Andrew Szent-Györgyi, M.D.

Biographical Statement

Andrew Szent-Györgyi is a cousin of Albert Szent-Györgyi. His father, 58 when Andrew was born, was a younger brother of Albert's father. Like Albert, Andrew was born and raised in Hungary. He earned his M.D. at the University of Budapest in 1947, then emigrated to Denmark, and soon thereafter to the United States in 1948. He joined Albert's laboratory, the Institute for Muscle Research, at Woods Hole, Massachusetts, which Albert had set up after his own emigration from Hungary in 1946. Andrew and his wife, Eve, worked at Woods Hole until 1962, when Andrew accepted a position at Dartmouth Medical School. Four years later he moved to the Biology Department at Brandeis University, and from 1975-1979 he served as Chair of the Biology Department. He has published over 140 research articles and has received numerous awards, including the Public Health Service Research Career Award (1962-1966), a Guggenheim Fellowship (1966), and a Merit Award from the National Institutes of Health (1987-1997). He has served as President of the Society of General Physiologists (1970-1971), President of the Biophysical Society (1974-1975), and is an Honorary Member of the Hungarian Academy of Science.

Interview Synopsis

Dr. Szent-Györgyi describes Albert Szent-Györgyi's family background, education, and scientific career in Hungary through 1946, when Albert left Hungary for the United States. He also describes conditions in Hungary and in Albert's laboratory at the University of Budapest during and after World War II, especially the period 1945-1947, when Hungary was under Soviet domination. Andrew explains his own as well as Albert's and other scientists' emigrations to Europe and the United States after the war, and the pioneering work in muscle research that occurred at Woods Hole. That work was an extension of the work that Albert and others had begun in the 1930s at the University of Szeged in Hungary.

Dr. Szent-Györgyi also describes Albert's values, his sense of humor, and his passionate commitment to science and scientific research. As he explains, Albert found himself at odds with the American system of grant applications and always struggled to find funding for his research efforts at Woods Hole. Lastly, Dr. Szent-Györgyi describes the evolution of Albert's research interests, from muscle contraction to submolecular biology, and assesses Albert's contributions to science over his long and sometimes controversial career.

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Interview with Andrew Szent-Györgyi
Conducted on May 26, 2004, by Adrian Kinnane

AK: You were telling me that your father was a younger brother of Albert's father, but just by one year.

AS: Just by a year.

AK: And he was fifty-eight years old . . .

AS: When I was born and that explains the apparent generational difference.

AK: And people sometimes think that Albert was your father, that you were his son.

AS: Yes, yes. I usually have to correct people.

AK: Now, you decided to become a scientist. How did you make that decision?

AS: At first, when I was in high school, I decided I don't want to be a clerk or a lawyer. My family were lawyers and justices, most of them. There were only two professions where you didn't have a set time, working wise. One was engineering, the other medicine. I cannot draw at all. I cannot a draw a straight line even with the aid of a ruler, so medicine

remained for me. Of course, I was influenced by Albert a great deal, so I already considered to go into science, but there was no Ph.D. program, as we consider it here, in the physiological sciences in Hungary, so I entered medical school.

Once we got into clinical studies, I had to wait so much in the hospital I really got a little bit bored. Already Albert arranged that I should, as a student, work in the physiology institute of the medical school in Budapest. He even invited me in '43 to join his laboratory in Szeged in '44. I fortunately didn't, because I probably wouldn't be here. By that time the Germans had come in to Hungary. He was under house arrest because he was on their black list. He could go to the lab, but he had to ask permission to go to Budapest.

AK: Under house arrest.

AS: Under house arrest. The Germans, Gestapo, wanted to arrest him in '44. The Germans came in March '44, and when he asked for permission to go to Budapest, they surrounded the railroad station, but just the day before, his son-in-law came down and said, "Oh, you want to go to Budapest. Why don't we drive?" He came down by a car, so the Germans couldn't capture him. He arrived back to his second wife, who was also sort of hiding, because she was not quite secure.

AK: This is Marta?

AS: Marta. And as he arrived, she received a phone call from her sister, Babci, saying that the Gestapo was in the sister's home and had arrested Marta's father, believing that he was Albert Szent-Györgyi. So they weren't too smart. They didn't have a photograph. So Babci said, "You should disappear," and that is when he went into hiding.

AK: When you were a boy, before you were going to medical school, what were your contacts with Albert?

AS: Well, you see, when I was a boy, he visited us after the Nobel Prize, but he was abroad and in Szeged, and I wasn't in Szeged at all, you see. My most important contact was with his daughter, who told him that I was interested in working in the physiology department with Professor Beznák, but that I was having difficulty getting in. He was extremely delighted that somebody from the family continued to be in science. So without my knowing, he wrote a letter to Beznák, "Would he allow me to enter in the physiology department and start to work there a little bit."

Albert very rarely visited us. He visited my father when he became ill, but mostly he stayed in Szeged and he was extremely busy. For instance, he requested my father to write our family history, which my father, of course, did. I have photos here of the

family. But the scientific family branch was the Lenhosséks. You know about the Lenhosséks, so I don't have to . . .

AK: Yes. This was Albert's mother.

AS: Albert's mother, yes, was a Lenhossék, who, by the way, was very musical, and she was in doubt, whether she should get married or be a professional singer. And Gustav Mahler was then the director—you know the story?

AK: Go ahead. Mahler was the director . . .

AS: Gustav Mahler was the director of the opera and she had an audition, then Mahler advised that maybe it was better to get married, and that's how Albert came to be.

AK: A decision by Gustav Mahler.

AS: You see the result of Gustav Mahler. But [his uncle] Mihály Lenhossék was a great influence on him, because his father was a landowner and stayed away from Budapest. So Lenhossék was a substitute father, and he was a well-known scientist. Good scientists, not just Mihály, came to visit. Albert describes in his autobiography what an influence his uncle had on his development. He matured late, and you know the story.

AK: Now, you and he were together in Budapest in 1945?

AS: Yes, '45, then I joined him. He became a professor at the medical school in Budapest and I was in third year medical school in Hungary. I started my clinical studies. I helped to set up the lab for him.

AK: You helped to set up the lab there?

AS: To be around, because there was some building construction going on. I remember only one very lucky time, when I was really useful, is that we had a secretary whose wife was ironing just next door—the office was next door to the chemical room—and forgot to turn off the iron. And I was sleeping there on the same floor and woke up and saw the smoke. So in that sense, I was helpful, because if the chemical room had gone up, it wouldn't have been very healthy.

AK: And it wasn't very easy to reconstruct that lab after the war.

AS: Well, it was a lab which was previously occupied by someone teaching public health. He got a donation from a rich person, Stephen Ráth, who—because the country was very poor otherwise—financed much of the rebuilding of the lab. Then Albert decided that he would let me work in his lab. That was in '45.

AK: Those were difficult years.

AS: They were not enjoyable. They were very difficult years. You see, at first it was very difficult during the war. You either survive it or not; you cannot do much, so you start to live as if nothing happened. It was very difficult, because when the Germans came in, they collected the Jews, and Albert was extremely early to discover that Hitler—that's where the [Sir Hans Adolf] Krebs letter is. I don't know whether you read it, Hans Krebs, in Kaminer's *Search and Discovery*, and then he started showing the slides of Albert's handwritten notes in 1933, advising him to leave Germany, because he [Krebs] was Jewish.

AK: Albert also said that after the war was over, that there was what he called "wishful thinking" about the Soviets.

AS: Oh sure. Sure, you see, because one had the feeling right after the war was over that there would be a real democracy developing. You see, nobody knew how the Russians would behave, and it took a year or something like that, when one could see, maybe a year and a half, that it was hopeless. So all his efforts, because he was the president of the Hungarian-Soviet cultural collaboration and so on—but we didn't realize the agreement between Stalin and Churchill and Roosevelt that Hungary would be under the domination of Russia, and they didn't explain that the Russians would be as bad as they were, you see,

with collecting people when they occupied Budapest, who were young, and taking them as prisoners of war, and how many prisoners they captured. My brother was, for instance, in Russia as a prisoner of war for two years. They weren't tortured, but they were hungry all the time. They had to work. It was a very unpleasant thing.

AK: When you and your wife Eve went to Copenhagen in '48, I believe it was . . .

AS: Yes, '48, April.

AK: Did you know at that time that you were going to be trying to come to the United States?

AS: Yes. Yes. As a matter of fact, a few days before we left for Copenhagen—it was a complicated story, because the Danes had no legation in Hungary, no consulate, so we had to stop over in Prague. And it was obvious what was happening, because one looked at Romania and Poland, and the same thing would happen in Hungary, but it was delayed by six months. So we knew if we wanted to leave, semi-legally at least, from Hungary, then we better move. And the letter of invitation from Buchthal, from the University of Copenhagen, was delayed or lost, so we had to write to him and when it came, we decided we would just leave right away.

A few days before we left, the American consul, Mr. Revey, came to the lab looking for me, because it was arranged by the New School for Social Research that we could enter the country, because it was a teaching job. He wanted to talk to us, so we went to him and he said, "You have a visa. You can come." I told him that no way can we get a valid passport to the United States. We had a passport valid for three months to Denmark. I asked him to send the documents by diplomatic pouch to Copenhagen and we would get in touch with the consul there, and please don't phone, because there are other people whom Albert wanted to get out and no names should be mentioned. So we left and we went to Copenhagen. Buchthal was there, a German Jew who was saved by the Danes. A marvelous person. He knew exactly how we would feel and he was the nicest person you could imagine.

We went to the American legation in Copenhagen, where there was an elderly consul from California and so we told this unlikely story that we had an appointment to the New School for Social Research. We were told we have a visa. That was our story, and we came. Of course, he didn't get the diplomatic pouch letter and he was asking us, "How do I know that you are not Communist?" So my answer was, "It is very hard to prove a negative, but I am convinced that by the time this visa arrangement was made, there was enough investigation to establish that we weren't Communist."

So we went repeatedly. We visited him week by week, or every two weeks by two weeks and he asked exactly the same type of questions and so on. Finally, he called up, "Your visa is ready, but I will need $10 to contact Budapest," which he had to do officially. At that time, $10 was a lot of money. We had $20 when we left Hungary, and forty pounds of luggage and so on. He talked to the people at the legation in Budapest. He got in contact and could let us know: "Your visa is ready."

But then we ran into difficulties. The difficulty was that we arrived to Denmark legally, but our passport was not valid for the U.S. To enter the country, you have to go through Immigration. If the immigration officer is suspicious, then they can send you back to the country you came from, but they couldn't send us back to Hungary. And the Danes said, "Why should we want to give them a return permit?" Professors Buchthal and Kalckar—Herman Kalckar, who were good scientists and were also Jewish, and they were saved by the Danes; 50,000 Danish Jews were transferred to Stockholm in a day when the resistance movement found out that the Gestapo wanted to transfer them to a concentration camp. So they got in contact with a Swedish ship captain, Mr. Peterson, to smuggle us into Norway, because if our problem was that we entered the country illegally, we would get a return permit if we were illegal. [Laughs] Then Buchthal went to the Ministry of Education saying, "Isn't that ridiculous?" So they gave us a return permit.

About five years later, we met him, Buchthal, at the International Biophysics meeting in Boston, and he told us, "You know what was really the story, I never told you? The story was, that the Danes said, 'If the Americans want to have them so much, why don't we keep them in Denmark?" So anyway, that is my story about entering the country. We were detained at Ellis Island before we were let in the country, because I looked too young to be a professor or assistant professor.

AK: Now, Albert was already at Woods Hole.

AS: Already at Woods Hole.

AK: Is that where you went first?

AS: Yes. We spent two weeks in Tarrytown, because he had a younger brother there, and then we went directly to Woods Hole and stayed there for fourteen years.

AK: Tell me about those early years at Woods Hole. Now, Albert had just gotten there in 1947. He'd arrived in 1947.

AS: In 1947. Then he did his last really important research in the field of muscle research. He developed glycerinated psoas muscle; it's skinned fiber which is permeable to the

solutions and that is still being used, with some modification, in the present, for the single fiber x-ray diffraction structural studies.

AK: Still being used.

AS: Still being used, you see. It was an extremely important discovery, even though it was a method, in developing the system in which you can do real structural studies. That is what H.E. Huxley describes in *Search and Discovery*—edited by Kaminer for Albert's eightieth birthday—the sliding-filament model. I don't know how much you will know about . . .

AK: I saw a publication on the wall outside, in the building here.

AS: Yes, it is the fiftieth year celebration by the Royal Society, you see, of the sliding-filament theory. If you are interested in the very early stages, I was asked to write a description of muscle research between 1942 and 1972. If you want, I can give you a copy. There were three important discoveries, three phases. One was the work in Szeged, which established the modern biochemistry for the study of muscle contraction, and that is what Huxley also describes very nicely in the book. With the aid of a viscosimeter, polarizing filters, and centrifuges, he realized that the myosin which was considered to be already known, it was part of the contractile system, really consisted of

two proteins, actin and myosin, and you could make from this a thread—they called it actomyosin, the complex versus the old myosin . . . If you make a thread, which H.H. Weber produced, and you add muscle juice to it, the system will contract.

And what they isolated from the muscle juice was ATP, so that ATP, actin, and myosin is the contractile system, and that was the basis of the sliding filament model, which was the second great improvement by Andrew Huxley and Ralph Neidergerke, but mostly by Hugh Huxley and Jean Hanson. And these two proteins form filaments in the muscle, 400 molecules in the muscle, and there are two types of filaments, thick myosin-containing filaments and thin actin-containing filaments. The thick filaments are in the center of the sarcomere, in the A band, and the thin filaments from the Z-disk go within the A band. And light microscopy showed that the A band stays constant and, the actin filaments stay constant, so the filaments slide, which is the basis now of the next thirty years of muscle research.

So of course, each individual molecule undergoes a cyclic process, which includes a structural change, and that is what produces sliding. Mihály and I showed that you can split up the myosin molecule with trypsin into soluble and insoluble fractions, light meromyosin and heavy meromyosin. Light meromyosin is responsible for filament formation; the heavy meromyosin and even the head, S-1, which Susan Lowey and Weeds isolated, combines with actin and has ATP-ase activity that provides energy. They

are soluble, so you can study contraction with single molecules; insoluble was the next major breakthrough. The third breakthrough, of course, was crystallization of the myosin head by Rayment and colleagues and crystallization of actin by Kabsch and Holmas.

AK: Crystallization.

AS: Actually x-ray diffraction can determine the structure at atomic resolution, and you can see where changes occur and what is the more detailed. It is still not completely solved, but at least in the detailed state, a great deal is known about the structural changes.

AK: Tell me about the adjustments that Albert had to make when he came to the United States. I mean, he believed in the universality of science, that there are common values around the world among scientists. In fact, I think he said he would be more at home with the scientists thousands of years ago than he would be with his own milkman.

AS: Yes, that's right. He feels closer to an Indian scientist than a milkman, even though he was quite patriotic, you see.

AK: Were there any cultural changes or adaptations that he had to make or couldn't make?

AS: Well, yes and no. He really decided to go to Woods Hole. He decided because he remembered a Physiology Congress in 1928, when the Congress was invited to Woods Hole by Dr. Warbasse, who was a well-known surgeon and wrote a book on surgery, and he was one of the founders of the Co-op Movement.

AK: The what movement?

AS: Co-op. Co-operative. And Albert remembered the place, and he knew there was a marine biological laboratory there. He remembered and went there, although originally he worked a year or maybe two years at NIH [National Institutes of Health] and came over to Woods Hole in the summer. In '47, he spent about six months, I guess, I don't exactly know, at MBL, then went to NIH. By the time I came in '48, I know that in the winter he was there, because we visited him there with some of the young Hungarians whom he took on, he brought over.

So he was very well treated. The impact of the muscle work was realized only when he came to the U.S. because of the isolation of Hungary during the war. He didn't want to publish in Germany, in the German language. He couldn't publish in English, because Hungary was at war, so that's why it was in Szeged that all the original contributions were printed. You see, they were unavailable. I don't know whether you have a copy of the

institute publications, of the Szeged. Is it of interest to you? I don't know whether the library has it.

AK: I can certainly take a look.

AS: If not, I can give you a copy.

AK: Yes. Thank you.

AS: A Xerox copy, because I have it. Anyway, so that was easy. He didn't have the same respect, you see. He was so well known in Hungary that when he returned, his first visit, when he became eighty years old in '73, he was interviewed, and I was told that in Budapest, the whole traffic stopped. Everybody was listening to the interview. My daughter, who was studying in Hungary at Botpalád—a godforsaken small village, because it had to be the most primitive one—she said that everybody knew him and listened to him. Even now, we went to Hungary, because they decided that Malév, the Hungarian airlines, which directly flew from Budapest to New York and to Toronto, decided to name the transatlantic airplanes after outstanding personalities in Hungary, and the first airplane was named after him, and we went to the christening this last March.

AK: So in Hungary he was a national hero.

AS: National hero. You see, he was the only Hungarian Nobel Prize winner who got the Nobel Prize because he worked in Hungary, and Szeged was the center of paprika production. But they knew how he behaved during the war, and he was really still a national hero.

AK: Well, he became, of course, very well known here, but not . . .

AS: Not the same. Not the same. The U.S. has a hundred Nobel prizewinners, you see. Hungary had actually twelve, but only one who lived in Hungary and exposed himself politically and so on, and took great physical risk. The second thing which was difficult was his past sympathy towards Russia with which Hungary had to coexist. As McCarthy became powerful, he had difficulties. I don't know the details, but I know that he writes that he was considered too friendly to the Soviets by McCarthy.

And he didn't want to join a university. Irving Klotz—by the way, I am sorry to deviate, because I will forget. There was among the papers "The Wisdom and the Wit of Albert Szent-Györgyi." You have that little short article? Anyway, I will give it to you.

AK: I hadn't seen this. Thank you. This is "The Wit and Wisdom of Albert Szent-Györgyi" by Irving Klotz of Northwestern University.

AS: That's right. He is a well known chemist who taught in Woods Hole for a while, in the summer courses. You see, some of his wit comes through. You'll see a part of the "ignose" and "godnose" story and a number of things. You will see it. I don't have to go into it. So anyway, he got an invitation—and he describes in there—with the whole group, the Northwestern University, with a salary of about three times more than what other professors got, and he decided he would stay in Woods Hole.

AK: Why?

AS: Because he had found a place in Woods Hole and he didn't want to participate in this academic life.

AK: Meetings, committees.

AS: Committees and so on. He came into the lab until he was ninety-two years old, every day. Every day came in at nine, stayed until five, and then he went home, and that is when he wrote and thought about things.

AK: Well, he decided against a formal academic career, and yet he was known as a wonderful teacher.

AS: Yes.

AK: So you could you tell me a bit about his teaching? When did he teach? How did he teach? I know he gave an annual lecture.

AS: He gave an annual lecture, at the physiology course, which was an outstanding course.

AK: At Woods Hole.

AS: At Woods Hole. The concluding lecture was his and it was a great lecture.

AK: How about more informal teaching?

AS: It was in the lab. It was in the lab with the people, and it was not really any formal teaching. You didn't learn, as I describe in *Search and Discovery,* the method. You didn't learn techniques from him, or methods. He had fantastic intuition and some of his ideas were childish, some of his ideas were great. You see, it was a mixture. He really formally taught, in the first years in Szeged. Straub and Laki were his students. Laki describes that he couldn't answer the questions in the exam, not all the questions, and when he was called into the office, he was afraid that he failed. But he was invited to join the group.

And also Straub was invited to join the group as a first year medical student. Both became, especially Straub, real internationally, well-known scientists. Both of them contributed in their work on the C-4, the carbon cycle, which preceded Krebs, and which Krebs extended and expounded. But there was discussion and the atmosphere was one of absolute happiness to work. It was the greatest thing to become a scientist and do science.

AK: And he communicated that.

AS: And he communicated that. The atmosphere, you see, was the absolute honesty and excitement of doing science. Atmosphere was more important, you see—you had to learn yourself. You had to be fairly independent in knowing what to do. But even though he switched from muscle in 1952 and I stayed in muscle, he didn't mind it. As a matter of fact, it was then I found something important. It was in '52, '53, a paper on light meromyosin and heavy meromyosin, which I discussed here and in a Hargittai interview. The atmosphere carries with you. I must say, I had the feeling, once I decided to go into science, that if the situation was so bad and in Hungary was no certainty, that even if I do it for one year, it is worth it. I remember my attitude. Then in addition, with contact with him, it was worth any sacrifice or whatever happens.

AK: In 1939, he gave a series of lectures at Vanderbilt University. It was the Abraham Flexner lectures on fermentation and other subjects. He made the following observation, which I would like you to comment on. He said, "To the outside spectator, all of this work of the biochemist in which he shifts little hydrogen atoms and the phosphate molecules from one substance to another, must seem a little like play for big children. Thank heaven that this is really so and that biochemistry is a lovely game of refined cookery, very fit for the amusement of big children. I often suspect my really good colleagues of being attracted to their working desk less by the desire to elucidate profound problems or to save humanity, than by the childish pleasure of playing about to make fluids boil and to pour them from one bottle to another." Scientific research as play for big children.

AS: Yes. I think that is a very good description of him. He knew himself, you see. He knew himself. He was very realistic about himself, that which comes from the Apollonian and Dionysian and so on. I gave you the article, which is quite interesting, why does one do research. And that is the short one-page article where he describes that a scientist needs to be curious. If you ask scientists to be useful, it will kill science. That was the attitude, you see. It will kill science, and he considered it to be his life. I mean, vitamin C was very important health-wise, of course. And people think that he discovered vitamin C because he wanted to save lives and that's why he worked. But nothing could be further from the truth. What he said was that if anybody wants to save lives, he shouldn't do

science, shouldn't come to the laboratory. They should go to an agency which—insurance agency or agency which donates money and so on—that supports health. But not in the lab. It was, of course, an exaggeration.

AK: It was an exaggeration. He said "most of my really good colleagues I know are like this, too." Would they admit it?

AS: I don't know. I think most people are committed, really, to do things which they are good at. It is no use to try to do things—I couldn't work in cancer research, you see, because I am not good at it. I don't know what to do. I remember my technician said about some work I was doing—it was good work in the lab—he said that I was interested in it to find a cure. I said, "No. I am doing research because I am curious. I want to satisfy my curiosity." Of course, I would be interested if I knew what I should do to find a cure, and I don't think Albert denigrated clinical research. But what he said and very strongly emphasized was that there will be no clinical research if there is no basic research. And it used to be great, NIH's support for basic research. Now, of course, Albert had difficulties with NIH. You know the story probably. He was not able to write a grant.

AK: You said at one point he was constitutionally unable to write a research grant. He just couldn't do it.

AS: He couldn't do it. He wrote somewhere that he doesn't know what he will do three years from now, so he has to lie. And if he would write a research project three or four years ahead, he would have to lie, you see. And there is much truth in it, because people write research grants for things which they only started to do or for things where they know what will happen. In the American system, young people get money regardless of their bosses. It is great, and whenever you want to have good science going on, you have to allow young people to do their own ideas and sink or not. But the fact is that past achievements don't matter. What matters is what you have done lately. He wanted to have a relatively small grant, you see, and he couldn't get it, because his ideas were many times considered childish. He was trying new things with submolecular biology.

AK: To what degree was this Albert and to what degree was this a cultural difference between science in Hungary or science in America?

AS: Science in Hungary was not healthy in many respects, because the professors had absolute power, and they usually didn't work in the lab by that time, only very few. So his institute, the laboratory in Szeged or in Budapest, was a very unusual situation in that the professor, Albert, talked to you. For instance, personally, I didn't consider going back to Hungary, even if the revolution in '58 had succeeded, because of the graduate education in this country. Here, if I gave a lecture, graduate or undergraduate, if you didn't agree, you could speak up very politely, but you could speak up.

AS: With my graduate students, I had to justify every statement which I made, because they would politely disagree if they thought that it is not so, and that was a very healthy attitude. In Japan or in some respects in Hungary, they want to prove the professor is right. Here, the students are challenged to prove their professor is wrong, you see, and that is a very healthy situation.

AK: Which he fostered in Hungary.

AS: And he fostered in Hungary, you see.

AK: Well, now, even in the United States there have been conflicts within the world of science about support for basic research versus applied research, and how you justify the money expended on the one. So in a sense, he was responding to tensions that are endemic to the field, really.

AS: Yes. Sure. That's one of the reasons I gave as a first little paper, "Science Needs Freedom," because it is partly historical here. Science, physiology and biological science is started by practicing medicine. The attitude towards basic science developed slowly and great change came. Two aspects, the Nazis expelled the Jewish scientists, number one. Number two, science was supported by the government where Congress and authorities didn't interfere, where it was essentially basic science. This has been changing

and I'm sorry to say it started, at least what I remember, with Johnson's Rose Garden speech. You see, they want to see what are the results. Then [U.S. Senator] Proxmire took, for him, a ridiculous sounding grant . . .

AK: What did he call it? The Golden Fleece Award?

AS: The Golden Fleece Award. Even though NIH maintained its support of basic science, especially when science became really developed partly due to Sputnik. Then the agencies were asking you to apply for grants, you see. That spirit is more or less eroding. You could see it when these two Democrats, by the way, Johnson and Proxmire, and now the extreme conservatives want to—you see the hullabaloo about abolishing 200 grants for ideological reasons.

I am convinced, and Albert was convinced, a number of people are convinced, if you look at the history of what has been going on—for instance, the identification of genes which go wrong developed by accidental conversation between Berg, Boyer, and Cohen. They developed things which were not even dreamed of by Watson and Crick. I remember the first isolation of a gene was by Brown, the microsomes, which was a tremendous amount of work, two years' work. But now undergraduates can do it because of techniques developed due to basic science.

AK: Yes. You mentioned that article in *Népszava*?

AS: *Népszava*, yes. That is what it was. It means "The voice of the people." It was a social democrat, leftist publication, before the war.

AK: He wrote that in the early forties, in Budapest, in wartime, and it was called "Science Needs Freedom," and of course, he said, as you mentioned, that "to judge scientific research by its usefulness is to kill it. Science aims at knowledge, not utility." Now, did that view of his change or modify over time? What I'm thinking of . . .

AS: When his wife had cancer. He wanted really to be—you see, he had a very tough time with cancer, because his second wife whom he loved very much, died at the age of fifty-three, and his only daughter at the age of fifty-one, of cancer.

AK: This was Nellie?

AS: Nellie. Marta and Nellie. And he was convinced that with his approach, he would know what to do. Now, in a way, the retine and the promine was intuition, you see. That will be a balance of an inhibitor of cell division and an advancer of cell division, so that was okay. On the other hand, he couldn't appreciate molecular biology, so the techniques which are employed there were beyond him—he didn't develop with them. That happens

with every elderly scientist, you see, because by that time he was sixty-three or something like that. But he was convinced that he would be able to solve, or make a significant contribution to solving, the problem.

AK: Let me read something else that he wrote, and I would like to get your reaction to it. This was in his 1972 book, *The Living State,* with observations on cancer. He was seventy-nine at the time. I found this paragraph quite touching. He says, "My own scientific career was a descent from higher to lower dimensions, led by the desire to understand life. I went from animals to cells, from cells to bacteria, from bacteria to molecules, from molecules to electrons. The story had its irony, for molecules and electrons have no life at all. On my way, life ran out between my fingers. The present book is the result of my effort to find my way back again, climbing up the same ladder I so laboriously descended. Having started in medicine, it is befitting that I should end with a medical problem, cancer, which took away most of what was dear to me." That sounded quite sad.

AS: Yes, in a way it is. The end had lots of sadness in it, you see, and that is in the [John T.] Edsall memorial, in *Nature,* when Albert died. You have that one-page article. [Looks for article.] Oh, here.

AK: Oh, yes. I have seen this.

AS: You see, well, there is a sentence . . .

AK: "Like many jesters, he had a deeply tragic vision of the world. . . .He was appalled by the follies of its rulers, teetering constantly on the brink of destruction. Somehow he still managed to cherish hope." This is from John T. Edsall of Harvard University, upon the death of Albert.

AS: So when he became ninety years old, of course, you don't improve with age. [Laughs] I mean, your mental ability doesn't improve with age. I remember the time we went to the Caribbean, it was a sailing trip, and he invited us, my wife Eve. That was before Eve's death. It was, I think, in 1980, if I am recalling. Already he had this disease, a sort of leukemia, but not a rapid leukemia. He had an idea and he wanted to write and he had difficulty writing by that time, an article to *Science* which he found very important, and it was very, very touching.

We still played chess. He was an enthusiastic chess player, and his colleague Guba, who came to Budapest, was his partner usually. When we came to Woods Hole, I became his partner. The rules were such, you see, that we don't think too long. Within a minute, not more than a minute, and if somebody makes a foolish mistake, then we say, "You shouldn't do that," because that is when interesting situations developed. And that was the fun. And everybody knows when one's abilities are going down. He was a marvelous

writer and great at making statements. Everybody knows about the big hook and the fish and so on. There is a statement which he wrote, "Discovery is an accident meeting a prepared mind." You know this? And everybody who sees . . .

AK: "To see what everybody sees . . ."

AS: . . . "but to think what nobody thinks." The three nuggets. So he knew that he was not up to what he was.

AK: Now, his pursuit of cancer may be partly motivated by his personal losses, but . . .

AS: He had this curiosity, too. He had this curiosity about free radicals, because he was doing the classical biochemistry, trying to isolate the substance that causes cancer, or which in a way prevents cancer. The problem was the normal techniques. After all, you know, something like the P-53, which is destroying cells and destroying cancer cells, and if you have a gene problem there, you will certainly develop cancer. Now, of course, cancer will have many different genes, depending on whether it is colon or breast and so on. But he tried to isolate promine and retine, so that was the normal biochemical process, which used to be very useful.

AK: Right.

AS: But now the production is greatly eased by expressing, if you can express the protein and the gene products. That is what, to me, the human genome is facing now. You have these 20,000 or 30,000 genes. You have to express them to see what does what, and the system is very complicated. He used a simple approach, you see. At the same time, he had the idea of free radicals, which, again, it was not completely foolish, because after all, we drink red wine now to reduce the free radicals, which is a good thing; nothing against it. [Laughs] But here I think his intuition was not matched by the technology required to test it. But he had lots of fun, and if you read—again, I think I will give you all these things, because I already printed it all, but if you read the epilogue by Michael Kasha—Michael Kasha is an excellent chemist–where he describes Albert's eightieth birthday.

AK: I was going to ask you about a time back in the 1950s, when Albert spent six months at Princeton's Institute for Advanced Studies. He was hoping to learn more about electrons and quantum physics, and seven years later, when he wrote his book, *Bioenergetics,* he said in that book that he "found at Princeton a profound and sympathetic interest in biology; however, when I revealed that living systems contained more than two electrons, physicists turned their backs on me in terror, mathematical difficulties being insurmountable." [Laughter] And he said, "I have no doubts that the coming century will witness a profound revolution, the establishment of a quantum mechanical biochemistry." Was he right?

AS: I don't know. You see, I think that was an optimistic statement, very optimistic statement. At least, you see, that's why he had the Pullmans over.

AK: The Pullmans, yes.

AS: You see, I had my doubts. Not that I know anything about quantum mechanics. Now, Albert had, and that was partly the influence of Laki, but the 1941 article already shows what he was thinking for the future, even though that time, '42, really, was one of the epoch-making discoveries in muscle research. I wrote this old history of muscle research, because the new generation doesn't realize the beginnings. Us old folks, you see, getting gaga, still remember, and H. Huxley describes it very beautifully, and it comes not from a relative but it is from a person who did the second major discovery, how he built, how he couldn't have done it without that; it was enabled by the work in Szeged.

AK: Well, Albert's thoughts in the mid century about quantum physics and what it might say about biology may have been optimistic, but there have been developments in submolecular biology that he was anticipating.

AS: Yes, sure, and he used ESR, electron spin resonance. Yes, but he would have needed probably a large group—I don't know. Maybe he had five people, not forty. And he never claimed to be a quantum mechanics expert, which would be needed to achieve what he

was thinking. But he couldn't have worked with forty people. It was not his style. He could work with one or two or three or maybe four people. You could see it in the Szeged work—Straub, the muscle part, [F. Bruno] Straub and [Ilona] Banga. The rest was also contributed, but it was not the main contributors, if you look at the first studies, and the second studies. It was a miniscule group.

AK: Let me ask you about another aspect of his beliefs about science. This also goes back to the *Népszava* article. He set a very high standard of commitment. He wrote, "Scientific research is a passion. The real scientist is driven by this passion and is ready to bear privation and, if need be, starvation, rather than let anyone dictate to him which direction his work must take." That's a very high standard. Partly it's because he's writing in Europe in 1943. What is your reaction to that standard?

AS: When he left Hungary and went to study in Holland and so on, he was very poor. In 1926 there was a congress I think in Sweden, and he ran out of money because Hamburger died and he was replaced by a psychologist who was completely uninterested in biochemistry. When Albert showed his paper to him, an important paper, which resolved the differences between Wieland and Warburg about cell respiration, his response was "Throw it in the wastebasket or you can publish it, whatever you want." He sent his daughter and wife home and decided to attend the congress and then to commit suicide. Whether he would have committed suicide or not, we don't know. But [Gowland] Hopkins mentioned his

name and work in the opening lecture about three times. After the lecture Albert introduced himself. Hopkins saved him by offering him a job in Cambridge.

AK: He was in fact starving at the time.

AS: He was starving at that time.

AK: He had edema.

AS: Yes. But there were a number of things which are in the biography, so you know it. Through Hopkins he learned the most, but they hardly ever discussed science. Hopkins was completely different from him. Hopkins was a shy, modest person. Albert wasn't shy.

AK: But Hopkins appreciated that.

AS: Oh, yes. Albert had this tremendous sense of humor. In one photograph he pretends he's hanging himself, and the caption is, "The theory didn't work." So he was quite different from Hopkins, but he had a love towards Hopkins. He writes, in '45, his first trip, when he traveled, he was going to see Hopkins. He was now old and very sad, and he told about him, being in the barbershop, talking with his friends. There was some—I will have

to recall—some shouting developing, a struggle, and Hopkins realized he might be in trouble; in the barbershop, after all, they had the razor. So he jumped up and ran out of the barbershop to go to an island—a traffic island—not to be pursued, because he was afraid that it was a setup. Albert used to tell the story, "You know, if I wanted to talk to Hopkins, what I had to do, I had to knock on the front door and rush back to the back door, because Hopkins, when anybody knocked, was escaping." [Laughs] But you see, of course, they discussed things. Again, it was an atmosphere at Cambridge at that time. He was with lots of outstanding people.

AK: What price did he pay for his adherence to his ideals. Even at Seven Winds, I understand, he didn't even heat the house in the wintertime.

AS: That's right. In the wintertime, it was a very small part that was heated, because he didn't have money. He decided that when he had difficulties between grants, the Armour grant and so on, that he would pay it from his house.

AK: Mortgaged his house.

AS: Mortgaging his house. He expected that from everyone, in a way. The salaries which we were paid were miserable. I remember I had $250 a month, which was not a very large amount, having a family, even at that time.

AK: Was he sympathetic if people left and did something else? Or did he feel as though they had . . .

AS: I never discussed money with him. I cannot tell you, because people left. Most of the Hungarians left very soon. I was practically the only one who stayed for a long time. People asked me, "Is it hard to have a cousin"—they said "uncle" really—"who is so famous and such a personality and to work there? How did you feel?" I said, "Well, of course it was at first disturbing because I knew I would be on my own, but I had the feeling that I really hated all the old biochemists, because they already had discovered everything worthwhile to discover." You see, that is the feeling which one has very young. Nevertheless, I decided that if I don't find something which is sufficiently interesting for me to stay, then I can always go to medicine. That was my attitude, so I refused—I was getting invitations. I refused all of them until '62.

AK: And then you went to Dartmouth.

AS: To Dartmouth, yes. It was time.

AK: When Albert was over ninety years old, he was asked what he would have done differently with his life. This is in Ralph Moss's biography.

AS: Yes.

AK: And he said, "I would have devoted as much attention to my love relationships as I do to my scientific work." I wonder if he was rethinking some of the . . .

AS: I don't think it was a real, honest statement. [Laugh] I don't think it was, because he was very devoted, with the exception of the third wife. I am told, and I cannot say it was, that when he divorced the first wife and there was a transitional time, then he had a number of affairs, until Marta came along, and he still loved her twenty years later. He appreciated his fourth wife, who took very good care of him.

AK: This is Marcia?

AS: Marcia.

AK: This might be a broad question, but I'll ask you anyway and you can narrow it if you wish. If you stand back and you look at the large sweep of Albert's long career as a scientist in Europe and in the United States, one might think of two general kinds of contributions he made, two kinds of achievements. One was what he discovered; the other might be the way that he discovered it. The one being the substance of his work; the other one being

his very unique style of working. Which of these two do you think had the greatest influence on you and which of these two do you think is the largest part of his legacy.

AS: Well, I have to be careful, because I almost said, "It is the style," but it is more than that. The style was built on the achievements. I don't know whether you have seen his book on respiration—I have it here. I learned English from that, because my mother, who spent time in England, taught me English. English was not taught in school, so I remember at the age of fourteen to seventeen, I was reading and rereading his book on respiration, because it already had—there it was, his style. He started the work even before he went to Cambridge, the famous very short article proving that both oxygen and hydrogen had to be activated, therefore, Wieland and Warburg are both correct.

AK: So when you say that it's difficult . . .

AS: You cannot separate the style from the substance because he was a real person. His contributions were real, and he had a style which was quite a unique style. His style in politics, especially at first in Hungary, was that he was not a politician, but a critic. Whenever he felt something was wrong, he said it without hesitation. For instance, he was invited to give his belief in education, which he thought was very important for young people. Their mind is open, and even at ten years old, they are willing to get things. But you teach them and they come out as dull individuals, looking for things that

are not necessarily the important ones. He went to the teachers meeting and he told them that their methods of education had to be changed. That was a central issue, so he had no hesitation in stating his opinion. So there was this drive to improve things. He said that it is very important to have gymnastics and athletics. He was really a good athlete—he enjoyed sports. But that was part of the style and that was what made such an impact in Hungary.

He took risks in politics. When he wrote the letter to Krebs, he realized in 1933 how disastrous the Nazi policies are and what will happen to the Jews. In the synagogue, one of the big, large synagogues in Hungary, in Budapest—I think in Europe one of the largest synagogues, maybe the largest—is a museum where they show about the Holocaust. They show an article which he wrote after his Nobel Prize condemning the Hungarian anti-Semitic policies and what happens in the Nazi deportation of Jews. He and Bartok were two people who opposed the Nazis. Bartok forbade the playing of his music in Germany and emigrated to the U.S. in 1940; Albert stopped publishing in German and risked his life in opposing the Nazis.

AK: I partly asked the question because the style was so inspirational, and it caused people to change their own ways of thinking.

AS: Yes.

AK: Their own approach to research. So many people have said that.

AS: I was at a conference on cell assembly and I discussed muscle. It was in Crete. And Karpus, an outstanding theoretical kineticist, was there, and he told me, "When I was young, I read Albert's books and heard him in Woods Hole as a student and that made me to go into science." And Albert influenced other people such as George Wald. In a small country like Hungary, the reverberation was much greater. The influence was much greater.

AK: George Wald, with his Vitamin A work.

AS: Yes, Vitamin A work and, yes, and they became very close friends. I remember in '48 we lived for a while in Albert's house, about three months, and George Wald was one of the first persons who came over, discussing. That was before George Wald's Nobel Prize. I happened to meet a large number of people who usually one wouldn't have occasion to meet. So there were many pluses in my connection with Albert.

AK: I'll just ask one more question of you, and I've covered so much that I think what I wanted to, but I would like to ask you, is there anything else that we didn't talk about today that you would like to say about Albert, anything that we didn't cover?

AS: You see, he was—this is a little bit personal. He was a fourth generation of scientists of the Lenhosséks. I was in Transylvania for a student science meeting, and they were interested in Albert so I gave a talk, "Albert, Scientist and Humanist." I can give you lots of quotations from various books about Albert, and the autobiography.

The first Lenhossék was a poor boy, the son of furniture maker, who became a sort of valet or footman, at a very young age, to the Jesuits, who discovered how bright he was. So they sent him to gymnasium [high school] and sent him to medical school. He became professor in Vienna of physiology, wrote a five-volume—I think five-volume in Latin—book of medical physiology. He was among the first in Hungary in the early 1800s, who introduced Jenner's smallpox treatment. He became the chief medical person in Hungary—here, what is it?

AK: Surgeon General?

AS: Surgeon General here. He introduced vaccination by telling the priests not to baptize anyone, any young child, who was not vaccinated. He was the house doctor of one of the famous Hungarian counts who started the Hungarian academy in 1825, giving one year of his own income to build an academy, and defined the constitution that no political influence should govern the academy. Lenhossék's son who became a professor of

anatomy, was Albert's grandfather on the maternal side. His son, Albert's uncle, became a very good scientist and was Albert's substitute father who greatly influenced him.

Albert was the fourth generation of scientists on the maternal side, covering about 200 years. Albert's uncle died maybe six months ahead of Albert getting a Nobel Prize, so he never knew it. By contrast our common family were judges and lawyers for generations. I know our grandfather was what we would call, I suppose, a Supreme Court justice, in Hungary, but nevertheless, Albert was delighted for me to become a scientist and he was delighted that my son became a scientist, a molecular biologist.

You may want to see some of the photographs.

AK: Yes, I do. Yes, thank you very much.

[End of interview]

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