Chapter 33

SIR HANS L. KORNBERG

Boston, Massachusetts

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VM = Vivian Moses; HK = Hans Kornberg

VM: This is a conversation with Hans Kornberg in Boston on the 26th of July, 1996.

Hans, what was your background at the time when you first went to Calvin and why did you go there?

HK: Well, I first met Calvin when he attended a conference in Sheffield which had been convened by my dear teacher and friend, Hans Krebs, in 1949 or 1950. At the time I was just a beginning graduate student — it must have been 1949 because Calvin was then on his way to the first Congress of Biochemistry in Cambridge. As part of the Sheffield meeting, Hans Krebs had commissioned me to take photographs of all the notable participants and I took numerous photographs of Melvin Calvin and it was on the basis of this, and of hearing his paper at Cambridge, that I decided he was a man I would very much like to work with, albeit briefly. In 1953 I was awarded a Commonwealth Fund Fellowship and found myself working, for the first of the two years of the Fellowship, in the laboratory of Professor Efraim Racker at Yale. Racker decided that one year at Yale, or at least *that* year at Yale was sufficient and he would really like to return to New York, where he had been working for many years, so he accepted, in l954, the position as Director of one of the divisions of the Public Health Research Institute of the City of New York, Inc. My Fellowship allowed me, no required me to travel for at least one and preferably more months between years one and two, and given the option of either travelling and seeing the United States at the expense of the Commonwealth Fund or lugging bottles and lab. equipment from New Haven to New York, I naturally chose the latter (*Editor: he surely means "former"!*). So I wrote to Calvin saying that I would be passing in the direction of California in May of 1954 and could I possibly spend some time working in his laboratory to learn these new and exciting techniques in return for which I would be prepared to offer my new-found expertise as an enzymologist and perhaps enable him to decide the question which he had posed publicly, namely, whether the cleavage by CO_2 of ribulose *bis*phosphate should lead to two molecules of 3-phosphoglyceric acid or one of 3-phosphoglyceric acid and one of glyceraldehyde-3-phosphate with the input of

reducing power. And I said, "Well, you know, vi haf vays of finding out using enzymes." And he said, "Well, anybody who knows enzymes must be terrific and, you know, useful, so, welcome!" So he then went to one of his young post-doctorals, Professor, oh then Dr. J. Rodney Quayle, and said, "Clear a bench. Kornberg is coming." And that meant, of course, in everybody's mind, that it was Arthur Kornberg. And I suspect also in Melvin's mind (*laughter*) because when I appeared there was a little frisson of non-recognition as it was quite clear that I was not Arthur Kornberg. As I always made clear to everybody, including Arthur Kornberg, although we are often confused by name nobody who has ever seen us could possibly confuse us because he is a man of medium stature with receding hair and glasses! (*Laughter: Hans Kornberg, of course, is exactly the same!*) Anyway, I duly appeared and Rod Quayle, who was, of course, in awe of this great biochemist descending on us (*them?*), also gulped and then decided that, well, maybe he could teach me something.

VM: Of course you didn't know Rod at that time.

HK: Not at all. Rod Quayle, at that time, was a man who had had an interest in insect pigments and had somehow been deflected from this on coming to Calvin's lab. but was quite willing to teach me about the chromatography if I would teach him about ammonium sulphate precipitation and what NADH stood for and glyceraldehyde-3-phosphate and so on. So that is how we started a very happy collaboration. Of course, it was a marvellous introduction to a group which was inspired, I think, by a common passion for solving the problem but had an enormous range of approaches to tackling that problem. It was not free of amusing episodes in this, some of which were, in fact, slightly embarrassing. Like Melvin's tendency to come in, demand to know the results of an experiment he had suggested and then going off to give a high-powered seminar somewhere in which he announced the tentative speculations I had advanced as established fact. It was this, of course, which led, of course, to the famous incident of the sequence, which you already know about.

VM: Which you will tell us, anyway, because this is a record...

HK: Right. Well, what happened was that Melvin would come in, as I said, and demand to know what the experiment showed and whether or not it was glyceraldehyde-3-phosphate or 3-phosphoglyceric, and I would say "Well, you know the evidence is strongly in favour of ruling out glyceraldehyde-3-phosphate but I need to run some more controls and I need to do this and I need to do that." And he would gradually shed clothes, as you know, when he would talk to you. I finally managed to get him down to his vest on one occasion when we argued about, you know, the significance of the results and what still needs to be done. But anyway; I was then rung up from a colleague who had heard Melvin talk in, I think it was New York or it may have been St. Louis, who said, "Gee, that's great work you are doing. I have just been hearing about your definitive ruling out of the (*Editor: not clear; glyceraldehyde-3-phosphate?*)" Of course, my hair (in those days I still had hair), stood on end at this point because I thought, I've done nothing of the kind. And so when Melvin came back, I said, "Look, Melvin," (and publicly because everyone was listening), I said, "Melvin, I think it would be better if I did not discuss on-going experiments with you

until I am in a position to tell you which way they go." And all the boys around me sort of nodded and nudged each other and said, "Right, you know, we'll get him for this". I was living as an inmate of I-House at the time and after my honest day's work I had gone to a party that evening and had gone to bed at about midnight and about two in the morning I was woken up by a thunderous knocking at the door and there were two of what appeared to be armed guards outside who were actually campus policemen, or actually Atomic Energy Authority policemen, who handed me a note which said, "Security violation. Notebook marked 'secret' left open on desk." What apparently had happened was that the boys had gone to the safe and had got out the purple stamp marked 'Secret' and had marked the first page of my notebook 'Secret' because I wouldn't tell Calvin the results. Since I had no security clearance to be working in the Atomic Energy Laboratory in any case, I immediately had visions of myself either in handcuffs or in chains or on Ellis Island or deported, so I went to see Nate Tolbert, who was then the Acting Director because Calvin, fortunately, was away.

VM: Bert Tolbert.

HK: Bert Tolbert. I beg your pardon. Thank you. Nate was his brother. And Bert said, having listened to my story, he said "Oh, don't worry. We'll clear this up." So he talked to whoever was the security chief up in the laboratory on the Hill, we went up there, and we sat down, me quaking in my boots, and the security guy said, "Well, this is a very, very serious matter." And Bert said, "Well, it is all a trivial mistake and a joke and so on, could we just not forget it." "Oh, no," he said, "a file has been opened and if a file has been opened we have to follow procedure and this will have to go to Washington." And I thought, oh, my gosh! And Bert said, "Well, just to be clear, could we possibly just see the evidence on which you are taking action?" And so the man said, "Well, yes, here it is. It is a notebook with on the front page a big stamp saying 'Secret' which clearly ought not to have been left out of the safe." And Bert took this notebook and immediately ripped out the front page and he said, "I don't see any stamp." And that was the end of that. (*Laughter*) And I returned.

VM: And you never knew who did it?

HK: I suspected. Anyway, my return back to the land of the living, as it were, was greeted with the usual applause and, of course, a party; we had a party that evening. And we went off camping in Yosemite and we did all sorts of jolly things that one did. So when I left to go back for the second half of my trip across the country...

VM: To go back to Racker?

HK: To Racker, but this time in New York, I was touched but not at all surprised that my by this time bosom friends should wish to give me farewell presents and these farewell presents included all sorts of goodies like reading matter for the journey home, all sorts of paperbacks and books of various kinds. And I gratefully accepted these and packed them away and that was it. When I finally returned to England, I went on board ship and again there was a farewell party and then we set off on the good ship the *Flandre* to England and I was sitting on the deck deciding that I ought

to read some of these books which I had been given and I picked one and opened it and this time, inside, there was a red stamp which said 'This document contains material vital to the security of the United States and should on no account be, etc., etc." And I thought, "Agh," and I tossed it overboard and picked up the next book which had an identical stamp — every one of these books had the same stamp in it! Fortunately, I managed to get rid of all except one, which I've kept. I thought to myself, you know, if this had become public knowledge I might not be sitting here where I'm sitting now.

VM: It's not in this very room, is it; this book?

HK: This book? No, alas, no, not here. But anyway it is one of the many pleasant memories of that time.

VM: When you first got there, you started by talking to Calvin about what you were going to do?

HK: Yes, indeed.

VM: To him alone?

HK: Talking to is perhaps too strong a word. Listening to, and then trying to get a word in edgewise.

VM: He knew exactly what he thought you ought to be doing?

HK: Well, he certainly did. I had explained to him the power and utility of enzymology as a means of picking up intermediates even in small amounts with cell-free extracts of *Chlorella*. Not that I had ever used *Chlorella* or knew how to break them up or done anything other than, you know, talking off the top of my head. But he was bowled over by that approach because he was basically a chemist and he had a chemist's view of how nature operated. "Bio-Organic Chemistry" was the name of the group, after all, which seemed to be a contradiction in terms; but however, oddly still tautologous since originally organic chemistry was defined as the chemistry of organised or living matter so it didn't need "bio" as well. But anyway, the thought that there were enzymes involved which had to act in concert and were regulated was not one which was very current in that group and, indeed, most of the people there were, I think, non-enzymologically contaminated.

VM: How were you going to show the presence...the formation, direct or...Let me rephrase it: how were you going to show whether or not glyceraldehyde phosphate was present?

HK: Well, there were two ways I was planning to do it, if I remember now. I am now talking about 1954 which is 42 years ago. I was going to see if we could trap glyceraldehyde-3-phosphate, if it were formed, either by doing the thing in the presence of arsenite and we did, indeed, use arsenate as a replacement for phosphate, and show the formation of, you know, the usual triosephosphate dehydrogenase

reaction; or I was going to use the fact that it was in equilibrium with dihydroxyacetone phosphate and reduce it. And there we ran into trouble because there was a very active NADH oxidase system in the crude preparations we used but I found that by eliminating membrane fragments I could get rid of the NADH oxidase and then putting in control glyceraldehyde-3-phosphate (well, it wasn't actually; it was actually fructose-1,6-*bis*phosphate) and hoping that there was enough aldolase and triosephosphate isomerase present and α-glycerophosphate dehydrogenase I could show that it really did work and that it was dependent on fructose-1,6-*bis*phosphate and that there wasn't NADH oxidation. And this was done in a very crude, manually operated, Beckman spectrophotometer because there was no such thing as...I mean, we had recording spectrophotometers but these recording spectrophotometers were used for extremely chemical purposes and were not adapted for biological use. But we could show that if there were glyceraldehyde-3-phosphate present in fractions of a micromole, I would have been able to pick it up and I didn't. So that gave me confidence with both tests, both with the arsenate procedure and with the DHAP dehydrogenase procedure, to believe the primary cleavage was, in fact, two molecules of 3PGA and not glyceraldehyde-3-phosphate.

VM: Were you there when he, or whoever else might have done so, formulated the proposed mechanism for this carboxylation and split? For the carboxylation of...

HK: It had already been done by the time I'd arrived. The *Path XXI* paper had already appeared and also the famous paper which had the chromatogram of phosphate sugars labelled — do you remember that?

VM: Well, I don't know about *the* famous one. There were so many which had phosphate sugars labelled.

HK: The one which I remember is the one which had spots labelled fructose-P, glucose-P and Godnose-P.

VM: Godnose-P! I don't remember that one; I'll look up that one.

HK: So that had been done and there was a delightful man called Wilson...

VM: Alex Wilson.

HK: Alex Wilson, who was a New Zealander, who was largely responsible for the Godnose-P identification, I think. Altogether was a very lively character. I remember, also, Malcolm Thain and that is another part of the story because Rod Quayle and I, and his wife Yvonne, became very, very close friends as a result of our working together and, indeed, we went on trips together to Yosemite...

VM: May I ask you, were you married at the time?

HK: No I wasn't. No, no, I was very much not married at the time. And we went on trips together and so on and so on. And then I met a young lady when I, myself, had gone back to England and was working in Krebs' laboratory for one year only (although it

turned out later to be six) and as a reward for my baby-sitting for a week for some visiting Americans, these visiting Americans took both of us to the theatre to see Beckett's *Waiting for Godot* and who should be sitting in front of us but Rod Quayle and his wife, Yvonne. And they had, Rod had come back to England and the only job that was open to him was in the Tropical Products Laboratory in which Michael (*Editor: should be Malcolm*) Thain, indeed, was one of the senior people. Rod did not find this very congenial; he didn't like the Civil Service atmosphere, he didn't like the type of work he was being offered and he was clearly unhappy. So when I went back to Oxford, I went to see Krebs and at that time my work had just stumbled onto something which later turned out...we had just published the glyoxylate cycle. And I mentioned to Krebs that the desirability, nay the necessity, of establishing that this cycle, which had been worked out largely on the basis of enzymology and simple experiments with microorganisms, that this ought to be put to the test by measuring the distribution of isotopes from labelled acetate in components of the Krebs cycle or in amino acids derived therefrom because it was quite clear that the glyoxylate cycle should lead to a distribution very different from that of the Krebs cycle and if both were operating then some intermediates should be done.

VM: You remember, of course, the work of the Carnegie group had been very active in the Krebs cycle?

HK: Indeed. And I modelled myself very much on that and got to know Phil Abelson quite well through that. Well, the upshot was that I told Krebs that I was neither sufficiently adept as a chemist nor knowledgeable in isotopery to be able to do this on my own but by very happy coincidence there was a man who was and Krebs immediately said, "Well, arrange for him to have a fellowship." And so Rod Quayle and I worked on opposite sides of the bench very happily for a number of years until he finally took off and became a Professor of Microbiology in his own right and later, of course, a Vice Chancellor.

VM: When you were in Berkeley you worked in the Old Radiation Lab.?

HK: Indeed, the old wooden building.

VM: Were you in the main lab., in the big lab., or pushed away in a corner somewhere?

HK: No, I was in the main lab. In fact, Rod and I shared a bench and we were in the main lab. Memories are dim, but I do remember that it had a number of delightful features not the least of which was that if you smoked, and I did smoke, furiously, in the wrong place the fire alarms would go off and the next thing you'd know is that the benches would magically open where you thought there would be a cupboard containing glassware and out would pop a fireman with helmet and all and dragging hose after him. The first time I encountered this phenomenon I could hardly believe it.

VM: How do you mean the benches would open? Where was the fireman actually coming from?

HK: The building, was I think, partly on stilts and they went underneath and then climbed up...It was a matter of some astonishment to me.

VM: This happened to you actually while you were...

HK: This happened to me on one occasion, yes. I made myself very scarce in case I was asked for an explanation of how this visitation came to be about.

VM: When you were there, was Andy still there or had he gone?

HK: Oh, no, very much so. Andy was really the CEO of the lab. Melvin came in as *deus ex* machina but Andy Benson and Al Bassham were the two people who really ran it. And then there were people with special expertise like Rod with his chemistry and Clint Fuller with his botany and so on, but it was Andy who was, without doubt, the genius of the place, and listening very carefully to how his mind worked and how he explained things and how he devised tactics rather than strategy, it was clear that the inspiration and the realisation that the formation of a C_3 compound from C_1 was not $C_1 + C_2$ but 2 x C_3 arising from $C_1 + C_5$. That was very largely Andy Benson's. Looking back over the history of the thing, I am increasingly convinced that Andy should have, by rights, shared in the Nobel Prize for that work.

VM: That was also the time, approximately, I don't know if precisely, when the other rearrangements in the cycle were being considered — the C_3s and C_4s and C_7s and so on, which was surely relevant to the work you were doing with Racker.

HK: Well, indeed. This was another reason why Calvin was anxious to offer me a place because I had been working in Racker's laboratory on transketolase. My friend, Paul Srere, at the next bench, was working on transaldolase. So between us we managed to combine some experience in shuttling C_4, C_3 and C_5s around. And we could, therefore write down two schemes, both of which would have led to ribulose *bis*phosphate, one involving aldolase and one not involving aldolase. In a later book that Krebs and I published in 1959, called *Energy Transformation in Living Matter*...

VM: '49? Must have been '59?

HK: I mean '59, I beg your pardon: '59...we put forward both schemes and said there was no evidence to decide between them and they may, in fact, be a mixture of both.

VM: When you were in Berkeley, then, was this a lively item of discussion — what these rearrangements were?

HK: It wasn't. Occasionally Dan Arnon and his group, particularly Bob Whatley, who was amongst them and whom I still see occasionally and still regard as a good friend, would come over, but unfortunately, for reasons of personal chemistry, the visits engendered more heat than light and I don't think...I mean Dan Arnon and Mel Calvin were like oil and water and it was clear that we were not going to get any further by discussing these. And I think Dan Arnon, to do him credit, was probably

more receptive to the possible involvement of these enzymatic transformations than was the Calvin group.

VM: Arnon actually came into the building, did he, and discussed with...?

HK: I saw him...this is how I came to know him better because he had also attended the International Congress of Biochemistry in 1949 and had also visited Sheffield and I visited him in his home. But it is clear that he came in only when Calvin was not there, at least in the period that I was there. But Bob Whatley came very frequently.

VM: We are going to see Bob Whatley in Oxford.

HK: He was a neutral messenger.

VM: Yes. You mentioned some of the people who were there at the time — Al and Andy, Alex Wilson and Rod — do you remember others who were there at the time?

HK: I remember some people from the Donner Laboratory. I have already mentioned Bert Tolbert, and Dick Lemmon was another person whom I became quite friendly with. There was also Dr. Stanley, of course.

VM: Wendell Stanley.

HK: Wendell Stanley, yes.

VM: But he wasn't part of Calvin's group...

HK: No, he wasn't.

VM: He was in the Virus Lab.

HK: Very much so. I remember also there was a rather peculiar failure of identity there, or recognition of identity, because when I came in to be introduced to him — Dick Lemmon, I think it was who had catalysed this meeting — magically all doors were open (I was only a post-doc., damn it) and he came out beaming, with his hand out straight and said, "Dr. Krebs, Dr. Krebs, I'm delighted to meet you." And I grasped his hand firmly; I said, "Dr. Livingstone, I presume." (*Laughter*) I can happen.

VM: Did you know two ladies, one of them was Lorel Kay who was...

HK: I knew her, yes.

VM: degrading sedoheptulose, I think, and there was somebody else whom I can't remember...

HK: I think I met her but she hasn't made a very deep impression.

VM: All of those sugar degradations had really antedated your visit...

HK: Yes, yes.

VM: ...and the cycle was really being put together in its final form, I suppose, at that stage.

HK: Well, *Path XXI* had just appeared, I think, and that gave the whole cycle. The other work which led to it had been done before I came.

VM: And your activities really represented, perhaps, the first detailed investigation into the carboxylation mechanism itself.

HK: Probably, yes, because at that time there were all sorts of schemes (*which*) had been drawn up on how the carboxylation might occur. It was known that somehow the CO_2 carbon inserted itself between what was two and three on the ribose (*ribulose*) but there were all sorts of chemical compounds with double bonds there were postulated for which there was very little evidence. And some of these were actually tried and didn't work, of course.

VM: In retrospect...

HK: Hamamelonic acid. I remember now. Gosh; I haven't thought of that for 40 years!

VM: ...a favourite compound.

HK: Yes, that's right.

VM: Hamamelonic acid is not something one lies awake at night dreaming about.

HK: No, it's one of the...one of the great...It always reminds me of Christopher Fry's comment in one of his plays when the dead heroine mourns her young husband taken from life untimely and says, "How sad to be a coming man already gone." I think that is the state of hamamelonic acid, too.

VM: Looking back on it after so many years, what's your view about Calvin's lab. and labs. like that?

HK: Calvin obviously had a tremendous dynamism which swept you along. He was blessed by having a very happy home life. His wife was absolutely delightful, very firm with him but a wonderful hostess. He had the ability, also, to inspire not only loyalty but also affection in all with whom he came into contact. I mean, you could dislike him or at least you could fear him. He could be extremely fierce. I remember on those awful Friday mornings when we started off with a seminar at 8:00 am. I was leading a hectic social life so I believed that little good could be expected of any day we started by getting out of bed and to start at eight o'clock was just impossible. And you appeared and everybody would be sitting there quivering with apprehension because you didn't know who was going to be asked to speak. You know, he would look...this eye, this cold eye, would look around and he would say, "Hans, would you care to tell us what you are doing?" And the answer was, "You'd better!" He inspired

awe, fear, respect, admiration for his quickness of mind because I've never known a man who was quite so able to respond to novel ideas, assimilate them and then play with them and see the outcome as Calvin did. And although this all sounds negative, in fact he inspired in no one that I have met who worked with him anything other than affection and admiration. I really came to like him immensely.

VM: Do you think that he was really the whole of the group, the whole guiding force of the group?

HK: I think it is like saying, "Was Beecham the whole of the London Philharmonic Orchestra?" Without the orchestra he would have been nowhere. He would have been beating his arms in the air. I think it was Andy Benson and Al Bassham who provided the string section and the brass section and the percussion and everything that made it go. I think they were the leaders of the orchestra; he was the conductor.

VM: Have you seen other groups like that?

HK: This is a group which he directed. Now I have been in many other groups. I have been in Racker's group, I have been in Krebs' group. I've had groups of my own, as you have. But none of those have been directed. They usually have been people suggesting certain things and being there as, perhaps, courts of last appeal or perhaps of people who might suggest a directions, but they haven't been directed. Calvin directed. He actually came in and directed. Now, funnily enough, Krebs did the same thing to his immediate group, that is, he would come in and individually design the protocols of today's experiment with Len Eggleston or Reg Hems but he didn't do that to me because he didn't know anything about enzymology and he didn't know anything about microbiology, and he distrusted both of them.

VM: Those people with whom he did do it — were they technicians or independent scientists?

HK: They were technicians who became independent scientists. I mean, for example, the Professor of Biochemistry at Sheffield who succeeded Krebs at two removes, Walter Bartley, was his technician. David Hughes, who became Professor of Microbiology at Cardiff, was his technician and that goes on.

VM: Yes, I remember, you were his technician.

HK: Well, I was his technician for one year only. I was his technician ...This is how I got into biochemistry was by applying for the post of junior technician at 30 shillings a week rising to 35 when I left school because I didn't know what I wanted to do and I was summoned for interview and because of the remorseless intelligence of my answers, of course, I was appointed for the job. It was only later in life that I discovered that another contributory factor might have been that I was the only applicant!

VM: I think I can conclude that, at least in your view, Calvin's group was a rather remarkable and unique activity.

HK: It was unique in my experience. I have never known a group of people from such a wide background who could work together as happily. If one had to draw an analogy, it is not an analogy with which I am personally familiar but I am personally familiar with people who are personally familiar with this. It is the people who coped with breaking the Enigma code during the war, which is the only...

VM: Well, it has struck me that there are several analogies with wartime experience and because that was an AEC-funded activity perhaps their own focused wartime activities in the military sphere would have primed them for that type of organisation.

HK: Yes, it could very well be, yes.

VM: Two last points, I think, before we finish. One of them is your view of the significance that the building itself, ORL, might have had in the way the group organised itself and the interactions that went on. Did that strike you at the time...?

HK: It didn't strike me at the time. In fact, I found on my subsequent visit to Berkeley, when the round building had been built, I felt the atmosphere had deteriorated. There wasn't this awful...there wasn't this delightful intimacy that there had been before. People had been literally compartmented. Somebody, I can't remember who, it may have been Krebs or somebody even older than that, once said that you could divide a scientist's career into three. The first is when he does his important work in intolerable conditions; the second is when he spends all his time designing a new building; the third is when he shows visitors round it. And I think the inception of the round building also marked, I think, the departure from the high point of Calvin's group's activities. It became much more pedestrian.

VM: Interesting. That quotation appeared on Al's old glassed-in office door or wall after he came back from Oxford. He might well have heard it from Krebs at that time.

HK: And, of course, Al and I overlapped at Oxford.

VM: Indeed. And the very last thing, although you mentioned that when you left Berkeley, of course, you went back to Racker and then you went back to Oxford. When you were in Oxford: Krebs had moved by that time?

HK: Krebs, in fact, came to visit me at Yale and this, again, is typical of Krebs. I mean, I had been a student in his department (not working directly with him but with R.E. Davis) and yet he had said that I could come back for one year in the first instance to him when I came back to England but he found it necessary to come to Yale in order ask me whether I would agree to his moving from Sheffield to Oxford. Which I thought was quite extraordinary.

VM: Whether you would agree to it?

HK: Whether I would agree to Krebs accepting the Chair at Oxford. Would it bother me if we were not to be in Sheffield? I thought it was remarkably courteous but it totally left me baffled that he sought my advice. I was then all of, what, 25?

VM: I remember when you were all of 25. In the pictures of you taken in the Berkeley group at the time, you were a much more dapper dresser than all the others.

HK: Not Calvin. Calvin was the dapperest of them all.

VM: Was he?

HK: He always had a fresh rosebud in his buttonhole. Always.

VM: He did, that's right. But you, you came up in these pictures in a white suit or a...

HK: Not a white suit. I could never resist a bargain and I bought some tropical suits and of course they were ridiculous, absolutely ridiculous. But I had them so I wore them.

VM: All the others were in their typical sawed-off jeans...

HK: Yes. This was the pre-jeans era.

VM: Well, whatever it was when they wore — sloppy clothes. Anyhow, you said you spent six years with Krebs at Oxford...

HK: What happened was that I spent six years from 1955, when I came back until 1961, when I physically moved to Leicester, with Krebs in Oxford although working very much independently after the first year.

VM: And you went to Leicester for a Chair in Biochemistry?

HK: Yes. That was my first academic appointment.

VM: How long did you stay there?

HK: Fifteen years.

VM: So that took you to 1975.

HK: 1975, yes.

VM: And then you went to Cambridge.

HK: I was appointed in '60 but I moved in ''1 and stayed there for 15 years building up the school and then became Sir William Dunn Professor at Cambridge from '75 'til '95.

VM: And here you are now with a new life and renewed living in Boston.

HK: "Recycled" is the word you're looking for.

VM: What's your formal title here?

HK: At the moment the Acting Director of the University Professor's Program. I am a University Professor and a Professor of Biology.

VM: I see.

HK: Very important.

VM: Well, thank you very much. I think that in spite of your having not remembered anything we have a number of useful reminiscences and views and thank you, indeed.

HK: Well, at this point the only name with which you can charge me, the only name I can think of is Alzheimer, but thank you.

VM: Well, most of the chaps we deal with are fairly elderly and you are less Alzheimic than many of them, that's for sure! OK. Thank you.