to see me and told me all the things that he expected and wanted for me to be a successful graduate student. It was sort of an intense and exciting entree into the laboratory.

VM: You had to do courses, presumably, at the beginning.

MG: Oh, yes.

VM: So only part of your time was free for lab work.

MG: What had happened is: I was awarded an Atomic Energy Commission fellowship that was funded through the university. These were early days when there weren't all that many fellowships available. This obligated me to do the proper course of study in the Department (*of Chemistry*) and to carry out research. Different from my fellow graduate students, I did not have a teaching obligation which gave me a good deal of freedom at that beginning to sit in on many of the research discussions in the Calvin group and to get to know many of the people in the group since the only diversion I had, and it was a substantial diversion, was to take courses and to do well in the courses.

VM: What sort of contact did you have with the rest of the Chemistry Department, at that time at any rate?

MG: I was one of the first year graduate students and, therefore, I had to have an advisor who would make certain that the courses that I took were appropriate, that I would jump the hurdles which were required of all graduate students.

VM: Who was your advisor?

MG: My initial advisor was Bruno Zimm who then was an assistant professor. During that first year he had decided to leave and to go on from Berkeley to General Electric (that's an interesting story, in and of itself because Bruno Zimm and I are now colleagues here at the University of California in San Diego). But at that point he launched me on my academic career and made certain that I would take the right courses.

VM: As a graduate student in the Calvin set-up, were you, as it were, at one with the other graduate students in chemistry or were you physically separated and didn't see much of them?

MG: We were separated. My laboratories were in the Old Radiation Laboratory. Calvin did not have many graduate students. Most of the other graduate students were distributed among the organic and the physical and the inorganic laboratories in the department in other buildings. However, I'm not one now, nor was I as a graduate student, hesitant as to go meet people and talk with other graduate students and to deal with the challenges that we had. So that there was a lot of interaction between me and them, although it was primarily in their laboratories and not at the Old Radiation

Laboratory because you still had to have (*a security*) clearance in those days to get into that lab.

VM: Did they have departmental seminars at which you and other graduate students were expected to be present?

MG: There were weekly seminars and a colloquium and, of course, within the Calvin group there was a weekly research meeting.

VM: What did you start working on?

MG: Calvin was very much interested in isolating 2-phosphoglyceric acid...

VM: 2-Phospho...

MG: ...and he had the feeling that it was possible to synthesise it and to separate it from 3-phosphoglyceric acid. My first project was indeed to try to synthesise 2-phosphoglyceric and to develop some way of chromatographically identifying it and differentiating it from the 3-phosphoglyceric. The belief then was that the first intermediate that occurred in photosynthesis was 2-phosphoglyceric which quickly rearranged to 3-phosphoglyceric acid.

VM: At that time, was paper chromatography in use in Calvin's lab?

MG: Yes. The large tanks for paper chromatography and rooms which contained these two-dimensional approaches to paper chromatography and radioautography, all the things.

VM: You quickly became involved, no doubt.

MG: I quickly became involved in paper chromatography and column chromatography and the attempt to scale-up from the paper chromatography led me to try much more chemically-oriented isolations of these sugar phosphates.

VM: Had you had experience of biological matters before your involvement with photosynthesis?

MG: I'd had very little laboratory experience. I had done some work as an undergraduate with Professor Sattler on sugar epimerisations and with Professor Irving Kay on quinolines, but these were simply isolations, typical for an undergraduate. At that time, I did very little in the way of complicated organic synthesis.

VM: In the Calvin group at that time, were there any biologists, or was everyone a chemist?

MG: No, there were many biologists in the group and a large number of chemists. But I would say it was a very catholic group, stretching all the way from botany-types to chemical physicists.

VM: I can't identify in my mind who the botany types were at that time. Can you drag anything up?

MG: The names that I recall include Vicki Haas (*Editor: later Vicki Lynch*), Bill Stepka, Clint Fuller...

VM: Clint was there when you arrived or did he come later?

MG: He came later. But these were people who were there during my stay in the Calvin group.

VM: You didn't overlap with Sam Aronoff, did you?

MG: I did not. And, of course, Andy Benson who really was a chemist and a plant biochemist and a major force in the actual functioning of the Old radiation Laboratory.

VM: Where in the building did you work? Where was your bench? Who was next to you? Who did you see through the rack? Can you remember all of those things?

MG: If you were to put the Calvin laboratories in the following axis: On one side the lab. pointed toward Gilman Hall and Le Conte Hall, the major chemistry and physics building. On the other side it pointed toward the Old Chemistry Building and Lewis Hall. My desk was the furthest in the Calvin group toward the Old Chemistry Building and Lewis Hall so I was against the wall.

VM: Who was next to you?

MG: Vicki Haas was just opposite me and back-to-back with her was Bill Stepka.

VM: This was in the big lab., the one with the big white table in it?

MG: The one with the big white table in it. When Professor Calvin would come into the laboratory from his office, which was in the Old Chemistry Building, the first bench he would pass would be mine. That was both a blessing and a curse!

VM: So you saw a lot of him in those days.

MG: Well, when he would come in, very often it would be to talk about recent chromatographic discoveries and everybody would gather round the big white table. The big white table became sort of the focus of everybody's attention when Melvin came into the laboratory.

VM: When Melvin was there, did he tend, naturally, to gravitate to the big white table and cause a group to develop there or did he go and talk individually with people at their benches?

MG: Both. But somehow, very often, we wound up at the big white table.

VM: So, there was a lot of back and forth chat, presumably, between you in a relatively small room. You could hear one another, I suppose?

MG: That was probably one of the most exciting of the learning experiences because you couldn't miss if you wanted to hear what was the latest, what was the conjecture, what was the abandoned hypothesis, it was all sort of put out there in a very open way for everybody to consider. Of course, behind us as we talked were the "gloop-gloop" machines which were harvesting *Scenedesmus* and *Chlorella*. I would go to bed at night very often hearing "gloop-gloop", "gloop-gloop" which was simply the motion of the shakers to keep the algae suspended and, therefore, growing well. But the noise became a rather onerous burden to take home every night.

VM: You were right close to them.

MG: Yes, very close.

VM: Who was tending them at that time?

MG: This is before Paul Hayes.

VM: I didn't know that Paul Hayes had tended them but...

MG: He was sort of a factotum in the laboratory. Martha? And...I am blocked. I can see the faces but the names escape me.

VM: I'll throw one at you; it's someone I didn't know myself. Someone called Anthea (*Editor: actually Altha*) Vann.

MG: Yes. She was also the dishwasher and a person who kept much of the equipment and glassware in good shape.

VM: What sort of difficulty did you have — or perhaps you didn't have any — in distinguishing your thesis work from the general work? Your thesis was to have been your work, presumably, and yet in such a melee of a lab. was it easy to separate out what was yours from what was other people's?

MG: Clearly, what Melvin wanted me to do was to try to isolate chemicals, molecules that were important for photosynthesis, and to do this in a way that was basically organic and analytical chemistry. That was very different from the focus of many of the other people who were far more involved in the biochemical processes. So, there was a relatively easy way to identify my focus and I stayed with that while doing other things in the way of learning and carrying out photosynthetic experiments.

VM: So, you started off isolating 2-PGA...?

MG: Which I found I never could make because what I isolated all the time was the cyclic phosphate. When that cyclic phosphate opened, it opened primarily, if not exclusively, to the 3-phosphoglyceric acid. That sort of thing went on for a while until it was clear that 2-phosphoglyceric acid was not the key intermediate in the earliest stages of photosynthesis. Then my emphasis changed to go toward knowing what array of organic phosphate molecules exist in photosynthesis and when do they appear and how do they change with the growth cycle of the algae.

VM: Can I pursue some of the points in relation to that. Were you there when PGA was recognised as being the first fixation product, or was that before your time?

MG: It was before my time. When I came, it was already known that 3-phosphoglyceric acid was a very early product in photosynthesis.

VM: After your failure to synthesise 2-PGA, was there any more suggestion that anything other than 3-PGA was the first compound?

MG: This sort of happened over time. More complex pathways were being considered; what ultimately did work out to explain the carbon cycle, but at the beginning many,

many different molecules were looked at for carboxylation and cleavage to give 3-phosphoglyceric acid and other products.

VM: But you weren't part of that?

MG: No, no. I was not part of that. I have the distinction, together with Dan Bradley, in that a major part of our work was to begin — this is later in my career — the path of phosphorus in photosynthesis. We actually published a paper with Professor Calvin, of course, in the Journal of the American Chemical Society entitled *The Path of Phosphorus in Photosynthesis. I.* There has never been a II.

VM: They allowed you to publish a number 1 without there being a number 2?

MG: I think that Professor Calvin certainly had the platform to carry that forward and we did that. I think in the paper actually showed why there could not be a simple path of phosphorus that could be traced because there were too many reservoirs of inorganic phosphate and the dynamics of phosphorylation and dephosphoryation to prevent that kind of analysis — very different from the path of carbon.

VM: When you explored the various phosphorus compounds that you said you went on to after the 2-PGA story, you got them from the paper chromatograms?

MG: Or from ion exchange chromatography since I was primarily asked to obtain larger amounts of these materials. The great mystification of my PhD thesis was the existence in *Scenedesmus* of a huge storage of polymetaphosphate. We really didn't know why it was there. It was by far the largest reservoir of phosphate, and certainly high-energy phosphate, and it just overwhelmed everything else that was there. Melvin speculated that this could be the kind of origin of high-energy phosphates and their ability to phosphorylate and It was kept ready to go, with proper co-enzymes and enzymes, as a mechanism for rapid phosphorylation of all kind of molecules.

VM: Were you using phosphorus-labelled materials for isolation or did you use C^{14}?

MG: Yes; these were all ^{32}P labelled materials so that I could follow them quite carefully and completely.

VM: You did P^{32} photosynthesis and then isolating the compounds in he usual way and chasing them up.

MG: Isolating large amounts of the material. So that paper chromatography didn't work but ion exchange chromatography did.

VM: Your job was to identify these materials?

MG: As best I could.

VM: Which ones did you succeed with?

MG: There were all kinds of hexose phosphates and even more complicated phosphates that we could identify, triose phosphates and even, I believe, 7-carbon sugar phosphates and things of this sort were easily identified. Some of the diphosphates were also apparent and the sort of build-up of these molecules made sense as one looked at what was developing as the path of carbon in photosynthesis. Finally, this

enormous reservoir, which was probably 80-90% of the labelled phosphate in *Scenedesmus*, turned out to be this polymetaphosphate.

VM: When you arrived in that lab., who else was there?

MG: I can complete the big lab. I was at the end, and Vicki Haas, then Bill Stepka, then Al Bassham. In the small lab. was Andy Benson, Dick Lemmon — there were some other people there and it sort of all turns into one.

VM: Was the upstairs of ORL in use while you were there?

MG: Upstairs was only used for paper chromatography. Downstairs in a very carefully shielded room were the Geiger counters. There's an amusing story about that.

VM: Please.

MG: When I arrived at the Radiation Laboratory, in those days everyone had to go through a physical examination. In the course of that physical examination the physician who examined me noted that there was a small growth on my thyroid. He said it was likely to be a benign adenoma but that he would urge that I see the specialist at the Medical School in San Francisco and have it out at my earliest convenience. That turned out to be over the Christmas holiday in 1949. I went to the university hospital and was operated on by Dr. Nash, who was a world-renowned thyroid surgeon, and he took out that adenoma and indeed it was benign. However, in the course of determining that I was asked to consume a small cocktail of iodine-131 to see the take-up of the iodine-131 of the adenoma and the thyroid and the whole general aspect of that. It was from that test that the surgeon believed that it was benign and that it would be an easy thing to take out.

Now I had that operation and returned to the laboratory and, of course, mingled with everybody and, of course, also had my opportunity to do some work with radioactivity in the counting room and every time I would appear in the counting room the Geiger counters would absolutely go berserk. Nobody knew why. I had not connected the two, that my gamma-emitter was really sending the background up 5-10-fold at least, if not more. This kept going on and one day it was Andy Benson sitting and counting and I walked down in there and suddenly the counts went wild. He looked at me and he said: "Why don't you leave the room for a second". I did, and then he motioned me to come back in. I came back in and then they put the Geiger counter to my throat. Of course, that was the origin of the problem and I was told very firmly to take a vacation from anything near counting in that laboratory and handle my research responsibilities distant from them as much as possible. My colleagues in the graduate school came to believe I did this purposely to get out of all the things that were involved in hours and hours of counting. After a while, even the iodine-131 disappeared and I was accepted back into the fold.

VM: I think you mentioned earlier on that you were there when Calvin had his heart attack.

MG: I believe it was in November 1949.

VM: Soon after you got there?

MG: Yes. I arrived on the 7th of September, so this is perhaps 2-1/2 months later and Melvin had come into the lab. and was complaining of indigestion and talking with many of us, we were around the white table. He left us to have a scientific meeting

with Dr. John Gofman in Donner Lab. The next thing I heard is that he was in the hospital and he'd had a coronary. The story was that he'd really had the coronary while meeting with John Gofman who was a medically trained researcher and understood the symptoms and immediately recognised what was happening and had Melvin taken to the hospital.

VM: What effect did that have on the work in the lab.?

MG: For the next six months Melvin was basically at home. He would call into the laboratory and spoke primarily with Andy in the Old Radiation Lab. (by that time, the other person who was there with Andy in that small lab. was Nate Tolbert) and with Bert Tolbert in Donner, and with Ed Bennett who was also in Donner, and with other folk. He kept in contact with many of these people and I would write brief reports on my progress in terms of the synthesis and some of the isolation of the organic phosphates, give them to Andy and that would be sent off to Professor Calvin who then would send back notes of things he thought I should be doing. It was six whole months before I saw him again after that November morning.

VM: Was he very changed?

MG: I saw him in November. He was still smoking and he was close to 200 pounds. When I saw him in spring, approximately six months later, he was about 145 pounds and, of course, was never to smoke again. He looked like half of Melvin Calvin that I had seen before and much better looking and healthier looking person than the person who was so heavy the previous autumn.

VM: When he came back into the lab, was it the same man, doing the same sort of things, acting in the same sort of way?

MG: Pretty much. You could see the same sort of love and excitement and intensity of reaction. He was nowhere near as agitated; he was much more methodical and he had a regimen enforced by his wife, which he followed. He had an eating regimen and a time and rest regimen which he also followed.

VM: In the lab. generally, in the group generally, was there much sense of competition between individuals?

MG: It was a very large group and, as I said before, there were very few graduate students. Among the graduate students, there was really very little competition. We were all sort of overwhelmed by the large number of highly talented and successful postdoctoral researchers, visiting professors and research giants who were in the laboratory. I remember, in one case, Hans Schmid from the University of Zürich who succeeded the great organic chemist Paul Karrer as professor at the University of Zürich, was in the laboratory and was full of incredibly useful knowledge and important ideas about natural products and chemical reactions. Just being with him was enormously rewarding to me as a graduate student oriented toward organic chemistry.

VM: All of these people were interacting freely with everybody else; there was no sense of hierarchy inside the lab. was there?

MG: I don't think so. Again, thinking back to those days, the overall impact is one of enormous and free exchange. I'm talking primarily about the Old Radiation Lab. group. The other group in Donner Hall were doing different things and were much

more involved in things like nuclear aspects of medicinal chemistry and some other work in biochemistry that was quite different from what was going on in the Old Radiation Laboratory. ORL was completely focused on photosynthesis.

VM: How much contact did you have with the people in Donner?

MG: Every week we had a research group meeting and I would hear what they were doing and I would listen to some of the work, primarily that of postdocs. and staff scientists. I don't recall any graduate students over there. There was another group working on R_h factors in the Old Chemistry Building, that was a small group...

VM: This was part of Calvin's overall empire?

MG: ...part of Calvin's overall group. Elmer Schallenberger was one of the people working there (*Editor: actually, Schallenberger was working in* Old Chemistry); he was a graduate student at the same time I was there.

VM: Tell me also about the group seminar, the famous Friday morning seminars?

SM: You mentioned Elmer Schallenberger, was part of a third group? I'm sorry; I didn't catch what the third group was.

MG: He was doing work, I believe, on the porphyrin world in terms of structure and biochemical and organic chemical aspects. It's a long time ago and I don't remember specifically what he was doing. It was something to do with haem and haem proteins.

SM: This was not with the photosynthesis group in ORL.

MG: This was not with the photosynthesis group in ORL...

SM: And not with Donner?

MG: ...and not with Donner.

SM: Where was he?

MG: It was in the Old Chemistry Building in a lab. in the basement, I believe.

(*Editor: The work on the R_h factor in the Old Chemistry Building was done in collaboration with a bacteriologist, Merwin Moskowitz, and a medical doctor, Dr. Robert Evans. They were attempting to isolate the factor from blood that causes the R_h incompatibility. This project was initiated by Professor Calvin after the death of his first son from R_h incompatibility. He received a grant from the Rockefeller Foundation. However, the work was inconclusive. One of the people who worked in the lab. on this project was Genevieve Calvin, Melvin's wife.*)

VM: To get back, then, to the Friday seminars: had you ever met phenomena like this before?

MG: It was totally new to me. It was a meeting of everybody, beginning no later that eight in the morning and no one knew who was to be called on. It just turned out that we all came, either prepared, or prepared to explain why we were not prepared.

VM: So some of you actually came in there carrying stuff, carrying data or materials?

MG: Yes, that was not unusual. If one presented results which were quite positive and new, then things went famously. If one didn't accomplish what was expected, clearly there were frowns on Melvin's face and very simple admonitions to get going, "to get off the dime", to make progress, to do the right thing.

VM: Presumably, there was some sort of approximate rotation in which people would come round every so often and give a talk. It would be their turn again periodically.

MG: In a global sense, you are right. You would say, "Well, I spoke last week, it's not likely I will be called on again this week", although there were some exceptions because Melvin turned out to be very interested in a particular area and wanted to hear more and more about it. Most often, if you had spoken and it went all right, you could think of about a two-month hiatus before you had to begin preparing again.

VM: Presumably, if Melvin heard something interesting, he would be close on the heels of that person for the succeeding days and weeks.

MG: That's right. That's exactly how it worked. If it was interesting, that afternoon it was likely that he would be at your desk and your lab. bench, saying "let me see what you have done; let me understand, let me see if I have it right". If you didn't explain it correctly, I remember the famous strong rejoinder was "I don't understand". That meant you had better stop, slow down, pick up the pieces and come back with an explanation that he could follow and pick up very quickly. He was so fast and so rapid in understanding so many things that if it went in a direction it didn't mean that you were being too complicated, it means that you were not being clear.

VM: He was clearly the outstanding person in the group, but he didn't do everything.

MG: That's right.

VM: How can you evaluate his contribution versus other people's — can you? Maybe it's too difficult.

MG: After so many years of being a research director, I realise that he played many, many roles. He was a teacher, he was a resource person, he was a knowledgeable fountain on a wide variety of aspects of the science that we were interested in from physical chemistry all the way to botany. He was knowledgeable about what was in the literature, he was up-to-date about recent discoveries, he had ideas about new directions. All of these things made him an enormously successful research director. Now most things that were analysed by him and undertaken as a result of his suggestion did not work out. But the few that did were the hallmark of the laboratory and the great success of the laboratory.

To do that, he had to have people of great ability with him, in both an anticipatory sense and in a follow-up sense. That if the ideas were somehow out there, how do you put them into practice? That was one of the exciting aspects of being in that lab. Because if something were suggested, which came out of discussion around the white table, let's say between Nate Tolbert, Bill Stepka, Al Bassham,. Andy Benson and Melvin Calvin, there was an immediate plan of how to do a series of experiments, how to do this rapidly. That was a very clear characteristic of the lab. at that time. You had to have those people!

VM: So I suppose, that although he may have been the single most prolific source of new ideas and inspiration, he didn't think up everything that went on. Other people made enormously valuable contributions to the development of the research.

MG: Absolutely and prime among them was Andy Benson, who was very much a broadly based scientist in this area of photosynthesis and technically highly accomplished. He was in the laboratory at all times, so that he had that sense of how to make it happen in the lab. And, of course, discussions always involved him. He was, and is, a relaxed, contemplative person who is very easily accessible and in the day-to-day kind of world of being a graduate student or in functioning in ORL, I would say that Andy had most of the responsibility for reacting to what was going on, analysing what was going on, suggesting routes to new experiments. That was a major emphasis of each day in the lab.

VM: One of the things that particularly interests me, and I've really little idea of how it happened and who was involved, was the working out of the later parts of the cycle with the heptoses and the pentoses, and so on. Were you there at that time?

MG: Yes, I was. This was a very exciting time.

VM: Who were the participants, where did the ideas come from?

MG: That's hard to say. Certainly it was quickly recognised as something of substance at the discussions around the white table. People were at this particular point thinking of other routes beyond the simple 2-carbon mechanism. I think there were all sorts of analyses of what kinds of enzymatic processes could go on and, without being able to say who mentioned ribulose diphosphate or sedoheptulose diphosphate first, it was there, as I remember it, that part of an active series of discussions. Almost in a sense of discovery, they were talking about these kinds of reactions and suddenly everybody was talking about them. My recollection is that this was something that made sense in terms of suddenly beginning to think of aldolases which were then somehow enzymatically being looked at in other contexts — nothing to do with photosynthesis. The aldolases were the entree into carboxylation of these keto sugars that then led to the unraveling of the path.

VM: As far as you know, was there any direct contact with Racker and Horecker who were the other people obviously working in this area?

MG: Not that I know of, although their names, of course, were then known in the laboratory after that; but not that I know of.

VM: There were also other external influences and discussions, debate, arguments that I know little about. The relationship with Arnon, with Gaffron, with Gibbs and people like that. Did they impinge at all on the internal life of the lab. as you, a graduate student, saw it?

MG: There were competing laboratories and there was a real sense of primacy of our group over them. There had to be a concern of the laboratory with information that would of necessity keep us ahead of the competing laboratories. It was sort of a distant sense of competition and not something that day in and day out was a topic of conversation. There was Fager and Gaffron and their work but it was somewhere near Chicago or in the midwest and that was far distant. The Arnon era and that was all after I had left.

VM: So people didn't come in and say "My God, one these other people are getting very close — we had better hurry up".

MG: Not that I recall.

(*Tape turned over*)

VM: Carrying on the discussion: you actually finished your work in about '52 or something like that?

MG: Yes, in September of '52.

VM: How was your thesis produced? Did you have to type it all yourself or did you get help? What happened in those days?

MG: The initial draft was typed by Zelda, my wife of one year at that time, and then, after it was proof read and corrected from the standpoint of content, it was sent up to the Rad. Lab. on the Hill. They typed it as a Rad. Lab. document and duplicated it, and that was my thesis.

VM: And a copy or several copies were bound from that print-out?

MG: Yes.

VM: Who was on your thesis committee?

MG: Jim Cason, Chet O'Konski, Melvin, of course; I don't remember the others.

VM: Melvin, presumably, would have been well aware of what was in it as it was being produced. What about the others — did they want clarification, did they want to talk about it?

MG: Oh yes. I think that there was discussion with each of them and modification before the final draft was typed. We didn't have formal defence of theses, the University of California doesn't have that. But it was read by the people and critiqued and then submitted in partial fulfilment, etc. and the degree was awarded. I guess the official time would be June of 1953 but it was in September of 1952 that we left, toward the end of September.

VM: You've actually kept in very close touch with a number of people from that lab. ever since you left?

MG: Yes.

VM: You have been aware of the developments that have gone on and much of the progress which has been made. To what do you ascribe the success of the group, particularly in the early days, particularly in the photosynthesis era?

MG: It was a confluence of the timing, the energy and talent of Melvin Calvin, the funding through the Atomic Energy Commission, the bringing together of very highly motivated, talented researchers in a truly interdisciplinary function. It was an interdisciplinary laboratory in the days before "interdisciplinary" became popular. And the fact that the problem chosen, the path of carbon in photosynthesis, was

solvable by the techniques of tracer chemistry. All those things made for a special character and productivity in the laboratory.

VM: The individual personality and abilities of at least the leading people were critical to the success...

MG: Oh, absolutely.

VM: ...and the funding, presumably, was an important factor.

MG: That's true. One could set up a kind of permanence in Berkeley which was different from the administration or the faculty and this, of course, worked very well in the Bio-Organic Group, the Biodynamics Group and also, of course, in the Radiation Lab. as a whole. That meant that people were not concerned about where will I get a job next week, that this was something they could count on in a way quite different from ordinary postdoctoral researchers.

SM: Before we say more about this, I was thinking that we had Zelda come and go, as it were, to Berkeley, and clearly this happened in 1952? 1951. I think it would be interesting to have Zelda's impressions of what she came to and how it seemed and how welcoming the group was and how she felt about it and how different it was from things you'd known before.

ZG: This was completely new because I had lived at home. I had never lived away from home before, and here I was married, away from home, but it was probably a welcoming group. I think that one of the things that was impressive that the Calvin's had lab. parties, had the group over at least once or twice a year and they were, Gen particularly was very mother-hennish about all the graduate students and all the people in the lab. and treated them very much as a family. I think that was a very important influence on my life. I certainly cannot judge anything that went on the scientific level. I know that I learned to wash glassware, I think I did some counting with the Geiger counter at one point and used to be in the lab. late at night frequently. But what went on scientifically, I have absolutely no way of judging or assessing in any sense. I don't know what it was all about.

MG: I do remember that there would be small dinner parties at the Calvin's and often Zelda and I were included, and that was supportive and reassuring to me as a third-year graduate student. We got married in August of 1951, at the conclusion of my second year as a graduate student. Now the problems of completing a thesis and settling in to married life really needed some help from the Calvins and I think what Zelda alluded to was quite correct. She (*Gen*) was supportive and did help. In his own way, of course, Melvin is detached from things of this sort and as long as he was sort of guided by Gen, it made our first year more pleasant than it would have been ordinarily.

VM: Zelda, what did you see of the social scene, as it were, a lab. spouse; how did you inter-act with other lab. spouses?

ZG: I think the only lab. spouse I really remember was Trudy Bradley, wife of Dan Bradley who was a graduate student with Murray. (*To Murray:*) Dan came after you came?

MG: That's right. Dan came one year after me, when I was in my third year he was in his second year.

ZG: We became good friends, and Trudy and I used to spend the evenings in the laboratory. She was also working for a PhD at the time at the University in another field, I believe psychology. She was busy with her own "thing" and it was interesting because I think that even their friends were probably more from chemistry than from her field, which is quite interesting in retrospect. There were other graduate students we were quite friendly with, but I don't know that I think of it as a spouse kind of thing. It was Berkeley. Berkeley is Berkeley. It's very different, everyone is a student, even when you are in your sixties. It's that kind of life.

MG: Let me add something to what Zelda is saying. There were very few graduate students. I think Dan and I were the only married graduate students. The others — Alex Wilson, Elmer Schallenberg, Anne Zweiffler at that time — were not married. We did socialise somewhat with graduate students and certainly with Dan and a bit with Anne. The postdocs. and the senior people in the group were a good deal older than us and, therefore, it was not usual as students to be in their milieu, although they were friendly and we did have all kinds of laboratory events, dinner parties, we would be at Andy Benson's home, and things of that sort. Most of our social activities turned out to be in the community outside the lab. (we can talk about that) within Berkeley.

VM: Do you think that was typical of most of the lab. residents, that the lab, although it had a strong internal cohesiveness as far as professionalism is concerned, it was not a social centre? Am I right in that?

MG: I believe you are

ZG: But it was also very social.

MG: Oh yes, but it wasn't something that people-in-the-laboratory's total social experience were with people in the laboratory.

VM: In talking with people about the whole experience, many comments have been made about the nature of the building in which you worked and the effect this had on the way you interacted with one another. How do you remember it?

MG: Well, the Old Radiation Laboratory was a wooden building, one storey — actually there were two storeys...

VM: Actually there were three storeys.

MG: Yes, yes, yes! But from our standpoint it was that one-story that we lived on and worked in. The building was sort of ramshackle; it was certainly no architectural wonder. We had the chemical laboratories on one side of the building and on the other side was the big machine shop. There was also, I believe, a glassblowing shop there also. There were environmental health and safety (or its precursor) were also in that building. In the basement were counting rooms and on the top floor we had the chromatography rooms. It wasn't a place that had compartments. Within the lab. it was all essentially open. There was nowhere you could hide from anything. Andy's office was glass enclosed, the secretaries had an office (glass enclosed), so you could see everything and everyone. If I wanted to see or talk to Andy, it was a very simple thing to do. There were no barriers.

VM: Were the doors left open, incidentally? Did he have a door and was it left open?

MG: He had a door, I think it was always left open. This created an atmosphere of openness in the laboratory. It makes it much easier to do things together. It also makes it all-knowable if you goof. So it's a two-way thing and I think the net effect is very positive.

VM: Did you tend to stay together during the day, did you tend to eat lunch together?

MG: I tended to each lunch with the graduate students and seek them out because we had exams to take, we had courses to take, so I most often would lunch with them. After Zelda and I got married we would have lunch together in Faculty Glade almost every day with many of our friends.

VM: Did people work late at night in the building?

MG: Many did, yes, certainly I did.

VM: Zelda, I wonder if you have any comments about Murray's working late at night.

ZG: Yes, I do. I think for a while I would go to bed, wake up in the morning, go to my job, but frequently I would also go to the lab. with him and stay up until three o'clock in the morning and try to work the next day. Murray assured me that this was going to change after he got his degree, that this was the life of the graduate student. For many years, probably 30, I believed him. I no longer believe him, because it never changes.

I think there's another point that I would like to include. I think that the life in Calvin's lab. and the way Calvin ran his group was a very, very strong influence on the way Murray has run *his* group and the fact that there are people from all over and that there is a somewhat interdisciplinary (*atmosphere*), never a concentration on only one direction, that there is an openness, and certainly the group meetings, which are not Friday morning but Friday afternoon!

MG: But for many years it was Friday morning and it was at eight o'clock in the morning and the difference is that I would make certain that people were prepared to talk. I wouldn't do the whole Calvin scenario, but only part of it.

VM: For an hour, you have talked only in positive terms about this experience. Was there nothing negative?

MG: Oh, there were many difficulties and problems. I think one of the aspects of such a big group that many graduate students felt was that Calvin was unreachable, that he was sort of up there on Mt. Olympus and it was difficult to get to him for free and open discussions or advice which graduate students typically come to mentors for. I think there were difficulties certainly, or problems for me, that came from Melvin's heart attack. His departure (*from the lab.*) was not easy for me to take because it was some vacuum and void in terms of the *faculty* mentor. Now Andy Benson did step in and do a wonderful job in trying to help but it never really was complete because I would look at the other graduate students whose mentors, whose faculty mentors, were there with them and for them. I didn't have that.

VM: So Andy didn't quite fulfil that role?

MG: He couldn't. He tried to do all that was useful for me in the laboratory and that was excellent and I appreciated that. But when it came to all kinds of hurdles within the academic requirements — which courses to take, when to take them, what kind of

research report was in my file that could be used, let's say, for the committee to evaluate my progress — that's where Calvin's absence really did hurt.

VM: Was it difficult for some graduate students to get to him? Was he very taken up with the latest discovery and the person who made it, to the detriment, at least temporarily, of some other people?

MG: I think I would put it in another way, in a kind of statistical weight-fraction of the group. There were so few of us who were graduate students that we didn't really make a great impact on what were the discoveries day in and day out, what coming out of the laboratories. There were many postdoctoral people and visiting professors and people on sabbatical and things of that sort, that they would be creating aspects of research which were very professional and at the cutting edge. Graduate students were less involved at that, even if we did make some important discoveries and Calvin would sit and listen and explain and catalyse new experiments, they were so filled with the others that we tended to feel a little bit left out.

VM: After you had been there a couple of years, you were almost as experienced in the technology of photosynthesis research as anybody else.

MG: Not so, because there were changing aspects of the photosynthetic experiment that were continuing and continuous throughout the time. What I was used to six months before was no longer being done; the lollipops continued to change, the radiation detection, the whole development at that point post-Beckman DU into the area of the Cary spectrophotometers, which were a total revolution in measurement; the coming into the laboratory of infrared spectroscopy; these made everything change so rapidly that graduate students just could hold on to try to keep up.

VM: So graduate students, or at least your view as a graduate student, were really you were at the junior end of things.

MG: I did have that feeling and I was a learning vessel into which lots of knowledge was being imparted. What my role was, was to learn how to do research. That I learned in the Calvin group with many, many miscues and many experiments that didn't work out. But out of it came a thesis and papers and that ability to say I can work in the best of laboratories.

VM: As we've mentioned earlier, you have kept in touch with a lot of the group, including Calvin on and off. Therefore, you are familiar with the round building, although you've never worked in it, as I understand.

MG: That's right.

VM: What do you think...? Well, let me phrase it in this sense: obviously, the group, any group, including that one, changes in character as it goes on and as it develops and so forth. Accepting the fact that a new building was to be built in the early 1960s, what's your view of the way and the concept of that building, and do you think that its attempt to recreate, in a sense, the atmosphere of ORL worked?

MG: I was at the dedication of the Round House and I remember all of the explanations. I could see that the laboratory was designed with the idea of openness and there it was an attempt to recapture and to stimulate the activities that went on in ORL. It was a much bigger building, it had many floors, and it attempted to do much more than

what went on in ORL. ORL was focused on photosynthesis and photosynthesis only. Here there was everything that at that point was of interest to Melvin Calvin...

VM: Or had been of interest.

MG: Or had been of interest to Melvin Calvin, and because of the size of the building there had to be alliances and obligations and interactions with the Chemistry Department which brought in people of great talent who were really totally independent of Melvin Calvin. So I could see the activities in the Round House as being much more complicated and really not in any way able to recapture that open simplicity of ORL. It looked too formidable an edifice to do that.

VM: Do you think they should have done something else?

MG: It's not for me to say.

VM: It's too late to change the issue, the building is there.

MG: I can see why they did what they did. I wonder, after all these years of being in science research in the university whether any golden era can be recaptured. It's golden because it probably cannot be recaptured.

VM: The problem is that the people survived the golden period with many years of work still ahead of them and they want somehow to prolong the aura of that time; the "aura" is the right word, isn't it?

MG: I think that if I were in that position, what I'd want to do would be at the time when the change was necessary, to change not only the building but the direction of activities completely. I would look to do things that were not comparable. I would look to try to make an impact somewhere else.

VM: I think Melvin tried to do that while at the same time recognising that there were a whole set of obligations to people who were part of the group who were not necessarily able, or willing, suddenly to change direction as a body in some young, new way. By that time, there were many of them, there were eight or nine almost independent or semi-independent research groups. It was a consequence of the way the group had developed later on that the position was arrived at.

MG: He couldn't give up the commitment to many of the research activities of the ORL and Donner period because the people who were doing that work, many of them came with him to the Round House. The funding remained in a major sense the same as it was in Donner and ORL. Therefore, although he did try to do some new things, a central emphasis had to be what went on in ORL and Donner. I think that had built into it the seeds of failure. Not that people didn't try. But the questions became different. Suddenly protein chemistry was interested in the photosynthetic centre, the whole trans-membrane proteins, and the structure and the chemical physics of the process. Melvin had really solved the organic/biochemistry interface beautifully and now there were other questions being asked. And the people who were in the group weren't ready to go in that direction. They weren't the X-ray diffraction specialists, they were not the chemical physicists who had come into the field. So, I feel that was the problem.

VM: What happened to you since 1952; it's now 1996? Can you very quickly recapture the last 42 years?

MG: I'll try. What happened was that through Melvin's efforts I obtained a postdoctoral research fellowship in the laboratories of John Sheehan at MIT and I wanted to go in that direction because after my experience in the Calvin group, what I wanted to do is more synthetic organic chemistry. Sheehan gave me that opportunity. I worked in the area of peptide chemistry with John Sheehan and also in the area of amide bond formation as a general phenomenon. I stayed with him two and a half years and from that laboratory I went to Cambridge, England to work with Lord Todd and George Kenner, again in the field of peptide chemistry. I decided that peptides and proteins, or that interface, was the area of research I wanted to follow for my own career. In Cambridge, England I learned a good deal about the kinds of chemistry involved in peptides, nucleotides and conjugates of the two.

VM: Was that the occasion on which you were accused of obstructing the Queen's highway because you opened the car door in the face of a cyclist?

MG: That's correct. That was a particular unintended event, of the fact that I had a car with the wheel on the right-hand side and I don't think I was completely used to getting out of the car on that side into the roadway.

From Cambridge, England I was offered an assistant professorship at the Polytechnic Institute of Brooklyn.

VM: Is that where you did your undergraduate work?

MG: No. I was an undergraduate in the City University of New York and the campus that I was at was Brooklyn College which is different from the Polytechnic Institute which was a private institution. I began my career there in September of 1956. I rose through the ranks from assistant to associate to full professor and I worked with a group in another golden age, this in the area of polymer chemistry. The director of the laboratories and the chemistry effort on polymers was Herman Mark, a renowned figure, and he really pulled together a great and distinguished group of polymer chemists. I joined them and emphasised the bio-organic and biopolymer area of research and continued that when Herman Mark retired. His successor as director of the Polymer Institute was Charlie Overberger; and Charlie Overberger was a very successful organic chemist in synthetic polymers. After one year he was hired away by the University of Michigan and went to Ann Arbor as chair of the Chemistry Department and Vice President of Research.

I succeeded him as Director of the Polymer Research Institute in 1967 and had the opportunity of hiring Dan Bradley who had left the Calvin group, gone to the National Institutes of Health and then I brought him back into academia as professor of physical chemistry and polymer chemistry in the Polytechnic Institute of Brooklyn. That year was 1968-69. But, by that time I realised that the golden age and the golden era that Herman Mark had created at the Polytechnic Institute of Brooklyn could not be recaptured or recreated. And I decided to leave.

In 1970 I accepted a position as Professor of Chemistry here at the University of California at San Diego and in 1971 we moved, and I have been here ever since.

VM: And lived happily ever after!

MG: I have been Chairman of this Department of Chemistry and Biochemistry, I have been Acting Provost of Revelle College, I have chaired the Academic Senate, so I have

done all the things that are asked of a professor in the University of California. But most of all I enjoy being just a professor and doing the research and teaching that brings me together with my research students, my graduate students, my postdocs. and the undergraduates.

VM: And, of course, remembering the good old days!

MG: Yes indeed.

VM: One last question. Unfortunately, we can't talk to Dan Bradley because he died. When was that and what were the circumstances?

MG: During the late 1960's Dan had been diagnosed to have high blood pressure. He was on medication and he didn't like taking the medication. It interfered with his thinking, as he would tell me. I am only supposing that he went off the medication and was actually working with his wife, Ria, in a Montesorri school, painting the place in October, with all the parents, in

ZG: 1970.

MG: In October of 1970. He was on a ladder, painting, collapsed with a massive stroke and died.

VM: While he was still in harness at the Polytechnic Institute?

MG: Yes. He was 41 years of age.

VM: It's a sad note on which to finish what has been an otherwise entertaining hour. Thank you very much. And we've recorded it for posterity.

MG: I hope that it has been useful. It's difficult to remember it all factually, and what I've described I hope was true and what I forgot I hope you'll forgive me.

VM: OK, we will. And thank you very much.

(*Later*)

MG: We talked about the collection of organic phosphates and storage phosphates in *Scenedesmus*. This took some time and my harvesting of *Scenedesmus* continued over a period of weeks. When I had collected a sufficient amount, I concentrated the algae and then, with acid extraction, took out as many of the organic phosphates and other molecules as I could and then undertook a very careful ion-exchange separation and isolation of individual compounds.

Now the compounds were very radioactive because they were loaded with P^{32} and therefore, whenever I ran such an experiment, I put up signs around my lab. bench saying "Danger — Radioactivity; Be Careful", all the things that were in use to alert people to the fact that we were working with extensive amounts of phosphorus-32. On this particular day, I was undertaking my hottest separation of the radioactive organic phosphates and I'd set up a very careful automated delivery system of the extract to the top of a rather substantial ion-exchange column and also had worked out, after much effort, a fraction collector that was really controlled and oriented to work directly with a specific volume of eluent that would come through the column.

I set this up in the late afternoon and decided I would check this after dinner sometime. Zelda and I were invited to have one of our typical spaghetti dinners with Larry and Lee Schechter (Larry was a graduate students in physics and we were close friends with the Schechters) so we went to their house. We had dinner and we were chatting a bit and we were trying to decide what to do — whether perhaps to go to a movie or something of that sort in the evening and I indicated that I should go back to the lab. and check this elution of highly radioactive organic phosphates. And Larry came with me and we came to my lab. bench and to my horror noticed that the fraction collector was moved rather substantially out from under the dripping ion exchange column. And a very large puddle had accumulated on the lab. bench and with a simple Geiger counter I could see that it was very radioactive. After blanching and sort of gasping, what I did was, of course, stop the experiment immediately and survey the scene; and it was clear to me what had happened.

Every night in those days, the security at the Radiation Lab. checked all of the windows. And even though I had the sign which indicated that there was radioactivity, the guards were used to those signs and, as the guard checked the window behind my lab. bench and desk, his gun, which was in a holster on his hip, must have hit the fraction collector and moved it. He never realised it, checked the window and left. And here this experiment which took several months to prepare was in danger of being destroyed simply on the basis of this accident.

Well I was so stunned that I didn't know what to do. I suggested that we go back to the house and take stock. We did. We decided that there was nothing to be done then. I had roped off the area and made sure nobody would go anywhere near that and we decided we would go to the movies, at least to give me time to think before going back and deciding what to do. We went to the film "High Noon" which was, I guess, with Gary Cooper and was a western adventure film. And this, of course, held my attention and riveted my thoughts on what was happening in the imaginary world and did the job of at least giving me the time to make a plan.

Following the movie, Zelda and I went back to the lab. and I cleaned up and, of course, alerted the health and safety people so that they would be able to come in the morning and check out and decide how to further secure the area and make it habitable. What we did — what I did — was to work up the rest of the experiment completely and what came out of that was a sufficient success to go forward and complete my thesis. But if it wasn't for the accident of the gun, probably the gun hitting the fraction collector, I'm not sure that I would have found that huge reservoir of polymetaphosphate because it came in so much later after the rest of the radioactivity that it gave me the time to continue the experiment and to find this rather substantial reservoir of high energy storage phosphate.

VM: So all was well that ended well.

MG: Yes, and it taught me that it was probably not necessary for the security guards to check the windows that frequently and without being concerned about what was going on around them.

VM: OK.

MG: By the way, that experiment did take me all night to work up. Zelda and I finished in the laboratory about six o'clock in the morning and went out driving back home as we watched the sun rise. So it was not only a reasonable success but it wassort of a very nice feeling to watch the sun rise over the Berkeley hills as we went to our apartment.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Murray Goodman

Date of birth July 6, 1928 Birthplace New York City

Father's full name Louis Goodman

Occupation Retail Store Owner Birthplace Ukraine

Mother's full name Frieda Bercun Goodman

Occupation Homemaker Birthplace Ukraine

Your spouse Zelda Silverman Goodman

Occupation Teacher & Homemaker Birthplace New York City

Your children Andrew (Boston 1954), Joshua (New York 1957) David (New York 1963)

Where did you grow up? New York City

Present community La Jolla (San Diego) California

Education PhD (Chemistry) UC Berkeley 1953, B.A (City Univ. of New York (Brooklyn College) 1949

Occupation(s) Professor of Chemistry

Areas of expertise Bioorganic, Peptides, Drug Design and Biopolymers

Other interests or activities

Organizations in which you are active

Chapter 15

ANN M. HUGHES

Berkeley, California

June 12th, 1996

VM = Vivian Moses; AH = Ann Hughes; SM = Sheila Moses

VM: This is a conversation with Ann Hughes on Wednesday, the twelfth of June 1996 in Berkeley. Ann, can we start with how it was you became involved with the Bio-Organic Chemistry Group?

AH: This is where I had hoped to be able to start. I was working in Philadelphia at the Navy Yard in Aviation Medicine and after seven years of that I became bored with life, not with the people but with the work, it was not that interesting. So I wanted something new and different to do.

VM: What was your training?

AH: Physiology and pharmacology at Duke University. I talked to a friend of mine who knew what was going on in various labs. all over the United States and she said: "Well, why don't you write to two people at Berkeley, Hardin Jones and Melvin Calvin, and see if either of them can give you a job?" So I did exactly that. I never heard from Hardin Jones again in my life.

SM: Which year was that?

AH: That was the spring of 1952. In about one week I got a letter from Melvin and he said "Would you meet me in Baltimore on Sunday something or other, for an interview". So I drove to Baltimore, which was not a long distance, and because of the time differential he was somewhat late, or I was — something went wrong. But anyway I met with him there and after about 15 or 20 minutes he said "Well, why don't your pack your bags, get ready to move; I'll go to California and start your clearance". That's all there was to it.

VM: Had you ever been in California before?

AH: No, I had never been here in my life!

VM: So, you didn't know what you were letting yourself in for but you'd heard good stories, I presume.

AM: I had a friend who had been here and I heard a lot from him and he was coming back to Berkeley and also my brother who was in LA said "Well, if you are going to California — he had just been taking teachers' training — if you are going to be in Berkeley I'll look for a teaching job up in the Bay Area, which he did, which all worked out very nicely. So that summer I finished up my job, gave my notice, packed everything. A friend of mine with her two children, ages 3 and 5, drove out with me across the country. She did not drive: I did all the driving. We stayed in motels all the way across the country. That's how it started.

VM: With all your worldly possessions in the back or the car or did you ship them separately?

AH: Oh, no. I had to have a moving van move the furniture. I had a lot of furniture. The one thing I brought in the car with me very carefully packed in the trunk, and it stood the trip perfectly, was my 100-year old Limoges china. Luckily not a piece got broken.

VM: Eventually you got here. Had Melvin discussed with you what you were going to do when you got here?

AH: No, not really. He just said "come on out, and I'll give you a job".

VM: And you didn't know what the job was about?

AH: I had no idea of what it was going to be about.

VM: What did you know of him before you ever made your first contact?

AH: Probably nothing, just that he was a professor at Cal who did offer me a job and I think I knew that he was at the Radiation Lab., though I don't remember even that. I had no knowledge of the kind of work he was involved in or what I'd be doing when I got here.

VM: So when you first met him, you met him only as your possible future boss and you knew nothing of his reputation or his publications.

AH: Nothing, not a thing.

VM: What sort of impression did he make when you first met him?

AH: A very business-like; I would not say he was friendly. He was not unfriendly, but very business-like and straightforward, making quick decisions, as obviously he did to decide in 15 minutes that he would give me a job out here. Not only did he give me the job but the Lab. paid all my travelling and moving expenses which I had no idea was going to happen, which I thought was rather generous for him to put in for all of that.

VM: So, you got here.

AH: I got here.

VM: What happened on arrival, what did you do?

AH: Well, I stopped in Southern California because my brother, Pete, was on leave of absence, living in Linus Pauling's house.

VM: Really — where was Linus Pauling?

AH: I think he was in Europe or something. Pete was there; he came to work with Linus and then Linus went off for a year or something and left Pete there. Owen was still down there, so we spent a week or so down there and then drove up the coast.

VM: Who's Owen?

AH: Owen is my younger brother who now lives in Alaska. But he had gotten a teaching job in Hayward. So my friend and her two kids and I drove up the coast and her brother, who had been the person I had known slightly, he had arranged a place for us to stay when we first got here, while I house-hunted. She stayed with me, I guess, two or three weeks and then went back to Philadelphia. I started work and I started working with Martha (*Kirk*).

VM: When you got here, was Melvin back by that time, did he see you?

AH: I don't remember little details like that. Oh sure, he was back. I had seen him in, I would have said, April or May, I think he was on one of his speaking trips or something, and I didn't get here until the middle of July, the end of July, something like that, and started work two or three weeks later, working with Martha.

VM: So you started working in Donner, did you?

AH: Right.

VM: So you must have made early contact with Bert Tolbert and other people.

AH: Well, let's see: was Bert here or was he gone? I guess he was here, but Ed Bennett was gone. One of the two of them was gone, I can't remember which. Bert was my first supervisor and, although this is not chronological, one of the funny stories — I had actually been working with Bert as my supervisor, not directly, for three or four years and I was busy at my lab. desk, I think I was counting fruit flies at that point, and Bert walked in with a visiting fireman. He said "Doctor so and so, I'd like you to meet, I'd like you to meet, I'd like you to meet....", finally I said "Ann". He said "oh yes. I'd like you to meet Ann, I'd like you to meet Ann", and I finally said "Hughes". He had drawn a complete blank, aft after we had worked together for several years. I never had very close contact with him actually in the sense of work. In the beginning of the course, working with Martha, she had all the contact with Bert or with Calvin and I just sort of did what she told me to do.

VM: For the record, that's Martha Kirk, of course.

AH: Martha Kirk.

VM: What did you start working on?

AH: We were studying, as I remember it, and this was the least interesting of my jobs, I never really understood too much about it, the respiratory metabolism of some compounds in animals. And Martha then got into, and I never actually helped her with that, I on with the animals, the respiratory metabolism of compounds with some

of the patients in Donner. Martha had a big helmet she would put over the patient's head and sit them in an armchair and whether she injected the compound or whether they swallowed it, I don't remember.

SM: Where were these patients from?

AH: There was a clinic in Donner, I believe it was connected with the hospital on campus. But they (*the patients*) would come over to the clinic there and people like Hardin Jones (who were the others? I can't remember the names of the others) would see the patients there in the clinic. They were interested in the study of some compounds in various diseases. So she (*Martha*) would do the respiratory metabolism of these compounds in these patients.

VM: Looking at "hot" CO_2 being breathed out?

AH: Yes, but I never knew much about that. I wasn't until I got into other things that I got excited about what I was doing. I guess the first thing that I got really excited about was Calvin wanted me to decide, or help decide, whether D_2O (we were beginning to get into D_2O at this is point), whether D_2O would prevent cancer, or help cure cancer, in animals. I was working with mice on the D_2O metabolism — not on the metabolism but the effect it would have on tumours that I got going in them (*the mice*). It never really did much in that sense. But along the way, one day I thought to myself, I wonder, if it (*D_2O*) does all that it is supposed to do to cells, whether it would cause sterility. Of course they were drinking the D_2O, 30% D_2O in their drinking water. I put little males and female mice together and waited, and nothing happened. And I waited, and nothing happened! And, so I told Calvin about it. He immediately jumped on the bandwagon. He got very excited about this. So, I went on into this in greater detail. The first thing I did was to give only the males the D_2O; normal females and then did it the other way round. If the females had the D_2O nothing happened. If the males did, the males were sterile and there were no offspring. We wrote a short paper (I don't know if it was for *Nature* or *Science*, and one of my big moments in the lab. was the day I got a phone call from Washington, DC from the *Washington Post*. They had picked up on this story and wanted to know whether it could be used as a contraceptive!

VM: Expensive!

AH: I pointed out that, no, it wouldn't work because 30% of everything that a man drank for the rest of his life would have to be D_2O. You couldn't just take it occasionally — it had to be constant. I was at work when I got the phone call. Someone said "Ann, you've got a phone call from Washington, DC" and it sort of surprised me. It was one of my big claims to fame was that.

From there, then we started, Calvin wanted me to find out why/how/why it caused sterility. This is one of the things about working with Calvin, at least in my case and I think in the case of many who were not set some special goal to reach, you could go off on any tangent you wanted to, which was amazing. You didn't have to stay strictly on one thing. I got into *Drosophila* genetics and I got into sperm production and worked with an anatomist over at the Medical School, all sorts of things like that. Calvin, at this point, and when he was interested in something, he would be on your tail all the time. He would be off on a trip some place and he'd come home and he'd call me at home on a Saturday morning: "What's the latest, Ann? What's going on?" He just couldn't wait for results.

So that was the next thing I did. In fact, the *Drosophila* genetics, I guess, went on — and the mouse, these two projects, we never did find out why it caused sterility; it was something in the spermatogenesis, but we never could figure out exactly what stage was effected. This went one, I guess, the rest of the time that I was in Donner. It seemed to me by the time I finished that work, I went to Europe for three months and when I came back we had moved into the Round House.

VM: So that was in '63.

AH: Finally, that was in '63.

VM: You didn't take any breaks between the time you arrived and '63?

AH: No real breaks. I took a summer vacation and things like that but that was the first trip I had taken out of the country; it was to go to Europe for three or four months.

VM: One of the things we have heard from other people, I wonder whether the same thing happened to you, was that when they were originally hired, there was no statement about long they were hired for; it was an indefinite arrangement.

AH: That's right. He never said, I'll hire you for one year, five years, ten years. Nothing like that ever came up. He just said "Come out and you'll have a job". I don't think he even said what my salary would be.

VM: That was negotiated later, was it, or announced later?

AH: It was announced. It was never negotiated. You didn't negotiate!

VM: I suppose with someone in your position, who had already been working for seven years, there was no question that it was a job you were there for, you obviously weren't going for a postdoc. But was very informal in those days, wasn't it?

AH: It surely was. I was thinking of trying to think of other little anecdotes and incidents of things that happened. I was going to write them down before I came over here so I'd remember to tell you about these things.

VM: We can see if we can prompt you in due course.

SM: As a matter of interest, or perhaps ignorance on my part, when you talk about the D_2O causing sterility in male mice, this was not a permanent effect.

AH: No. You would take them off the D_2O and gradually — this is how we semi-pinpointed the effect. We knew how long spermatogenesis took and so we could tell what stage of spermatogenesis was effected by the D_2O by the length of time after that they produced baby mice again. We never figured out exactly what it did to the sperm. We'd look at eggs of pregnant females— this I did over at the Medical School working with Laurel Glass — and I would mate the males with little females, then take the females who had been mated to the Medical School and we would take the eggs out of the oviducts and look at them and see if the sperm had entered yet, or whether they were trying to get in, or what they were doing and we could never really...

VM: There were sperm there?

AH: There were sperm, but they were defective in some way.

VM: The D_2O had no effect on tumours?

AH: No.

VM: What about toxic effects of D_2O on mice?

AH: At 30% there didn't seem to be any. At that level there didn't seem to be any toxicity. It was just that it made them...

VM: If you went up, higher contractions?

AH: I don't know that I did. I don't remember now why I picked 30% or whether Calvin picked it for me.

VM: Of course, the first time I met you was in connection with an extension of that where Calvin suggested that Ozzie (*Holm-Hansen*) and I look at the effect of D_2O on algae. That was '56. That must have been exactly the time you were doing it. Where were you working when you first arrived?

AH: I was always on the third floor (*of Donner*) and I was in the little lab., very narrow lab., next to the great big lab. As you went down the corridor from the back door, go past the office, there was a big lab. on the left and a big lab. on the right, with the stockroom in between. Mine was the little lab. beside the big one, the one where they made plates in the hood. That's where Martha used to teach people how to make plates. I used to sit there and sort of laugh to myself. She had such a strong Southern accent and all these foreign visitors I know, I'd listen to her explaining what to do, I know they did not understand what she was saying, because, in a sense, she didn't speak English, she spoke some Southern language. They'd stand there, sort of bewildered, as she was telling them what to do. I felt sort of sorry for them. But they learned, she made sure that they learned. That was in the hood in my little lab.

VM: Did you share the lab. with anybody?

AH: No. That was my lab.

VM: People have commented about the social interactions in ORL as being a relatively open building: what was it like in Donner, where you had individual rooms?

AH: Well, there wasn't so much social interaction, at least in the beginning. Actually, one thing my lab. was, it was a meeting place during coffee hour. Everybody tried to crowd into that little narrow lab. of mine to drink a cup of coffee. That's the one really social thing we had. You didn't get to know other people as well, or as soon, unless it was somebody you were working with.

VM: What did you do about things like lunch, did you have lunch together or go your separate ways?

AH: I lived close enough that I always went home for lunch. I walked home, ate at home and then came back again. This may have been part of the problem, not necessarily that I went home for lunch, but that we didn't all work together in one big room. When I first came, I found it very difficult to have any social life and make friends with people. When I had been in Boston, my boss and all the other big shots in the

lab., took turns. As soon as I started on the job there, they invited me to the house for dinner. So, I developed a circle of friends. Out here, nobody invited me to their house, not one person. Finally, I just took the bull by the horns and I gave a Christmas party and invited people to my house. This sort of broke the ice. From then on, I had a much better social life. I would invite a few over for dinner now and then and gradually they would start inviting me. This is what made me feel in the beginning that Californians were very cold and uninterested in outsiders. Of course, in the beginning I was an outsider. They didn't care about other people because they were so busy with their own little lives.

VM: You think that this was the characteristic of the group in Donner Lab., that people didn't socialise a great deal.

AH: I don't think they did.

VM: Did you notice a contrast between that group and the ORL residents?

AH: We really didn't get to know the ORL residents, you see, except at the Christmas party, or something like that. We were in our building, they were in ORL. Unless we went over there...this is why I don't remember a lot of the people you have mentioned because they were in ORL. I was in Donner.

VM: And you didn't move between the two very often.

AH: With my job, I didn't have to, so I didn't. If you were doing something that you'd have to go over to ORL for chromatography, or something like that, you'd go back and forth. But I had no reason to go over there, so I didn't, except for the Christmas party, which we had over there, which was much more fun than the later Christmas parties. In the old days we had a lot of fun at the Christmas party. Do you remember that we used to exchange gifts without knowing whom we were preparing a gift for? We knew who we were preparing it for, but we never knew who we got one from.

VM: As I remember, and I think Dick Lemmon reminded me of this, there was an upper level of 25 cents or something like that.

AH: Perhaps a little more than 25 cents but it was a very low level. You had to be ingenious to think up something, and people were.

VM: I remember, though, by the time we got there, I remember you, and I am sure Sheila does as well, as actually being one of the very active social people in the place. Everybody knew you and it seemed to me that you became friendly with many of us.

AH: I think I did, not in the lab. but outside the lab. I think I took it upon myself to invite all of our visitors to my home. Particularly, every Thanksgiving I'd round up as many foreigners as there were and have them over to my house for Thanksgiving dinner, which paid off in the long run. When I came to Europe, everybody had said when they left to go back home, they said "come see me when you come to Europe". I think out of three months in Europe I spent about four nights in hotels.

VM: Who have you kept in touch with from those days?

AH: Who?

VM: Yes. People who don't live in Berkeley, I mean.

AH: Of course, Norma Werdelin and I were always in touch and Helmet Simon and for a long time Helmut Metzner and Luise (*Stange*), you folks, the Freys and there was somebody else from London, John...John somebody?

VM: John Barltrop?

AH: No, it wasn't John Barltrop. I can't remember the name but somebody...I think it was the first family that I visited when I went to Europe in 1963; they lived in London and it seems to me that they moved to Liverpool.

VM: That's Duncan Shaw.

AH: Oh, Duncan Shaw; was that it? I couldn't remember the name, even. I stayed with them. The Freys were in Southampton...

VM: ...or Reading; I don't remember; they are there now but.

AH: I think at that point they were in Southampton. I stayed with them. Some of the people I kept in touch with were ones I didn't know until after we moved to the Round House. I have kept in touch with most of our foreign visitors for several years and I still do with a few of them. As I said, our Russian friend Zofia (*Kasprzyk*)...

VM: Polish.

AH: Ah, that's right, Polish not Russian. We kept in touch until maybe five years ago, which was a long time

SM: Did the Donner group not participate together with the ORL group at the Friday morning seminars?

AH: Seminars, we were together for seminars, that's right. I was always scared to death because in those days — maybe Dick or Ed has said this — in those days you never knew who was going to talk when. We would all get (*together*) — I think they were in the (*Donner*) Library, weren't they?

VM: So I've heard; it was before my time; in the Donner Library.

AH: We'd all get settled down and Calvin would walk in, and he would look around and you would think "Is it I, Lord?" And he'd say "Ann, why don't you get up and tell us what you are doing?" You had no time to prepare at all; you had no inkling, even a day ahead of time, so you had to be up on what you were doing all the time, just for the chance that you might be the one called on.

VM: Did Calvin go into Donner itself a lot, did he come and talk to people who worked there and discuss detailed matters?

AH: Not a lot, I think. He seemed to spend more of his time, his office was in Old Chemistry, wasn't it?. I do remember, Norma told me this, that in those early days there was quite a rivalry between Marilyn and Norma as to who was going to be Calvin's secretary. When he called the office where they both worked, which was Dick's office, they both would grab a...

VM: Tell me about the secretaries. Nobody mentions secretaries in great detail. Who were the secretaries?

AH: Marilyn (*Taylor*) and Norma (*Werdelin*).

VM: When we came, there was one called Dee Lea Harrison, do you remember her, she was in ORL.

AH: There may have been one over there; if so, I don't remember them very well. There were two in Dick's office and, as I say, it was Marilyn and Norma until Norma left and then, it seems to me, that Jo Onffroy took Norma's place. I don't remember anybody in the time between those two.

VM: Alice Holtham was before your time?

AH: I remember her, but she was over in ORL, wasn't she?

VM: I don't know. We're going to see her; I haven't got her story yet.

AH: I think she was in ORL; I don't remember her very well. I remember Pat, the chemist; I don't even remember her last name. She worked not in the lab. next to my little lab. but the one across the hall and Dick would remember her name. I really don't know what she did. She was a chemist, either an organic or physical chemist, I'm not quite sure what she was doing even. It was still while we were over there (*in Donner*) was the time that Ed Bennett was chopping off the heads of rats, that was in the big lab. next to me, and he and...I guess Marie (*Alberti*) was there then already (Marie came in *1959*) the would drop a rat in a little cage in a container of liquid oxygen.

VM: Oxygen!?

AH: Yes!

VM: Really?

AH: We had to be very careful about fires and things when they were working on their rats. Big signs up: "Open oxygen". This was the way they could kill them the fastest. Then, you would hear Marie and Ed, I guess both of them, pounding almost with sledge hammers to chop the head off...

VM: ...of these frozen mice?

AH: ...of rats, it's even worse with rats; chop the head off and then Marie would open the head up and take the brain out.

VM: To come back to the secretaries, for a moment. They had an office, did they?, the two of them Norma and Marilyn, and the two of them occupied the office, there was no one else in the room?

AH: No, but Dick's office was off of theirs, with an inner door. As I remember it, that office did not have an access to the corridor. You had to pass the secretaries to get into his office. I am trying to remember where Bert's office was? It couldn't have been both Dick's and Bert's at the same time.

VM: Bert might have left by then. I think Dick took over from Bert, didn't he? (*Dick Lemmon took over the office that Bert Tolbert had occupied when Bert Tolbert went to the University of Colorado in Boulder in 1957.*)

AH: Maybe. I can't quite remember. The two desks and as I there was a lot of rivalry between those two women and, of course, another problem with them. Marilyn used to always complain. Norma was an inveterate smoker and the air would be blue. After Norma left, Jo Onffroy was an even worse smoker. The air was just blue with cigarette smoke. In those days, you couldn't tell them not to smoke in the office. I do remember that.

VM: In those days, of course, you could eat in the lab. as well.

AH: On, sure. We all did. When Ed got back from Europe, there were six desks in the office, right at the top of the stairs, it was right next to my little lab., and Ed and I, Pat probably had a desk in there; there were six of us that at various times shared that office.

VM: That was your office space as distinct from your lab. space?.

AH: All you had for office space was a desk. Whether we even have one drawer in a filing cabinet, I don't remember because it was so crowded with six desks in it; that's all you had.

VM: Was it "matey" with people around, did you feel close, or was the presence of other people a hindrance to you getting some peace and quiet? What was the atmosphere in the office like?

AH: Usually it was very quiet in there, except maybe at lunchtime. Since I didn't usually eat lunch there, but otherwise people would come in and sit quietly at their desk, doing their work, not talking to anybody else — they wanted to get their work done.

VM: What did you do about writing papers and reports? Did you have typewriters then or did you do it in longhand?

AH: It was all done longhand. I guess then we took them up to the front office for Norma or Marilyn to type up. Those are the details that I don't remember too well. I remember that not only did I have to write reports but, particularly when I was doing the sterility work, I had to make charts and graphs of the progress of the sterility in these little mice. I guess we took them over to the other side of Donner. We did a rough drawing and there was a drafting place (*room*) in the other half of Donner where they would make the final copy.

VM: This person was not part of the Calvin group?

AH: No, a sort of a service. Do you remember Peggy Smith? She was in that office, too, but I don't think we were all there at the same time. I guess at one time, Bob Noller...

VM: Don't know him.

AH: Well, he was a grad. student, and he's gone now (*i.e. he died*);he had diabetes very badly. He and his wife went backpacking with a friend of mine and I, we introduced Bob's wife to backpacking one summer; she never got over it.

VM: Then there were the quarterly reports. Do you remember those?

AH: I remember them from later. I guess I had to write then but I don't remember writing it. The paperwork just doesn't stick out in my mind particularly but I guess I did; probably had to particularly when I was on my own. When I was working with Martha Kirk, I suspect she did the writing; I was sort of her assistant.

VM: You said when you were on your own. Eventually...

AH: I mean when I got into this sterility business, the D_2O business, and things like that. Then I was more or less — I guess Bert sort of supervised me, but I was more directly responsible to Calvin.

VM: So you had periodic technical discussions with him.

AH: Marilyn would call you and say "Dr. Calvin wants to see you", and you would tremble in your shoes, pick up your papers, and go travelling over to Old Chemistry to talk to him about what you were doing.

SM: How many years did it take before you didn't tremble in your shoes at the prospect of Calvin? I'm sure you didn't later.

AH: Eventually, I guess I didn't. I was never really comfortable with him scientifically. Socially, that was all right. But in a scientific way I don't think I was ever completely comfortable with him.

VM: During the time you were doing this work, who was notionally in charge of all the animal work? Was it Melvin himself? Was Ed (*Bennett*) responsible?

AH: I guess Ed was. It may be that at this point I came under Ed's jurisdiction. Of course, there was not much animal work done. This was it; I was the only animal physiologist in the lab.

VM: Did you have any visitors or students working with you?

AH: No...

VM: Never?

AH: ...never. This is why it was so easy to fire me. I couldn't bump somebody further down the scale. When you were told that your time was up, you could always bump somebody who had less years of service. But the hierarchy was very clever, they knew that I was the only physiologist. There was nobody I could bump so they could fire me and I had no recourse to reinstatement.

VM: But that was very much later.

AH: Yes. But I mean, when it came about. This was the thing. I am saying this because you were asking who was in charge of the animal work. Well, Ed with his rat brains and of course that work was really not done, except cutting the heads off, in Donner. It was all done in (*the*) Psychology (*Department*). So I really was doing the animal work (*for the entire group*). Which, I think, that later, again it was not during Donner time, but later they bought me this trailer up on The Hill, they bought a trailer and put it up on The Hill so I would have a place to keep animals of my own.

VM: You mention that eventually you were fired, as you say. You really mean "fired" from the lab.? Encouraged to take early retirement? How did they sell it to you?

AH: Marilyn called me one day and I was up in the trailer working away. She said that "Dr. Calvin would like to see you". I said, "OK". I had no idea what he wanted. I came down, went into his office, and he sat me down and said: "Ann, in three months you don't have a job". Period; exclamation point!

SM: Which year was this?

AH: Seventies; I don't remember. Well, I'd been working there 27 years so ass 27 to 52: '79, I guess. There was no suggestion: "Would like to work for somebody else, would you like me to get you a job on The Hill in a different department, would you like this or that?" He just said "In three months, you don't have a job".

VM: Explanation?

AH: I found out later it was strictly financial. He could fire *me*, save my salary, the overhead of the trailer, all of this, the cost of my animals, and save a lot of money.

SM: Because it was a discrete operation.

AH: Yes.

VM: In your experience — another point we haven't mentioned to anyone else— in the 27 years that you were there, was anybody ever fired that you know about?

AH: Not out and out fired. There were a others whom Calvin found a job for in other departments. But he didn't do that with me.

On a completely different subject (we can come back to this if you want to)...

VM: No, I think we've explored that.

AH: ...the other very exciting thing for me was that I was the first person in the department, ahead of Calvin or his wife even, to know that he had won the Nobel Prize.

VM: How did that happen?

AH: It was in the days that I was mating little mice at all sorts of hours of the night so that I could take the females over to the Medical School the next day to get the eggs out. I was driving home from the Animal House; it was the old Animal House in those days, we hadn't built the new one yet.

VM: Where was it?

AH: It was close to where Bldg 90 is, I guess. It was over on the other side of the Hill campus from where the present Animal House is; way the other side. I was driving down, sometime after midnight, maybe one o'clock in the morning, driving home and I had a news broadcast on. The announcer said that "Melvin Calvin has just been awarded the Nobel Prize in Chemistry". I nearly dropped the steering wheel.

VM: What did you do about it? Did you call him?

AH: He wasn't even in town. He didn't know for another day or two 'til they could find him. I think he was in West Virginia. He was on a speaking tour. Gen was home. They called her the next day, I think, but then they had to find Calvin to tell him about it.

VM: So what happened to you next morning? I suppose it was public knowledge, was it, by them?

AH: Well, I don't know. The morning news had carried it. I remember I dashed into the lab. at a reasonably early hour and started saying "do you know?", "do you know?", "do you know?". Most people in the lab. hadn't bothered to turn on any morning news, and the reaction was " What?" It was really quite exciting.

VM: I don't know remember expected that was. Do you remember?

AH: I don't know whether it was or not. You never know about these things.

VM: I vaguely remember, but I have to say it was very vague, that there was some sense every year around Nobel Prize time, things got a bit tense. But I can't really remember it to be true.

AH: I had no idea what was happening. I didn't notice any tenseness. Maybe Calvin himself and the big shots got tense and maybe Marilyn and Norma did. I had no idea that he was a candidate even, you see.

SM: Do people know when they have been nominated?

VM: I think they often do know.

AH: You mean they know before it's made public?

VM: They know they have been nominated.

AH: I think so. Because my brother was nominated twice. He never actually made it but he was nominated twice for the Nobel Prize.

VM: And he knew that?

AH: He knew that. Or at least Eleanor, knew it, his wife knew it. She's the one who told me. So I guess he had been told that he was nominated.

SM: You know if you're in the running.

AH: You know that you're in the running.

VM: You remember the party at his house, in celebration of his Nobel Prize, although I can't remember whether it was before he went to Stockholm or when he came back?

AH: I don't remember the sequence of events and, of course, there also was a party in the Faculty Club. I can't remember which of those came first, the one at his house or the one at the Faculty Club?

(Tape turned over)

AH: I went off the Europe for three months and, when I got back, all of my belongings I had carefully marked what went here what went there — they picked out a workbench for me; everything was moved for me. I was not at all involved in that business of moving.

VM: So there was no equivalent of the big white table in Donner in terms of a meeting point?

AH: No, you met in each other's labs., I guess; you didn't really meet except for the business of discussing your immediate work. There was no big blackboard where you could diagram what you were thinking about or spread papers out as we did on the white table and, as I said, we did gather in my little bitty lab. for coffee in the morning. But other than that, we didn't really get together.

VM: You are one of the people, of course, who have really seen the development of the group from what it looked like inside Donner, what you remember looking like inside ORL, and later what it became in the round building. What have been your thoughts about a group of that sort? Did it strike you always as being a remarkable occasion or a run of the mill set of people?

VM: What did you think of the Calvin group as a whole?

AH: I guess I thought it was rather special. I didn't stop to meditate on this sort of thing much.

VM: If we encourage you now to meditate a bit. What does it look like from the standpoint of your experience, looking back at it?

AH: What do you mean?

VM: Well, what do you think — the group was clearly successful in some way.

AH: Yes.

VM: Calvin got a Nobel Prize, clearly they made discoveries of significance; what do you think was important about that group and the way it was put together?

AH: I do think that it was the interactions of the various types of people that he brought together, you might say the various disciplines. He had such a variety of disciplines of people who could work together on a project, if it was necessary. You could work on one little project by yourself, or you knew that so and so knew something about how to solve your problem maybe, and go and talk to this other person who had more knowledge than you did in some field of endeavour. It didn't happen too often to me, at least not so much in he group.

I did have to go outside the group because we didn't have any geneticists but I did go down to LSB and work with Curt Stern and his people; they were very good to me. I guess Calvin may have called them and asked whether it would be all right if I consulted with them. They were the people who got me started on the *Drosophila*, how to grow them, how to make up the media for them and all of these things. I remember in connection with that, the media was similar to hot corn meal. You had to cook it up and it was similar to hot corn meal. For some reason, the Office of Safety,

or something, wouldn't allow it now, I'm sure, but I was mixing this stuff up in a great big 4 litre glass beaker and it broke one time. Of course, I always wore ankle socks. The whole beaker went right down my legs. I went up to the office and grabbed the keys to the lab. car — this was in Donner, in my little lab. — grabbed the keys to the car and dashed up The Hill to first aid. The thing that was funny, when I got back down, one of the high wheels up on The Hill called me and said "you were driving that lab. car awfully fast through the lab". I told him that I had just poured hot liquid down my leg and was heading for emergency, He said "oh, well that's all right then".

SM: You positively enjoyed the difference in the availability of group interaction and sharing thoughts with other people, once you got into the Round House?

AH: I think so, yeah. To me it wasn't a more congenial group, because they were the same people, but it seemed more congenial because you had a chance to sit and hobnob with others and share experiences, share knowledge, this sort of thing. In Donner I spent almost my entire time in that little lab. and when I was doing the Drosophila I mean all my time. I worked 18-20 hours a day sometimes, right through the night, sitting at my desk counting *Drosophila.*

VM: But doing it by yourself?

AH: By myself. There would be grad. students there maybe until after midnight working on their stuff in their lab., but I would be sitting at my little desk just working away, all by myself.

VM: When you published, did you write papers by yourself, or did other people collaborate with you?

AH: Most of them were with Calvin, of course. Hughes and Calvin, Hughes and Calvin, Hughes and Calvin.

VM: What was it like writing papers with Calvin? What was it like for you, we have all had our separate experiences. How did you find writing papers with Calvin?

AH: He really was very good and constructive. I would pretty much write it and then go over to his office and he would have read it through and made notations. We would sit down and in a very friendly way, he would say "Well now, I think we ought to say this, this way", or "I'd like to change this sentence". It was always done in a very friendly way. At that point I didn't really have any quaking in my shoes because I was the one writing a paper.

VM: (The microphone has got a little bit twisted.)

AH: Because, as I was saying, I was the one writing the paper so I had a little more or prestige or something then and he was...

SM: Clout!

AH: A little, for the time that I was working on the paper with him.

VM: I suppose he must have done this himself some of the time, but in my experience, and apparently in yours, he never actually did the first draft of any papers, or any of the collaborative papers.

AH: Not that I know of. Certainly not in my experience. I did the first draft. I can't remember whether Ed would go over it with me before I went over to see Calvin. Ed certainly didn't do any writing but he may have gone over it with me before I went to see Calvin. Because, before that when I was working under Bert with Martha, if there were any papers written it was Martha and Bert who wrote the papers. Martha, Bert and maybe Calvin.

VM: What did you think — what *do* you think of the Round House as a concept which sought to carry on the best of what had previously been the accommodation?

AH: I think, I still think, and I thought at the time, and I still think that it's a good idea, whether they still carry on as they did when we were there, I don't know what goes on over there now. I haven't been in he building in a good many years. I don't even go in for their annual Christmas party. I don't go near the place.

VM: Well, it's obviously changed because there's a different management.

AH: A different management, but the concept, I think, is a fantastic concept and to do it you've got to have people of a lot of different disciplines in the same building. It wouldn't work that well if you were all chemists, or all biologists, or all physicists or something else. I think it has to be the interdisciplinary thing to have its value.

VM: I think there were some other characteristic things. I wonder what you think. The fact that it was a single organisation, virtually with a single budget, meant that all sorts of things could be unified — the stockroom and the secretaries — you didn't have to parcel up between different jurisdictions. It all belonged to everybody. That's remarkable, isn't it?

AH: I think so. I know I never really worried about money for equipment or money for animals. It was there within our general budget. I didn't think, well let's see: because I'm working with animals I have to get money from a different budget or things like that. It was all there in one big bag.

VM: So for the fifteen or sixteen years you actually worked in the Round House you found it a good place to work

AH: Yes.

VM: Let's also come to the extramural activities, the countryside, the mountaineering and all the other things. You've always been very active in that sort of thing. Who were the people, did you go climbing and did you go out for weekends with lab. people or were they mostly from other sources?

AH: They were usually from other sources, other friends of mine. I would say that for the first fifteen or twenty years we took a 10-day backpack trip in the Sierras and, as I said, one year this friend of mine, who is very good at this, and I would go. I had done backpacking when I was a kid with my brothers up in New England, but she was the one who introduced me to the Sierras. And then we introduced Bob Noller and his wife and, sometime later, after we were in the Round House, I did take one of Rod Park's grad. students backpacking, but that was with my brother. You see, I also had my brother, his wife and two small children, and we did a lot of things together.

VM: Did they live locally?

AH: They lived in Hayward. So trips to the beach or backpacking or picnics or this or that were more with him and the family than with people from the lab.

VM: I think it is becoming fairly clear that the people who were here on the short-term basis were concentrated much more socially in the lab. than the ones who were permanent residents in Berkeley. Hardly surprising. That's what we tended to see at the beginning.

AH: As I said, I got involved in entertaining all these foreign visitors as they arrived and I know some of them have said to me over the years that I was always so easy to understand. I think this was partly in contrast to Martha with her southern accent. Because I knew that these people — they spoke English but they didn't understand the way we speak so I would always talk slowly and carefully to them and many of them have remarked to me "well, you're so easy to understand".

AH: You do speak very distinctly and I think, in contrast, Martha had this strong Kentucky accent, as you say, and maybe it was more difficult for people who were not native English speakers.

SM: Maybe New England is more than half-way to Old England!

AH: Could be.

SM: Speech-wise.

AH: I don't know. I would speak more slowly, I think, to these foreigners. I may speak rather fast sometimes, but with them I would speak quite solely and distinctly if I could. I guess we went on trips, I don't remember...Oh, yes. There was another backpack trip; now this may have been after we were in the Round House, though. There were two Germans, twin brothers, Chris and what was the other? They were in ORL.

VM: Not Christof and Dieter?

AH: Christof and Dieter.

VM: Palm, wasn't it?

AH: Palm.

VM: That was later; that was Round House time — I think.

AH: I took them backpacking one time. The guy from Sweden (*Goran Claesson*), I took him backpacking one time. So that I did get a few people out.

VM: In addition to the Christmas party, there was a lab. picnic, wasn't there?

AH: It was up in Tilden Park. We would have a big picnic with hot dogs, hamburgers and stuff like that. I remember one of those picnics, I guess it was fairly early on, I got to baby-sit Ed Bennett's oldest child — he was so cute, he may have been two years old, or maybe less than that, I got to baby-sit him at the picnic.

VM: At the picnic?

AH: At the picnic; yes, just at the picnic. Was this while we were in Donner and ORL? Calvin used to have picnics at the ranch?

VM: That was later. The ranch came after he got his Nobel Prize. He did have picnics there.

AH: I can remember him standing over, because he did all the barbecuing, he would stand there turning chicken legs by the dozens, or by the hundreds.

SM: Gizzards also. I remember being offered gizzards.

AH: I don't remember that. He stood there himself, perspiring, wearing shorts and I remember the first picnic we had up there. Before the picnic it had been announced or we were warned about poison oak. There was a lot of poison oak there. The seminar morning before the picnic I came in with a branch of poison oak between two sheets of plastic, or in a plastic bag, and put it on the seminar table. He looked at me and asked "Ann, how did you get that". I said "well, very carefully, with gloves and tongs". Then he showed it to all the people. The foreign visitors didn't know what poison oak was. He was really amazed that I had managed to get some, without succumbing to it, and bringing it to the seminar for everybody could see what it looked like.

VM: Thank you very much for sharing all these reminiscences with us. We will build them in somewhere into the story.

AH: Good!

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Anne M. Hughes

Date of birth 22 Aug 1917 Birthplace Rockhill, S.C.

Father's full name Walter L. Hughes

Occupation Teacher Birthplace Trenton, N.J.

Mother's full name Clarissa Jewel Dawes

Occupation Teacher Birthplace Trenton, N.J.

Your spouse N.A.

Occupation Birthplace

Your children N.A.

Where did you grow up? New England

Present community Berkeley, Ca.

Education B.S. and M.A. - Duke University (Physiology + Pharmacology)

Occupation(s) Medical research

Areas of expertise

Other interests or activities Folkdancing, hiking, gardening

Organizations in which you are active

Chapter 16

RICHARD L. MEIER

Berkeley, California

June 13th, 1996

VM = Vivian Moses; RM = Richard Meier; SM = Sheila Moses

VM: This is a conversation in Berkeley with Dick Meier on June 13th, 1996. OK Dick: all yours.

RM: How did I come to photosynthesis? That's a rather interesting question. It was a round-about process. I was doing a doctorate in chemistry at UCLA and I was telling the faculty that the kind of chemistry they were teaching was going to be out of date very soon. So the faculty challenged me to give two seminars to the department on what I thought would be the chemistry of the future.

VM: When was this?

RM: In 1942. I gave two seminars, one on the chain reaction and the other one on nuclear reactions. They came together, announced, in '45, and suddenly I was an atomic scientist without ever having been part of the Manhattan District . And, more than that, they had sworn an oath to the FBI that they would keep the secret and I was not supposed to know any secrets. But I knew all of the literature up to the time when they stopped publishing.

I was working at that time in order to avoid the draft at the Standard Oil of California laboratories, now called Chevron, in polymer chemistry (chain reactions, again). In doing this, as soon as the bomb had dropped, I sat down and tried to figure out how it must have been done and then started talking to people around here at the University who might know something about it. Of course, they couldn't talk, but they couldn't object. They could object if I was dead wrong. So, I became quite confident. Then they organised a branch of the Atomic Scientists here in the Bay Area and changed the name almost immediately to the Federation of American Scientists; I have forgotten the exact name of the scientists in the Bay Area but it was a branch of the Federation of American Scientists. I became the chairman of the research committee that we organised immediately; there were about 500 of us in the Bay Area. Later on, I became the acting chairman which I was until 1947. So, some of the people are still around here, like Connick and Brewer; others have gone on.

In that particular period the question was: what to do with nuclear energy in terms of international control, or could it be used for power? I had set up, while I was still

working at Standard Oil, a study together with economists and engineers here on the campus so we did a study of the feasibility of nuclear energy. We came to a number of conclusions — plants had to be large, they had to be away from the city, and that sort of thing — which were still new. Then we circulated a manuscript around the members of the scientist organisation. One of the copies happened to go to Pacific Gas & Electric. Pacific Gas & Electric had noted, without any signature on the manuscript, as to where I, the chairman of the committee had come from. They tracked me down to Standard Oil and they then approached the president of Standard Oil and said "remember we had an agreement that if you didn't go into electric power we would not go into energy". I later on ran into these agreements in Britain, too. This very much embarrassed the President of Standard Oil and he said "Well, it won't happen". So he came down to the laboratory, to the director of the laboratory, telling me "it won't happen". Besides, I wasn't competent; I didn't have any background in the field. I said, "well, I might have agreed with you". But it just happened that the morning before I talked to him, the editor of the leading journal in America, in chemical engineering, had read the manuscript and he wanted it. He said it was the first manuscript with facts in it. So, here I was — we finally ended up with a situation where it was published, but without any names on it. It was amazing that only the sponsor, which happened to be a foundation that had provided the clerical work, and that sort of thing at the University, and the chairman of that particular fund was on the paper. All the rest was just a committee.

VM: Unmentioned? Unidentified?

RM: Unidentified.

VM: Which was the journal?

RM: *Chemical Engineering*. So, it got published this way but I could see from the flack that it was going to be a little difficult to do interesting research. So, I started looking around further. At that time, in fact, I approached Calvin. Calvin was sympathetic to the aims of the atomic scientist group.

VM: Standard Oil research, was that in Richmond at the time?

RM: Yes.

VM: Still the same place?

RM: Yes, still the same place, different buildings but same place. So I approached Calvin at that time, hoping that despite interviewing me, he might be able to write a short note recommending me for going beyond. That was the first time I really got to meet him. He said he couldn't really do that — I hadn't worked for him or with him. I understood that.

VM: Can I ask you a couple of questions about where and when did you meet?

RM: In the same building that you are talking about, the ramshackle building. It must have been 1946.

VM: He was quite heavy at the time, wasn't he?

RM: I got the impression only that he was very active and enthusiastic. I didn't judge about heaviness or anything like that. He was not thin, but he was certainly not heavy from

any way that would give me an impression. What I saw around me was very interesting kinds of action and him pushing the postdocs. and doctoral candidates. You always have to look at the latest instruments; that was impressive.

VM: Just as a casual visitor to the lab.

RM: I had not seen a lab. with that much vitality in it.

VM: In '46, that was very early days.

RM: Yes. Those were the early days. Actually, later on, I moved on to Washington to become the secretary of the Federation of American Scientists, representing them to the press, to the public, to Congress and that sort of thing, handling the political affairs and trying to get the National Science Foundation started. So I was there (*in Washington*) from 1947-1949. While I was there I began to realise that the secrecy was going to be so high in the future that it did not pay to continue a career that close to nuclear energy. That you had to get security clearance, which I didn't have at the time, and as a result the FBI was on my tail all the time, listening to everything I said in public, a spy at the office door, etc.

VM: They were simply suspicious of your knowing what you apparently knew from the public domain.

RM: Later on they told me that I was letting out secrets all the time.

VM: Although you actually didn't know any secrets.

RM: I didn't know what they had classified. We were very careful, too, in the sense that the people that I talked to said well, this part, is worth looking at in the *New York Times*, or sometimes in an overseas publication. One always pieced together what was secret from what was published and made the deductions of what went on in between, how it got that way.

VM: If I may harp back, just for a second to the way you started, how long did it take you after the bomb had been dropped to work out more or less what had happened, how it had been done?

RM: Overnight.

VM: How old were you at the time?

RM: I was 25.

VM: I asked the question because I was 17 and a friend of mine were on holiday together, and we spent the whole night trying to work it out, but we failed. Younger than you were and less experienced!

RM: I had had the chance of looking up all the published literature and the library was fairly good at UCLA. It had no one, however, who knew the field. They'd already been off in the various laboratories, so we had lost half of our faculty actually to different parts of the Manhattan District.

VM: Sorry, I took you back from the FBI.

RM: I then made a resolve that I had to do something that would not be classified and still would be important and interesting. I decided, because there was a man by the name of Mayer, a Frenchman, who later on became professor of nutrition at Harvard, a very young and very enthusiastic man. We met and talked fast with each other. He was talking about the food problem of the future. He was forecasting by 1990 there was very possibly going to be a protein shortage. So I said, "You know, that's an interesting problem. It takes that long for fundamental research from the forties to the nineties before it could make a difference. So you could do some important work at this stage." With his encouragement I went to the Library of Congress, which is the best American library, and started finding out what there was available. Mostly it was done through *Chemical Abstracts* which was comprehensive. So then I could begin to see that, yes, it was possible. The Germans had been doing some work during the war. It was just being released in '47 and I had identified it. I checked with my friends, who were on the FAS council who were biologists, and they said you can't believe that work because it's German, Goebbels' propaganda. I discovered the next week that it could be reproduced in New Jersey. I could then feel that I was on fairly safe ground.

So, I began to synthesise from food yeast what might be possible. Starting from sugar cane, going through food yeast into protein, etc. And it would be reasonably economic. One of the things I had learned at Standard Oil is a lot of chemical process technology, chemical engineering, and I had also been *the* person in the research group that understood prices, economics, and that kind of thing. So I could do economic calculations: what was economically feasible. So, that's what I could apply to the food yeast.

The difficulty was: Was there enough room in the world for the sugar cane, to grow the protein that the world needed by 1990? I had then decided I had to find out was there a better way yet, and that meant going directly to photosynthesis and trying to understand enough about the process to see what would allow one to convert it to this continuous flow technology that the chemical engineers were just then developing.

VM: So, you were adopting an engineer's approach to the problem.

RM: I was adopting any approach. I also adopted the social sciences approaches and everything else. For instance, I went to Margaret Mead and heard that she had looked at food preferences — would people eat the stuff? — and she would tell me what was needed to get people to eat the stuff, and she doubted it. But she also indicated the kind of research that was necessary. In other directions, as well. I was looking at total feasibility, not just production.

VM: In late 1940's terms.

RM: In '47.

VM: Before the biological revolution, as it were.

RM: Yes, that's right. Before the code was broken, before the genome.

VM: Before one knew there was a code to break.

RM: Well, there was a suspicion. Information theory came out in '48. At the very end of the war, while I was working for Standard Oil, I had been trying to figure out where the breakthroughs would occur next. I decided there had to be some way of

translating from biology into hardware-type technology. I found two other people in the Berkeley library looking at various journals trying to get a clue so we could discuss it with each other. But I was unsuccessful; I couldn't find it. Nor were they successful. It turned out to be Shannon of Bell Labs., who was successful. But because I had been asking the questions, somebody whom I still don't know sent me a copy of Shannon's book as soon as it came out, which was only a few months after the paper came out. I and a physicist in my office in Washington saw it and we said "This is it; this works!". So the next year, I took the slow boat to England with a lot of students on it and used the students as subjects and did all sorts of experiments on the students. The idea of a code already existed prior to the time that it was found and how it related to language was relatively easily handled.

About '48 it was then realised that the world population was going to get very likely still larger than there would be sugar cane land. Therefore, we had to do this approach to fundamental photosynthesis. I, during this period, was travelling around to the various centres that the atomic scientists had their activities: from MIT, Harvard all the way over to Berkeley, through the main labs. at Los Alamos and Oak Ridge, Chicago. In all of them I started asking questions about what they know. All the figures pointed to: at Chicago Gaffron and Franck and at Berkeley, here, and one man whose name I cannot recall right now who was simultaneously working but over in the more classical biology department, using microorganisms, and apparently working on some kind of mechanism of photosynthesis.

VM: Here in Berkeley?

RM: Here in Berkeley, in the 1940's.

VM: Were you thinking of Dan Arnon?

RM: Arnon! Yes, Dan Arnon. I couldn't think of his name last night. I tried to visit all of these people while I was making these visits to the atomic scientist groups. Shortly thereafter, toward the end of '48, the atomic scientists ran out of money. I had a family in Washington and they didn't know how they were going to maintain the office so that I looked around, talked to some of the friends that I'd made in Washington, told them I had to live by my wits and run the office with my left hand. One of them was a Pulitzer Prize winner for the major newspapers in Minneapolis, who also published *LOOK* magazine. He said, "Look, I'll hire you as a consultant, just for ideas that might lead to major stories in the future". I then said to him, "well, the real problem is going to be food in the long run, and therefore the only thing that you can take a picture of at the moment are the facilities in Jamaica for making food yeast and another facility run by a parallel producer organisation in Trinidad". So he sent me with a photographer to those two places and I managed to get a little bit of the product. They didn't advance the idea of photosynthesis or anything like that except that I had stopped in here (*i.e. Berkeley*) to test out the ideas of having an open cultivation that would be relatively cheap. Everybody here was very fastidiously trained to keep their algae from becoming infected one way or another.

VM: Can I ask you a question about what sort of framework you had in mind? Were you thinking of perhaps improving photosynthesis to give you better yields or were you thinking of better organisation in order to make use of existing facilities and existing abilities — what had you in mind?

RM: First I had to have a feeling for what was the simplest possible method of producing protein with photosynthesis and algae.

VM: Yes, but why algae? Why not field crops or trees?

RM: It was fairly evident from the calculations that you could get 50% to double the yield if you had a simple process.

VM: But you had capital expenditure in some sort of equipment.

RM: You would have to have capital expenditure to agriculture and, therefore, you look for something simpler than agriculture. The calculations there had suggested what we really need is stirring. Jack Myers over in Texas had indicated that the yield went up quite a bit. The way they were stirring at the moment was something that was fairly expensive in the laboratory so that this began to add a new component in order to get another 20-30% increase in yield per unit area. Another question was about edibility. Nobody ever tasted the stuff except a scientist at Columbia University. He said, "Well, I took some of the product that I had accumulated in test tubes and kept in the refrigerator, and put it together and managed to swallow it". I said, "What happened?" He said, "I had to stay within sight of a bathroom door for two days!". So, it did not seem to be edible. We had to do a search on amino acid distribution and all that sort of thing in the protein, which is pretty good, quite balanced for human nutrition. We couldn't figure out what was wrong. In the case of *Torula* food yeast the psychology of it was that it really didn't quite fit human nutrition. People didn't like it and they didn't know why they didn't like it, nobody could figure out why they didn't like it. I managed to get hold of some and made chocolate milk out of it for my kids; they loved it the first time, the second time, they consumed it, the third time they said "do we have to drink it?". I could see that there was something in it that was wrong. A little bit later on, in the early fifties, I discovered that there was a product that was fat soluble in yeast that could be taken out of it that made it very compatible; the natives from Brazil, all the way through the Caribbean, had been doing this for a long time. Wherever they could find yeast or yeast-like materials they had been boiling them in oil with cornmeal or something like that. Here's the way one learns.

The stage after that was one where we started looking, we are now about 1949, and Leo Szilard had always been somebody behind my life sort of pulling strings, and that sort of thing, and he had managed to find some money at Pabst Beer for me to give lectures in microbiology at the University of Chicago in the spring of 1949. Since I had to live by my wits, I had to accept things like that. I gave as thorough an analysis of the way in which the microbiological work might affect world food problems at that time.

VM: You had, presumably by then, decided that you were not going to make a career as an experimental scientist.

RM: No, I hadn't. It could be experimental science too; if so, I wanted to do those key experiments. In order to get the "key" I had to take all the relevant factors into account. At that particular time, as a result of those lectures, the Division of Social Sciences at the University of Chicago offered me a position to sort of teach the people in social sciences about the new world of science. They wanted to put me on the faculty in Social Science. But I said that social scientists don't make the money that scientists to. They said, "we'll give you the salary of a chemist". But I said, "Do I have the freedom in the Division of Social Sciences? Suppose there is a new theory on the origin of life that I thought was interesting. Could I do this at the University of Chicago?" I was pretty sure I could, because the University of Chicago was a leading

institution at the time around the country; it really stood out as compared to the others in integrating disciplines. They said "yes".

I said I have one other thing. I want to go to Britain to find out how it was that all these refugee scientists that I have been dealing with here, as a upstart, young Turk leader of the atomic scientists, how they got the way they did? I had some theories already, given to me by a few of them, one of them by a lawyer who is also a political scientist, a social scientist whom they named the (*social science*) library here after — it's a name very similar to Calvin's and that's why it is blocked. At any rate he had some theories that I had to test out. I made arrangements, through an economist friend of mine in Britain, that he would find me the right place to work as if I were a British scientist in a British laboratory.

VM: I'm not quite sure what you are looking for. Could you clarify?

RM: There was a special kind of capacity for imagination that I could get in 20 or 30 different scientists who had these European origins which I had not found in any of the Americans up to that date, even Calvin, in that regard.

VM: Native-born Americans.

RM: Native-born Americans who were captured and held by American culture and these (*people in Britain*) had started out in another culture. So I wanted to start from the beginning and try to find out for myself what was the environment that might be responsible for this. He got me a post as an ordinary bench-type chemist at the Petrochemicals, Ltd. in Manchester, which was a subsidiary of what was called Petrocarbon, a somewhat larger (*company*), that was bringing modern petrochemical knowledge to Britain at the time. At the same time I made arrangements to be with the Manchester School of Political Economy and with the Botany Department in Manchester University and a few others like that. So I was running around trying to be a laboratory scientist four days a week and doing other things. In addition, I was trying to figure out: suppose I were to join this new programme on planning that they had in the Division of Social Sciences (*in Chicago*), what would I do and what would I say?

I decided that I would also like to practice some planning in Britain since they claimed to be doing it. Here was another kind of activity; that started in '49. In the meantime, I had contacted a young man (he was still a graduate student) by the name of (*William*) Oswald. I don't know whether or not you ever met him. He was growing algae over in the engineering station in Richmond.

VM: Richmond here? California?

RM: Yes, that's right.

VM: I don't think I know him.

RM: He was in the Sanitary Engineering Department, of all places; he was kind of an upstart in that sense. He was trying to apply real science to sanitary engineering. I could understand that whole area very well because in order to work my way through as a senior at the University of Illinois I was an assistant in the sanitary engineering laboratory. So I had to learn their language and everything else.

Anyway, he was taking chicken shit from Petaluma and doing things with it, like growing algae. Not only that but getting higher yields, too, so if you added what he could do with some kind of tincture of chicken manure and add that to Myers' idea of fairly rapid stirring, then you could begin to see quite high yields accumulating in algae, if only they didn't get sick, if only viruses or bacteria or something like that would not take over. So in '49, just before I left for Britain, I did publish a paper, the first paper, in *Chemical Engineering News*, which was a review of the field up to then and with some extrapolation into the future. It was something that I could at least take around to the laboratories in Britain and say "this is the only thing I've got in print so far but these are the things I am talking about".

I had to go to the Seaweed Research Institute in Scotland and I did look around to see if there was any work that was going on in Britain itself. There was a little bit in Manchester but not enough to count. Oxford, Cambridge: nothing. So I didn't learn very much about photosynthesis in Britain at that time, or about microbiology, really. But I did learn a great deal about just what it was in the milieu of Europe. Because this petrochemicals laboratory was 50% refugees from Western Europe. The Director, by the name of Steiner, was Viennese. He had a number of visitors coming in, so that Herman Mark, when he visited Britain, would stop in at Manchester to this laboratory. He was my idol in polymer chemistry. He could then tell a whole lot of fascinating stories about the bomb secrets that Szilard had before the war, etc., which he was in charge of for a while. So we had very active conversations.

The next stage after coming back...

VM: Excuse me. What was the answer about the European scientists, or are you coming to it?

RM: One was a kind of camaraderie based upon being a gentlemen, not quite the nobility, but there was a level below the nobility of professionals that would achieve this. There was a participation in the culture of the city which was an intellectual culture as much as anything else. So that the opera; for Franck (James Franck) to describe how he had gone to the opera in the evening almost three or four times a week after he had been to the laboratory all day is something that nobody in America would have done. But he felt it was necessary; it was part of life. It is this participation in the life of the intellectuals as much as anything else that had counted . But also the intriguing idea, high competitive intellectual activity between themselves, that is, to get the really advanced idea first. We had it in this country, too, but the idea of being the first man there was not as important as the one of being there "solider", in America. As I had moved around America I could see that difference fairly strongly. A whole lot more is difficult. Later on I looked at the elites of Japan and China and India and other places. India had a little bit of that British (*attitude*), very little. China and Japan were so utterly different that I had to recast myself in order to understand it.

VM: Looking at the scene, 45 years or so later, have things changed in your view?

RM: Between America and Europe?

VM: Has America changed? Has Europe changed?

RM: I would say what America has done is it has brought in the finest minds of the rest of the world as postdocs., doctoral students, etc. and that the interaction is fantastic. If you look at the journals right now and look for the ideas in *Science*, or any of the other major scientific journals, you'll find a high fraction of Asian names. Yet, when

you go to Asia you can't find any productivity in those schools. America has not only attracted some of their best but it has given them (*the Asians*) the freedom and the interpersonal contacts, the equipment, the laboratories, the library and everything else and made it easy for them to shine earlier, I'd say. In Japan you have to wait until you are 50 and you have to retire at 55.

VM: But are you saying that changed American science now is reflecting the Asian component in the population?

RM: Also European. Previously it was heavily European. Now there are even more Asians than Europeans.

VM: But, the Americans are still not going to the opera, are they?

RM: The others are really Americans. That's the important thing. They think of themselves as American, they can't go home again. They are Americans. We are an open society. We can absorb these populations quicker than the Europeans. I had two room mates at UCLA who were Japanese and we had others that came from Europe, refugees. They were no different. We were already co-opting at that time and it's much more intensive now.

VM: What do you see nowadays, looking at the European scene?

RM: I haven't been trying to look at it for quite some time. I did look at Vienna about six or seven years ago and I forecast that Vienna should be coming back in vitality. It was a dead city and they are all growing old together, including the buildings, and I didn't see any hope in the mid-eighties. But when I got back in 1991, or so, I began to see that there really was hope; that the old families, the business families such as the Stoner family that I had married into, if those families remade the connections that they had had in Eastern Europe that there was a job to be done and it was likely to be done. The one thing that I could not foresee in 1991 is the Russian Mafia moving in, into the banks and other facilities. There is now an infection, you might say, that's come along with the dynamism that is slowly, it's not intense or anything, that has made a future for Vienna.

VM: Interesting what you say about the attraction of Europeans and Americans in the forties and fifties because Calvin's lab., I suspect at that time, was unusually full of European postdocs. and graduate students. Maybe that was one of the contributing factors.

RM: I am sure it was. Also, they were recommended to him by people whom he trusted.

VM: Yes. He makes a point of that in various statements that he has made.

RM: This is true in every institution which I talked to which tended to get outsiders. I had been one of the corn belt type, coming from Illinois. In fact I had managed to earn my way into college by picking tassels out of hybrid corn when it was just invented. Otherwise, I was far too poor to go to college. Being the corn belt type, I had done studies on why the bright boys chose chemistry. It was because it was so arcane that nobody in the family would understand and they could then proceed without any limits. For instance, my family had a number of school teachers in it before the depression and, therefore, they said school teaching is safe and you should be a school teacher. Of course, I would resist that. They were saying that you ought to be a farmer; farming is safe, etc. So we had a number of them that would be good in any

subject. University of Illinois at that time was sort of the world capital of chemistry and as I interviewed the honour students senior level at the at the University of Illinois I could see that these were people who were going to establish themselves in the American fashion but they didn't have the verve or the imagination that I discovered among the Europeans that had come here.

VM: In the early days of the Calvin group, in the forties and fifties, when there was such a high European component, was this unusual? Was Calvin much of a pioneer in recruiting people of that sort, or was it, in your experience, happening everywhere in this country?

RM: At that particular time, after World War II, the University of California in general was sort of leading the pack. I visited maybe twenty universities between 1946 and 1949 in America and I was very inquisitive in more than one department. I could say that in that sense the University of California really had the capacity to accept and utilise the brains and fit them in various ways.

VM: That's an interesting connection. The fact that Calvin was both a professor at the University but also involved so heavily with the Radiation Lab., which, by its very nature anti-foreign because of the security considerations that they had, meant that Calvin's students and his postdocs. had to get security clearances of some sort from the Radiation Lab. to work in his building. It was, in fact, not that easy for students from other departments actually to gain access literally through the front door.

RM: I worried about that. I knew about the "Q" clearance problem and things of this sort, working for the Atomic Energy Commission. I thought Calvin had some extra money around that he could spend outside of the AEC money.

VM: I think that's true. But somebody or other was telling us that it was literally difficult to be a casual visitor in the building as you could into other departmental labs. because of the security consideration, because it was an AEC building. It may be the case that in Calvin's lab. the attitude of the University of California, in spite of the Radiation Lab. bureaucracy, plus the Radiation Lab. money produced this very large and very interesting and unusual group at that time.

RM: Well, it certainly produced the equipment. After that, Calvin had to be the selector of the personnel and now to get them through the bureaucracy was something else again, which he apparently took the effort to do. So that the idea of creating a kind of world team was something that I most admired. For instance, (*the University of*) Chicago had an awful lot of the refugees from Europe but they were quite elitist; they did accept some of the people from outside, very seldom Oriental, however — only Hungarians and people like that; Hungarians and Poles — so they were somewhat exclusive. They worked hard to make sure their people could get into the laboratories, but the laboratories were smaller at Chicago and the pace was much slower.

VM: Sorry; I've interrupted your main theme.

RM: That's all right; this is fun!

SM: As an aside, it may be of interest to note that, of course, Calvin spent some time as a postdoc. in Manchester.

RM: I didn't know that.

VM: Between '35 and '37 he was there working with Michael Polanyi.

RM: I worked with Michael Polanyi later on; I didn't know that.

SM: Obviously you both appreciated the same sources.

VM: Calvin himself was also, as you know, from the midwest, from Minnesota.

RM: From the corn belt.

VM: So, he was under the same influences.

RM: It's amazing. Michael Polanyi at the time I arrived in Manchester had just published a paper proving that planning would not work. Here I was, thinking of joining a planning department. I looked carefully at the paper and said "Hey, you know, he's using the same kinds of models that we use for polymers. By using his logic I can prove that rubber won't stretch!". When I pointed this out to Polanyi, he said, "Let's not talk about planning after this." We talked about everything else in the world but not planning. He had this feeling primarily because of his European experience. Thank you for telling me about the background of Calvin.

SM: It seemed oddly coincidental.

RM: He was certainly a kindred spirit, there's no doubt about that. I can recognise that. So now we are returning from England, many other things were happening in England at the same time. I did have a chance to practice planning, working with the Scottish Council which was an official group in Scotland, and tried to figure out how they could get high tech. industry into Scotland and a few other things. Nobody would believe me, but it came and it came in the places that I said it should be put. So Scotland did win out in the long run but it didn't come as a result of planning so much as taking advantage of opportunity that they'd heard about. I had spent half a dozen trips to Scotland working on Scotland.

Coming back, I then knew a little bit more about what I had to learn to teach (*at Chicago*). This particular department was taking on the developing world and they had a special little experiment going on in Puerto Rico where the chairman of the department was Professor (*Rex*) Tugwell who had been a New Dealer under Roosevelt, one of the main New Dealers. The person he picked as a right-hand man had gone down to Puerto Rico to think through a post-war plan for Puerto Rico. So here we had Puerto Rico as our "experiment" and we were using it for laboratories, as we called them.

(Tape turned over)

One of the first things I got into at Chicago was what would one do about taking a place that was as poor as Asia and pushing it ahead, using modern social science and that sort of thing, plus anything I knew about science and technology. So teaching was that way until '52 when the budget cuts at the University of Chicago had forced somebody to get off the payroll. So, they decided that for six months I would get off the payroll and go to Puerto Rico and get on the Planning Board's payroll in Puerto Rico.

I was shipped down the river, so to speak, as they call it in America. The result of that is that I'd gotten well started. The Puerto Ricans taught me all the things that were not

in the textbooks about development so that I had to go back and write a book on how development, based upon the things that they had invented, how development might proceed in the Third World. I had looked over very carefully their own technology and biotechnology, and they (*the Puerto Ricans*) had done advanced work in yeast but they had not gone into food yeast for reasons that were already well understood. They also had the very first automatic factory in canned orange juice. It's very surprising that an automatic factory would be down in that backward territory. Just at that time there was an article in *Science* indicating that possibly the world population problem might be solved by a special product in the citrus rind that could be extracted, a very simple phosphorylation could be undertaken, and it could be "the pill".

VM: The "food pill"?

RM: No, in this case, the pill for population (*control*), anti-fertility pill.

One thing I had left out: In 1951, Szilard and I and Gaffron and some of the postdocs. in the institutes at Chicago had had an evening discussion group. It's now evident that the scientists are relatively free to choose their own direction and responsible scientists like "us" would try to choose a direction that might make a difference, recognising that it'd take thirty years for research to have an impact. We decided to look at all of the things that were going to be scarce in the future — food was obviously one, another one was water, still another was minerals, another was energy. We could then ask ourselves what could scientists say about these things that might restrict further social and economic development? When we did population, Szilard had discovered that the Rockefeller Foundation had had a study going on about the feasibility of an oral contraceptive. All the scientists at that time had thought that oral contraceptives should be the solution — it might be a liquid, it might be a solid. So the Rockefeller Foundation was investigating it. It had been coming to the conclusion, with all the doctors and sociologists, that it was impossible. That got Szilard mad and so he looked around for five top-ranked chemists, they were all easterners, and he discussed the problem with them. Within a matter of six weeks or so he found five different potentially feasible chemical paths to an oral contraceptive. He showed it to the Rockefeller Foundation committee so they didn't publish the report. Then, a year or so later, about '52, one of the persons he'd sought at Worcester Polytech. had gone a little further and he'd discovered that the tools were already there, and he did a few experiments, so they were getting ready to do field tests on an oral contraceptive and they chose Puerto Rico for these field tests.

Several things were coming together on the biological side. On the food side, we were involved in this but, of course, the Korean war was on. Then a notice came to the University through the Office of Naval Research that if ever we could have a Manhattan District in protein that would really be a great thing because at that time MacArthur was being pushed back to the end of the peninsula and we might have to do a Dunkirk. The Navy was thinking through how it would defend Japan against submarines from the Soviet Union when all the protein in Japan — not all it but a very large share of it — was coming from fish protein in their waters and they couldn't protect the fishing vessels. So here you'd have an American army, which would have taken leave of Korea, in a starving country. What could be done? They needed a Manhattan District in food.

After having that set of discussions we said that in food the highest priority would be trying to find a way of getting mass production of *Chlorella* probably. The Navy said "see what you can get up; we would like to have it as soon as possible"; the Office of

Naval Research. We had to immediately contact the top man: the only man who could understand us in Japan was Hiroshi Tamiya. Did you ever meet him?

VM: No, I don't think so.

RM: He had a biological institute. When we contacted him, I was the person that contacted him, he said, "I am sorry we just can't collaborate in any way whatsoever. The food problem is so severe in Japan at the moment that all my postdocs. and graduate students are out in the yard cultivating vegetables in order to keep us alive. We do have equipment but we can't use it: we have to use our efforts to stay alive."

VM: Can I ask a question: at this stage, during the Korean war, therefore early fifties.

RM: It was 1951 going into '52.

VM: What was your contact with the photosynthesis research activities around the country at that time?

RM: Every summer I'd be coming to Berkeley, as well as some other places, but at least Berkeley, from Chicago (this was where I was located) because my mother-in-law lived here and had a house on Warring Street and we could leave the kids with her and my wife and I could go and to the High Sierras for a major holiday. While I was here (*in Berkeley*), I would finish writing a paper or something. At the same time I would drop into see Calvin's lab. to see what was going on now, and see any other labs., including the engineering lab. Also, I'd go over to Stanford and the Food Research Institute. They had two sides: one side you may have visited but the other side was in economics. Fortunately, I could talk economics by that time so I would then talk economics of food scarcity with the Food Research Institute at Stanford. The connection was primarily that I was free during the summertime.

VM: Therefore, you were aware of the advances being made in Calvin's lab., at least in a general sort of way.

RM: More than general. I was mapping out now what were the specific enzymes and what were the amino acid constituents and what were the mechanisms that were involved and what were, more important for any kind of chemical technology, what were the rate-determining steps in reproduction and that sort of thing?

VM: Your view and your interests of Calvin's work was how you could use this information to increase food production.

RM: Absolutely. That's the reason he let me in.

VM: Presumably you were also in contact with Gaffron in Chicago.

RM: Yes, but he didn't have that sympathy. I could tell him these things, and gave some seminars in his institute, but it was something, it was applied research, it wasn't "pure", which is another feature of the European attitude.

VM: He wanted to keep his operation "clean", did he?

RM: That's right: pure. Calvin didn't care. Calvin came apparently from the midwest, so he was interested. He thought it was a socially responsible thing to do, if ever one could do it, to transfer the know-how to somebody who could use it.

VM: So Gaffron was a much purer scientist, as it were?

RM: Much more classical in the European sense.

VM: Calvin and Gaffron presumably knew one another and had contact at some point.

RM: I was around virtually at the time that the break had occurred between them. You must have been around also, I guess.

VM: No, it was before my time. I never knew Gaffron.

RM: Apparently what had had happened is that (*William*) Lawrence of the *New York Times*, the science editor, had come in, was introduced by Berkeley's Lawrence (*Ernest O. Lawrence*) of the Radiation Lab. who just left him with Calvin. Calvin didn't have anything else to say but what he was doing. The science editor could understand a good share of this. So wrote it up, in quite long stories, in the Sunday *New York Times*. That caused the whole contingent at Chicago to blow up. "Calvin is a publicist and he wants to cop the credit for himself"; this is the sense of priority in the European. Calvin didn't even know what...it came as a surprise to him. For several years, no matter what I said when I was talking to the Chicago people, it's just incredible that anybody would behave that way, I was the only person that knew what was going on in both laboratories. I would convey what Calvin was doing before it was published to Gaffron and the team that he had and I would convey Gaffron's material to Calvin.

VM: Gaffron himself was Austrian was he, or German? I don't remember.

RM: He had worked in Germany and I don't think he was Austrian. He could have been something else. He could have been right on the French border.

VM: He was European, anyway.

RM: He was definitely European.

VM: Were the bulk of the people in his lab. also Europeans?

RM: Half something like that; they were fewer in number.

VM: But there was an American influence in his lab. as well?

RM: Oh yes; there was definitely some American influence.

VM: Would they not have recognised Calvin's legitimate interest in the practical consequences of what he was doing?

RM: No, that had nothing to do with it. What really happened is that they felt that he (*Calvin*) had gone to the press and made these exaggerated claims.

VM: The antipathy was really all on Gaffron's part and not on Calvin's at all.

RM: That's right. I knew it all the time and I acted as a kind of an emissary, being in Chicago and coming usually more than once to Berkeley each year.

VM: As someone who, as you said, knew what was going on, perhaps the only one who knew what was going on in both labs., what sort of progress was Gaffron making compared with what Calvin was doing?

RM: About one-third, one-quarter the rate.

VM: Along similar lines?

VM: They were looking at enzymes, and that sort of thing, looking at mechanisms.

VM: So Calvin had the livelier and larger organisation.

RM: Absolutely. He had a larger group, he had the better instrumentation, although I must admit that some of the things that came out later on that became extremely important, such as the acrylate type gels and that sort of thing for separating out enzymes, I first heard about it in Gaffron's lab. But a year later I saw that Calvin was having great big print-outs of it so, in other words, Calvin ran faster.

VM: I think that one of the things that Calvin must have learned as a result of the introduction of paper chromatography to his lab...

RM: In fact, paper chromatography came first and then came the acrylate.

VM: I think that paper chromatography must have taught Calvin the benefits of being alert to what's going on in the latest technical developments. Because he was always, at the time when I knew him later than that, he was always very receptive to (*new technology*).

RM: Yes, ever since I knew him. He was really preaching that to the people around him.

VM: So once something happened, then he had the ability to capitalise on it. He would, as you say, invest in large measure in what he thought was good for the work he was undertaking.

RM: That's right.

VM: And Gaffron was more cautious, was he, in that sort of regard, or perhaps he didn't have...

RM: He was more sceptical.

VM: Maybe he didn't have the resources.

RM: Even when he had some of the key things (that turned out to be key later on) he didn't push them the way Calvin did.

VM: There's no point in trying to make a comparison between the two men, but you have told us interesting things about the two of them.

RM: I could also compare with Jack Myers. Jack Myers is a middle western type too, no European contacts, and he was a pragmatist in the sense of, well if something didn't work, try another way. He was the one finding out the very significant effect of stirring and quite a few contributions to what might become commercial later on came out of his way of cultivating algae. Again, it was a matter of style.

VM: But Jack Myers was never the breakthrough scientist that Calvin was. He never had the breadth of knowledge or experience or vision, I think.

RM: He didn't have that European contact. I would say he could have been a Calvin if he had had the European contact and exposure.

VM: That's an interesting comment.

RM: I was really impressed with what he was able to do with what he had.

VM: I'm sorry, I keep diverting you.

RM: This is fun. I'm interested to see what it is that you connect with.

VM: As you know, we are interested primarily in the history of the Calvin group and therefore I'm trying to bring into contact and make it explicit because the record that this is, we have to make it clear for those who may listen.

RM: You're absolutely right. I'm doing my best. I have to introduce context.

VM: Right. So where were we?

RM: We were back around 1952, end of the '52, the Navy says "no"; it isn't going to be in time. We have turned the Koreans around, the North Koreans, so we don't have to worry. Actually '53 they suddenly hit the Chinese, but that was something else again. Certainly no Manhattan District based upon protein was going to help at that time. In effect, there was no real demand. But by that time, some food — in fact, one of the things that had happened, we had passed the hat around among the scientists and we got maybe $500 and we sent it off to the starving Japanese scientists. Because we had done this, the Rockefeller Foundation was shamed and sent them $5,000. So, they could eat again and they could go back to work! They then set up what we thought was the highest priority and that was ponds, open ponds, and cultivate the algae. As soon as the algae were growing rapidly, they outgrew everything else around and everything else starved, even the bacteria starved. It was easy and here people in the laboratory had been spending all this effort trying to grow algae rapidly and keeping them sterile and everything else.

I had decided, when I was at Chicago in 1951, maybe I might go back to a laboratory and do what seemed to be the obvious thing and set up an ecosystem. open to the atmosphere, and try to find a way where algae would become dominant and, therefore, it would not be any kind of secure, sterilised kind of process. This was submitted to Washington — at that time the National Science Foundation had not yet been founded — but whatever source it was sent to. And the referees sneered at what I had proposed: "Everybody knows that you have to keep these things sterile; therefore, this is an incompetent kind of proposal".

VM: Well, I think there were really two attitudes. You were interested in the food potential and you didn't mind if there were bacteria...

RM: I told them I was.

VM: That's right...but people like the Calvin, and no doubt the Gaffron, group who were using them as experimental material really had to keep them clean so they dealt with one system at a time.

RM: What I am trying to say is that their results would have been obtained even if they hadn't been clean.

VM: There would have been more argument.

RM: They would have been doubted.

VM: Was this food situation in Japan at the time public knowledge?

RM: Public in the US?

VM: Yes.

RM: Not very much information about it. It was known to MacArthur and other people in Japan. Things were very tight.

VM: I was wondering whether the photosynthesis research people (Calvin, Gaffron and the others), knew then of the possible significance of their work in terms of food production, whether this was and issue...

RM: They did know it. In fact, at Stanford in either '52 or '53 (more likely '52), the Stanford people brought in Tamiya, his wife and one research assistant and they brought all the people who were growing algae in America in laboratories together at Stanford, and they exchanged information with each other about techniques that they had learned. They were thinking very much about algae as food. Then Mrs. Tamiya would take the product out of the laboratory and each time she would make a new dish out of the laboratory product and serve it at the tea seminar. I got in only for one of the seminars, it was held during the summertime at Stanford, but I got the reports now. What was she doing? I knew there were problems in the consuming of algae They said, "Well, her cakes were pretty good, she had a bread that was pretty good, she had a flavouring for rice that wasn't bad, but that algae stew, or so", one algalogist a specialist in Pacific algae) made a terrible face. He said "that was the most awful stuff". He said they had a Japanese name for it, and he gave it to me. "Oh, you mean she could do *that* with algae?" That would have made algae acceptable in Japan. This is the extent to which, I think one of the postdocs from Calvin's lab., was in that same group, (*algae culture evolved*). We had this feeling that "Gee, it looks now as if it ought to work for places like Japan", except that you had to find a market. You now had to think business-wise.

I didn't ever get a chance to see Tamiya again, although I corresponded with him several times, until I got to Japan myself in '66 and went over to visit him. He said, "You know what happened as a result of that? My students went into commerce." That was despicable because he had the European tradition. He said "And not only that: they are competing with each other which is un-Japanese". I said "What are they making?" He said the most successful thing so far is *Chlorella* yoghurt because they have heard from the western world that yoghurt is very good for people who have upset stomachs or want to keep slim, and green is a lucky colour and the two fit together very well. So, they made yoghurt and that was the first commercial product. That must have been sometime around 1960 or so that it came on the market.

SM: Was it actually green?

RM: Yes, it was actually green. It starts green and unless they do something to the chlorophyll, why it will stay green. So here was a product which he was basically responsible for in teaching the students but the students didn't have any jobs and they had to sell their careers to entrepreneurs and they had to invent things at that period in order to stay alive.

On the same trip, about 1966 it was, I was in Taiwan and somebody was knocking on my door early in the morning and it was an old friend from the sanitary engineering side who had been visiting. He said "Dick, you've got to see this. You know, there is the biggest damn *Chlorella* culture in the world here and it's ten hectares and ten stories high". I couldn't believe him, of course, and they're doing something to the algae that I can't make out because the translation problem from Chinese to English was so bad: "You've got to find out. How long are you going to be here?" And I said "three more days". I didn't even bother to see it. I got a story as to what it was. The ten-stories high was only a drying unit; algae contain a lot of water and they will spoil if you don't dry them very quickly, so they were drying it. The ten hectares was correct because the Japanese in order to make their *Chlorella* yoghurt had decided that the weather was better in Taiwan than in Japan and that they would be safe to have ponds in both places. They got the central committee of Taiwan to actually invest in this and, I couldn't figure out how, but they got the highest level support. I then had to do detective work. It turned out that the person I could meet was the planner, the chief planner in Taiwan, and I finally got him to talk. He said, "Look, I'll tell you what went on. It's so fantastic, nobody will believe you. Therefore, no matter where you tell it, you know, it's incredible".

What was happening is that the Taiwanese government claimed to be the legal government of China and, therefore, it had to maintain in its cabinet, departments for each province of China. There was a bureaucrat there, a mandarin, who was collecting all the information possible and this information, because China was communist, had to be used by the US government which was using some of the territory of Taiwan for its troops. He said what happened was that we weren't getting enough information, because China was a closed book. We had people in Taiwan from every province in China who spoke the dialects and everything else and knew their way around, but we could never get them there. And if we could get them there, we couldn't get them back. So, then the CIA and the Taiwanese government got together and said we will provide you with aircraft for drops in empty spaces, we will give the parachute-dropped people about five days of provisions based upon American K-rations, and then they have to be on their own and they have to move through China back to the coast and we give them special radio signal to send and we will pick them up by submarine off the coast of China. This way the Taiwanese could fill in data about the respective provinces and their duty was fulfilled and the Americans would get their evidence.

One of the things that happened there is that the people managing to get back (more than half of them did get back) was that they felt sick. The more they looked at it they detected it was the milk powder in the K-ration because many Chinese don't have the enzyme that allows them as adults to digest it. They said "What if we put in algae?" They got some of the Japanese algae and it was great. Immediately there was a military use for algae for the survival of these paratroopers. Then, the Japanese said "Look, you've got to find your own markets for this because whatever we do with this kind of thing we want to reduce the risk and broaden the market". The Chinese looked around for what kind of markets they could discover and they found that in the

Chinese pharmacopoeia all the secret dried mixtures that are sold by peddlers and through the grandmothers of households and the grandmothers then decide which one fits what ails you, that some of them required iron, others required minerals and others protein. Here, was *Chlorella* that could be used for all three. So they did the extracts of algae and provided them with different-looking materials for their respective ones (*requirements*) and they looked similar to what the regular pharmacopoeia would indicate. They then used this material for the Chinese pharmacopoeia. The man, the planner, is now the president of Taiwan! He managed to survive all the politicking and everything else. He was a very competent, very serious guy.

We then took people from the sanitary engineering lab. who were going to bring in microbiology and waste handling and a few things like that to the new Asian Institute of Technology in Bangkok. They were getting a new location, out on the edge of the city, so we very carefully instructed everything that might be known from Calvin's work and Myers' work and the Stanford work and everything else to this man who had a good background in microbiology. He then took it to this area outside of Bangkok and he set up ponds, and found very economical methods — he found low land area that he could defend against the water that would wash in off the hills, with plastic sheet bottom for the ponds, and they would have sides, they call them "buns" there, maybe three feet high, and they would scrape the bottom for stirring — that had been learned again here in Berkeley in sanitary engineering — and they could then produce a lot of algae. The head of the algae group (*in Berkeley*), Oswald, got a cable that "we're having trouble with our algae. Our chickens are getting spattled-legged (they were feeding it to the chickens) and we don't know whether it's our algae or whether it's our chickens. So, send us five kilograms of Berkeley algae as quickly as possible." Which took a little time to prepare.

VM: How did you get five kilograms of Berkeley algae?

RM: Because they were growing it out of chicken shit in the experiment station.

VM: Oh, I see. This wasn't Calvin's activity?

RM: No, no. This is more about what happened as a consequence. So he (*the man in Bangkok*) then received this algae, compared the Bangkok algae with the others, and the answer that Berkeley algae was great and Bangkok algae was bad. Now, they were grown by the same formula, the same timing, continuous flow process and everything else. But, they had forgotten that in Richmond that they were in the blast of the cold air that came through the Golden Gate. You had real tropical background so that the problem that turned up then was that the algae that they grew in Bangkok were already old, given the conditions. As soon as they grew them faster and got a higher yield, then they were the same as the Berkeley algae. So we began to understand more about the physiology of ageing. It also confirmed this Columbia scientist who ate a couple of test tubes of stuff that he had been consuming old algae.

VM: Can I ask you at this point, since your story is getting on into the middle of the fifties, I guess, some such period...

RM: This part is already in the sixties, a kind of sequel to it.

VM: Calvin's work on the path of carbon in photosynthesis was well over by that time.

RM: It was well over. We were continuing on the momentum — the Japanese plus American work.

VM: What would you evaluate the significance of Calvin's work, or Gaffron's too come to that, for the sort of thing that you are now talking about?

RM: It was absolutely essential.

VM: Can you explain how?

RM: Let me give one example only and that is that he had identified one of the key enzymes that would be necessary, and then somebody else a year later or so determined the fact that it was maybe 20-25% or so of the total and it was beautiful in terms of amino acid distribution for something like fast foods. We could imagine right away that the Mexican foods brought into the taquerias in California might very well use an extract of algae the way it was being done in Taiwan. The rest of it, the chlorophyll itself, was not very digestible, etc., so it was a way of separating out the major component of the algae by rather quick...They had already identified the methods of purifying or separating out the key component.

VM: Did their work enable you to get better yields of algae? As I recall by that stage, nobody had really gotten very far in getting photosynthesis to work any faster or anything of that sort.

RM: Jack Myers in Texas did some. The addition of the chicken manure added 30% to the output.

VM: Sure, in terms of culturing techniques, but not in terms of implying that the fundamental biochemical understanding to knowing how to organise the system...

RM: What happens as soon as you go into technology you're confident that you can begin to control things because of the background. You know which enzymes work, you know the temperature effects, you know what's the limiting reaction. Because of all this background you then don't make mistakes in technology, and mistakes are expensive.

VM: Can I stop this tape here?

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Richard L. Meier

Date of birth May 16 1920(?) Birthplace Kendallville, Ind

Father's full name Walter A. Meier

Occupation Teacher + Clerk Birthplace River Forest, Ill

Mother's full name Mary Lottmann

Occupation Secretary Birthplace Houston, Tex

Your spouse Robin Standish

Occupation Non-Profit Consultant Birthplace ~~B~~ Petaluma, CA

Your children Karen, Andrea, Alan

Where did you grow up? Chicago slums, Palatine, DeKalb County

Present community Berkeley, Ca.

Education teacher's college, U of Ill senior, UCLA PhD chemistry

Occupation(s) scientist, planner, futurist

Areas of expertise general systems, development planning, futurism

Other interests or activities many more

Organizations in which you are active half a dozen

Chapter 17

VIVIAN MOSES

Berkeley, California

June 16th and 29th, 1996

VM = Vivian Moses; SM = Sheila Moses

SM: It is Sunday, June 16th, 1996 in Berkeley, California. I am Sheila, Vivian Moses' wife, interviewing him about his part in the Calvin group during the 1950s and early 1960's Having been married for eighteen months we came to Berkeley together in 1956.

How did you first hear about Melvin Calvin?

VM: There are, I suppose, three strands in my wanting to come to Berkeley. I had been working with a graduate student, whose name was Glenn Bartlett, at University College in London on the metabolism of a fungus with which I was particularly associated, using radioactive techniques and also using paper chromatography and radioautography which we tried to work up following the example in a paper, I think by a man called Vickery but I'm not sure. It wasn't too difficult to get the chromatograms and to get the radioautographs but what was extremely difficult, and I subsequently found out why this had been the case, was to identify what the spots were. It had taken Calvin and his group many years to do this and I and my graduate student were simply not up to it. That was one strand. The second strand was that it was very much in the mood in England in the fifties for postdocs. to go to America for a period in order to pursue their scientific careers. Naturally, I wanted to do that too and I was alert to the possibilities of where to go. The third point was that someone whom I knew, someone in the family had suggested to me that if I was going to go to America it would be very nice to go to Berkeley because Berkeley was a good place, good academically and also very pleasant. Furthermore, if I went to Berkeley inevitably one would then see the east coast whereas if you just went to the east coast you might not see the west coast.

So, I was very receptive when Calvin's lecture which he gave, I suppose, towards the end of 1955 at the Institution of Electrical Engineers, as I remember, in London on his photosynthesis work was announced and I went. I think I must have known about him at that time otherwise, I don't see why I would have gone. I was terribly impressed with what he was doing and what he was saying, and this was clearly going to be the answer to all my technical-scientific problems.

SM: Were you able to meet him on that occasion?

VM: No. I have to confess that it didn't occur to me in the (*lecture*) hall itself to try and meet him. I knew already that it was very difficult if you weren't known to anybody on an occasion like that actually to attract his attention. So I didn't. It took me a little while, I suppose a day or two, to wind myself up to the idea of just what to write to him. By the time I decided to do that and decided what to say and so forth, and found out where he was, he had gone. So I missed him on that occasion and then had to write to him in Berkeley. So, that was how I came to know about Calvin and came to want to go to him.

SM: I now understand what decided you that you would like to do that. How did you go about it?

VM: I needed to get money. I wrote to Calvin and expressed an interest — I can't remember the exact terms — and he wrote back what I subsequently found was pretty much his standard letter that he wrote to people whom he didn't know. That was: "Get yourself money on a competitive basis and come, and if you can't get quite enough money (I'm not sure whether he said this explicitly to me but it was certainly what he said to many people), if you can't get quite enough money then we'll supplement it". The point he made was an entirely valid one, I think. For people he didn't know and who were not specifically recommended by colleagues and close acquaintances of his, he needed some filter to ensure that they were at least of reasonable quality. The filter he used was their ability to get money.

So I started to look for money. As I did not have a permanent faculty position at University College at that time (I was the equivalent of an assistant lecturer), as far as I know I was not eligible for a Rockefeller award. Looking around for possible sources of money — and I cannot now remember how many possibilities I found at the time — the one I eventually applied for and got was a University of London Postgraduate Travelling Studentship, at which I was successful. This paid the princely sum of £1,000 which, at that time, was $2,800, to cover all my expenses for a year — travel as well as living expenses.

SM: For me to ask you how you reckoned that this would be enough is rather silly because obviously I know. Tell me how you managed to work this out. After all, you had a wife, you needed both to travel and to live in Berkeley for that period.

VM: At that time it was very easy for British-born citizens to obtain American immigration visas. As you were willing and able to work, and you were a British-born citizen, you applied and were granted an immigration visa. And the intention was that you would work when you got there and indeed, that's exactly what happened. For the first year, at least, you certainly earned more that I did with my grant. Between us, of course, we managed. We were taking a gamble but, as far as I remember, the advice we got from other people it wasn't that difficult for people with your professional experience to get jobs and that's what we counted on.

SM: And what made you decide to stay...no, perhaps that's a little premature. Of course, I know that you arrived in Berkeley in the autumn of 1956. How were you greeted and what did you think of the place and the people you met on that first occasion?

VM: Perhaps I should say just a little bit about how we actually made the journey. We travelled by sea on a Dutch ship called the *Ryndam* which took eight days to cross the Atlantic. It was quite exciting, even the boarding of the ship. We boarded at Southampton but the ship wasn't in the dock — it was out in the roadstead and we

were taken out on a lighter — not just us, but other people as well. And at night time, as I remember, but I'm not quite sure about that. Eight days of wallowing on the Atlantic were not entirely to my taste, and we landed in Hoboken, New Jersey, that's where the Holland-America line had its base, then at any rate. We were met by members of our family, as it happened, and we stayed for a few days in New York and made arrangements to ship direct to Berkeley, or direct to Oakland, a trunk — or maybe more than one trunk full of our possessions because we were advised to take things like bed linens, blankets, etc. to the United States since we were likely to get an unfurnished apartment. We did all that, stayed for a few days in New York and went across country by Greyhound bus. Again, there were two reasons for that. Firstly, it was the cheapest way of travelling and secondly we thought we would get some idea of the country. And, indeed, we did. We had never been anywhere before where we had travelled over such vast distances by land and although there was only modest comfort at times, it was a fascinating journey.

So we stopped for a few hours in Chicago to change buses and we also met some friends who lived there. We travelled across country, stayed in Reno overnight so as not to arrive in Berkeley at a ridiculous hour. Finally, we came one day to Oakland bus station at about midday and were met by Paul Hayes who was then the, I suppose you would call him the laboratory manager. He picked us up, took us to the lab. I can't remember now whether I actually met Calvin on coming into the door. I met him pretty soon because we stayed that night in his house. I must say the organisation was incredibly efficient. There was a secretary in the Old Radiation Lab. called Dee Lea Harrison who immediately took charge of us and set about finding a flat. I think, but I'm not quite sure, even on that first afternoon she had already started to drive us around. But if it wasn't on the first afternoon, it was certainly the next morning. We found a flat very quickly on Bonita Avenue at $75 a month, which was unfurnished, or at least partly unfurnished. I remember that the lab. in general always seemed to have a stock of odds and ends of furniture and household goods, and so forth to lend to postdocs. who came on a temporary periods and all sorts of people brought stuff. I have a distinct memory of Calvin himself coming upstairs with a couch, a cane couch, on his head — or something like that. He certainly was a contributor and he certainly helped. He used to do things like that; he would join in and carry stuff around.

SM: Did it strike you then that this manner of greeting and informality was in enormous contrast with your experience of British university departments and the way that people behaved towards each other?

VM: Well, a total contract, of course. Perhaps I should say at this point in order to make my own position clear. By the time I came here I had actually six years of research experience (by the time I came to Berkeley), three years as a research student at University College in the Botany Department (what was then the Botany Department), and then followed by three years as an assistant lecturer in the same department, but with very light teaching duties, so it was essentially still doing research. My formal supervisor as a research student was Professor W. H. Pearsall who was an elderly gentleman, rather deaf, he had an old fashioned microphone and amplifier which, as far as I remember, he wore on his chest. The story around the department was that if he didn't want to hear anything, he would simply turn it off and sit there, not hearing and looking benign. He was really not very experienced in the sorts of things I was interested in and actually didn't play much of a role in supervising me. But there were three younger people in the department with whom in fact I worked and from whom I learned what I learned.

And so, among that small group of people there was friendship and they were all much of a muchness in age, each within a relatively few years: although I was the youngest, the others couldn't have been more than three or four years older than I was. But there was certainly not the enthusiasm in the place that I found when I first came to Berkeley. It was really a revelation to see what went on. Interestingly enough, ten years later another person, Ian Morris, came to Calvin's lab. exactly from the same place that I had come from, from the Botany Department at University College, and I was very amused and entertained to see that his reactions to Calvin's lab. really paralleled my own very closely. I was, in a sense, reliving my first arrival in the place when Ian came.

SM: So after this first impression of the contrast which you have just described between the manners, if you like, between people in university department in England and in how it struck you in California, there was, of course, the Calvins' personal hospitality and helpfulness and informality and warmth. How did California strike you after post-war austerity England?

VM: There's one thing, of course, I must correct, that is I was not aware of all the British university scene, only the bit that I saw myself and so that was only some of it. Maybe other places were indeed different.

SM: Of course, but this was your experience.

VM: Correspondingly, in Berkeley, although in time I got to know other places, for the first couple of years I really had very little contact with groups on the campus other than Calvin's group. So the comparison I can make is only between University College before I came and Calvin's group when I was here.

California, in general, was a very exciting place. There were all sorts of new things to see that we hadn't experienced before, many of them trivial now — supermarkets and parking meters and lots of other things like that. On the other hand, because we were native English speakers there were no difficulties of communication at all. I say, at all — very occasionally there would be a lack of understanding of a word in either direction. On our way across country, you may remember, we stopped in Laramie, Wyoming, and you wanted a coca-cola and I had great difficulty in making myself understood in a bar. Fortunately there was a Canadian there who felt, no doubt, that he was midway between the two cultures and he was able to translate my version of coca-cola to the local version and get the drink. That happened very rarely. There were obviously differences in language, differences in usage, differences in pronunciation, but they hardly interfered. We rapidly found the local way of saying things and people seemed to have no difficulty in understanding us.

The style of life was not unfamiliar. After all, we had been to the cinema enough and we knew roughly what to expect. There were mistakes that we made simply because we made the wrong associations. We would occasionally go into a post office to look for a public telephone because in Britain, at that time, the telephones were owned by the post office. We found here that they weren't and you had to look elsewhere. Aside from minor things like that, there was really very little difficulty. Before very long in our flat we were able to buy a car. That was a very exciting thing, this was the first car we owned; I think we must have answered an ad in the newspaper. One day a red 1950 Plymouth convertible drew up outside our door, this was the car that was being offered, and we were, of course, terribly excited and we bought it for $250. It had 97,000 miles on the clock at the time and, by and large, it didn't do that badly. One or two things went wrong, but on the whole it served all the time we were in Berkeley

and eventually took us back across the country on a rather zigzag route. We finally left it with my cousin in New York to sell on our behalf. Eventually he told us he got $20 for it. We owed him a few dollars for something he had sent us in the meantime. Anyway, that's what happened with us. Freeways were something else, of course, that we hadn't seen at that time. We quickly got used to it.

SM: Clearly language within the working group was something else because there were other postdocs. who came from other European countries and elsewhere. So, tell me something about who the people in the group were and the people with whom you worked mostly, and then, perhaps, something about the social activity both inside and outside the lab.

VM: When I first got here (*to Berkeley*), whenever it was that I saw Calvin for the first time to talk science and what I might do, he suggested that I should work with Ozzie Holm-Hansen on the behaviour of deuterated *Chlorella*. To relate it to what other people have said, Ann Hughes at that time had already been working on deuteration in mice, deuterating their drinking water, and because of a lot of experience that was already in hand with photosynthesis, as far as I could tell, for no specifically good reason but simply because it might be interesting, Calvin (I presume it was him) had thought it might be a good idea to look at *Chlorella* in a deuterated form. Because I didn't know the techniques and the ways of the lab. and so forth, obviously he thought it a good idea that Is should work with somebody who did, and that someone was Ozzie Holm-Hansen who was a plant physiologist/plant biochemist and clearly we could easily talk to one another. He knew how to do everything and, therefore, I would learn from him. So, that's the first thing I started to do.

SM: Can you tell me for my own information, what is deuteration?

VM: Yes. Hydrogen atoms exist in three forms which are distinguished primarily by their weights. The common form of hydrogen has a unit weight of 1; there is a heavier form which has a unit weight of 2 and yet a heavier form, which is also radioactive, with a unit weight of 3. Because the hydrogen atoms differ in their weights, there will also be differences in the way in which they perform chemically because, if you like, the sluggishness with which the heavier isotopes will move. So, there are energetic problems associated with that. Hydrogen is a very important element from all sorts of points of view. It's heavily involved in determining the three-dimensional structures and stabilising those structures of nucleic acids and proteins. It is to be expected, and indeed it was known at the time, that the heavier isotopes of hydrogen would have effects on the stability and structures of these large molecules. So that replacing the ordinary hydrogen in an organism by one of the heavier varieties would be expected to have consequences not necessarily easy to predict. Of the two heavier isotopes, deuterium is stable and not radioactive and, therefore, not in the least dangerous to use. The other one, tritium, is radioactive and there are many more limitations, therefore, on the way in which you can handle it.

What we tried to do was to replace all the hydrogen in *Chlorella* with this heavy version called deuterium, simply because it has two units of weight. We were not successful at that time; I think later people did do that. We could not get the *Chlorella* to grow if the deuterium concentration in their water was more than 60% but with such bugs we conducted photosynthesis experiments in what, by then, had become the traditional Calvin mode of lollipops and radioactive carbon dioxide and so on. It provided for me a vehicle of learning the ropes and, in the course of a few weeks, I suppose, I became as skilled as the rest of them in doing all the things that had to be done.

SM: Describe a "lollipop" and its function.

VM: A "lollipop" was simply a glass vessel about 4 or 5 inches in diameter, circular in view, flattened so that the space between the two sides of the lollipop was relatively narrow — I would say something like 5 mm — with an opening at the top for pouring liquid in and a large stopcock at the bottom. The idea was that you put the algal culture of *Chlorella* which looked like a green liquid, but it was actually microscopic plants in the aqueous medium, in the lollipop, shone lights from both sides so the algae were very highly illuminated, squirted in whatever radioactive material you wished to study the algal conversion of and, when you were ready to take a sample you opened the stopcock, which was a large stop-cock, and the liquid suddenly fell out straight into boiling alcohol and killed the plants very quickly, and, as it were, "froze" everything for later investigation. It was called a lollipop simply because it looked like the top end of a lollipop on a stick.

SM: Obviously, it must have done from the way in which you describe it. You are working with Ozzie (*Holm-Hansen*) on this problem which you say was later worked on and solved by others, with whom else did you work and what else did you do?

VM: The only person that I knew in the lab. whom I had known before we got to Berkeley was someone we had only met a few weeks before; that was Bob Rabin who at that time was in the biochemistry department at University College. A mutual friend of ours told me that we were both going to go to Berkeley so we made a point of meeting one another and so when I got to Berkeley, I can't remember which of us came first, but anyhow he was the only other person I knew. At that time, there was a very strong overseas contingent, although the staff members of the lab. — Calvin himself and the people who were in the senior and continuing positions, people like Bert Tolbert (who may have left by the time I got there; I really can't remember), Dick Lemmon, Ed Bennett, Al Bassham, Ozzie, I thought (although subsequently it turned out that Ozzie was not as permanent as I had imagined at the time), people like that and of course the secretaries and the technicians and so on — were essentially all Americans. The transitory population of graduate students and postdocs. and academic visitors had a high overseas component. Most of them at that time, my guess is, from England and the countries of Western Europe. Germany and Switzerland tended to be well represented but people also came from Scandinavia and from the Low Countries and from France. There were one or two Japanese, not many, one or two and, as far as I remember, nobody at that time from China or from Hong Kong or Southeast Asia or from India, that I can remember.

We, of course, became variously friendly with all sorts of people. There was a lot of community among the postdocs. because they were temporary, they were living in apartments which were not their permanent homes, they had no families for the most part. They might have been married but they didn't have any children and they were not established in the place. Therefore, all the domestic responsibilities which befell the senior members, or at least the permanent members of lab., didn't apply to the postdocs. So they had much more socially in common and ended to mix primarily, I suppose, with one another. Although we knew the permanent staff (Calvin and the others), we knew them well and we saw them socially to a reasonable degree, we were closest in fact to people more of our own age and more of our own, as it were, standing in the lab.

SM: As I remember, the permanent members of staff were hospitable, would invite us and other transient visitors to their homes, and generally the atmosphere was a very

friendly one socially. There were also, as I remember, weekend trips and picnics and things of that sort among the group.

VM: There were all those sorts of things, I think. There were occasional invitations to people's homes. I think they were not that frequent. but they certainly occurred, and there were gatherings, parties, whatever you like to call them, of greater or lesser magnitude. And, indeed, we must remember when we first arrived we stayed with the Calvins for maybe it was only a day or two because we got a flat very quickly. But later, when we came back in 1960 with our own kid, then we stayed with them for something like 10 days until we found ourselves a suitable apartment. So the Calvins had a basement in their house which I think they used for that often and they were very important vehicles for helping people to get settled because they were able to offer them at least temporary accommodation until they got fixed up. So many people certainly stayed at Calvins' house for a shorter or longer period while they got settled in.

SM: I think that much of their hospitality and their warmth was engendered by Genevieve, Melvin Calvin's wife, in which, of course, he joined enthusiastically. But she was enormously hospitable and kind and helpful. So apart from the social aspect of things outside the lab., tell me about how the group worked together, the physical aspects of the lab. and what difference this may or may not have made to the working of the group.

VM: Before I do so, let me say that it's true: Genevieve, of course, was very responsible for and was very conscious of the people in the lab. But, so was Melvin, in a rather different way. I think everybody felt that he always did his best for people in making sure they got jobs when they left him. Many people had come as postdocs. or as students without necessarily having positions to return to when they left his group. I think the general feeling was that he was very assiduous in helping people to get settled when they left him. One of the points I remember coming up for discussion at some time, but I can't place it and I can't really remember who said what in this connection, was the reluctance of people to accept as postdocs. those whom they felt they might have great difficulty in placing afterwards. There was certainly in later years, I think, the opportunity to have postdocs. who were much older that the norm, people who might have been in their forties, and there was reluctance, not necessarily because the people were not good or not suitable, but because of the feeling that it would be very difficult to help them to get a position later because of their age and difficulties of older postdocs. and so forth.

I think Calvin was always very responsible and to the best of my knowledge those of his senior staff who sought ultimately to have faculty positions he helped as much as he could. Now, I think it has been generally recognised that he had great difficulty — and many people have said this — great difficulty in securing faculty positions in Berkeley for his own staff. Indeed, only two of them — Rod Park and Ken Sauer, as far as I remember — ever succeeded in doing that. Other people, for one reason or another who, in the he end, wanted to have faculty positions which they did not have as Radiation Lab. employees in Calvin's lab., felt they had to leave; some of them, of course, did that.

SM: Since you are talking about personalities and those who were part of your life in the lab. and became friends, I think that this might be a point at which to mention one particular group member and this was Ning Pon. Perhaps you would like to say something about him.

VM: Ning was certainly one of the "characters" in the lab. I have to say that in that group of people, and I can't really remember how many there were at the time when I first joined in ''56, some tens of people at any rate, there were clearly some people more colourful than others. Ning Pon was, I think, one of the colourful ones. He was a graduate student at this time. He had been there for some years, two or three years at least, and he had some time to go. He was a very friendly person and he worked closely with our friend Bob Rabin and so naturally, both inside the lab. and socially, so we saw a lot of Ning. We saw a lot, actually, of lots of other people as well.

It soon became clear to me in the context of the lab. that collaborations between people were highly encouraged. This was not something that I felt had happened in London before I ever got here but was very much the name of the game in Berkeley. The pattern was that people would talk to one another — I was going to say continuously; certainly continually — they were talking all the time. The place in which they worked, the Old Radiation Lab., at least for the photosynthesis group where I was, was centred around this big white table that everybody talks about which was the social and academic centre of the building. It was the place where people gathered to drink coffee, it was the place where the discussions took place because it offered the opportunity for laying out the large chromatograms and other bits of paper, so you could put a lot of stuff on one flat surface and get a number of people around to look at it and talk about it. It was very much the centre of things and by the very nature of the building, which was rather an open building inside with a...well, there *were* walls and doors but not as many as...people did not work in cubby holes... there was a lot of gathering around this table. On such occasions, it was clear that people would take the attitude in discussing problems, they would say "why don't we do so and so?" Faced with something that needed to be resolved, someone would say "why don't we do this, or why don't we do it that way?" and somebody else would join in as say "we could modify and so forth..." And before very long you would find a new collaboration had been started. In addition to whatever it might have been that those people had been doing before, they added a new thing. This was continuously going on — people were constantly forming and reforming collaborative associations. And, of course, it happened to me just as it happened to everybody else.

So I started working with Ozzie on the deuterium problem, and I can't remember the sequence with which I worked with other people, but in the first year I must have worked with three or four other people as well — Chris Van Sumere is an obvious one I can remember doing a paper with. I can't really remember exactly what the sequence was: Luise Stange, but she might have been in a later year. I don't remember exactly what it was; I would have to look up my papers for that. In the course of the two years that I was there, I think I got something like 15 publications (two years as a postdoc., this is), 15 publications with various people in these collaborative groups, most of them with Ozzie because he was the person that I was closest to but there were others as well with whom I collaborated here and there.

SM: You mention the two years that you were there and, of course, I know that you were there for two years. This was rather unusual for a postdoc. How did it come about that you stayed for a second year?

VM: As it got towards the end of the first year, or at least when the end of the first year was in sight, I felt I had so many things going which would not be able to be completed were I to leave literally at the end of the first year, and, incidentally, there was nothing special for me to leave for because I had no job waiting back in England, that I said to Calvin that I would very much like to stay on. I suppose I said to him, could he help? What he said was that he appreciated the situation, he recognised how

much was going on, he was clearly favourable to the idea of me staying on and so he offered me a year's salary. That year's salary was what I needed in order to be able to stay on for the second year. Of course, you had a job which could simply continue and so that part of it was assured and, therefore, we could continue on our second year on a somewhat more favourable financial basis since the salary that Calvin was offering me was a bit more than the university had. We were actually all set up and, of course, had everything running by then. I remember thinking at some time during that second year, or perhaps by the end of it, that had I had to leave after one year, I would have had no more than about three papers but I think by the end of two years there was something like 15.

SM: You had found this a good situation in which to work and, obviously, you had found Dr. Calvin responsive to what you were doing.

VM: Sure. I was well aware, it wasn't that I worked that much harder in the second year it was simply that the things that I had got going in the first year came to fruition in the second year, and there were new things as well. Staying for two years simply was more than twice as good as one year would have been, simply for this reason. So that was all very satisfactory. Within the lab. itself there was this continuous discussion going on. It was the year that Al Bassham was away in Oxford...

SM: This was '55/'56?

VM: '56/'57. Al Bassham was away — can't remember the exact sequence — but this was towards the end of the time when the carbon cycle was being finally wrapped up. There was a lot of debate both inside the lab. and between the lab. and other workers in other places, about just what was right and what was wrong. There was not just a lot of debate, there was heated argument, very heated argument in some cases. During that year one of the things that I think all of us will remember was the arrival of Otto Kandler. I should say that there were two German postdocs. in the lab that we were already friendly with, as far as I remember: one of them was Helmut and Hildegard Simon; Helmut is an organic chemist, really, from Munich (at least, he's in Munich now — I can't remember where he came from at the time), and the other was Helmut Metzner and his wife Barbara and he was a plant physiologist/plant biochemist and he came from Tübingen. They were postdocs. as we were; Metzner was a bit older but Simon was about our age; Metzner must have been about five years older, something like that. They were part of the gang, if you like, part of the gang of these postdocs. Another one that I remember was Duncan Shaw and his wife Elizabeth. Duncan Shaw was English and he had been an RAF reserve fighter pilot and I remember that he had crashed at least one fighter, maybe even more than one, and we used to rib him and ask him whether he had to pay for it out of his wages of sixpence a week or something of that sort! There were those two and I mentioned Luise Stange and Ozzie was really a postdoc., as he saw it anyway, he stayed in the lab. for only three years, and he and his wife Harriet also lived roughly in the same part of Berkeley as we did. So there was a gang of all these people. Oh, I'm sorry: Chris Van Sumere who came without his wife because she, I think, had just had a baby.

SM: He was, of course from Belgium and Luise from Germany.

VM: He was from Gent. Luise at that time was from — Göttingen? Can't remember; she was German, anyway. There was also a Japanese called Kojiro Nishida and his wife, whose name I don't remember, who was from Kanazawa in Japan; he was someone with whom Ozzie and I collaborated at one stage. So there were all these people

around and we were stable. We'd been there for six months roughly when Otto Kandler arrived.

Now, as I understand it, the reason Otto Kandler arrived was that he had been on a Rockefeller fellowship with Martin Gibbs on the East Coast somewhere. Martin Gibbs had many points of contention with Calvin and with Calvin's group about details of the results and, in turn, their interpretation. And, as I remember but really it may need correction, the Rockefeller people had suggested to Kandler that he should split his fellowship half time between Martin Gibbs on the East Coast and Calvin on the West Coast. The story was that Kandler really didn't want to do this but they insisted anyway. He arrived and there ensued, as far as I could tell, six months of heated argument between him and Calvin. One could see them in this glassed-in office (*in ORL*), which was Al's (*Bassham*) in his absence, waving their arms and scribbling furiously on blackboards. You couldn't hear what they were saying. I think, as far as I can remember, you could hear that voices were raised but I think you couldn't hear the details. Anyway, they went hammer and tongs at it for many times, for many, many hours. Part of the upshot was, there was, as a result of this, one final overall discussion where all the outstanding issues were hammered out in, as it were, plenary session, among all the interested parties in the lab. and we all gathered, as far as I remembered, we all gathered in the seminar room that we used in the Faculty Club. By that time the group was holding its Friday morning seminars in the Faculty Club (*in the Lewis-Latimer Room*), and I think we used that room: there was certainly nothing in ORL which was big enough for us. We all gathered around the table and went at it for...again, I remember it as being days. Maybe I'm wrong but it seemed a long time. We finally arrived at an agreed position. I can't really remember whether Kandler also subscribed to that agreed position at the time but anyhow that's what happened.

In the meantime, of course, there were lots of incidents around the lab. One of the ones I remember — and it must have taken place at that time — took place on a Friday afternoon. We had the habit of going to Laval's, on the north side of campus just at the beginning of Euclid (*Avenue*), which had a beer garden and people would go there on Friday afternoon, fourish or so, and have beer before dispersing for the weekend, although I shall come back to the question of what this dispersing meant. Calvin would sometimes come but not always. One Friday afternoon, Duncan Shaw and I (Ozzie Holm-Hansen said he was there as well but I don't remember that he was) — but anyhow, it doesn't matter. Duncan Shaw and I were in the big lab in ORL, as far as I remember standing in front of the blackboard which was above a drying oven or something like that, or maybe it was just a cabinet, and we were doodling fantasies on the blackboard. We came up with a new carbon cycle for photosynthesis which started off with a polymerisation of carbon dioxide to make what we called "polycarbon dioxide", this was the first enzyme "carbon dioxide polymerase", then we doodled on from there; I can't remember the details. We headed this thing the *Dephlogisticated Soot Cycle*. (You remember? You don't. Phlogiston was an old alchemical theory about combustion and I'm not sure I've got it right but anyhow, it doesn't matter.) *Dephlogisticated Soot Cycle* — and we were doodling away like this, putting the finishing touches, when Melvin came through the door and said something like "Hey, what are you doing?" We were embarrassed at what we were doing and so we took an cloth, ready to rub the whole thing out. He said "hold it, hold it. There might be something in it!" We had then to persuade him that there really wasn't anything in it, and he would be better off drinking beer.

SM: Was this typical of his approach and his reaction to whatever anybody happened to be doing anywhere?

VM: I think, you see, he is a man, was a man at that time, full of ideas, not all of them stood up to much scrutiny, but he was very prolific with his ideas and they were often provocative, that's to be sure but actually they didn't all stand up to scrutiny. And he was very receptive to other people doing the same thing and he would be equally critical. You could propose anything to him, providing you could support it because he would start by looking at the holes in whatever you said. He was entirely happy to have these things presented to him. I expect there were lots of occasions like this. It was his practice to come into the lab. often, maybe every day, I just can't remember how often. He would come in and he would actually sit down at people's desks. The pattern in the lab. was that everybody had a lab. bench, a flat working surface with shelving in front of them, and across the shelves would be the other half of the bench with somebody else working. Behind every person there would be a chemical rack upon which you could hang equipment and that was shared, in turn, by the person behind you and at the end of your bench, nearest the wall, there would be a writing desk and that's where you would sit; that would be your base.

It was commonly the case that Calvin would come into the lab. and, I don't know how often he knew who he was looking for or he just picked on people by chance, then he would start talking unless, I suppose, someone was obviously desperately engaged in something where they couldn't be disturbed. He would start talking to individual people at their desks, he would want to see the latest piece of paper with the result, whatever form it might be — chromatograms, graphs, read-outs from this or that machine. He would literally sit and pore (*over the material*) with the experimentalists, demand explanations, want to know why this had happened, why that had happened, offer explanations. There would be argument and discussions in an attempt for him to understand and to resolve what was going on. Sometimes, of course, he did this at the big white table and I suppose the first thing he would do if he saw a discussion going there, he would tend to join it. But there wasn't always something going on and then he would go to the other people. This was very commonplace. He was actually very acquainted at that time with what people were doing individually. He was familiar with the technology and the techniques and he often had things to say about it. I can't remember whether they were useful or not but he certainly was familiar. He would discuss the nature of the chemicals you were using or, in chromatography, he would have suggestions about what this material might be or how one might change this or that or the other. He was very much a part of the technology although he wasn't an experimentalist himself any more.

Later on, of course, it changed. Later on, when we got into the round building and the whole organisation became bigger he became less familiar with that part of things and he spent much less time in the lab. actually talking to people. He adopted the tendency of bringing people into his office to talk about (*their work*). Things eventually changed as the lab. became bigger and as the senior staff became older and, in fact, took responsibility themselves more for the graduate students who were all usually still in Calvin's name. Whereas in the early days, he took personal responsibility for the graduate students, later on, in fact, they were supervised by the senior staff and only notionally by him.

SM: Can we take things chronologically a bit later on about the later period. Tell me some more about the nature of the building. It was called ORL because it was the Old Radiation Lab., I understand, and it was the shape that it was because I think the 37-inch cyclotron had been situated in it. Am I correct or would you like to...?

VM: The 37-inch cyclotron had been located in it. It was certainly gone by the time that I got there. I don't think the shape of the building had anything to do with it. The building was much older than that, as I understand it. It was a first world war temporary building, I think we'd been told, and the cyclotron came 20 years later.

SM: But the shape of the lab. might have reflected that.

VM: How the building had been built originally I have no idea. By the time we got there, the ground floor was largely open labs. in the part that we worked in. I think there was another part of the building which housed the machine shop to which we had access; they were not part of the Calvin group. The structure of the building, as far as we were concerned, was to have at least one big lab. on the main floor with several people working in it (I can't remember how many) and there were other labs. also with multiple occupancy as well as one or two offices. There was this glassed-in office of Al's, I remember in particular. Upstairs there were more labs. And, indeed, my office was upstairs and the first year I certainly shared with Ozzie and with Kojiro Nishida. I must say that that office of ours, of which I have a picture somewhere, looked as if an earthquake had hit it. It was always strewn with papers — all three of us were totally untidy when it came to our papers and all three desks were covered with stuff. Our lab. bench was also upstairs, I think in another biggish lab. but not as big or not as densely populated as the lab. downstairs. Then there were chromatographic rooms upstairs and I really can't remember what else. In the basement there had been built a concrete-lined room for doing the radioactivity counting of the chromatograms.

Perhaps I should explain that the technique generally was to use two-dimensional paper chromatography for the analyses we did of radioactive carbon. There were a number of points to be noted about these. The first thing was that the radioactivity actually had to be deposited on the corner of the paper and dried down there. The papers were then dangled in a tank with some very noxious solvents associated with it.

(Tape turned over)

So the person doing the work put the papers in the tank, I was always smelly-ish of course, and when you put the solvent in it got smellier and then you closed the lid and left the papers to...let the solvent travel across the paper for however many hours it took.

SM: Am I correct in remembering the smell which you would then pick up was phenol and you used to come home smelling like that?

VM: Yes indeed. Phenol was one of the solvents and it was also a corrosive solvent and you had to be careful not to get it on your fingers. As far as I remember, but I'm not sure about this, we didn't use rubber gloves. But I really can't remember.

Then you had to take the papers out of the tanks in order to dry them and the way in which this was done was to use some stainless steel paper clips to make sure the papers remained attached to a sleeve over which they were draped: anybody interested in the details had better look up the relevant paper. And you then lifted up this sodden wet piece of paper, held in place by two or three paper clips, and delicately took it to a drying rack in some sort of oven, hood really, fume cupboard. During this process, you got the full force of the vapours in your face unless you wore the "space helmet", which someone had thoughtfully provided, which was literally a

dome-shaped thing which fitted over you head and had an air supply in it from an external source which would keep the vapours out of your face. But, in practice, I don't remember that people used it. I think I tried it once and it was so unpleasant that I held my breath or something like that while I carried the paper across.

Every now and again, you would jerk the papers a bit too hard and they would tear; of course, they were wet paper and wet paper has no tensile strength. And if you weren't careful, these papers would occasionally simply tear and rip off and fall on the floor and that would be the end of that one. But most of them survived and you then dangled them in this hood arrangement and left them, I suppose overnight, and eventually they would be dry. They never stopped smelling, incidentally; they were always stinking of the solvent but, of course, they were not so strongly smelling as when they were wet.

You took the dry pieces of paper and then, in a dark room, and I can't remember now where this dark room was, but in a dark room you folded them round some X-ray film and put the folded paper round the film in a special envelope and let it sit for a protracted period for the radioactivity to cause a latent image on the film, and eventually you'd develop the film, all in a dark room.

SM: Was this not the dark room in the basement?

VM: No. I don't remember. There might have been a dark room in the basement; I just can't remember where the darkroom was but somewhere in the building there was a darkroom. Of course, you didn't spend that much time in there. It had a dim light, enough to see and it wasn't a very skilled operation; you quickly got used to what you were doing and you'd spend a minimum time in this dark room putting the papers next to the film and putting them in their envelopes and so forth, and later doing the actual development.

Having done all that and developed the films, the next thing to do was to mark up on the sheets of paper...let me back-track: align each sheet of paper with its relevant film; they were numbered and somebody had also produced a little rubber stamp which you could wet with radioactive ink, stamp it on your chromatogram and that would produce an image on the film. So you could actually like the film up very accurately with the paper afterwards and, using a box with a light inside in order to be able to see through properly, then you would mark out, trace out with a pencil, the positions of the various black areas (which represented interesting compounds) from the film onto the paper.

You then had to "count" the radioactivity, that is to say, measure how much radioactivity there was in each of the spots and you did this with an end-window Geiger counter. They were quite large, by the time I was there, they must have been about 2 or 2-1/2 inches in diameter and they had replaceable Mylar windows on them because the windows used to tear after not too long and so every now and again you could replace the window easily enough. Later on I think we used gold-sputtered Mylar windows; they were rather better for some purpose or other. Anyhow, the upshot was that in order to measure each particular spot, you had to make sure that the adjacent spots were not interfering in any way with the counting measurement being made at the time. Because carbon-14 is a low-energy isotope, the β-particles it emits are actually stopped effectively by sheets of paper, or certainly thin sheets of card like index card. And so the technique was to shield all the other material, apart from the bit you wanted to count, with bits of index card and then gently place the Geiger counter down exactly on the bit you wanted to measure.

It was a most painful activity. There were many such spots on each chromatogram and it was very easy to run large numbers of chromatograms, so one was faced often with dozens if not hundreds of these operations that one was actually trying to measure. It was one of the most boring things I have ever done in my life; it's true enough that if there was anybody else in the room you could sit and talk to them but since you were measuring each spot usually for a minute or two, you were constantly having to change things and carefully arrange the cards round some other pattern on the paper and do it again. It was awful.

SM: You mentioned the toxicity of the solvents. You are also talking now about carbon-14 and radioactive materials. How much care was there taken as far as health and safety were concerned?

VM: As I remember, it was minimal. People were aware of the fact that they were using radioactivity and that in principle it was dangerous; they didn't slop it around — occasionally, I suppose, it got spilled — and occasionally, I think, people were actually called in to clean up spills but I don't remember anything very dramatic. Minor spills you would clean up yourself, minor spills on the bench you would clean up yourself. You would be reasonably careful but not fantastically careful. I think the practices we used then would probably not be permitted nowadays according to current regulations.

Anyhow, we used to spend lots of time there for counting these spots on chromatograms and it was on that, on the data from those measurements, that everything depended. And so it was very important to get it done. Ideally, the spots should have been counted on both sides of the paper because the paper itself absorbed part of the carbon radiation and it was by no means clear — in fact later on it became clear that it wasn't the case — it was by no means clear, even in the early days, that the two sides of the paper would give equal levels of counting but I think the problems of manually doing anything about this were just too awful to contemplate and people didn't do anything about it. Later on, I became interested in the problem and recognised the need to count both sides of the paper, and at that point developed alternative methods where you didn't have to do it by hand. Anyhow, that's another story which came much later.

So a lot of time was spent in this underground counting room, underground — it was a semi-basement; you entered it from outside the building down a few steps and I think somebody said that it had been built specially and had been concrete-lined in order to shield out the radiation from the Crocker cyclotron which was in a adjacent building and, without shielding, caused impossible background levels before the shielding had been put in.

So that's what we spent our time doing. We did the experiments on the lab. bench; I don't remember that there were any special places where we used our radioactivity. As far as I remember they were on the lab. bench. We prepared the chromatograms more-or-less on the lab. bench, they were run in the special chromatographic rooms. Of course, these were smelly and people were aware of the toxic effects of the solvents and so they were careful not to run them in the open lab., and then they were dried in the drying places wherever they were and then counted downstairs in the semi-basement. So that was the practice for much of the time that I was there, and other people: that's what we spent our time doing — and talking. Of course, we would bring our material up onto the big white table and discuss what it means and do the calculations and write the papers; things like that.

SM: As I remember, as it came to the end of the two-year period, we had undertaken according to the terms of your visa particularly, to return to England. At that time Dr. Calvin was anxious that you should stay on. Would you like to say something about that and why you decided not to and what happened subsequently.

VM: What happened was that, as I mentioned before, I did not have a job waiting for me in England and Calvin knew this and Calvin had already been employing me for a year; as it had turned out, I think he employed me for about a year and a quarter. He suggested, therefore, that perhaps I shouldn't go back and I should stay there and join his staff permanently. That was the implication; whether he put it in those words or not, I don't remember. And I said I had a visa which required that I return to Britain for two years and he said it would be possible, or he might be able (I can't remember what he said), to get a waiver of visa requirements through a private Act of Congress; it's apparently much less grand than it sounds to do this, and enable me to stay. At that stage, however, I was really undecided. There were rumours that the situation in Britain was changing out of the post-war gloom. I wasn't sure that I wanted to stay in Berkeley on a permanent basis and so what he said was "Well, go back, if you want to, and take six months or so to think about how you want to respond to the offer and take it from there".

So that's exactly what we did. We went back to England in the autumn of 1958 and there wasn't much difficulty getting a fellowship; I got one at King's College Hospital Medical School but it was before the period when the new universities were being founded in England, which wasn't until the early sixties; there were no more than rumours, as I remember, at that time and in the two years that I actually spent in England, I think there was only one job that came up which I might have wanted. The job prospects were really not very good. And so after I'd been there for six months we decided that I would take up this offer of Calvin's and we told him that that's what we would like to do but that I had already started doing all sorts of things and that I would like to sit out the two years that the original visa requirement had stipulated and that would enable me to complete things in England.

Of course, I'd already had the experience that two years in Berkeley were worth much more than one year and I felt the same about the time I spent at King's College Hospital as well, and that it was very worth while staying on for two years. So that's how it came that we came back eventually in October 1960 to a permanent, that's to say open-ended, staff appointment. I don't remember now what was written on paper. There was a letter of appointment of some sort; don't remember quite what it said. It must have stipulated a salary and I think the lab. was rather generous in paying removal costs of our stuff across the Atlantic.

SM: I'm sure that it must have been signed "Greetings and very truly yours". (*Calvin's standard salutation at the end of his letters*).

VM: It probably was but it might not have come from Calvin. There was a formal letter, I'm sure, that came from the Personnel Office on The Hill and incidentally one of the points was...At that point I had to get an immigration visa which still was easy for native-born British citizens and, of course, I was employed for five years in the lab. without being an American citizen and therefore didn't have to sign any of the loyalty oaths or anything of that sort until eventually I did become a citizen, when the five years was up, and then I was employed on the same basis as anybody else.

However, to come back, if I may, to some of the social activities in the lab. that I can recall. The people worked very long hours; that it to say, the lab. was always populated. I suppose there might have been times when there was nobody in it but they were rather few. I think people were working there — somebody or other seemed to be working there day or night. It was often the practice, since we lived fairly close, for us to go in in the evening, at least for a little while in order to do something: to start something, to stop something, to put a chromatogram on or to take a chromatogram off, or something like that. So it was notionally a matter of five or ten minutes; it probably lasted into half an hour or an hour. It happened, I think, most evenings and there was always somebody there. I think the lights were always on, there was always a car around and the place was always populated. I never stayed there all night and I don't know literally whether other people did; it was the impression one had.

SM: As I remember it, we never went out on a social occasion in the evening without returning via the lab.

VM: That's right. So it was the sort of place where people were always busy and always active. Our association with the postdocs., as I remember, was very close. That's to say, we spent lots of our evening with friends, partly your friends, of course, that you had made in your own work, and partly friends that we had from Calvin's group and one or two other places on campus. But as I remember, there weren't many friends we had from other departments on the campus; they were all from the Calvin group as far as I remember, or essentially all.

So as postdocs. without domestic obligation, we were able to spend our evenings together and often our weekends together. There were many visits to the countryside, various places up and down the state, to the mountains, to the deserts and so forth. Camping was the common style of doing so because people had neither the money nor the willingness for the most part to stay in hotels although I subsequently found I much preferred hotels to camping! So we'd go for the weekend to Yosemite or to one of the other National Parks, or to some of the desert areas or whatever, and that was a common activity for people to do. Groups would go up. They would go up in the winter to go skiing and we tried that not very successfully on one or two occasions. And of course they went in the summer time and in the other parts of the year.

SM: I remember, actually, backpacking into Lake of the Woods — with help!

VM: That's right; we did. We were — I think neither of us was very good at mountaineering and the carrying that was involved. And then there were one or two nasty things that happened. I remember once when we had arrived in Yosemite quite late at night and pitched our tent, and I was still asleep in the morning and reluctant to get up, Ozzie Holm-Hansen brought the tent down on my head in order to try and get me moving. And I was very incensed at that and insisted that he put it up again before I would consider moving. However, by and large it wasn't bad and one got a bit scruffy after a weekend out in the wilds and you came back and cleaned up. We would eat food which we'd barbecued or did various things to; all not terribly much to my taste but I suppose I got used to it at the time. Inside the lab...There were also beach parties: I remember one party we had on the beach at Stinson Beach or somewhere like that and I think there may have been more than one of those.

SM: You've forgotten Death Valley.

VM: We went to Death Valley, I think, fairly soon after we arrived, and I remember that you cooked curry for us in pitch darkness or by the light of a torch or something like that, and you inadvertently put rather a lot of curry powder in and it was the hottest curry we've ever had. There were reminiscences which were nice.

There was one very good trip we took together with Luise Stange and Erminio Lombardi which was to the Southwest, to Grand Canyon and Bryce and Zion and places — Monument Valley. That was all very nice. We had one story there where we were on a side road in Monument Valley and the car slipped off the dirt road and got stuck on its axle and we simply couldn't move it. There was nobody around; I think perhaps one motorist passed by but there was nobody around to give us a ride. So we were about four miles from Goulding's Trading Post, across the main road through Monument Valley, and I remember — I suppose Erminio and Luise stayed with the car — while we set off trying to get help. We got down to the main road and waited for a truck to come and we flagged one down and it stopped. It was driven by an Indian, an American Indian. We explained at considerable length what our problem was and what we needed and he looked at us totally without blinking, and without saying a word, put the lorry into gear and just drove off! So we realised there was nothing for it but to walk the other couple of miles to Goulding's and eventually they sent something out and towed us free. There were always these stories associated with going on trips.

On that same trip, Luise Stange, who was much more athletic than Erminio or we were, hiked down to the bottom (*of Grand Canyon*) and back again in one day, 5,000 feet down and 5,000 feet back up. I think we sunned ourselves on the top (*at South Rim*) and waited for her to get back.

SM: She was a keen botanist apart from anything else.

VM: She was and much keener on all natural things than any of the rest of us were.

Then, inside the lab., there were, apart from the daily social gatherings at coffee times — we had lunch together: I seem to remember that we went to a cafeteria which I don't think exists any more, somewhere on the south side of campus. It was some sort of university cafeteria, roughly between ORL and Sather Gate, but where it was and what it was I really don't remember except that we went there and I have a distinct memory of Ozzie and I and Utz Blass standing in line one day, arguing about how you pronounced the name of the animal that looks like a horse and has black and white stripes. I insisted it was "zebra" and Ozzie insisted it was "zeebra" and we consulted Utz. Utz said "Of course, you're both wrong; it's actually 'tsaybra'." (*in the German style*). We must have gone there; I think we went there every day; I can't remember very clearly.

In the lab. itself, or as lab. functions, there was an annual picnic which I think was held usually in Tilden Park on one of the campsites there; everybody turned out and there was the usual park-type melee of picnic with barbecued this and soft drinks and people wandering around...

SM: And hot dogs and marshmallows.

VM: Yes, and playing silly games, children around, of course. The people with children would always bring their children to things like that. And we also, of course, had lab. Xmas parties which were held in the big room in ORL and people from both parts of the Calvin group, the Donner people as well, came in and I think we gave each other

gifts and I think there was a rule that we...not sure what the arrangement was. You either.. either the gifts were made anonymously or they weren't prescribed. I have an idea that you...everybody had a recipient nominated but the recipient didn't know where the gift came from. And you were limited to 25¢. Obviously, the idea was to be as funny as possible within the confines of this limitation. So we did that and that was one of those social occasions; they were always very free and easy and, of course, the Calvins came, Gen Calvin came, probably their kids came — I really don't remember.

SM: Was the punch alcoholic? Because in those days you weren't permitted to have alcohol on campus; can you remember?

VM: Just don't remember. I really can't. Was it punch that we had?

SM: There must have been something.

VM: There must have been something; I don't remember. There were also farewell parties, I remember. I can't really remember when they were.

SM: There was a particular gift that was given each time.

VM: That's right. Everyone who left the lab. received a little cylinder, a little plastic cylinder which had been — if only I could remember exactly what it had been. It had been irradiated in the cyclotron in some way and then tapped with a nail at one point and this produced a feathery structure inside the cylinder which was, as it were, the "trademark" of the lab. Somewhere I've a piece of paper which describes actually what it is but I can't remember for sure. So everybody got this as a leaving symbol of their time in the lab. and I think, since I left twice, I actually had two of them.

SM: Before we go on to the differences during your later period with Dr Calvin's group, you mentioned in passing the Friday morning seminars. Would you like to say something about their character?

VM: Well, they were very characteristic. The first thing that everybody commented on was their time at eight o'clock on a Friday morning. Calvin was a very early riser and was very active the mornings. He tended to go to bed early and progressively wound down as the evening wore on. So he was a morning man and, since classes started in Berkeley at eight o'clock in the morning and so on, he felt it entirely reasonable to have a seminar at that time. I must say I never encountered any such thing; all the ones I'd been to before than had been around tea-time. Anyway, there was this thing at eight o'clock in the morning that the British contingent always objected to it, but it didn't do us any good. And the format was entirely ritualistic, almost. I understand that, before I ever knew it, it had been a gathering where the whole group came together on Friday mornings and Calvin would look round the table and choose at random .

SM: Excuse me, the whole group constitutes both the ORL group and the Donner group?

VM: That's right. And Calvin would choose, not at random perhaps, but anyhow would choose someone to talk. By the time I got there, the decision was made at the Thursday lunch. The senior staff of the lab., a group that I subsequently joined when I came back, used to meet for lunch on Thursdays at the Faculty Club to decide lab. business and, at that point, the Friday morning seminarist was chosen and was informed after lunch that he was going to talk. I think people had some idea that it had been sometime since they'd talked, or they knew they'd come across something

good and that Calvin knew about it and would like it to be talked about, so people were not totally surprised but you never knew for sure until one or other of the lunchers on Thursday came back and told the seminarist that he was on. And people then began to prepare frenziedly for Friday morning and some of them were up all night getting their stuff ready, getting all their illustrations. Some of the people who were less confident at speaking, I suppose, were making preparations.

This went on for years — must have been ten years or more because it was not until sometime in the mid-sixties that we began to get fed up with this. The people were really not producing very good seminars and were getting so disruptive that we persuaded Calvin that we ought to have a programme of seminars where people knew ahead of time that they were going to have to talk at some time and could get ready. And, furthermore, you could then space people out and it would have the dual advantage of giving people time but also making sure that people knew that they would, sooner or later, have to talk and make sure that they had something to say at that time. But that came later; that came sometime in the mid-to-late sixties, by which time all sorts of things were very different.

So the seminars themselves had a characteristic form. There was a long table at the time when I first knew them in a room in the Faculty Club and the format was later preserved in the round building. A long table and Calvin sat , if you had your back to the blackboard, Calvin sat at the front of the room at the left-hand side of the table. As far as the speaker was concerned, at the left-hand corner of the table. He always sat there. The senior staff, for the most part, tended to sit round the table and the more junior people tended to sit on other chairs distributed around the room, although there were some of the juniors at the table as well because there were enough chairs.

And so, come eight o'clock or a few minutes after, the seminarist would speak. I don't know if this is literally true, but the impression I have is that it was rare that the speaker got past the first sentence without Calvin interrupting them with a question. Some people, of course, felt that this was very off-putting, the ones who were readily intimidated or didn't speak English very well or whatever, went into some sort of "tizz" when this happened. Other people took it in their stride. Some people even told him to wait and contain his patience until they'd presented their stuff. Calvin was always the first one but other people did it too and, at various times during the presentation, there would be interruptions from the floor and this was to be expected. And, in a way, it took the place of an open discussion at the end because the discussion had actually taken place during the course of somebody's talk so that, by the time we'd got to nineo'clock or soon after, the subject had usually been talked out and that was the end of it and people then packed it in and people went home.

SM: They didn't go home, surely they ... ?

VM: Well no, they went on to work. I'm sorry. We also had to write quarterly reports. At that time, in the fifties and for much of the sixties, all the support for the lab. — or essentially all the support for it — came via a block grant to Calvin from the AEC and our obligation was to write an annual report, I think, in which we had to say what we had done in this year, what we were planning — how was it — what we had achieved in the last year, what we were doing at the present time and what we were planning for next year; and this was an ongoing thing and, as it were, one moved the story down a notch each time you used it, so that next year's proposals in the following year became this year's actuality and the year after that became history. So we had to do that and the system was such that that was sufficient for Calvin to make a case to raise the money he needed.

Internally, we would write the quarterly reports for a publication which was actually called 'The Quarterly Reports', which was properly typed up and presented in some simple bound copy in the lab. and that was, in a way, the first form of record beyond people's original lab. notebooks and often formed the basis for subsequent publications.

And so there were lots and lots of these quarterly reports and what followed then was the UCRL reports, the University of California Radiation Lab. reports, which were rather like full papers but in typescript and not formally published by a journal. So people did that and it was expected that people would write a quarterly report every quarter or at least most quarters. It was thought that everybody must do enough work in a quarter to enable them to write a few pages of quarterly report, so that was very common. As far as I remember, the technique for writing was that people did not have typewriters then. In the beginning, in the fifties, people wrote longhand, gave it to the typists, of whom there were a number in the group, either in Donner or in ORL or in both, and they typed it up and made all the corrections so that typists had to retype things several times often in order to get rid of all the mistakes. This was before the days of memory typewriters or word processors.

SM: You mentioned in passing typists as support staff. I understand that there were other support staff. You also just mentioned the quarterly reports in order that Calvin could justify his funding. From that I understand that, beyond that, no-one in the lab had any worries about funding.

VM: That's right. I think Calvin made the case for funding on the basis of the formal application we submitted which was this three-year view of the lab's. work — next year, this year and last year.

SM: What was the source of the funding?

VM: The source of the funding was the Atomic Energy Commission via the Radiation Lab. That I think paid for essentially all the funding except Calvin's academic salary would have been different, but the people who worked in the lab. were paid out of that fund and the funds of the lab. itself.

SM: And was there any problem about the equipment that you felt to be necessary, and so on?

VM: No. At that time the supply of resources was very lavish. The impression one had, and I think this was subsequently borne out when I joined the senior staff and attended the Thursday lunch, that pieces of equipment with a reasonable case were usually funded pretty easily — unless they were vastly expensive. If you wanted to buy enormous things then, clearly, you had to look more carefully but run-of-the-mill things which, as I remember, were pretty lavish by other lab. standards, really were discussed but there was not much doubt about them. Chemicals and biochemicals and radioactive materials and so forth, I don't really think we discussed them at all. We just bought them. There was a mechanism for buying them. You told Paul Hayes, or the storekeeper or whoever it was, that you wanted these things and they came.

SM: And what other support staff were there in addition to secretaries?

VM: Well, one other thing, may I say. The problem with postdocs. was rather more difficult. There was enough money in the budget, certainly in the later years when I

knew more about it, to support a number of postdocs. each year. Now, some of the postdocs. came with their own money, as I mentioned earlier, and they didn't need support — or not much support . Some of them stayed on for a second year, as I had, and were funded by the lab., and so that used up postdoc. salary money, and then there were some people who were specifically recommended by people that Calvin knew and trusted and was prepared to offer them a salary on the basis of their recommendation. We, the rest of the senior staff, would also make the case that there were individual people who might have written to us and wanted to come who were worthy for support. So there was some debate as to how we were going to distribute this postdoc. money. I think, in the end, it was Calvin's decision how to do it but he certainly heard the views and received the advice of other people on the senior staff as to what to do.

There was some unrest late in the day, in the sixties, when — after very careful juggling that everybody was gradually reaching a consensus — we found out that over the telephone to one of his friends Calvin had promised another postdoctoral salary to someone and threw out our calculations and so on, but I don't know anything about this in the fifties because I wasn't party to it. So I got the impression that most of the postdocs. certainly came with their own money. That is it say, they had secured it competitively from some source or other and received no more than a supplement from the lab.

SM: You mentioned this earlier. Is there more about funding you need to say, or can we talk about the support staff?

VM: I don't think I know any more about the funding. The place was very lavishly supported in general terms. There were secretaries, I seem to remember two in Donner. Marilyn Taylor and Norma Werdelin, were — I think — the two in Donner and there was at least one who lived in ORL who was Dea Lee Harrison. There may have been others whom I can't remember. We had access to a carpenter and to a machine shop, I think, maybe one of the main Radiation Lab. machine shops was present in ORL and we, as a Radiation Lab. unit, had access to it so all of that sort of stuff was very well done. There were safety people around when you needed them. There were, inside the lab., ladies who washed the dishes, washed the glassware, who ordered the chemicals, who ordered the supplies. I don't remember that there was any shortage of support staff at all, certainly by any other standards that I'd been used to at the time, so it was very well set up.

SM: I think I remember your mentioning that there was a glassblower?

VM: There was a glassblower. Yes, I'm sorry, there was. There must also have been someone responsible for electrical work, but I can't remember who it was. So things really worked very well. I think there were also ways in which you could get to the Chemistry Department under some circumstances and have work done if you needed to, but I don't remember that any of this was an issue at all. It all went very well.

SM: You've talked at fair length about how the group worked and how it interacted socially. This was during the first period when you were there as a postdoc. and, then again, during the later period when you came back as a permanent member of the staff. Were there notable changes during that time, or would you like to go on, if there weren't, to plans for the building that would be housing the group in the future and how this came about?

VM: The big change that had occurred between my leaving in '58 and coming back in 1960 was the fact that ORL had gone and the group who had been there had had to leave because the building was demolished to make way for Latimer Hall, the new Chemistry building. The ORL people had been relocated to the basement of Life Sciences Building and this had many detrimental effects. For one thing, they were much further both from Donner and from Calvin's office in Chemistry. Remember, all this time Calvin had been a Professor of Chemistry and had activities and obligations in the Chemistry Department, so his office was always in Chemistry and the old Chemistry building where he was literally next door to ORL so it was a few steps for him to go from one to another and not much further to go to Donner. And then, with the move to LSB, it was several minutes walk away down the hill and much more awkward. So that was one factor.

The second factor was, in LSB, the Calvin group was distributed along a corridor in a number of rooms, bigger and smaller. There was at least one big lab., maybe two, but the atmosphere was never the same as it was in ORL. That was quite sure, and I personally occupied an office and it had bench facilities on it and it was much more isolated than it ever had been before. So that was one thing. The second thing was that, when I came back I was a senior staffer on an indefinite employment contract and so I could begin to look at things differently because I was not there on the short term that I knew I was going to be the first time. And so I began to become involved in scientific work which was likely to take much longer to come to fruition. I began to work with people on that basis. For example, the work that I did with Karl Lonberg at that period and then extended through to other people like David McBrien later on was of that sort. I also had my own technician, Julie Chung, I think was the first one. Chang?, Chung? — can't remember now. And she and I worked together in my room but it was a less satisfactory arrangement than it had been.

SM: When you came back in 1960 had the work been completed which led to Dr Calvin's award of the Nobel Prize?

VM: Yes, I think it had. The lab was proportionately much less involved with photosynthesis than it had been and the nature of involvement with photosynthesis had changed. Al Bassham was always, throughout his career, primarily involved with carbon pathways in photosynthesis and their ongoing consequences, and he was the one that carried the flag for that right the way through. But, by 1960 when I came back, much of the emphasis had shifted towards what remained still unknown in photosynthesis and that was the nature of the light reaction and so the whole emphasis had moved much more towards the physical end of chemistry or biophysics, if you like, as distinct from biochemistry. I am not a biophysicist and so my involvement with photosynthesis, as far as I remember, did not carry on — was not renewed after 1960. In fact, I was involved only between '56 and '58 during that first period. When I came back I was involved always with other things.

SM: You mentioned that you are not a biophysicist. I don't think you have mentioned, in fact, what your background has been?

VM: Well, my first degree was in biochemistry at Cambridge. It was primarily in microbiological biochemistry and then I took a PhD in microbiology at University College in London. So essentially I started out by being a microbial biochemist, but certainly not a physicist. So, the way the lab. had shifted its emphasis in the photosynthesis area was nothing that I became involved with later on at all.

SM: So the group, in fact, consisted of several types of scientific approach?

VM: It had always done that ever since I knew it. You have to remember, Calvin himself is a physical organic chemist. That was his training and that's his basic understanding. He later involved himself in all sorts of other things but that's where he starts from. Some of the people, in fact, I would say many of the people that originally associated with him were chemists. The biological involvement tended to come rather later and I think that much of the early work, and he said this and it is really fairly clear, much of the early work in photosynthesis was very much a chemist's view of how to approach the problems. It had of course been influenced — it must have been influenced — by the work of Ruben and Kamen before the war in similar sorts of things. But even there, Ruben and Kamen were chemists and, although they worked with Hassid, who was a plant biochemist, the emphasis was chemistry. So that I think from the very beginnings the Calvin group were very chemical, always very chemical, and there was a strong emphasis all the way through. There were always people there who were themselves chemists, had been trained as chemists, and were looking at whatever it was they were doing very much from a chemical point of view.

When I got there in 1956 there were already physicists. There was Power Sogo in particular, who was developing NMR and ESR and things that I actually knew little about. There were, in Donner, people like Dick Lemmon who is a chemist and, I think many of the people in Donner — not all — many of the people in Donner were also chemists and there was a large chemical activity there producing isotopes and looking at the chemistry of isotopic carbon which was a Donner-based activity. Some of the postdocs. in ORL were chemists. Helmut Simon was a chemist, Duncan Shaw — as far as I remember — was a chemist. I can't remember what all of them were but many of them tended towards the chemical. And I was, if anything, rather towards the biological end or the biochemical end rather than the chemical end.

SM: Who else was there who were biologists?

VM: Well, Ozzie, he was a biologist. Chris van Sumere was a biologist. Bob Rabin was very much a biochemist. Ning, I'm not sure what Ning was originally. He might have been a chemist. There were not many straight biologists, if any.

SM: And clearly there must have been an advantage in the interdisciplinary character of this group in that people learned a great deal from each other and were able to help each other with lots of different problems.

VM: Yes. It's difficult to know quite how this worked. It was certainly true, and I suspect that the presence of Calvin as a cement between these various activities was maybe the most important factor because Calvin seemed to be able to encompass all of these things rather well, that is to say he could interact with the physicists, it seemed to me, pretty much on a physics level even though he wasn't himself a physicist. Better, I think, than some of the other people could do because remember that his was always an overview of the situation. He was never, in my experience, involved in lab. work; he never did it himself — sometime in his life, no doubt, but not when I knew him. Because of his understanding and the breadth of his experience and all the time that he spent, I think he had by far the best unifying view of anybody in the group and I think he knew enough of all these areas to be creative and constructive in all of them.

Now, for the rest of us who were involved day to day in doing our own experiments, I think that was probably less true, that is to say, we listened to what other people in other disciplines were saying but I think we found it much more difficult to be constructive and creative in them.

SM: Is it usual for groups to be interdisciplinary in that manner?

VM: Not in my experience but my experience, of course, is limited. Calvin's group, I suspect, was very unusual for its time and may even be very unusual now but, of course, groups now, forty, fifty years later, are not the same, not constructed the same as they were then. But for a group to have been chemically based with extensions into physics on the one side and biology on the other, led by a man with that sort of experience and view, and having furthermore that sort of funding stability which might have been a very important factor since people were not having to worry about where next year's funding was coming from. I don't even think Calvin had to worry about it. I didn't get the impression that he worried; I think he was pretty confident that he knew where it was going to come from. So I don't think that funding was a worry in that sense; it was well-funded, lavishly you might say, and people were pretty confident of its stability.

Another important factor was the hierarchy, or rather the lack of hierarchy inside the group. Calvin was the leader, Calvin was the only academic; there was no...what shall I say?...there was no conceivable competition on the part of anybody else to lead the group. Calvin was the acknowledged and only leader of the group and beyond that there wasn't really a hierarchy. There were permanent employees — people like Al Bassham and Dick Lemmon and do forth, *de facto* permanent employees, and there were the transients: the postdocs. and the graduate students. I don't think there was any conflict between these various groups of people; they each recognised where they were and I think each recognised that they were not one of the others. So no-one was going to be Calvin; the postdocs. for the most part didn't necessarily want to stay there and therefore weren't looking for permanent positions, and so it went. This produced, I think, a very relaxed set of relationships between people because they weren't competing for anything among themselves. All they were concerned with, naturally, they wanted to get their names on the papers that they'd contributed to but there seemed, as far as I can remember, no conflict of any sort with that sort of thing.

SM: And, of course, collaboration.

VM: And collaboration. I think it was...everybody was almost euphorically collaborating, if you like; in a way which I have personally not experienced elsewhere, people were working together for something which was coincidentally for the common good of the group and their own individual benefit and interests. They were absolutely coincidental at that time as far as I can tell.

SM: It sounds very exciting as an atmosphere in which to do science, as it were.

VM: I think it was and, as I keep saying, in my experience I haven't encountered another environment which paralleled that one. But, of course, there may have been others which I don't know about and there may have been...

SM: OK. Talking of environments, you had mentioned briefly or begun to talk about the necessary move to LSB which you returned to in 1960 and therefore the physical fragmentation to a greater extent of the group as a result of which...

(New tape)

SM: Today is Saturday, June 29th, 1996 and this conversation concludes my interview with Vivian Moses started on June 16th.

When we were last talking, our discussion ended on our return to Berkeley in 1960. However, you have pointed out to me that there are several things which should have been mentioned during the earlier period. Perhaps you would like to talk about some of those now.

VM: The first thing to talk about is the actual scientific work that I did when I was in ORL in that period from 1956-58. I think I mentioned that Melvin suggested when I first arrived that I should work with Ozzie Holm-Hansen on photosynthesis in deuterated *Chlorella*. as a way partly of studying that problem and also as a way of getting into the technology and learning the tricks of the trade. And so I did start to do that in the lab. on the second floor American, first floor European — the upper floor anyway. There was a lab. there with several benches in it, at least more than one. I think Ozzie and I both worked up there as well as one or two other people; it certainly wasn't as big as the lab. downstairs. We also had an office there (I may have mentioned this already; forgive me if I am repeating myself); it was one which I shared with Ozzie and I think with Kojiro Nishida. It was a fair old mess; none of us was tidy. Before very long I rapidly found out the style of collaboration in the lab. and the fact that everybody was interested in what everybody else was doing, very much the feeling that we were all working in the same area and what each of us did was of interest to the others. There was continual chatting, continuous chatting one might almost say, about the science and everything else under the sun. We all got to know one another and what we were doing very quickly. There was an endless set of comments like "why don't we...?" whenever ideas came up. Groups would form and reform in order to pursue particular ideas that came up in conversation — many of the conversations round that big white table that people keep talking about, at coffee time but also at lunch and the other times we met.

I can't remember the order in which these things happened. Ozzie was certainly my main collaborator, I think, throughout the period I was there. Not every paper that I did was with him but many were. Then I did some work with Ozzie and Chris Van Sumere and I think that was on the discriminatory effects of photosynthesis against carbon-14. I also did something with Nishida and Ozzie, I can't remember now what that was without referring to the papers. I did some work with Luise Stange and Ed Bennett in which we looked at the further incorporation of radioactivity beyond the first intermediates into the macromolecules of the cell and we tried to take the cell to pieces in a gross chemical way to find out where things were. As far as I remember: this is all without looking up the papers.

SM: And we're talking now of the period between 1956 and 1958?

VM: Yes. It was all done in that period and all done in ORL.

During that time also I worked with Ning Pon and Al Bassham (I think maybe almost the only paper I did with Al Bassham) in which we looked at two things. I can't remember again which authors were which. It was my first interest at that time in this whole question of compartmentation which later occupied me for quite a lot and subcellular architecture. We did two papers. One of them was to try to take spinach leaves to pieces and find out what each of the bits did and see whether they would together to account for the activity of the whole cell, all with regard to photosynthesis. And the other was an investigation, as far as I remember, in *Chlorella* of the interaction of the respiratory and photosynthetic mechanisms which, of course, biochemically overlap to a great degree although the respiration and photosynthesis are moving in opposite directions. As we knew that both of these things could happen

in *Chlorella* we were interested in their interrelations. That was a paper I think that I did with Al and Ozzie and — can't remember who the other one was. One of the things about that paper which was a little bit disappointing: it was the first paper, as it happens, ever submitted to the *Journal of Molecular Biology*; I had hoped it would be volume 1, page 1, but some fellow named Paul Doty, although he submitted his paper later us, actually got the first slot so we were the second paper of that one.

SM: You means this was the first paper, or one of the first papers in the existence of that journal?

VM: That's right and that journal became subsequently a very prestigious journal and it carried the new name of molecular biology which was not common at that time. I think the journal actually came out in 1959; we probably submitted the manuscript right at the time that I was leaving or maybe it was even after I left that the paper was actually submitted. Anyway, the journal came out I think in 1959 and we had the second paper in the journal.

There were lots of papers that I published in that period. As far as I remember there was something like 17 papers, although some of them may have been conference reports, at the Biochemical Society after I went back to England. Even if there were, there couldn't have been more than two or three of those so there were something like a dozen or 15 actual papers and one or two reviews. I felt that was very promising. That's why I was so impressed with the whole organisation.

SM: Were there not a couple that were significant in the winding up of the Calvin cycle?

VM: Yes. Together with Melvin, I think just with Melvin, I published the last two papers in the *Path of Carbon* series, numbers XXII and XXIII. The first one tentatively identifying the β-carboxylic acid which had been postulated as the addition compound between carbon dioxide and ribulose diphosphate. The second one was the final identification of erythrose-4-phosphate, not a particularly remarkable thing, but I did it. It was a chromatographic exercise simply to find the thing. That wound up everything.

There was, I found out only last year, Andy Benson had published a paper XXIV in the series, but that was not until 1970. It was quite different and really not related to the first twenty-three. You could say that I published the last two papers in the series.

Something else that I did which is of some interest: at the time that I was there the Path of Carbon, or at least the initial stages of the Path of Carbon, the so-called Calvin or Calvin-Benson cycle, was pretty much complete. I may or may not have mentioned earlier on about the big discussion that we had after the arrival of Otto Kandler when there was a great arguing of all the loose ends with the final agreement that that's the way the cycle stood.

SM: You did mention this controversy quite graphically actually.

VM: Right. Because of the success of the path of carbon there was naturally interest in looking at the other elements involved. There are three other elements and there are stories associated with all of them, two of them with me and one of them not. The one not with me was oxygen and that was done by a Swedish lady called Ingrid Fogelström-Fineman and she and others, I guess, tried to use oxygen-18 as a marker and then to irradiate it into fluorine-18 which was radioactive and could be measured. Let me say to complete the story at this stage what they tried to do. Oxygen-18 can be

bombarded, I think with neutrons (*actually with protons*) in a cyclotron, and it absorbs a neutron (*proton*) and emits a proton (*neutron*) and is converted to fluorine-18. So that oxygen-18, a stable isotope, becomes fluorine-18, radioactive with a half-life of a couple of hours. The idea then was to feed oxygen-18 as a tracer to photosynthesising systems, to run conventional chromatograms of the products, but only in one dimension, and then to elute the chromatograms sideways onto a strip of tantalum, as I remember, in which some of the resolution of the chromatogram would have been destroyed because the elution would have been done through a series of serrated points cut in the paper to produce a lot of little drops, some of which would have contained oxygen-18 material and some not. This tantalum strip was then bent into a circle with the dried spots on the outside, fixed to a wheel and it was that wheel that was rotated in front of the cyclotron beam. Then, when they'd irradiated it enough they put it against photographic film and got an image in the usual way. That was an attempt, I remember not very successful, to trace the path of oxygen in photosynthesis.

What I subsequently did was to use tritiated water to trace the path of hydrogen. Now, with carbon dioxide and using radioactive carbon one can, of course, control the amount of CO_2 one admits to the system and so the carbon dioxide itself, or the bicarbonate in the form in which we actually used it, was intensely radioactive. There wasn't much radioactivity but there wasn't much carbonate either. What there was very radioactive. You can't get to that state with water. Inevitably there is a lot of water around all of these systems and in order to make water sufficiently radioactive to have any hope of using it as a tracer, you have to use enormous quantities of radioactivity. That's what I planned to do.

Whereas in the course of the normal carbon experiments we would use a few microcuries of radioactive carbonate per experiment, in the one I did with tritium I used 5 curies which is getting on for a million times as much radioactivity simply in order to overcome the diluting effect of the ordinary water which had to be present. That was potentially a dangerous thing to do. It certainly couldn't be done in the open lab. the way the other experiments were done at that time, or with minimal precautions. The whole thing had to be done in a sealed glove box which had to be worked by extending one's hands into gloves into the inside of the box so that one was actually isolated from the material itself. That meant that all the equipment that we normally used had to be reconstructed in a miniature form in order to go inside this glove box which I would guess was something 4 or 5 feet long plus perhaps 2 feet deep; it couldn't have been much more than that or one wouldn't have been able to reach the back from the outside.

SM: Was it necessary, if these things were miniaturised, to use some sort of magnification in order to see what you were doing?

VM: No. They weren't miniaturised in that sense. They weren't tiny but they were compact so that the incubation vessel, I remember, was a little shaker bath which was about 6 inches by about 4 inches and it was constructed in small scale, not miniaturised in a minute sense. Also, it could be pushed inside this glove box.

SM: This way you were hopefully completely insulated from radioactivity.

VM: Insulated and isolated. Throughout the time that I was working in the box, literally all the hours I spent in the box, there was a Radiation Lab. monitor (that's a person, not a machine) with a machine (*i.e. a radioactivity counter*) next to me, monitoring all the time what was coming out of the box, if anything, to make sure that there were no

escapes and that I wasn't receiving any radiation. There was no danger to me so long as the box remained sealed because the tritium radiation is very weak and would not have penetrated through the gloves I was using or the glass screen of the box. So long as none escaped, then I was quite safe. It's now been 40 years and there's no obvious sign except I don't have as much hair as I used to!

SM: Were there not audible Geiger counter noises?

VM: No, I can't remember how it was done. There was something either visible or audible. I think there was a chart recorder which traced something that one could see immediately if anything went wrong.

One of the problems was that the amount of tritium which actually got incorporated into the materials in which we were interested was, of course, a very tiny proportion of the stuff that we introduced. Part of the technique, an essential part of the technique that we used was to evaporate all the water away when we finally spotted the material onto the chromatogram. In the course of that evaporation all this radioactivity would be evaporated, too and we had to get rid of essentially all of the 5 curies. This could not be released in any normal way. I have to say that the carbonate that we used in our C^{14} experiments I think just went into drums or large bottles for disposal, which were sitting around the lab. I can't really remember what we did with them. I don't think we threw it down the sink; that wasn't permitted. We had collection bottles. This tritium was much more dangerous. It was in water which, of course, evaporates and so had we just put it in bottles of that sort it would have evaporated into the lab. and that would have been very dangerous. So all the material that came out, all the tritiated water that had been removed from the samples, was distilled out in an evaporating set-up and collected in a cold finger, cooled by dry ice...

SM: A cold finger?

VM: Yes, a glass vessel, sitting in dry ice which was very cold and, of course, froze out all the water that was distilled off. We collected it in this dry ice trap and then I remember we had designed this cold finger in such a way that it could be disconnected from the distillation line, sealed inside the box, and then we put it...The finger also contained vermiculite which is some mineral material which is good at absorbing water. So we finished up with this cold finger, sealed, containing the tritiated water in vermiculite, and that was then placed in a steel cylinder with a screw cap. It was only then that I was allowed to pass it out of the box with great checks on the monitor as we actually opened the lid or opened the vent and took this stuff out, this sealed vessel out. The vessel, I think, was encased in concrete together with other so-called low-energy waste and dumped at sea. Once I had got this pipe out of the box I was finished with it.

SM: From the way you describe the security...

VM: Safety.

SM: ...the safety measures that were taken with regard to this kind of experiment, it seems that they were far more serious than your earlier rather casual remarks about possibly throwing things down the sink. So I am sure that nothing was thrown down the sink.

VM: No, I don't think things were thrown down the sink, but I think they were kept, as I said, in storage bottles of some sort for disposal. But people were not terribly worried about the ordinary amounts of radioactivity we used around the lab. People were

careful but not manic about it. But when it came to these quantities of tritium, then there was a totally different response and it was quite clear that I could only do it if I were properly supervised and proper safety measures were taken which, of course, was very sensible.

So we got a paper out of that which was somewhat interesting, it wasn't that fascinating, but it showed something. I remember that on our way back across America, when we got to the East Coast we went to visit Racker, Efraim Racker in his lab. in the New York Public Health Service at the foot of East 14th Street (everybody remembered this address: the foot of East 14th Street!) and he was interested in these sorts of things which is why I went to see him. I think we were bantering one another over lunch as to how much radioactivity we had used in our experiments and he told me how much phosphorus he had used in some of his and I told him how much tritium I had used in some of mine. I think I won that particular contest.

SM: How is he?

VM: How is he? I'm not sure I have ever seen him since then. I'm not sure that he is still alive but I don't think he died young.

The last thing that I want to say in this context was nothing that actually came off but it was a good story.

SM: You mean succeeded?

VM: It was never put into practice for reasons which I will describe. That was the question of nitrogen. Although nitrogen is not directly involved in photosynthesis it is, of course, an essential element in all subsequent metabolism involving amino acids and all the other things. Nitrogen, of course, has no convenient radioactive isotope to use in the same way that one can use carbon. The radioactive isotopes of nitrogen all have very short (*half*-)lives and there are only some heavy isotopes, one in particular (N^{15}), which people use extensively. However, the use of heavy isotopes as tracers is much, much more cumbersome than the use of radioactive ones because it is not easy to measure their presence or their quantity; you have to perform much more elaborate analyses. You certainly had to in those days.

So we conceived the idea of possibly going around the back door and the tip for this actually came from something that I remember Andy Benson had done which I heard about (maybe he had published it at that time and I had read about it). He had been looking for phosphorus compounds by irradiating normal phosphate compounds in a reactor, or near a reactor, and getting, I think, an n-gamma reaction which converted P^{31}, the ordinary form of phosphorus, into P^{32}, the radioactive form without altering the compound in which the phosphorus was located. It was, as it were: he tickled the compound and the phosphorus in it became radioactive and therefore he could see which compounds contained phosphorus and do on.

Working from that idea, and I think this now must have been rather later — this was probably in the early sixties, I can't quite remember when it was — we came up, I and somebody else — and I can not for the life of me remember who it was but the sort of person it might well have been was Karl Lonberg because he and I would think up schemes of this sort — we were tipped off by the embarrassment the Atomic Energy Commission was having at that time in its bomb testing programme. They were very anxious to show that their activities were also directed to peaceful uses of atomic energy which was, indeed, part of their remit. I think they had solicited

experiments from people which could be used in association with the bomb tests they were doing. That is to say, they were going to blow off bombs, wherever they did this, to suit their own purposes and they invited other scientists to contribute experiments which could be placed in suitable locations with respect to this blowing off bombs.

SM: You mean experiments which were physically located near the explosion of bombs in order to see a reaction as a result of the blowing up of the bombs?

VM: Exactly, to use the radiation which came off the bomb, or the compression wave, or one of those factors for something other than just the bomb tests, for some scientific purpose. We conceived an idea; you may not know this, but I'll tell you. Carbon-14 in the first instance is made by neutron bombardment of nitrogen-14 and the neutron is absorbed and a proton emitted, and nitrogen-14 gets converted to carbon-14. Atomic bombs emit a large number of neutrons. So we had this idea that if we grew cells totally on N^{15}, the heavy isotope, which would not be toxic to them and then used N^{14} as a tracer, if we could so arrange things that the results of the analyses were radiated by neutrons from the atomic bomb, all the N^{14} would be converted to C^{14} which would be radioactive and we would then be able to find it on the chromatograms. We were very skilled at finding things on chromatograms and identifying them when we'd done so. That was the idea, to use the (*atomic*) bomb for this purpose.

We knew from Andy Benson's work that the paper that we used in chromatography was not very tolerant to high levels of radiation by neutrons and would crumble. We realised we couldn't use paper for this but would have to use something else. The other material which came to mind was to use silica gel spread onto glass. Of course, the glass would be OK and the silica gel, as an inorganic material, would also survive. The idea, then, was to run chromatograms on these silica gel plates, big plates if necessary, and then put them in a suitable position to be irradiated by the bomb blasts.

So the next thing to find out was what was the flux — let me put the thing in the right order, how close could we get a sample to the bomb? was one question. With that answer, what was the flux of neutrons going to be like at that point? and, therefore, could we expect to get sufficient conversion? The third answer was what would the shock wave be like at that point on our sample? There was great reluctance to give us this information. I suppose it came under some element of secrecy. Eventually, they told us that we could actually get to within 700 yards of the bomb, that was the closest, and they told us what the flux would be, and that it was OK, it was enough. It would have given us a reasonable result. When they told us what the effect of the shock wave would be like, it would be like hitting this glass plate with a sledge hammer. So, we realised we couldn't do that.

Then we fell back onto another idea. At the Atomic Energy Establishment at Hanford River, I think in Washington, there was a reactor which had — I don't know exactly what this was like — but it had somehow ports into which you could put samples which could be lowered into the depths of the reactor. There you could leave them for a protracted period and they would get irradiated. Apparently, the cumulative flux there would have been sufficient over the course of days or weeks or whatever to give us the effect we wanted. You realise the flux with the bomb is an instant flux — it happens and then it is finished. In the reactor, it just keeps going on at some rate, obviously less than the bomb, but you leave it (*the sample*) for a long time. So we inquired about the possibility of doing this at Hanford River. These ports were, I think, 6 inches in diameter so that we would have had to have miniaturised things compared with the way we normally did them because we ran our chromatograms on

sheets of paper which were about 18 inches by 24. That didn't matter; we could envisage doing this thing on small sheets of glass. There would be no shock wave, no physical hazard to the material. The only problem was to gain access to the reactor. It didn't sound quite as attractive as the bomb one. We were looking forward to writing a paper in one of the scientific journals in which the method section would say "take one atomic bomb...". That sort of thing had not appeared in biochemical journals before. Anyhow, we were prepared to do this with the Hanford reactor but we couldn't get anywhere near it. There was a guy called Admiral Rickover (*Hyman G. Rickover*) who apparently totally monopolised these ports for use on experiments intended for nuclear submarines. We just never got anywhere near it. It remained a story of the sorts of ways in which we would think and it was one of the many things that never saw the light of day.

One of the other things, I think, that I was involved in that first period...

SM: We're back in the first time?

VM: I'm sorry; the bomb thing might have been later, as I said. In the '56-'58 period, another thing I was involved in, together with almost everybody else in he lab. as far as I remember, was preparing a display for the Brussels World Fair of 1958. The bit that I was involved in, in particular, was the photosynthesis display. What that consisted of was a series of painted, back-illuminated panels showing in progressive enlargement the photosynthetic apparatus. That is to say, it would start with a leaf, go to a cell, from a cell to a chloroplast, from a chloroplast to the components within the chloroplast. There was a series of painted panels which for a very long time actually lived in the hallways of the round building after it was built and apparently were taken away only in the 1970s when the storeroom was turned into a lab.; the storeroom in that building was turned into a lab. so the whole corridor became littered with store cupboards and they had to take away these wall-mounted panels which were obstructing...

SM: Are you about to talk about the Brussels exhibition and what was sent in?

VM: I am, this is all the Brussels exhibition, yes. So there were these panels and the second item was a very large working model of the photosynthesis cycle. I can't quite remember how big it was but at a guess it was 10 or 12 feet square. It was a large panel, translucent plastic panel, which had the chemistry of the photosynthetic reduction cycle marked on it and behind it was a series of lights, worked by microswitches, which represented the introduction of radioactive carbon dioxide and the lights showed how each atom within the various compounds of the cycle progressively became more and more radioactive. When this thing had gone around the appropriate number of times, the idea was that a sugar cube would be delivered to the audience from one corner of the thing. On this sugar cube would say: "This sugar cube represents the amount of light which falls on so many square meters of land over such and such a period". A lot of time was spent putting this thing together and I can't be sure that I ever actually saw the thing working. It was sent to Brussels and was on display, and I think it was working. Paul Hayes was the one who was the lab. representative in Brussels. He spent a lot of time getting the stuff there and he was in attendance in Brussels throughout the exhibition. I never saw it in Brussels. It came back after the exhibition was over and, again, was mounted — I don't know where it was mounted to start with — I think in LSB; but later it was against the wall by the back door of the round building, for many years until finally it went away.

It never worked properly when it came back. The system was based on a series of microswitches; it was before the days of electronic switching. Those microswitches never worked properly and I think the sugar cube was never actually delivered at any time because something broke down in the negotiations with the sugar company (C&H I think it was, but I'm not quite sure).

One of the things I had to do was to help write the brochure, our bit of the brochure, that went together with the exhibits, and this brochure was written in English, I remember.

SM: What was the Brussels fair?

VM: It was a world fair; it was one of those fairs that are put on from time to time which show all the world's industry and technology and culture, like the 1851 or the New York fair of 1919 or the San Francisco fair of 1939. They have these things every now and again.

Our bit of it was, of course, written in English. It was then apparently translated into Flemish and edited in Belgium in Flemish and then retranslated into English for our comments and at that point I think I went to town on it a second time. I'd originally written at least much of it and the editing in Flemish had really knocked hell out of it. I never went there, as I said, didn't see the exhibition. At some time later, with great delight, I remember receiving a certificate from the US State Department — remember I was not an American citizen at that time — and the certificate, which I still have somewhere, says something, thanks me for my help for the Brussels World's Fair and it says that citizens like me that make our country great; words to that effect. I was very pleased to get that.

SM: Right. As a matter of interest and curiosity you talked about a number of papers which were produced with your name on them during the '56-'58 period. Do you remember, by any chance, roughly how many there were?

VM: Well yes, I said, I think there were 17, but three of those, I think, might have been conference reports, that is to say, preliminary presentation of data which then appeared as a paper so they would be duplicates.

SM: The sheer quantity would seem to me to reflect the amount of activity that went on in the group and the amount of collaboration that went on because clearly these were working in different directions with different people over the period.

VM: Yes, you are right. There were, of course, common themes, but you are right. That does reflect, and also reflects how much more one can do in the second year if one has two years. I remember thinking at the end of one year that I would have had only about three papers and so much was started which could not have been finished in one year but could be finished in two.

SM: Going back to the earlier period, in order that we can conclude that and go further, you mention something about relationships between individuals and the informality of the group.

VM: Everybody was on first name terms within the group except Calvin. Calvin was always called "Dr. Calvin" or "Professor Calvin" by absolutely everybody except his wife who always referred to him as "Melvin". When she talked about him she always referred to him in that way.

SM: Hopefully when addressing him she didn't call him "Dr. Calvin" either!

VM: I don't remember what she called him. "Honey", I think she called him. She called him "honey" and he called her "Babe"...

SM: Right.

VM: ...as I remember. But nobody ever called Calvin by his first name. Within the lab., as I said, with one minor exception, and that was, of course, among the Germans, there were in our period in 1956 three or four sets of Germans (husbands and wives in some cases) in the lab. and there was a hierarchy between them which was not reflected anywhere else. We were particularly friendly with one of these German postdocs. who was not the most senior among them and we were very impressed that while everybody called everybody else by their first names, he referred to his colleague as "Sie"..

SM: ...which is the formal mode of address in German.

VM: Right. They themselves were very friendly, they spent all of their time together, because they came from the same place. I asked our German friend why did he not call his colleague "Du". He said in the nature of things his colleague was the senior and until he was invited to do so by the colleague he couldn't do so. Apart from that, everybody else was on a first name terms.

SM: Is this in any way in contrast to experience in British labs.?

VM: In British labs. fortunately...When I did my PhD, actually we did call everybody by our first names except the professor. Everybody called him "professor" or "prof". When I went back to England in 1971, I was — I don't know quite what the word is — I was embarrassed and I found it hilarious that all the secretaries would insist on calling me "prof". It was only after much trouble, much persuasion that I got any of them to call me "Vivian" which eventually they did. One of them, whom I have known now since 1975 (21 years) since she was about in her early twenties, still calls me "prof"; she can't be persuaded to change.

Anyway, this all went on in Calvin's lab. without change until I would say, late in the sixties. By that time Mel Klein and I (Mel Klein had joined the group in 1963, I think), Mel Klein and I decided that we would put an end to this and we would call him Melvin; we had known him long enough. We wondered whether there would be any reaction. There was none at all. I don't know whether he noticed the fact that we called him Melvin but we suddenly one day decided from now on we would call him Melvin and we just did; and there was no response.

SM: As I remember, Dr. Calvin himself was completely informal in his conversation with everybody.

VM: I think he might very well have been oblivious of this or maybe he felt that that's what they wanted to do, let them get on with it. He simply didn't react when we changed this. Now, another 20-30 years on, I think everybody now calls him Melvin. All the people he has known over those years call him Melvin. But it took them a very long time to do so.

SM: Is there anything else, during the earlier period, that you cant to cover, because to have some form of chronology seems to make some sort of sense....

VM: There are one or two things. There is some continuity; they run on a bit into the next thing...One of the things I would like to comment on was Calvin's lecturing style which I first met when I heard him talk in London in the autumn of 1955, as I think I mentioned earlier on. He was a very enthusiastic ebullient lecturer, very informal. He had a lot of good stuff to present. The audience wanted to hear what he had to say and, I guess, he knew that and he acted in accordance with that. Instead of standing behind the rostrum or behind the lab. bench, or whatever it may have been, he would tend to come round to the front of it, as I remember, and sit on the front of the bench and talk off the cuff — he never had any notes and he would use the slides as prompts, as most scientists do anyway. He would sit there and he would be showing the slides and waving his arms around and explaining in his enthusiastic way how it had happened, what it all meant, and so on. The only trouble with that was that he lost all track of time and he would just go on and on and on, and suddenly he would look at his watch or the clock and realise how late in his allotted time he was. Then he would ruin, or partly ruin, everything by gabbling through what he felt had to be said of important results which he hadn't yet got too. People recognised that the quality of the lecture would change about ten minutes before closing time as Melvin got to that point.

SM: Would it be out of the way to jump ahead a little bit, just on this particular topic, to his preparation of his Nobel Prize address.

VM: Indeed; indeed it would. When he was awarded the Nobel Prize in 1961 he was told, I understand, that he had to give a lecture in 40 minutes and that it was as very bad form to exceed that time. This was something which he had never done before. What he did was to work out a lecture that he was going to give and then Paul Hayes and I edited it for him to make sure that we cut out redundancies in order to keep him down to 40 minutes. We timed him bit by bit and he arranged with Gen, his wife, that she would be sitting in the projection booth with a script so that he didn't even have to ask for next slide but she would know when it was due and she would show it, which would save him a few seconds here and there. So, he gave this lecture in Stockholm from his script, as far as I know, and got it through in 40 minutes. I'm afraid in some ways it ruined his subsequent lecturing (*style*) because then he became addicted — not addicted, he from time to time at any rate, maybe often, used a script. There was all that spontaneity and ebullience that he had in his earlier years had really largely gone after that. The Nobel Prize exacts a high price in some regard.

SM: We've jumped ahead on that, because you were talking about the manner of his lecturing. Were you also going to talk about the way in which he approached the writing of papers?

VM: Not just the way he approached the writing of papers. It's the style of writing papers in the lab. By the time I got there, I suppose, this was well established. Many of these papers came out with many names on them because of the number of people participating in the projects. The agreement was that the person who wrote the paper had their name on first. And usually, not always, usually Calvin's name was last and the rest of the names were on alphabetically. I suppose there might have been variants in that but that was the general style of things. It certainly happened that when I wrote papers myself, when I was the first author, that is to say when I wrote the bulk of the paper, which meant that I had done the bulk of the work — I think the person who did the work wrote the paper. That's the way it went. Sometimes all the names after the

first a author were alphabetical. As I remember there was never any argument about this. It was an accepted way; everybody thought it was reasonable. Nobody seemed to object to having their names included with lots of others. Everybody wanted their papers published with Calvin. These were all youngish people who were in fairly early stages of their career — I suppose Andy Benson would have been older but he had gone by the time I got there — and so they were pleased to have Calvin's name associated with theirs and saw it was a plus for them. Not every one of my papers was published with Calvin in those early days but most of them were. There were one or two in which he really played no part or in which he had no particular interest — for example, the fungus that I had done my PhD on in London was something I brought with me and I did some work on that as well and that was not something that Calvin was concerned with. So that was published just by me as far as I remember.

In the course of writing these papers, I should say that Calvin was not a passive partner. He would argue over every comma. He was very argumentative over some bits of the paper which he saw as being most important or not to his taste. He was not a sleeping partner, he was a contributor, had often contributed, I would say, usually contributed in the formative ideas, had certainly contributed in the detailed discussion of the results as they came through and also in the discussion of the paper, of the written draft as that was being produced. His name was properly on the papers and the fact that he may have produced in his life 600 papers or so represents contributions that he actually made. He was not the sort of person who would put his name on simply because he was the head of the lab. That wasn't his style.

SM: Again, from your first impressions in the earlier period, I can remember your reaction to the eight o'clock Friday morning seminars. Can you tell us something about that?

VM: The first reaction was that it was so early in the morning! I had come from a culture where, as far as I can remember, one got into work in time for coffee which was something between 10:00 and 10:30. True enough, it was in a city where there were commuting problems, but nevertheless, the idea of eight o'clock in the morning was foreign to me and also to all the other English in particular, of whom there were a fair number in the lab. at that time. Nevertheless, Calvin insisted on it. It was actually ten past eight in the morning that we started. I don't think I need to go into the style of the seminars. Many of the people that have contributed to this oral history have mentioned them. Calvin always sat in the same place, at the end of the table; he always interrupted early in the seminar — some other people also interrupted but not the way he did.

SM: Did you find yourself intimidated in the way that some people seem to have?

VM: I don't remember being intimidated, but then, he was not...I was about to say he was not aggressive. Perhaps that's not the word to use. He was not attempting to put you down, he was asking questions which he genuinely felt ought to be answered or at least he wanted to know the answers. If you said "I picked up the test tube" and he said "what sort of test tube?" it was because he wanted to know what sort of test tube you were talking about He was not suggesting that you should have been picking up something else. It was constructive criticism. But I suppose people felt it intimidating because he did it so much. His style, I suppose, was a bit aggressive in the way he asked. And the fact that he started so quickly: as soon as somebody opened their mouth, usually within the first sentence or two he started.

One of the problems with the seminars was that they often went on much too long. They were supposed to be from eight to nine but sometimes he got very wound up and really wouldn't let them go.

SM: Who took part in these seminars?

VM: Everybody.

SM: By everybody, you mean from the Donner group in addition to the ORL group at that time.

VM: That's right, and Calvin and all his senior staff and all the postdocs. and all the students and the technicians. Everybody except secretaries and support staff took part in the seminars — were expected to be there and people came; I don't think people failed to come to the seminars. These things would often drag on, well after nine o'clock, and the thing would go on and on and on, and people became more and more restless with Calvin's nagging sometimes at some of the abstruse points. But it was very difficult to get out. The people who could get out were those students who had classes at nine o'clock. You remember, many of the graduate students in American universities take courses and some of them would have course lectures at nine o'clock; they could legitimately leave. Every now and again more of the more senior people would slip out in the general melee in order to get out of this if they could see it happening. It was a problem and it did go on often and people got fed up with it.

SM: Am I correct in remembering that the way that Dr. Calvin felt about this situation as in every other situation was "we are all here to learn", can there ever be enough time spent learning?

VM: Yes, that's true. What he wasn't sensitive to was other people's needs to learn. If he needed to learn something, he presumed everybody else was interested in it. He never, that I remember, adopted the approach, "I'd like to pursue this with you when it's finished". He probably did but I don't remember it. He would pursue it there and then, in the seminar, and keep everybody else around while he did it. So, he was not sensitive to the fact that some people might want to get out.

SM: Instant mental gratification.

VM: Yes; I think that's a fair way of putting it.

SM: Have you now exhausted this earlier period so that we can get on to where we had chronologically left the interview last time, which was your return in 1960. Of course, the most exciting thing about that period early on was Dr. Calvin's having been awarded the Nobel Prize for Chemistry.

VM: No that's not the earlier period, that's the later period; that's 1961.

SM: Yes: I said "on your return". So you came back and, I think we had spoken about this earlier, to find that the group had been physically fragmented because it had been necessary to leave ORL because of building work on campus or what have you and so the inmates of ORL had gone to the Life Sciences Building.

(Tape turned over)

SM: Right: so the group had been physically fragmented — well not any more than before — but into different places.

VM: It was not more fragmented than it had been. It was simply that the ORL contingent was moved bodily to Life Sciences when I was not there. That must have been some time in 1959 when I was not in Berkeley. So when I came back, that's where they were, physically much further away and under much less pleasant circumstances along a corridor — I think I mentioned this — along a corridor in separate rooms to a considerable extent although there were one or two big labs. The atmosphere was totally different.

SM: And plans for a new building has already begun, had they not?

VM: I think they had already begun by then and I remember being party to the discussion. The plans had not been laid by the time I came back in 1960 so that the form of the new building was still very much up for discussion. I suppose that the first things that had to happen, and I was not aware of this, was the raising of the money and other people have talked about where this came from and how they did it. So I think until the money came, there was no point in serious architectural designing of the building. By some time in late 1960 or early 1961, I suppose, and perhaps even before then — I can't really remember when it started; it will be in the records, of course — there was the planning for the new building. Everybody had been very impressed with ORL, not literally for its properties, it was a crumbly old building, but the fact that it had been so open and the fact that people had been placed together so effectively.

SM: And encouraged to collaborate visually and actually.

VM: People felt that the building had played some significant role in that. It wasn't just the people but it was the way the people were physically dispersed with respect to one another which encouraged that sort of activity. People in the lab. felt that was a significant fact also in the scientific success of the group. They didn't want to lose it. They saw dramatically what a difference it made when they moved to LSB and they had earlier seen, of course, what a difference there had been between ORL and Donner. Donner was a building based on corridors. It was quite clear that the atmosphere in the two places was different.

Various people have said that in Donner they worked in rooms and stayed in rooms. In ORL there wasn't that; you moved freely around the building, you weren't conscious of being placed in rooms. So when it came to the opportunity of having a new building, naturally the thoughts turned to how do you reconcile the design for a new building with what had been perceived as being so successful in ORL. From that grew the idea that we ought to have large labs. The main working areas ought to be large and ought to be open, allowing maximum communication. At the same time, of course, it was recognised that there were other activities like smelly things, or noisy things or whatever which really had to be kept in separate rooms. One needed some combination of large open spaces plus small protected areas for chromatography and centrifuges and other sorts of things.

I don't know at which stage the idea of a semi-circular working lab. arose. I don't know whose idea it was. I certainly don't remember being present when it first came up. The idea did arise that the labs. would be of that form, semi-circular, with the lab. benches arranged like the spokes of a wheel around the periphery, around the wall. Everybody would have, as they had had in ORL, a lab. bench with a chemical rack

behind them and a writing desk at the far end with a maximum degree of privacy; the far end against the wall. Within that...

SM: Far end against the window, I think.

VM: ...against the window wall, against the external wall; there would be some windows and some not. There was some discussion about how big a window you could have to do with the heating, lighting, air conditioning, etc. At the inner end of the benches there would be a space for a walkway — no wall, but a walkway — and then there would be equipment which would be common equipment used by the people who were working at the various benches — things like spectrophotometers, instruments, balances, all sorts of instruments — would be placed in this inner area. In the very middle of the room there would be the big white table, at least on one floor there would be the big white table; that would be the focus. As there were to be two floors like that, the lower floor had a circular coffee table. In fact, the second floor coffee table became the social centre of the building. The big white table became the scientific centre for the third floor only at that point.

SM: Once you were in the round building, of course, both groups were together. The Donner group was there as well as the group which had been in ORL and subsequently in LSB. Did they work discretely? Was there one (*group*) on one floor and one on the other? How did it work?

VM: To a degree there was. What happened was that the staff members...I should say that at each end of the arc containing the lab. benches around the semi-circular labs, were two glassed-in offices. Those were to be the homes of the staff people, people like me, Dick Lemmon, Al Bassham, Ed Bennett and all the others; we each had one of these glassed-in offices. There were to be eight of those, four on each of the upper two floors and, naturally, each of us had our own group of activities near our offices on the same floor as our offices. The original idea was that people would be all jumbled up, at least within a floor and maybe within the two floors; can't remember that.

SM: What do you mean by people being "jumbled up"?

VM: That's to say, people in my group would be scattered anywhere on the semi-circle, not necessarily next to other people in my group.

SM: Do you mean the animal people might be next to the photosynthesis people, too?

VM: Yes, certainly my people and Al Bassham's people, and I can't remember who else were on that floor, would all be mixed up, in order deliberately not to separate them. In practice, that tended to die away over the course of the years and we tended to occupy blocks. But, they were open blocks, that is to say there were no physical barriers and there was talking back and forth and movement back and forth across the room. So it wasn't much of a barrier.

SM: You mention the semi-circular lab, but you didn't actually mention that this was part of a circular building which might not have been the case.

VM: Just a minute; that's right. The original design was for the semi-circular labs., and where the other "buildings" (*should be "facilities"*) were to be placed was not so decided. The original design, which I remember Al Bassham came up with (I think he came up with), was to have a semi-circle on a block and the block would be

conventional rooms for the service facilities. The architects felt that would be too fussy for a building of the size this one was to be and it was they, I think, Michael Goodman the architect, who suggested it should be completely round and that would also then solve the problem of not having to worry about which way it was sited.

SM: Do you know, or have you ever known, what the diameter of the building is?

VM: I have known and I don't know. I think it is a 24,000 square feet. I think it is 30,000 square feet gross and 24,000 square feet net on three floors which would enable one to work it out but I'm not going to do it right now (*i.e. about 110 feet diameter*).

This last point, that the architects felt that a circular building could be sited — you didn't have to consider which way you were going to twist it — and therefore the access road and all the rest of it could be built for maximal convenience for other considerations. It didn't matter where you put the front door and the back door whereas on a block you would have more restrictions. So that's how it was done. All the other rooms around the edges were service rooms.

The ground floor was never like that. The ground floor had a central storeroom, which was very large, and all the labs. were in separate rooms around the periphery. The ground floor was offices and physical equipment for physics and for some reason, which I don't know at this stage, the physicists did not want big labs.; they wanted smaller rooms.

SM: I think the reason for physics being on the ground floor is that often the machinery is very heavy.

VM: That might have been a contributory factor but I think that the...I don't really remember what the argument was for what you put on each floor.

So, the building was constructed on that basis and, I think, it actually worked very well. It was clearly impossible to rebuild ORL in its original guise somewhere else — no one was going to build an old wooden building at that stage — the building had to be a modern building and I think that the compromise between modern design and modern construction and the openness of ORL was achieved very successfully. But there were limitations. I should say first of all that in the construction of the building itself, I think, I have only ever spotted two faults in that building from the point of view of the original users. One of them was that the lift (*elevator*) that was installed was a hydraulic lift which had been terribly slow; a hydraulic lift operating by a ram from below rather than from cables from above; it always crawled up and down. The other thing: we forgot to put floor drains in the cold rooms, as I remember, but apart from that it worked very well.

However, seeing what has happened to that building since the Calvin group stopped being the Calvin group, has shown that it's actually not as flexible as one would like. In its present form, where it is occupied by a number of pretty discrete groups, there has been a tendency to erect barriers between the groups on the various floors, barriers made by pieces of equipment and cupboards all over the places and big refrigerators. The place now looks a mess and it certainly doesn't fulfil the function that it was originally designed for.

SM: The ethos of the group is not as it was and it's not a group. You have just said it. It's several groups and they have sought to separate themselves.

OK. Let's get back to where we were. Talking about the new building; the new building is opened, it's working, and you feel that it worked well. On your return, you were a staff member rather than being a postdoc. for a limited period. Did this make a great deal of difference to the manner of your working or your responsibilities or how you felt about things?

VM: It made one big difference which I was very conscious of from the very beginning and that was that I had a job which was permanent, or at least open-ended and, therefore, I wasn't so conscious of having to start lots of things in order to get lots and lots of papers in a short period of time which I was certainly alert to in the first period. I could then devote myself to a much longer-term development of ideas, such ideas as I might have had. That's indeed what happened. I never again worked on photosynthesis after 1958. I was aware of what was going on because Al Bassham was in the same room. Even in LSB I knew what he was doing and then in the round building he was on the same floor, and so I was always aware more or less of what was going on but never did it myself. I was embarked on a number of other things, which are not really terribly relevant to this discussion, but which then I was able to pursue over the long term with a group of my own, more or less, in the way I have described — postdocs. and students who chose to work with me over the eleven years that I spent there.

SM: You have sought in the whole of this oral history project to cover the period from the beginning of the work of the photosynthesis group to the operation of the round building as a new lab. Obviously no small part of this during the very end period was the award of Dr. Calvin's Nobel Prize. Would you like to say something about the events and the feeling around that time.

VM: As far as I remember, there was talk every year in the late fifties and early sixties that he might get it. I wasn't very conscious of Nobel Prizery at that time but apparently there were lots of rumours always flying around as to who was going to get it. It was said that Calvin was a distinct possibility and it was also said that every year that it was announced he didn't get it he showed signs of disappointment. In the end, he did get it and obviously we were all very pleased and he was very pleased. I think everybody agreed that he regarded it as commendation for the group as a whole and not just for himself. He was the representative of lots of other people. As you can imagine, there was lots of partying that went on, apart from the preparation for the speech, which I have already talked about. There was a big party at his house, I remember, and I think it was on that occasion that a cake in the shape of the new building first made its appearance. I think there are napkins still around, one or two preserved, which have the design of the new building, the shape of the new building, drawn in the corner.

SM: As I remember myself, this sort of occasion was very much participated in, initiated by Genevieve as well as Melvin Calvin, so I have myself very warm memories of these particular happenings. Not only was everybody made to feel a part of it, so were there families. On the Calvins' return from Stockholm, as I remember, a gift was brought to each lab. member...

VM: Well, at least to us; I presume to others as well. The party I remember was at his house and it was a large party with lots of people. I think also that it was the only occasion on which we ever met his mother who came up from Los Angeles for the occasion. A sprightly lady at the time.

SM: We had always, from the very first time we were in Berkeley, known Genevieve's mother who lived together with them...

VM: ...in a cottage on the same property. When Calvin got his Nobel Prize, the University arranged a press conference for him in one of the University buildings to which I went. It was a bit formal and it was literally for the press, the local newspapers, I suppose, and the nationals and television people were there. That's the first time I heard him tell the story about how he thought of PGA. It's his story and he has reported it elsewhere. It goes that he was with his wife in the car and she had gone into what was then a frozen food supply store on the corner of Cedar and Grove (now called Martin Luther King Jr. Blvd.) and the car was parked in the red zone. While he was sitting in the car waiting for her to come out, he realised the significance of what he'd got and that PGA was the answer to it. That's the only story I know in relation to how PGA was dreamed up. He has told it often enough so he believes it.

SM: We've talked about the group during the first period that you were here and also after your return in the new building. You have talked about how the building worked as compared with the atmosphere in ORL. Is there anything you would like to talk about before we come to your ultimate decision to leave Berkeley in '71.

VM: Only a couple of things. One was that this group was always supported by the Atomic Energy Commission, paid for by them, and was always subject to some of their security considerations. I think that people from outside the group did not have free access to it. I'm not sure about this, but I think it was not easy for people simply to walk into the place. I think there was always some implied limitation on who could gain access to it. I know that people who worked in it had to have some sort of clearance. It couldn't have been very elaborate because there were lots of foreigners there who were not subject to American clearance procedures of the more stringent variety. But I think many of the Americans had what was called "Q" clearance which was some level of clearance.

SM: A minimal level.

VM: I don't know how minimal it was but it was a factor in the place. The foreigners were in some way vetted. I vaguely remember that there was always difficulty getting people in from Communist countries. There were one or two people who were to have come or, in fact, did come and I think it was much more complicated to get agreement for them to come. I remember Zofia Kasprzyk from Poland, who was there I think in the period I was fifty... around that period. And then there was Nekrassov...

SM: Fifty what?

VM: Fifty eight, I think; I can't remember exactly when she was there. (*Lev*) Nekrassov who was there in the early sixties I think also, was, of course, from Russia. It was more elaborate to get agreement for those people to come in.

My contact with security apart from that and getting clearance was very minimal. There were two incidents I remember. One of them was during the period we spent in Life Sciences Building: I remember one afternoon one of the security people arrived in our part of the building and Lynn DuBois, who was then the secretary there, called me as being the senior member of staff who happened to be around at the moment; I guess Al Bassham was out. What this man had come to see was whether I could give him any background information on Marilyn Taylor whose security was coming up for review. I pointed out to him that first of all I had nothing whatsoever to say

against Marilyn's presumed loyalty to the United States. I did point out, however, that I was not an American citizen but he seemed to think that made no difference: as long as I answered the question properly that was the only thing he was concerned about.

SM: There was one thing which I don't know whether you recall and this was when you were about to go to Russia.

VM: Yes, I was about to say that. When I was about to go to Russia to the cancer conference in 1962, which was being paid for, incidentally, by AEC funding, I was solemnly warned by the security people about all the temptations which could come my way when I went to Russia and how I must be careful not to fall for any of them.

SM: Not to be lured by the young men, women, or anything else.

VM: They didn't mention men, they mentioned only women. This was before men were thought of in that context in this part of the world. They warned me about being lured by young women. But I had to say that there wasn't a young woman in sight that was lure-worthy while I was in Russia, as I remember.

SM: Or even seeking to lure.

VM: When I came back from Russia on that occasion, I think they had asked me to write a report about my visit; they'd asked me that in advance or they told me they would be asking me. They never did; I don't think I ever wrote a report about the visit. They never showed the slightest bit of interest; they never came back to me and asked me anything.

SM: Weren't you also warned against possible approaches to be asked to spy for the other side, as it were?

VM: I can't remember now.

SM: This was the Cold War period and I think I can remember that element.

We were about to discuss your decision to return to England which you did in 1971. How did this come about?

VM: From a personal point of view, I think there were obviously a number of factors. One of them, family-wise, was that our oldest son — son, oldest kid — was going to go to secondary school in 1971 or thereabouts and that we felt it would be unfair to move him from whichever secondary school he went to once he started. We began to feel that we would prefer him to have the sort of secondary education that we ourselves had had in England. For that, of course, we had to go back. The second factor was, I think, that I had become, for want of a better word, homesick and much as I enjoyed living in Berkeley in the end I decided really I'd rather live there for all sorts of cultural and identification reasons. Then, there was another factor. That was I was then in my early forties and still working as a formally untenured member of Calvin's group, as a senior staffer in the Radiation Lab.

SM: Although you were a research scientist, and this is what your particular appointment, was you had been doing some lecturing.

VM: I wanted the variety. I spent half my career, twenty years or more, essentially doing research all the time, except for a little bit of guest lecturing. I did an evening course

in biochemistry; I had done that for several years in Berkeley, but it wasn't very much lecturing. I felt really that I needed a change; I needed a change in order not to do the same thing for the second half of my career as I had done in the first. I thought at that point I would welcome the variety of an academic career and be able to do teaching and participate in the administrative decision-making and running of an institution; all the rest of those sorts of things that one wanted to do. I thought I would like to do that and therefore wanted a faculty job.

It was very difficulty to get a faculty job in Berkeley. There were only two people from Calvin's group who ever did that: one of them was Ken Sauer and the other was Rod Park. Nobody else ever succeeded in getting a faculty job in Berkeley. Al Bassham was an adjunct professor in biochemistry but not a full professor. And so, I realised that if I wanted to get a faculty job I would have to leave Berkeley. Looking out from Berkeley, I didn't want to live in the middle of the United States; looked towards the east coast and having looked that far somehow just over the spires of Boston appeared the spires of London and I was really tempted to go back home and in the end that's, of course, what I did. Simply to say that for the following 22 years, from 1971-1993, I was professor of microbiology at Queen Mary College in London and retired at 65, according to my contract, and in the last several years (about six years) I was also a co-founder and the science director of a small biotech. company called Archæus which was working in enhanced oil recovery and microbial methods for that. I became very involved in biotechnology towards the end.

SM: But you have been doing things subsequently. Your activity certainly hasn't ended with your nominal retirement.

VM: No. Subsequent to formal retirement from Queen Mary College I have been involved with various publishing, editing and book writing activities — a project on motivations for university biotechnologists to become involved with industry, involved in a human genetics groups and, of course, with this oral history. The oral history originally, you may remember, was proposed in 1970-1971 before we went back to England. But then, of course, we did go back to England and the idea arose again last year (that is to say, early in 1995) in a discussion we were having with a man in Bristol. As I was coming here (*to Berkeley*) only a month or so later I used that opportunity to talk about it to Calvin, Al Bassham, Marilyn Taylor and a number of other people — Andy Benson. They all agreed that they would participate. So, we spent several months getting the idea worked out, making contacts, securing our funding and are now doing it (*the interviewing*) in 1996.

SM: As I remember the original thought was that there should be an oral history of Dr. Calvin which, in fact, subsequently there was. The idea that there should be one of the whole group is something more recent.

VM: Last year when we talked about doing this present project, I first thought about it as a photosynthesis oral history. But it subsequently became clear that we really couldn't isolate the photosynthesis part easily and, instead, it has become an oral history of the Calvin group, the Donner as well as the ORL parts of it, in that early period. We recognised the 1945 and onwards period as being exciting, the beginning of this group. It had to stop somewhere in our story and we decided as a convenient place to end would be the opening of the new building, the move into the new building in November 1963. It's essentially (*the history of*) that 18 year stretch.

SM: You have also included a few people who were not part of the group but were part of the background to the work that was being done by the group.

VM: That's right. The last thing I might say is thinking of Calvin as a scientist. I have to say that he is the most remarkable scientist I have ever met, or at least met and known well; maybe some of the other people that I met and didn't know well were more remarkable but I didn't know about that. He was, as everybody said, quite outstanding in the fertility of his imagination, in the enthusiasm of his responses and in his ability to co-ordinate and stimulate and activate people. When we first knew him in 1956 and for many years afterwards, this was an overwhelming characteristic of him. It was also very clear in those early days that, apart from his science, he really had little understanding and little interest in other things and indeed appeared quite inexperienced in the affairs of man outside of science. That changed to a considerable degree after his Nobel Prize and, I think, perhaps one of the main factors was his appointment to Kennedy's President's Science Advisory Committee. He began to know the Washington scene. He also travelled a lot. In the early days, I think, he travelled only for conventional scientific purposes, to visit labs. and appear at conferences. I think later on he probably travelled in other respects as well, on a wider basis, and I think this must have broadened his horizons. He certainly became more interested in politics and other matters in the sixties and afterwards than he had done originally.

I remember indeed taking him home, I think, giving him a ride home, one day after he had just come back from a meeting in Washington and he was beginning to wonder whether perhaps he shouldn't give up the directorship of the lab. and devote himself wholly to that sort of activity. I remember discussing with him at the time that if he wanted to be a scientist, he simply had to have a scientific base and in my view he should not give up the lab. I never heard any more from him that he was even thinking of it. So I think he's; he was quite extraordinary. Unfortunately, he is now no longer able to do that. In his heyday, he was a most remarkable person and I think that part of the success of the group, a major part of the success of the group, undoubtedly has to accrue to him. He was, of course, also lucky and in the right place at the right time but another person would not have been able to rise to that occasion the way he did.

SM: In the sort of response he elicited from those who worked with him, apart from all the other things you have mentioned, what do you feel has been the overall effect of what might be called your own personal Calvin experience on your own professional and personal life?

VM: I would say it's major but it's really impossible to say what would have happened if I hadn't come (*to Berkeley*) because one can never say...

SM: But you did come.

VM: I did and it was a mind-blowing experience. It introduced me to America and to Calvin at the same time. It's a bit difficult to sort out altogether which was which. They were both very stimulating experiences and the fact of the matter is that as a result of them we subsequently immigrated into this country and at that stage intended to stay and indeed did stay for eleven years.

SM: In your professional life since leaving Berkeley, when you have been running your own group...

VM: I've certainly been influenced by Calvin in all sorts of ways, some of which I recognise and some of which I probably don't. I interrupt people at seminars the way

he used to — I probably got it from him. I'm not conscious of which properties I acquired from him but I am sure they are there. I could perhaps try and work them out but I have never done so.

SM: So that one might almost say that what might, as in a seminar situation, even be seen as abrasive qualities still come across as admirable in a sense.

VM: I think so. I think that the whole point about science is that you must not be afraid to ask questions. Clearly, there are social ways in which you ask questions to elicit responses from people — don't put their backs up. Calvin could put people's backs up and I think in some cases he probably did. But for the members of his group who were used to the way he operated, I think it was not offensive. But for strangers, it might well have been. I have to agree.

SM: One now has some sort of feeling of the effect that he has had on you personally and on your way of doing things. Just, finally, back to this oral history project of the group. We have talked about the sort of people who have been asked to participate. How did you put this project together? In what manner was it facilitated?

VM: I'm not quite sure...facilitated?

SM: You made approaches...

VM: OK; there were two sorts of things. First of all, I had to find the people to talk to. Marilyn Taylor kept very extensive records of people who had been in the lab. Many of the names were familiar to me: either I knew them personally or I knew of them, people who had been there before I was, knew of them from their papers. Many of the people were not difficult to find because Marilyn had the addresses. For some of the others, it was easy to chase them down through the scientific literature, and a couple of letters or e-mails would make contact. I must say that of all the people we have so far approached, by now it must be around 40, not a single one has declined to participate in this programme. They all have very warm feelings of the period and are all happy to say what they can.

In terms of support, I think we have been very fortunate in getting support for our expenses from the Chemistry Department in Berkeley; from the Chemical Heritage Foundation in Philadelphia; from The Royal Society in London; and from Gresham College in London. And, of course, we have also had the hospitality of the Office of the History of Science and Technology (*at the University of California*) in Berkeley and of the Bancroft Library who are going to assist with the archiving of this material. Everybody has been extremely helpful, co-operative and welcoming. It's going very well.

SM: Geographically, where will you be talking to people and where have you been talking to people?

VM: The biggest concentration of people has still been in Berkeley. We have been in La Jolla to talk to some and in Tucson, Arizona; to Mendocino (*California*). We are going up to Seattle to talk to a couple of people. There are visits to be made in Boulder (*Colorado*) and East Lansing (*Michigan*) on he way back across country and then in the Boston, Washington, and Richmond, Virginia areas. Subsequent to that, we have already seen some people in England, in various parts of England, and there are others to see and then within the next few months we need to go to France, Belgium, Holland, Germany and Switzerland, I think, in order to see maybe another

dozen people. That, of course, still won't get everybody. The chances of being able to get to Warsaw to see Zofia (*Kasprzyk*) are not very great but maybe she comes to Western Europe sometimes and we can see her.

SM: Fiftyish participants is not a bad representation.

VM: Fiftyish should about do it. There were, of course, hundreds of people through that lab. but it's just impossible to see so many of them. And many of them were later than the period in which we are working.

SM: The science, of course, was written at the time the work was done. As I understand it, the purpose of this oral history is to put on record the stories of the people who made the science possible.

VM: Yes; that's the way I see it, too.

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name VIVIAN MOSES

Date of birth 17 MAY 1928 Birthplace LONDON

Father's full name SOLOMON MOSES

Occupation DIAMOND POLISHER Birthplace POLAND

Mother's full name BESSIE BRODETSKY

Occupation SECRETARY Birthplace LONDON

Your spouse SHEILA SHINE

Occupation EDITOR Birthplace LONDON

Your children KEVIN – GENETICIST (UNIV. OF SOUTHERN CALIFORNIA)
SUSAN – DEVELOPMENT OFFICER (NATIONAL MUSEUM OF WALES)

Where did you grow up? LONDON (AND BEDFORD)

Present community LONDON

Education CAMBRIDGE (TO M.A.), LONDON (PH.D. AND D.Sc)

Occupation(s) PROFESSOR OF MICROBIOLOGY UNTIL RETIREMENT
NOW – VARIETY OF RESEARCH, WRITING AND EDITORIAL ACTIVITIES

Areas of expertise MOST RECENTLY – BIOTECHNOLOGY

Other interests or activities BIOTECHNOLOGY: MARKETING AND PERSONNEL MATTERS / CAREER DEVELOPMENT; HISTORY OF SCIENCE (ESP. BIO-ORGANIC CHEMISTRY GROUP, U.C. BERKELEY; GENETICS: Y CHROMOSOME IN POPULATION STUDIES

Organizations in which you are active
QMW, KCL AND UCL – ALL COLLEGES IN LONDON UNIVERSITY.

Chapter 18

MARIE (HEBERT) ALBERTI

Berkeley, California

June 16th, 1996

VM = Vivian Moses; MA = Marie Alberti; SM = Sheila Moses

VM: This is a discussion with Marie Alberti in the Round House in Berkeley on June 17th, 1996. Marie, let's start with you telling us how you came to join the group.

MA: I always liked chemistry and as I took classes at UCLA in college then I began to like biochemistry more. I took a physiology class; the fellow was just so enamoured with Calvin. He said "This is the most magnificent scientist of our time and it's such a privilege to read what he has done" and he kind of detailed what he had done. This would be in about 1957.

VM: Who was that who said that?

MA: I don't remember. He taught Physiology 101 which I took as an outside my major class.

VM: At UCLA?

MA: At UCLA.

VM: Might be able to go and look it up, then.

MA: Well, you'll probably find it in 1957. I kind of had in the back of my head that if I ever went to UC Berkeley that would be a person. I met (*indecipherable*), a fellow student who was going to do a PhD here at Cal. So I came up to look for work and, of course, applied at several places, mainly to the Calvin group, that was my most interesting thing. It turned out when I was here that I interviewed with Dick Lemmon, who had an opening with Ed Bennett. There wasn't really an opening in the Calvin group and I didn't realise that we were going to be physically separated or even what I was going to do. Ed Bennett never interviewed me because it was done through Dick Lemmon. They talked about brain chemistry and I thought that's biochemistry of a sort so I felt that would be interesting to start with; at least I'd be in the group of Calvin. So, that's what happened. I came up then in the spring of '59 and I came up and started work July 1st, 1959 in the Donner Lab. At that time ORL was already gone, it must have been just a few months gone, because they were just talking of the

knocking down of that area. And there was a door that was saved and people were having fond memories of this thing...

VM: Already!

MA: Yes...which was so new to me. The other group was down in the Life Sciences Building. I never got to see Calvin very much because he spent most of his time down in LSB or in the Chemistry Department, because we were a smaller group on the third floor of Donner Lab.

VM: So you never actually knew ORL itself apart from the photograph up there on the wall?

MA: No. I was happy to get those...I had those made, the big shot, for the (*Calvin*) reunion (*in 1989*) and was very happy to see what the thing actually looked like because I had never seen it.

VM: The people down in Life Sciences were the ones who had been in ORL.

MA: I guess that Ed Bennett had probably worked there; I think everybody was there, weren't they?

VM: No, no. There were people who were in Donner who stayed in Donner. It was only in 1963 when this building was built that they all moved into it. It was the ORL group that had to move down to Life Sciences. You didn't get to know the people down there very well, or not very fast?

MA: It turned out that Hiromi (*Morimoto*) and Ann Orme, which (*sic*!) were connected also with Ed Bennett, were down there because there was a room down there for them. When we had to do work together, then we would go down there for them. The rats were down there so whenever we killed the rats for the brain chemistry project we were down there. I could know that there was this other group down there no, but I didn't really get to know the people. I knew Ning Pon just briefly at that time, the graduate student that was there for 12 years or something...

VM: The record.

MA: I also knew his future wife (*Lynn DuBois*) because she was the illustrator (*for the group*).

VM: You really didn't get to know all these people until everybody moved together into this building.

MA: That's true. And then a lot of the shakers and movers were gone; a different group then.

VM: What about the social occasions that the group always had.

MA: They were wonderful. I think that was one of Calvin's big ideas, people should get together socially so they would know each other and be able to exchange ideas. That didn't work so much exchanging ideas on the brain chemistry although I think maybe on the Ed Bennett level he interacted with the people. I did get to know the people through the parties. Martha (*Kirk*) and Ann (*Hughes*) continued having parties, probably twenty years worth of parties.

VM: You were a young married at the time?

MA: Yes, a young married.

VM: You were free and could do what you liked.

MA: That's true then towards the end of the sixties decade I had children and then I was only working part-time and that took me away from the group.

VM: In your early days with the group, you actually did mix socially with a lot of the people, did you?

MA: Yes. Not as much as the people who all went camping with them. I didn't really know a lot of the people.

VM: You didn't go camping?

MA: I never went camping with them. I guess I just wasn't into all this really physical camping, although I got into it later and did backpacking and that.

VM: In the light of all that you have done since then, in particular in setting up the Calvin Reunion arrangements in — when was that? '89?

MA: In '89, January of '89.

VM: What was the occasion?

MA: I think Dick Lemmon and Marilyn know it better. There was some discussion about how much older Calvin was getting and how many years he still had left to be able to appreciate a get-together. I came into it because I was the treasurer of the Biodynamics Connection, which is something that we formed for keeping together the alumni of the people that were in the Calvin lab. I was really the only active remaining member because the Connection, after Calvin left and was no longer director, really didn't have any activities or any meetings or anything. I was stuck with some money, it was several thousand dollars, it was no small amount of money. When I heard about the reunion, I said "let me get in on this and let me spend the money". What better purpose than to have a reunion of the group that the committee was designed for; the Biodynamics Connection was designed to keep these people that had worked together (*in touch*).

So I knew some of the names because they would appear in my book as having paid their $5 or $2 or something.

VM: It was $5; it was very little.

MA: And that was a one-time $5; we had several hundred members, maybe 300 or so. We still have all the records of that if anybody's interested because I was the treasurer and I had the records. In fact, every once in a while I still have to file an income tax thing to say that the club no longer meets, defunct, no money, no income, no expenses.

VM: Talking of records and the saving of artefacts, I remember when I came here a couple of years ago looking for things, you were actually the person who had saved stuff.

MA: Well, when Al Bassham retired, his whole group then disintegrated and all the stuff that he had here was left. People that moved into his area would open drawers and find things. I was the one that was kind of the watcher of radioactivity in the building and some of these things were radioactive.

VM: Oh I see: it was in that capacity that you got into it.

MA: I was in that capacity that I got into it. This was in 350B, the bench that was occupied by Al Bassham people for a very long time. On the bottom was as large stack in several drawers, was a bunch of chromatograms. And some of the radioactive chromatograms, of course, being carbon-14 they are still radioactive for zillions of years. I had to dispose of those. But the films of those were in there also. So I got together with Al, and maybe some other people but mostly Al, to determine which are the important pictures that were left from that era. I wrote on all the films what they all were, just so that there would be some memory of this. I thought that the films that made the photosynthesis pathway might someday be of interest to someone. We saved them and kept them kind of in chronological order that made some sense. When we had this reunion, we pulled them out put them on the wall so that people could see the history.

VM: You've still got them?

MA: Oh yes; I've still got them.

VM: Have you got large numbers of them?

MA: Maybe twenty, thirty maybe. I think they are in three boxes.

VM: These are the critical ones that you pulled out.

MA: Determined by Al because I figured that any of work I saw it going on...but I never did any of it.

VM: A lot of the less interesting ones, presumably, were just discarded.

MA: Yes. Things that were like duplicates, things that didn't work, the old stuff.

VM: When I came two years ago looking for these things, you also knew where the other bits and pieces were, some of the old Geiger counters and the lollipops in that cabinet...

MA: We did find some of that stuff. Every once in a while I find something odd and I put it in that case up on the second floor (*an old wooden bookcase with glass doors opposite the elevator on the second floor*). I think that odd stuff should be saved.

VM: That's all that is left of the good old days, is it, what's in that case?

MA: Yes, I think so.

VM: There's no hidden stock anywhere else.

MA: No, I don't think so.

VM: Not much, is it?

MA: No. A lot of the things like the radioactive machines and growing areas, were all contaminated so we had to get rid of a lot of the larger things, they were contaminated with carbon-14.

VM: It wasn't worth the expense of decontamination.

MA: Nobody wanted to use them in the future.

VM: OK — so to come back to your own role in the place; there you were in 1959 working in Donner...

MA: Right.

VM: ...and what were you doing?

MA: We had several different things that were going at that time. There was tracing the path of adenine in the body. This is kind of a weird thing, we would take a whole mouse and throw it into a blender (I thought that was pretty cruel but it's very quick but we wanted to have...)

VM: A live mouse?

MA: Well no, I think we killed it first, but it was almost like a live mouse. We would skin him and look at the skin, and the liver and various organs separately to watch how radioactive adenine moved around the body. Ed Bennett can tell you a lot more about that; that was one thing we were doing.

And then there was a new collaboration that had been going on for a couple of years with Dr. (*David*) Krech and Dr. (*Mark*) Rosenzweig in the Psychology Department. They were growing rats and training them in mazes and they wanted somebody to dissect out their brains. Well. this involved a rat. Now a mouse is a little thing that's not so bad. In fact I think I had Ed Bennett kill those mice before I threw them into the blender. But these rats were kinda cute looking and were bigger things and I didn't think I would be able to kill them. So I had to kinda come to terms with that. My husband said, "well, if you want the job, you are going to have to kill them, because that's part of your job. Just think about it that they would never be there except that they were bred to be in that experiment". So I started thinking of the rats as chemicals rather than as live animals and I got over the point of having to chop off their heads and take out their brains. That was my job for many years. I got to be quite expert at using a little T-square on the brain to cut out the same point (*each time*). We find repeatable, although small, differences in these trained versus isolated animals, maybe on the order of 5%. Although there was some variation between animals, we matched up brothers into two conditions and then found repeatedly that they had thicker cortexes. That was determined by the weight of the piece I took out (*from the brain*).

VM: Apart from Rosenzweig and Krech who were in the Psychology Department, in the Bio-Organic Chemistry Group (the Biodynamics Group as it later became), the people were Ed, you and...

MA: Hiromi Morimoto and Ann Orme. They were doing more of the chemical part of it. I would do the dissecting and freeze the brain sections; they would then homogenise the brain sections and test them for chemicals.

VM: You worked in Donner and they worked in Life Sciences. So you had this to-ing and fro-ing.

MA: We also did some acetylcholine assays; I was doing that with the frog muscle, to watch the tension of the muscle with acetylcholine. I was doing some chemical assays too.

VM: Of course, you went to the Friday morning seminars like everybody else, did you?

MA: I can't remember them before they were in this building. I don't remember them impacting us over at Donner Lab.

VM: I wasn't here just at the time when you arrived, that was the two years that I was not here when I went back to England then returned later. Before ORL moved, they (*the seminars*) had been in the Faculty Club, in, I think it's the Lewis-Latimer Room which is a room with a movable divider and they made just one big room out of it, with a table, just like the table in fact...(*in the Round House*). For posterity, I might say that I am sitting in the very Calvin position at this moment at the end of the table (*here in the Round House*). You remember: Calvin always sat at this corner of the table and he did that in previous (*seminar*) rooms also, the same corner of the table as it was. You don't remember having gone into those (*seminars*) at all?

MA: I just don't.

VM: You would have met the rest of the group there, I should have imagined.

MA: No — because I never really knew what all they were doing in their research so I don't think that I did. Which was probably too bad because I would have known more about what was going on and have been of more help to you.

VM: When it came to moving into this building and the design for this building, as someone who had never known ORL, were you party, did you hear the discussion that went on with respect to this building, this round building?

MA: From what I understand, there was a centre table, which everybody worked around the outside of, and that was part of the discussion for designing (*the round building*). I don't think the building was round to start with, I think it was half-round. One day, somebody says why not make it all round!

VM: I think that was the architect who felt for some reason thought it would be difficult or too fussy with the building this size to have it in two styles. We just took a look at the big white table, it's very cluttered nowadays, with stuff, and people obviously don't use it in the same sort of way at all.

MA: There was a big *round* white table). That has been gotten rid; that was on the second floor and I sort of thought that was also involved with (*the original group*).

VM: No, no, no. That was the second-floor version of the big white...*the* big white table was, of course, not a table. It was a set of drawers and cupboards which had a top made for it.

MA: It's what we have on the third floor (*of the Round House*).

VM: That's right.

MA: I just redid all that. You know, I decided those drawers were full of stuff that nobody ever used and I have gone through all the drawers, taken out all the stuff that nobody wants, some of it's thrown away but most of it's stored away because I don't throw things away.

VM: What sort of relics did you find in that?

MA: Some funny old caps and bottles that fit together in strange ways.

VM: We might have a look at them: I might recognise some of them. There were lots of these micropipettes, these lambda-pets

MA: They are throwing those away now.

VM: Nobody uses them

MA: Nobody wants them. I wouldn't do it but somebody else did. I said I think they want them thrown away but I won't do it because I remember they were a dollar a piece. There was $10,000 worth of pipettes there.

VM: They were very valuable things at the time.

MA: Now we just do it all by Eppendorf. We discard the tips. I still have a bunch of those...

VM: Those lambda-pets?

MA: Yes because I won't throw them away.

VM: I should think not. The big white table, the original one, is the one on the third floor now. The one on the second floor was (*a round white table*) just made for this building. It was never...

MA: Dick O'Brien probably made it.

VM: I don't remember — or whether it was purchased; that I couldn't tell you. But it was never the scientific focus the way the other one had been. The other one was built so they could lay out chromatograms. That's why it was the sort of thing that it was. The other one was simply a coffee table, the central focal point for the second floor. Since the second floor was the middle of the three floors, people had coffee on that floor in the days when you could have coffee in the lab. Nowadays probably you can't have coffee in the lab.

MA: No we can't; we have our separate rooms.

VM: I'm not sure that everything is progress but that's what happens.

MA: Yeah. There isn't the discussion that used to go on. We would all gather and everybody would talk. We got to know people.

VM: That was the idea and that's, in a sense, what underlay the design for this building — was to have a focal point like that.

MA: There were often scientific discussions that went on at the coffee or they were discussing the next trip that they were all going to take.

VM: You, as someone who'd not known the original, you found that this design, this round building, was an effective way of organising a (*indecipherable*).

MA: Oh yes, I liked it a lot. I have lived in many different areas. I have lived on the first floor, second floor and third floor.

VM: You used to occupy a small room, didn't you?

MA: Yes, we had 136, with Hiromi. I have had a couple of benches on the second floor and now I'm on the third floor.

VM: As a user, how do you find the design of this building as a place to work?

MA: I like it a lot. I have heard a lot of complaints, certainly the fireman when he comes here has tremendous complaints about how it's all one big open space with a central stairway that a fire could just move from one lab. to the other. We have been under restrictions that we have to close off the halls and shut down that central winding stairway and we just haven't done it. We claim that we have no money, which is true. So, we just don't do it.

VM: They really want you to close it in?

MA: Oh yes. We were threatened of closure of the building until we got it walled in. We just didn't do it, we said we don't have the money and we will never have the money because we like it this way. It's really great: I can just run down and peek down and see if the Xerox is free, see if somebody is on the second floor.

VM: It's nice to know that thirty-odd years after it was designed that put into practice and, as it were, the management and the style inside the building has no doubt changed a great deal, the design is a useful one and you enjoy using it.

MA: Originally they wanted us all mixed up so I wasn't near anybody that I worked with. I was near Rapoport people or near some of the cancer people — just any other person, they would never put two people together (*who were working in the same area*). As the years went by, groups wanted to be more together because they got so specialised that they couldn't talk; the physicists and biologists parted ways. And so now we are much more clustered, not totally, but much more — there are little wedges, the Hearst people, and there are the (*indecipherable*) people over there.

VM: But you are still close enough so that you know one another well?

MA: Oh sure and being around so long I know everybody.

VM; Is there still a rapid turnover the younger people, transients through the lab.?

MA: What I see is a tremendous number of undergraduates who come in very short hours, often work nights and weekends, so I don't see them. I'm not here during the (*day??*) nights and weekends; I tend to work in the morning and then leave in the late afternoon and they tend to come in at night. But these people are only here for a semester or a year.

VM: Just doing an undergraduate project?

MA: A large number. The postdocs. most often stay for two years. We have a lot of postdocs. now because with funding being as it is we have very little staff. It's mostly graduate students or postdocs. or these undergraduates.

VM: There's lot of throughput of people coming through the lab.?.

MA: Sharing space even. It isn't as bad as over at UCSF. We were over there the other day, collaborating with somebody, and people had just one little section, about six feet of lab. bench, that's all they had. They had a little desk somewhere and somebody else was working right behind them.

VM: In this people still occupy the space as originally intended?

MA; Unless they share the space with an undergraduate.

VM: They have this eight- or ten-foot bench plus a writing desk...

MA: ...and then a rack behind them.

VM: Do you find in the building now that people mix socially well?

MA: Yes. That still goes on even though there is rapid turnover and much more pressure, it seems, to have to do a lot of work and less opportunity to socialise because we don't have the parties when people get together except Vangie putting them on in the building.

VM: Sorry — that name Vangie?

MA: Vangie Peterson is our building manager.

VM: I don't recognise the name "Vangie" but you just said "she" so I know it's a woman.

MA: "Evangeline". She has been working for the lab. at least ten years, maybe twelve years. She's the building manager and takes on a lot of responsibility including doing all the social things.

VM: Do you have parties when people leave?

MA: No, within the (*individual*) group they do but there're no more big parties. Maybe a retirement party.

VM: You have a Christmas party?

MA: We have the Christmas Party. We have a Halloween Party. We had just a spring party the other day, maybe two weeks ago. Whenever Vangie decides to put on a party. There are no more parties at people's homes that the whole lab's. is invited to. I think the Kim group does things where they go off camping; the Hearst group goes off camping for a weekend or something.

VM: What about seminars in here? Does the Friday morning occasion still remain?

MA: I think it happens once a month and people tend to go to the times when their group is there. I don't go on a regular basis anymore.

VM: So the population in the building is nowhere near as coherent as it used to be?

MA: I would say that's true. Not as coherent. I think within (*individual*) groups they do get together; when somebody passes a prelim., then that group gets together. It used to be you would invite everybody. Oh, somebody passed a prelim. and the whole building would be involved. Now it's only the Wemmer group, the Sauer group get together.

VM: The director of the lab. doesn't occupy the same role as Calvin did when he was director.

MA: Oh no.

VM: That clearly is what makes the difference.

MA: His style of directoring is different.

VM: So you've seen actually a lot of changes.

MA: We had (*George*) Pimentel as director for a while.

VM: You have been here for 36/37 years; something like that?

MA: Since '59; yes, you're right.

VM: That's a fair time to see things happen. Perhaps it would be invidious to say whether things have got better or not because that's not really a fair question.

MA: It's just the change. When you see what has happened in industry, in the world I think we still have a very special place here. People leave, having gotten their PhD or done a spell of postdoc. here, and we talk to them later: "Oh gosh; I didn't realise what we had when we were there — it was so wonderful". A gal that postdoced with us went back to Spain, went back to the same school she was in, but, of course, now she was in there as a professor and she said that she didn't realise how it was until she came to our lab., how the people helped each other so much and opened their doors and there was sharing.

VM: And that was recent, was it?

MA: She just went back last November and then she wrote me this in February.

VM: That's exactly the same sort of thing that I found when I first came in the mid-fifties and I think that lots of foreign visitors who came here found that. We never knew whether it was this place or just America.

MA: She was a little bit nostalgic, when she was here, for her university in Spain. But, then she got so used to...she never even had spoken English until the day she arrived here; she knew it as a language, as a study language but never talked to someone who could not revert to Spanish. She had a (*indecipherable*) and picked it up very quickly and made lots of friends throughout the building and did a project that there wasn't anybody really to guide her for. It was a different project than her graduate student work, new field, molecular biology, she was a biochemist and protein chemist. She

asked people all around to help her and got so much help. When she went back to her same university she said that everybody closes their doors.

VM: It's a different style of doing things, I think; it's part of the culture.

MA: She says she is going to try to bring our spirit to her university.

SM: I don't know whether I missed it earlier, but do you have a job title?

MA: Research Associate, Principal Research Associate.

VM: One of the things I have noticed, just in the last few days since I've been here, is that in the building there are a number of elderly people knocking around, wandering in and out. Calvin now lives here; Marilyn is with him; John Otvos; people like Dick Lemmon and Al Bassham and Ed (*Bennett*) come in for coffee. Do the youngsters have any idea who all these people are?

MA: No, I don't think they do. When we had the reunion, we had it just for the Bio-Organic Group, and the people presently in the lab. (at that time — 1989) were not invited. But they saw we were inviting; we told them what was happening and we had posters all around the building and I put up one poster that had dates and we copied some pictures form the LBL files and put up this big older poster. They looked at it and said "My goodness. This was before I was born!" They couldn't believe that all this was going on with such a history.

SM: How does Ning fit into this as far as people are concerned?

MA: He is in the Hearst group now, so I work with him. He was in the Bio-Organic group in LSB and I always just knew who he was because he's such a friendly character and fun to be round. He and Hiromi were good friends and since I worked with Hiromi I would see Ning. I didn't really know him personally at all 'til he came now. He's a retired professor from (*UC*) Riverside and just works here for free. He comes when he wants to and doesn't come when he doesn't and contributes a lot of information from antiquity — this is how we used to do it.

VM: Ning must be the longest serving scientist, as it were. He came in about '53 or '54 as a graduate student and therefore he has been here longer than you have, not continuously but at least in the building. Calvin is no longer active as a scientist so Ning is the...

MA: Marilyn, too.

VM: She's not a scientist. After Calvin, she's, as it were, the oldest inhabitant. She joined in him in '48.

MA: In '48? That's about the time Ed Bennett came too.

VM: Something like that. Some people have been around for a very long time! That's quite true.

So you say the youngsters were surprised. They know it (*the building*) is called the Melvin Calvin Laboratory? Did they not know what that meant?

MA: Sure because of the Calvin cycle in photosynthesis would have been studied.

VM: They knew about that?

MA: Yes, I'm sure they would know that.

VM: They realised that this building was in some way...

MA: We had the pictures up. We had pictures of the ORL; they don't know what it is but they know there's some history. And then the picture of Calvin getting his Nobel Prize hangs in the reception.

VM: Tel me, as someone interested in relics, do you remember, you were here more or less at the time, do you remember that big working model, that never really worked properly, of photosynthesis, the big thing with the screen and the flashing lights that used to be by the backdoor of this building?

MA: And the panels on this wall here. It never worked very well.

VM: What happened to it?

MA: I think they finally took it down. Vangie might know. I can ask her; she's not in today. She might have been involved in taking it down because finally people decided they couldn't get it working.

VM: No, no, they couldn't. It was before the days of electronics and it was all done on microswitches or whatever and it was a right pain in the neck.

MA: I have seen it work. It was working.

SM: Was that the thing that dispensed a sugar lump at the end?

VM: It was supposed to dispense a sugar lump...

MA: I didn't see it dispense a sugar lump. I just saw lights go around the cycle.

VM: I don't think anybody saw it dispense a sugar lump. The thing originated with the Brussels World Fair of 1958...

MA: Yes that's what I...

VM: ...and it was designed for that and I was part of the team. I can't remember what I did: I wrote some of the programme notes or the paper that went with it or something like that. It was designed to show the system of carbon dioxide going in, following the carbon...

MA: I have seen it work with the lights so I know that it at least partially worked.

VM: The idea was that the carbon atoms would go in and every so many carbon atoms in a sugar cube would come out of the dispenser at the end. This was to have been either C&H — or Spreckels sugar, I think it was. It was to say on the wrapper in umpteen languages this sugar lump represents the sunlight falling on so many square whatever over such-and-such a period.

MA: How wonderful? Did it dispense sugar?

VM: I've never seen it dispense sugar and I have an idea that it never did. Something — I'm not quite sure — I have an idea that something went wrong with the sugar manufacturers and the contract got blown and they never actually delivered the sugar.

MA: So it never dispensed it because there wasn't any sugar!

VM: Sure; I think that was part of the problem but that was the original idea. It was actually a clever idea and, had it worked properly, it would have been very good. But you're right: it stood there for a long time and eventually it went, it disappeared. You know, that there is am archive, a warehouse down in Emeryville belonging to LBL. I went down there a couple of years ago and there is surprisingly little from the Calvin group down there; in fact, very suprisingly little.

MA: There used to be tons of (*relics*) from gross purchases, where you get 144 for the price of 100 and then you only use about 20; the rest of them are all down there. We had toluene that no one would ever want anymore and we had to get rid of all that stuff.

VM: There were only two pieces of equipment. One of them was that very large automatic counting machine that Al used to use.

MA: That's something from the relic times.

VM: That's right. That's something that I and Karl Lonberg designed, Al modified and the remains of it are down there.

MA: Do you know what I found that belongs to that? The plastic.

VM: Ohm the mylar.

MA: The mylar.

VM: Do you still have it?

MA: No.

VM: That's something you threw away!

MA: It was just a roll, and it was that wide, and I said that's what used to go into that monster (*the appellation for that automatic radioactivity counter*).

VM: That was a 5-inch roll of mylar, 5/5-1/2 roll. We had special ordered the mylar. (*...indecipherable/confused; too many people talking at once...*) We had two sheets of mylar wrapped up in brown paper.

MA: You would stick these little leaf-shaped things...

VM: You watched them do that?

MA: Yes, it was wonderful. It would just go at a rate, everything was going through. You would stick them in and you could see...It was wonderful.

(*This exchange refers to cutting the chromatogram spots from the papers [the "little leaf-shaped things"] and encasing them between two layers of very thin mylar film*

for transport between two Geiger counters. Details can be found in V. Moses and K.K. Lonberg-Holm: "A semi-automatic device for measuring radioactivity on two-dimensional paper chromatograms", Analytical Biochemistry, 5, 11.)

VM: That's the height of my inventive skill.

MA: I wish we had a video of that. You know, we didn't have video at that time. It would be fun to watch that operation going on.

VM: It's all part of the record, and it might just as well go in now as at any other time. We once had a competition — well, let me back track. In order to use that machine we had actually to cut out the radioactive spots from the paper, separate them, and we once had a competition in the lab. one afternoon as to which was the quickest way to cut spots out. There were three people, and I was one and Martha (*Kirk*) was another, and I don't now remember who the third person was. One of us used a pair of scissors and cut it (*the spots*) reasonably carefully — they were marked out on a piece of paper. We made a series of fake things on bits of filter paper so that everybody had the same problem. Somebody else used a scalpel, as it were, to slash it out, and I used an engraving tool; it was a thing which vibrated a point for engraving metal...

MA: You had to plug it in?

VM: You had to plug it in...you sort of bashed it around the thing (*i.e. the spot*) and it just sort of destroyed the paper and you were left with the spot.

MA: But it may have made edges.

VM: I don't remember that. Anyhow, we had this competition; to tell you the truth, I don't remember who won the competition but it was teatime one afternoon no doubt at the big white table.

MA: I had cut out those spots and put a little tang on them and hung them up on a little tray and collected into a little vial...

VM: ...eluting them.

MA: Eluting them.

VM: That was a technique invented by Alex Wilson about five years before you joined the group. We met him a couple of weeks ago in Tucson and he told us about it then.

MA: I remember there was an apparatus, but that's long destroyed, that you would fill with the appropriate elutant and then watch the bottle.

VM: I must actually come round and see what sort of relics there are because I am not collecting relics at this time, but I think we might well be collecting photographs of relics so that we can use them in whatever we write up in addition to pictures of people — pictures of some of the equipment.

MA: You probably won't find to much.

VM: Well, whatever there is. The publishers will never let us use hundreds of pictures anyway.

MA: That's true.

VM: But we may be able to get a few pictures of this and that.

We talked before that you were responsible for doing most of the organisation for the Calvin reunion in 1989...

MA: Not much, just part.

VM: A significant part.

MA: I think Marilyn did most of the organisation.

VM: You've been giving us goodies, either to keep or to borrow.

MA: One of the things, we had this money I told you about, several thousand dollars, and I wanted to spend it down. We were afraid to overspend it because there was no source — there wouldn't be anything more, so we wound up with about $700 left over which we then bought (you can see it right there [*i.e. in the seminar room*]) we bought that television that we can play — it's got a VCR in it. We can play tapes for teaching people because now all learning things are coming out in tape form. This is a gift. You can see on the back of that a sign that I put on it: "This is a gift from [the Calvin Reunion — not the Reunion but] the Biodynamics Connection".

We found out who was coming to the reunion and I looked up the years they were there so this is the chart (*which is really a timeline for the people and the period of their stay in the lab.*) People then knew who was in the lab at the time they were. This was afterwards: "Currents" put out a whole page of the "Currents" pictures (*about the reunion*).

VM: Is this a lab. publication?

MA: Yes; "Currents" comes out once a week by the lab. They sent down their photographers just for the afternoon part of the party (*i.e. the seminar presentations*). Several of the people in the group, like Dick Lemmon, were taking photographs on their own. We hired with the Biodynamics Connection money a video-taper. When we had the programme we had it professionally videotaped by the lab. out of our costs.

VM: Those are the tapes that you lent me to be copied?

MA: That's right. Those are the tapes of the afternoon meetings of Calvin with the different aspects of his research. In the evening we hired somebody to audio tape (*the remarks*). Unfortunately, the audio tapes are not too good. We audio-taped the proceedings. We showed a bunch of slides of the group get-togethers and then had peoples' reminiscences. That was totally off-the-cuff and the people were all over the room and they didn't talk into the microphone very well. So the sound was difficult to hear. Very soon after that, I started listening to it and typing it up as best I could. Marilyn sat and she and I would take turns. She would listen to it and add what she thought might have been said.

VM: And those are these transcripts here.

MA: Those are the transcripts and this is just a list of the participants.

VM: That's very useful indeed. I'll get these tapes copied and at least, even if they're not totally clear, I can use the transcripts to help me understand.

Lastly you have given me something which describes some of your own work, you said from about 1983..

MA: From '83-'90. Well, I still am working for John Hearst but the project I was involved in was sequencing all the photosynthesis genes which happen to fall in a cluster — lucky for us. We have that cluster isolated. It's about 48 kilobases It's pretty long, and this is of the photosynthetic bacteria, *Rhodobacter capsulatus*. We even had T-shirts made at one point, all the people that were working in the group; we called it the "Photosynthesis Cluster". This is the thing I was sequencing. I was working my way along the gene, all the genes. We have about 44 genes identified on that cluster that we sequenced.

VM : So you've come a long way from chopping the rats' heads off to sequencing genes. Well, the last thing I would like to...

MA: I was glad to get into photosynthesis. Remember I told you that I came here with my ideal to work on photosynthesis. When I was handed this project I just said "whoopee, yes I will do this". I had to learn all this molecular biology which I didn't...which had come in since I was a chemist. I did.

VM: I would like to thank you for all the time you have spent with us and all the material you have provided for us to look at and say that I still would like, when we have a few minutes, to look at whatever the artefacts are that remain and maybe take some pictures of those as memories of the good old days and we'll come back and see you within a few days or so and fix a time to do that.

MA: OK.

VM: Thank you very much — and you have time for your meeting and we for ours.

MA: Thank you.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Marie Alberti

Date of birth 10/1/37 Birthplace Los Angeles, CA

Father's full name Theodore John Salvinger

Occupation Salesman Birthplace outside Vienna, Austria

Mother's full name Anna Ruth Kottmann Salvinger

Occupation Housewife. Birthplace rural area near Ellsworth, Kansas

Your spouse Raymond Alberti

Occupation Ironworker Birthplace Oakland, CA

Your children Scott Hebert
Cheryl Hebert Cole

Where did you grow up? Los Angeles, CA

Present community Berkeley, CA

Education BS in Chemistry, UCLA

Occupation(s) Research at UC Berkeley Lawrence Berkeley National Lab

Areas of expertise Sequencing DNA, other microbiology techniques.

Other interests or activities

Organizations in which you are active

Chapter 19

BOB B. BUCHANAN

Berkeley, California

June 18th, 1996

VM = Vivian Moses; BB = Bob Buchanan; SM = Sheila Moses

VM: This is a conversation with Bob Buchanan in Koshland Hall, Berkeley, on June 18th, 1996.

Bob, as I mentioned a little while ago, I saw the Calvin group from the inside, you saw it from the outside. When was that? When did you first become associated with photosynthesis here?

BB: I first came to Berkeley to work with Jesse Rabinowitz as a postdoc in the Department of Biochemistry. I had worked with fermentative bacteria. I joined Daniel Arnon's group about a year later, this was 1962 I came. So, in 1963, after he (*Arnon*) heard me give a lecture at the Federation Meetings on ferredoxin, which was a contemporary topic, timely topic, and he asked me to join his group. That was in September 1963. I guess I came in kind of at what was the second phase of his photosynthesis career, when he started working with proteins, ferredoxin in particular. The earlier part would have been with the discoveries with isolated chloroplasts. Bob Whatley would be have been the one. Bob Whatley and I overlapped by a year. He went back to England, to London in 1964, I believe.

VM: Did you overlap with David Hall?

BB: No, but I know David. David had left already.

VM: He's a colleague of mine at Kings College; I have an office at King's.

BB: You have an office at King's? Kunyo Tagawa (*correct spelling?*) was here; we overlapped one month but David had gone.

VM: When you were working with Jesse you had nothing to do with photosynthesis?

BB: Nothing. I just heard tales of Arnon and Calvin, really from the outside (*laughter*).

VM: What tales did you hear?

BB: That there were these two groups that were pretty isolated...

VM: From one another?

BB: ...from one another. Each worked in its own kind of territory. They didn't interact much. Occasionally I would see one or the other at a seminar, and I was a young postdoc., and that was very interesting. I never thought I'd be involved in any way with either.

VM: There was, of course, contact at the lower level, but at the upper level — for reasons I haven't totally discovered — there was considerable antipathy. I haven't yet quite run it down. When you got into Arnon's group, what did you start working on, what did you start doing?

BB: He heard me lecture about ferredoxin in *Clostridia,* in fermentation. He had found, with Tagawa, that ferredoxin is important. The protein that San Pietro had earlier identified as PPNR is actually two proteins: ferredoxin and the enzyme, ferredoxin-NADP reductase. He was interested in ferredoxins and I provided just a new expertise in that area. He saw, I think, an opportunity to do things that he hadn't been able to do before. I also had a strong microbiology background.

VM: How long did it take you to become suffused with the photosynthesis story?

BB: Quite a while. As a matter of fact I'm still learning things even though I'm not working in photosynthesis any more. He had the idea, Arnon had the idea that the PGA, the 3- phosphoglycerate which Calvin and associates had identified as the first stable product of photosynthesis under their conditions, might not be the real first product. He had thought that maybe one could use the reducing power of ferredoxin to bypass the ATP step needed for the formation of phosphoglyceraldehyde. One would start out with RuDP and get phosphoglyceraldehyde directly and circumvent this step. That was one of the things he had in mind when he invited me to join his group. Overall, he was thinking, perhaps a little more generally than that, that ferredoxin might be involved in CO_2 assimilation.

And so we started to work on that aspect in bacteria and it has been known for many years that fermentative bacteria break down pyruvate to CO_2, hydrogen and acetyl CoA and ultimately acetate. Ferredoxin then proved to be a carrier in the electrons from the pyruvate dehydrogenase to hydrogenase. Our first discussion was: can that reaction be reversed? It had been reversed *in vitro* by Wolf, at Illinois, using hydrosulphide or dithionite, and it was very, very marginal, but it happened; the extent was low. Would ferredoxin do that under physiological conditions?

I worked with Reinhard Bachoven (*spelling?*) who was a postdoc. from Zürich; he was here for about a year. We overlapped for about a year and then he went back and like many of these people from Berkeley he lived happily ever after with a chair in Europe!

VM: Some of us do — yes!

BB: (*Laughter*) Though he's still living happily ever after he retired a few years. We were able to show that ferredoxin can indeed reverse this reaction so one can get pyruvate synthesis from acetyl CoA and hydrogen and CO_2. We found this first with the *Clostridium* in which the reaction usually goes in the breakdown direction. But, it was an experimental achievement so we published that. Bachoven left and we began to look at photosynthetic bacteria: so this was my first entrée into photosynthesis was

with photosynthetic bacteria. I had started some work to look for this reaction in photosynthetic bacteria and we were joined by Mike Evans, who is now a professor, again one of these who rode back to a chair in England, at University College I believe. He spent two years here. He had a very strong microbiology background, especially with photosynthetic bacteria since he had worked with Elsden (Sidney Elsden) in England and Davenport; he knew Davenport very well, they were buddies. Evans came and we looked for this more seriously, this reaction, which we called it the "pyruvate synthase reaction".

We found it in green bacteria. The green bacteria at that time were very neglected. Roger Stanier was here at the time and he gave us a culture of *Chlorobium thiosulfidatum* str. Tassajara because it had been isolated in the Tassajara Valley here. Stanier came over and talked about growing it and so forth, and we learned how to do that. It turns out that it was great at assimilating acetate, the whole cells, but they required CO_2 to do that. Van Niel had done some work on this earlier. It was a very interesting organism and seemed to be different from the other photosynthetic bacteria which had been worked with much more extensively at that time — the purple bacteria, such as *Rhodospirillum rubrum* (purple non-sulphur) and the purple sulphur such as *Chromatium*. And so we looked and we found the reaction was very active in this organism. The evidence that Elsden and collaborators had obtained earlier with acetate showed that acetate seemed to enter by this group and the alanine was labelled where it should be labelled from CO_2 and acetate if this reaction operated. It looked like it really worked *in vivo*.

VM: Were you using chromatographic techniques for identifying compounds?

BB: Yes we did, at that time paper chromatography. So that was the way we identified the pyruvate, with acetyl CoA, ferredoxin which we reduced with chloroplast in the light and crude preparations of the hydrogenase. We would run the reaction under carbon monoxide to inhibit the hydrogenase so that all the electrons didn't go off as hydrogenase. It wasn't such a poison in those days. (*Laughs*) Now you would have to take great precautions. We found evidence for it. Then we said: "is this the only place that ferredoxin can act in this capacity?" Because again, there was labelling evidence in the literature that the α-ketoglutarate dehydrogenase reaction could be reversed in photosynthetic bacteria. We looked for that, and we found it. We had an α-ketoglutarate synthase reaction.

Then we said: "Does this really mean that there is some kind of cycle operative that would not be the reductive pentose phosphate, or Calvin-Benson, cycle?" So we looked and we obtained evidence that indeed there was. What this organism was able to do is to reverse the citric acid cycle and incorporate CO_2. It uses reduced ferredoxin to do this, and it runs it backwards: instead of the condensing enzyme it has an enzyme that cleaves citrate and all the other steps were reversible. So, it seemed to work this way. We used a lot of chromatography. We did short exposure experiments. We used the "lollipop" that had arrived in the drawer at some juncture and carried out photosynthetic bacterial experiments in the lollipop and did short exposure experiments that showed that the first product was not PGA, we didn't even see any PGA, but was amino acid and glutamate was a very early product as one would expect if the citric acid cycle were operating in the reverse direction.

VM: Can I ask you some questions before I forget them all because you're going very fast? The question that immediately arises is: were these techniques already developed in Arnon's lab. or did he go to Calvin to get them?

BB: The chromatography techniques had long been in place. When they discovered — Arnon, Whatley and Allen — CO_2 assimilation by isolated chloroplasts, they looked at the products. I'm not sure who brought the technique.

VM: Whatley might know that

BB: Whatley would know that, where they came from. They were indeed in place and Losada in Spain had done quite a bit of chromatography work. Arnon used to be in LSB but then, by the time I came, he had moved into Hilgard Hall for about one year. We had a deluxe chromatography room with many cabinets. Actually, chromatography kind of went out after we got there. (*Laughs*)

VM: There were one or two stalwarts who kept it going but its time had gone.

BB: Its time was very limited but we did use it a lot with the *Chlorobium* so it was all in place. And Evans had experience with that, too.

VM: The other question I wanted to ask you which was some minutes ago: You mention that Arnon had postulated that the first product was already in a reduced form of glyceraldehyde. Was this something which arose while you were with him or had he thought about it earlier?

BB: No, he had thought about that earlier.

VM: Because I wonder whether this might have been a cause of some of the tension between the two groups.

BB: Yes, it may be.

VM: Calvin was, of course, very proprietary about PGA.

BB: About PGA — right.

VM: And then Arnon came along and said "perhaps not".

BB: But that was one of the things that he thought. It's still PGA, of course. What it did it provided us an avenue to discover other things.

VM: And, of course, the other systems, the Hatch-Slack system which came later, I think, when Calvin was already well established, caused less of a trauma than a possible undermining at a rather earlier stage — when, perhaps, a certain recognition had taken place!

BB That's true. We did find evidence for this cycle in *Chlorobium* and I don't think that was received with open arms by a lot of people. Then we looked to see if it had the Calvin cycle and it turns out it doesn't. The genes aren't there, the enzymes aren't there, nothing is there. To get this cycle into textbooks took 20-25 years because there was so much resistance to it that this was the only cycle in autotrophic cells.

VM: When you say, so much resistance, how did you perceive resistance? Who were the resistors?

BB: There were reviewers, whom one never identifies, they were people who spoke in reviews that Rubisco was a marker of autotrophic life. That has since been shown not

to be the case, not only with our systems but with others. I don't think there's anything peculiar to this particular group of people — cast of characters, one might say, were it a play — but it happens with others. Once an idea is accepted, and I am sure Calvin had his own problems in getting the cycle accepted, then science tends to think that's it and that there is nothing new.

VM: So the resistance didn't just come from Calvin's group? It came from...

BB: From other people. There may have been some from there, of course.

VM: There was perceived wisdom by that time and you were, in a sense, counter-attacking and that's what was causing the trouble.

BB: He might remember that, I don't know. I wouldn't say he was the major force. There were others. Finally, the opposition admitted in print in 1988 — we published it in 1966 — that it seems to go that way; it took that long. Now it's in textbooks and so on. It's a good lesson for me because we did it again in other things.

VM: If I can come back to your earliest days here in Berkeley with Arnon and focusing on the Calvin group, what did you see of the Calvin group, what was your understanding of it, or reaction to it?

BB: I just know that there wasn't any contact.

VM: You never had any yourself?

BB: No, I didn't. I did my job. I had plenty to do and I never went up to see them. We had everything we needed. In those days, money was no question, no problem anywhere with quality groups. I guess the first thing, and I didn't know the people — I met Al Bassham much later — the first kind of move to get the groups together was John Olson, who was a professor in Denmark, I believe; this was some years later, this may have been as late as, I believe this was in the seventies. He came to spend a sabbatical year with Calvin or with someone in the group. I don't know whether you remember him, or not.

VM: I don't think I knew him. I'd left by '71.

BB: It must have been a little after that. What he did was to organise a weekly seminar in the Round House; they had a beautiful seminar room. That actually, people started going. It became a fixture on the campus for many years when photosynthesis research was continued very actively. So the two groups did get together.

VM: Did Arnon and Calvin come?

BB: Arnon came a number (*of times*). Calvin didn't come very often because I think his mind was on other things., It had shifted from photosynthesis. I don't remember seeing him there; maybe once. Arnon was a little reluctant at first but then he started to going to all of them. Then, I began to meet the people and so forth. We were in a College of Agriculture and my job wasn't an instruction job, it was a research job. I was an assistant microbiologist which was 100% in the Agricultural Experiment Station. It's a tenure-track job but you don't teach.

VM: When you say "assistant microbiologist": what does that lead to in promotional terms?

BB: If I had not become a professor, if life hadn't unfolded in that direction, it's the equivalent of assistant professor in salary and everything, except one doesn't teach.

VM: You could have become an associate microbiologist?

BB: That's correct, and a microbiologist at the same level as a professor without the teaching duties. As I look back on it, that was a pretty fortunate appointment in some ways. It lacked stature but you don't need stature at that stage. I was able to get a lot done. With time, I started teaching.

VM: How big was Arnon's group at the time you were there?

BB: I think at its peak it probably got up to 15, maybe as high as 20.

VM: Everybody included?

BB: Everybody; 15 to 20.

VM: That was a sizeable group for an academic department.

BB I think I came pretty much at the peak of its size.

VM: Was Arnon himself very hands-on?

BB He had been earlier but at that stage he wasn't really. He did try to visit everybody every day.

VM: To discuss nuts and bolts?

BB: Yes, right.

VM: So he was very much a part of the activity.

BB: He knew everything that was going on and he had his own way of doing things that was different from most Americans.

VM: How do you mean?

BB: He ran a very tight ship, I guess you might say. Notes had to be kept a certain way and he was really very on top of things. He spent a year with Warburg, or six months with Warburg, and he really saw many faults with Warburg but he saw many, many virtues. I think deep down he liked the way the institute ran, which was very Germanic. Some of that, as American science permitted, was translated here. He ran it with a kind of Germanic style. Bob Whatley would, I think, confirm that. And I'm not saying it in a derogatory way; it worked.

VM: Yes. It's just that one somehow feels that that style of management doesn't sit very well in the American psyche.

BB: That's right. But he was successful. It worked very well for me. I came up from the south and we're pretty much used to anything, and it didn't bother me. But it would have bothered some people.

VM: What was he like in the lab.: was he a relaxed person, was he a formalist?

BB: He was pretty formal in the lab. and things were quiet.

VM: What did you call him? How did you address him?

BB: Dr. Arnon.

VM: Never Dan?

BB: Not for some years, no one did.

VM: Everybody else was first names in the group, were they?

BB: That's right. Bob Whatley did, I think.

VM: It was a very parallel situation with the Calvin group. It was a long time before people addressed him by his first name, although his wife always referred to him as Melvin talking to other people.

BB: There was a formality. When Bob Whatley left, it became pretty formal.

VM: Is this characteristic of the relationship between professors in groups generally? I would have thought they all would be first names.

BB: It was not. For example, when I was with Jesse Rabinowitz everybody was on a first-name basis. There may have been situations in Biochemistry, with more senior people, that was a little different but not quite the same as here.

VM: At that time in Arnon's group did he get all the money to support the group?

BB: Yes, he got all the money.

VM: So all you did was to support him in whatever he needed.

BB: Yes. It was very effectively organised. People had different obligations and so there was only a secretary and a repair person who was very good, a retired Navy petty officer who could fix anything. Arnon told me that if you are on a ship you have to be able to make do with what you have and that's why he hired him. That paid off. I was in charge of all the ordering for the laboratory, for example. Someone wanted an order, I would...

VM: So you divided up the housekeeping duties?

BB: The housekeeping duties; we didn't hire people. And since there was no teaching — we can't get by like that now; everybody has to teach. Those days are all gone. But it worked well in this time. Some people didn't find it as comfortable as others.

VM: Because of his dominance of the group, or what?

BB: And direction, overall direction.

VM: He really did direct?

BB: Oh yeah. The ideas. He had a lot of ideas.

VM: The ideas predominantly came from him?

BB: Not all, of course, but many did.

VM: Was he receptive to other people's suggestions?

BB: Not at once. That wasn't his style. We had a seminar, a group meeting once a week, these could go on for hours, and he said it was like a kitchen where you try the dishes before they are served. That's where there were great discussions and debates about ideas. I think sometimes, he was a genius it seemed, of course he knew what was going on, he knew there was an idea that would supplant his. But he would hold out, I think, because it would bring even newer ideas and he was a genius at getting people to express their creativity.

VM: Well, it also has the advantage that it would force people to support their ideas with evidence and arguments.

BB: That's right but he would hold on to his (*ideas*) forever until it became kind of ludicrous sometimes. In the meantime, of course, something would surface that was worthwhile.

VM: Did he stop people following their own initiative in the lab. in experimentation?

BB: No, not that. I think you could do that. You would always be put in the light of a certain idea.

VM: Did his name go on every paper?

BB: Pretty much until I came, that's true, and then it began to change. For the first two or three years his name went on all of our papers. Then Mike Evans and I published a paper, the α-ketoglutarate synthase paper, by ourselves. We also published a paper on the presence of a non-cyclic electron transport system in bacteria which is the only one that has really held up. That was on our own. About the time I came it began to change a little because he could see that people had to develop careers independently.

VM: What was the style of authorship of papers? If his name was on all the early papers, did he contribute, did he at least read the manuscripts of everything?

BB: He wrote them.

VM: He wrote them?

BB: Absolutely. He was a scholar to the nth degree. Every reference had been looked up by him, oh yes, and written. Kunyo Tagawa describes it in one of the write-ups for the special issue of *Photosynthesis Research* and I saw it but I guess Kunyo Tagawa saw it even more so. When a paper was in the works....First of all, there's always a deadline, a PNAS deadline, or some deadline to meet. There were no word processors in those days and so everything had to be typed fresh. He would have the secretary type everything and make carbon copies initially (but ultimately Xerox copies) and make ten of those and give it to everybody in the lab. to read to get it to perfection. (We may have to leave; maybe not [*the room in which we were talking had been booked by another group*]).He would say once it's published you can't use an eraser.

He was very much a part of every paper. There was no perfunctory name-adding. He knew everything.

VM: But he wasn't necessarily the original drafter of every manuscript, was he? Presumably the guys at the bench...

BB: They would write the first draft. But they didn't bear much resemblance to the final draft. He taught me how to write. He taught me so many things, writing being one of them.

VM: That was rather different from the Calvin style where other people wrote the papers in the first draft and he certainly went through them, and there were endless arguments over commas and other factors. The style was the guy who did the work and wrote the paper came first in the author list. He (*Calvin*) usually came last and in between they were alphabetical.

BB: It was usually obvious who should be the author. Arnon was always last except in the early days when he was actually participating in the work with Arnon, Allen and Whatley, this team and then sometimes he was first. I learned a lot with this system...

VM: I can imagine.

BB: ...and I look back, and there are several people I quote when I tell students things, and my mother is one, for wisdom, and Dan Arnon is another. You never forget some of those things.

VM: It's very interesting, the comparison of these two leaders of photosynthesis, working within a few hundred yards of each other, at arms length, more or less ignoring one another's existence, at least for a long time. I hope to get some more insight into that.

BB: I think one interesting distinction between the two groups is the habitat. Calvin was in a very strong Department of Chemistry with many luminaries. Arnon was in a College of Agriculture in which he was doing most, if not all, the fundamental work that was being done. There were not a lot of colleagues around him who even appreciated what he was doing. It was a very different type of situation. No kind of stars to help him along. We probably have to...Come in!

VM: Obviously, we are being thrown out of the room. Thank you very much.

BB: If anything comes up...

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name ~~Jeanne M. Segate~~ BOB BRANCH BUCHANAN

Date of birth Aug. 7, 1937 Birthplace Richmond, VA

Father's full name Ben Keys Buchanan

Occupation Farmer Birthplace Glade Spring, VA

Mother's full name Hazel Weisiger Buchanan

Occupation Homemaker Birthplace

Your spouse Melinda Spears Buchanan

Occupation Instructional Aide for Handicapped Birthplace Raleigh, NC

Your children Alice Jean, Elizabeth Branch, Catherine Keys, Anne Stone (Buchanan)

Where did you grow up? Washington County, VA

Present community Berkeley, CA

Education PhD

Occupation(s) Professor, Plant & Microbial Biology

Areas of expertise Plant biochemistry/physiology
Microbial biology

Other interests or activities Swimming

Organizations in which you are active Amer. Soc. Plant Physiologists, National Academy of Sciences

Chapter 20

LOREL (DAUS) KAY

Mendocino, California

June 19th, 1996

VM = Vivian Moses; LK = Lorel Kay; EK = Eric Kay; SM = Sheila Moses

VM: This is a conversation with Lorel Kay on June 19th, 1996 in Mendocino.

Can you tell us how your scientific life started and how you ever came to be in Calvin's lab.?

LK: I was always interested in science from the time I was in high school and, as far as getting into Calvin's lab. goes, I was doing graduate work at the University of Michigan and one of the professors, Professor Westerham (*spelling*?), had asked Melvin Calvin to come and give us a seminar on photosynthesis, path of carbon type of thing. I listened and I was absolutely fascinated so that I got over my natural reluctance to talk to people and I went down to the rostrum and I said: "Professor Calvin, is there any possibility I can work in your lab.?" When I came out (*to California*) job interviewing, I went up to the Radiation Lab. and I got the job.

VM: What did he say to you when you asked him that?

LK: He said, "Get in touch with Mr. So and So (whose name I have forgotten) who handles personnel" and so that's what I did.

VM: You were a chemist then, were you?

LK: I was strictly an organic chemist. I did my graduate research pouring two stinky chemicals together and getting a lachrymator. Pretty much organic chemistry.

VM: When you heard Calvin give this talk, was he doing his chromatograms and photosynthesis stuff?

LK: Yes and that was what was so fascinating. The type of thing that I had been doing was organic synthesis and just the idea of doing these separations on something as simple as paper chromatography was just mind-blowing.

VM: You hadn't any special thing you wanted to do in his lab. when you went there?

LK: No.

VM: Were you a graduate student, did you go to him as a graduate student or did you have a PhD?

LK: No. I was looking for a position, a money-producing position. I had finished the graduate work at Michigan.

VM: One of those things one has to have to live on.

LK: Right.

VM: So you came there. What happened when you arrived in Berkeley?

LK: I started working in Donner Lab. and at first I worked with somebody who was working actually on nucleic acids. We were hydrolysing them and sending them through columns and we came out with this fascinating little piece of information as, gee, there is the same molar quantity of two of the bases as the other two bases. Isn't that remarkable? I have forgotten the man's name now that I was assisting in that.

VM: This was before the great event?

LK Absolutely. Not long before the great event. So in '52, when I was on a visit to Europe, we went to the Cavendish Labs., we saw the labs. that were to produce this. It was really fascinating.

VM: I was just trying to remember, you can't remember who it was that you did this DNA work with in Donner?

LK: No, I don't.

VM: Ed Bennett, I think, had been in Kalckar's lab., hadn't he, in Copenhagen?

LK: Yes, but that was later.

SM: May I ask what year was it that you started?

LK: I started in '49, early in '49 as a matter of fact.

VM: When you first actually got to the lab., Day 1, Hour 1: who did you meet and how were you received, what happened, what did it look like when you got there?

LK: That was a little bit awkward. It turned out that when I did the physical exam my blood count was a little bit off and I had a spleen that they could feel so they wouldn't let me start to work immediately. We were monitored all the time for radiation and so on. I kind of hung around for a while. I can't really remember: we were up in the second floor of Donner (*actually it was the third floor*); of course, Ed Bennett was there, this other man that I worked with. I worked with a young woman called Marilynn Meinke. We did propionic acid metabolism in mouse liver slices. Those data came out...my maiden name was Daus and I called that the Daus Mouse paper when it was published.

VM: When you say that you couldn't work at all, did they literally not let you work or you couldn't work with radioactive materials?

LK: They literally would not let me start working until my blood count became within the range. They figured it might have been a subclinical case of mononucleosis. But anyway, here I was, with something I had never heard of, "Why can't I work in this lab.?". It resolved itself. I can't really remember the very beginning. I am sure I must have talked to Bert Tolbert at that time because Bert was probably the first person I really would have talked to in the lab. when you ask what happened right at the beginning. I was probably talking to Bert. Somebody called Dorothy Mabee was also working in that particular lab.

VM: In the same room?

LK: In the same room — Dorothy Mabee, Marilynn Meinke and I were in that room, and I think we also did this hydrolysis of the DNA stuff.

VM: You were presumably aware of the fact of the group being split between the two buildings.

LK: Oh sure.

VM: Did you get to know people in both places?

LK: Absolutely. Remember, other people undoubtedly have told you that we had weekly meetings so you knew what was going on in both (*places*). At that time, Vicki Haas (is that her last name?)...

VM: I think so.

LK: ...she was working over there, Al Bassham was there, Martha Kirk must have been there at that time. I wasn't working directly with them. Sure; we knew what was going on in ORL.

VM: When you arrived they were already in those two buildings, originally they hadn't been, they had all been in Donner, but by the time you were there they were in the two buildings.

LK: I think so. The downstairs (*of Donner*) was Gofman's group at that time.

VM: How did you progress in your own work in Donner? How long were you there?

LK: I was about two years, I guess, in Donner or two and a half and then some of us decided that — we had all these people visiting, postdoctoral years from Europe and everybody was going to Europe. So there were going to be four of us who went to Europe and actually, it ended up with just two of us going. At that point, that was about two years — Dave Kritchevsky, for example, his name must have come up.

VM: Yes; he's in Philadelphia at the Wistar Institute.

LK: Right, exactly. (*He had*) a wonderful sense of humour. That was one of the incentives: hey, let's go to Europe. As I say, two out of four of us actually did. When I came back and I wanted to work again, then it meant going over into the photosynthetic group which I really was very excited about.

VM: Did you actually have to resign your job in order to go to Europe?

LK: We did because we were doing the "Hey, we're going to stay until our money runs out." Which is basically what we did; we were over there about five months. We had enough money to get back and then we needed a job. The woman that I went with had worked for Gofman.

VM: So you came back and presumably went back to the same people.

LK: Yes: "Can I have my job back again?" And they said "yes" so I took it.

VM: Then what did you do?

LK: Once I got over into the ORL?

VM: Well, presumably whoever...Did you think you would go back into Donner, doing the same sort of things you had been doing before?

LK: I can't really remember that. I don't know. I just know I was very pleased to be in the photosynthetic group. Because, let's face it, I think that had a little bit more status. In Donner there were a number of small but not very coordinated efforts going on. Dot Mabee was working on carcinogens and there was this little DNA project going on and we were doing animal metabolism and that kind of thing. I think (*Eu*)gene Jorgensen, for example, was working in one of the labs., not the lab. that I worked in but on the second floor of Donner.

VM: If I can hark back to Donner for just a bit before we get to ORL: did people in Donner in the separate rooms each know what the others were doing?

LK: That was partly this community that worked for Calvin. Yeah; we had the meetings. Rapoport, for example, another faculty member, would sit in on these. Somebody would present what they were working on. I think we knew what was going on.

VM: During the day, in the course of your ordinary activities, were you always wandering up and down the corridor in and out of other people's rooms? Was it that sort of thing?

LK: No. I think you were doing your work where you worked. You had a lab. bench, you'd go down to the animal room, say, if you had to sacrifice an animal. So you knew what was going on elsewhere and you went elsewhere in the building but it wasn't too much of a going in and talking to somebody and then coming back.

CM: Was that different in ORL?

LK: ORL, of course, was kind of one great big open barn kind of place, rather than separate rooms.

VM: Did that encourage people, in your experience, to interact more directly?

LK: Probably, yes. Plus in ORL pretty much we were all working on aspects of the same problem. You had photosynthesis and I was doing the sugar degradations over there as were a couple of other people; we were all using the same chromatography room so you were in and out there. I think there was definitely more interaction in ORL than there had been in Donner.

VM: When you got into the photosynthesis group, at what stage were they and what did you start to do?

LK: I was pretty much on the sugar end of it; I was the sedoheptulose person. At one time I probably knew more about the middle carbon of sedoheptulose from very quick — you know from the first half second of photosynthesis — anybody else in the world: expert (*in*) a very small area.

VM: Rather important.

LK: I started on sugars and, of course, we separated the phosphates on the chromatography, the technique was already, you know it was just a technique that we used but almost each of the carbons in the 7-carbon chain had to be snipped apart by a separate method. Sometimes you'd get two of them together and then you'd get one of another (*sugar*) and your data wasn't as neat and clean as perhaps you might have wished it to be. But you were working with very low amounts of radioactivity that got in in this very short-term sugar phosphate.

VM: Did you develop the methodologies yourself?

LK: No.

VM: Where did they come from?

LK: Out of the literature.

VM: I see. There were established methods for doing this.

LK: Yeah, they were pretty much. For example, I don't remember exactly, but I think I had to make some aldolase from rabbit tissue at one time as the method of snipping off one of the carbons. I hadn't done it before; you know, it was right out of the literature. I would say that probably on things like that it was probably either Andy or Al that may have suggested where to look for these things. I have forgotten whether the people, wasn't it Ann that was working on the ribulose?

VM: Ann? Which Ann was that?

LK: What's her last name?

VM: Zweiffler?

LK: Yeah.

VM: The one who married Bert Tolbert?

LK: Yeah.

VM: I don't know.

LK: I'm a little dubious on these names. Whether she was using exactly the same methods that we used on the sedoheptulose, I'm not sure.

VM: Can you remember at that time, when you were doing those degradations of sedoheptulose and other people were doing degradations of other sugars, and,

presumably, day by day and week by week you were accumulating results and no doubt discussing them, was this the time when people were drawing schemes and trying to relate the information you had?

LK: Absolutely. I remember that in some of the early days the data that I was getting didn't fit with the two CO_2s coming onto the recipient at the same time. So, there was almost, you know, a little pressure: "hey, are you sure that this data is perfectly good?". I would say "to the limits of how good I know it is, it says this and not that". Yeah, schemes were always coming up and being abandoned.

VM: I'm trying to get a sense of the frequency with which you did this. Were you talking, as it were, every day, did this topic come up again: what's the latest today and what do we make of it, or was it less frequent than that?

LK: Certainly with Calvin it was much less frequent. With Andy, who was in the lab. all the time, it was more if I was having problems getting something. And each one of these degradations, you had to do the experiment in the lollipop and basically the things that I was working on were mainly the soybean things rather than the algae. So, you would do your experiment and then you had to do the extractions and then you would have to run your chromatograms and then you would have to do the X-ray things from them. Then you would have to cut them (*the radioactive spots*) out, elude them — elute.

VM: Elute.

LK: OK. See even with this time some of the technical language has certainly escaped me!

VM: But much of it is obviously still there.

LK: So it would take quite a while. Then you would have to have the correct — well, for example, when something had happened, was when for one of the degradations I had to have carrier sedoheptulose. So, we went out on a sedum collecting expedition and got a whole basketfull of sedum that is supposed, the particular genus of sedum, that was supposed to have sedoheptulose.

VM: Where did you find it?

LK: It was up near where Calvin's house was. I don't think it was on his lot but it was on somebody else's lot. We came back with the basket. That was kind of a fun process, to do the mashing, like making wine perhaps? Mash it down, boil it down, eventually crystallise it and then I had my carrier to put in with the sedoheptulose that had the radioactivity in it from the short-term photosynthesis.

VM: I guess those were the days before you could buy it commercially; you had to make it yourself.

LK: We certainly didn't buy it commercially. We made it ourselves.

VM: Did you know what sedum looked like yourself?

LK: That particular sedum?

VM: At the time when you needed to collect it?

LK: I know what sedums, the general kind of sedums. I don't think I knew what that particular sedum looked like. Remember, we had plant physiologists in the group, we had lots of people — that was the good thing about it. You had people who knew physics, people who knew chemistry, people who knew plant physiology and everybody brought these things together.

VM: Can I hark back just for one minute while I think about it? When you first started and you first had this talk with Calvin in Michigan, and you were a chemist and this was not the sort of chemistry you had heard about before, presumably — was it not a bit, what shall I say, ambitious, did it not seem a big jump to go from where you then stood into this new area?

LK: Sure it did. On the other hand, I think it was probably the kind of thing that I really had been hungering for even when I was in straight organic chemistry. In other words, getting closer to the natural world was definitely something that I think was more normal for me than making lachrymators.

VM: Had you done biochemistry before?

LK: I never had any biochemistry.

VM: Or biology courses of any sort?

LK: Only in high school. I had the interest in plants, was very strong, the interest in the out-of-doors was very strong. So the idea of doing something that was more related to that than straight chemicals. Perhaps if I had come a little different way, I probably might like to have gone into biochemistry. But, you know, at the time, when you make these decisions, chemistry seemed a very good place to go.

VM: OK; back to the sedoheptulose and the degradations at the time then. I recall that the earlier degradations had been of the hexoses and this had enabled people to get an idea of how the hexoses were formed. Then you were in the next stage, trying to work out this complex of what came next.

LK: That's right. That was both the sedoheptulose and the ribulose diphosphate which eventually turned out to be *the* acceptor.

VM: Who were the other people around you at the time who were doing degradations? Can you recall them?

LK: Anne, and I, of course Andy was involved...

VM: Al (*Bassham*) would have been an interested party, I expect.

LK: Absolutely. Certainly the method of doing the short-term photosynthesis with the radioactive CO_2, that was Al's set-up, and we were certainly working together on all of those experiments. I am not sure I know who else had been on that sugar degradation. The names are on the sugar degradation paper and you can look it up.

VM: That's the big paper, Path XXI, isn't it? I can't remember.

LK: There were the two papers, you know, that they cited in the Nobel, the one was the reservoir and that was the one that Alex Wilson was involved in and I'm not sure

again who else was working on that, and then the sugar degradations. I didn't do anything with the ribulose. I was strictly the 7-carbon kid!

VM: These kinetic experiments were being done, as you say, by Alex. Were you also there when Peter Massini did his (*experiments*)?

LK: Oh yes, absolutely, sure. That's a name I had forgotten.

VM: I have found him as well. He is retired and lives in Switzerland.

LK: Oh. Where does he live?

VM: Switzerland. Those people presumably were talking to you as avidly as you were talking to them about what was going on.

LK: I can't really remember that much about who you talked with about what.

VM: In the photosynthesis, in ORL, can you remember which room you were in, who your neighbours were?

LK: Sure. I remember the room, I'm not sure about the neighbours. I can see the room. I was in the room, the office, if you came in from the east side, the offices were to the right and the cold room was to the left. And then as you went through that kind of corridor you came into a big room and that's the room that I was in. There were a couple of aisles down there. I was on the left side of that aisle, for most things. You know, there was a rack...

VM: That was your home bench?

LK: That was my home bench, where I boiled down the sedum and stuff like that. But there were the racks where we did the original experiments; those were both in that room, there was a hood in the next room over. I couldn't place other people at the benches, though, who was actually where.

VM: You presumably spent a fair amount of your time running chromatograms which, I think, were upstairs.

LK: They were upstairs, yes.

VM: I can't remember where the dark room now was, where you put the things against film — must have been around somewhere.

LK: I'm not sure I can remember either.

VM: It might even have been downstairs in the basement somewhere with the counting room.

LK: I certainly remember the chromatogram room but I don't remember the dark room.

VM: Did you write some of the papers?

LK: No. I usually gave the data to other people who wrote the papers.

VM: We talked earlier on, before we started recording, about all the social activities that were going on in the lab. and you clearly had pleasant and strong memories of those sorts of things.

LK: Oh yes.

VM: What struck you about it as a group? You were one of the young people in a group of lots of young people.

LK: They were young and enthusiastic people and we spent time not only at work but much of our social life outside of work was with people from the lab. and it was usually outdoor activities — hiking in the he summer and skiing in the winter. We would take off often on, late on Friday night and get back late on Sunday and be back in the lab. It was a really wonderful sense of camaraderie that permeated both working together and playing together.

VM: Did almost everybody go?

LK: That's hard to say. No, not always. There were a couple of things that I remember once and I don't remember whether that was still — I think I was still at Donner, when we went as an entire group, including Dr. and Mrs. Calvin (Gen and Melvin came too) and we all went camping at Twin Lakes near Sonora Pass. That was really a fun trip and I think some of the people went mountain climbing, some went swimming, and so on. That was one instance I know when pretty much the entire group went. It was over some three-day weekend or something like that. Often it was smaller groups but sometimes a pretty large group. For example, there was kind of a standard climb of Mt. Lassen on skis on Memorial Day. One time from there we went on, some of us, up to Mt. Shasta and went up to (*that?*) Shasta. Rosemarie and Hans Ostwald, for example, went on a number of the ski trips. So there were different groups that went. Then there were some people that, I guess, weren't interested in that.

VM: When you went out on these activities, did you continue to talk science, as you remember, as you climbed the mountains, or...

LK: I can't remember that!

VM: I bet some of the time but not all of the time. Did people work all hours of the day and night in the lab.?

LK: I'm not sure that I really remember that. I don't think we kept 8-5 hours and if you were doing an experiment that needed to keep going, you would go. And there were probably some people that did more all-hours than I did. I was a 23-27 year old "kid" and I was very interested in the life that wasn't in school. I was really enjoying that part of working there. It was a wonderful group to work with, the science was exciting and after you had been in school for how many years it's been really great to do these outdoor things, that we hadn't been able to do before.

VM: Did you have lots of friends in Berkeley outside the group or were your friends mostly inside?

LK: I lived with a number of other young women and some of them were certainly on these trips, also interested in that.

VM: So when the group went out, the Calvin group went out, there tended to be other people who friends coming along...

LK: Sometimes, sure, both roommates and I had a friend who was up at Davis at the time and she would come sometimes. I would say at the time it was the Calvin group that was kind of the main group.

VM: Looking back on it now, and we'll talk a little bit about the contact you have kept with them, looking back to how it was when you knew it, did you find it a remarkable phenomenon? Had you experienced other things like that? What do you think made it work the way it did?

LK: I don't know. When I was a sophomore at UCLA I had worked for Dr. Dunn who actually was in biochemistry down there, doing...eluting out amino acids and checking their polarity and things like that. That certainly was not this kind of a group. I think I knew at the time that it was a very special group.

VM: What do you think made it special?

LK: The excitement of the project, the fact that everybody was young and interested in it, the fact that there were so many people coming from different, from Europe because of the excitement of the project, the enthusiasm of Calvin himself which, of course, was what had gotten me there. I think part of the unity of the group came from Gen who really provided warmth, a real warmth. For example, when our child was born, Gen was giving me motherly advice, how to take care of this baby and she gave me this lovely little figure, bronze figure of a little boy — here it is — and I thought that was the most charming gift to give to somebody who has just had a baby. Not something for the baby to wear or something; this is one of my prize possessions and Eric and I really enjoy it. That was the kind of warmth, I think, that Gen provided to people in the group.

VM: Did you see her often? Did you see Gen often?

LK: She would come to the lab. and there would be affairs where she was there. They didn't usually go on these "outings" kind of things except for the one time that I told you about. But there were certainly get-togethers. We knew her, yes.

VM: You said that she gave you that figure when you first child was born. Were you married while you were still working in the lab.?

LK: Yes. Eric and I met each other after I got back from the trip to Europe when I first really started working in ORL. He actually was a friend of the young woman that I had gone to Europe with. We met and very shortly decided that we would get married. We met, I guess, in August and were married the next February. Before I left the lab. I had my first child. I kept on working after that.

VM: Apart from the formality of getting married, was it duly celebrated in the lab. as well?

LK: I'm sure. Althea Van made chicken salad for our wedding and she was one of the dishwashers. On the other hand, the wedding was a very small wedding and there really weren't many people from the lab. who came to the wedding.

VM: I was wondering whether in the lab., apart from the formal arrangements you had made for the wedding itself, whether informally people were celebrating your

wedding, your marriage. I seem to remember it as a place where anything which could be celebrated was celebrated.

LK: I don't remember that particularly. I remember more getting ready to get married; I was more focused at the time on Eric than I was on things in the lab. I can't really remember that.

VM: Can you remember any other people getting married while you were in the lab.? Nobody has actually mentioned this in all of the people we have talked to. Maybe you were the only one.

LK: I don't know. I really don't remember. Alice (*Lauber, née Holtham*) was married after and Marilyn, I think, was already married.

VM: I don't know; I must ask Marilyn.

LK: I really don't know. Dick, I know, was married sometime during that time but he may have been married even at the time I was in Europe. I don't really remember. I was focused at the time on my life and the changes in my life.

VM: What about the other sorts of parties they had, Christmas parties? Which I seem to remember as a time when celebrations took place.

LK: I am sure they did but I can't give you particulars. I remember: there was certainly some kind of exchange of presents because I remember when Kazuo and (*indecipherable*) (*Shibata*)... he was the person that gave me something at one of these parties and it was a lovely hand-written little poem about eight by two (*inches?)* in Japanese on a gold kind of a rice paper of some sort. That was in ORL. In Donner I don't remember any parties especially. Maybe we didn't, I don't know. I remember that one party at least where we even exchanged gifts.

VM: Then at some stage you presumably decided to leave.

LK: Eric was going back to school.

VM: I see: in Berkeley, presumably?

LK: No, this was University of Washington. So that was the reason we left.

VM: What sort of contact have you kept with people since then?

LK: A number of them I keep in sort of...well, Alice, more than yearly, Dick Lemmon more than yearly, Ed Bennett Christmas card, Ed Melly (*spelling?*) Christmas card, and when I go to the lab. I see people...

VM: Did you get down there reasonably often?

LK: No.

VM: Is it special occasions when you happen to be in Berkeley, you drop in?

LK : It's more if I happen to be in Berkeley, sometimes I will drop in. But there are very few people there now that I know.

VM: During the years there have been people, over the years since the time you left until the present time...

LK: We didn't live near there. When I left, we were 3-1/2 years in Seattle and then we moved to San Jose and I had three kids and I was working and I didn't get up to Berkeley very often.

VM: I want to come back to the Berkeley scene but, now that you have mentioned it: briefly what did you do with your life after you left Berkeley? You said kids and you were working and moving...

LK: When we went to Seattle, I worked for Frank Huennekens in the Biochemistry Department. It was, I think, a 3/4 time thing (*i.e. job*) because I already one child and later on I had another. (*Note: Frank Huennekens was a former associate of Melvin Calvin in the Chemistry Department.*) So that was good; I enjoyed doing that. We were working on folinic acid, one-carbon metabolism still, but folinic acid and a number of other biochemical approaches to things. Then, when Eric finished with his doctorate, we went to San Jose and then we had our third child and I stayed as just a mother, I was very busy for about five years. As soon as the youngest one started to school, I in the meantime had gotten a teaching credential so I started teaching because that fitted very well with having three school-age kids. Then, we went to Berkeley again; Eric was on a sabbatical in Berkeley; while we were there I did some volunteer work at the Lawrence Hall of Science but I didn't work while we were up there (*in Berkeley*). I also worked in a special project of teaching math to, I guess you used to be able to say, economically disadvantaged kids in the San Jose area. It was a really fun project. As far as the teaching that I've done, I think that was the most rewarding kind of thing that I've done.

Then I taught some more at high school and then we had...Oh, in the middle there I also worked for three years for the US Geological Survey as a geophysicist...

VM: Was that in Menlo Park

LK: ...in Menlo Park. I was in two groups there: one was studying Alaska earthquakes and the other was analysing data in the attempt to get earthquake prediction. At the time they had what they thought was a pretty good predictor, it turned out to be a false goal. They certainly don't do that anymore. That work was fascinating. Then I guess I got down into teaching again and then Proposition 13 wiped out all the teaching jobs, so I got a job at Lockheed by selling myself as a programmer. At that time I had sent out three...I have done a lot of job searches, so I had three resumes: One as a teacher, one as a chemist and one, even though all I had to put on it was that I had worked at the Survey on some programmes, one as a programmer. I got a job as a programmer at Lockheed and I moved then from Lockheed to IBM as a programmer where I stayed for eleven years.

VM: That's a pretty a varied career...

LK: Very varied!

VM: ...one way and another.

LK: Jack of all Trades and Master of None.

VM: You said it, not me.

LK: Yes, I did!

VM: One of the points about which people have commented is the influence of ORL, the building itself, the way that place was set up, its openness, and so on. Did it strike you at the time and what do you think of it now?

LK: It was a thunky old building, I can say that.

VM: Was it nice to work in? Was it good for the work you were doing?

LK: The place itself had sort of a fallen-apart feel to it but the people were fantastic. The openness, I think, was very good. Yes; I really liked the openness much better than the newer lab. in Donner where, you know, you were in your own lab. and you went down a long hall to talk to anybody else. That part was good. It did have kind of a feel of a building that was falling apart.

VM: In the end, of course, it was encouraged to fall down and they built a new one, they built a round building trying to embody the philosophy of that.

LK: I know that building.

VM: How well do you think they did? You've been in there: how did it strike you?

LK: Just by the fact that it is so much bigger, I don't think you can get interaction with that many people. The idea behind it, I think, was great. Now it was quite a small group when I was in ORL because that was pretty much in the early days. I think you really knew everybody in the group. The times when I have gone to the Bio-Organic Building, it seems like kind of any other building. There are just lots of people scattered around and I don't see how anybody can really know everybody that's working tin here. I don't know whether that's true or not. I have never worked there.

VM: Well, of course, if you do work in a place and see people every day then you do get to know them better than if you just look in.

LK: Oh sure; I don't think you can evaluate that.

VM: What's the fisherman story?

LK: There certainly was the fisherman story, and you are going to hear about everybody.

VM: But tell us.

LK: It was a lot of fun because we had a lot of fun with it. Alex Wilson was writing up the work for the paper which was going to be both his thesis work and the paper in the JACS. There was a diagram in it which involved the steady-state bubbling of the gases through the reservoir. The reservoir looked like a beaker, as it was originally drawn, a square beaker, with bubbles in it. Alice Holtham (at that time; [*now*] Alice Lauber) was doing the drawing and, of course, it hadn't been reduced yet. Just for fun, I don't remember who got the idea of putting the fisherman there but once the idea was there some of us really said "go for it". So the fisherman was placed sitting on the rim of this reservoir and one of the bubbles was converted into a fish and the fishing line, he was catching that fish. We all thought that was very funny. Then, of course, there was the pressure: put it in the paper, put it in the paper! And, again, I'm,

not sure who-all knew that this was actually going into that paper but it got past the two referees and it's in the JACS. It's probably the only humorous thing that was ever published in JACS. I was glad to have been there when it happened.

VM: And your name is one of those on the paper, isn't it?

LK No, not on that paper.

VM: Oh, not on that paper. That's right: that's Alex.

LK: The was the other. You know, the two things that came together in the Calvin.

SM: You mention Alex, this was Alex Wilson, I take it.

LK: This was Alex Wilson. He had been a graduate student and this was the work for his doctorate. The paper, I don't know whether it went into his thesis, but it certainly went into JACS. I have the reprint somewhere to prove it.

VM: Yes, we've seen it. It is there, as you say, it's very small, once it was reduced.

LK: Once it was reduced it was very small and I can see how the referees didn't notice it, but it's still kind of funny.

VM: The fisherman story apparently had repercussions, as your husband Eric will tell us.

EK: Recently at Stanford one of the graduate students in the group that I am associated with found out that I had been to Berkeley earlier. It turned out that the way we identified both his family's affiliation and my affiliation with Berkeley was through his father. The way that came up is that he essentially said "If you knew my father, then you surely know about the fisherman story". Of course, I immediately said "indeed I know about the fisherman story". That clearly identified Bruce as Alex Wilson's son, the very same Alex Wilson that worked with Lorel at an earlier time.

(*Tape turned over*)

VM: In this lab., clearly many people had an influence on ideas and so forth and Calvin, I guess...do you feel was the main source of originality?

LK: No, I certainly don't. I think Andy had a great deal on some of it on the smaller issues. Somebody like Al. I never worked really closely with Dick Lemmon but I would say there was a great deal of originality there. But on the big issues, I think that where I heard the big theories come from was from Calvin. I think I mentioned earlier than when I would take my data over, sometimes I had to argue that the data was right even though it didn't fit with, say, this dicarboxylation theory that I think was published actually before I went to the lab. I was bringing in data that didn't agree with that and he tried to talk me out of it and I said "no".

I made use of this later on when I was teaching high school because to me it seems that it's really a measure of greatness to have good ideas, to test it, and if it doesn't fit, you abandon it. This is very hard to get over to kids who want something to be right or wrong and not to stick their necks out. I would tell this to kids who were in my classes that I once worked for a guy that eventually got the Nobel Prize and he published something that was absolutely wrong. Then, what did we do? We got more

data and then he published something that is now actually in your biology book, because it was in the book I was teaching from.

VM: Presumably there were lots of false leads at various times in the lab.?

LK: Certainly there were, at the lower level where we were doing things in the lab.; everything that you do didn't always work. Obviously, I mean that's part of science. Then you would do it again in another way and eventually you get data that you feel is accurate enough to use, to draw more conclusions from.

VM: When you observed Calvin in discussion about ideas, some of which may have come from him, although, no doubt, he defended his ideas, was he willing to listen to good arguments against them?

LK: Obviously, he did; sure, obviously. We did have these weekly meetings, you know, and everybody would be there and some people would be more vocal than others. I guess I mentioned that Rapoport would be there and sometimes they would even argue about what was going on. Definitely; obviously he was open to new ideas but he was willing to jump ahead. There for a while he was very interested in the origin of life kind of things and some people were doing some experiments to see what kind of things were there. Obviously that's still an open question. So everybody still has to have an open mind as to how it may really have happened.

The neat thing about Calvin is that he was willing to think about these very universal questions. He was able to do the physical chemistry and the physics. Some of the people, and I didn't interact very much with them, were really much more interested in the quantum part of the photosynthetic use...the ATP and the energy end of it, let's say that, and I didn't follow that that much. He was certainly able to have ideas in that area as well as in the path of carbon biochemical end of it.

VM: He might have been quite unusual in not using his authority to push through ideas which were untenable and would be more influenced by argument, I think, than perhaps some people of similar calibre might have been.

LK: Could be.

VM: That's the sort of story one might...

LK: That's what we sort of feel that some of the discussion...

VM: Well, thank you very much for telling us all this and for inviting us to stay among the redwoods of Mendocino. It has been pleasant.

LK: Well, you've been very welcome.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name LOREL LIU KAY

Date of birth April 9, 1926 Birthplace Los Angeles, CA

Father's full name Paul Harold Daus

Occupation Math Professor Birthplace Chicago, IL

Mother's full name Daphne Harriet Fortney

Occupation Librarian Birthplace W. Virginia

Your spouse Eric Kay

Occupation Physicist Birthplace Heidelberg, Germ.

Your children David, Erica, Andrew

Where did you grow up? Los Angeles, CA.

Present community Mendocino, CA

Education AB U.C Berkeley; MS U. of Michigan
Teaching Credential San Jose State Univ.

Occupation(s) Biochemist, Teacher, Geophysicist,
Computer Programmer

Areas of expertise

Other interests or activities

Organizations in which you are active

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...

PROFESSOR RAPOPORT DID NOT WISH TO COMPLETE THIS FORM
The following information was obtained from "American Men and Women of Science"
HM.

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name HENRY RAPOPORT

Date of birth 16 NOV 1918 Birthplace BROOKLYN N.Y.

Father's full name

Occupation Birthplace

Mother's full name

Occupation Birthplace

Your spouse

Occupation Birthplace

Your children

Where did you grow up?

Present community

Education MIT (SB 1940); Ph.D (org. chem 1943)

Occupation(s) Professor of Organic Chemistry (retired)

Areas of expertise Alkaloids; heterocyclic compounds, natural products, pigments, biosynthesis

Other interests or activities

Organizations in which you are active Am. Chem. Soc.

Chapter 22

GUS D. DOROUGH

Livermore, California

July 5th, 1996

VM = Vivian Moses; GD = Gus Dorough; SM = Sheila Moses

VM: This is talking to Gus Dorough in Livermore on July 5th, 1996.

What was your background, before you went to Calvin, and how came you teamed up with him?

GD: I think I should start back in the year 1941 was when I arrived at Berkeley. I had gone to San Diego State College for the first two years and the practice in those days for people interested in science, if they couldn't go to Berkeley originally, went to Berkeley in the last two years; you would take the first two years at a state college or perhaps UCLA or whatever. In any event, I went to San Diego State and then transferred in what was my junior year to Berkeley.

It was a very different time, then. The College of Chemistry was still very much G.N. Lewis' College of Chemistry. The faculty were pretty well along in years. Many of them had come with Lewis, so that was before World War I some time, and Calvin in a sense kind of stood out because he was a young faculty member and very out-going and very easy to approach. It isn't that some of the older ones weren't but they struck a note of awe in the minds of a typical undergraduate of that time. So, I arrived at Berkeley and I think, perhaps that very first year or at least shortly thereafter, I took a course from Calvin; I can't even remember the title of it but it had at least one unique feature. I remember we did literature searches, original literature searches, and developed knowledge about some subject not by taking out a textbook but by actually going to the original literature and writing up a report on that and giving the report verbally in his class. It was then critiqued by Calvin.

VM: So the classes, presumably, were not too big.

GD: That particular one was pretty good size but, as I remember, we broke up in teams. Everybody didn't give a verbal talk but everyone was involved in the literature searches. I'm not sure that's all we did in the course. I am sure Calvin lectured as well and he struck me then as a very, very good lecturer; very easy to listen to.

VM: You were going to major in chemistry?

GD: Yes. To back up a little bit: in high school I had a couple of brothers who were physics and chemistry teachers — I still remember their names: Gilbert — and they somewhat steered me to science curricula; I wasn't quite sure whether physics or chemistry was what I wanted. But I was pretty sure I wanted one or the other. And at San Diego State I had again good mentors and good teachers so I sort of got oriented more into chemistry. When I transferred to Berkeley, I was pretty sure chemistry was what I wanted so I entered the College of Chemistry there.

I really didn't see much of Calvin. The war came along, of course.

VM: This had been before the war when you first got there?

HR: I started Berkeley in August of 1941. In December of 1941 the war for the United States started in earnest and everything got quite changed, quite immediately. I recall that the first thing that happened was that we were informed that we could probably continue in school on a student deferment because of our field, that is science, but school would be continuous.

VM: Can I ask a question? What was the position with the draft at that point?

GD: That's why I say a student deferment would allow you to get through to graduation.

VM: You had already registered with some body, had you?

GD: Yes, I was registered. When the war started, I went down, perhaps naively, but I thought "gee, maybe a naval aviator" would be a good thing to be.

VM: Out of San Diego, why not?

GD: I applied but was quickly rejected for childhood asthma and other complaints that they said wouldn't work well for them. So, I was just in the draft and given a student deferment as a science major which was sort of standard at that time. I would have graduated with a bachelor's in June of '43. Because of the continuous classes I got out in February 1943 at which time, again because of the war, the College of Chemistry invited me to stay on as a graduate student. Up until the war that was, I think, quite not the practice. They sent their students away to other schools, got good students in from other schools for the graduate courses.

The College of Chemistry might have been regarded as a little bit ingrown in the sense that all of the faculty members that Lewis appointed starting, I don't know, again way back before World War I sometime, were all UC graduates. I didn't know this until later but Melvin was sort of the first exception since before World War I. I think his thesis professor might have been a Berkeley graduate, I'm not sure of that. But he was certainly a graduate of the University of Minnesota and he was appointed to the faculty. He was the first non-UC appointment for a long time. That was kind of interesting. I gather he stood out because of his just general friendly, outgoing characteristics — easy to talk to and he was quite a younger member of the faculty. During my senior year I remember I did work with two of the older members, C.W. Porter and Thomas Dale Stewart, both of whom are long since gone now, I guess. They were so-called organic chemistry professors. I think everybody at the College of Chemistry was really a physical chemist by design and you might sort of dabble in organic chemistry on the side. My recollection was that it was certainly not a strong school for classical synthetic organic chemistry as you found in the big mid-western

schools. It was really a school of physical chemistry, or chemistry as a whole, and you could sort of emphasise organic if that was what you wanted.

VM: That suited you well, didn't it?

GD: Yes. I was sort of deciding whether I wanted to be a physicist or chemist, anyway. And Melvin, of course, I guess I would initially describe him as a physical chemist but with a strong bent for looking into all kinds of other things. When I graduated, as I say, I did some work, so-called individual honours work; but as a senior you could work with a faculty member and do a little research project. I think I did one with Porter and I know I did one with Stewart and quite enjoyed that. When I graduated and they told me I could stay on as a teaching assistant if I desired, I asked Melvin if I could come with him as a PhD. candidate. He agreed. We didn't really start much in the way of research work because your first year you are still pretty busy taking courses and then lots of other things sort of got in the way. I got quite ill in the fall of '43: I had a lung abscess, pneumonia and all kinds of terrible travails which put me in Cowell Hospital, the hospital on the campus, for some months. I think I got out early in '44. Not too long after that, it was suggested strongly that I should go up The Hill to the Radiation Lab. and go to work there. Which I did and then that was, of course, the Manhattan Project.

VM: And that was not with Calvin?

GD: No, it was not with Calvin. I was now a full-time employee of the Radiation Lab. doing war research work on the atomic bomb and still was registered as a graduate student and was still at Berkeley. I came down in the evenings and did some research work. But it was a very mixed-up time. Melvin was heavily involved in — he had a group of people, I have forgotten all their names — Ferguson...

VM: Was Martell one of them?

GD: Gee, he might have been. They were studying chelates of various kinds.

VM: Yes; Branch. Was Branch one of them too?

GD: No. Branch was, of course, a full professor. He (*Calvin*) had written a text with Branch. I don't recall Branch being involved in that project but he may well have been. Branch had kind of, my impression, was although a very bright man, he had sort of "retired". He spent a lot of time at the Faculty Club playing bridge. I took his course in theoretical organic chemistry, which he and Calvin had written together, from Branch — not as inspiring a lecturer as Melvin. He was certainly a very typical chemist in his own right. In any event, Melvin was heavily involved in these chelate studies, I think to try to find an oxygen-carrier, something they could basically package oxygen up in a solid state rather than putting it in tanks, using chelates for that purpose. He had quite a large group of people working with him. I wasn't involved directly in that although — I've forgotten what it was — he did ask me to look into the synthesis of a chelating material and I did that for some period of time, unsuccessfully: never was able to devise a route to this.

VM: Had you decided what your thesis title was all about?

GD No, it was still kind of up in the he air. Because of the war and because I was now working full-time on the Manhattan Project which, in due course, sent me to Oak Ridge and then down to Los Alamos; you know, it was a very mixed up period. I

wasn't around Berkeley very much. I did manage to basically complete all the course work I needed to take my prelims. for the doctoral examination and get that out of the way. But then I was off at Los Alamos.

I returned in February of 1946 and at that point I had a National Science Foundation fellowship. As I say, I had really completed my course work and I had my prelims. out of the way, I had financial assistance from the National Science Foundation so I had a year ahead of me at least basically unfettered just to do research.

VM: Can I ask you a question about the earlier period before we move away from it? Did you know Sam Ruben?

GD: Yes. Not well but I was there when he died.

VM: You remember the occasion of the accident?

GD: Very vividly. I don't know whether he was a close compatriot of Melvin's or not; I just don't know that. It was a very tragic event. I think it was sometime probably along in '43. Sam was working with phosgene of all things, quite a large quantity, and we had a lot of phosgene, I remember, in the storeroom, little glass vials made in Germany, and I think one of those vials broke in a Dewar of liquid nitrogen. Of course it boiled right up in his face. They brought him out on the lawn: he was quite mobile and kept wanting to get up. I remember Latimer came out and just insisted he lay still until they could get an ambulance or something, gurney down from Cowell (*Hospital*). They took him to Cowell and within a couple of days he died. That was a very vivid memory.

VM: Were you aware of the photosynthesis work that he had been doing before he was diverted onto war work?

GD: No, I was not familiar with that.

VM: Or his discovery of C^{14}?

GD: I knew of that but I didn't really know Ruben well except to know that he was very well regarded. He was quite an up and coming young faculty member.

VM: How about Kamen? Did you know him?

GD: Yes, I did get to know Kamen, quite well as a matter of fact. Kamen was on the project at Berkeley and I got to know him there. He ran into all kinds of security problems which you probably know about. To jump ahead a minute, when I graduated with my doctorate I went to Washington University in St. Louis and taught chemistry there for some years. Kamen also went to Washington U. in the Medical School. In the forties and fifties I got to know Martin very well, just as far as a friend; we never collaborated. He was a very nice gentleman.

VM: Anyway; back to your return to Berkeley.

GD: Finally, after '46, the war ended, of course, in late '45, August '45 and I stayed on at Los Alamos cleaning things up and left in '46. Melvin said, "OK, let's go to work". I was basically free to do that. It was a most exciting year. I probably will remember it as one of the highlights of my life, if you will. I did do some voluntary teaching, just as a teaching assistant but not with pay, just to sort of do that. Other than that, I

worked in the lab. I remember Melvin saying, "well, we won't try to describe a detailed route to your ultimate thesis but we have some areas here to explore. Let's explore them, let's see what kind of develops and we won't try to pick a specific topic or a specific thing now". That seemed reasonable because we had some interesting areas to work in.

The area that I sort of eventually concentrated on was dealing with the system of porphyrins which is the basic ring structure of chlorophyll and the iron haemin and haemoglobin. I guess, I don't know — Calvin had several graduate students; Sam Aronoff was one; Russ Ball was a master's candidate; and there was a man at Antioch College, in Silver Springs, Ohio, I believe, that had explored the synthesis of tetraphenylporphyrin; the phenyls are hooked on the carbons that hook the pyrrole rings together The reaction of benzaldehyde and pyrrole leads directly to this porphyrin and in fairly good yields. Rothman had found two major porphyrin-like species and he thought they were the NH-isomers. If you have two hydrogens in the middle they could be either of two configurations which, on theoretical grounds doesn't seem to make sense. They should exist easily and separably. I think that intrigued Calvin some and he and Aronoff looked into it and decided that really it was a porphyrin and a chlorin: one of them was the porphyrin tetraphenylporphyrin and the other material that Rothman was thought was an NH-isomer was, in fact, the chlorin molecule which is the one with two extra hydrogens. It's the basic ring structure of chlorophyll. Here are these two very clean porphyrin structures, one the porphyrin ring and then one with two extra hydrogens like chlorophyll, fairly easily made and with lots of interesting spectral and other properties to study. They kind of represent the basic ring structures of important biological materials. Obviously, Melvin was interested in photosynthesis; it kept coming up in conversations. But he had not started the carbon work then. This came, I guess, shortly thereafter.

The short of it is: we discovered a reaction that would transform the chlorin quantitatively to the porphyrin, take the two hydrogens off. The reaction was the chlorin molecule with some quinone — napthaquinone, I believe it was o-naphthaquinone — and quinone gets converted, the hydroquinone, picking up those two hydrogens off the back of the pyrrole ring of the chlorin molecule. The reaction, which occurred only in light and turned out to be independent of the naphthaquinone concentrations. It depended only upon the light intensity and the concentration of the chlorin, which was kind of puzzling. How could it be independent of the quinone?

It turned out eventually what we discovered was lots of evidence that the light excitation which was in the furthest red band of the chlorin (nothing else absorbing light), that transition takes you from a ground singlet to an excited singlet for the chlorin molecule. That excited singlet fluoresces, comes back down to the ground state. Or, some of it can get diverted to a triplet state which G.N. Lewis had studied. He was the first one to propose that phosphorescence really results of a triplet-singlet transition which is basically a forbidden transition, so of low probability. If some of the excited singlet can get over by just non-radiative processes (collisions, or whatever), over into this triplet state which lies normally below the singlet state, excited singlet state, that you can then kind of trap this molecule in that triplet state. This trapped molecule either can get brought back to the ground state by non-radiation processes (collisions, or whatever) or, if you put it at very cold temperatures which is what Lewis did where the collisions are much less, it just sits there until it decays by radiation transfer. So, a light comes out and that is phosphorescence. This was Lewis' proposal for phosphorescence which I think is presumably widely accepted now.

What we found was that this actual chemical reaction was taking place through the triplet state. That was a reactive species. It was independent of the quinone, as long as there was enough quinone around, and it could snag off one of these triplets before it was knocked off by some other process; didn't matter how much quinone was there. The whole key was simply to excite this molecule and get the triplet that reacted quantitatively with the quinone and made the hydroquinone. That was really a fun piece of research I got to because of its closeness to the work that Lewis did. I had several long conversations with Lewis. He was always held in such awe, and considered to be a little bit of a curmudgeon if you made dumb comments to him, but I found him extremely kindly and supportive and very interested in what I was doing, suggestions of what I might try...

VM: So he was aware of research going on in the department outside his own immediate area.

GD: Oh, yes. I think so. Partly. you've heard of the famous colloquia which he chaired right up to his death. Those were held once a week, I've forgotten the afternoon now but many people can tell you. Lewis sat in the same chair, all the time, up in the front of the room, facing the blackboard, smoking his cigars. They always had two talks: one given by usually a graduate student reporting on perhaps his own research or perhaps even something from the literature, and then one of the faculty members reported. They were pretty awesome. I gave several talks before that (*group*) as a young graduate student. I still remember it, that's something!

VM: Did Lewis interrupt you to ask questions?

GD: Not frequently but he would often ask questions. You started this by saying was he (*Lewis*) knowledgeable about what was going on in the department. That was one mechanism. He had such a broad interest in things, no matter who was talking or what they were talking about, he usually had cogent remarks to make.

VM: I have two questions. One of them: were you still in Calvin's group when his own group began to have their Friday morning seminar series?

GD: No. I had left by then. I left in February '47.

VM: I see, OK. And the second thing was: by the time left you, you would no doubt have seen this — Calvin's group by 1947 was already becoming a distinct entity. Was Lewis interested? Did Lewis interact with that group?

GD: Now, he died in 1946.

VM: I see; OK.

GD: Yes; he died in March of...That's another thing I remember but not terribly well. I came back in February of '46 and a fellow graduate student, Paul Gilles I remember, came to my lab. late at night (I don't know: eight or nine in the evening, or something) and said did I know that Lewis had died that afternoon? I didn't. He died in his lab. of a heart attack.

VM: When was it that you had seen him (*Lewis*) in these seminars?

GD: My earlier graduate years. I started graduate work in '43 and so I saw him then. I think I actually might have given a research seminar on some of the honours work I did with Stewart or with Porter. I had several times in the barrel.

VM: I would just like to reconstruct the scene in Lewis' seminars because it's very reminiscent of Calvin's own seminars. You say Lewis sat at the same position at the table...

GD: Always, yes.

VM: ...this was a long table running the length of the room?

GD: It was a somewhat shorter table because it was a room in Gilman Hall (it still must be there). The faculty sort of sat up in the front chairs — can't remember; they sat around the table; I think they just sat in the front chairs — and behind were all the graduate students and younger faculty. People were free to ask questions. Sometimes somebody would ask a rather foolish question. That was very dangerous with Lewis there. He might make some rather pungent remark about it and I guess that's where he got the reputation of being a little bit of a curmudgeon. I don't know this first hand. His reputation was he didn't suffer fools very gladly.

VM: Nor did Calvin!

GD: Nor did Calvin. In fact, I want to talk about that. That's also his (*Calvin's*) reputation and I worked with him closely for a year, I made some dumb mistakes, did some foolish things. Never was chastised in any mean way. If it was clear he wasn't happy with what I had done, he would sort of get a serious mien on his face that was clear that this was not going the way he wanted it to. But there were never any unkind words. In fact, it was always the reverse. There was encouragement and sort of recognition that humans are frail and you do blow things now and then, but let's get on with it. It was strong encouragement to do better and not anything that was mean or cutting. That year was just a fun year. I would describe it as lots of humour. I wish I could remember some anecdotes but I can't remember a single one but I remember we had a fun time together.

VM: Where was it? Where were you working?

GD: I worked in the Old Chemistry, a little room right on the south-west corner of the old buildings (it's long since gone now) and on the lower floor. My recollection is that Melvin would come by, if not every day, certainly every other, every third day at the most. We would have a good conversation. He just wouldn't dash in and dash out. He'd come in. He'd want to look at results, he'd want to talk about what I was doing now, he would make a ton of suggestions — if I did them all I would never get any sleep ever. You had to kind of pick and choose, you know. If you picked the right ones and they worked out, that would obviate having to do any of the others anyway. It was a good exercise to try and figure out, of these many things he might tell me about next time, which one should I zero-in on?

I would describe him as just a great mentor. He was supportive, he was important to you, he had a great curiosity about physical things, about chemistry. We'd also...you'd digress and talk about all kinds of other subjects as well. He was just a friendly, outgoing guy, had a great smile, as you know, a dimple. He would sit in my office and do this gesture which I guess he has done all his life, he would sit in

seminars and do this, maybe in the seminar you described, that's probably what he was doing.

VM: playing with his hands.

GD: That was a very familiar gesture. I guess what I am trying to say: I remember that year with Calvin, that's really the only time I worked closely with him. It was just a great year. Of course, he had a lot of other students and a lot of other activities going on, and he had probably begun the move over to the Old Radiation Lab. where his group got started and ensconced before I left, but that's 50 years ago and I just can't remember.

VM: I should have come around 20 years earlier and done this, but it wasn't possible at that stage. You really don't recall any contact with the people in ORL.

GD: I knew Dick Lemmon and Bassham, Jim Bassham, they were fellow graduate students. I think they graduated somewhat behind me. I graduated in February '43, I think they were a little later. I knew that he (*Calvin*) had got on this idea of using carbon-14 to track the path of carbon in photosynthesis but I was just happy as a clam with my little porphyrin problem, which was very peripheral to photosynthesis and yet it was studying some very basic physical chemistry of a very fundamental ring structure.

VM: As a resident in the Old Chemistry Building you were not drawn into activities with the people in ORL.

GD: I'm not so sure he had moved into ORL. Do you know when that occurred?

VM: It was sometime in '46, I think, maybe later. There was also a group in Donner, of course; in the Donner Lab. He group started in Donner. Lemmon certainly and Bennett. Do you remember Bennett?

GD: Yes, Bennett.

VM: They certainly started in Donner and indeed stayed there until the round building was opened. The photosynthesizers, the carbon people, moved into the Old Radiation Lab. which Andy Benson set up for them and that must have been — Andy's told us...

GD: Benson was one of those who worked on the chelates too, I think, during the war. (*Note: Benson did not work on the chelate programme during the war. He was away from Berkeley as he was a conscientious objector.)* It was a Navy-sponsored research programme or something. He had several...have you heard of Lloyd Ferguson?

VM: No.

GD: (*Indecipherable*) He was a graduate student of Calvins, about my vintage, a black man: this was in a time when the US was much more racially divided than it is now. After graduation he had a terrible time, Lloyd Ferguson did: he had a PhD, very well qualified, getting a job somewhere. He wanted to teach; he finally got a teaching job in a small southern university which I think was very difficult for him. I think he ended up going to Washington, DC. There's a very well known black school there, name I can't remember (*probably Howard University*). I think he eventually came back to the west coast. He wrote a theoretical organic text, somewhat patterned after Branch and Calvin, but, I guess, had updated with newer things. I have lost total track

of Lloyd, but I remember Lloyd as being one of the people in that chelate group. Benson was another (*no; this is in error*).Sam Aronoff, I don't know whether he stayed around to work in that area or not; he was another early graduate.

VM: He was contemporary with you, more or less.

GD: Yes, well, I followed in his footsteps in a sense because Aronoff did some of the early work trying to ferret out what this porphyrin and chlorin were and, as I mentioned, Ball and Aronoff elucidated the porphyrin and chlorin structures. I don't know whether that was published or not. I remember trying to get enough of the porphyrin and chlorin that we could actually run carbon-hydrogen analyses. They're not very different because in that big ring structure, adding two hydrogens doesn't change things very much. We did do that and, as I recall, the results were quite consistent with the fact that chlorin did have two extra hydrogens, a hydrogen count that was higher. But I don't recall that we ever published that.

VM: In addition to the people that Melvin had in Donner and ORL, whether or not that coincided with your tenure, about how many students did he have with him in Chemistry?

GD: I would think during my career there were very few but it began to build up because of this carbon work.

VM: For or five: that sort...

GD: Half a dozen people.

VM: Were you a coherent group among yourselves? Were you socially friendly?

GD: We were socially quite friendly and we would sort of talk about our work with each other but we didn't have collective seminars just of his group at that point. I think it became much more focused, you know, when it became this path of carbon activity with quite a few people involved, really a focused activity with a very clear aim of where you are going. It wasn't a sort of individual PhD thesis type of research.

VM: Before that, in your period, it was much more like the usual arrangement between the professor and a group of students each one of which is partly independent...

GD: ...each one which is sort of working a somewhat separate area but all along Melvin's interests have always been very broad, that has been my impression. He had Aronoff and Ball working on porphyrins and chlorins and I kind of followed. I think after I graduated he might have had some other graduate students looking at that same system because it is an interesting system, if you will. He had these folks doing chelate work. I don't know what Benson's thesis was; I presume on the chelates.

VM: I don't remember whether he told us or not but clearly we could ask him. *(It was with Carl Nieman on the structure of sphingosine.)*

GD: He (*Benson*) was a sort of transition. As I recall he was working on the chelates, the oxygen-carriers, and then he was involved, as you say, in the early set up of the lab.

VM: He was also involved with Ruben and Kamen in the earlier period before Ruben died. He was involved in some of that work.

GD: Where did Benson go? I sort of lost track of him.

VM: After he left Berkeley?

GD: Yes.

VM: I can't remember the exact sequence: he went to Penn State and then he went to UCLA. But for 33 years now, or so, he has been at the Scripps Institution of Oceanography in San Diego. That's where we saw him, hale and hearty and very lively still!

GD: I just remember him mostly socially, a very nice helpful fellow. I think he was a year or so ahead of me. It was still, I think, up through the time I left it hadn't quite ceased to be G. N. Lewis' College of Chemistry. The students there, even as undergraduates in the College of Chemistry, were unique. They didn't take the same courses as those in the College of Liberal Arts did (*Dorough might here be referring actually to the College of Letters and Science*). In fact, you could actually get a degree, a BS in chemistry, and take practically nothing else but chemistry, physics and mathematics, which was kind of unique. Some people think that might not have been a good idea.

VM: The College of Chemistry wrote its own rules for degrees?

GD: Yes. It was a totally separate college. If you wanted to, you could take a chemistry degree, a BA degree in chemistry, in (*actually the College of Letters and Science*) but then you had other requirements which the College of Liberal Arts laid down — history, English and a much broader spectrum of courses. You came out from the College of Chemistry a very well educated chemist but you might not have a had a very broad education.

VM: (*Noise of aeroplane flying overhead — this conversation was being recorded outdoors.*) I think you might have to repeat that when this thing has gone away! So you were saying you came out of the Chemistry...

GD: Where's it gone?

VM: It's gone behind the house.

GD: Horribly loud!

VM: You were saying, you came out of the College of Chemistry...

GD: I guess it was a criticism of the time that you might graduate from the College of Chemistry and be a very well educated chemist because you've had basically chemistry, physics and mathematics for four years, very well prepared for any graduate school in the country. But you might not be a very well-rounded individual in the sense that your academic training was somewhat narrow.

VM: That's exactly the system that I grew up under in Britain, it's exactly like that, very concentrated and very focused.

GD: You gain your appreciation of other fields and other activities by some other osmotic process.

VM: By talking to students in other areas, that's part of being at university. You don't just derive...lectures.

UP TO HERE

GD: In that regard, as an undergraduate at Berkeley, if you were in college in the College of Chemistry you got to know your fellow students pretty well, it wasn't such a huge mob. Particularly in the junior and senior years the classes were smaller, you got to know those awesome professors pretty well. Although they were quite a bit older in years and had these big reputations they were friendly people.

VM: Where you worked as a graduate student in Old Chemistry, were you in a room by yourself?

GD: I shared it with Russ Ball until he graduated and then it was mine, just a little cubby-hole down there. The things we did in a lab. — there were no hoods and I had all these organic solvents I was dealing with the whole time — benzene...I don't know; God took care of us! We survived just fine. Occasionally terrible accidents like Sam Ruben's happened but you were kind of responsible for your own safety. You knew that. The things you were dealing with were generally toxic and had to be treated them with respect.

VM: Were the other members of Calvin's group in Chemistry close by in adjacent rooms?

GD: We were scattered up and down the bottom, the first floor — it was kind of a basement floor of the Old Chemistry Building. His office was up on the first floor where there were at that time organic chemistry labs. So, we had a stairway to climb to go up to his office.

VM: Were you equally in communication with graduate students working for other professors or was there any particular sense of cohesion in Calvin's group at that time?

GD: Yeah; you knew your fellow graduate students. You went to the colloquia with them, you went to courses with them, you got to know them socially in various ways. I got to know Mike Kasha pretty well. He was G.N. Lewis' last graduate student.

VM: So the graduate students were a body among themselves and were not divided up into professorial groups.

GD: No, we were certainly encouraged. I got a lot of help from Mike Kasha and from, I think, McClure (*spelling?*).

VM: How do you spell Kasha?

GD: K-A-S-H-A. He's a professor at Florida or Florida State, I forget now. He came...I'm digressing all over the place.

VM: That's fine.

GD: There's a G.N. Lewis annual luncheon, supposedly for compatriots of Lewis or people who were in the College of Chemistry in Lewis' time, so that rules out pretty much any students after 1945. It's kind of an "old mens' club" now but we have a luncheon every year, slightly dwindling group. Melvin always comes. I have noticed,

unfortunately, a considerable decline in his health since he's come to those. Actually, it's very sad to watch. Back to the point then: Mike Kasha was the featured speaker at the last meeting and he just gave a talk about his remembrances of Lewis and the late times. He also wanted to clarify — there's a lot of rumours about G.N. Lewis' death, that he committed suicide or some crazy thing. Kasha is firmly in the belief, and I think all the evidence at the time would certainly support him, that Lewis died of a heart attack. He happened to be in the lab. at the time.

So, then I guess, what I have tried to say that I had this marvellous year with Melvin as a sort of mentor and friend, compatriot and colleague and PhD advisor, etc. Got to know his wife, Gen, a little bit; she was a most charming lady. They had just been married rather recently, I think.

VM: I must stop you there; it (*the tape*) is just about to run (*out*).

(Tape turned over)

What sort of contact have you maintained with him and with the other people since those days?

GD: Well, as I guess I indicated earlier, I went off to Washington University to teach chemistry in 1947. I maintained moderately close contact. I came back to Berkeley and worked in the Old Radiation Lab. I did a series of experiments with oxygen-18...

VM: When was that?

GD: About 1949, perhaps; something like that.

VM: How long were you there for?

GD: Just a summer leave, from Washington U.

VM: Oh; we'll have to explore what you remember of *that* period in a few minutes, at any rate.

GD: I don't remember too much about it except that I worked in the Old Rad. Lab. and Bassham and all those fellows were there. Everybody was busy chromatographing on paper, two-dimensional paper chromatography. We had a little sample of water, of O^{18} water, didn't have much O^{18} in it, it was not highly enriched. We did some experiments, I guess mixing algae with water and we were going to try and find where the O^{18} went. We suspected that it ended up in some carotenoid-type, I mean carotene-type product. We isolated the carotenes and looked for oxygen-18 in them. It appeared enhanced in both dark and light, so the experiments weren't very conclusive and the oxygen-18 sample wasn't really rich enough to do very definitive experiments. We wrote up a little, tiny short note, kind of saying this is a teaser, probably could be some fruitful work done here but this is what we did and it was inconclusive because we didn't really have the right tools. It might be something that someone might want to explore at some future.

VM: You worked in the main lab., in the big lab., in ORL?

GD: In the Old radiation Lab. I think I came back one other time but I can't remember what the heck I did! It seems awful but I must not have led anything very specific.

SM: I just wanted to check on which time you were working...

GD: My guess is that it was about 1949 but I'll have to...I think I can find the paper. (*It was The Path of Oxygen in Photosynthesis in the Journal of the American Chemical Society, 73, 2362 [1951]*).

VM: I've seen the paper. That's actually how I found your name. I hadn't heard of you before until quire recently when I came across that paper and asked "who is this guy"? He's writing from this lab. and I didn't know him.

GD: Well, that was just a very short contact; I was there two or three months during the summer. When I came out on things like that we tried to pick some, you know, rather narrow little topic and in a couple of months you might be able actually to accomplish something. That was a neat little project. The oxygen-18 sample was too small and not enriched enough to do anything definitive.

VM: How long were you in St. Louis?

GD: I was there about seven years or so. Like many small privately-endowed colleges, the support of chemistry and physics became an increasing problem. I was a pre-World War person, if you will; my idea of academic research was you taught at a university and you did research of your own choosing, let your mind explore what you will. That was the great tradition. When chemistry and physics departments cost a lot of money, it gets harder in a small liberal arts college to support that. The short of it is that contract research began to appear in ever-increasing amounts. The physics department began with heavy Navy contracts. My department said we've just got to have some support. I ended up doing basically contract research with grants. I was supporting all but $1,000 of my salary, all my graduate students, supplies, equipment; I was a little business ensconced in a university. I became, frankly, disenchanted with that. If I want to do contract research, maybe I just go do contract research. I had worked on the Manhattan Project during the war. What became the Lawrence Livermore National Laboratory was starting up out here; I had invitations to come. So I came and looked and decided to return to California.

VM: So you've been here for about 40 years.

GD: I came back in '54. The last part of my Washington U. career I wasn't doing anything similar to what Calvin was doing. We had some discussions, I remember, on the structure of bacteriochlorophyll which has two more hydrogens in it. But I can't remember: I think he had a graduate student who worked on the spectrum of such materials or something. Seely (*Gilbert N. Seely*)?

VM: I don't know. There were periods, of course, when I wasn't there.

GD: This was back more in the early fifties.

VM: That would have been before my time. I don't remember any such person. There's a lot of stuff about his work that I didn't know about.

GD: He maintained an interest in this porphyrin... Initially when I went to Washington U. I continued some work on these basic porphyrin structures. We looked at the N-H isomerism, we looked at the spectra, we looked at chelating — they are great chelators, they add a base like pyridine; drop, you know, make a strong pyridinate complex. We made tetrahydroporphyrins, did a lot of interesting things.

VM: Since coming to Livermore you haven't been involved with that sort of thing at all?

GD: No. I was strictly involved in mostly high explosives research.

VM: Do you get to Berkeley to visit the group there often?

GD: No, I never returned really after my couple of visits in the late forties or early fifties. I never went back.

VM: You've never seen the round building?

GD: I've never been inside the round building.

VM; I'm afraid it's beyond its heyday because it's no longer a building occupied by a single group. But, it's an interesting concept and, as someone who once lived in ORL...The idea, of course, was to try somehow to recreate the atmosphere and the climate in a modern building. That building is now 33 years old but you ought to go sometime and see it.

GD: I've heard of it, the structure and design so that people almost have to interact.

VM: That was the theory. It start out...and people really feel it did that at the beginning. You are still in touch with people like Marilyn Taylor, are you? Do you know Marilyn Taylor?

GD: No.

VM: Calvin's secretary. No — perhaps; she was '48. I think sometime when you are going to be in Berkeley you should ring up his secretary, she certainly knows who you are, and say you'd like to have a look at the building, just for the hell of it.

GD: I haven't even seen the new big chemistry building. At the G.N. Lewis luncheon, which is held in the old Faculty Club...

VM: Well, that's very close.

GD: ...they usually offer a tour and, unfortunately, I had another engagement this last time and had to dash out.

VM: Next time perhaps you will go.

GD: Perhaps we should go now (*to lunch*); we can easily talk some more, transcribe some more.

VM: That's very helpful and very interesting what you have said and I'm very glad we could get together and talk in this way.

GD: It kind of predates your main line of endeavour here.

VM: The whole project is really...

GD: It's part and parcel of a whole.

SM: Of a history...

VM: Of course, Melvin is the focal point. OK; let's leave it there. Thank you very much.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Gus Downs Dorough, Jr

Date of birth 5 March 22 Birthplace Los Angeles, CA

Father's full name Gus D. Dorough

Occupation Banking / Insurance Birthplace Grand Saline, Texas

Mother's full name Lois Jo Downs Dorough

Occupation Housewife Birthplace

Your spouse Raehem J. Dorough

Occupation Real Estate Broker Birthplace Los Angeles CA

Your children Susan, Lisa, Daniel
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Where did you grow up? Los Angeles Area

Present community Livermore CA

Education B.S. - Chemistry - Univ of Calif - Berkeley
Ph.D - Chemistry - Univ of Calif - Berkeley

Occupation(s) Retired

Areas of expertise Physical Chemistry

Other interests or activities

Organizations in which you are active Am. Chemical Society

Chapter 23

ALICE (HOLTHAM) LAUBER

Seattle, Washington

July 7th, 1996

VM = Vivian Moses; AL = Alice Lauber; SM = Sheila Moses

VM: This is talking to Alice Lauber, who was once Alice Holtham, on the 7th of July, 1996 in Seattle. Alice, how did it happen that you joined Calvin and his group?

AL: Well, I was fresh out of business college and the University of California had advertised for secretaries. I interviewed at two different places on the campus and took the job in the lab.

VM: Where had you been living at the time?

AL: I was living in Oakland.

VM: Are you a Californian?

AL: Oh yes.

SM: What year was this?

AL: That's what I don't know. Certainly, it has to be in the '40s but I don't know the year.

VM: When you go there...well, let's start with the interview. Who did you talk to in the interview?

AL: I talked to Bert Tolbert and I think I talked to Andy (*Benson*). I mainly remember talking to Bert.

VM: And so you didn't meet Calvin at the interview itself?

AL: No.

VM: When did you meet him?

AL: The first day of work, of course.

VM: What happened — what was it like?

AL: I don't remember that at all. He came through, I'm sure. He was much more concerned with the research that was being done than who was in the office.

VM: Did you know anything about the group you were joining?

AL: Not a thing. Chemistry was my worst subject — I don't know how I ever ended up in a chemistry lab.

VM: It wasn't a matter that concerned them that you didn't know anything about chemistry?

AL: No, it wasn't. Bert asked me if I liked to ski and I said, yes, and I was hired.

VM: Yes, that does seem to have been an important aspect of why people got hired. So you joined the group and...where were you working?

AL: In ORL.

VM: In ORL, itself?

AL: Right

VM: Whereabouts in the building, do you remember?

AL: Well, yeah. When you come in the door and you turn to the left and then turn to the right and went through a small office into the office where I was working.

VM: Were you alone in that office?

AL: Well, not the whole time.

VM: But it was an office, presumably, which was not that segregated from other people. Was there a lot of throughput of people wandering in and out?

AL: Oh, yes, because that office fed right into the lab.

VM: So from where you were sitting could you see people working there?

AL: Oh, yes. And, in the first little office that we went through. there were people using that as an office. That's where Kazuo Shibata was and there were other people in there.

VM: I see. You were the only secretary there at the time?

AL: Well, the only secretary in ORL.

VM: Who else was there?

AL: Marilyn was in Donner.

VM: She was already there when you got there, was she?

AL: Yes.

VM: Alone? Was she the only secretary in Donner?

AL: I think she was alone there. And then Norma Werdelin worked there and there were some others, too, that came in.

VM: Did you spend all of your time in the group in ORL?

AL: When Marilyn, I think it must have been when she had her children, I would go up to Donner and work for Bert.

VM: I guess that was the main office, was it? Because Bert was really administering the whole thing, wasn't he?

AL: That was the main office, that's right.

VM: Did Calvin have his own secretary, as you remember, over in Chemistry?

AL: As I remember he didn't. As I remember it was Marilyn and I did secretarial work for him, too, when Marilyn wasn't around or when she was too busy.

VM: So when you got there, which was sometime in the latish '40s, you think, probably, who was there?

AL: Well, it was the earlier '40s, I think. No, couldn't have been: must have been the latish '40s. Who was there? Gosh.

VM: Well, who comes to mind?

AL: Al was there and Andy was there; Vicky...

VM: Lynch?

AL: ...Lynch was there. I can't...

VM: People like Murray Goodman. Was he there already when you got there?

AL: As a student, he might have been.

VM: Alex Wilson?

AL: No, Alex came later.

VM: So it was really quite a small group, was it, when you first joined?

AL: It was. And I don't remember what foreigners might have been there.

VM: Did you get the impression that the whole building was full of people or was it not yet...?

AL: It wasn't. There wasn't anybody upstairs. There was a glass blowing shop there and that was about it.

VM: So you were all in the main section on the lower floor there.

AL: Yes. There were two labs.

VM: Did they have at that time those underground rooms where they did the counting?

AL: Yes, the counting room. I spent many an hour counting down there for Andy.

VM: Did you do counting, yourself?

AL: Oh, yes.

VM: Secretarial work wasn't restricted to typewriting.

AL: Secretary was just sort of a word! I did the drawings and arranged the parties, took care of the symphony tickets.

VM: I didn't realise that all these things went on. That's a very liberal form of secretarial activity. And how long did you stay there?

AL: I left in 1955, in winter of '55. Probably I started in the early '50s because I wasn't there in '49.

VM: You weren't there when Melvin had his heart attack?

AL: Yes, I was.

VM: That would place it. I think that was latish in '49.

AL: I think I was because it was just a part of the life there so I... I'm sorry my memory is very poor. I know that I went over to take dictation from him one day and I got half way down the stairs from where he had his office and I heard this terrible noise coming from him. And I thought, oh gosh, he's had a heart attack, and I went racing up the stairs and all he done, he had forgotten to tell me something and he was calling out! I was sure it was the end.

VM: As a secretary presumably you would work more regular hours than some of the other people?.

AL: Yes, oh I did.

VM: Did you used to get in at eight in the morning or something of that sort?

AL: Yes.

VM: Were other people in as early as that? Did you unlock the doors?

AL: Some. Sometimes Andy was there very early and sometimes he came in later.

VM: The others would drift in, presumably, during the course of the day?

AL: That's right.

VM: In terms of the social interaction in the building, there was the big white table people talk about. Was that there when you were there?

AL: Oh yes. That was funny one day. We had Alice and Altha as the dishwashers...

VM: That was Alice Smith, was it; she was a black lady?

AL: Yes; and Altha Vann. Kazuo had worked all night and had stretched out on the big white table (*to take a nap*). And Alice came in — she must have been the first one there because she was sure he was dead!

VM: And you used to gather with everyone else for coffee time. What used to happen at lunch time — what did you do for lunch?

AL: I usually went out; I mean I ate my lunch and then went out for a walk or went to the Avenue (*i.e. Telegraph Avenue*) or things like that.

VM: Doing your own errands, not necessarily together with other people.

AL: That's right.

VM: Were you married at the time?

AL: No.

VM: Did you tend to socialise with other people in the lab.?

AL: Oh yes.

VM: In the evenings?

AL: Oh yes. It was a family. They worked together all day and then all partied together on the weekends when there was a party or everybody would go to the symphony or everybody would...That was the unique part about it.

VM: And that was commonplace for people to do that?

AL: Yes, I would say so.

VM: And you were part of the organisation that fixed things up?

AL: At times, yes.

VM: Who was the stimulus for getting people to do this — how did it happen?

AL: People would just...When the party was the Calvin's, of course, Mrs. Calvin would call me and said find out how many can come and get it all arranged. But otherwise things just happened. Of course, there were always the trips. Everybody went on trips together — I don't know if they did when you were there?

VM: Yes. Those weekends trips, usually to the mountains and places like that; the seashore, maybe?

AL: Death Valley was usually the first trip and then we would go up to Lassen and then we'd go skiing to Yosemite in the winter.

VM: So there was the annual cycle of trips.

AL: Yes.

VM: And the Christmas party, as I remember: was that in your day, too?

AL: Yes.

VM: In ORL, was it?

AL: In ORL.

VM: Were you there at the start of the Christmas parties or were they ongoing?

AL: I think they were ongoing because they already had the eggnog recipe, the famous eggnog recipe. Some of the Donner women did that.

VM: How did you find the relationship between the people in Donner and the people in ORL? Were they very much the same gang, do you think, or were they separated a lot?

AL: They weren't always at the parties. They were separated. They weren't as together up there as we were.

VM: Did you ever work in Donner?

AL: Only to help out when Marilyn wasn't there.

VM: And people in Donner, as I remember, were in separate rooms, weren't they, up and down a corridor more or less.

AL: More or less, yes.

VM: And did they tend to stick inside their rooms to a greater extent than the ORL people?

AL: Well, of course, ORL was a big open room so it was different.

VM: When it came to writing papers you, presumably, were part of the process of turning thoughts into publications.

AL: Turning *words* into publications not thoughts.

VM: Obviously that was before the days of computers and even the days before photocopiers.

AL: Pretty much. I was there when we got a photocopier and it was a very laborious process.

VM: One of these terrible wet, smelly things, wasn't it?

AL: Well you had to run copies through like photographs, put it through all the solutions.

VM: So you were one of the people, anyway, who had the job of multiple typing of a manuscript to go through the various stages and carbons and all the rest of it.

AL: That's right.

VM: Did everybody write papers? Did Calvin write papers, himself? Did you deal with him as a paper writer?

AL: No, I don't think I did; I think Marilyn did that. His name was on every paper but the people in the lab. that I worked for would write the papers.

VM: And they would present the stuff to you in handwriting. Difficult to cope with?

AL: Sometimes, particularly the foreigners.

VM: All the foreigners or just some of the non-English ones?

AL: Well, you know, some of the penmanship is different to ours because it was all hand-written.

VM: Presumably the people were fairly amenable about corrections.

AL: Well, they had to be.

VM: And did you have to go through many copies of producing these papers before they were finally sent off?

AL: At times.

VM: You were also doing the drawings, you mentioned?

AL: Yes.

VM: So you did graphs and things like that?

AL: Uh huh.

VM: And there was this famous drawing in which you were involved.

AL: That's right. Alex's.

VM: We've heard it from Alex's point of view but as one of the artists involved what's your...?

AL: I didn't have any artistic training, any drawing training, and that was probably the most difficult one to do because it was so complicated and so involved.

VM: Oh, you did that whole complex drawing of Alex's?

AL: The whole thing. You've heard of Alex's couple of days of running his research — everybody in the lab. was involved — that was set up right outside my office door.

VM: So you were aware of all this activity going on.

AL: I was one of them.

VM: Oh, you were one of them.

AL: One of them; he had to use everybody that was there to operate it.

VM: Right. And then you drew the equipment. Was it your idea to put the fisherman in?

AL: Well, I think it was because that drawing just about finished me and I had got to do something. And, of course, it went all the way through the reviewers; it passed all the reviewers.

VM: Nobody spotted it?

AL: Nobody spotted it and just before it was sent off to print I went up — I finally couldn't stand it any longer — I went over and pointed it out to Dr. Calvin and he said "yes, he had seen it: didn't bother him a bit!" But Eric Kay's (*husband of Lorel Daus Kay*) professor here at Berkeley (*this should be "Seattle" — see below*) evidently didn't like it.

VM: Lorel was one of the authors, was she, on that?

AL: No, I don't think so.

VM: When you say "Eric's...?

AL: Eric came up to University of Washington to do his graduate studies (*in chemistry*) and his professor here objected to it, Eric found out.

VM: It's so small, it is very difficult to see in the actual printed version.

AL: Yes; you have to really look at it.

SM: You have to know it's there.

AL: You do.

VM: I think that's, perhaps, the original joke in the ORL papers. There weren't actually too many.

AL: No, there weren't.

VM: There should have been more but that was a good one. You were never tempted to do it again in some of your other artistic efforts?

AL: It wasn't the right kind of set-up for it; I mean, that was perfect for a fisherman.

VM: Did you have fishing in your background somewhere?

AL: No!

VM: Well, it certainly turned out to be very nice. But just to explore it a bit more because this is clearly a story we are clearly going to have make much of: it was your idea to put the fisherman in?

AL: I think it was. Alex and I...Alex was right round joking with everybody and whether he came up with it or I came up with it, why ....

VM: So even you had he would certainly have agreed.

AL: Alex? Yes, of course!

VM: (*Question apparently omitted*)

AL: Whenever I couldn't find something in the files, and had to tell Andy that I couldn't find, it he would say, "I think it's home under the bed," and he would bring it back at lunch time.

VM: Oh, I see. He used to read...

AL: He would take work home and leave it there.

SM: You should see his (*Andy Benson's*) filing cabinet in his office now. It's full of things which many years ago he took home. Vivian will tell you what sort of things.

VM: Well, he has, in addition to all sorts of papers, there are many relics he took home — bottles of various dubious solutions — which he pulls out of his filing cabinet and says, "Oh, it must have evaporated in 40 years!" It is a long time since Andy left there but that's what he's got and he hints, I have never actually seen this, but he hints that he has stacks of stuff hidden away elsewhere.

AL: I wouldn't be surprised.

VM: Things like chromatograms and stuff like that. Among the people who came through when you were there — you were there four or five years or something about that...

AL: About four, I think it was.

VM: ...there were presumably lots of foreigners coming through?

AL: I should say, yes.

VM: You remember some of the ones you dealt with while they were there?

AL: Rod Quayle, Malcolm Thain, Jean Bourdon, the Italian fellow whose name I can't remember (*perhaps it was Franco Mazetti*), Peter Massini, Arnold Nordahl, Alex (*Wilson*)...

VM: So there were lots of them.

AL: Lots of them. In fact, Norma Werdelin, the other secretary from the other office, and I got a crazy idea to take five of them to a football game and it was very exhausting trying to explain football to people who understood all different languages.

VM: This was presumably American football.

AL: American football, yes. One of the Cal games.

VM: I remember my own difficulty the first time I was taken to a football game.

AL: Well, multiply that by five and we had a real problem on our hands.

VM: But most of the foreigners who came, I think, probably spoke English reasonably well, didn't they?

AL: The Italian, what was his name (*Franco Mazetti?*), had difficulty. It was obviously in the fall when football was played and, I think, all of them had just come.

VM: So it was all very novel to them.

AL: Very novel to them, yes.

VM: When they first arrived, were you one of the people who helped them settle in?

AL: Yes.

VM: Did you have to help people find apartments or places to live?

AL: Well, sometimes or meet them at the train or try to get things together for them — furniture sometimes — and tell them where to find shopping and other things.

VM: Was there not some sort of lab. supply of furniture?

AL: No, there wasn't. But it seems to me, if I remember correctly, there was something on the campus, through I-House or something, where they could borrow things.

VM: Was it difficult to find places for people to live at that time?

AL: It's always difficult to find places which people can afford.

VM: Yes. Because I guess these guys probably didn't have too much money, did they?

AL: No, not really. Of course, the single ones lived at I-House.

VM: Oh, so some of them came with wives.

AL: Yes. Dr. Calvin preferred them to have their wives there.

VM: Preferred them to have their wives there?

AL: That's right. Encouraged them to send for their wives.

VM: Why do you think he did that?

AL: I think he just liked the idea of family. Maybe he thought they'd be happier if their wives were there.

VM: He didn't think they might work shorter hours if their wives were there?

SM: You mentioned that Gen used to be in touch with you about setting up the parties in their home. Can you tell us something about what you feel her part in the group was?

AL: She was definitely the person to relate to everybody and to make everyone feel welcome — the scientists that came in. Dr. Calvin was the scientific person and she was personal person.

VM: He was always friendly to people, wasn't he?

AL: Yes, but it was always on a...he always wanted to get back to science.

VM: He was friendly on a professional basis, as it were.

AL: Yes. That's kind of humorous. He would come in first thing in the morning — some of the people weren't even there — and say, "Well, what are the results, what results do you have?" You've probably heard of Paul Hayes.

VM: Yes, Indeed.

AL: Finally Paul made a little flag and when there were results he would raise the little flag.

VM: Where was this flag?

AL: Right next to Paul's desk.

VM: His results or anybody's results?

AL: Well, Paul wasn't doing...didn't have results but he would know what was going on. Just to lighten the mood a little bit.

VM: And did Calvin take any notice of the flag?

AL: I don't think so.

VM: I wonder if he knew what it meant.

AL: I don't know.

VM: So he used to come in regularly, did he?

AL: Every morning.

SM: I don't think he missed much.

AL: No, I don't think so either.

SM: He probably enjoyed it.

AL: And he was always worried when we would all go off on a trip.

VM: Worried about what?

AL: That something...He would come in very early on the Monday morning afterwards to be sure everybody made it all in one piece.

VM: He would check you all back in.

AL: ...check us all back in.

SM: He was concerned for your safety, presumably.

AL: Yes, or whether the research was going to go on or something.

VM: Did he ever go on those trips himself?

AL: No.

VM: I remember how strenuous they were for someone who's not an outdoor type. You presumably are an outdoor type, are you?

AL: Yes.

VM: You found it easy to do, then?

AL: Yeah. Well, it was wonderful...well, it was a wonderful place for the foreigners because they never had to feel as outsiders and were always welcome to be able to be taken out to all these places and see what was there.

VM: What about parties — indoor parties, evening parties or weekend or picnics?

AL: I think there used to be a summer picnic for both labs. but I don't think it went on for too long. I vaguely remember that. There used to be parties in people's homes.

VM: Were people generally, do you think, friendly with one another, people in the lab.?

AL: I think they were. There was a lot of give and take around.

VM: Was it your impression that the people who worked together also spent time out of work together?

AL: Yes, they did. The skiers, of course, skied together and the hikers hiked together.

VM: So it was pretty much a tight social group?

AL: That's the feeling I had.

VM: When you left there, presumably you began to work in other places, did you ever find anywhere like it?

AL: Oh, no. It was very unique. I worked for years in one of the engineering departments here ...

VM: Here in Seattle?

AL: Yes...and they barely spoke, certainly never said "good morning".

SM: How do you account for this sort of social set-up?

AL: I think maybe one reason was that everybody was about the same age and there were a lot of single people at the time; and I think that makes a difference.

SM: But on the other hand, you say many of the foreigners came with their wives and they, too, were made welcome and joined in.

AL: Yes, they were.

VM: So did not too many people have their own kids at the time?

AL: No, not too many.

VM: So they weren't tied up, particularly, with domestic arrangements?

AL: That's right. Clint (*Fuller*) had kids, was having his family then, and Louisa (*now*) Nishitani and her (*then*) husband, Rich Norris had their family when they were at the lab. because Rich replaced Louisa when she got pregnant.

VM: Louisa lives, actually, not that far from here but it's not going to be possible for me to get to her.

AL: Well, it's about three hours, anyway.

VM: Yes, but it's not a thousand miles away. Anyway, I gather it wouldn't be very convenient for her anyway, right now.

AL: That's right.

VM: So I'm not going to get to see her. But you are friendly with her?

AL: Yes.

VM: What did she do in the lab.?

AL: She was a botanist and she did the algae, she took care of the algae.

VM: Was she there when you got there?

AL: No.

VM: Did she stay after you left?

AL: No. She was gone by then.

VM: I see; so she was there really a fairly short time.

AL: Her husband came to get his Ph.D. in California and Louisa came to work at the lab. And then when she got pregnant was just when he was finishing up so he came and replaced her in the lab.

VM: And that was Rich Norris. He was one of those guys, was he, who stands in the picture with the deerstalker hats?

AL: Yes.

VM: He's a tall fellow, is he?

AL: No, not that tall.

VM: Maybe he was just standing on one of the upper steps.

AL: Might have been. Or the other deer stalkers were short, I don't know!

VM: You remember that, do you?

AL: Yes, I do now. Malcolm (*Thain*) had one and I think Rod (*Quayle*) might have had one.

VM: Yes, Rod had one but he said his grandchildren had wrecked his. But I've still got Malcolm and Clint to see and they might have them.

AL: Well, I hope Rod still has his cowboy hat; he was give it.

VM: We have a picture of Rod in his cowboy hat which he was given on the way out.

SM: Which is still in pristine condition, absolutely perfect.

AL: That means he hasn't worn it!

SM: No, he has. It is obviously a very good one and has kept very well. His grandchildren get to it but it has kept well.

AL: I think that somebody in the lab. (*it might have been Rich Norris*) went to the south-west and bought it. They were going there on a trip and were commissioned to pick one up.

VM: Well, he certainly treasured it for the last umpteen years and he's got it now.

SM: Had you organised his farewell when he left?

UP TO HERE

AL: Yes.

SM: Tell us about it.

AL: I don't really remember it, actually.

SM: You all went to the train station and pretended they were just married?

AL: Probably.

SM: And threw rice all over them and this was the occasion of the deerstalker; no...

AL: No, the cowboy.

VM: Was there generally a going-away party for people when they left or was Rod a special case?

AL: Rod was a special person, really. I think we probably did something for everybody when they left.

VM: You mentioned when the tape recorder was off a few minutes ago...you asked whether I was going to see Hans Kornberg. Hans Kornberg must have some memory for you, then. Does he? Do you remember him well?

AL: When Hans left, you see, Rod and Yvonne and I went with him down to Yosemite, to Death Valley and to Grand Canyon. He left us at the Grand Canyon (*to drive on to New York*) and we took the train home back (*to Berkeley*).

VM: He wasn't in the lab. for a very long time: over a summer, I think?

AL: Yeah, it wasn't very long.

VM: Have you tended to keep in touch with people that you knew at that time?

AL: Not really, no. Lorel and, of course, Louisa. And I did see Hans in England once and I saw Leonard Poel in Scotland once. Grant (*Buchanan*) took me up to Cambridge to meet Rod before Rod came over because Rod had some questions.

VM: Oh; before Rod ever arrived you were in England?

AL: Yes.

VM: I see; I hadn't realised you had been there at that time. You don't get down to Berkeley very often?

AL: No, now that my parents are gone so I don't get down there.

VM: But you have been to some of the reunions?

AL: Just the one.

VM: There's just that one?

AL: Yeah.

VM: Of course, many people who were part of the group...there is a core that sticks together.

AL: Yes, there is.

VM: I guess the people who live there.

AL: The people the certainly who worked at the lab. until they retired.

VM: Right, and they still gather every Wednesday morning. But clearly, people who live further away find it more difficult.

AL: Well, I mean I wasn't part of the scientific part of it — it's a little different with me.

VM: You've never been tempted to become a chemist?

AL: I told you: chemistry was my worst subject.

VM: Well, a biologist, you know: whatever you want to call it.

AL: Found it fascinating, actually.

VM: Since you spent your time working in ORL and you would have imbibed the spirit of the place and you visited the round building subsequently, how well do you think the round building was able to embody the spirit of ORL, bearing in mind that times were different and it's a modern building and all the rest of it? Do you think there is any remote connection between the two?

AL: I didn't get the feeling that there was although it was all open and people could interact. But then, I don't know...sometimes it is just the people that make things work and I don't even know whether it has worked. I haven't heard.

VM: Some of the people were, of course, the same people although necessarily they, themselves, were older and the times were different. It is an unfair question but if I were to say to you that ORL was to be pulled down and there was a chance of building something else, what does one build? Very difficult to...

AL: That's true. It is also hard to build the spirit. The spirit just has to be there.

VM: Yes. But did you get the impression while you were there that the building played an important part in formulating the spirit?

AL: It probably did because it was kind of ramshackle and it wasn't pristine enough to scare people to keep them in their rooms.

VM: The people in Donner who were, after all, part of the same general organisation, in many ways were similar people, you felt didn't have this same pulling together sense.

AL: I don't feel they did.

VM: I wonder whether that might partly have been because Donner was a much more conventional building not given to the sort of community activity.

AL: Well, I don't think the Donner work was as interesting to Dr. Calvin as the ORL work and that might make a difference. There was certainly a spirit of things happening in ORL.

VM: That's right. Since you left there what did you do?

AL: Well, I came up here to Seattle and...

VM: Were you married already in Berkeley?

AL: I got married and came up here because my husband was from here. I worked on the (*University of Washington*) campus, I worked in the Admissions Office, and then went into Materials Science and Engineering and eventually worked for the Associate Dean of Engineering and then he became a Chairman of the Department and worked there until I retired.

VM: So all of your working life essentially has been spent in an academic environment.

AL: That's right.

VM: Have you had any regrets?

AL: No, I think it is a great place to work.

VM: Had you not been going to get married, or had your husband-to-be been in Berkeley, would you have been content to go on working in the place where you had been working or was it time to move on anyway?

AL: I think if I had gotten married (*even to someone in the Bay Area*) I might have moved on because the life in ORL was so consuming and when you get married you have to have other interests and considerations.

VM: So while you were there, if I may pick you up on that, while you were there you mean it really dominated your life as well as just the 9 or 8, 8 to 5, whatever it was?

AL: It did, it certainly did. Socially as well as everything else.

VM: Many of your friends were from the group?

AL: That's right.

VM: And presumably that's was because you wanted it that way. These were congenial people for you.

AL: Yes. They were doing things that I enjoyed doing with them.

VM: And the fact that you were not yourself a scientist...did you feel to any degree significantly cut off from what they were chatting among themselves?

AL: I found it very interesting to listen to them and there were always results coming out that were exciting to the scientific community, particularly when Calvin gave the seminar on DNA. That was very exciting because everybody was so impressed.

VM: When you joined the group, what sort of group did you think you were coming to?

AL: I had no idea. I was just going to work!

VM: You weren't warned about what sort of people you might come in contact with?

AL: Well, the Personnel Manager, when I was hired, said, "Now, you have to remember that they are just little boys." And I guess I sort of became almost a den mother.

VM: But you knew they weren't actually little boys — what did it mean to you when he said that?

AL: Well, that they had to be taken care of.

VM: I see; they knew their size but had to be taken care of in the unknown. Were they...were they little boys like that?

AL: Well...I think all men are still little boys!

VM: But among men in general they weren't worse than any body else?

AL: Oh, no, they weren't worse than anybody else. And I would much rather have them than some of these others that are posing all the time!

SM: The Personnel Manager probably recognised you as somebody who would respond to people in that way, anyway. He wouldn't have said it otherwise!

AL: Either that or it was a warning!

VM: Well, I think the sort of secretary they needed to have in a group like that had to be someone who could, herself, mix easily and socially with these people. If you had been remote and stand-offish, it would have been as difficult for them as for you.

AL: It would have been, especially since I didn't necessarily do secretarial jobs; I did anything that came up.

VM: So it needed someone with a fair degree of flexibility and understanding and willingness to muck in with other people. That's fair enough.

AL: It makes a much more interesting job!

VM: Absolutely. But I'm not sure the Personnel Manager didn't get it right: (a) in telling you and (b) in hiring you. I think that probably was entirely appropriate.

Thank you very much for talking to us. I have to say for posterity that it's a marvellous day here in Seattle. It has been well worth the trip to come up and sit on your patio and chat like this.

AL: Well, I'm glad you could come on a day that is marvellous. It doesn't always happen.

VM: So I gather; thank you very much.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name ALICE LAUBER

Date of birth 5/19/27 Birthplace PALO ALTO CA

Father's full name OSWALD A. HOLTHAM

Occupation JEWELER (DEC) Birthplace ENGLAND

Mother's full name ELIZABETH HOLTHAM

Occupation HOUSEWIFE (DEC) Birthplace SCOTLAND

Your spouse ERNEST LAUBER

Occupation TEACHER Birthplace SEATTLE WA

Your children NA

Where did you grow up? OAKLAND CA

Present community SEATTLE WA

Education 1 YR UNIV OF CA

Occupation(s) SECRETARY

Areas of expertise

Other interests or activities HORTICULTURE

Organizations in which you are active HORTICULTURAL SOCIETIES