CHEMICAL HERITAGE FOUNDATION

ROBERT KAMEN

Life Sciences Foundation

Transcript of an Interview
Conducted by

Mark Jones

Boston, Massachusetts

on

10 November 2014

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Center for Oral History
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ABSTRACT

Robert Kamen was born in Brooklyn, New York, but raised in Queens. His father was a stockbroker, and his mother was an author and homemaker. Kamen was interested in science from an early age, working in a lab cleaning pipettes at the age of twelve. He attended Amherst College in the early sixties where he majored in biophysics. After a summer internship working in the laboratory of Harriet Ephrussi-Taylor, Kamen attended Harvard University for his PhD, working on purifying phage enzymes in James Watson and Walter Gilbert's lab. After receiving his PhD, Kamen worked with Charles Weissmann on Q beta replicase before moving on to Imperial Cancer Research Lab in 1971, where he was the head of the transcription lab. Kamen then joined Genetics Institute [GI]. At GI, he managed a lab and started project management groups for new research, such as into factor VIII. By the late eighties, Kamen had become burned out by his work at GI and was let go, after which he worked briefly as a visiting scientist at Whitehead Institute. After being contacted by Robert Weinberg, Kamen began working as the president of BASF Bioresearch's US division, as BASF Bioresearch was planning on building a new facility in Worcester, Massachusetts. While working for BASF Bioresearch, Kamen oversaw the development of Humira. Kamen was later tasked with selling the company's pharmaceutical division, which went to Abbott Laboratories. While Kamen stayed on with the company, working for Abbott Laboratories required him to keep in constant contact and often travel to the company's offices in Chicago, Illinois. Growing tired of constant travel, Kamen helped the company look for a successor for him, after which he retired.

INTERVIEWER

Mark Jones holds a PhD in history, philosophy, and social studies of science from the University of California, San Diego. He is the former director of research at the Life Sciences Foundation and executive editor of LSF Magazine. He has served in numerous academic posts and is completing the definitive account of the origins of the biotechnology industry, entitled *Translating Life*, for Harvard University Press.

ABOUT THIS TRANSCRIPT

Staff of the Life Sciences Foundation conducted this interview, which became a part of our collections upon the merger of the Chemical Heritage Foundation and the Life Sciences Foundation into the Science History Institute in 2018. The Center for Oral History at the Science History Institute edited and formatted this transcript to match our style guide, but, as noted, Science History Institute staff members did not conduct the interview.

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INTERVIEWEE: **Robert Kamen**

INTERVIEWER: **Mark Jones**

LOCATION: **Boston, Massachusetts**

DATE: **10 November 2014**

[. . .]

JONES: Maybe we could get started with a little bit of biographical background—where you're from, where'd you grow up, your family background?

KAMEN: Okay, I was born in Brooklyn. Raised in Queens in a little community called Laurelton just by the Kennedy airport or . . . as it was called Idlewild in those days. So I went to public school. Then I left New York and went to Amherst College, which was something entirely different from my upbringing.

JONES: Right. So you're coming from a middle-class, working class background?

KAMEN: Middle-class.

JONES: Yeah. And what did your parents do?

KAMEN: My father was a stockbroker. My mother was a frustrated author, home keeper.

JONES: Okay. And you must have been a good student, yeah?

KAMEN: Yeah.

JONES: And interested in the sciences from the beginning?

KAMEN: From an early age. I think my junior high school biology teacher whose name was something like Mrs. Iskowitz. She was short and pretty and very lively. [laughter] That probably got me excited about biology. But that's not . . . I worked in labs washing pipettes when I was twelve or something like that.

JONES: How did that come about?

KAMEN: Connections of my father. I don't know what the . . .

JONES: Was he working with healthcare companies or at the—

KAMEN: People or somebody he knew.

JONES: Right, well, that's interesting. So companies in Queens?

KAMEN: No.

JONES: Lab testing or . . . ?

KAMEN: No, it was in Manhattan. New York had a pretty good enrichment program for kids interested in science because I didn't go to one of the exam schools. I went to just a big public high school. Southeastern Queens is too far from Bronx Science. That's ninety minutes each way by subway. So I used to go to Columbia on weekends. They had a biology university course with some people who turned out to be fairly famous later on when they were younger. I never understood a word of what they said.

JONES: Yeah, do you remember their names? Who?

KAMEN: Yeah, it was this guy named Lewontin.

JONES: Richard Lewontin at Harvard?

KAMEN: Yeah, I think—

JONES: Who's doing . . . yeah, he's still . . .

KAMEN: Yeah, he's in population . . .

JONES: In race and—

KAMEN: Population geneticist. Yeah, he must have been a graduate student. I don't know. But he was lecturing these high school kids and covering the black board with incomprehensible stuff. But he was very exciting, so . . .

JONES: Through high school then you're thinking, "Oh, I'll go to college and study this"?

KAMEN: Yeah. I think I probably decided quite young. I don't know when. And I wanted to go to a small college. So I picked Amherst. The catalog was purple. I think I liked the color. And I didn't . . . we didn't get much advice about where to go.

JONES: From . . . at your high school?

KAMEN: Yeah.

JONES: Yeah. Turned out to be a good choice?

KAMEN: It was a good choice. Great place. And I had the grades to get in, so I went there. Amherst is a wonderful place. There's a lot of individual attention. I majored in biophysics to avoid all the premeds since I knew I wanted to be a scientist, not a doctor.

JONES: And they weren't clued into molecular biology at that point?

KAMEN: Molecular biology hardly existed. This is 1961.

JONES: Well, biophysics is sort of the . . . one of the precursors, yeah.

KAMEN: Well it wasn't really biophysics. The biology department was all premeds, and if you wanted to avoid that, you majored in biophysics and you got straight science. **<T: 05 min>** No bio . . . I don't think they knew what biophysics was, but what they meant was biology, chemistry, math, and physics. So I had to take courses in all of those, which was pretty rough.

JONES: But also pretty valuable, yeah?

KAMEN: Very valuable. Very valuable. I've forgotten almost all of it, but it became difficult to do your senior year because I'd be taking a few physics courses and the physics majors would be taking all physics courses. So it was pretty challenging.

JONES: Yeah. And you never had any notion of becoming a doctor? I think that's common to a lot of people.

KAMEN: No, I didn't want to do medicine.

JONES: Just science.

KAMEN: Yeah, just science. But the biophysics department had six full professors and three students. And so we had seminar courses in professors' homes. And Amherst is—still is—and then was a really great place where students were really encouraged to be creative and to learn a lot, so we didn't learn from textbooks at these seminars. We had to present papers. So I remember reading the first edition of the *Journal Of Molecular Biology* and I guess I had to present the Jacob & Monod paper on gene regulation.

JONES: Great. It's a good place to start.

KAMEN: But my real connection with serious science came by accident. So I was working as an assistant for a geneticist counting fruit flies. I found that was kind of boring. And in the early sixties, bacterial genetics was the buzz word. This was the hot thing to do, and that wasn't available at Amherst. They had no modern biology really at all. So I decided a NSF summer course, which I saw advertised on a bulletin board, and they were offering a studentship in bacterial genetics which I saw . . . I still remember that bulletin board. It said bacterial genetics. So I got excited. The downside was it was in Cleveland, Ohio, which wasn't really where I wanted to spend the summer. And it gave the name of the person whose lab this would be; it

meant nothing to me. I didn't know anyone in science. So it ended up I spent two summers in the lab of a woman named Harriet Ephrussi-Taylor.

JONES: Ephrussi?

KAMEN: Yeah.

JONES: Related to . . . ?

KAMEN: Boris. Boris's . . . so you know your history. Boris and Harriet had blown out of France. Boris was running modern French science and had a big dispute with the government and he got angry. He got angry often. He was a Russian. And they took this job in Cleveland when Needleman, I think, who subsequently went to Searle . . . he's the father of Celebrex. Great pharmacologist. He was in Cleveland. He was trying to get faculty. And he had a little building which he gave to Boris to, kind of, get him to come to the US. And they spent, I think three years in the US, and they went back to France—all forgiven. So I was lucky enough to work with Harriet Ephrussi. She was Avery's student. Probably Avery's best student. And so she worked on pneumococcal transformation.

JONES: She was there for those experiments?

KAMEN: Yeah . . . well . . .

JONES: Maybe, maybe not?

KAMEN: Avery's great work was in the forties, right?

JONES: Late forties, I think.

KAMEN: Yeah, she was probably there. Yeah. So she was working on pneumococcal transformation and experiments in pneumococcal transformation. And got to know . . . the second year I stopped shaking in my boots when Boris came around. He was pretty emotional.

JONES: [inaudible] character. Yeah.

KAMEN: Yeah. But very, very impressive and a brilliant guy. And I got to know the personally. We used to house sit their cats. And my connection with, sort of, the mainstream came through them. So I don't know if you know the history that . . . so Boris and Max Delbruck were best friends, and Max Delbruck was the guy who started the whole thing. When the Ephrussis lived in France in the fifties, they had a farm, and Harriet was a good cook. And she's also . . . she's a very impressive Boston Brahmin lady. Brilliant, just competent lady. Also great cook. Great French cook. And this young kid Jim Watson used to come and hang out on their farm to be fed by Harriet.

JONES: In France?

KAMEN: In France. Yeah. This is '54—something like that. Anyway, the reason why I'm telling that story is that **<T: 10 min>** my last summer there, it was between my junior and senior year in college. I had been living in the Ephrussis' house in Shaker Heights, Cleveland—had a big old house with a garden sitting their Siamese cat. And the Ephrussis came back from their vacation. They were having tea in their living room, and I asked Boris where I should go to graduate school, thinking he would know where to go to graduate school. And he said, "Go to Caltech."

JONES: That's where Delbruck was.

KAMEN: That's where Delbruck was. And I said something stupid like, "They have smog there."

JONES: Which is true.

KAMEN: Which is true. And I'll never forget he said, "Well, you'll have to go work with Jim, then. Harriet, arrange it." [laughter] And the next day I come to the lab, and I ask Harriet, "Who's Jim?" I had no idea who he was talking about. And then she said, "Jim Watson." And I said, "Isn't he dead?" [laughter] He already had the Nobel prize. He was an old guy.

JONES: Wasn't that old at that point was he? Still forties?

KAMEN: No, no, he wasn't, so she wrote me an amazing letter of recommendation. So I got into the Watson-Gilbert lab and discovered many years later that Jim had been afraid of me all

the time I was there because he would never think of not doing what Harriet instructed him to do and she wrote this really strong letter. [laughter]

JONES: I see, yeah. So she . . . very forceful pair, very forceful personalities.

KAMEN: Yeah. Charming too. But they were quite something.

JONES: Yeah. So during that period, you're in college. You're set on a career in science, an academic career . . . that's your . . . you're on [inaudible].

KAMEN: Yeah.

JONES: And so you're asking where is the best place to go? Those are the two top choices, yeah?

KAMEN: Yeah, and in the Watson-Gilbert lab in the . . . so I went there in '65, which was, kind of, the peak of the strength of that lab before Jim went to Cold Spring Harbor, and it was a great place. I think there are three . . . three of my peers there got Nobel prizes since that time.

JONES: Who was there? Gilbert was there.

KAMEN: Wally Gilbert was co-running the lab. And Mario Capecchi taught me about chemistry. He was there. I was succeeded by Bob . . . . what's his name? Horvitz. We overlapped for about six months. And it was an amazing place. Discoveries were going on. Watson and Gilbert weren't the nicest of people to train with.

JONES: Well, say a bit about that. What was you experience when you arrived? You have to, sort of, get acclimated to the place? Was it different than the labs you'd been in before?

KAMEN: Well, I arrived and had a brief discussion with Jim. Very, very brief. And he told me that I would be working with a guy named John Tooze. And then he walked away. So I had to go up and down the corridor trying to find out who was John Tooze. I don't know if you know John Tooze?

JONES: No.

KAMEN: He's . . . oh, John went on to be . . . he was the first editor of *Nature* New Biology. He's published lots of books, and he's published lots with Cold Spring Harbor. And he was a bench scientist there, but he wasn't much of a bench scientist. But a very interesting British character. In the Watson lab, you had to find your own way. Jim didn't really talk to his students very often.

JONES: How did that suit you? Was it . . . ?

KAMEN: It was fine. I was happy being independent. But then there were a lot of . . . he expected people to train their people. He just, sort of, expected it all to happen. He would provide general direction and a special kind of enthusiasm. I mean, he knew he was great and he expected anyone . . . everyone working with him would be great. And you were expected to make a discovery. Probably get a Nobel prize. That was sort of—

JONES: So did you feel you had a greatness in you at that point?

KAMEN: Oh, I don't know. I don't think so.

JONES: Yeah. But enough confidence that you could do something good to stick around a while?

KAMEN: Yeah. I think a certain degree of arrogance was encouraged. And so Jim's interaction with people was at tea . . . he is still a great anglophile. He lives in . . . he has a house in London. So we all got together at tea . . . for tea at four o'clock. That's when people talked. And then there were noon seminars, which were often abuse sessions. They **<T: 15 min>** were really rough.

JONES: So you'd present your work and everyone else would rip it apart?

KAMEN: Yeah, particularly Jim, Wally, and Mark Ptashne. Questioning was pretty direct. I got well-trained in asking pointed questions. But people survived.

JONES: Sure. What did you start working on, and how did you get into it? I mean, you're making connections in the lab.

KAMEN: Well, the lab was largely an RNA phage lab. In the days before recombinant DNA, the model system was bacteria phage, which you probably know the phage group history. This is Delbruck's physicist's approach to biology: work with something that's simple. So a lot of the work in Jim's lab was working on Arnie bacteria phage. So I started working on phage called R17 and got interested in the whole history. So this was the first in vitro replicating biologic system. You could in an in vitro system you could replicate the RNA-phage RNA and make infectious RNA, which would infect back into bacteria. And it was quite interesting to work on the enzymology of that. So my thesis project ended up being purifying for the first time the phage enzyme, which was responsible for replicating RNA.

JONES: What specifically . . . how did it work?

KAMEN: I mean, a review was just published. It just got figured out. It's complicated.

JONES: Oh okay. [laughter]

KAMEN: Jim's lab worked on a phage called R17. In order to do what I wanted to do I had to switch to a rival phage that his lab had never worked on. So that was a confrontation with Jim, but he went along with it. And then I had to set up a collaboration with one of his big competitors that he didn't get along with too well.

JONES: Who was that?

KAMEN: Saul Spiegelman. One of the—

JONES: He was in New York too? In New York.

KAMEN: Well, he was in Illinois at that time, and then he moved to New York. And he didn't get along with Watson lab too much.

JONES: So he really did . . . he didn't want you to do it. How did you persuade him?

KAMEN: No, he just . . . he didn't oppose it. It just was—

JONES: You had to make a case for it?

KAMEN: Yeah, make a case and it was fine. I end up purifying this enzyme, which turned out to be extremely interesting. It was a precedent. The enzyme had four polypeptide subunits. This is the early days of SDS gels where you could really purify a protein, look in a gel, and see what it was. It had just been . . . SDS gels were developed in Jim's lab. So people were just doing that.

JONES: Who did that?

KAMEN: Weber and Osborn. Klaus Von Weber and Mary Osborn. Postdocs. Klaus then became Harvard faculty and moved back to Germany, and he's a great in German science. So the curious thing was there were four subunits, but the viral genome wasn't big enough to encode those four subunits and it also encoded other proteins. So I figured out with the advice of a postdoc in the lab how to do an experiment which would prove that it was an intra-specific hybrid enzyme, that there was only one polypeptide chain from the virus and it was co-opting three host proteins. I showed that in an appropriate experiment and then showed that . . . took the enzyme apart, put it back together again. I showed that the host proteins—the e. coli protein—were required for the activity.

JONES: Had anything like that been discovered before?

KAMEN: No. No.

JONES: So that's a pretty big deal, yeah?

KAMEN: Yeah. So that became a *Nature* paper. And I had a major competitor working on Q beta replicase, who was the great scientist of the field, Charles Weissmannn. And Charles was, sort of, a friend of the Watson-Gilbert lab because he was in a big war with Spiegelman about how replicase worked mechanistically. And the only people that really supported him were Jim and Wally. They recognized that he was brilliant and infallible. So he came to visit. But he disagreed about the structure of the enzyme.

JONES: Your enzyme?

KAMEN: Yeah, well, he'd also purified it about the same time. He wasn't convinced . . . he thought these other polypeptide chains were contaminants. He came to visit one weekend, and I think it was Wally Gilbert and I worked on Charles in the corridor at a blackboard, and I won't go through the details. He had a rational reason why **<T: 20 min>** he thought they were contaminants. We convinced him that I was right. And then at the end of that, he mentioned that he had a paper that had been accepted in *Nature* and maybe he'd change it a bit. So we ended up publishing back to back. And he changed the story to agree. But the big advantage of working for Jim . . . I don't know if you know this, but Jim never put his name on a paper of anyone coming out of his lab. I think there's one Cold Spring Harbor symposium big review with everybody's names—Jim's name is there. Otherwise, all this great work was being done, and he never put his name on a paper.

JONES: He didn't need it, right?

KAMEN: Well, he had this special kind of beneficent arrogance I would say. In his point of view, anyone who he cared about knew who his students were anyway. So but I think as I learned later, he copied this from Francis . . . this is MRC Cambridge. Is it Francis Crick would never have put his name on a student's paper, and it wasn't the custom at MRC Cambridge to . . . for big professors . . . professors that did experiments with students were talked to now and then, but you didn't sign their papers. So that was a big advantage and also Jim didn't like speaking in meetings, which he doesn't do very well . . . or at least at that time, he did very badly. He mumbled, and he was a bit too shy, I think, to speak publicly, so he would sort of appear in your lab at eleven o'clock at night. The lab ran until four in the morning. People really worked hard. He used to walk around eleven to midnight, stick his head in, rarely spoke. Just stare at you. It was very disconcerting. And what he really wanted to hear eureka! He wanted to hear that you did something exciting. And if you had that conversation . . . this is vague memories now, he'd come and plunk himself down on a lab stool and go like that and stare at you. He wanted to hear the story. And if it was something really, kind of, interesting he'd say something like, "I've been invited to give a talk at a meeting next month. I don't want to do that. Why don't you do it?" So the next day you'd talk to the secretary, "What's this meeting? What's it of?" It's a plenary lecture. It's a symposium, and you're a second-year graduate, third-year graduate student. And it was a way of tightening the screws because he figured you'd really work hard, get the story finished. It was good training.

JONES: Yeah. Yeah. This is all going well. Did you have any sort of obstacles or setbacks along the way or things not working out?

KAMEN: Oh yeah. Work didn't always go . . . it was up and down. The Q beta replicase stuff, that went well. But before that I was, kind of, in limbo, lost a couple times, Wally was . . . I don't know if you know these characters, if you've met—

JONES: Jim Watson, Wally Gilbert. I haven't met Mark Ptashne.

KAMEN: Yeah, Wally was the shoulder I used to cry on if you can believe that. [laughter] Literally, no, Jim was a tough guy. He fired me once.

JONES: Did he? What did you do?

KAMEN: Didn't smile enough. I was probably grim. Probably too serious, too grim.

JONES: It was a serious place.

KAMEN: I don't know. But he forgave me.

JONES: Yeah. And you're observing what other people are doing there and they're doing great things, right? What was Gilbert doing at that time? This is . . .

KAMEN: Gilbert was purifying the lac repressor with Benno Müller-Hill, which is the work he really got the Nobel prize for. [inaudible] I think an excuse to hang it on. But one amusing story . . . that was sort of the great days of molecular biology in which there was . . . at least Jim had unlimited money. He had no problem with grants. So the graduate students stayed four years and then five years because of the war. Jim would never let somebody leave and get drafted. So he found money to keep people until they were over twenty-six. So you got to do your . . . at that point they wouldn't draft . . . they weren't drafting people past twenty-six, so everybody in Jim's lab stayed on. He didn't want to send anybody to fight the war.

JONES: He was opposed in principle?

KAMEN: Yeah, strongly. So the last year the graduate students had technicians if you can believe that. And you were allowed to hire your own technician, so I hired this guy named Allan Maxam, who had dropped out of the PhD program at Brandeis for some emotional crisis. I

never could pin it down. But he was a bit of a special kind of character. So he . . . his hands got me through my PhD work.

JONES: He's good. Good experimentalist.

KAMEN: Yeah. Really good. Fantastic. And super organized. **<T: 25 min>** Great guy. And when I left, Allan went to work for Wally. And Allan is the guy who figured out how to sequence DNA. So all these wonderful detail protocols that went around the world, and Wally had the idea, but . . . and Wally was a great experimentalist with his hands. But he wasn't working in the lab enough to do something like that. But Allan really made a huge contribution by working that out. So my contribution was bringing Allan into the lab.

JONES: And Ptashne at the same time was working . . . he was working on—

KAMEN: He was working on lac repressor competing with Wally. They were fierce rivals and good friends.

JONES: That must have been an interesting mix.

KAMEN: Yeah, it was very interesting.

JONES: Was it . . . but this was a good introduction to science, yeah? The top, elite . . .

KAMEN: Yeah, it was a very exciting place. There were some . . . Joan Steitz was there. Great person.

JONES: And she could mix it up with the rest?

KAMEN: Oh yes. Well, Jim was always partial to women.

JONES: Made it a little easier for her to . . .

KAMEN: She was one of the few people in the lab that seemed to be a close friend of his. They were . . . they got along very well. They communicated well. Jim tended to triangulate. He didn't talk to student A about what student A was doing, but he knew even if he never told them. He knew exactly what you were doing because Jim has complete photographic . . . I don't know if it's photographic, but he doesn't forget anything. Or at least at that time he didn't. He knew exactly what was going on. But he didn't like . . . you didn't have a lab meeting with him and tell him what you were doing. He figured it out detail by detail.

JONES: So you're . . . you work on your dissertation. You're moving that forward. And what was your plan for . . . well, I know where you end up.

KAMEN: So the custom in that lab, you did well, you left Jim's lab and then you went to Cambridge. That's what [inaudible] did. And I was always a bit of an iconoclast, and I didn't really want to follow what everyone else did. There was another student, Falk or Focht, who . . . it was a German name, but he was an American of German heritage. And he decided he was going to go to Switzerland, which in the end he didn't end up doing. I can't remember why.

JONES: He was going to go to Weissmann's place?

KAMEN: I don't remember where he was going to go. But he was going to go somewhere either Germany or Switzerland I remember. So I had gotten to know Charles over this competition and asked Charles if I could go to his lab to continue working on Q beta and he thought that was a great idea. So he invited me to come combine forces and work with him.

JONES: Was his lab the way he ran it much different than Watson's?

KAMEN: Oh, very different. All the key lab reagents were kept in a locked freezer, and only Charles had a key. And you have to ask Charles for small aliquots for experiments. And he also counted the lab pipettes, the micropipettes. You had to keep your set together and turn in the broken one to get a new one.

JONES: Was this because of budget constraints there that Watson didn't have?

KAMEN: No, no. Very different personalities.

JONES: Just the . . . yeah.

KAMEN: Charles was on top of the detail of every experiment. Also a super strong personality. But I got along with Charles great. I loved him.

JONES: And what was . . . in particular, he brought you in to . . . what was the plan? You were going to . . . ?

KAMEN: Well, I don't think my postdoc was strictly successful with him. I continued to do some work on Q beta replicase. But it wasn't so good. But I learned a lot from the lab. And it was helpful for me because he was in the transition from . . . that was in the beginning of recombinant DNA. So he was exiting the work on the RNA phage. He'd gotten into retroviruses. So half the lab worked on RNA retroviruses. And he had a very rigorous journal club that met twice a week where we actually read the whole scientific literature he assigned people, and I learned a huge amount about all sorts of . . . about animal viruses. Those sort of things I knew nothing about.

JONES: Yeah. So you mentioned recombinant DNA. [inaudible] Bob Burke's.

KAMEN: Yeah, yeah, that was all going on, <**T: 30 min>** and my successor in the lab—we overlapped by about six months—was Richard Flavell. We shared an apartment. We've been friends ever since. Richard was involved in the early interferon cloning stuff. Probably about . . . when was it? I left in about '71. Well, some years later they cloned the first interferon messenger . . . must have been '77—

JONES: Seventy-eight, '79.

KAMEN: Seventy-nine. It was a while later. If you want to continue with the biography, when I finished up in Charles's lab, I decided that I wanted to work on animal viruses. I was following the Delbruckian accident. We'd, sort of, done RNA phage. All the questions left I thought were derivative. That turned out not to be true.

JONES: A lot of people were going in this direction.

KAMEN: Yeah, I went to Cold Spring Harbor, took the Cold Spring Harbor animal virus course, got really interested in animal viruses.

JONES: Who taught that course?

KAMEN: Aaron Shatkin and Bill Katz with a little help from this guy named Art Levinson who was a Katz's PhD student, I think. He was, kind of, like the TA of the course.

JONES: Very small world.

KAMEN: My image of him is this guy in a white t-shirt and sneakers without socks and jeans running around like a rabbit all the time, always intense and energized. So I wanted to do a second postdoc in animal virus work. But at that time the animal virus labs were large labs run by rather controlling individuals. The field was just starting. There were a couple of big labs at NCI; there was a big lab in St. Louis. It wasn't . . . there weren't tons of them. And I was at a point in my career when I wanted to go and work for somebody. I wanted a lab that had facilities, and I could do my thing.

JONES: Did you have a plan at that point? You wanted to do something and get somewhere . . . you wanted to get back to the States and . . . ?

KAMEN: No, I wanted to work on gene expression and DNA tumor viruses.

JONES: And do what you wanted to do rather than what somebody else wanted you to do?

KAMEN: Yeah, I didn't know more than that. Not even sure I really wanted to work on DNA tumor viruses. I wanted to work on tumor viruses and wanted to figure out how genes worked. Fortunately, I remember this wonderful man—Lionel Crawford—was another Jim friend who used to come and hang around the lab in Cambridge in summers, I think. I don't know. A really nice man. And very smart, but not known for being overbearing. So I wrote Lionel . . . and he was at Imperial Cancer Research Labs in London. So I wrote Lionel a letter and asked if I could come and join him. And he wrote back right away saying, "Sure, come. We've got space for you." And that was just wonderful. So during the seventies, from '71 till '82, I was at ICRF on the famous fifth floor of ICRF, which was an unbelievable thing that Lionel had built—and a few other people. Most of the people who until a few years ago ran molecular oncology in the US were there. And there was a close connection with Cold Spring Harbor. So Phil Sharp was coming over here, Joe [Joseph] Sambrook was coming over. I shared a lab with Frank McCormick, Tony Pawson. And oh, they discovered P53 while we were there. Bruce Ponder was there, or he was Lionel's student. It was an amazing thing.

JONES: And what was your status there for those?

KAMEN: Initially, I was . . . I came as a post-doctorate fellow, but Lionel just provided facilities. He just didn't . . . this was pretty much the British amateur tradition. He had a technician student, and he worked at the bench and did his work. And he provided core facilities. Very good core facilities. It was a wonderfully set up lab. The whole institute was beautifully set up. And they paid for everything. There were no grants. He didn't apply for everything.

JONES: This is heaven, yeah?

KAMEN: Yeah, it's like working at the NIH but on a smaller scale. There was a storeroom; you went and got supplies from the storeroom, signed your name. I was there for a couple years as a postdoc. Then I came back to the States, did my job-hunting tour. Jack Strominger hired me to be one of the first employees **<T: 35 min>** at Dana Farber when Dana Farber was expanding and going into basic science. That was '76, I think. I would have shared a lab module with Bill Haseltine, who was . . . I also shared a lab with him in Jim's lab. Bill was Wally's student, so we knew each other. And that would have been a disaster sharing with Bill.

JONES: Why? Why do you say that? He's another strong personality?

KAMEN: A little bit. A little bit. [laughter] I don't know if you know Haseltine?

JONES: Yeah, I met him.

KAMEN: Yeah. Yep, yep. Character. He's a friend. I like him. But he's a character. If you want to get real . . . he comes back into my history much later. So I got this job at Dana Farber. I wrote a grant with David Livingston and became very close friends, and the grant was approved—fully funded. And then I had, kind of, my three-month before exit interview with Michael Stoker who was the wonderful man that had revolutionized ICRF. I told Michael I was leaving. And he said, in a polite way, "You don't really want to do that. Why don't you stay? We'll give you tenure. Just hang out here." He didn't say hang out here. He just said, "Just stay." And I thought that sounded pretty wonderful. Why should I go through the whole US system when I could stay there and not apply for grants?

JONES: And did you like living in London?

KAMEN: Yeah, I loved living in London.

JONES: Yeah. It was a little more affordable?

KAMEN: So I just basically carried on. My title changed. I was no longer a postdoc. I was the head of a lab. I've got a name. It was the transcription lab. And I could take graduate students and postdocs.

JONES: And how did the position work? I mean, you're subject to review on . . . ?

KAMEN: Quinquennial reviews they had. Every five years you got reviewed by three outside folks.

JONES: Yeah. So you're looking at it saying, "This is fine. I'll do this for . . ."?

KAMEN: Yeah, so I was . . . and so I did that for six years—something like that. I was getting a bit frustrated because the limit . . . something has to be limiting. And what was limiting there was space. I had two labs, say two . . . lab A is about the size of this room. I had two of these. That was the space. Plus, a shared instrument room. I had thirteen people. It was pretty crowded. So it was a bit frustrating. You really couldn't grow the lab. I felt, incorrectly, that we couldn't compete with the big American labs. But as I said, the people from there mostly moved back to the States and became big American labs. That was just a misperception. But mostly, the problem that, sort of, got me was lab turnover. I was still working at the bench. I loved doing experiments. I was very close to my students. Then they'd leave. And new students would come in, and they were younger. They seemed to be younger. So I was on my third turnover and thinking I just didn't like that. I didn't like that sort of thing. And I didn't like writing papers. I liked doing science, but I didn't like publishing and things like that. But those are minor things. But the biotech industry was starting.

JONES: Yeah, were you . . . yeah, I guess, began '76. So while you're there, are you paying any attention to it?

KAMEN: Oh yeah. I came back to the States occasionally. I remember a lunch I had with Wally where he'd told me that he and Charles were going to start a company.

JONES: What did he say?

KAMEN: Well, he thought it would be a good way to provide jobs for postdocs.

JONES: That was the principle?

KAMEN: That's what I remember. I don't know what he said, but . . .

JONES: Well, there were . . . I mean, he did have some problems at Harvard, right? With . . . ?

KAMEN: Wally? No. I don't think so. He had problems getting tenure.

JONES: Well, I mean, he had to . . . well, he wanted to be an officer in the company, and they wouldn't let him.

KAMEN: Oh, oh. After that. Yeah. No, I was somewhat on the outside. I knew what Charles was up to, so it was, kind of, interesting. It seemed exciting.

JONES: What did you think about the business angle of it where people are investing money and you have to give them returns on that? Did that ever enter into the . . . ?

KAMEN: No, I don't think so.

JONES: Not at that point, yeah?

KAMEN: No, it was just sort of exciting new thing of using what we knew to make drugs and do something practical. It wasn't just focused on making drugs. There were all these biomass projects. All these crazy things. It was applying recombinant DNA, and I found that **<T: 40 min>** somewhat attractive. It wasn't that I really went out to do that. It happened like most things that happen in life—accidentally. So Wally and Charles got going with Biogen. And I don't know if you know the history of Biogen. They decided to make it an offshore company in the Cayman Islands—or I forgot where it was—and the lab would be in Geneva because they couldn't build a lab in the US then. It was still on all the recombinant DNA uproar. So it was to

be in Switzerland. But Charles being what Charles was like, Wally couldn't agree to the lab being in Zurich. So they put it in Geneva so Charles couldn't be there all the time, which was a stupid idea. It was very hard to run that lab. And after some period of time—maybe it was two years or less—they decided they really had to hire someone to run Biogen—a head of research. I was, sort of, in the circle of people they were looking at.

JONES: Who else? Julian Davies?

KAMEN: Yes, Julian Davies . . .

JONES: Richard Flavell?

KAMEN: So they ended up . . . so they offered the job to Richard, and he turned it down and went off to the Canaries on vacation. And then Phil said he would take it.

JONES: Phil Sharp?

KAMEN: Yeah, so Phil Sharp was going to leave MIT to take that job. And then Richard changed his mind and took the job. I forgot the thing with Davies . . . maybe they split it at some point . . .

JONES: Well, they opened a place in Cambridge here, so maybe Richard came here?

KAMEN: Yeah, that's right. You're right. So Richard and I . . . by this time, Richard had left Weissmann's lab, moved to Holland, and if you look through the genealogy of Dutch microbiology, he basically started . . . well, he implemented the starting of molecular biology in Holland. There's a guy named Pete Boris [who] was Charles's postdoc in New York who went back to Amsterdam and so Flavell was Pete Boris's graduate student who was sent to Charles to learn real molecular biology. Then went back to Pete's lab and Richard ran the lab and trained a whole generation of molecular biologists who are now in their sixties. Then he moved to London. So the last few years I was in London, Richard had a big lab in Mill Hill. And I was in downtown London. And we had a lot of collaborations. People going back and forth. Worked together a lot so [inaudible] socially all the time. So when he got the job at Biogen, he asked me to come with him. So that was actually how I got—

JONES: To go to Geneva?

KAMEN: No, to go . . . no, this was—

JONES: Oh, to come here.

KAMEN: This was in . . . so this was already must have been some years after the—

JONES: So yeah, ‘82.

KAMEN: This is ‘82. Yeah.

JONES: Okay. And what did you think about that offer?

KAMEN: So I thought it was, kind of, interesting. I didn’t say no immediately. So I went to look at Biogen and spent a day there talking to people. And I’ll say I wasn’t impressed.

JONES: It was disorganized? They didn’t know what they were . . . ? My understanding was—

KAMEN: You have to be very careful. There were a lot of Wally postdocs there. They were all . . . all the people I talked to were very bright. But they mostly made me uncomfortable. It was almost like a personality or an emotional thing. There was something . . . you could see these people weren’t going to make it independently in academia. It was like he, sort of, parked people. That was my impression. I could be wrong. It made them based on . . . I won’t mention names. So I didn’t . . . and also the idea of going to work for your best friend didn’t seem like the wisest thing to do although I’m sure it would have worked out. Richard is a wonderful guy. It would have been fine to work there.

JONES: And did they . . . were they talking about specific projects that they had in mind?

KAMEN: No, I didn’t get that far. I don’t know. This is some time ago. I don’t recall all the details. But what had happened at about that time, I’d done some of the nicest work I’d ever done in my career in collaboration with a guy in Nice named François Cuzin. And together we discovered the idea of oncogene cooperation of the transforming gene and immortalizing gene, and we published a paper. **<T: 45 min>** This was all in a DNA virus system. And I went to give

a talk at a meeting in Houston—the first presentation of this concept of oncogene incorporation. And Bob Weinberg and David Baltimore were sitting in the front row, and this became Weinberg's big thing afterwards. And Tom Maniatis was there. So Tom invited me to lunch.

JONES: Had you met him before?

KAMEN: Of course.

JONES: When he was at Harvard.

KAMEN: He was at Harvard when I was there.

JONES: Harvard, yeah.

KAMEN: Yeah, and under Tom—

JONES: Oh, he was in . . .

KAMEN: He was . . .

JONES: I forget where he was going.

KAMEN: He was at Caltech and then he went . . . I can't remember.

JONES: I think he made like a brief stop in Cambridge, but I think that was after you were gone. That doesn't matter. We can figure that out.

KAMEN: I knew him from my time in Cambridge, and I had sent him my best graduate student as a postdoc. A guy named Richard Treisman who has been the head of ICRF for years now—brilliant guy. So I guess Richard must have told Tom that I was looking at Biogen; word gets around. So he knew that . . . and Tom and Flavell were good friends. Somehow he knew I'd been poking my nose around Biogen.

JONES: And did you know that he was working with Ptashne?

KAMEN: So at lunch he told me that he and Mark had founded this company and it would be silly for me to go to Biogen. Why didn't I come and be head of research at . . .

JONES: And why would it be silly to go to Biogen but not to go to GI [Genetics Institute]?

KAMEN: I don't remember the conversation. The pitch was don't go there. Come to us. So that's when I got involved and then I went to . . . met Gabe and was hired shortly after that.

JONES: Yeah, well what kind of offer was it? What kind of work did they want you to do? Did they have plans? What was the salary? Did it all seem like a . . . ?

KAMEN: Well, it seemed like a lot of money.

JONES: It was better than standard?

KAMEN: Well, we didn't get paid much of anything in London.

JONES: Did that have any . . . play any part in your decision the fact that—

KAMEN: Sure, yeah. Having some money. I'd never had any money since I was a . . . well, graduate student.

JONES: Right. The idea was you could still do some good science there?

KAMEN: Well, the deal was, so I was . . . what was I called? Director of research? Something like that. I was the first technical management. They had twelve, thirteen recent postdocs, and Tom and Mark were trying to run it, and Gabe had been brought in trying to run the business but didn't really understand what was going on.

JONES: And Tom and Mark, they were still in their academic positions full-time so . . .

KAMEN: Yeah, and they didn't . . . well, at least from the time I got there, they didn't spend very much time with the company. They were available. But they weren't really—

JONES: Yeah, the one day a week or whatever it was.? Or not even that? They weren't there.

KAMEN: Meetings and things. They were helpful. But they weren't really . . . also, the company wasn't . . . the disconnect—they knew some methods but knew nothing about the subject matter the company was working on—not that anyone else did. But it wasn't that . . . it wasn't like today where a company would launch working on what a founder was working on. They weren't working on . . . they weren't expert in what the company was doing, except from the technological point of view, some of the methods they had developed.

JONES: Did the fact that you're walking out of academia into industry, did that mean anything, or did you think, "Oh, what if I go back, can I go back?" You were very well-connected, yeah?

KAMEN: That's a good question. Well, part of the deal that you were allowed to have a hobby lab. They didn't call it that. But to recruit somebody in, sort of, early, mid-career, you were allowed to bring some people and have a basic research lab. So I brought three or four people over who mostly got absorbed into the company. One of which I married, which was the best thing in my life. My wife was a postdoc who joined my lab in London just a few months before I started talking about the GI thing. And she decided to come and finish her postdoc at GI. She left Wyeth twenty years later.

JONES: So she . . . yeah, made a career.

KAMEN: Now she works for AbbVie. So she made a very good career.

JONES: Oh, so she followed you to . . . ?

KAMEN: No, she left . . . she joined . . . we'll come to that.

JONES: Okay, all right. Well, the job basically at that point, it's managing a lab. So you didn't think this is a **<T: 50 min>** different kind of managing—

KAMEN: Well, no, it was different. Because they were . . . they had, sort of, this funny space in the back half of the old Boston Lying-in Hospital with the entrance at the back by the dumpster. And there were . . . there was some lab space, and there were twelve or thirteen young PhD scientists beavering away. And, you know, the vast majority were very good. But there was no organization. I mean, no one ever figured out how to organize a biotech company. And there were postdocs that weren't used to reporting to any . . . the whole idea of having a boss, I didn't understand having a boss. Gabe was this guy who hired me. Nice guy. I talked to him about—

JONES: Well, how was that working with Gabe? I mean, he's . . . it's new for him, right? What do I do with these people? These aren't like anybody I've worked for.

KAMEN: That wasn't always the most comfortable relationship. Worked out pretty badly at the end, but we were all trying to figure it out. So I'd never . . . it was a big organization for me. It was a challenge to figure out how to get those people to accept some sort of supervision; some of them wouldn't meet with me one on one. I'd have to invite them out to dinner.

JONES: They would just refuse to do it?

KAMEN: They made it clear they weren't comfortable with it somehow. I'm recalling a great animosity. It was more like . . .

JONES: It's like no, we're going to keep this academic.

KAMEN: More like . . . no, not academic. They just weren't used to having management. Scientists weren't managed. They were all from MIT and Harvard and they didn't . . . the thought of a boss, they didn't . . . and they were all a bunch of interesting personalities. Some of them eventually accepted some kind of organization. Others did their own thing.

JONES: Who was there? Jay Toole was there.

KAMEN: Jay was there. Jay I had to take out to dinner. He didn't like the idea of supervision.

JONES: But did he adapt to it?

KAMEN: We got along.

JONES: Marginally?

KAMEN: Yeah. And working for Jay was John Knopf. He was quite a handful. And John Wozney. I don't know if you know John Wozney.

JONES: I never met him.

KAMEN: He's retired now. He was the . . . became the great BMP guy. Ed Fritsch. Glenn Larsen. Randy Kaufman. Chuck Schumacher.

JONES: Steve Clark?

KAMEN: Steve Clark, sure. Sorry. Steve was the first one I met. Yeah, Steve.

JONES: Did that make a difference to you? You had to meet the people before you decide, "Well . . ."

KAMEN: Yeah, I guess I probably spent—before I took the job—I spent some time talking to people, and my impression was this was like night and day from Biogen.

JONES: Just in terms of the quality . . . ?

KAMEN: The quality, the commitment, the . . . . I thought that Mark and Tom had hired very well. No, it was an exciting place. Great people doing interesting things. But I knew nothing about the topics. The project—

JONES: Well, where did the topics come from?

KAMEN: I'm not sure I know.

JONES: Which were the first . . . ?

KAMEN: Well, some things . . . factoring project was probably Gabe. It came from Hyatt. And it was the hot topic in the biotech industry at the time. A bit obvious. They were working on tissue plasminogen activator. And that was—

JONES: Were you doing any . . . something with interferon like everybody else?

KAMEN: So, the initial idea was to work on interferon gamma. And they brought in. . . I guess Ptashne brought in David Golde as a senior consultant, who was this lymphokine guy—expert from UCLA. He was important because he had these human leukemic cell lines, and he had a cell line that was thought to make gamma interferon; this was very important at that time. Steve's first contribution to GI was to spend a few weeks in Golde's lab and prove that these cells didn't make gamma interferon.

JONES: What were they making?

KAMEN: Well, Golde wasn't . . . he said, "Well, they make GM-CSF. Let's work on that." [laughter]

JONES: So did you start on that? Pick that up?

KAMEN: Yeah. But I had never heard the term lymphokine when I took the job. I didn't know anything about that. And . . . ?

JONES: And EPO was in there somewhere?

KAMEN: And there were . . . EPO was in the mix and then there was some other stuff that didn't get continued. Then there was an agricultural group that was starting up. **<T: 55 min>** There was an applied enzymology group.

JONES: So you were going to make industrial enzymes . . . is that the . . . ?

KAMEN: They did make industrial enzymes. Really good guy, brilliant guy. David Rozzell. He's still . . . he's a major person in that kind of work. But the value is very small. What we learned is if you have . . . if you invent a much better way in making commodity . . .

JONES: It's still a commodity.

KAMEN: It's still a commodity, and if you save them fifteen cents for a ton, they'll give you two, you know? Something like that. It really wasn't any business there. Although the quality—the intellectual quality of the work—Rozzell was a guy who engineered cell-free enzyme pathways. You take this enzyme, turn it around, couple it with that enzyme, turn it backwards, and end up with a bioreactor that made a kilo of valine in a day or something like that. It was always very impressive. But we didn't keep that.

JONES: Yeah, so at some point, you cut it loose?

KAMEN: Yeah. But it was a steep learning curve. We had to figure out what it was to have a biotech company and what the right subjects were and how to manage the projects. The whole idea—project management—didn't know anything about that. So we had the benefit of having partnerships. In fact, Ray was partnered with Baxter, which had its pluses and minuses. And they also had some growing to do. But after a while, they got . . . at least they got organized, and they had this very charming guy—I've forgotten his name now—who was a project manager. and he seemed to be incredibly helpful and really things worked better when he joined, so I took him out to lunch and asked him what he did. And he explained that he was a project manager, and he did this. I said, "Let's get back." I told Gabe, "This is a great idea; we should have a project management group." But that's how we were doing it. We were just picking out . . .

JONES: Yeah. And what were the principles of project management that you tried to apply?

KAMEN: Well, just the idea that you wouldn't . . . to divide the organization of the project, the timelines and the budget and the meeting schedule and the reports from doing the science. Just because you have a brilliant scientist to encumber him with . . . these projects were getting big. You know, lots of moving parts. That's what's exciting. You have fifteen, twenty, thirty people on—well, I guess we never got that big—say fifteen people on a project all doing different stuff. You got to keep that organized. You got to check on those people. So we, I guess, innovated at least at that time. I hired a project manager, and he, kind of, figured out what to do, and he worked in parallel with the senior scientific leadership doing the administrative organizational part of the project to get the projects moved forward. The first guy I hired was David Stone. I don't know if you know him. He became an investment analyst—quite successful—for Calvin and Company for a long time. Now he's a venture capitalist.

JONES: So how did these projects proceed? Factor VIII, that was . . .

KAMEN: Factor VIII went—

JONES: Was a successful one?

KAMEN: Factor VIII was successful. It was tough, and it was successful to a certain extent. It was hard to make a lot of factor VIII. But it was one of the few battles with Genentech that we managed to clearly win. And we've helped a lot of hemophiliacs. I think the GI recombinant factor VIII was the first approved and still in a lot of . . . I still occasionally meet a hemophiliac who has been taking it and that's a good experience. EPO as you know was, a little bit too late—two weeks.

JONES: Well, there . . . do you think it was the timing? Is that how—

KAMEN: For the patent? Yeah. That was a bit of a strategic error.

JONES: The filing?

KAMEN: No, no. The person who deserves all the credit for the recombinant EPO is a very bizarre Japanese urologist, I think, named . . .

JONES: Yeah, I know who you're talking about.

KAMEN: What's his . . . blanking on his name now. Miyaki. Miyaki. Because he purified EPO at University of Chicago for Goldwasser, Eugene Goldwasser. But Goldwasser had nothing to do with it. This guy was a senior clinical guy who'd collected urine from aplastic anemic patients and dried it down and brought it to Goldwasser's lab to purify EPO. He didn't know any **<T: 60 min>** biochemistry and not the foggiest idea how to purify something. But he managed it, and he gave a sample of his EPO to Goldwasser who gave it to Lee Hood who had it sequenced in his lab. Then put the information into Abgent—the sequence. So GI hired . . . oh, Rod Hewick is the one person I left. Wonderful man.

JONES: And you purchased . . . did you purchase a sequencer? What sequencer—the ABI?

KAMEN: Yeah, so Rod was involved in . . . was the postdoc in his lab, who with Hunkapiller, invented the gas phase sequenator. And Hunkapiller went into ABI, and Rod came to GI. Really good move that Gabe had. It was very competitive. Abgent wanted him. And he brought with him a little bit of EPO sequence. So the two companies started out with the same amount of information, but Abgent had more EPO and got more sequence.

JONES: You had a couple of errors in the sequence? Did Abgent have the same errors?

KAMEN: No, I think it was an intron. It wasn't enough information anyway. So Ed Fritsch was working diligently trying to do genomic cloning with synthetic oligoes based on Rod Hewick's sequence, and he wasn't really getting anywhere and we'd had some contact with . . . Miyaki had since left Chicago. Probably going back to Japan and then took a job in some place in Columbus, Ohio. Kind of a strange institute. Charity-funded institute. And he came to visit us and gave a seminar, which I found most underwhelming. I found out later the guy was rarely sober, which . . . it was . . . the guy was bizarre. And the quality of the work, it would have been luck to get something out of it. And that was early on when I was at GI so I poopoo-ed it and sometime later I'm seeing that Fritch is, kind of, bogged down and I'm seeing, whoa, we really need some more EPO and realize I made a mistake that we should really suck up to Miyaki and see if we could buy some more EPO, and there had been some discussions, I think. I had been out to visit him in the meantime at this strange place he was at and that hadn't led to anything. He was hard to communicate with. He didn't speak much English.

I don't recall the details anymore, but it was just kind of a bizarre interaction. I still remember very clearly. I had a meeting in my office, which was a patient recovery room in this old hospital, with Ed, and . . . Ed Fritsch . . . and suddenly realized that he was stuck. It just came to me that this was not going to work. And in the meanwhile, the business part of GI had hired this guy, Manchu Yang, who was Taiwanese, I think. He was Asian anyway. And he had, sort of, an Asian way of dealing with things. And I picked up my phone, and I called Manchu, and I said, "Can you come in here?" And he came in. And I said, "We need some more EPO. Would you please get on a plane and go talk to Miyaki and figure out how to get some EPO?" I just had this idea that Manchu could probably deal with him. You know, Taiwanese, Japanese.

Well, but he had . . . Manchu had a way with him that he would . . . I thought that he could communicate. Just a guess. So he went there and some days later he called from the airport and said, "Miyaki wants three hundred thousand dollars. Can I pay him?" I forgot – whether it was two hundred thousand or three hundred thousand. Gabe agreed, and we sent the money or agreed to pay the money and Manchu came back with a little vial of EPO and Rod sequenced it, got more sequence, and Ed cloned it in less than a month from that phone call. So, as I said, we should have first time we had this bizarre man in, we should have realized that he was special.

JONES: Okay, so when you cloned it, then two weeks earlier Abgent.

KAMEN: Abgent had cloned it earlier, yeah.

JONES: But there was a patent dispute. You had . . . it was the sequence? Did you patent the sequence?

KAMEN: Yeah, so Hewick filed a patent on pure EPO because the Miyaki EPO was the gold standard and the sample we got from Miyaki was Miyaki's pure EPO. Rod put it on an HPLC and discovered it was half-pure. So he took the peak that was pure EPO and filed a patent on pure EPO and **<T: 65 min>** in parallel just before that, Abgent filed a patent on the CGA sequence. But realistically both companies had applied state-of-the-art technology. They hadn't invented anything. The real invention was this heroic effort to spend five or ten years collecting urine from aplastic anemic patients and then figure out how to purify it without knowing anything about purification.

JONES: Yeah. Well, that's . . . we know how that turned out ultimately, which was bad for GI later on.

KAMEN: Yeah, that was.

JONES: But there were some successes. Well, I guess, gamma interferon didn't really go anywhere.

KAMEN: But GM-CSF did, and we ended up having a very strong hematopoietic growth factor group that Steve Clark led, and they cloned lots of stuff. So GM-CSF moved into clinic. That was a collaboration with Osundo, which was difficult. They were very early in the recombinant protein learning curve, and it was challenging working with them.

JONES: What were the problems? I mean, you guys were the experts, right?

KAMEN: They didn't have people to carry the work forward. They weren't really committed. They were very disorganized. It was not impressive.

JONES: So was it . . . ?

KAMEN: So I had to not infrequently get the head of R&D on the phone and try to complain about the performance of the people and try to get them to do something, and it was managing things like this.

JONES: Sure. At some point, did you get the feeling that we need to back out of this somehow? Is there any way . . . ?

KAMEN: We often . . . we had lots of meetings at GI about getting out of the deals. We offered Baxter a substantial lump sum to get factor VIII back.

JONES: Because you thought— to derive the value from it?

KAMEN: Well, it was a great opportunity for GI early on to get equity from Baxter. But they weren't exactly quick in development. And also at least, I—I guess we—came to realize that they weren't the ideal partner because they hadn't maximized the value of their factor VIII business; they couldn't underprice their blood-derived factor VIII. They had a franchise to maintain. They'd have to figure out a strategy to roll the next gen in increasing value but not dip into things. Had we done it ourselves or given it to somebody who wasn't in the business, they could have done . . . they could have competed with Baxter—got it faster and a lower price. So we thought we could do better. But of course, that was not something that Baxter was going to agree to. It didn't go anywhere. It was a bit frustrating . . . the whole . . . the pharma industry was much, much less able than it is now. And we had a lot of partnerships, and I was the main liaison between the management of the two companies, and all of those relationships were challenging.

JONES: And the bone growth factors, who were the partners in . . . ?

KAMEN: That was unpartnered.

JONES: Okay. So nobody else was really doing that? That was one project that . . .

KAMEN: No, the scientists at GI had to whip me into agreeing to do that. That was . . .

JONES: You didn't think it was a good idea because there was nobody else . . . ?

KAMEN: No, I agreed but they . . . there was a guy named Bob Kay who left GI. He is the guy who set up what became METRx, which is now the vital part of BMS [Bristol Myers Squibb]. He left GI, moved to the West Coast and built . . . he hired the guy who is now the head of BMS's oncology division. But that's a . . . this is transgenic animals that make antibodies. We were working on that too. Yeah. So Bob had . . .

JONES: Oh, so he was working at it there at GI and then . . . ?

KAMEN: No, somebody else was.

JONES: I see. Did that person go?

KAMEN: No. That's a separate story. So Bob had gotten excited about BMP. But the literature was really bad. It was really murky. Mystical stuff. And wasn't very convincing, so we had a whole series of meeting, and eventually Bob and a couple of other people convinced me that we should do it. But that wasn't my first predilection. He gets a lot of credit for . . . he's very persistent. But then this woman, **<T: 70 min>** Elizabeth Wang, actually purified BMP.

JONES: It was a difficult thing to do?

KAMEN: Very hard.

JONES: Why . . . but there wasn't . . . cells didn't produce very much of it?

KAMEN: Cells. [laughter] Cow legs. Elizabeth is quite something. She's a biochemist but a really tough lady. So she agreed to do this. She was excited about the challenge. She read the papers, and the next day she came into my office—or the next week—with a requisition for a bone saw, a bone grater. [laughter] And we went to the slaughterhouse and got these legs and cut the hooves off. So there was a published procedure from Leon Urist—I think is his name—who discovered, who invented BMP.

JONES: So nobody knew where these . . .

KAMEN: Well, no, you extracted it with urea by grinding up bone—cow bone—cooking it up in urea to take the soluble portion out and then fractionating the extracted protein. It stank, and it was awful. So Liz managed to purify what she thought was BMP and that led to the project, but she wasn't the easiest of people to deal with. Had not the best interpersonal skill. Brilliant, but a bit of a special person. And she had a scientist that she hired named Sampath—Kuber Sampath, I think. I don't know if you want to know all this sort of detail?

JONES: Yeah, yeah. Absolutely.

KAMEN: And he was I think either from Urist's lab or from somewhere else at the NIH, and he actually had experience in BMP, so he was quite knowledgeable, but no one had ever purified with homogeneity. And Sampath and Wang ended up having a conflict. And she thought that he was fudging his data because what he thought was BMP was not what she thought was BMP—something at which peak on a column was active. The other problem, to purify it the only assay was a one-month rat implant model. So you took your extract, you write it down, put it in a collagen capsule, a gelatin capsule, and you plant it in the flank of a rat and waited for a bone shape. Can you imagine purifying something . . . ?

JONES: Yeah, so did . . . at the beginning when you're trying to decide should we do this or not do this, did you know that this was going to . . . ?

KAMEN: Yeah, that's why . . . I thought this was . . . I said he had to wear me down; this didn't seem like science. The whole thing was very strange. So these two scientists . . . Liz was the boss, and she wanted to fire Sampath. And I ended up backing her because she seemed to have a rational reason. So he went to another company, which became the other source of BMP. Of course, there are fourteen BMPs; they were both right. But we didn't . . . at that point in science, we didn't know.

JONES: Yeah. You ever get a chance to apologize to him?

KAMEN: I don't think it was all that acrimonious. He did okay. But at that time, the thought was we were purifying BMP there would be one peak coming off a column. It hadn't occurred to us that they both could be right because the personalities were in conflict.

JONES: And that turned into a product.

KAMEN: Yeah. Yeah.

JONES: Or maybe several . . . a family of products?

KAMEN: No, no, it's one product, which is in trouble now. I think in May, it hit about a billion.

JONES: A blockbuster.

KAMEN: Well, for Medtronic, it was the most successful project ever in spine. But it has some side effect now. The sales have dropped tremendously. And that was developed with Medtronic because it had to be a device. It's an implantable device. That was after I left.

JONES: Other notable projects or things happening during this period?

KAMEN: We didn't talk about tPA [tissue-type plasminogen activator]. tPA was a big project. And that was a collaboration with Joe Sambrook at Cold Spring Harbor—probably his idea. That was before I joined GI. And put a lot of work into that. That was partnered with Wellcome [Sanger Institute], and that was quite a good partnership. Welcome was good to work with.

JONES: Why were they better than the others?

KAMEN: Good people.

JONES: And the interests aligned?

KAMEN: They had a company called . . . they were just staffing up. I don't think **<T: 75 min>** they were scientifically up to what GI had, but they had a unit called Wellcome Biotech. I forgot the guy's name who ran it. He and Gabe became very good friends. I'm blanking on his name. Very dynamic business guy. And so that relationship went fine. And we developed first-generation tPA which is the same as Genentech's. That was just one of these competitions. But we were a little bit later than them and then we developed a next generation tPA with improved PK. And those unfortunately both got involved in a patent suit with Genentech. And the second generation had been developed particularly with regard to the patent position because the

Genentech patent claims were extremely narrow. They were just tPA because they really hadn't done anything. Désiré Collen had given them a portion of the tPA he had purified in order to treat the patients with. So he had tons of it. They sequenced some and cloned it. But you couldn't get a patent these days for something like that. But they did. But because of the narrow scope of their inventiveness, they got a patent only on tPA itself, not fragments thereof or anything like that. We had a, sort of, deletion mutant of a variant of tPA which had improved properties. And unfortunately, the patent case on the first-generation tPA . . . Genentech in its legal wisdom included in the suit arguments including the second gen because it was the same partner that had both. And the case went to a jury trial, and the jury decided in favor of Genentech on the whole case. I don't understand any of it, and the judge in his final remarks pointed out to the jury that Genentech didn't have a patent on the second gen, but it was too late.

JONES: There was no appeal?

KAMEN: It was appealed and reversed six years later. [. . .] No, it was after I left. I think it was relicensed. Welcome gave it back. It was licensed to BMS I think. And they did a big clinical trial, which I don't think was positive. But that was after I left. But it was still late.

JONES: Yeah. Well, how did you feel about . . . you have to deal with these commercial and legal dimensions of the work, which you hadn't done before.

KAMEN: That was all very exciting.

JONES: Was it?

KAMEN: Yeah. So the GI . . . so Gabe had this, sort of, management team that ran the company, which is him and two or three or four people, depending on the times. I was in that group and involved in everything.

JONES: Yeah. Very stimulating? Did you ever feel that . . . ?

KAMEN: It was stimulating but very stressful. By the end . . . by '89 I was pretty much worn out because GI had had a pretty rocky period. We were involved . . . we lost the tPA case.

JONES: And then EPO was—

KAMEN: EPO, I just didn't agree with Gabe about the EPO strategy. I didn't think we—

JONES: The legal strategy?

KAMEN: Legal strategy. I just didn't think it was worth fighting. I proved to be right, but who knows?

JONES: Right, so that was the source of—

KAMEN: But it was very difficult times for Gabe, for everybody because the company was on the line, we had quite a few people, he was worried about the company, and one week we'd have this executive committee meeting and he'd be selling the company, and then the next week he'd come in and say everything is fine, and the data hadn't changed. So I would drive him crazy because I kept picking at the financial data, and I think he was just trying to do his best. And we ended up in great conflict.

JONES: And at some point, you decided to go?

KAMEN: No, he kicked me out.

JONES: He did? Yeah.

KAMEN: It was pretty traumatic. We've connected again. I was just burnt out. It had been very, very difficult. I was working seventy hours a week. It was pretty tough. The other thing that was challenging in the end of the eighties, it was hard to know what to do with a therapeutics recombinant DNA company because most of the newer projects we started failed. The whole idea that you could have a cell line, fish around in a supernatant, and find something to cure cancer, [inaudible] it made no sense.

JONES: It's a lot more complicated.

KAMEN: Yeah, because once you learned about the pharmaceutical **<T: 80 min>** industry, you had to start with the disease, find out the disease mechanism, and then make a drug that would interfere with the disease mechanism. It doesn't mean that just because you had a cell

line that made some activity . . . so it was a little depressing, I think, probably throughout the whole industry at that time. The ripe, the low-hanging fruit had been picked and where the industry was going was a bit unclear. Maybe that was just in my mind.

JONES: So but that did start to dawn on you late eighties?

KAMEN: Yeah, late eighties. And I also was tired of being on the biotech side of the table since I had . . . maybe we had six, seven collaborations and maybe one or two of them went well, but dealing with big pharma from an inferior position. So my goal was to switch to the other side of the table.

JONES: Did you have . . . what was your . . . well, first of all, let me ask you, this is getting frustrating—all kinds of problems. Were you at any point longing for the good old days in academia?

KAMEN: No, no. Never, never. I loved working in industry. Having resources and a big team and getting stuff done and having a stable group of really high-quality . . . we could really do stuff at GI. Really good professionals. You didn't have to worry in general about scientific quality; things just got done. I loved that. Bigger would have been better from my point of view.

JONES: Yeah, so and you discovered this by getting into GI and seeing how it developed?

KAMEN: Sure, yeah. And I liked doing the whole integrated thing of having a company. It was . . . academia is intellectually exciting but also a big ego trip, and I didn't like that part of it. And at a company, you're really doing something. You're helping patients. It was wonderful. So the idea was how to do that but do it in a more . . . maybe secure environment free of Wall Street. At that time, Wall Street didn't understand biotech the way they do today, and I had to talk with analysts all the time. And it was frustrating that you were almost forced to inflate because they discounted anything you said tenfold. So if you just told them the truth, it would be like the company is falling apart. I think since then things have gotten a lot better. The industry is a lot more sophisticated. But back then the interactions weren't very pleasant. At the time, when thing were getting difficult with Gabe, I was being recruited by a major pharma company, and I was so loyal to GI, I couldn't even talk to the people. It just seemed wrong. So I thought, "Well, if I just left, it wouldn't be so bad if I just move onto a big pharma company."

JONES: Well, what was your notion of what big pharma . . . you were dealing with those folks. Did you have the very different kinds of organizations . . . even when GI gets to be over a

thousand people, it's still tiny compared to a big pharma corporation. Do you think you had a good idea of what working in an organization like that would be like?

KAMEN: No, since I ended up doing it, the answer—the honest answer—is no, but I don't remember what I thought. No, my thought process was mostly that I didn't want to be on the inferior end of a partnership again. Having to . . . I was comfortable managing people but having to manage people indirectly by going to their boss to . . . that was just not something I wanted to continue to do. That's what I was thinking about. And pharma was improving by the end of the eighties.

JONES: It was still . . . it was the age of blockbuster drugs, so they were very rich, right?

KAMEN: Yeah, I hadn't thought about that.

JONES: Well, so what's your move? Gabe decides it's time for you to go. And so, well, there you are. What's your . . . ?

KAMEN: There I am. I had no job.

JONES: So how did you respond to that? What was your reaction?

KAMEN: Well, I was pretty low. I have a good wife. I did a lot of gardening and planting plants and looking around. I had options, but I also didn't want to leave the Boston area. So I wanted a senior job at a pharma company in the Boston area, which was a little difficult in 1990.

JONES: Was your wife still . . . she was still at GI?

KAMEN: She was still at GI. Yeah. Yeah.

JONES: So there's a lot going on here, though. You didn't want to go to . . .

KAMEN: There wasn't any big pharma.

JONES: Yeah, you didn't want to go to another biotech . . .

KAMEN: Well, I went . . . it was about a six-month process with this company that was trying to recruit **<T: 85 min>** me, which ended up with a dinner with Roy Vagelos, and he said no.

JONES: Is this Regeneron?

KAMEN: No, Merck.

JONES: Oh. It was when he was still at Merck?

KAMEN: Yeah.

JONES: I see, yeah. Okay.

KAMEN: So the job was to be Scolnick's successor at Merck. But Scolnick wanted me, and Vagelos didn't, so that was just . . . well, it turned out it was a life saver because there's been at least ten people who got hired at that position who got hired and chewed up and spit out. That would have been disaster.

JONES: So that took a . . . that was an extended process.

KAMEN: Well, yeah and then—

JONES: And had they contacted you when you were still at GI?

KAMEN: Yeah, yeah, yeah. Ed had. We'd had dealings with Merck about tPA, and Ed had really wanted to license tPA, and he and I knew each other from academic life, anyway. We had reacquainted ourselves in our new commercial roles, and he had insisted that it was impossible to make recombinant protein in CHO cells and that they would only enter into a partnership with us at tPA if we switched the cell line to yeast.

JONES: Why did they think that?

KAMEN: I don't know. So he and I had this conversation in Merck cafeteria, and I remember I said, "Ed, you're wrong. We're not gonna switch." And that was the end of it. But I guess he realized some years later that I was right and tried to hire me.

JONES: I have in my notes here that the Whitehead Institute, visiting scientist.

KAMEN: Oh yeah, that's right, so David Baltimore invited me to hang out at the Whitehead. So I spent some time there that . . . I felt really like a fish out of water going back from GI to the Whitehead. It was really strange.

JONES: Yeah, were you thinking about maybe starting up some kind of line of research?

KAMEN: Well, I toyed with it. Actually, I did interview for some academic jobs, but it just wasn't me. It wasn't me. Then fortunately Jim stepped in again. As I said, Jim knows what's happening. I hadn't talked to him very much at all through this whole process since we weren't close. But one day out of the blue, I got a call from Michael Wigler at Cold Spring Harbor. He was this superstar genius at Cold Spring Harbor. Mike and I knew each other from our graduate student days. And Mike says, "Bob, Jim tells me you're out of work. I got an idea for a company." And Mike—

JONES: A startup company?

KAMEN: A startup company, yeah. Mike wanted to start up a recombinant antibody company.

JONES: Produce them in CHO cells or what?

KAMEN: Well, he'd invented what ended up becoming phage display.

JONES: I see. He was involved in the first patent, right?

KAMEN: Yeah. There was a company that was going . . . Mike wanted to start a company, and I agreed that I would work with him, started thinking about antibodies, which I hadn't thought about very much. GI had a little antibody group, but it wasn't . . . it never . . . the person doing it

wasn't very good. So he had this recombinant way of making antibodies by screening libraries. And he wanted somebody to help him make that into a company and figure out what to do with it. So I started thinking about antibodies, and it was like, eureka! This is the drugs, you know. You could think about disease process. You could make an antagonist.

JONES: And these are human antibodies you're talking about?

KAMEN: Yeah. But I hadn't thought about using antibodies chronically because they're a mouse, and you couldn't do them chronically. But the problem at that time was . . . well, making drugs that blocked protein to cell surface protein receptor interactions like making IL-1 antagonist. It was a hot project for many companies. Trying to do a small molecule is impossible.

JONES: Why is it impossible?

KAMEN: Just basic physics. The surface of interaction between proteins are very large. The affinity is very, very high. And to have a little— a small molecule is just too small. So small molecule drugs block receptors that are small molecule receptors.

JONES: Yeah, okay.

KAMEN: But blocking a protein, no one's ever done that. It's impossible. I suddenly realized if you really make an antibody, this would be hot. This could be a whole big thing. So we worked . . . it was funny. So Mike brought in a friend of his from Columbia named George Yancopoulos who would have helped start this company.

JONES: Okay, so this is Regeneron?

KAMEN: No. Well . . .

JONES: Or would have been . . . yeah, anyway.

KAMEN: Actually, could have been. I only met . . . had a couple meetings with George. He was **<T: 90 min>** impressive, and Gustav Christenson was involved on the business side. I don't know if you know him. He was Gabe's lieutenant. Another Baxter guy. He's now the

CEO of Dyax. So Gustav . . . was he between . . . ? He was also between jobs, I think. Must have been. Or he was doing it on the side I recall. So we started this company called Hematech. And we had a Japanese guy involved, who was very well plugged in with a wealthy young Japanese businessman. So this was '89; it was the high point of the Japanese economy. And there was this guy who was the heir to a noodle company that was getting involved in pharmaceuticals. And we had this plan that he would finance it, and he agreed. He wanted to put in a lot of money; it would have been a wonderful deal. But his father stopped him. So that whole thing fell apart. But I had been excited, and I decided I wasn't going to go to VCs [venture capitalists]. I just didn't want to do it.

JONES: What did you know about VCs that did . . . ?

KAMEN: Probably naïve. I don't know. I don't know. I can't recall.

JONES: Yeah. So you just dropped that?

KAMEN: Yeah, so I dropped that and started looking for a job again and then fortunately another one of these strange phone calls. It was about six o'clock in the evening. I was cooking dinner for my wife. I was home cooking. And Bob Weinberg calls me up and says, "Hi, Bob." I hadn't spoken to Bob Weinberg in like three years—something like that—and he says, "Hi, Bob. You live in Sudbury, don't you?" I said yeah. And then he says, "Is that anywhere near Worcester?" And I said, "I don't know. I don't know where Worcester is." [laughter] So I said, "I suppose so." And then he . . . I can't remember the rest of the conversation, but he said that he was on the scientific advisory board for this company, BASF Bioresearch, and they were looking for a president, and they were going to build a company in Worcester. Would I be willing to talk to them?

JONES: So a biotech arm of . . .

KAMEN: Yes. So at that time, I was well along with taking a job working for Berlex, which is the US arm of Sheering because they had bought two companies on the West Coast—Triton and Codon—and they wanted somebody to put the two companies together and run that, and I had been spending a lot of time on the West Coast.

JONES: So you were looking at a lot of different things?

KAMEN: Yeah, well, actually two German companies, it turned out because that was run out of Berlin. But we didn't want to move to the West Coast. The kids were in school, and it would have been difficult. So then it happened . . . it only took about a week or two, so I knew about the BASF Bioresearch because there was a GI attorney who'd gone there. They were in Cambridge. Just started up.

JONES: Small, very small.

KAMEN: About fifteen people—something like that. So I—

JONES: Were they going to . . . was the plan to do pharmaceuticals or industrial things?

KAMEN: Pharmaceuticals. This part of the history, you have to learn about BASF Bioresearch. This scientist from Germany flies over. I meet him; he's a very nice guy. He drives me out to Worcester. They bought twenty-five acres of hilltop property just above the medical school. It's beautiful piece of property. They're building a 376,000 square foot building. Just the foundation was in. And this guy Werner says to me, "Would you like to do something with this? Figure out what to do with it. We're building . . . we want to build a big biotech company here." So I went to Germany and met some other people. Very nice. I was more happy . . . I spent thirteen years in Europe, and I like working with Europeans.

JONES: Why?

KAMEN: Cultural. I don't know. My wife is Dutch. We spend a lot of time in Holland. And I like . . . my daughter now is retro-emigrated. She lives in Germany. She's a Dutch citizen. So the family—

JONES: How would you describe the contrast, though? The cultural contrast. What are the differences?

KAMEN: I have to think about that. I don't know.

JONES: My brother lives in Germany, and he says Germans are a lot like Americans.

KAMEN: Well, that's a long topic.

JONES: Okay. So this seemed like something really good . . . it's—

KAMEN: Well, this was perfect. So it was a big company. I had no appreciation how big. Of course, I had to ask who is BASF? I never heard of BASF. I knew about the tapes, and you probably don't know about BASF either?

JONES: Well, it's a big chemical company. It's been—

KAMEN: It's the world's biggest chemical company. It's huge. You know, it's . . . **<T: 95 min>** at that time they . . . on the Ludwigshafen headquarters site, they had a hundred and ten thousand employees. The site is twenty-seven square kilometers. [laughter] So this is a big company. Very complicated, very well-run. And they have . . . see BASF North America is a Fortune 100 company. They have forty-seven big factories in the US alone.

JONES: Yeah. It's monstrous. Yeah.

KAMEN: Every part of the world. I didn't know that when this job came along. And they had a small pharma division, which was largely a German company called Knolle. That was the only other business in this small city of Ludwigshafen, Germany. I took that job. It just seemed wonderful because they wanted a [inaudible] president. I didn't have anyone over me at least in this part of the world.

JONES: Great facility.

KAMEN: And the budget, I guess my peak annual budget was about 125 million—something like that.

JONES: Which is way beyond anything that you . . . at GI, what were your budgets?

KAMEN: I don't recall.

JONES: A tenth of that or something?

KAMEN: Probably a tenth of that. But so . . . and they didn't really have a firm idea of what to do. And it turns out—

JONES: Did you have one?

KAMEN: Well, I'll give you the list. So BASF Bioresearch came about. It started off as a collaboration between a small biotech group in Germany, and BASF decided in about 1980 they should do something in biotech, and they have smaller now but then, they have this big central laboratory. About fifteen hundred scientists—something like that—serving the whole company, so mostly chemistry research. But they for . . . I don't know the reason, but they decided they should do something in biotechnology. And they had a small biotech group. And the guy who came over was the head of that group. And they also had bought some years before this pharma company. And they brought the medicinal chemistry group from the pharma company into their main laboratory and that was, sort of, side-by-side with the biotech group. But they realized they had to collaborate. So they did some deals. They did a deal with Integrated Genetics for tPA. Had a deal with Biotin for TNF. And the two senior people from this main lab were coming over about every three months to Cambridge to meet with Integrated Genetics and with Termeer—well, the company Termeer bought.

JONES: It was after Termeer had bought them?

KAMEN: No, this was before. This was '85. They started working with Integrated Genetics.

JONES: Oh, okay.

KAMEN: And they were working with Biogen, and these two guys got this idea they really shouldn't do biotech in Germany. They should build a company in the US in Cambridge. And they were eyeing a site; there was a vacant site right next to Biogen. So they actually wrote a proposal to BASF that they should buy that land and build a company, but they never took it to the BASF executive board. They . . . in a big company, you don't take something [inaudible] you don't know it's going to get approved, and they were getting strong messages that they were not going to do research out of Germany. This main laboratory was a hundred years old; they had two Nobel prizes. They thought they were the best in the world and the idea of doing this in the US at least for the senior people in BASF was too foreign.

JONES: So did they go ahead in Germany?

KAMEN: So they went ahead in Germany. They had TNF in the clinic. They took over the clinical development. They were using TF as a cancer drug—trying to use TNF as a cancer drug. But BASF being a chemical drug is very manufacturing-oriented. And what it does is build huge manufacturing plants and organize them. That's the strength of the company. So they viewed biotechnology as another manufacturing thing. So they designed and broke ground for a TNF production plant—bacterial recombinant DNA plant—on a site in the big headquarters site. And in '89, the German government in its wisdom passed the famous gene law where they outlawed biotech production in Germany. So the same two guys who had had this idea for doing it in the US said, "Ah ha! Maybe they'll listen now."

They dusted off this old document. Within a few weeks took it to the BASF executive board, got approval. And they also knew that if you want approval in BASF, you ask for capital because chemical companies spend a lot of capital. They're very organized for capital decisions. There actually are commissions within BASF which vet capital requests. There's all sorts of paperwork and **<T: 100 min>** numbers. So they asked for a hundred million dollars to build a building because they knew that would be a decision that could be made. So that was when I got hired because I learned this after I went to Germany.

JONES: Yeah, that's fascinating, yeah.

KAMEN: But they didn't really have a . . . it's all—

JONES: An idea of what to do with it?

KAMEN: It was on rebound. They had this idea that the biotechnology was really going to go somewhere.

JONES: Did they sell TNF in the clinic, and it was . . . ?

KAMEN: So I took this job. And what I had in mind was well, I'll do antibodies. You know . . .

JONES: Phage display?

KAMEN: Well, yeah. By that time, Greg Winter had invented phage display, and I thought that well, if you could make an antibody and if you could use it chronically, you could open a

whole new field. And what I decided to do . . . we had a portfolio of all sorts of different projects, but one of the things we decided to do was to try to build a fully human antibody platform. It was like GI in the beginning. We hadn't moved into the big building. We were in Cambridge in rented labs. And there were about nineteen, twenty people. Nobody with any industrial experience. All postdocs. Same thing, again.

JONES: Yeah, so this . . . but it is inside in the big company so that makes a big difference for you when you're negotiating partnerships . . .

KAMEN: But so we were in a position where . . . so we decided we're going to do antibody, and we picked TNF as a target because we actually had a balanced anti-TNF in the clinic for sepsis too. The guy who invented the antibody was . . . oh, to set up BASF Bioresearch, they approved the hundred million dollars, and they approved sending over six young scientists as delegates. All of them under thirty. Three from [inaudible], three from BASF, and they shipped them off to the States and said, "Build a company." One was president. It's an amazing thing that BASF develops talent. They just—

JONES: Here, go do it. See if you can sink or swim or . . .

KAMEN: There was supervision from Germany. But it was pretty hands off. One of the five, Erich Schlick, had been promoted back to Germany after a year-and-a-half, so I succeeded him as president, but with some more authority than he had. But one of these scientists who had been shipped over actually was coming back to Cambridge; he'd been a postdoc at MIT, and he was monoclonal guy. And he made a mouse anti-TNF. We had kilos of high-quality recombinant TNF, and it seemed like TNF wasn't a bad target for a therapeutic antibody. We didn't have a well-defined clinical indication, but—

JONES: Okay, so this is the beginning of Humira?

KAMEN: Yeah.

JONES: So it starts . . . I didn't realize that.

KAMEN: Yeah, so we needed a target. And I knew enough about industry to say they were romancing me with this great job. But I knew if I didn't have something in the clinic pretty damn fast, I wasn't going to keep that job and they weren't going keep this unit. You know, honeymoon doesn't last forever. So we wanted to do antibodies, so we had to collaborate with

somebody. We didn't have any technology to make a human antibody. And the target had to be something that this small group of people knew about. They knew about TNF in assays. So, you know, it seemed like a logical thing to do. And you could write down what were ten possible indications. None of it really proven. So we set out to make a human equivalent of a mouse antibody that was already in the clinic because that couldn't be given repeatedly. And by that time, Greg Winter had founded CAT with a guy named Dave Chiswell. You know Chiswell? He's the great leader of British biotech. Dave had been postdoc at ICRF.

JONES: Oh, you knew him?

KAMEN: Yeah. And I had taught David something . . . western blots. I don't know. Northern . . . probably northern blots—something like that. So we used to go drink at the pub together. Shortly after I took this job with BASF, decided antibodies, thinking about TNF, and we were. . . there were the beginnings of Abgenix and MET-Rx making transgenic mice, one of which was an ex-GI guy. We knew about these technologies. We knew about phage display, but we didn't know which one. So I went out to a PEGs conference in San Diego, ran into Dave Chiswell. Had lunch with him. He told me about this company—Cambridge Antibody Technology—that he and Greg had started, that they had a couple of people, they were getting going, and they thought the technology worked, and they weren't really sure. So we said, "Okay, we'll do it together. I got some people; you got some people. Put a deal together. We'll try to make a full human antibody against TNF." It took lawyers and all the other people. Time. And had to be in approvals and all that sort of stuff.

JONES: But it is **<T: 105 min>** easier for you now doing this inside BASF?

KAMEN: Sure, because I was the superior in this case, so . . . and Chiswell was amazing. He wasn't . . . he was a CSO, he was running the company. They went through three or four CEOs and finally had the wisdom to give him the job. But he put us—what he propositioned, he said, "The deal is you pay us six hundred thousand pounds when we deliver an antibody or part of an antibody fragment." I don't remember the detail. "Which you think is good enough to develop clinically." "If we don't make it, you don't pay us anything." Those were the terms. And then we got, I think, three more targets on the same terms.

JONES: So no problem getting that approved?

KAMEN: No, and I tried to do some collaborations based on chemistry but that piece I wouldn't accept because they didn't think anyone else could do medicinal chemistry as well as they could, which may or may not be correct. But they were pretty damn good. But in this field that's what they want . . . that's what they hired me to do. They wanted to use the network.

JONES: Yeah. Did it make sense to be located in Worcester?

KAMEN: Yeah, it worked out very well.

JONES: Well, what were the amenities?

KAMEN: We moved into Worcester in '94. [laughter] This massive building . . . the building was half production plant, half research lab. It was amazing production facility. They were largely in shell, but the infrastructure for water purification, waste disposal, BASF really knows how to do that. Amazing miles of beautiful stainless-steel piping—much better than whatever GI ever had. There was a guy, Peter Moesta, who was one of the German delegates who was super. He was in charge of the facility. He built the whole production thing. So that was a big asset. They were willing to back this collaboration. So it made a great deal of sense. At the time, it took . . . everything has its ups and downs. They tried to shut us down a few times; it wasn't easy. Developing any drug is never—

JONES: Sure, they resisted. At some point, there's somebody saying, "It's taking too long." I mean . . .

KAMEN: There, you know, at any—

JONES: You got supporters and detractors inside the company, yeah?

KAMEN: Yeah. And politics. They're people. All normal stuff. But you got to fight for anything. But for me being involved . . . I'd never gone to a big company and this was really . . . this was a big company.

JONES: Yeah, so what did you learn about that?

KAMEN: Well, I learned the culture of German companies, which was the opposite of what I expected. I expected it to be rigorously hierarchical, and it was rigorously democratic with a few outliers. But everything was done by consensus, and I learned not surprisingly in retrospect that strong, dominant individuals are feared and not respected in Germany. That you're supposed to be collegial and everybody has to agree, and if there is somebody with a certain responsibility,

they may be hierarchical three layers below you, you can't order them around. You got to convince them if they don't agree. So it was all consensus building. It was interesting.

JONES: So how do you feel about that approach?

KAMEN: I think it's a very sound approach.

JONES: It can get tied up, though, yeah?

KAMEN: You have to be reasonable.

JONES: Everybody has to be reasonable.

KAMEN: But you don't want a one-man show. You don't want to order people around, but it was very interesting learning experience. And we did lots. . . so the company was . . . it went from about twenty people to about four hundred and fifty before I retired. And it was very good. We recruited a lot of people. Being in Worcester was . . . you know, academics will come and give talks if you send a limo. But your employees like to have a nice place to live they can afford. I don't understand the whole Kendall Square thing honestly. Not for biotech. For a start-up, I understand it, but if you're building—

JONES: Maturing and coming in, yeah.

KAMEN: Because Worcester is, it's . . . we had this lovely site. Easy to get to. There's no traffic. And employees can live well. Live in Shrewsbury. There are lots of communities around which are, you know, good schools.

JONES: Well, for a lot of people, Cambridge is an attractive place to be.

KAMEN: Yeah, but not on a salary you get paid as a starting salary. You just can't afford it.

JONES: Yeah, I guess.

KAMEN: They drive two, three hours and live in New Hampshire. It's misery. **<T: 110 min>**

JONES: Yeah. So how far along was Humira when you decided to retire?

KAMEN: So I'll tell you the Humira story, so we started work on Humira with CAT, and that was a fifty-fifty research collaboration. They wouldn't let phage display out of the house. So they did the phage display, but we did everything else. And we . . . how can I be polite? We supervised the collaboration. I asked a young German scientist, Jochen Salfeld, to lead the program.

JONES: Under you, not over . . .

KAMEN: Yeah, he had been hired by BASF before me. He was from . . . he was a graduate student at the University of Heidelberg, which is in the BASF [inaudible]. Ludwigshafen is close to Heidelberg, and that part of Germany is more or less run by BASF. It's by far the largest employer. There's not a lot of other industry there. But he'd been a postdoc with Bill Hazeltine and was hired out of Dana Farber into BASF Bioresearch. And he was a molecular biologist, so I asked him if he would be in charge of the recombinant antibody work, which was the best decision I've ever made. The guy is fantastic. I knew his . . . his supervisor was one of the cofounders of Biogen, was on the original scientific advisory board.

JONES: Tom Snyder?

KAMEN: No, there was one guy from Heidelberg . . .

JONES: Oh, yeah, I know. It's a . . .

KAMEN: Bacterial geneticist.

JONES: I can see his face.

KAMEN: He's gone now, I think. Anyway, shortly after I took the BASF job, I went to university and had lunch with him and some of the other people there. So I asked him, "What about this guy Salfeld?" It was another memorable conversation. He said, "It's very simple. There's only one thing you have to know about Salfeld. What you ask him to do, he will do.

That's it." [laughter] That turned out to be, so Jochen is now the head . . . he's the head of Biologics for AbbVie. He's been there forever. He's got a hundred and eighty people in that group, and he's pretty much responsible for Humira from start to finish. He still spends 30 percent of his time on Humira from 1991—almost all patent litigation. So we worked with CAT, and it took a couple of years, but, sort of, by brute force, we got phage display to work. It didn't have good libraries, but they improved along the way. So we got Humira to work. And meanwhile, Mark Feldman gave this incredibly important talk at Keystone Meeting in '93 where he talked about treating twenty patients with severe rheumatoid arthritis with what became Remicade. This failed Centocor anti-TNF chimeric antibody. Dan Tracy—my head of immunology—was at the Keystone meeting. He called me up actually after the talk. He was so excited. He said, "We have the indication. Nothing has ever worked." I don't know if you know about that paper. It was a paper in *The Lancet*.

JONES: No, I'll have to look it up.

KAMEN: It's . . . Marc Feldmann is the senior author, and Tiny Maini who is the head of the Kennedy Institute in London. They treated twenty patients, and eighteen had a remarkably positive response. And Centocor jumped on it, Immunex jumped on it, we jumped on it. We knew what to do. We got some clinical people vaguely interested within the company. About a year later, we had a clinical candidate—we thought—put into production. We had a group . . . we'd already built up a production thing, so we made it in Worcester and then had to get people in the company involved to do clinical trials, which was difficult because there was no experience.

JONES: Doing biologics?

KAMEN: No, no experience with rheumatology. So Jochen Salfeld knew . . . . I won't explain the details . . . but he knew there was one senior clinician who probably would jump on this because he was jealous of somebody else and was a real bull of a guy and if he jumped on it, it probably would get through. The problem was he was an oncologist and didn't know anything about arthritis. But between the two of them, they hired consultants and they hired a guy who got fired by Roche, who was a good **<T: 115 min>** rheumatologist as well, plugged in with KOLs, and delivered the clinical plans so they got into the clinic and after a while convinced the US company to be interested. It took a lot of discussions. They weren't interested in the beginning. And at that time, we had reorganized into, kind of, a therapeutic area organizational structure. It was a really good structure. So within each therapeutic area, there was an organization that went from marketing to early project selection, and everybody worked together and you have one budget.

We had an immunology, oncology therapeutic area that I co-led with a very good marketing guy. And in '95, I think, we had a key meeting where we pitched our overall budget,

including a lot of money to take what is now called Humira into phase two, I think, or to continue to phase two. And this committee in its wisdom said, "No, we won't give you any more money. You have a budget for your therapeutic area. It's your problem. Figure out how to pay for it." So we went out of the room, and we came back and told them we were exiting oncology. We were shutting down the therapeutic area.

JONES: And put all . . . so you could put all your money into clinical trials?

KAMEN: And not only that, we sold the pipeline for sixty-five million to Dan Von Hoff's company in Texas, which then got bought by Genzyme. But we got sixty-five million to put into the Humira.

JONES: And you could do that? There was . . . that was . . . yeah? Did that make any waves or . . ?

KAMEN: No, it was . . . I had fought to have . . . I had my own BD group and a little group. Two people in Worcester. So they cut the deal. I hired my own legal team.

JONES: So that funded phase two?

KAMEN: Well, it funded . . . it helped. But we had to fire people, move people around. That, sort of, went on, and towards the end of the nineties—'97, '98—I was increasingly concerned that we wouldn't be able to market Humira because the Centocor drug had been approved, Enbrel was about to be approved. And we had a little Knoll Pharmaceutical Company in New Jersey, which was not the world's finest pharma marketing company. And they wanted to keep it. They wanted to market it. Of course. It'd be a potential big drug, or at least significant drug. I don't think they ever thought it would be a huge drug. And I didn't agree with that. I didn't think that was suitable. I thought we should partner it. So I tried several times to convince senior management to partner it. First time, they agreed to do an exercise. The partnering was run out of the market . . . sales and marketing company in New Jersey. And they reported back that there wasn't really much interest, which I knew wasn't true. They just . . .

JONES: They didn't want to do it?

KAMEN: They didn't want to share it. So some years later, we pushed again. Got approval to look for a partner. But I had learned my way a little bit around the company and had gotten approval that we would run it out of the research site in Worcester, and the condition was that

we had to change the reporting structure of the local BD person to report to the BD person in Germany, which was fine. The guy in Germany was a fine guy. So they ran a partnering exercise. This is '99, I guess. Drove us in phase three. And we made a list of who was good rheumatology, who to talk to and Pfizer had already been pushing us. They were interested, and I was amazed. It was a feeding frenzy. I didn't appreciate the value of a [inaudible]. There's nothing better for a big pharma company than an advantage [inaudible] that's two years behind because you know it's going to work. You can calculate its value, and then it's up to your marketing muscle and your strategy, but, you know, it's not like investing and something novel in phase three so it's execution risk.

JONES: Yeah, but you can get surprises in phase three, right?

KAMEN: Yeah, well, of course but nothing without . . . but compared to something in phase three that's a novel mechanism, it's actually prioritized, or at least it was then. There's always trends. We started this partnering campaign, and a lot of companies wanted it. We had the pleasure of turning **<T: 120 min>** down Genentech.

JONES: Yeah, that was a pleasure.

KAMEN: Well, they refused to give us a Cabilly license, so the terms for giving us a Cabilly license was the drug, so we told them no. Also, they didn't have anything to offer in rheumatology. They had no sales force. We ended up cutting a deal with Eli Lilly. What are you looking for?

JONES: Oh, I'm just looking through my notes to see if I have notes on who else . . . you mentioned Pfizer.

KAMEN: That's not . . . that wasn't public at the time. None of this was public at the time. It's long enough. We know non-disclosing is not public, wasn't public anyway. Pfizer was very interested but backed out because this was before they had bought Sorrel, they had a deal with Sorrel about Celebrex, which was their top drug and rather late in the day their legal department told them they couldn't enter into a deal with BASF for an anti-TNF because there was a non-compete in their Celebrex contract defined by therapeutic area, and it included anti-inflammatories. Their lawyers, they just thought it would be a mess, so they politely backed out, and we really liked Lilly. They were a big . . . a real strong biologics company and good people. We actually . . . while we were negotiating the deals, the teams were coming together and people liked each other, and they really had something, they knew something about—

JONES: Who were you talking to? Who were the individuals?

KAMEN: I don't remember now. I wasn't handling the negotiations; that was a BD person. We actually had got ready to execute the contract and that was . . . would have—at the time — would have been the biggest pharma deal ever done. They offered two million dollars up front for basically a Celebrex deal for co-promotion in the US for three years. That's all we would have gotten out of it. They were very excited to do that but . . . I have to be careful about how much I can disclose here, but at the same time, in parallel, I was working on a project to spin out my unit and merge it with another leading US biotech company.

JONES: It starts with an A?

KAMEN: I'm sorry?

JONES: It starts . . . which starts with an . . . Oh, a biotech company.

KAMEN: A biotech company.

JONES: Oh, okay. That's what you can't disclose?

KAMEN: Yeah, I probably shouldn't.

JONES: Yeah, okay. I'll try to puzzle out who it is.

KAMEN: Yeah, this was . . . this wasn't Millennium [Pharmaceuticals], but this was . . . you could look at who had a billion dollars at the time. This was a time when the biotech industry was booming.

JONES: What year? This is . . . ?

KAMEN: This was a bubble. This was when people were raising billions of dollars. Or at least a billion dollars and were looking for products. You can figure it out. Look who else worked with CAT and . . .

JONES: Okay, we'll get it. Yeah.

KAMEN: I'd been trying to convince senior management at BASF—not the BASF pharma things—but my protector within BASF was a guy named Eggert Voscherau, who had been my boss in the US. The organization of BASF Bioresearch was rather complicated because of the corporate structure of BASF worldwide, so legally we were a subsidiary of BASF North America, not of the pharma; we weren't owned by the pharma company. We were owned by the North American chemical company, so formally I reported to the head of life sciences in the North American division of BASF to whom was also responsible the head of the sales and marketing company, and this guy was Eggert Voscherau, who then became the head of BASF in North America and then went back to Germany and became the number two in BASF on the executive board. A brilliant, wonderful guy and he in the various political problems within the company he was always protecting his Worcester site, which he thought was very important for the company. He and I had a good relationship, and I had been trying to convince him to do the spinout. I hadn't told the other people in the pharma division; nobody else knew about it. The major motivation was the people because all these guys in the biotech companies had stock, and nobody knew about us. We had this . . . because we weren't . . . **<T: 125 min>** Wall Street had no interest, so there's no reason to write about us. We had a really good group—four hundred people—production, good research, long molecule, large molecule. Nobody had any stock. We only had one share. BASF went to the New York Stock Exchange to give everybody one share of stock. Imagine how motivating that was. I still have it. What do you do with it? [laughter] What do you do with one share of stock? I thought if we could merge with a biotech company, everybody could get some shares and maybe they'd be rewarded like the Millennium people would be. It may be naïve, but I thought it was my job to try. When I had this Eli Lilly contract on my desk, I guess I would have co-signed it, but it wasn't my decision. It would be through the management of the BASF pharma division. I said, "Well, I better talk to Eggert because if we give away the asset, there's nothing to merge." I called him up and told him about this, and he said, "How come I don't know you're partnering this thing? Nobody ever told me."

JONES: He didn't know? You hadn't . . . ?

KAMEN: No, he was my boss's boss's boss. He was the chemical company—the parent company—leadership, so the head of BASF pharma reported to him, and my boss reported to that guy.

JONES: Yeah, and it's just too many layers to—

KAMEN: Well, it turned out he'd just been busy. He'd just been buying the agricultural business with American Home Products, so he had been very busy. I don't know. I don't know what happened. Anyway, he says, "Come over and have lunch tomorrow." So I dropped what I'm doing, go to the airport, get on a plane, go and have lunch in the BASF executive board.

They get rather well treated by the company. They have a wing on the company—dining common—that's just dining room for the executive people and we have lunch in this octagonal room is all mirrors all around, one table in the middle. I look at the setup, just me and Eggert. Waitresses just for us. They served an elegant lunch. They cook very well there, and he asked me, "What's up? What's with this?" So I explained the whole Lilly deal, and he said, "I've been trying to sell this division for three years." He used an expletive which I won't mention. Because there's been all these rumors that we're aware of that BASF was getting out of pharma. Never confirmed but there were all these rumors. You think there must be something there. He confirmed that in fact they'd been trying to . . . they had decided they wouldn't continue in pharmaceuticals.

JONES: And you hadn't known about this?

KAMEN: Not formally. I had no formal . . . I just knew it was in the newspapers. He said that now I realize that we've been making a mistake. I've been trying to sell a mid-sized German pharma company. I'm going to sell an American company with a potential blockbuster that happens to have ten thousand employees and a pipeline in Europe. Then he said, "Would you tell those nice people in Indianapolis that the terms are a little different? Around ten billion they can have the division." I'm not going to . . . so he was the guy who would have to sign this contract. "I won't sign it." Then he says, "You'll be called by some investment bankers in London next week. Please dedicate the next six months for the sale of this division." That's how Humira got made, and Lilly unfortunately was in the middle of their anti-depressant that had just come off patent, and they were anticipating a decline in sales and they couldn't—

JONES: Couldn't afford it.

KAMEN: Well, I've talked to some of the Lilly people since then, and they really regret that they hadn't figured out a way, but hindsight. Who would have known? So we spent six months in London with the executive team from—

JONES: Again talking to a lot of different people?

KAMEN: Yeah, a lot of different people, and the company that came in last was Jeff Lidon leading Abbott, and Jeff is something . . . I didn't remember I knew him, but I knew him. He had been a consultant for GI about something. I had forgotten that.

JONES: He was a business guy, right?

KAMEN: No, Jeff Lidon is a scientist. A little Abbott history. So Abbott, Jeff Lidon was the chairman of cardiology at the University of Chicago Medical School, although his research field was T cell transcription factors. He was a Jack Strominger postdoc at Harvard. Maybe I met him when he was at Harvard. I don't know. No, it couldn't have been, he's younger than I am. He had been added to the Abbott board in the late nineties; the pharm companies like to have a senior clinical person, and he'd **<T: 130 min>** been asked by Miles White who'd just become CEO of Abbott—maybe '98, '99 something like that—to do a board level review of R&D because they hadn't been productive. You know, the usual problem. Jeff had done that, and Miles liked the report a lot and insisted that Jeff join Abbott to execute his plans so he convinced him. In the meantime, Lidon had moved to a position at the Harvard School of Public Health. He was Ed Haber's successor with some famous chair at the medical school. Jeff took the plunge. He was forty, I think. He'd never run . . . I think he started and sold a small biotech company. Never run anything except his lab. He took over Abbott, but he wouldn't take the job to be the head of R&D. He would only take it if he had the division, so he went from professorship to running Abbott pharmaceutical division, and his first priority, his plan was significant in that position. They needed a product, and he asked the BD people to come up with a really good acquisition candidate, and it turned out that they had been outbid by J&J and by Centacor a year-and-a-half before to get Remicaid. The same guy who led that project knew about D2E7, so they told Jeff, "You've got to go out and get this German company." Jeff came in with a mission; they made the highest bid. He made it possible, so he was just fantastic because he'd just come into the job. He could do whatever he wanted, and he told . . . the deal with me is he said, "You're going to be wing fenced. Don't look at what we're going to do with the rest of BASF pharma. We can't keep it all." But he promised that the Worcester site would be doubled and that I would report to him. I wouldn't be integrated into their R&D hierarchy and that he would make what was called D2E7 at the time, a successful drug. It was wonderful. They just put everything on it. They just made such a big push and more resources.

JONES: Did they think it was going to be bigger than you thought it was going to be? I mean did anybody have an idea?

KAMEN: Yeah, I thought they thought it would be big. I mean we were predicting six hundred million in sales, something like that. When we . . . I stayed on I guess about a year-and-a-half until the BLA [biologics license application] was filed. They had really beefed up the whole effort. Jeff told everyone that Abbott is based on this drug being approved, and I don't want to take any chance. Everything has to be done exactly the right way. We need resources. Everybody in Chicago just jumped on board. People stopped what they were doing and they joined the team. We had 629 people on the project team when the BLA was filed, which is quite a big push. They had no knowledge in marketing, they built the whole thing from scratch. It's pretty impressive.

JONES: Yeah, that's an interesting story in itself, and you thought that at that time your job was done?

KAMEN: No, actually I liked working with Jeff a lot, and he was very inclusive. He brought me into his executive team. I was on the executive team that ran the Abbott pharma business—not just R&D—and it was really interesting. They expected me to run the Worcester site, and that was growing. We had hired a lot of people and worried about . . . most people don't know Humira was made for the first five years in Worcester. It's probably the most valuable single product ever made in Massachusetts if you look at the sales price of that, there was probably a billion dollars a year worth of Humira.

JONES: You had a great manufacturing site thanks to BASF.

KAMEN: Yeah, and this guy Peter Wester who was running it, who was just a gem. He, sort of, reported to me for a while, but I didn't have much impact. I would just listen to him and shake my head and say, "Yeah, you're probably doing the right thing." He by the way now runs manufacturing for BMS. He retired after thirty years at the age of fifty-six and switched companies. They just did a great job of launching it. Oh, I said why I retired, so the job was just undoable because I was getting involved in all the Abbott-wide things in Chicago, and Jeff really wanted me to participate and by phone if you're the only person on the phone and everybody knows each other, **<T: 135 min>** it just doesn't work. I was flying back and forth to Chicago, and before that for ten years I'd been flying to Germany, New Jersey, and doing a lot of traveling, and it was stressful. I decided I would hire somebody to run the Worcester site. I would stay based there, but I would hire someone under me. We did a search and realized there wasn't room in the job description for the quality of person I wanted and realized that the only way was to—

JONES: Get out of the way.

KAMEN: The only way was to get out of the way, and so I went and talked with Jeff and said, "Look, I really want to retire, and I promise I'll get somebody who is ten years younger and ten times better." He said, "Okay, you do that." We were lucky enough that I knew through old academic connections about this guy, Sandro Aruffo, at BASF. He was about to become head of discovery at BMS, and I never met him, but I knew his PhD daddy who thought he was the finest thing ever, and he'd gone directly from his PhD work into BMS and worked his way up, and he was about to become head of discovery at age thirty-eight or something like that. We ended up calling Sandro, and he indeed was moveable. He's now unfortunately gone. He did very well with Abbott and then got cancer and died very young. So you have the whole story.

JONES: Yeah, that's great. Thank you for taking the time. You've done a lot of interesting stuff since. Maybe another time we could just—

KAMEN: Yeah, it goes on.

JONES: Good for now and maybe we can talk again sometime.

KAMEN: Yeah, get an old guy talking, and you'll hear all the stories.

JONES: Well, these are important stories and fascinating too, and we need to get them from the source.

KAMEN: Yeah, and BASF did well with their divestment. People criticized them.

JONES: How much? What was the price?

KAMEN: 6.8 or 6.9 billion USD, which was a number BASF was happy with. In retrospect it's a joke. Humira will do 13.2 billion this year. I think there's a lot of German press that criticizes BASF for getting out, but we all know we couldn't have done it. It wouldn't be that drug, but then even more importantly, this year when I got my notice to attend the annual BASF shareholder's meeting—because of my one share BASF stock—I went online and looked to see the performance of BASF since the sale of their pharma division and it's quadrupled. They've outperformed the S&P by twofold. [laughter]

JONES: Yeah, they're doing fine.

KAMEN: How can you criticize them?

JONES: Right. Very good. Anything else?

KAMEN: You should have more than enough. I could talk forever.

JONES: Okay, thanks.

KAMEN: Salfeld and I would like to write a book about Humira at some point, but as long as he's still at AbbVie, we can't do that. The lawyers won't allow it.

JONES: How long is he planning to stay?

KAMEN: He might be there for a while, but he's very interested in history. He's written a history of his family, but there's so much litigation pending. The lawyers quite correctly they don't want anything more written. Every document has been in so many trials now.

JONES: Yeah, I think it would be doable. I mean, if it's been in trial, a lot of that stuff has to be publicly accessible.

KAMEN: Everything that is written down is public, but they would be worried about something else. I sympathize with them.

JONES: Yeah, there's ways to write those stories.

KAMEN: It's a dream. Someday it should be because I think most big drugs have a similar story. Any enterprise, it takes years. They have big teams, they get killed, they get born again, they have ups and downs, and what pharma management doesn't appreciate enough is that it's always key people who keep things going. As I said, this guy Salfeld would've. . .

JONES: Yeah, well, I think the way to write that book would be to get all those key people and interview all of them, and we could help with that. **<T: 140 min>** This is what we do. We can get them all—

KAMEN: Well, I'll talk to Marc about that. I just saw him a couple days ago. I think he's worried until there's still some big litigation undecided.

JONES: What are the issues?

KAMEN: Oh, currently, there's an ongoing suit between . . . it's AbbVie suing J&J for their Humira rip-off. They have a fully human anti-TNF successor to Remicaid, which literally infringes a Salfeld patent, and that's in final appeal.

JONES: Who's appealing? They're appealing?

KAMEN: I haven't followed the details. I'll know this week. I've got to be a witness in it, but I haven't followed the . . .

JONES: Antibodies are kind of tricky—right? —because they're so complex. Or is it . . . what's the patent on? Is it on the antibody? Is it a process?

KAMEN: No, the patent, the case law about how to interpret claims in antibody patents is evolving, and a lot of the case law is based on Humira-related litigation.

JONES: That's an interesting story, too.

KAMEN: Yeah, and what surprised me is that it's already been decided that the J&J product literally infringes . . . so the claim is a human antibody against human TNF alpha with an infinity of something or better, and the something is minimal, and the J&J drug is a human antibody against TNF, so it literally infringes, but I wasn't aware that there's been case law that narrows these claims so that you have to actually be demonstrate something more like small molecule patents. You have to demonstrate structural relatedness, so this case is about is there—

JONES: Different antigens or . . . ?

KAMEN: No, between the two antibodies, the two antibodies have to be structurally similar, which makes no scientific sense, so I'm not an expert on this, but in small molecule drug patents, the patent of course were always trying to limit the scope to allow other invention to the . . . you have to describe the molecules you're covering. And with antibodies they've never required that because any scientist knows that two antibodies don't really have to be too structurally related because you don't look at the structure when you're making it. Immunize a mouse or immunize an Abgenix mouse, and you look at something by functional criteria; you don't look at the structure. There's no inventiveness looking at the structure, but the courts are not looking at it that way at the moment.

JONES: That's interesting.

KAMEN: Well, I think it's going to turn out that biochemistry will win the day because at least for a small target like TNF where there's only a certain domain on the surface of TNF where a neutralizing antibody can work, so that puts structural constraints on the antibody, so even if the scientists making the antibodies haven't thought about the structure of the antibody, natural selection probably has dictated certain structural similarities.

JONES: Well, how well do they know the structure?

KAMEN: Well, they know the structure now, but that was done years after it was approved.

JONES: It's an interesting case.

KAMEN: Yeah. Lawyers find it interesting.

JONES: It's interesting because of the consequences, too. The way they decide and what it means going forward. Interesting for historians and policy people and so on.

KAMEN: So are you a science historian? Is that what you do?

JONES: Yeah, right, which is writing great stories, and what we try to do is improve public understanding. Tell the stories in a way that we can reach people. People should know about this stuff, and they're also just great stories.

KAMEN: Yeah. Oh, I left out one thing, one story, which maybe I shouldn't say, but I probably will. I mentioned . . . I did mention that my leaving GI wasn't exactly pleasant, and of course, if you're an R&D manager, **<T: 145 min>** you're always a little bit uncertain about whether you made a contribution because your job is empowering other people, and GI had a lot of great scientists. Like you guys published the Factor VIII story of GI, I wasn't mentioned.

JONES: That's right.

KAMEN: That was painful.

JONES: Oh, I'm sorry. Well, we hadn't talked to you.

KAMEN: Yeah, I know. Don't bother yourself over that, but those sort of things you, kind of, wonder it might be true because you talk to Jay . . . Jay Toole thinks he didn't need any managing.

JONES: Well, yeah . . .

KAMEN: Anyway, you don't have to revisit that, but objectively as a scientist, you only know if something is true if you can reproduce it, and when I was a graduate student, I had the great privilege of being hired as an extra on the filming of the Steve McQueen version of the *Thomas Crown Affair*. Did you ever see that film?

JONES: Yeah.

KAMEN: It's a great Steve McQueen, Faye Dunaway film. I spent three weekends making a little money being an extra and hanging out with Steve McQueen occasionally, and then I saw the movie, which I couldn't tell if you're an extra what they're making and at the beginning of the film, there's a golf scene and the McQueen character sinks a very, very long putt, and the guy he's playing against is pissed off and hands over a big pile of cash. They were clearly playing for a lot of money, and McQueen looks at him and this guy is very upset and McQueen takes the money, gives it back to him and says, "I'll do it again." I've always thought about that, so BASF Bioresearch was my second putt because even then I know Jochen Salfeld built Humira, and it's hard to say not in the patent, you don't really know what you're doing as a . . . maybe this is egotistical to talk about.

JONES: No, I don't think so.

KAMEN: Having a chance to do another GI from scratch up to exit, that's why I feel good about that. I feel that I may actually have . . . significant probability I contributed.

JONES: Yeah of course, and these are important things and think of what's the counterfactual. If you hadn't been there doing X, Y and Z along the way, would somebody else have done it? Maybe, but . . .

KAMEN: Of course, you don't know. You don't know.

JONES: Yeah, but those things had to be done, and you did them. Now I'm very sorry that we left you out of the Factor VIII story.

KAMEN: Well, I'm sure it had to do with the people you talked to.

JONES: Well, yeah, I mean, we were just . . . we didn't really talk about a management angle, and I forget the story precisely but we talked about the—

KAMEN: Well, I know the people that did [inaudible]; it was factually correct, but there was a puppeteer behind that.

JONES: Yeah, right. Well, we didn't have that role in there. That's why it's important to talk to everybody, and we're trying to do it and that's why I say if you write the Humira book, identify the people—as many of the key people as you can. We're trying to do it.

KAMEN: Sure. No, I'm not criticizing you about that story, but it was good otherwise.

JONES: Oh, thanks.

KAMEN: I think it's a wonderful thing that you're doing this.

JONES: It's fun too.

KAMEN: Before, I guess, people die.

JONES: Yeah, thanks so much.

KAMEN: Are you going to do one about George . . . ? Have you done one about George Rathman?

JONES: We've got oral histories. We didn't do an oral history with George, but Sally Smith Hughes at Berkeley—

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]