

# INTRODUCTION

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**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  $\text{C}_6\text{H}_{12}\text{O}_6$ . 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  $\text{CH}_2\text{O}$ . Just a little simple arithmetic would solve the problem: six molecules of formaldehyde, each one of them  $\text{CH}_2\text{O}$ , would somehow combine to give one molecule of glucose which was  $\text{C}_6\text{H}_{12}\text{O}_6$ . 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<sup>1</sup> 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<sup>1</sup>, 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<sup>2, 3</sup> 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<sup>14</sup> 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<sup>14</sup> 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<sup>2</sup> 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<sup>3</sup> 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<sup>4, 5</sup> 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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