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INTERVIEW WITH

DR. JOHN GERGELY

June 21, 2004

John Gergely, M.D., Ph.D.

Biographical Statement

Dr. Gergely was born and raised in Hungary, and earned his M.D. degree at the University of Budapest in 1942. It was there that he met Albert Szent-Györgyi, who became head of the university's Department of Biochemistry in 1945. In 1946, with assistance from Szent-Györgyi, Dr. Gergely traveled to the University of Leeds in England, where he studied quantum aspects of biochemistry—the idea that proteins can exhibit properties similar to semiconductors. That year Szent-Györgyi emigrated from Hungary to the United States. When Dr. Gergely earned his Ph.D. from Leeds in 1948, he too came to the United States and obtained a position at the National Institutes of Health in Bethesda, Maryland, where Szent-Györgyi had been awarded a research grant. Dr. Gergely stayed at NIH for two years, then moved, in September 1950, to the University of Wisconsin's Enzyme Institute for a year's study before obtaining a position at Massachusetts General Hospital. He spent the rest of his career in Boston, as a biochemist in the Department of Neurology at Mass General, as Associate Professor in the Department of Biological Chemistry and Molecular Pharmacology at Harvard Medical School, and as Senior Scientist at the Boston Biomedical Research Institute. In 1987 he was awarded an honorary Doctor of Medical Science (D.M.Sc.) degree from Semmelweis University in Hungary.

Interview Synopsis

Dr. Gergely reviews Szent-Györgyi's years at the University of Budapest in the mid-1940s, as well as his own, and Szent-Györgyi's decisions to emigrate to the United States. He then describes how his career path in the United States led to permanent positions in the Boston area, while Szent-Györgyi found real happiness conducting independent research in his own Institute (for Muscle Research) in the peaceful atmosphere of Woods Hole, Massachusetts. Dr. Gergely describes Szent-Györgyi's evolving research interests, from muscle to quantum physics, and its application to biology (the "living state"), and finally to cancer. He also discusses Szent-Györgyi's work ethic in the laboratory, his happiness at Woods Hole, his dedication to science and personal independence, and his flair as a lecturer and teacher.

**National Library of Medicine
Telephone Interview with Dr. John Gergely
Conducted on June 21, 2004, by Adrian Kinnane**

AK: Interview with Dr. John Gergely, June 21, 2004. You received your medical degree in 1942 from the University of Budapest and you were there also for three more years in the Department of Pharmacology.

JG: Right.

AK: You had known of Szent-Györgyi's work, of course, after the Nobel Prize he received in '37. When did you first meet him?

JG: I think it was in late 1945.

AK: And it was when both of you were at the Medical School of the University of Budapest then?

JG: Yes. But let me say something in connection with how I joined Albert's department. I took a course, not formally for a grade, in the Fall of 1945 in Technical University given by a professor of physics named Gombas on quantum mechanics. I told him of my interest in what Szent-Györgyi had written in 1941 in the magazine *Science* on the role of quantum mechanics in biochemistry. Gombas was the catalyst to bring us together, which resulted in my moving into Szent-Györgyi's Biochemistry Department. I had worked there as a

medical student when it was a different smaller department, before Szent-Györgyi took it over.

AK: Szent-Györgyi did?

JG: Yes, Szent-Györgyi, in the summer of '45, I think it was. I joined him there in '46.

AK: In January of '46.

JG: Right.

AK: He left the next year, I believe.

JG: Yes.

AK: And then you left also?

JG: Well what happened is that during, maybe it was during '45, while I was in the Department of Pharmacology, the Hungarian government announced scholarships or fellowships, doctoral level, for visiting professors abroad and I did apply. And I knew that there was some interesting work going on in England and I did apply for a fellowship to go to England. I received an award but there was no money available so this was put to the side.

And in early '46, the British Council started operating—you know the British Council?
Are you familiar with them?

AK: No, but explain.

JG: Well the British Council was an organization that fostered connections between scientists abroad and in England. And they started operating in Hungary in 1945 or early 1946 and were about to offer the competition for fellowships. And they got hold of the list of those people who had been awarded fellowships to go to England by the government but were not funded. And out of those they selected the group to start working in England in the fall of '46. Now here again, Szent-Györgyi came into the picture because he himself was familiar with England because he spent—but you know that.

AK: Yes, at Cambridge in the 1920s.

JG: Right. One of his colleagues, Koloman Laki—he died in 1971 I believe—had spent a year, 1938 or 1939, in Manchester in the Department of Chemistry. He worked with M.G. Evans, who was at that time a senior lecturer or reader. You know that's just like an associate professor. He was a person who was working on quantum physics applied to chemistry. And by the end of the war he became a professor in Leeds. So when I was discussing my plans with Szent-Györgyi, he and Laki said that I should go to Leeds

because that was the best place to get involved in the quantum chemical approach to biochemistry.

So I contacted Evans and arrangements were made for me to go to Leeds in the fall of '46. And Szent-Györgyi, I think, next spring left for a trip just to visit Europe and I think also America. But in the meantime, things in Hungary politically became worse and I think he decided not to go back to Hungary. Particularly, he had a friend and associate who was dealing with financial aspects of the fund and I think legally he had some problem with the Communists. So he decided not to go back.

AK: Right, this was Stephen Rath, I believe.

JG: Oh yes, you know about that. And so Szent-Györgyi then decided to settle in Woods Hole and also he had a connection with NIH also, sort of a senior visiting fellowship there.

AK: Now how did you decide to come to the United States? How did that happen?

JG: Through Szent-Györgyi actually. Well when I arrived in England, M.G. Evans said, "Well why don't you"—he knew that I had been taking courses in chemistry at the University of Budapest—"Why don't you register as a"—what do they call it—"as a graduate student, research student?" And I did that and actually this led to my getting a Ph.D. by the summer of '48.

AK: At the University of Leeds.

JG: Yes. And in the meantime, Szent-Györgyi, after he had settled in the U.S., started to gather former associates who had managed to get out of Hungary and found temporary positions in Europe to get to the United States. While I was in England, I was able to help with arrangements for Laki to come to Leeds. And he was one of the first to join Szent-Györgyi at the NIH in Bethesda.

AK: Koloman Laki?

JG: Yes. Of course, I don't have to tell you Andrew Szent-Györgyi's story. And there was another, a man called Rozsa, did this name come up, Rozsa, George Rozsa?

AK: George Rozsa, yes.

JG: Well I think he spent some time in Switzerland before he came to the States and he also ended up at NIH. Now, the chief reason that led me to Szent-Györgyi, apart from the paper in *Science*, was that the work which was done in Szeged became published as *Studies from Szeged*. And I was really impressed by it. Somehow, it seemed to me that it was a different kind of science than what prevailed in the literature at that time. It was much more focused on specific proteins and on muscle where the functional proteins clearly

could be connected with their physiological role; i.e., muscle contraction. And so this seemed very attractive and this was an important factor in my interest.

Just to give you more of my background. When I went to England I went alone. And I had a wife then and two small children. It wasn't easy to leave Hungary, but luckily at that time there was a collaboration or cooperation between Britain, I think France, and the Soviet Union. There was sort of a military committee in charge . . . well Hungary was still occupied. And so the British managed to get exit visas for my family and so when things changed for the worse and Szent-Györgyi had already left, then they were with me in England, so we could undertake the move.

Actually at the time when I had decided to go to the States, M.G. Evans was going back to Manchester as chairman of the chemistry department there and he actually offered me a position there, but that was a straight chemistry type of thing, and I thought that working with Szent-Györgyi, who combined physical approaches and biochemistry, would be more in the line of my interests.

AK: Yes, now you came to the United States then in 1948?

JG: Right.

AK: And where did you go?

JG: Well, I had received what I think they call a special fellowship.

AK: At NIH?

JG: At NIH.

AK: Does that mean that you were in Bethesda?

JG: Yes.

AK: And how long were you there?

JG: Well, actually towards the end of the year we were talking about the possibility of my joining Szent-Györgyi's group in Woods Hole, but I think there was some financial reason that he felt he couldn't expand on there and I continued at the NIH. He himself had left by the summer of '49. But we maintained contact during that time, 1949 to 1950. You know, he founded at the NIH a little group of Americans who had been there on the staff, Bill Bowen, Sam Spicer, in addition to Laki.

AK: Bowen, okay.

JG: Let's see, there's another one who had been introduced to muscle biochemistry by Szent-Györgyi. And they had positions there and were members of the . . . NIH had sort of a semi-military corps. You're familiar with that?

AK: Sort of a Public Health Service officers.

JG: Right. But . . . they stayed there but I still had contact with them and I published some papers together. I published some theoretical stuff with Laki, but I never published with Albert.

AK: How long did you stay at NIH?

JG: I stayed from October 1948, '49 to September '50. I actually wanted to learn more about biochemistry as it was carried out in this country. And I spent a year in Wisconsin, at the Enzyme Institute with David Green. And in the meantime I did receive . . . an American Heart Association established investigatorship. And so I was considering going back to the NIH. But by a quirk of coincidences, through an old friend who settled in Boston, I found a niche at the MGH in 1951.

AK: At where now?

JG: At Massachusetts General Hospital. And I've been here in Boston ever since, but that's a different story for our purposes today.

AK: Well, during the '50s then, tell me a bit about your contacts with Szent-Györgyi. Did you go to Woods Hole every summer, or was there a regular . . .

JG: I somehow never got into the habit of setting up a lab in Woods Hole for the summer. We found a house in Nahant.

AK: Nahant

JG: And that's right on the ocean and we eventually were a large family with eight children. We decided, you know, that this would be good place to stay and a very good place in the summer for the children. So I didn't feel that setting up a lab in Woods Hole in the summer as many people did would be worth the effort. But I had occasional contacts with the people at Woods Hole, with Andrew Szent-Györgyi and some of the other people who stayed there from the old days. But Albert's interests really moved out of muscle. His last real work on muscle was—and it was very important, as it turned out—on glycerinated fibers, which became a very widely used model. He was getting involved— well, just as a background for you—I think he had many friends and contacts with physicists and physical chemists and so on. But those years, I don't know whether in the end that led him into anything like his previous discoveries.

AK: Well let me ask a little bit about that. In 1950, he spent about six months at Princeton's Institute for Advanced Studies hoping to learn more about electrons and quantum physics and in 1957 in a book that he wrote called, "Bioenergetics," he said that he found at Princeton that there was 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."

JG: Well, you know, that was very typical Albert Szent-Györgyi. I mean, he liked to make a dramatic statement.

AK: But then he went on to say, "I have no doubts that the coming century will witness a profound revolution, the establishment of a quantum-mechanical biochemistry." What do you think? Was he correct?

JG: Well, I think that this sort of quantum . . . I couldn't put my finger on direct developments that could be labeled as quantum-mechanical biochemistry . . . well spectroscopy, in a sense, yet, they're all quantum chemical concepts and techniques that are involved in fluorescent spectroscopy and other optical techniques. But when I came to Bethesda I was hoping that we could find something experimentally about these presumed molecular orbitals in proteins.

Interestingly, you know how things happen serendipitously, the idea was to digest myosin. We did some spectrofluorescence measurements which, as I look back now were very primitive, I must say, hoping that this may reveal something, as a protein gets digested, that some of these phenomena which we hoped to attribute to these orbitals would change in a way that would show some connection. Well, interestingly, this experiment led me to a very important finding that you could digest the myosin and still have its ATPase activity intact, and this is an area where Andrew Szent-Györgyi then picked up and did some very important work, which I'm sure you've heard about that. So I think some of Albert's ideas—I'm not sure if they were really received by the professional biophysicists or protein chemists with the same enthusiasm that he had for them. I don't know if you have any impression of this from others.

AK: Yes, I understand.

JG: Am I the only one who sees it this way?

AK: No, I've read that elsewhere. I'd like to read for you some statements he made and maybe this would be one way to approach this. In March and April of 1939, he gave a series of lectures at Vanderbilt University, the Abraham Flexner Lectures on Fermentation, Oxidation, and other Subjects.

JG: That was, of course, the work that led to the Nobel prize.

AK: Yes, right.

JG: Although I think that many people think that he got the Nobel Prize . . . well, it was a combination of things, but vitamin C, of course, led to some conflict with an American, [Charles G.] King.

AK: That's right, yes.

JG: But I think the Nobel was mainly based on his study of the role of tricarboxylic acid in intermediary metabolism.

AK: Yes, right. This is one of the things he said, "To the outside spectator all this work of the biochemist in which he shifts little H atoms and the phosphate molecules from one substance to the other 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."

JG: Well, I must say that I became a little bit infected. I remember now that when I was in Bethesda I was still thinking in these terms—there was at least one future Nobel Prize winner, Arthur Kornberg—that they had a little group and Herbert Tsbor was also present. It was sort of a journal club and I gave a little overview of what I had been doing and was interested in at that time. And I used this idea that in a way biochemistry is really kind of

a "cookbook." As I think back now probably that sounded ridiculous to some of these people who were, you know, honest to goodness biochemists. And I think that I sort of moved away from this kind of thing. This is one of the things that he, I think, kept cultivating. In a way I think this may have kept him from things that in a less talented person would have led to . . .

AK: Difficulties?

JG: No, no, I mean it led him to some discovery in areas that others would have considered mundane.

AK: Oh I see. Let me ask you about something else. Szent-Györgyi believed very strongly in the universality of the scientific outlook and in science's capacity to transcend national boundaries.

JG: Absolutely.

AK: It made me wonder though, what do you think were some difficulties he may have had when he came to the United States and he was trying to be a scientist in a different country, with perhaps a different set of approaches? One of the things that comes to mind, for example, is that he always seems to have had difficulty applying for research grants.

JG: Well I know he was almost proud of it. I remember he once said that when he got to the part of the application where you have to fill in your goals for the next five years, he put down, "If I knew then I wouldn't apply for this money," which was a *bon mot* but didn't endear him to the more conventional scientists and newspapers. But he certainly believed that science is an internationally uniting thing. He had close friendships with people . . . well he spent a decade in Europe after the First World War, in various countries. Astbury [W.T.] was one of his great friends, who was in Leeds after Cambridge at the time I got there.

Szent-Györgyi was a very stimulating person inculcating a strong kind of work ethic. For instance, he would say when you write papers that's for the evening; when you're in the lab then you do experiments there. I think eventually the custom here became that scientists soon, very early in their careers, give up their direct work in labs and much time is spent on grant writing, and paper writing during normal working hours, and that style, I think, was generally a little different from what his ideal was.

AK: And perhaps that's why he never really sought an academic appointment in the United States.

JG: I think so. But I think he felt happier at Woods Hole. He always had some interesting sayings. He'd say, "When I look out of the window and I see these marvelous birds flying

around, I just realize that no scientist would dare produce some bad work when nature is so . . ." you know, to him this was . . .

AK: He was happy there.

JG: He was happy and he felt a devotion to scientific discovery and an appreciation of nature there. It all made one integrated picture.

AK: That's interesting. In 1943 he wrote an article for *Nepszava*—by the way could you tell me what that means?

JG: *Nepszava* was a paper of the Social Democratic Party and the word means "voice of the people."

AK: I see. The article that he wrote for *Nepszava*, was "Science Needs Freedom." He said, "Scientific research is a passion. A 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.

JG: Indeed.

AK: Now partly he was responding to the political situation in Europe at the time, and in Hungary.

JG: Well I'm sure that you've heard stories about how he ended up serving in, well, it wasn't quite like the French resistance, but he ended almost arrested by the Germans and there was a story that he went to Istanbul to meet Allied intelligence agents.

AK: Yes.

JG: But you know that book, "Free Radical."

AK: Right, his biography, by Ralph Moss.

JG: Have you talked to him yet?

AK: No, but we're going to. When you mentioned his work ethic and that when one writes papers, one should do that in the evening at home, but when one is in the lab one does lab work, I was reminded of his quotation about scientific research being a passion and of his commitment to science in a very idealistic way, and that he was willing to pay a very high price for that at times. Do you think that he paid a price for his ideals?

JG: Well not really. He certainly was sought out by the Hungarian government in '29. Count Klebelsberg was, I think, the minister of Education and Szeged was the new university to which he brought Albert back to Hungary. So I don't think he paid a price, because of science, but certainly with the political situation towards the end of '44, I mean he certainly was known as a person who was opposed to Nazi politics.

AK: But this was more politics than science, I think you're saying.

JG: I don't think it was the science. We've already talked about how there are certain rules of the game in this country about how you get money for research, but this doesn't infringe on freedom. I'm not sure whether that was really an issue for him. I mean, that was actually more true in communist Russia, with Lysenko, the geneticist. Somehow the Mendelian or the Darwinist didn't fit how the communists in Russia saw things. I mean until the very end Szent-Györgyi was highly respected in Hungary and had access to the highest levels of government, you know, with Miklos Horthy [regent]; I'm sure their politics didn't agree, until the very end of that period when the extreme Hungarian Nazis—the "arrow wars"—and the Germans wanted to arrest him. You, I am sure, have heard all this.

AK: I suppose he didn't want to encumber his scientific work with administrative responsibilities or some of the committee work that would have come with having a career in a university.

JG: But he did actually. I mean he combined it for something like fifteen years because he was the head of the department in Szeged. He probably had able assistants who took over some of the daily chores. But I think he regularly lectured to medical students.

AK: Did you ever see him lecture?

JG: Oh yes.

AK: When and where?

JG: Well I mean, you know, at scientific meetings mostly. In the classroom I never did. He was always a very vivacious lecturer and he had some simple but impressive experiments like shaking a tube with some liquid that turned red because of the oxygen, things like that.

AK: Dramatizing.

JG: Have you seen the book on the 1972 meeting at Cold Spring Harbor, there was a big meeting on muscle. Have you access to that?

AK: In 1972?

JG: Yes, I think so. Cold Spring Harbor.

AK: I'll look that up.

JG: There are some pictures from that volume that are very typical of him, the way he stands there before the audience . . .

AK: I've seen that picture I believe, where he has the test tubes up on the stage.

JG: In the days when he started biochemistry, there was much less reliance on sophisticated equipment and he was sort of standing before the window and shaking the test tubes. I mean the Szeged work is based on very simple, and yet very important and meaningful experiments where one of the criteria might be whether, when you add ATP to a solution, do you get a sort of fine precipitate. And he would stand there—and that is very vivid in my memory—before a window and shake a test tube and look at it.

AK: Where did that occur? When did you see that?

JG: I saw this in action in Bethesda. He was still pretty much day-by-day there, with little breaks from Woods Hole. This would be the fall of '48 and the spring of '49. I remember a meeting that summer in his house in Woods Hole and he gathered some of the people who had been with him earlier. One of them was this fellow Wolfried Mommaerts. Unfortunately he died. Has this name come up?

AK: You asked me about that name when we spoke on the phone to arrange the interview.

JG: So this fellow had the initials W.F.H.M and he—and this was still in Hungary—and it shows that Hitler was not popular with many Hungarians. He was a Dutchman and he said, "Well this initial stands for"—do you speak some German?

AK: No.

JG: Well anyhow, the WFHM, he said, stands for "Woe to Hitler and Hail Mommy."

[Laughter]

AK: Oh, that's good.

JG: "Hail, Mommy," you know, we used to call him Mommy.

AK: When was the last time you saw Szent-Györgyi?

JG: Well, it must have been a meeting in Woods Hole. He died in '86, so it must have been the early '80s perhaps. By that time, I mean, we were still on a friendly basis, but his interests were in these more esoteric problems which I'm not sure had easy solutions. He got

himself involved in theories of carcinogenesis and free radicals, etcetera. It was before, you know, this whole genetic outlook took over.

AK: Let me ask you a broad question. It might be too broad but you can tell me. If one stands back and looks at the large picture of Albert's very long career as a scientist in Europe and in the United States, one might think of two general kinds of contributions that he made. One might be what he discovered and the other might be the way that he discovered it. Or to put it another way, one might be the substance of his work and the other was his style, his style of working. Which of these two do you think had the greatest influence on you?

JG: Well I certainly was influenced by both, I must say. Certainly his work on Vitamin C and the tricarboxylic acid cycle and the early work on muscle and myosin; plus, this really unbounded enthusiasm and devotion to science, that kind of thing personally I felt was very important. But certainly I wouldn't have become interested in muscle had it not been for the work that I actually became aware of just at the right time, when this was quite the new thing. I mean for many years this was a very lucky thing for many of us. This was a fairly unbounded field for many years. But it did attract a lot of people over the years and eventually led to its development.

But that first year in Szeged, with actin, myosin, ATP . . . it was interesting actually, that the spark for this was the Russian, Engelhardt. It turned out that Szent-Györgyi showed that myosin, which people thought was a simple one-component substance, is really

myosin plus actin. And what brought him into this was that Englehardt and his wife, Lyubimova, published, just on the eve of WWII, a paper showing that ATP is hydrolyzed by what then was known as myosin. This, I think, put Szent-Györgyi on this track and then by inspiring some of these people who were with him in Szeged who really did extremely well. He started something which is still important today.

AK: Right, so his work at Szeged on muscles and myosin had a great influence on your choice of research interests. Did you ever participate in any social events at Woods Hole? Szent-Györgyi was known for having parties and playing volleyball, and . . .

JG: Yes, well I wasn't directly . . . that was perhaps mainly with people who spent more time there.

AK: In the lab, yes.

JG: He and his wife were really gracious hosts and I remember parties there.

AK: Did you ever see him frustrated with his work or did he always seem to be enthusiastic?

JG: Perhaps I did see more of his enthusiastic side. I don't know if he . . . frustration, maybe perhaps later, when people didn't always react positively to his work. Is that what you're thinking?

AK; Yes.

JG: I mean, certainly the muscle period . . . I remember minor challenges . . . I remember meeting in—this was in England, maybe the International Physiological Congress in '48 or '47—and Albert gave a talk and he was talking about the temperature dependence of some actin-myosin interaction. And a famous physiologist, A.V. Hill, got up and said, "Well one of your apparent findings would lead to the conclusion that frogs would not swim at zero degrees, but they swim." Well it turns out that there was some little glitch in the theory and we straightened it out. But these were all minor and he would be stimulated by anyone who had some critical comment and then come up with better evidence or, in some cases, he was ready to give up theories if they were not supported by later work. I mean, the whole myosin field in the early days was based on a lot of assumptions which later had to be modified.

But I remember another example. He had a model for muscle contraction which somehow was like a ruler but it was pliable enough that you could bend it to a circle. And his model implied that you would have something that has two conformational states. One would be this bent circular form and then the other would be a straight form. And to illustrate this he would bend this model, which was made up by some rubber and metal, and he bent it and he threw it up in the air and then caught it and then it was straight, and he said, "That's how muscle works." It was very amusing to the audience. He lectured to

people who didn't always understand the minutiae, the details, but he tried to use all means, including various bodily movements and such, to convey his ideas.

AK: Yes. Well, Dr. Gergely, those are the areas I wanted to ask you about. But let me just finish by also asking, is there anything that we haven't talked about or anything that you would like to say about Albert Szent-Györgyi before we finish?

JG: Well, I certainly can say that he influenced both my science and my personal life in the end, you know, just getting me to this country and I've always thought very highly of him and that he was inspiring. As things developed, I thought that perhaps he was using too wide a paint brush, but then again in a way that's the attribute of a genius where many of us were more plodding and ended up working on the details. But actually I think that he made many friends. Did you run into people who were negative about him?

AK: No.

JG: Well, as you say, if I can see this then maybe something will come to my mind that I've excluded.

AK: That would be wonderful.

JG: Is there anything else specifically that you would like to ask?

AK: Not right now, I think I covered everything that I wanted to cover. It's been very helpful and thank you very much.

JG: I enjoyed it, going over some of these things, old memories.

AK: And we will send a typed transcript to you soon and you can review it. Now I understand you're having some eye surgery on Thursday, so good luck.

JG: Thank you very much, I've enjoyed talking to you.

AK: Thank you. Goodbye.

JG: Goodbye.

[End of Interview]

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