

Barnett Rosenberg

February 2, 2001

Jeff Charnley,
interviewer

Charnley: Today is Friday, February 2nd, the year 2001. We're in Holt, Michigan, at the laboratory of Dr. Barnett Rosenberg. I'm Jeff Charnley, interviewing Dr. Rosenberg for the MSU Oral History Project for the sesquicentennial to be commemorated in 2005.

As you can see, Professor Rosenberg, we're taping this. Do you give us permission to record this interview?

Rosenberg: I do, indeed.

Charnley: I'd like to start first with some questions about your general educational and professional background. Where were you born and raised, and where did you go to primary school?

Rosenberg: I was born in New York City. I am a New Yorker by choice, and I spent most of my life there, not most of my life; by now I think it's about 50-50. I went to Abraham Lincoln High School in Brooklyn. I became interested in science early on before that. My friends and I were always tinkering around with different things, and we built an astronomical telescope. My friend had a rather good chemistry set, and so we picked up some chemistry that way and were interested in chemistry.

Charnley: Did you have any clue as to what sparked that interest initially? Was it a person, a teacher? You mentioned your friends.

Rosenberg: Initially it came about by virtue of reading an encyclopedia that was on the market at the time, starting with page one. What I found in reading it was that the most interesting things to me were the articles that had to do with science. I knew then at a very early age--I'm talking now six and seven--that I was going to be a scientist. This was going to be my role in life.

Then movies affected me, things like *The Invisible Man*. There was a scene, for example, of a wealthy man owning a large mansion and having in the back of the mansion a laboratory and some two or three people working with him on a particular research project. I was struck with that. That sort of sat in the back of my mind for many, many years until, finally, I put it into action, as you can see. That's my house over there, which is only about 200 yards away.

So very early on I had a deep commitment to science. Now, which science, however, varied as my life progressed. I went to Lincoln High School, where science was a very significant part of the curriculum there. It was one of the best high schools in the country at the time.

From there, I was affected by the war, of course. I went to Brooklyn College after I graduated from Lincoln High School.

Charnley: What year did you graduate?

Rosenberg: 1944. Then I went to Brooklyn College and majored in chemistry. After about a year and a half of studies, I reached the age of eighteen and went in the Army. I spent eighteen months in the Army overseas in the Philippine Islands. There I met a fellow, a German scientist, who had left Germany because of Hitler, and he was giving a course in physics after the war ended for the soldiers that remained on the island. This is the Philippines.

I took the course, and I was fascinated by it, so I began to switch from my chemistry interests to physics. I realized then that what interested me most about chemistry was physical chemistry, so physics came easy to me. I had a girlfriend at that time who was very much interested in physics, as well. She, indeed, eventually did get her degree in physics and remained so.

So, at that time, after my graduation in 1948, I went to Europe for a year and studied at the Swiss Polytechnic Institute in Switzerland. [Telephone interruption.]

Charnley: So this was one year in Switzerland that you studied?

Rosenberg: Yes. For me it was just a year to play. I enjoyed it enormously. I did take courses, but, unfortunately, I sat in on a course or took a course that was given by--oh, God, I'm going to start losing names very quickly.

Charnley: That's okay. We can add them.

Rosenberg: The great theoretical physicist at the time. Anyway, he was a theoretical physicist, and he started out with a set of equations that I had just finished entering into in my undergraduate

work. He went on from there at a rapid rate, never looking at the people, always looking out the window, and, eventually the kids started to leave his class. It was just too hard. I stayed there for about seven weeks, I think. Then there were about five of us left, and I left at that particular point because it was totally useless to me.

So, in any case, at that time I came back to the United States after a year and started my serious studies at New York University's Department of Physics as a physics major. I did some interesting work and I made some interesting discoveries and got some patents that were very interesting, too.

Charnley: Very early in your career.

Rosenberg: Yes, it was quite early.

Charnley: What were they related to?

Rosenberg: Solid-state physics. It was sort of like a magnet that you can demagnetize and remagnetize, but this was an electrical charge. You could put a charge on something and have it permanently charged or you could discharge it. You can't keep the charge. It separates two charges, so it's polarized. So you can have it either polarized or unpolarized, and it would stay that way. So it's a way of making a new memory device.

This was in the 1950s, so there was some interest in it in terms of a memory system for a computer. I did some postgraduate years' work at the computer center at New York University to develop this thing.

After a while, however, I became aware that solid-state physics was not that fascinating to me. Other things were far more so. In fact, a lot of physicists who were involved in the atomic bomb research came to the same conclusion, out of perhaps guilt feelings or other things that opened up that were more fascinating, but a lot of them went into a new field called biophysics.

I started to look into this and, indeed, decided I was going to be not a geophysicist, but a biophysicist. I took a job at the physics department at New York University with a government grant to do some biophysical work. Shortly thereafter, I started to look around for a permanent position. I met some people who were also interested in a permanent position. The three of us got together and decided we would try to set up a biophysics department at some university.

We looked around at a number of different places and had interesting offers from all of them. The only one that I regret turning down was Berkeley, because of the weather.

But we got the offer from Michigan State University. The reason why the offer was made was because of John [A.] Hannah. John Hannah had the wisdom to believe that science was going to be important to our future world, and our university was deficient in that area, except for the agricultural part of it. So he then set up a committee of different scientists to search out for a biophysicist. Well, we met with them, and they were pleased with us and offered us the job, all three of us.

Charnley: Who were the others?

Rosenberg: One was Leroy Augenstein, who at the time was a true biophysicist at—what's the name of the camp up on Long Island (Brookhaven [per comment by Mrs. B. Rosenberg])? There's a large science laboratory. I'm blocking on the name of it, but, anyway, he was there. A friend of mine, who I met at many meetings, an Englishman, was also interested in joining us, so he joined us, too. His name was Ronald Mason. He is now Sir Ronald Mason, still in England.

Leroy, of course, was killed in a plane crash a few years after he joined us. It was a great pity, because he probably would be President now if he had stayed. He was interested in politics and he was sufficiently charismatic as an individual that he almost got elected to the Senate from Michigan. He didn't make it, but then he decided to run again. It was when he was flying from place to place that he had an aerial accident and was killed.

Charnley: He piloted his own plane?

Rosenberg: Yes. Something I highly recommend people don't do. I've lost too many friends with accidents.

Charnley: What year was that offer made?

Rosenberg: We're now talking about we joined here in 1961. I think he was killed in 1969. Now, all of this, of course, was due to John Hannah. The reason why we took the offer from Michigan State rather than any of the other four major universities was because of John Hannah. He made a

very simply statement. “You do what you want to do. There are no restrictions on you. Set up any system you want. You’re totally free.” Now, that’s a statement you rarely ever get.

Charnley: From a university president.

Rosenberg: So what we did is we chose to be a graduate department only so we wouldn’t have any undergraduate teaching. We wanted to do mainly research. This was the dominant thing.

Biophysics was just beginning and therefore research was the most important aspect of it. So, John Hannah’s freedom that he offered us was the deciding factor in our coming there.

Charnley: It sounds in some respects similar to the cyclotron experience where he went out and actively recruited and brought Dr. Blosser here.

Rosenberg: That’s right. And it was about the same time. It was in the, I would guess, late fifties, early sixties, that MSU began to blossom from a cow college into a reasonably competent university.

Charnley: Did you have reaction from friends who said you’re going to Michigan State?

Rosenberg: Yes.

Charnley: What did they tell you?

Rosenberg: Most of them thought it was crazy. “Now, what the hell do you want to go to Michigan State for?” We did not have a reputation at that time, but I was young and I thought we had time, maybe we could build a reputation there. The other places had such overpowering powerful individuals already established that we would probably have been swamped had we gone to those places and never gotten the freedom and capability that we had to do the work we wanted. So, all in all, I think it was an excellent offer, and we had not seriously regretted that offer at that time, not for many years.

They gave us complete freedom, and it was that complete freedom that allowed me to do the experiments that led to the discovery of the cisplatin drug and the anti-cancer activity. Had I not had that freedom, I never would have.

Charnley: What was the focus in your early years here after you set up the program?

Rosenberg: Well, remember, I was a physicist, but I had been working with light and crystals. So this was the interaction between light and matter that was fascinating to me.

Now, in the biological system, there are two areas where this plays a role. One is in photosynthesis. But Calvin Cowen at Berkeley was already in that area, and successfully so. He won a Nobel Prize for it. The other one was in vision. So I chose to go into the area of vision. How does a photon of light entering into the eye and being absorbed trigger off a neurologic reaction in the brain that tells you you’ve seen something? This was the problem we attacked.

There were a number of other problems that we were involved in, and we published. About a third of the papers were on that subject. About a third of our papers were on the properties of proteins and their ability to conduct electricity, which is important for their function in biologic systems. That took about a third of our work.

Then, of course, we hit upon this accidental discovery that finally led eventually to cisplatin. That was another third of it, about a third of it. I got them all down one quarter, because there's a fourth area that we were now in for a long time, and that was aging. So we were able to go into that.

You can see what this freedom allowed us. When I was forty, I suddenly became aware at the birthday party my wife threw for me when I was forty, that a good part of the conversation of the faculty members who were friends of mine at the party was on the fact that forty is a dangerous age for men scientists. It's the time when they sort of have the equivalent of female menopause. So they start to become worried and they do all sorts of foolish things. It's when a professor divorces his wife and marries his graduate student. I didn't want this to happen to me. I was happily married with two lovely children, so I said, "Look, aging is something that we're all intrigued by and worried about. So let's go into it and see what the hell it is." So we went into aging.

So all I had to do was apply to the government, get funds, etc. I had a laboratory, and, poof, off we went. The university had no say in the matter at all.

Charnley: Were those grants easy to get at that time?

Rosenberg: Yes. Biophysics at that time was a hot topic in government support.

Charnley: Were there any other institutions that were involved in aging research significantly, or was the MSU project one of the earlier ones?

Rosenberg: I think we were one of the earlier ones. There had been individuals working on various aspects of aging, but nothing from the point of view of a biophysical process.

Charnley: In dealing with Dr. Hannah after the initial recruitment, was he supportive of you as time progressed in the later sixties?

Rosenberg: More important than that, he ignored us. When he said, "You do what you want," he meant it. He wasn't going to stick his nose into our work. He was happy at the successes we had and he congratulated us and we had a wonderful relationship with him, but he never told us what to do or even got into details as to what we were doing.

Charnley: How about the provost at that time, Jake Nevel [phonetic]?

Rosenberg: He was a reasonably supportive individual. I mean, you had grants and that was a success. He felt that was bringing something to the university. Then he was replaced by--oh, God, who was the one after him? It was not--

Charnley: You were involved with Milton Muelder, probably.

Rosenberg: Muelder, yes, very much so. We were at that time talking about the general idea of building a science complex, commercial, around the university to take advantage of it. This was a topic that was just becoming interesting. A number of universities were moving in that direction.

Milt Muelder, who was a representative of the university in relationship to these other places, to see what they were doing. He wasn't a scientist, mind you, so it didn't really matter. He was a good man. He worked well with us.

When did Cantlon come in?

Charnley: Probably about '69. I don't know if he was there for the transition between Hannah and Walter Adams, but he would have been provost about that time, right.

Rosenberg: Let's see. Walter Adams appointed him provost. I'm going to have a put a lock on my tongue with regard to John Cantlon.

Charnley: You've written a lot about the 1968 discovery of the anti-cancer activity, and I'm sure every reporter asks you about that. But what I'd like to do is, for this institutional history, have you talk about maybe that "eureka" moment, if you had one.

Rosenberg: When I was still at New York University and in the physics department, I was working on this idea of the vision aspects of things. It was physical considerations that I was undertaking at the time. I then accepted the offer here and felt, well, if I'm coming here to a

biophysics department, I've got to know some biophysics and I've got to do something that's biophysical in my research laboratory. So what could I do?

One idea occurred to me that intrigued me. If you look at the picture of a cell in the process of dividing, you see a very specific pattern, almost like a football with lines coming up. To a physicist, this looked very, very much like what you would see if you took a bar magnet, put a piece of paper on it and sprinkled it with iron filings. It's a dipole field.

Charnley: And experiment they do in elementary school.

Rosenberg: Right. So when I first saw these pictures of a cell in division and they reminded me so much of the dipole field, I thought, "Well, is it possible that the dipole field may be involved in the division process?" This turned out to be not an original idea on my part. Someone else beat me to it, but he didn't do any experiments. I did.

So what I wanted to do was to see, does an electric field have an effect upon a cell's growth? We set up an apparatus, rather complicated, and I'd rather not go into the details of it here, but to see whether we could affect the division rate of a cell by applying an electric field across a solution containing these cells and just applying electric fields.

It turned out that there was a profound effect. As soon as you turned on the electric field-- and of course, we used platinum electrodes so that we could bring the field in. Platinum is known to be nonbiologically active. It's inert. As soon as we turned it on, the growth stopped and we were in danger of washing out all of the living cells. So this was an interesting thing. God, you don't often find things like that.

So we began to look into what was going on. The first thing we did was look under the microscope at the cells, and what we saw was--as you walk downstairs, as you go out, you'll see a picture of what it looked like. Normally, the cells that we were using, in this case it was called e-coli, have the configuration of about my forefinger, the first digit, and that's what we expected to see. It grows to twice the length, and then it pinches off in the middle, and separates into two. This is the way the stuff grows.

Instead, what we saw was one that kept on growing, not dividing, up to 200 times its normal length, 300 times its normal length. When we turned off the field, all of them started to divide. So we had a process of control over the cell growth that was rather unique and nobody knew about before. We decided to look into that because it had the possibility of doing something which we initially had in mind; namely, if we could control the growth of a cell with an electric field, could we control some cells with a frequency of one sort, other cells with a frequency of another, and then we could attack a tumor by choosing a unique frequency and affecting only the tumor cells and not normal cells. So this was a means of treating a cancer. So we had that in our mind.

Here we had an effect where turning on the electric field has a profound effect on the cells, so we went after searching for the cause of the effect. It turned out, after two years of searching, that the effect was due to the fact that we were using a chemical called ammonium chloride. We had the cells growing in the culture, and we had platinum electrodes in the culture. Some of the platinum is going into solution as a very specific chemical structure, which took us another two years to identify. It had the effect of the chemical alone, without the electric field. The electric field was just making the chemical from the platinum electrode. But once it was made, it was the chemical that had the effect on the bacteria.

Charnley: Not the charge.

Rosenberg: Not the charge, no. So we started to investigate the nature of that chemical effect on the cells. It turned out, eventually, that it was blocking cell division. We thought, “Well, if it blocks cell division, that’s good for cancer. Let’s try it.” And we did.

Charnley: Were there any others that were working with you at the time on this research or like other chemists?

Rosenberg: I had a group of people working on the other projects. We had about ten people in the laboratory. Two of them were particularly involved in this particular research project: Mrs. Loretta Van Camp and Mr., now Dr., Tom Krigas. They worked with me on this particular project. Later on, we had many, many more. I must have had some fifty different people working on the project, post docs—

[Begin Tape 1, Side 2]

Charnley: You had up to fifty people at some time or another working on this?

Rosenberg: Yes, I would guess perhaps even more than that.

So when we tried to inject it into a tumored animal, we found that there was an effect. It was toxic, so you had to keep the dose low. But nevertheless, a dose which was tolerable, the tumors were disappearing. You'd take a mouse, you put a tumor into it, and you let it grow for a while. You see some pictures downstairs of that, too. When it gets big enough, you inject the mouse with the drug and the tumor disappears.

Charnley: It was a matter of finding the right dosage level at first?

Rosenberg: Yes, right. Once we had that, we went to the Cancer Institute. I had a friend there. They invited me down to give a talk on this, and they were rather reluctant to become interested, because the idea of putting a heavy metal into a person was an abomination. "Heavy metals are poisonous. And you're putting a poison into a person? No way."

But I left samples with them, and they tested them and they got the same results. Then they gave me a call, asking me if I'd like to apply for a grant to continue the work. I accepted, happily, and it began to expand. We published the results in *Nature* and in other places.

Charnley: That's a British journal, isn't it?

Rosenberg: Yes, the top biophysical place to publish something.

So word began to get around, and then I started to give lectures around. Other people became interested, especially a lot of chemists. Chemists had no source of funds to do biological research. So the idea of a whole field of chemistry now allowing us to get our foot into the field of

cancer research, nobody else could do the chemistry research but the specialists in platinum chemistry. So they jumped on the wagon, and very soon we had large numbers of people very much interested in this. Other people published, and it grew and grew, and eventually the Cancer Institute tried the Neumanns [phonetic] in 1972 with success. And off we went.

In '78, we got the patent on it, and at the same time, the FDA gave us the approval for injecting it in human beings. Out of the number of companies, there were about five companies that were interested in the license for it, Bristol-Myers at the time, now Bristol-Myers-Squibb, got the license with the best offer, and they've been pursuing it ever since and with a fair degree of success.

Charnley: The political climate at the time, in terms of research and the biomedical research that you're talking about, you indicated that it was a hard sell at first until, obviously, they saw the results. Could you talk a little bit about that? This is what, the late sixties, early seventies?

Rosenberg: We're talking about when I went to the Cancer Institute, it was in 1968. Yes, it was '68, because our first paper in *Nature* on the anti-tumor activity in animal came out in 1968.

There is an interesting story about this. There was a gentleman, Sir Alexander Haddow [phonetic], who was the head of the Chester Beatty Institute in London, which was the major cancer research group in the world. He was the head of it. He always had a feeling [unclear]. He took his M.D. degree in Edinburgh. Edinburgh is a hot spot for people working on homeopathy. Now, in homeopathy you give a little bit of the poison that is causing the deleterious effects and that cures it. That's the theory. That doesn't work. But he grew up in that. He also had the feeling that if heavy

metals were poisonous, maybe they ought to try them as anti-cancer drugs. And he did try them, and he even tried some platinum compounds. None of them worked. Then he came across my paper. He got on the phone. I was in England two days later.

Charnley: You were preaching to the choir.

Rosenberg: I sure was.

Charnley: An easy sell.

Rosenberg: Right. From that time on, we had a wonderful relationship, with people going back and forth and they're doing cooperative research with us with the Chester Beatty Institute and the Cancer Institute and a bunch of other laboratories and major places.

So the success rate, particularly with testicular cancer, which was the greatest success rate they've ever had, at the time, the first time they tried it, it was at the Roswell Park Memorial Institute. They had--I may not be absolutely certain of the numbers, but it something like twenty-five individuals with advanced testicular cancer, of which about twenty-one the cancer disappeared. Now, they were not cured; the cancer came back. But they had never seen a response like this, a total disappearance of so many cancers in a deadly disease that killed 90 percent of the people. So they became very excited about it. They, too, got into it. Testicular cancer became the primary one that they began to investigate.

Then in England, in London, there's a hospital next to the Chester Beatty Institute, Royal Marsden, at which a young woman doctor who was interested in ovarian cancer in women, when she heard about this platinum, she called up the head of the British medical, something like the NIH [National Institutes of Health] here, and asked for permission to inject a few patients. Over the phone, he said, "Go ahead and do it." That's the beauty of not getting caught in the bureaucracy. It was this freedom. In England, they could do it. We couldn't do it here.

So she then injected it and she got beautiful results. So ovarian cancer was added to the list. Then they decided to add this to a number of other cancers, so gradually it became a well-accepted therapeutic modality.

Charnley: Were there difficulties in the sense that the people, were many of them terminal, the ones that they did the tests on initially?

Rosenberg: Yes, all were terminal. The first tests, the Phase One tests on a clinic for cancer, is always done on a terminal patient. Well, we couldn't cure cancer in those days, so they all were terminal, except for a few types of cancers, which some small amount of remissions would occur in them.

Charnley: How did you come up with a name? Did you develop it yourself or in terms of what's the story behind that?

Rosenberg: I had a totally different name in mind.

Charnley: What were some of those alternatives?

Rosenberg: I was going to call it platinamine [phonetic] because it had platinum and amine groups were important to it. So platinamine was the one I suggested. It was ignored by the Bristol company, who had the license by this time. They wanted to call it cisplatinumamine [phonetic]. The licensing agent for trademarks refused to give it to them because of the name “platinum” in it, so they had to change it. So they changed it to cisplatin.

All right. They fooled the government, because in Germany and in France the name for platinum is either platan [phonetic] or platine [phonetic], which is just platinum. So in essence, they got the name they wanted but in a different language.

Charnley: How about the university? How was this discovery? How were you treated here at the university?

Rosenberg: The cooperation was wonderful. About five or six faculty members joined in the research and did research projects in the laboratories. We published with some of them. They were very, very happy with the whole thing.

Charnley: What other departments were there? Was there any connection at this time with the new medical schools, or did that come later?

Rosenberg: No, there was no connection with the medical school at all. These were just individual science departments, pathology, physiology, chemistry, biochemistry, a whole group of them.

So, the university was now growing, becoming more scientifically oriented, and being able to raise more money and funds from the NIH and other governmental agencies. So you can see this is a growing process. Cisplatin was one of the most successful of the things that came out of this time frame in the university's growth. And, of course, it helped the university develop its reputation, raise funds, and things of this sort.

Charnley: In the medical profession, how did they greet this discovery at the time?

Rosenberg: As I said, the initial reaction of a medical doctor would be, "Hell, that's a damn poison. I'm not touching it." The side effects were pretty severe and they almost dropped it, until finally someone found out that if you hydrate the patient, that it's not as toxic. This was discovered in Columbia University.

Charnley: So you still had the positive effects of the anti-cancer treatment drug.

Rosenberg: Yes, but they diminished the negative effects to the point of where it was acceptable. It's still a nasty drug, no question about it. But we found a better drug since that time.

Charnley: What's that?

Rosenberg: This is carboplatin. We've tested thousands of drugs. I mean the whole panoply of inorganic chemistry was now open to us. All laboratories that did any inorganic chemistry were playing around with different ones. They'd send it to us or send it to other places or do it themselves.

But, in any case, one of my students, one of my post docs, went back to England and pushed something which he had done in my laboratory to a point of where it was accepted as an interesting drug to experiment with, and he convinced other people to do so, working with us. Out of this emerged a second drug, carboplatin. This was as good or almost as good as cisplatin, but the side effects were far less.

Charnley: Although you started out or would have been in the initial test stages with testicular cancer research or applications and then ovarian cancer, are there any others that it has been applied to with success?

Rosenberg: Oh, yes. What's the name of the woman who's the head of the chemistry department right now?

Charnley: I don't know.

Rosenberg: I'm blocking out her name, and I don't know why. Anyway, she was sitting here just a few months ago. She said she was very much impressed with the cisplatin story, because she

looked it up and she found that 40 percent of cancer patients receive cisplatin. Now the number of different cancer types is now about a dozen, where it's the first drug of choice, including lung cancer.

It keeps coming up with additional things. People are working. Well, let me just put it-- there are well over 30,000 publications having to do with the platinum drugs, just as an indication of the size of the groups. They were trying it on all different kinds of cancers and finding a great degree of success on them. Not as good as testicular. That remains the--

Charnley: The drug of choice.

Rosenberg: I mean 95 percent cures; whereas, there was a 10 percent cure before that. That's a nice jump.

Charnley: That sure is. Have you received any personal letters from any cancer patients or anything?

Rosenberg: Yes. God, and they've come to visit me. Have you heard the story of Lance Armstrong?

Charnley: Yes.

Rosenberg: I had nothing to do with that, no, but he was cured with cysplatin.

Charnley: Yes, the famous Tour de France winner.

Rosenberg: Yes. There happens to be a second story of that type to have occurred with cysplatin. The first one was a British jockey who was one of the greatest jockeys in England and won the Grand Prix. Both his horse and he came down with a disease at the same time. They were going to kill the horse, and they decided not to and it recovered. He was going to die, and then they decided to use the cisplatin on him and it cured him. The horse got cured. They ran in the next Grand National and they won.

Charnley: Sounds like a movie script.

Rosenberg: It is, actually. A book was written about it and a movie. I have a copy of the movie here. John Hurt played the part of the jockey.

But it's interesting, and then now suddenly we have a second case of a famous athlete being cured with cisplatin and going on to win honors in his field.

Charnley: So it wasn't the end of life.

Rosenberg: No. It was great.

Charnley: In the 1970s, after all this was going on, and the development, how would you describe your life and work here at Michigan State at the time? Did it change significantly?

Rosenberg: Yes, tremendously.

Charnley: How so?

Rosenberg: Remember, sales started in 1978, and money started to flow into the university. According to the contracts that the faculty had with the university, the inventors get 15 percent, which in this case there were three inventors, so I got 5 percent. It started to add up to a significant amount of money. I wanted to put into place the dream I had, that I spoke about earlier, having my own laboratory, an institute of my own.

I put it to the university that I would donate all of the royalties that I got if they would match it from their much larger portion of the budget, of the royalties. We negotiated for quite a long time and there was some significant interest, but one man killed it. I won't tell you his name. He antagonized me for the first time on campus. Up to that point, I think MSU did everything they could for me. He did the first thing that turned me off. So I decided to go elsewhere.

I now had sufficient funds to have a laboratory, and I tried a number of places. There was a good possibility, except for one problem. Those places where I wanted to live would not allow me to put a laboratory up next to it. And those places where I could put a laboratory, I wouldn't want to live. So it was very frustrating, and after a while I came back here. A friend of mind found this place. It was a farm. This was a horse barn.

Charnley: Lovely remodel.

Rosenberg: Yes. That was just a flat farm there.

Charnley: Close to the university.

Rosenberg: Yes. So I continued my cooperation with the university. We have joint contracts for doing research, and yet I'm here myself, my own boss. Nobody tells me what to do.

Charnley: So you were involved in teaching directly after? At what point did you retire, or how was that?

Rosenberg: Our teaching loads were very low. That was a decision we all made. One quarter a year, because we wanted to emphasize research. The students would take courses in other departments. There was very little biophysics that you could teach other than maybe a term or two. So, in any case, at that time I felt that my role at the university was at an end. I chose to retire from the university.

Charnley: What year was that?

Rosenberg: Actually, I didn't. I took a leave of absence, and that was in 1982, a leave of absence. Then we continued to work together with joint support of the research but under my control. That continued until '96, '97, at which time I then officially retired from the university on my seventieth birthday, but still maintaining the relationship we had.

Charnley: Were you involved with any international programs or study abroad-type things or scholars? You obviously attracted scholars from around the world.

Rosenberg: Yes, we had. From all over the world, people came and worked with us, or I would go there and lecture there and so forth. It was an international project, the usual example of cooperation between scientists. There were no bastards in the group, with one or two minor exceptions. I think everybody felt that this was too important to do nasty things. There was full cooperation, open cooperation.

We established another group, an office to make copies of papers and send them to everybody on our list who wanted to become a member of the club. My wife ran it. She did a damned good job. There were some copies she would make of different things, and eventually she would send out papers or lists of recent publications, etc. People would send us their publications or even blueprints so that we could get it out as fast as possible. Urgency was evident in all of the work.

Charnley: If you think back to your own development as a scientist, you mentioned John Hannah and his encouragement of freedom to do things, how would you describe your own approach to,

let's say, tackling a scientific problem like a cure for cancer? Do you see any evolution in that over time?

Rosenberg: I see changes over time. I'm a fairly inventive individual, creative. That's probably my one good point, so I've gotten a number of reasonably good ideas, published them. It's been accepted by others in the fields. Among the biophysicists, of course, I was fairly well known. Not amongst others, though.

Charnley: How about the recent studies on aging?

Rosenberg: I hate cancer research. Okay. That's the point that has to be made. I wanted to get out of it as soon as possible. At a particular point in 1982, I felt there were enough good people in the field, it was well established, it was accepted, I could pull myself out, I was no longer necessary as the organizer of everything. So I decided to pull out of the field of cancer research.

That's when I started the aging research with an associate of mine, Dr. David Juckett. So in the laboratory here, when we established it, aging research was the dominant thing that we wanted to do.

I think I'm off the topic of what you asked.

Charnley: I was just asking, you made the change to that, and you talked a little about your fortieth birthday and that sort of thing, but what would you say are maybe some of the active things that you have discovered as a result of that research?

Rosenberg: Well, we have a lovely theory of aging that we've published about fifteen papers on. We discovered, well, lots of things. I don't want to go into all of them. We have a theory of how the nose works, for example. How does it differentiate smell? We're actually building a machine that would emulate what the nose does, using the same mechanism as the nose does, that would allow you to distinguish odors. Smell is a piece of our work.

Charnley: Anything on memory?

Rosenberg: No. There is something on memory, but I cannot talk about it.

Charnley: Okay. I understand. Have you continue any of the cancer research?

Rosenberg: That was what I was coming to. I intended to get the hell out of the field, and for a number of years I was out of it. But then I was doing a study of how a population dies off with age as a function of time of their age. I got a very particular curve, it was a very interesting curve mathematically, and all of the diseases that we studied had the same shape of curve, but with different time constants. So some people with a given disease would die earlier and some later and so forth.

Charnley: In looking at some of your activities that you were involved with at the university itself, were you involved in any academic governance or anything like that within the university?

Rosenberg: In the beginning, I was one of two professors that were here, Leroy and myself. There are various different jobs that a professor has to do, members of committees and things of this sort. Since Leroy was the chairman, by choice of all of us, I used to take those jobs. I got tired of it.

Charnley: The committee work?

Rosenberg: Yes. And others picked it up afterwards and I just sort of dropped out.

[Begin Tape 2, Side 1]

Charnley: This is tape two of the Dr. Rosenberg interview.

You were talking about your committee work when you first came to the university.

Rosenberg: So, once other people had come into the department and we grew to about nine people, I think, they took over the committee jobs and I backed off. I have absolute abhorrence for any committee work, and I did none or minimally such, I mean trivial things. I simply stayed away from the administration.

Remember, in the beginning I told you John Hannah gave us the freedom and the rest of the administrators at the university kept his bargain.

Charnley: Yes, a whole lot of time can be wasted in administrative bureaucratic.

Rosenberg: Oh, terrible, terrible amount of time.

Charnley: Meetings on top of meetings. I know the feeling.

When you first came to Michigan State, now almost forty years ago, or forty years ago this year--

Rosenberg: '61.

Charnley: '61, so it will be forty years. You mentioned that there were people that were discouraging you from coming to Michigan State or why were you here. Then you have stayed in the area over that time. Did you anticipate you would be here forty years later?

Rosenberg: No, I thought I'd stay about five to ten years and that would be about it. But they were so good to us. I mean, they gave us everything we wanted. I just had no reason to want to pick up and leave. Had they irritated me, I possibly would have. But the first irritation came when they screwed up that offer to set up the platinum institute.

Charnley: In the 1980s.

Rosenberg: Yes.

Charnley: Some of the things we're interested, too, in the project is talking with people who, maybe we talk to our interviewees, and see if they might have suggestions of others that you worked closely with that are still around that you think that their story is an important part of that. Are there any colleagues that maybe you think we should talk with, either staffers or--

Rosenberg: I'm trying to think of the people who were involved in the early days of helping us set up the biophysics department. For example, Don Montgomery, who, unfortunately, is dead. He was a prime mover in helping to get the department established. In fact, he lent me the oscilloscope that I used for my platinum experiment out of his lab.

Charnley: Was he in chemistry?

Rosenberg: Physics. He then later moved to another department, but that's another story. I am having difficulty thinking of these people's names. I have not thought about them in years.

Charnley: If any come to mind, you can certainly let me know.

Rosenberg: There's one I'm trying to think of now because he was quite significant. No, I'm blocking out on it, but I can go next door and find his name. Anyway, he was also very much interested in biophysics. While he took his degree in zoology, his office and laboratory were right near ours, and so we interacted quite a lot. Then he went off to Oak Ridge for a number of years,

came back, and then joined the university, went through some different departments, eventually ended up in the medical school. He's the—[Tape interruption.]

His name is Dr. James Trosko, T-R-O-S-K-O. A brilliant scientist and was involved with us in the early days of the cisplatin, as were Dr. Surinder Aggarwal, who was in the department of zoology.

Charnley: Is he still living?

Rosenberg: Yes.

Charnley: Is he retired?

Rosenberg: No, he's not retired.

Charnley: In reflecting back on your career at Michigan State, is there anything for the record that you want to leave with future generations?

Rosenberg: Yes, and I'm afraid it's going to be slightly more negative than you would like. I left the university, and I think it was the best thing I did, for me. The reason I left was because the administrators of the university had lost the zeal and competence that John Hannah had. He was the one who pushed this University into the prominent position it now has. He has been followed up by a series of other individuals that did not share his vision of the university but retained the

vision of it as an agricultural college. It would not surprise me--and this is a quote from a friend of mine from the administration--it would not surprise me if in ten years we are back to being a Michigan State College.

We have not taken advantage of all of the developments in science that we could have. Occasionally, an individual arises who is in a position of power and does something, and we make a little move forward. But we could have been a heck of a lot better than we are had we had better choice of administration, of personnel. I'm not going to identify anyone.

Charnley: I understand that. I want to thank you for the time that you spent and the insights that you've shared. I appreciate it. Thank you.

Rosenberg: Okay.

[End of interview]

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