

Chapter 46

GÉRARD MILHAUD

Paris

May 14th, 1997

VM = Vivian Moses; GM = Gérard Milhaud; VeM = Véra Milhaud;

SM = Sheila Moses

VM: This is a conversation with Gérard and Véra Milhaud in Paris on Wednesday the 14th of May, 1997 — just; it's getting towards midnight.

You were in Berkeley in Calvin's lab. in the mid-fifties. What was your background before you went there and how did it happen that you came to his lab.?

GM: My background was first to study chemistry and to become a chemical engineer at the ETH in Zürich. And then I started to study; at the same time I prepared my PhD thesis with Professor Karrer who was a Nobel Prize winner on the constitution and synthesis of vitamins A and E. I started to study medicine, the first years of medicine. Then I went to Geneva and continued to study medicine and, after the war finished, I came to Paris and spent my time between Geneva and Paris and finished my medical studies in both places. Then came the time to settle down and I managed to get a position at the Pasteur Institute in Paris and it was obvious to me that we had been staying way behind in terms of modern biochemistry and, even more, in the use of radioisotopes. Even so, the first use of radioactivity of iodine in biology has been made to study the thyroid function in animals by Robert Corier (*spelling?*) and Joliot-Curie.

So the question was where to go to study the preparation and the use of isotopes in biology and in man. Obviously California was very attracting, especially Berkeley, with the importance given to this type of study by John Lawrence. Even so, John Lawrence did not have, as a scientist, the best reputation. It became obvious to me that what was going on in the laboratory of Calvin should have been very interesting. So I managed to get a fellowship from the French government for a year and I needed to find a way to have the travel paid. At this time, the Fulbright funds had been available for travel but the first requisite was to speak English. I spoke very poorly English. Before I went to the examination, my wife spoke well English and I started to brush up my English. The result was very poor and the Fulbright people decided that they could not possibly give me the grant due to my poor English. I said to them, "but it's my only hope to improve in English, if you give it." They gave it and we

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came to Berkeley and, more precisely, to the Donner Laboratory where Dick Lemmon was in charge of chemistry with radioactive carbon.

VM: You had made this arrangement, of course, with them before you came?

GM: I made the arrangement that Calvin would accept us in his laboratory. As a matter of fact, in 1951 the first world biochemistry meeting after the war was held in Paris. Andy Benson, we did not know, attended the meeting and we happened to know him to discuss with him. He was at the time working in Norway. He decided to come back to Berkeley so we could make the arrangements to come to Berkeley and to work for Calvin with him. Except, the first thing to do when you arrive there is to make the synthesis with radioactive carbon of a compound. Calvin at the time assigned me the synthesis of C^{14} -labelled hydroxypyruvate because he thought maybe it could be involved in photosynthesis.

VM: Were you a skilled enough chemist at that time to do that?

GM: Yes. I was a skilled enough chemist to handle the small amount of chemistry due to the training at the place of Karrer. I had no use of radioactive handling but having been trained in the best of institutes in the field of microbiology, the type of technique you use for radioactive tracer is not so different from the attitude you have to have against microorganisms for contamination and so on. This was not a big gap. What was a big gap was to get so and so many millicuries, \$5,000 worth at the time, telling you if you fail, you can have another one, but this is going to be the last one!

VM: How did you travel to America?

GM: In a very difficult way because we had at the time...we went on a French boat which was, I think, removed from the German, called *Europa* or something like this and we had trouble getting the permission from the state to work in AEC. Because at this time you had all the question of Oppenheimer and they had been very anxious. At this time — I don't know if it was the same for you — when we eventually got the permission to come, it was almost in winter time and we had a very, very rough sea and, as you we did not travel first class, as you can imagine, it's a very bad memory for my wife and not a very good one for me. We arrived, she arrived very sick in New York and then we travelled by plane from New York to San Francisco. Then really it was a fantastic discovery to come to this place of the world and to this highly exciting place of Berkeley at the time it was.

VM: Did anyone meet you at the airport or you went by yourself to Berkeley?

GM: No. I think Andy Benson met us at the airport and brought us to the lab. Then we met with Calvin, (*whom*) we did not know, and he said "where are you going to stay tonight?" and we said we would go to the Durant Hotel or some place like this. He said "no, come to our home, we have a room for you and you will stay with us as long as you find a flat you would like to live in."

VM: How long did you stay there?

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GM: A month.

VM: A month!

GM: A month, sir!

VM: You stayed in the main part of the house?

GM: Yes.

VM: He had a place below, didn't he?

GM: Yes.

VM: And you stayed in that?

GM: Yes. It was for us the most fascinating introduction to the scientific life of America to be with Calvin in his house.

VM: How so?

GM: Oh because he was very active, he was speaking all the time, telling you about his projects, making theories. It was very interesting to see this man acting and living the way it did.

VM: And he continued to do this when he'd left the lab. and was at home in the evening; he still continued to talk in the same way?

GM: For a while he collapsed because he had too severe a diet and so he was not very active. Then, after he had eaten something, he started again.

VM: Of course he had had a heart attack, serious heart attack some time before.

GM: Yes, yes. He was on diet.

VM: And lost a lot of weight.

GM: Due to the diet imposed on him by Gofman who started to study the relationship between cholesterol and protein and proposed a diet of index to foresee the chance of heart attacks in a person after ultracentrifugation of his lipoproteins. He was very strict about his diet and, of course, his diet did absolutely nothing to his brain and to his abilities and to his imagination.

VM: So he really collapsed when he came home?

GM: Then he started again.

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VM: Did he keep this going until late in the evening?

GM: No. You see, he was staying up very early in the morning, I would say between five and five thirty, and he would be going to bed latest at ten probably slightly earlier.

VM: Did you see what did he do at 5:30 in the morning? He didn't go to the lab. that early, did he?

GM: No. I think he was reading and writing in his study.

VM: Did he have a big library?

GM: He had a big library but only on technical chemical/physical chemical or botanical books.

VM: If you spent a month living in his house, you must have talked to him about many sorts of things? Was he interested, in your experience, in anything other than chemistry?

GM: First of all, he was very interesting in telling you how he went to England on, I think, a Rockefeller Fellowship, the time he spent there, what he learned there. He was also very interesting in telling you about the relationship he had with Ernest Lawrence and the beginning of the first experiments with the cyclotrons. He spoke mostly of his scientific personal experience and about his personal pathway.

VM: You didn't get a sense that he was a deeply cultured man, widely read man?

GM: No. I would say, what was impressive to us, that a man of his stature was really addicted to science and was not interested in literature or art or music. This was the impression we got. (If you don't agree, you should say something, Véra.) He liked very much to be in his garden and to watch the growth of flowers and plants. His wife was very interested in gardening.

VM: That's science again, isn't it?

GM: Well, practical science. His house was a beautiful house made out of wood by a famous architect — I forgot the name.

VM: (*Bernard*) Maybeck.

GM: Maybeck. It was a Maybeck house surrounded by much green and plants and trees — and it was a small heaven, without any noise, and in this beautiful landscape. He did not have much view because he was located too low but within the place it was really very nice.

VM: So you operated, in effect, from his house for a month. You presumably eventually found yourself a flat?

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VeM: We found before but he didn't let us go; he said it was not good enough. He wanted us to have something nice, good and we did not...

GM: So we found a flat which was very convenient for us, in the right range of price, 2525 Durant Avenue which was very close to the campus. So about three or five minutes walking time to the lab., and we used to work late in the lab. The only problem was that at this time we needed a badge and armed guards had been watching us day and night in the lab., of course, going in and out.

VM: So you went into the lab. then, you started to work directly with Andy at the beginning?

GM: No, I started first not to work in the lab. you know but I started to work at the Donner Laboratory, which is a complete building, not as nice as the Old radiation Laboratory. There I worked with Dick Lemmon. Dick Lemmon spent a year in Zürich and had a good training in chemistry. I didn't speak English, as I told you, but I used to speak German. Dick Lemmon spoke also German and in this way we could communicate. Then I started slowly to learn not English but Californian language. My wife had big trouble coming from a strange part of Great Britain to understand local language, but I practically none it because I started from nothing.

VM: How long did you work in Donner, all the time you were there?

GM: No, only until I finished hydroxypyruvate. Then I had to move to the Old Radiation Laboratory and to feed this to algae with Andy Benson and Bassham.

VM: So then you learned all the tricks of the photosynthesis experiments.

GM: Yes...

VM: ...how to run chromatograms...

GM: Yes, and how to elute and to count. Absolutely.

VM: You said that your wife also played some part in this and you have a picture of her...

GM: We had a picture of here and I think we could find this again. I will have a look and I am sure we can find it. So there is one of the...Andy made some pictures and Alice — you did not know Alice?

VM: I did know Alice — yes.

GM: So Alice made also some pictures of this time for paper chromatogram.

VM: Whereabouts in the Old Radiation Lab. did you work? In the big lab. together with most of the other people?

GM Big lab.

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VM: Who were your colleagues there at the time?

GM: Clint Fuller the closest. Benson was not very far away, Bassham was not very far away and the fellow of New Zealand

VM: Alex Wilson.

GM: Alex Wilson. And who else were?

VeM: Ed Bennett.

GM: Ed Bennett but Ed Bennett was, I think, more in the Donner Lab., you know; he was running some experiments trying to understand...to disclose learning systems in rats.

VeM: There were also two technicians...

GM: *Il y a deux filles*; there have been two technicians, a blonde one and...

VeM: Lorel...

VM: Lorel Kay.

GM: Lorel Kay.

VeM: And her husband, Lorel Kay?

GM: Yes, but you know, the one who was going to come later on, maybe this year; she wrote you a letter. You don't remember the name?

VeM: Ann Hughes?

GM: Ann Hughes.

VM: Yes; good heavens! And socially: how did you spend your time socially?

GM: Very well because, first of all, we knew very rapidly people within the lab. and outside of the lab.

VeM: The people are very friendly.

GM: First of all, due to the Pasteur Institute we came very quickly in connection with Roger Stanier and Gunther Stent and also with a strange fellow of the name of Chaikoff. Chaikoff told me "you made in your life a very big mistake; you should have been coming to me and not to Calvin.

VM: So you actually had a big circle of acquaintances?

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GM: Yes, absolutely. Next thing, we also had good contacts with Evans (*Herbert Evans*), the one who discovered some...the role of some growth hormone; and Li (*Choh-Hao Li*) who was a chemist, who made the structure of growth hormones. Then we became acquainted with the French Consul in San Francisco who had a very large territory because he had Washington state, California and, I think, Texas or Nevada. For some reason he wanted to visit the Atomic Energy Plant and he couldn't do this. So we organised this for him. From then on every time, he had a reception in San Francisco he invited us.

VM: So you had rather a good time, then, one way and another?

GM: We had a very good time. In addition, I had far related relative, cousin, of the name of Darius Milhaud...

VM: The composer?

GM: Yes, who was teaching music at Mills College. Darius Milhaud was convinced that we could not possible stand for a long time American food so every Sunday he invited us for lunch to have a French meal at Mills College.

VM: That was home from home. Did you socialise with people inside the lab.?

GM: Oh, yes, of course. We had a lot of invitations with most of the people of the lab., especially with Andy Benson, within the lab., with Lemmon with Bennett. There was also another fellow which we liked who was working with Evans, the name was (*Don*) Van Dyke. Van Dyke is the fellow who isolated urine erythropoietin. He was working on ion kinetics. He was the first one — he didn't left a name — but he was very active and pleasant, Van Dyke. He's the one who demonstrated the effect of erythropoietin from urine using ion kinetics at the Donner Lab. and at the Donner Clinic.

VM: Presumably coming from your Swiss background, you went on ski trips and mountaineering trips with the lab. people?

GM: We went on mountain trips but we didn't do skiing. We went on camping and to camp trips to Lake Tahoe and also to Nevada and we went north to Fort Ross.

VeM: We went to the desert with the Stents (?) for a very short time. They had a lot of rattlesnakes.

GM: And mosquitoes.

VM: After this work you did with hydroxypyruvate, did you then move into...you said you then moved into ORL.

GM: Basically, hydroxypyruvate was rather disappointing.

VM: But you looked at that from a photosynthesis point of view?

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GM: Yes. It was used but it was not the key of being an intermediate we would have been missing. So the next thing was that Melvin was convinced that thioctic acid would be an important compound in the energy conversion from light to chemistry. (*Tom*) Jukes who had isolated thioctic acid and produced a large amount of it at Lederle/Cyanamid came to the lab. and I was in charge of trying to disclose if thioctic acid would be involved by opening and closing the sulphur bond in the energy transfer. At the same time, thioctic acid could only be measured on paper chromatograms by bioassays so we made the bioassays with Clint (*Fuller*).

VM: You spent several months or a year or more working on this problem?

GM: Yes.

VM: You published, presumably, papers...

GM: Not many. I think we published two papers but not many really. Basically, we learned very well the techniques but the result had not been at the level of expectation so, therefore, I think that we rather published to paper to have a paper published but not really to disclose a major discovery.

VM: How did you find Calvin's attitude to publishing? Was he careful to publish or did he like to publish quickly? What did you experience?

GM: He liked to publish quickly. He was very impatient. He wanted things to be published as soon as they would be...We would rather to repeat this once to be sure; he said, "yes, but do it rapidly." He was very impatient.

VM: So you think he was conscious of competition?

GM: Yes, yes. I think he was very much conscious of competition and a person he profoundly disliked was Arnon.

VM: Why was that, do you know?

GM: Yes, because he thought Arnon may be closer than he was to the energy conversion. He was convinced that Arnon would be the guy who may take away from him the Nobel Prize.

VM: Even at the time when you were there, which was maybe seven or eight years before he got the Nobel Prize, he was conscious of the possibility and was...

GM: I would say the Nobel Prize was the goal of his life. He was very much pushing everything toward the Nobel Prize. At the same time, he was a very bright person. As soon as another topic, far away from photosynthesis, would be brought to him in a way he would like or in an intriguing way, he would say "go ahead. I'll find you the money and give you the things." He was very broadly open but his main objective was photosynthesis, path of carbon, Nobel Prize.

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VM: Do you think that he was really the intellectual leader and innovator of that photosynthesis work?

GM: I think, first of all, he was able to put the people together, to collect the money, to have the space, to attract interest and to speak, not to the media — at the time it was not media — but to chemical meetings and to seminars. From then on, I'm not sure he would have been able to find by himself the C₇ sedoheptulose-ribulose pathway. I'm not sure. I think for this he needed people like Benson. But Benson would not have been able to promote enough, I think, the pathway of photosynthesis to come to Stockholm. He (*Benson*) would have made the discovery all right but he was too shy and not enough pretending, it seems to me.

VM: It's very difficult to know what the fairness of this is because nowadays the name of Calvin is a better known name than the name of Benson.

GM: First of all you see Benson, you know Benson now, but at the time he was a person having a lot of difficulties, personal difficulties. First of all, he was a conscientious objector. He got every day the communist paper which made him in a very bad position...

VM: The communist newspaper?

GM: ...at the University of California. McCarthy. Once he was called by the FBI to go San Francisco, not for having dinner at Fisherman's Wharf, and therefore personally he was rather shy and very much worried about what was left in the States for a person like him in terms of human rights. So he would never have been, I think, strong enough and enough extrovert to come to the goal assigned by Calvin for himself.

SM: At that time, Calvin was completely apolitical, wasn't he? I don't think he had any...

GM: Calvin was always apolitical. I think Calvin was never interested into politics and when Oppenheimer got trouble and moved during the night we have been there and had to escape and move, he had Russian friends and so on, Calvin did not care at all. He had no sympathy for Oppenheimer and it was not his job, basically. He was on good terms with John Lawrence. He did not really respect (*him*) because John was not so bright as Calvin was. He was very much in respect with Ernest Lawrence and with the chemists. He did not like very much (*Martin*) Kamen and he had great respect for a fellow you may not have know, who was the most impressive figure in microbiology of the west, Van Niel.

It happened that I met Van Niel several times and then Van Niel decided to spend a year at the Pasteur Institute with Lwoff. After a month, he said "I am fed up with this guy, I'm going to work with you."

VM: He said that to you?

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GM: Ja. So he came to the lab. for a year and he is the one who isolated *Thiobacillus thiooxidans* from the earth of the Pasteur Institute. This micro-organism was isolated a long time ago at the Institute by a Russian of the name of Vinogradsky. He said “I know how to do it. You have to translate every day and I show how to do it and I go away for Christmas for two weeks and when I come back I want you to give me a new seed of this micro-organism.” He comes back. We completely failed and we said “we are very sorry, we did not succeed.” He said “OK, OK. I’ll go back to the hospital and remove some earth.” He brought this back and he said “you made a very big mistake.” The mistake was the following. You had to boil the water to remove oxygen because, with traces of oxygen, the micro-organism would not grow. You had to have an Erlenmeyer flask, to boil, and when you cooled this, you had to let no air go into it; otherwise you are lost. He showed us how to do this but did not explain this so we did not notice it. We had air and had not been able to grow it. For him, it took three days to take this out of the earth and have a pure culture.

VM: Did you work on *Thiobacillus thiooxidans* yourself?

GM: Yes. Because this micro-organism needs only thiosulphate and carbon dioxide. Out of this it’s making a new micro-organism. Therefore, it was obvious for us that we could see how specific the pathway of carbon for photosynthesis was or was not. Therefore, using the techniques we had been learning in California, it took us two weeks to demonstrate that we had the same pathway as in algae, with the C₅-C₇ sugar, and we could demonstrate the product relationship. Therefore it was absolutely sure that *Thiobacillus thiooxidans* were using the same pathway as algae.

VM: This was after you had come back from California.

GM: Yes.

VM: Back in Paris?

GM: Yes. This was due to the fact that we had access to the microorganisms through Van Niel.

VM: By that time here you had set up the right sort of facilities to do...

GM: When we came back the question was: will we be able to settle or not? The head of the French NIH, of the name of Gunillard (*spelling?*), was very helpful to me. He introduced me to the director of the Pasteur Institute and managed to get some grants and facilities from the French Atomic Commission. He was at that time head of a biologist same time of the French AEC. So we had plenty of possibilities.

VM: When you did this, and I seem to remember that the results showed that the same cycle existed in *Thiobacillus* as in the algae? I presume you told Calvin this, did you?

GM: We published this.

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VM: How did he respond to that?

GM: Andy was enthusiastic and Calvin did not want to answer. He was very worried.

GM: Worried that what?

GM: By the fact that the path of carbon he had disclosed with Benson was not specific to photosynthesis. It was obvious that it was unrelated to photosynthesis. As soon as you could supply energy, direct chemical energy, into microorganisms, or light energy, the same system would be working as in yeast. He did not like this at all.

VM: Which year was this?

GM: '55.

VM: I see; at that stage...yes.

GM: Late enough so it did not harm him from his Nobel Prize.

VM: But he was worried, was he?

GM: But he didn't like it, according to Benson.

VM: You remember that, at the early days of the cycle work, people often called the cycle by both Benson and Calvin's names.

GM: It was Benson; yes, absolutely.

VM: And then later on, somehow, the Benson more-or-less got lost.

GM: Yes.

VM: It was just Calvin. Do you think this was an unfair effect for Benson?

GM: It was very unfair for Benson and, at the same time, it's easy to understand. Calvin was always asked to give lectures at large meetings or seminars or lectures at schools or universities. Benson only seldom. Therefore, what we would call now the media pressure was in favour of Calvin and not of Benson. It's very clear to me that the pathway was discovered by Benson. At the same time, it is Benson who had the idea of making paper chromatography, radioautography and, what we did not mention, destroying selectively the compounds to get the location of the radioactive carbon within the sugar...

VM: The degradation.

GM: The degradation.

VM: It sounds as if you feel that, in the end, Benson was really not sufficiently recognised.

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GM: Absolutely.

VM: You feel that's the case.

GM: Absolutely. And, you see, what came later on, contrary to what did happen to Calvin, is very interesting for Benson. I managed, as a small recognition, to have him awarded the *Doctor Honoris Causa* of the Sorbonne. On this occasion I had to present Benson and, if you remember, when he left Calvin he did a lot of work on phospholipids. Phospholipids became at this time, he did not demonstrate this but it became then a major components of all the membranes. And he (*Benson*) had clear ideas about this. Then he worked on arsenic and later on he worked on fish biology and on sulphur compounds in algae. I think he did not stop at photosynthesis but made a few discoveries; each of them would probably have been enough to make an average scientist well known. I think he had at least four scientific lives, in addition to the work he did with the cycle.

VM: It's getting very late and we must stop fairly soon. There are really two questions...

GM: No, but I have still another thing to tell you. Finish the question.

VM: No, no: I won't forget my question.

GM: You will forget your questions.

VM: Well, OK. The two questions I had to ask you I will tell you so that we can fit them in together. I would like to know of the building where everybody worked, Old Radiation Lab. itself. Some people have spoken about that being an important factor in the way the group operated in Berkeley; I'd like to see what you think about that. The second thing I'd like you to tell us, just quickly, what happened to you in the rest of your life after you went to Berkeley.

GM: So let's start with the building. I think the building was really a fantastic place and I think the disorganisation of the building was probably the most efficient organisation you could find. First of all, all the people had been working at the same place, or most of it, and there was a constant exchange among the people which you could not reach with walls and offices. It was very exciting to go upstairs and to see on the blackboard with chalk the date of the Nobel Prize awarded to Ernest Lawrence for the cyclotron (1939) and to see the place where the cyclotron was located. So I think this building was really exceptional. What came later on, was not as efficient, not to speak about the charm of this type of this type of wooden floored military building. When they started to destroy the building, I removed a key lock which I still have, which was the lock going into the cyclotron room.

VM: Well, you should keep it because there is very little left of that building: I think one door, apart from your lock, is all that remains. You agree that it was an important factor in the way the group operated. The second thing is, in a sense, what did you learn from that lab. and what did it do for you in the rest of your life?

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GM: I learned, first of all, that if you have a goal, you should not save on money or on efforts. This was very alien to the habits we had in Switzerland, at Karrer's place or Ruczicka's. There was always limitation on material and money. At the same time, I think I learned that the most you speak to people and exchange ideas, you don't lose the property of the idea but you increase your chances of making progress. When I came back, as I told you, Roger Stanier wrote a letter to Monod telling him that he had known me and I should be working with him but I did not like to work on microorganisms so I said "no, I am interested in applying these type of techniques to medicine and to human disorders."

After the pathway of carbon in *thiooxidans*, I decided to go over to man and to study a very simple system — I thought it would be a very simple system — which is calcium metabolism in man with radioactive calcium. First of all, what was needed was to use kinetic analysis and modelling to understand, or to try to understand, what would be the normal behaviour of calcium pools, and sub-calcium pools, in bone formation, bone destruction, urinary excretion, faecal excretion, and so on, and absorption. Then we could have a way to diagnose disorders, knowing what drugs would be doing to correct disorders. I thought this would last one year or two and then the next thing would be to understand more precisely diabetes. As a matter of fact, I practically never went to diabetes, stayed on bone and calcium metabolism. This led me understand what compound discovered in Boston by Hirsch and Munson, which called at the time *thiocalcitonin*, in heart would be doing and working. At this time, they had made an observation and the existence in rat thyroid of calcitonin. They studied immediately human thyroid and they published that thiocalcitonin was absent in man and probably some vestigial hormone or something vestigial without any importance.

I repeated what they did and found thiocalcitonin in human thyroid and disclosed what thiocalcitonin was doing, which was immediately to suppress bone destruction. At this time, when we came back from the States, I stopped in Boston and met the group working of metabolic bone diseases Albright. So I was aware of osteoporosis and other metabolic bone diseases and I understood that I had the way to treat osteoporosis by stopping bone destruction.

So I extracted calcitonin from the human thyroid; there is not enough to treat patients. Then I found that hog thyroid was very rich in thiocalcitonin so I made a preparation of hog thyroid which I injected to the patient with heart beating. I could demonstrate that thiocalcitonin was highly active in case of hypercalcaemia especially due to vitamin D intoxication, Badgett's disease of bone, which had no treatment at the time but aspirin, and osteoporosis and some exotic diseases. This was due to the fact, having made a model for calcium metabolism in man, it was possible to investigate and to establish a mode of action of calcitonin.

Then I went to the industry, telling them that I had the way to treat osteoporosis, and this was a complete failure. I visited Lilly people in Indianapolis. They had

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thiocalcitonin themselves. They sent me some for testing. They did not believe in the action. I went to Lederle/Cyanamid. They had some but they dropped it. I went to Merck, Rahway: same result. I went to Sandoz where a schoolmate of me, Bressanard (*spelling?*) was head of peptide chemistry, he said “no, it’s extractive; we need a synthesis”, and so on. I went to Ciba, with the same good result. Eventually, Roussel took it but they never believed in it. Even so, it is nowadays 1% of the world pharmaceutical market, the Roussel people are practically out. I think it was very important to have been there for what came later on.

VM: Have you spent the rest of your working life in Paris?

GM: Yes.

VM: In the university?

GM: First of all, when I came back to the Pasteur Institute, in ’58 I became Professor of Medicine in Amiens. In ’61 I came back to the School of Medicine in Paris. Then, in ’66 I went to St. Antoine and from then I have stayed at the medical school...

VM: St. Antoine’s a hospital?

GM: Yes...having a laboratory that was financed both by CNRS (*Centre National des Recherches Scientifique*) and CERN. In France there have been three or four laboratories which have a large supply of money due to this double label.

VM: And you are still continuing that work?

GM: I still continue that work in term to find a way to inject once monthly calcitonin instead of three times a week or daily.

VM: I think perhaps we should stop there because it’s nearly the end of the tape. Let me simply thank you very much and tell you what a pleasure it has been for me to meet both of you after so many years of knowing your name. I hope it won’t be the last time.

GM: Until the next time, soon I hope. If you come often here for your business, let us know and we can meet again.

VM: Thank you very much.