SCIENCE HISTORY INSTITUTE

HELEN DONIS-KELLER

Life Sciences Foundation

Transcript of an Interview
Conducted by

Mark Jones

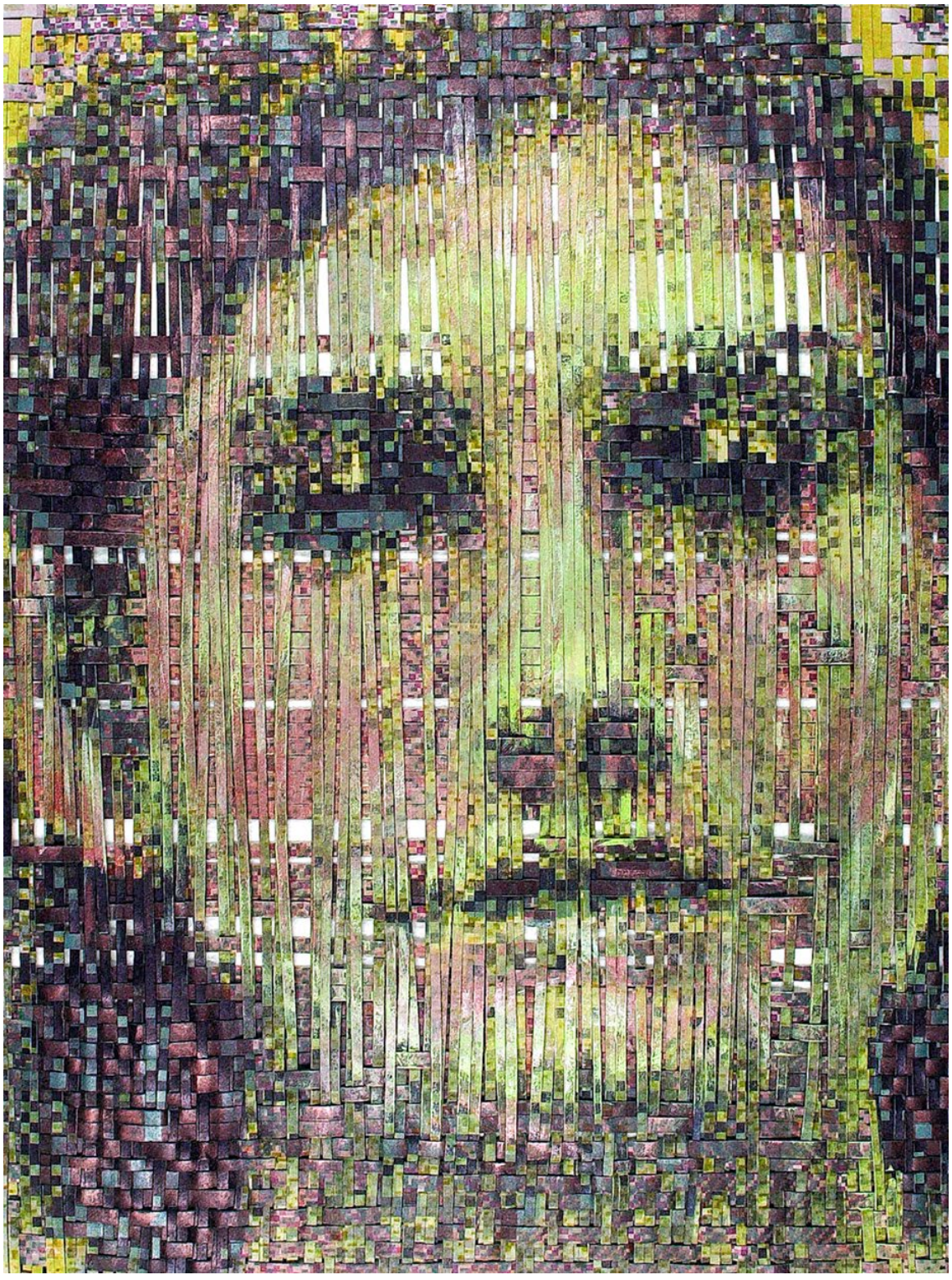
at

Cambridge, Massachusetts

on

27 May 2013

(With Subsequent Corrections and Additions)



Helen Donis-Keller

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HELEN DONIS-KELLER

1947 Born in Madison, Wisconsin, on 27 March

Education

1969 Major in Graphic Design, University of Cincinnati, School of Design, Architecture and Art
1973 BS, Lakehead University, Natural Science (First Class Standing)
1975 Honours BS, Lakehead University, Natural Science in Biology (First Class Standing)
1979 PhD, Harvard University, Biochemistry and Molecular Biology
1995 Doctor of Science Degree (*Honoris Causa*), Lakehead University
2001 Master of Fine Arts, School of the Museum of Fine Arts and Tufts University, Studio Art

Professional Experience

Biogen, Inc.
1980-1982 Assistant Research Director of Molecular Biology

Collaborative Research, Inc.
1983-1989 Director, Department of Human Genetics

Washington University School of Medicine
1989-1992 Professor of Genetics, Department of Genetics
1989-2001 Joint appointment, Professor of Genetics in Psychiatry, Department of Psychiatry
1992-2001 Joint appointment, Professor of Genetics, Department of Genetics
1992-2001 Professor of Surgery and Director, Division of Human Molecular Genetics, Department of Surgery

SmithKline Beecham Pharmaceuticals
1994-1997 Genomics Advisory Board Member

Olin College of Engineering
2001-present Professor of Biology and Art
2012-present Michael E. Moody Professor

Honors

1973	Lieutenant Governor's Medal (Canada), Lakehead University
1975	Dean of Science Medal, Lakehead University
1979	Helen Hay Whitney Postdoctoral Fellowship
1994	Marion Spencer Fay National Board Award of the Medical College of Pennsylvania, 32nd Annual Award
1995	Doctor of Science Degree (*Honoris Causa*), Lakehead University

ABSTRACT

Helen Donis-Keller was born in Madison, Wisconsin, but grew up in Elkhart, Indiana. As a child, she loved art, so she pursued a degree in graphic design and photography at the University of Cincinnati. She spent two work sections at Hallmark Cards in Kansas City, Missouri, designing stationery and gift wrap. In 1970 Donis-Keller and her then husband moved to Canada where they stayed for five years for her husband to avoid being drafted during the Vietnam War. She got a job as a graphic designer at Lakehead University and started taking college classes, including chemistry and biology, for fun. She became captivated by science and quit her job to become a technician doing histology. Donis-Keller soon realized that if she wanted to work on her own projects, she needed to have a PhD to do so. She applied to several programs and selected Harvard University after interviewing with Mark Ptashne and Walter "Wally" Gilbert. At Harvard, Donis-Keller worked in Gilbert's lab on the RNA sequencing method using an enzyme approach. Upon graduation, she briefly worked with Bernard N. Fields on reovirus before accepting an offer at Wally's new company, Biogen, Inc. As the company started to grow, they made a lot of proposals. Two of Biogen's projects were interferon and human serum albumin. After a couple years at Biogen, Donis-Keller thought she needed a break. She was encouraged to join Collaborative Research, which she did. There she worked on project development, including developing markers and creating a human linkage map. By 1985, they had started collaborating with Lap-Chee Tsui and Ray White, who both had cystic fibrosis families. In August 1985, they discovered a LOD score of 3.22—very high—on one marker. They continued to develop more markers and hired Philip Palmer Green to develop the algorithms they needed. Donis-Keller started working on a paper that they submitted to *Cell* in August 1987. However, after the results came out and the fanfare died down, the project was cancelled. She accepted a job at Washington University in St. Louis, Missouri, where she continued to work on genetic mapping. During a sabbatical, she took some art classes and decided when the sabbatical was over, she wanted to get a master's in fine arts. She closed down her lab and came back to Boston, Massachusetts, to move in with her new husband, Boris Magasanik, and attend the School of the Museum of Fine Arts. After graduation, Donis-Keller joined the faculty of Olin College of Engineering as a professor of biology and art. She talks about her artwork, including photography of Iceland and Death Valley, her teaching, and the importance of acknowledging women in science.

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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TABLE OF CONTENTS

INTERVIEWEE: **Helen Donis-Keller**

INTERVIEWER: **Mark Jones**

LOCATION: **Cambridge, Massachusetts**

DATE: **27 May 2013**

JONES: Okay. Well, what we'd like to do is really record a biography. So, maybe we could start at the beginning. Tell me a little bit about your background, your family, your education growing up.

DONIS-KELLER: Gosh, that's going way back.

JONES: Yeah.

DONIS-KELLER: Okay. Well, I'll tell you one thing. I . . . last night about nine o'clock, I went down to the basement and tried to dig out my files and things because it has been a long time, so I'm not sure that my memory is correct on a lot of these things. But I just started reviewing and sorting things into piles, so anyway . . . but I haven't been through it all, so I may need to correct things.

So I was born in Madison, Wisconsin, and my father [Jack A. Donis] was in World War II, and he met my mother [Maureen White] in England, and she came over as a war bride. And then when I was ten years old, we moved to Elkhart, Indiana, where I lived—

JONES: Oh, I grew up in South Bend, [Indiana].

DONIS-KELLER: No kidding? My sister [Annette Douberteen] lives there now.

JONES: Is that right?

DONIS-KELLER: Yeah.

JONES: South Bend's in bad shape.

DONIS-KELLER: Juday Lake Drive.

JONES: Oh, I used to live next . . . that's in Clay Township.

DONIS-KELLER: Yeah.

JONES: That's my stomping grounds.

DONIS-KELLER: Oh my gosh. Really? Oh, that's so funny.

JONES: That's where I grew up, yeah.

DONIS-KELLER: No kidding. South Bend's in bad shape. Why?

JONES: I think it's turning—

DONIS-KELLER: Just economically?

JONES: Yeah, it's turning into Gary, [Indiana].

DONIS-KELLER: Oh no.

JONES: Yeah.

DONIS-KELLER: Wow.

JONES: I mean, well, there's part of the city that's fine.

DONIS-KELLER: In Granger, [Indiana], yeah.

JONES: Granger's fine, and then any around Notre Dame University—Notre Dame—is fine—

DONIS-KELLER: That's where my sister lives, yeah.

JONES: But on the entire other side of town is just . . . yeah.

DONIS-KELLER: Yeah, that's really too bad. Yeah, Elkhart's had similar difficulties because of the . . .

JONES: Yeah, that's right. Yeah.

DONIS-KELLER: You know, the trailer industry has left and a lot of unemployment and . . . yeah, it's tough. But when I was a child growing up there, it was an up-and-coming, sort of, light industrial area. And it was the kind of place where people really didn't lock their doors at night. It was a very safe environment, and kids went out on their bicycles, and you'd leave in the morning and come home at night. You know? So, it was not a bad place to grow up.

I was always interested in art as a child. I can remember in Wisconsin drawing and just being interested in art in general. I really wanted to be a fine artist, but of course that wasn't exactly accepted by my father, the banker. [laughter] So, I had to choose something that he thought I could make a living at. And so, I went into graphic design and photography and went to the University of Cincinnati in [Cincinnati], Ohio, and was there for four years. And I just, I just loved it. I really liked learning and I loved . . . I was in the School of Design Architecture and Art, which is a pretty interesting place to be [as it had a co-op program that placed students in work sections (paid internships) during their education]. And that was 1965 I graduated from high school.

JONES: Right. It was very different back then. Everything . . . all graphic design now is done on computers.

DONIS-KELLER: Yeah. Oh, this was all by hand.

JONES: So this is different technology.

DONIS-KELLER: Oh, yeah. Everything by hand. Mechanical drawing. Setting type using press type and things and yeah.

JONES: I guess actually it was a real disruption to that business, to that industry when the computers came along and changed everything, all those skills that people had developed.

DONIS-KELLER: People had to adapt or retire or something. So . . . but I missed that. I missed all of that. So I went . . . I was in a five-year program. It was a work-study program and my first work section was actually at Hallmark Cards in Kansas City, [Missouri]. And I stayed there for six months [two work sections back-to-back], and I designed stationery and gift wrap. And everything I designed got produced, which was pretty exciting. It's nice to see your stuff advertised in magazines and things like that. I enjoyed it. I enjoyed the people; I liked the atmosphere. It was very . . . actually a very creative atmosphere and lots of good materials and tools to work with and people to mentor you. So . . . but I quickly realized that I would not be going back there for a full-time job, that it . . . there's just not enough to be interesting. So, I did that.

Another work section, I taught at a summer camp and I made—I think—probably twelve pinhole cameras, and I taught everyone in the camp who was interested—little kids from five years old up to high school kids—how to use a box camera. And we went in the dark room and developed our little pinhole negatives and printed . . . And that was . . . I had never been to summer camp before, so I just wanted to know what was that experience like. So, that was another work section.

At any rate, this is in the late sixties. And my boyfriend [David Keller]—who then, we married—he was in architecture first and then he switched to industrial design. And the Vietnam War came along, and we were so against it. So against it. He got his draft notice in '69, and we talked it over. He applied for conscientious objector status, but this was Indianapolis, [Indiana], and so **<T: 05 min>** they weren't having any of it. So, we went to Canada. We left the country, which was . . . it was really very hard to do. Well, we had . . . my daughter Christine was born in February 10 of 1969. And we just decided we had to leave.

So, we put all our stuff together, and we went to Thunder Bay, Ontario. Through, sort of, underground helping groups we learned about Thunder Bay and that there were some people there who might help us make the transition. And I applied at—there was a little school, Lakehead University—I applied to be their graphic . . . a graphic designer there. And I was hired there, and there were some really kindhearted people in Thunder Bay who helped us get settled. We had to get landed immigrant status and all of that. My husband had a hard time finding work, and he did a variety of jobs before he got hired by Canadian Car to work on designing subway cars for some various places in the world.

So we lived there for five years. This was from 1970 to 1975. I didn't finish my degree at Cincinnati. It was a five-year program, and I lacked two semesters, I think, to finish. So I

started taking courses at Lakehead. If you were an employee there, you could take courses essentially for free, and there were lots of areas I'd never had a chance to study, so I just began, sort of, randomly taking courses in night school and summer session and that allowed as part-time for my job. I got very interested in chemistry first—synthetic organic chemistry I just loved—and biology. And I had never taken any science before. Even in high school the only science I took was physics in summer school. I mean, I just . . . the thought of dissecting an animal was something I couldn't live with.

So, I really had no experience of science. But I took one of those, you know, science of soap bubbles and things like that for everyday . . . science for everyday people or something like that. Liberal arts, liberal science course. And I was just . . . it was like a whole different world opened up to me. It was just this world I never knew about that was so interesting. So I just became very interested in molecules and chemistry and how you make them and break them and then that transitioned into biology because biology seemed even more interesting than chemistry. So I got an—

JONES: And this is difficult work. I mean, if you're—

DONIS-KELLER: This is—I know.

JONES: Learning about chemistry, you really have to apply yourself to—

DONIS-KELLER: Yeah. But, you know, if you love something, you want to know everything about it. But it was definitely hard work. I was young then; I could stay up late. And it was just so interesting. And the professors were really very helpful. They were really helpful for a student who was interested, and they really were dedicated teachers, so that made a big difference as well.

So, doing work part-time, I had enough credits for an undergraduate degree in natural sciences. But that wasn't really enough: I wanted to go on and get more. And so, I quit my job and got a part-time job being a technician doing histology and just, sort of, different things that the professors would want done off their grants or, you know, whatever.

JONES: Did you have an idea at that point for a career path in science?

DONIS-KELLER: No, I just loved it, and I wanted to do it. And, at first I thought, well, being a technician would be, you know, a solid thing to do. But the more I did it, the more I realized that if you really want to go on and be able to choose your own projects and do your own work, you really needed to have a PhD.

And so, I got some wise counsel from one of the professors [Alistair MacDonald] who became a good friend. He wasn't that much older than us. He was a biology—botanist—a biology professor. And he really gave me some good advice about applying to PhD programs. So, I applied to a number of them in Canada, and then almost as . . . just almost like a dare, I applied to Harvard [University] and to MIT [Massachusetts Institute of Technology]. What the heck? You know? [laughter] Might as well. I have no . . . I just did.

And by this time I was very close to having an honors bachelor of science from Lakehead. And so, my father actually told me, "It **<T: 10 min>** would be really good if you went down and talked to the people at Harvard and had an interview." Which, you know, I was just clueless. I had no idea how does one go about graduate work. So, I came down, and I had interviews with Mark Ptashne and with Wally [Walter] Gilbert and with Rich [Richard] Losick. and I just loved the environment, what it was like to be a graduate student. At that time, I didn't know that they would actually pay you, that you would get a stipend. I thought I would have to raise the money myself, so that was really a surprise to learn that, no, they have these fellowships and things, and they'll pay for you.

JONES: Were, were you fully aware of the stature of these guys and the, you know, the world class . . . ?

DONIS-KELLER: I was, sort of, dimly aware. I could tell by talking to them they were very smart people, and this was Harvard, and I knew Harvard was a great place. But it was very interesting when I interviewed with Mark Ptashne, he told me flat out, "You'll never make it here." [laughter] "You'll never make it here." And his reason was that I had a child, and nobody in those days had children. And it was . . . I'll never forget the interview with him. Wally was much more friendly and told me about the work that was going on in the lab, and it just sounded incredible. This was before the DNA sequencing happened . . .

JONES: What . . . yeah, what year is this? This is . . . ?

DONIS-KELLER: This is 1974. Yeah. So, the DNA sequencing method didn't actually come out until 1976, I think. And that was just one of the projects they were working on there. And it was just absolutely fascinating. So, I really wanted to go to Harvard.

So, I came back to Canada. And I was working in the herbarium filing plants one day, and I got this call, and it was Wally on the phone, and he said I'd been accepted. And, you know, and then I was just, like, silent. And he said, "Are you there?" I was so excited I just had . . . was speechless. So I had been accepted, and I still have the letter of acceptance and what the stipend is and everything.

JONES: Which was probably a—

DONIS-KELLER: Like three hundred dollars. [laughter]

JONES: Yeah.

DONIS-KELLER: It was not much. So that. And I did get into MIT as well, but somehow just the size of MIT, I didn't go down to visit them when I came although I should have. And in retrospect, I would have gotten a wonderful education there. My husband Boris [Magasanik] is a professor at MIT in the biology department for many years. So that was '75. And so you had to take a . . . it wasn't an entrance exam but it was, sort of, like . . . it wasn't really even a placement exam. It was just some sort of exam to see where you were, and I had to come down to do that right before school started. And of course, I had no self-confidence whatsoever, and I tried—

JONES: But you were determined to—

DONIS-KELLER: I was going to do it. Yeah, I was going to do my best. So, and they sent the little instructions and you were supposed to 1) have a language and 2) know how to use a slide rule. I didn't have either one of those things because I'd never used a slide rule. I just didn't because I didn't have any serious math in high school and all that sort of thing.

Anyway, I got there, and the language requirement was lifted. I tried to learn French in the summer, but it was pretty hopeless. And then we didn't need to use a slide rule. They told us when we got to the test that you could use a calculator, but I had taught myself how to use a slide rule anyway. So it was clear when I took that test that I didn't really know much about molecular biology at all. I mean, it wasn't taught at Lakehead. I knew a fair amount of biochemistry and chemistry.

JONES: Well, that's, that's a good background to have.

DONIS-KELLER: Oh, yeah. It was very solid in those areas. So I just then became a graduate student and did rotations. Jack Strominger's lab was one, and then down at the Jimmy Fund—that's where one of Jack Strominger's people worked, so I was down there. And then, in Rich Losick's lab and in Wally's lab. And I talked to Fotis Kafatos because I was really interested in development, but the tools just weren't there in 1975, so I decided not to do that. I really wanted to be in Wally's lab. It just seemed . . . you know, there was chemistry going on, and there was

some just really interesting biology too. So, I asked to be in Wally's lab and became a graduate student in that lab. **<T: 15 min>**

And we all had, all rotating students had to sequence a little part of the lac operon. And in those days, if you got forty bases clear, you were . . . you thought you were golden, you know? So, I did my forty or so bases. The project I worked on in Wally's lab, the DNA sequencing method was just coming out and a lot of people were coming to visit, and I . . .

JONES: Do you remember who came through?

DONIS-KELLER: Oh, Susumu Tonegawa came. Oh gosh. Lots of different people. I have to think about the other ones who came. But Susumu, I really liked. He was fun.

JONES: So, that was the diffusion of the technology was face-to-face, people coming in, hands-on—

DONIS-KELLER: Oh, yeah. And Allan Maxim was Wally's, sort of, right-hand person and would always work with whoever the visitor was. And my bench was right across from Allan, so I always got to overhear what was going on and what was being sequenced and . . .

JONES: And what was the talk about? Now we can do this, was it talking about "Okay, we can do X, Y, and Z and . . ."

DONIS-KELLER: Yeah, "we can sequence these genes." And in fact, that was Susumu's interrupted genes for immunoglobulins was discovered there because of the sequencing, realizing that things, you know, weren't contiguous. So, that was pretty exciting. And I know a Russian postdoc, Kostya Scriabin, came.[1] He became, sort of, a big shot in Russian science although when he came we were sure he was in with the KGB [Komitet Gosudarstvennoy Bezopasnosti, Committee for State Security]. [laughter] So . . .

JONES: He was suspicious?

DONIS-KELLER: He used to go to New York to party a lot, and he didn't seem very interested in science, so . . .

[1] To the best of the interviewee's knowledge, this name is correct.

JONES: Really?

DONIS-KELLER: Yeah, no, he was a pretty interesting character. Gosh, lots of other people came. I'll have to think about who the others were. So I was set to work on developing an RNA sequencing method based on the same principles as the DNA sequencing method.

JONES: This was going to be your thesis project or . . . ?

DONIS-KELLER: Yeah, it was a project to start on. You, sort of, had to do some work and then, kind of, think about what the thesis would be. So, it was really my initial project, and Wally really wanted me to work on the chemical method, and that's . . . I tried that, you know, it was like beating my head against the wall for months. It just didn't work well. The gels were very blurry . . .

JONES: With RNA?

DONIS-KELLER: Yeah. The RNA would degrade. Just—

JONES: What was the problem? Just the chemicals? Did you try different chemicals to work with?

DONIS-KELLER: Oh, yeah. You name it, I tried it. And this was under the tutelage of Allan Maxim. And the gels were just very blurry because we were just getting a lot of fragmentation and a lack of specificity. But one way to get a nice ladder of the gel was just to hydrolyze, and base RNA very nicely gave you every single base all the way up the line. So I then wanted to turn to enzymatic methods because there were enzymes that cleaved RNAs, specifically after certain bases. And so . . .

JONES: When did the Sanger method show up?[2]

[2] The Sanger method—also known as Sanger sequencing or chain termination sequencing—is a method of sequencing the genetic code developed by Frederick Sanger and others in 1977. See Armando Totomoch-Serra, Manlio F. Marquez, and David E. Cervantes-Barragán, "Sanger Sequencing as a First-Line Approach for Molecular Diagnosis of Andersen-Tawil Syndrome," *F1000Research*. Accessed at https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5635448/ on 29 November 2021.

DONIS-KELLER: Oh, we were in . . . the Sanger—what?—DNA sequencing?

JONES: Yeah, yeah.

DONIS-KELLER: About the same time. And Fred [Frederick] Sanger came to visit several times.

JONES: And showed it to you or . . . ?

DONIS-KELLER: Yeah, he came to talk to everybody in the lab. And he had a colleague, associate, George [G.] Brownlee, who was also working on RNA sequencing, but we got there first. And we used T1 [that cleaved after G and U2 that cleaved after A but we didn't have specificity for C or U. Later I found an enzyme from B. cereus that cleaved after C and U and then phy-M, an enzyme that I worked up that cleaved after C and U, so then we could distinguish all four bases with these four enzymes]. And, anyway, so we worked out a way to cleave specifically after certain bases so that you could really read the RNA sequence up, and with end-labelling [with P^{32}] and separating [the fragments according to size] via gel electrophoresis [on acrylamide gels].

JONES: So, the enzyme approach worked much better?

DONIS-KELLER: Oh, the gels were so clean. They were just beautiful. Yeah, the first one was T1, and we knew right away that was it, that was going to work.

JONES: Did anybody ever work up a chemical?

DONIS-KELLER: Yeah, Debbie Peattie in Wally's lab then took on the project [later], but, you know, Wally just really wanted me to push the chemical method, and I waited, actually, until he went away on a trip and then used the enzymes. T1 was the first one. And when I showed him the gel when he got back it was like, oh, all is forgiven, you know? [laughter] "To heck with the chemical stuff. Just keep going with the enzymes."

JONES: So, what were discussions like about the two methods—the Sanger method and then working with enzymes and the chemical method—were . . . people are evaluating and . . . ?

DONIS-KELLER: Yeah, there wasn't too much as far as people in Wally's lab thinking about using the Sanger method. It wasn't as if **<T: 20 min>** "We have this; we have that." But we just really pushed the chemical method. That's what everyone thought at . . . in those days was superior. And the chemical method was fine as long as you didn't have to scale it up. And it worked very well. The gels were beautifully clean, and there was a lot of work in trying to get longer reads and different types of gels. They really perfected the cleavage reactions. They were really very nice. And the RNA sequencing method, our reads were pretty good too.

So we wrote that up as a method, and I presented it in '77 at the Nucleic Acids Gordon Conference, and people got excited about it. And it was used by a lot of people because in those days you didn't have the DNA equivalent of a lot of RNAs and so people wanted to sequence RNAs. And there were methods. Argiris Efstratiadis downstairs in Fotis Kafatos's lab—he was a postdoc—had worked out a way to decap messenger RNA. So, we did, like, the decapitation and then labelling, and then you could sequence messages too. So, we worked out things like that too.

JONES: So, this is about three years into your graduate training, and by this time you're feeling pretty confident? You . . . ?

DONIS-KELLER: Well, the method was working really well. And I had some collaborations and I worked on some viruses. I collaborated with Nancy Hopkins down at MIT in determining tropism with some viruses that she was working on—murine leukemia viruses, I think. And then a lot of people wanted the protocols for the methods, and sometimes I would go places and show people how to do it and take my little, you know, reagents along with me. So I went to [Robert Haselkorn's lab at the University of] Chicago and showed some people there.

JONES: Did you have to take radioactive stuff along?

DONIS-KELLER: No, never. They always had label there. That would have been pretty tough on the plane.

JONES: Probably.

DONIS-KELLER: Yeah, but I'd take my protocols and my phy-M, the enzyme that I worked up myself because it wasn't commercially available. So the method worked fine. And I did some virus sequencing [and worked out a method for site-specific RNA cleavage using oligonucleotide hybridization], and that was essentially my thesis. So I got that done.

Then after that it was just . . . by this time, oh, my husband had found someone else and I had my daughter with me, so I was a single parent, looking after her. We lived in Harvard housing, which made things very easy. And then I met David Botstein around the time I was just finishing up my work in Wally's lab. And so, there was incentive to stay in the area, and my daughter loved her school, and so I didn't think about postdoc'ing, you know, in California or anything, which I probably should have.

But I went down to Cold Spring Harbor, [New York], and looked there, and it . . . nothing really clicked there, but I got very interested in a reovirus. Bernie [Bernard N.] Fields down at the Harvard Medical School had been working on reovirus for a long time—it's a double-stranded RNA virus. And so, I thought, "Well, I'll clone that and sequence it," which was pretty much a disaster. It was impossible. I worked for a couple of years just trying to get anything cloned, and it just didn't work. There was probably a lot of secondary structure. There was probably a lack of homogeneity. You know, there were probably all kinds of problems that I was just too naïve to even think about, and I didn't like the environment in Bernie's lab either. It wasn't as intense at all.

JONES: So, it was very intense in Gilbert's?

DONIS-KELLER: Oh, yeah, we lived and breathed science. We couldn't wait for *Nature* or *Science* to come out. Somebody would run down to the mailbox and get it first thing in the morning.

JONES: And you're looking for what? To see if any competitors . . .

DONIS-KELLER: Oh, yeah.

JONES: Have scooped you. And who were the competitors that were on the radar screen?

DONIS-KELLER: Oh, well, there were . . . I guess [Paul] Berg's lab in California. We always had this East Coast/West Coast thing going on.

JONES: There was talk about that?

DONIS-KELLER: Yeah. Oh, yeah.

JONES: And was it . . . it was Stanford [University] specifically?

DONIS-KELLER: Stanford, yeah. Oh, gosh. And we even had our own little newspaper called *The Midnight Hustler*, and we would put this thing together at, like, a Friday night or something and distribute it in the lab, and there were lots of comments about the Stanford people.

JONES: Are any of those things still around?

DONIS-KELLER: I still have some.

JONES: Those would be great.

DONIS-KELLER: Yeah, I'll dig them out.

JONES: Can we copy them?

DONIS-KELLER: I know I have them. Yeah, they were just ridiculous. Yeah, I'll dig those up.

JONES: Okay. Great. Thank you. **<T: 25 min>**

DONIS-KELLER: I came across them a few years ago because there was a big celebration for Wally's [birthday], I guess, at Cold Spring Harbor [in 2005], and I dug some up for . . . and we talked about them then.

JONES: Did . . . you got an invitation for the Biogen thing, right? The Biogen event?

DONIS-KELLER: No, what was that?

JONES: Oh, no. You need . . . you didn't get an invitation?

DONIS-KELLER: No. What . . . ?

JONES: I'm sorry. We're having an . . . well, there's . . . I guess it's not Biogen necessarily, but yeah, we are planning a Biogen thing, I think, but this is not . . . On June 4, we're having an event here. I guess it's just generally Boston biotech and there's a panel . . .

DONIS-KELLER: Oh, I saw it on your website. Yeah, no, I didn't hear anything about it. The first I saw of it was this morning.

JONES: Oh, I'm sorry. We need . . . [we've] been remiss. I will have to make sure that, you know, all of that news comes to you.

DONIS-KELLER: Oh, good.

JONES: But you're welcome to come. So, there will be a reception and, you know, Wally Gilbert's going to be there.

DONIS-KELLER: Oh, you're kidding. I haven't seen Wally in a long time.

JONES: It might be a good chance to . . .

DONIS-KELLER: Yeah. So, the fourth is . . . what day is the fourth?

JONES: It's Tuesday. Next Tuesday.

DONIS-KELLER: Tuesday. So, it's in the evening?

JONES: Yeah, early. Yeah, I'm not sure exactly what time. It might be four to six p.m. or something like that. I'm not sure.

DONIS-KELLER: Okay. I could potentially do it; I just have to get some coverage.

JONES: I'm sorry that you didn't know about it.

DONIS-KELLER: Oh, no, not at all. I . . . well, you know, I'm always busy, never looking to see what else is going on in the world. Anyways, so, I really got nowhere with that project. It was just very discouraging. I did some, a little bit of work with collaboration at Yale [University] with Sid [Sidney] Altman cloning out—oh gosh, I don't even remember now. He was working on RNAse P, I think, [and I cloned out the RNA subunit M1 RNA of RNAse P and sequenced it]. Anyway, that was my only slight success.

And so I was looking around for something to do, and Wally had just become affiliated with Biogen—or founded it, I guess. And they were . . . they had been going for a while, and they wanted to have a Cambridge laboratory.

JONES: Had you heard about it previously?

DONIS-KELLER: I had heard about it because my really good friend Lorraine Johnsrud had gone over to [Charles] Weissmann's lab [in Basel Switzerland], and I think she was a Biogen employee or paid, and in Geneva, [Switzerland], [was the headquarters]. It was just getting started. So, I was just sort of vaguely aware of the Biogen over there.

JONES: What were the general impressions? Because this is a brand new thing—molecular biology—you mean you can make a business out of this, right? What were the general impressions?

DONIS-KELLER: I think a lot of people were skeptical that it would come to anything. I mean, Genentech seemed to be doing well, although it just wasn't clear how things would turn out. And in fact, the academics—many academic people—were very opposed to biotechnology. And I can remember . . .

JONES: Opposed to commercializing it?

DONIS-KELLER: And people—academic people—working there. There were a lot of people opposed to commercializing it, number one. It would become secretive, people wouldn't publish, all that sort of thing. But also that only second-rate people worked in companies. That was the academic prejudice. I can remember Dieter Söll, who was an editor at *Nucleic Acids Research*, at one of these Gordon conferences, he just spouted off, you know, "Only second-rate people join those companies." And at the time I just didn't know what that meant, you know? I just remember him saying that.

JONES: Well, you know, what does it mean when Wally Gilbert and Phil [Phillip A.] Sharp and Charles Weissmann and . . . they start a company?[3] What does . . . how does that affect the perception?

DONIS-KELLER: Well, yeah, I mean, that's it. Here are first-rate people who are looking at this interesting venture. And I think there was a fair amount of jealousy by other academics as well, but it wasn't clear that they could hire young people to work for them in these companies and that they could make any money. That any of this stuff could make any money. So it was very uncertain. At any rate, I was—I don't know—I guess I've just always been, kind of, a risk-taker in a small way, so I signed on at Biogen. I think I was the third employee hired for Biogen Cambridge.

JONES: What did . . . that year was . . . ?

DONIS-KELLER: That would have been [1980].

JONES: Well, by this time all the, the insulin work is probably . . . I don't know if Wally Gilbert, he's probably still continuing it? I don't know. But do you recall when the insulin work started? **<T: 30 min>**

DONIS-KELLER: Oh, yeah. Oh, yeah. That was a big humiliation, wasn't it? Yeah, they went over to—oh, where was it?—in England to do the cloning—

JONES: You were still in the lab?

DONIS-KELLER: I was in the lab then, yeah. This must have been about '77 or '78. Yeah, yeah. They lost out on that one bigtime. That was sad.

JONES: Were there great expectations that we can clone insulin and this is going to be . . . ?

[3] Phillip A. Sharp, interview by David C. Brock, Arnold Thackray, and Mona Ashiya, Massachusetts Institute of Technology, Cambridge, Massachusetts, 28 January, 29 May, and 20 November 2003 (Philadelphia: Science History Institute, Oral History Transcript # 0268).

DONIS-KELLER: Oh, absolutely. Oh, yeah. Yeah, I remember . . . I mean, Wally had this supreme self-confidence in anything he undertook. It was amazing. And so many things just didn't work at all, but . . .yeah, I don't know if Lorraine was . . . Lorraine Johnsrud was involved in some of that work too. I'll have to go back and rack my brain about that, think about that some more—those years. [Lydia Villa-Komaroff was a postdoc with Wally and was one person who worked on it.] But yeah, so they had Bob [Robert] Fildes was the president; Mark Skaletsky was the vice president. Rick Eustis was the lawyer and Devon Giacalone was like a marketing person.

JONES: So, this . . . and this is at this point the company can sit around this table?

DONIS-KELLER: Oh . . .

JONES: Almost.

DONIS-KELLER: Oh, yeah. I mean, there were only like four of—four or five—of us in the very beginning. And we rented space over a bakery in Harvard Square and, you know, the smell of croissants would just waft up. [laughter] It was sheer torture. Yeah, we just had offices. And then, we renovated a little building, which is now gone, down in Kendall Square there, and that was the first Biogen building. And then there were more scientific advisors that joined, and us inside people used to call it "the gang of thirteen" because they would, like, call the shots. And they had . . . it was competitive with them too because they had their own research groups and then there was the Biogen scientists, and so we felt like we were always competing with them for resources.

And then, Julian Davies became the research director over in Geneva, and we would go over there once a year to meet and talk about the work, and that was always very interesting and nice. But we didn't have a director for years. They had trouble, I think, attracting anyone for the same reasons: the bias against biotech and the academic life was seen as far superior to working for a company. And then they finally did hire—oh, gosh, what was his name? I didn't care for him at all. Do you remember the first director at Biogen? I'll have to go back and . . .

JONES: The first . . . ?

DONIS-KELLER: Scientific director, [VP for Research].

JONES: I'm not sure.

DONIS-KELLER: Anyway, it'll come to me. But he came with his own research group, and he spent all of his time—as far as we could see—with his research group and very little with us. And we were all fairly junior scientists, and, you know, we needed help and advice. And it wasn't clear what we should work on. We worked on interferon, human serum albumin. We made a lot of proposals. We went out to Monsanto [Company]. A lot of things were just trying to find our way.

JONES: Yeah, and Monsanto, were these pharmaceutical projects then? Or was it . . . ?

DONIS-KELLER: Yeah, well, we were pitching things to them. I don't know that they ever actually invested in us. I'm not sure. I don't think so.

JONES: Yeah, I can track that down.

DONIS-KELLER: Yeah, maybe they were interested in the interferon work. I'm not . . . I don't remember now. But we made lots of presentations to Japanese groups. They actually sponsored some of the human serum albumin work, so . . .

JONES: Weissmann cloned alpha? Is that . . . ?

DONIS-KELLER: I don't remember.

JONES: But you were working on . . . so you had separate . . . it sounds like it's, you know, there's different wings and you don't really know, you're not working together that closely. Once a year, maybe?

DONIS-KELLER: Yeah, we'd . . . and then they would come for various meetings and things. But there wasn't a lot of contact back and forth. There was, you know, probably more competition than anything.

JONES: Well, retrospectively, what do you make of that form of organization? Did it work or was it . . . I mean . . . ?

DONIS-KELLER: I think most people agreed that it didn't work because the scientific advisors had divided loyalties, that having taken money from the company to do research inside their labs and then trying to be supportive of the people inside the company generally wasn't . . . it wasn't a very good formula. And they didn't seem particularly dedicated to helping the Biogen scientists along although, you know, some did.

JONES: What was the rationale for even having a Biogen scientist if you've got all these satellite **<T: 35 min>** academic labs working on . . . ?

DONIS-KELLER: Well, I guess it has to be housed under one roof as far as bringing products to market.

JONES: Were you doing more development work, waiting for them to produce stuff, and then they would bring it to you?

DONIS-KELLER: It was too early. It was too soon. We were trying to raise money, to bring money in to work on projects. And we tried a lot of different projects. And I did one little trial in Indiana at Purdue [University] on interferon as a cold remedy. And so, we had these little calves in the vet school there, and we took our interferon out there and we dosed them every twelve hours and then, you know, subjected them to a cold virus. And, you know, it was a very expensive project. It took a long time to organize. And then, the results were sort of equivocal. We didn't have enough animals. And so, you know, I did things like that. But I think it was generally recognized that, sort of, on the pharmaceutical model that you had to have an existing freestanding company in order to be legitimate, and the scientists there needed to do the work.

JONES: Did you have a lot of interaction with Wally Gilbert? Or was he at the university?

DONIS-KELLER: He had an office there at Biogen, quite a nice office—and a sauna. [laughter] And he would appear from time to time, but he didn't come up, and, you know, talk to us in the lab very much. He was, you know, in a different sphere at the much higher levels than down at our level though I worked with Wally in the beginning to try and formulate projects. We'd meet and discuss what kind of proposals, what sorts of projects, and things like that. So, I worked with him. But, it was—I would say—it was very chaotic and very unstable, and there was just a lot of uncertainties and anxieties and backstabbing and all sorts of . . . you know, it was a very unpleasant environment to be in. I must say I really respected Bob Fildes, the president there, but then the higher-ups got dissatisfied with him for reasons which were never made clear to me, and so they fired him. And I just decided to quit because if they would fire him, I just didn't see a future there for me. It was just too chaotic. Who were they going to

get in to replace him? And we were working so hard and it was just you never knew what to expect, what would happen, and it was just too chaotic for me. So, I . . .

JONES: What was Mark Skaletsky's role in . . . ?

DONIS-KELLER: He was some sort of finance guy. He was Bob Fildes's number two person, so I guess he handled—I don't know—setting up contracts or something. I didn't have much to do with him.

JONES: Yeah. What about . . . you mentioned a lawyer? Was this . . . ?

DONIS-KELLER: Rick Eustis.

JONES: The business lawyer or the patent . . . did he do patent work?

DONIS-KELLER: He was more or less, I think, pretty much a business lawyer.

JONES: Yeah. What . . . did you talk to patent lawyers? Did you ever get to the point where, you know, you've got something that . . . ?

DONIS-KELLER: In Biogen I didn't do any patent submissions there. I did when I went to Collaborative Research, a couple that never yielded, you know, patents But patent applications. So, no, I didn't do any of that at Biogen. So, that was Biogen. A couple of years of struggle. It was interesting, but it was just too frustrating.

JONES: And nothing out of those two years? None of the projects that people were working on in Cambridge, [Massachusetts], really turned into anything?

DONIS-KELLER: No, no. Not that I was aware. I mean, when you think about it, we didn't have any background or any sort of intellectual capital, for example, on working on aspartame.

JONES: Did you . . . ?

DONIS-KELLER: That was one of the projects that we considered pretty seriously. But we didn't work on that. But I mean, we'd just, sort of, generate these ideas and then try to make a proposal without any real background in it, so there was a lot of that. And you'd start on something, and then you'd be pulled off it, and you'd start on something else. So, there was no . . . the ground was just shifting under your feet all the time. But we hired a lot of people—MIT and Harvard graduates. And, you know, we got the place up and running. And they had a small fermentation facility, and I think they had some success. But **<T: 40 min>** when I left it was pretty early days. I think eventually they all saw . . . they hired . . . Vicki [L.] Sato was the head of the science for a while there. And I just . . . I didn't have any friends to keep in touch with after that, so I don't . . . and now they have this huge . . . you know, it's huge. Yeah, I sold my Biogen stock and bought a couch. [laughter] But oh well. Anyway . . .

JONES: So, did you have a place to go when you decided to quit? Did you have something else lined up? Or did you know what you were going . . . ?

DONIS-KELLER: I didn't have anything lined up. But, you know, I just needed a break.

JONES: Did you feel like going to Biogen? Was that . . . do you feel like, "Oh, maybe I've ruined my chances for an academic career in science?"

DONIS-KELLER: Well, you know, I actually didn't see myself being in academics that much. I mean, when I was in Canada, it seemed like a nice life that my professors had, and that seemed very, very nice. And then when I was at Harvard, it was just so clear they're just so competitive, and people were not very nice to each other. It wasn't collegial the way that I had seen it in Canada. And somehow, it just—

JONES: Within . . . even within labs? Or . . . ?

DONIS-KELLER: Yeah, even within labs. But, you know, the labs pretty much stuck together against the rest of the world, but even so, there was competition within the students. And unhealthy. In graduate school I saw that, so I didn't really see myself . . . and I also didn't have a project. You know, I had done RNA sequencing, but I didn't have a project to take on as a postdoc. Things weren't working out in Bernie's lab. And usually, a postdoc would have a project to take with them, and some part of that . . . the advisor, they'd, sort of, decide who got what, and then that advisor would be your . . . continuing to be your mentor. And I didn't really have that, so I didn't really see myself having an academic career since I'd have to do another postdoc or something. So going to Biogen seemed like an interesting idea. Why not? So . . .

JONES: So, what was your next step?

DONIS-KELLER: Well, as I was then by this time living with David Botstein, and he was a scientific advisor for Collaborative Research.

JONES: Did he have any ideas about . . . you know, I mean, he's well-situated, right? Did he have any ideas about well, you could do this or you do that?

DONIS-KELLER: Well, yes and no. I went to talk to his colleague Gerry Fink to see about doing a second postdoc in Gerry's lab, and Gerry just thought I was just too grown up to do it or something. [laughter] Because by this time it had been, you know, several years past the postdoc. And he wanted me to work at Collaborative Research. And in fact, when I was working at Biogen, he was always, sort of, denigrating Biogen, you know? And it was . . . there were things I couldn't talk to him about because they were confidential and he was an advisor for this other company. But whatever I talked about, he had nothing good to say about it, so . . . [laughter] So he was really down on Biogen, and that, sort of, probably helped convince me that when Fildes left, I might as well just quit, take a break. So then I went out to talk to these people . . .

JONES: Well, that's interesting that it was . . . Fildes's leaving was sort of the—

DONIS-KELLER: That precipitated it for me because he was—to me—he was professionalism, he was stability, he, kind of, ran the place. And I liked his management style. I didn't like the management style of the academics who were just . . . I don't know, I just couldn't trust them in some strange way. So I went to—

JONES: And that includes Wally Gilbert, who's around, or is he not . . . ?

DONIS-KELLER: Yeah, I mean, Wally said he never put people in competition, but I could see that in the lab. People were in competition with each other. And he wasn't the best manager of people. There was a lot of strife in the laboratory, and, you know, he . . . being a good lab manager was not the best thing that he did. He was a great scientist. So, yeah, there wasn't anyone there that I particularly looked up to, and I had no idea who would . . . I don't even remember who they got in to be the next president. I don't know if Wally was acting president for a while or what.

JONES: I think he was, but . . . you think that would . . . ?

DONIS-KELLER: Disaster. [laughter] Well, he was finally, sort of, pushed out, wasn't he?

JONES: Yeah. But, while you're at Biogen, **<T: 45 min>** you're also aware of Collaborative Research?

DONIS-KELLER: Vaguely.

JONES: Was there anything else going on? Vaguely?

DONIS-KELLER: I was vaguely aware of Collaborative. I didn't know what they were working on. I knew about this guy, Orrie [M.] Friedman, and that they made oligos because we got oligos from them. And I thought they just basically produced chemicals.

JONES: Reagents.

DONIS-KELLER: Reagents and cell culture things. That's all I really knew about them. And I don't think they had really started up much in the way of biotech. What was your other question?

JONES: Well, I was just wondering what was going on, and Biogen had started. Were there other companies that were emerging at that time or . . . ?

DONIS-KELLER: You know, I don't think there were. I think Biogen was really the first one. And there in Kendall Square in that area, it was just a wasteland. That was . . . Biogen was the only thing, and then there were warehouses and just . . . it was a . . . you know, if you'd go there at night or on the weekends you were just, like, on the moon or something. There was just nobody around. And I think Genzyme got started later, but they . . . see, they weren't in Cambridge. And there were some other companies out there that I became aware of later in Framingham, [Massachusetts]. But as far as being, you know, sort of, hardcore biotech like we were in molecular biology, I think we were the only one. That's all I knew about, at least.

JONES: So, the Collaborative Research opportunity comes along and . . .

DONIS-KELLER: Yeah, so I went to talk to Alison Taunton-Rigby, who was the director of science—I don't know what her actual title was. And they really want somebody to help them think about what project areas to be in [strategic planning, and that was what I was hired to do]. And I had actually done that, sort of, project development stuff at Biogen, so I had done some thinking about it, so that seemed like a good place for me to start there. They came up with an offer, and it seemed like a reasonable thing to do. They seemed . . . it was a very small company that seemed like it was more close-knit, more family-like. I liked the people that they had there at that time. So then I went to work on project development there.

JONES: And was Orrie Friedman around?

DONIS-KELLER: Oh, yeah. They had two locations. The one that I went to was in Waltham, [Massachusetts], and then they had another location. It was up the street just off Route 2 [in Lexington, Massachusetts], and we used to call that world headquarters. That's where Orrie was, so I didn't see him that often. So the lab was down there in Waltham, and I spent almost my time there, and then I'd go up to world headquarters for various meetings and things. But . . .

JONES: Well, you know, Orrie is gone, so we'll never get a chance to talk to him. Maybe you could tell me a little bit about him and the impressions he made?

DONIS-KELLER: He was a pretty interesting fellow, a real entrepreneur in the old-fashioned sense. It was his shop, his deal. He was a very outspoken, [profane] person, and he would say lots of regrettable things, especially to the press. [laughter] One of the things that I'll never forget is he told them, "We own chromosome 7." You know? And he just wanted to be kind of with it, you know, and I think he just adopted that language to feel like he was really young and with it. But that was exactly the wrong thing to say.

And he was—I think—he was more of an obstruction than a help. I mean, it was his company after all, but you had to work around Orrie. And every time there was a deal, it would always get broken because Orrie would just throw a monkey wrench in there. And people would just wring their hands—the higher-ups—that Orrie was a deal-breaker all the time. He was just unreasonable. Just was not a good compromiser, didn't really see the big picture. You know, his personality, I think, was his biggest problem.

JONES: Was the company adequately capitalized?

DONIS-KELLER: No, and that was one of the first things we had to really work on was getting more money into the company. As a little company that made reagents, it was fine, but

then wanting to branch into this new molecular biology with this high-powered scientific advisory board . . .

JONES: And the board was assembled when you went over?

DONIS-KELLER: They . . . I think everyone on the board was there, although not 100 percent sure. There was Gerry Fink, Ron [Ronald W.] Davis, David Baltimore, David Botstein.[4] **<T: 50 min>** That was the scientific advisory board.

JONES: And you're doing project development, so you're talking to this board, right?

DONIS-KELLER: Yeah, but it's more talking to Alison and doing research and putting together proposals. And then we would have scientific advisory board meetings every quarter and as . . . I would put together these reports, and, you know, all the scientists would write their sections and then I would, sort of, get it together in one report. This is in those early days of word processors. I remember we had these things on our desk that were so huge and no memory. So I did a lot of word processing with those early word processors.

So, yeah. So, we put together these reports, and Botstein and Ron Davis were very interested in the idea of using RFLPs to map the human genome. And they had written this paper in 1980 with Ray [Raymond L.] White and Mark Skolnick. And David Botstein had a postdoc in his lab—I think she was a postdoc—Arlene Wyman, who had found the first sort of polymorphic marker in humans. But then she didn't find any more, and she didn't want to work on the project, and Botstein couldn't get anybody in his lab interested in it, so he actually never did any work in his own lab beyond this theoretical paper. And so, it was a question: Is there enough polymorphism? Can you find these things and really use them as genetic markers as had been shown for drosophila so many years ago?

And so, that seemed like a perfect project for a research group in a company. You can assemble your group, you have a certain number of tasks to be done, and it required, you know, large scale in the sense of more than one person working on a project. It wasn't the sort of thing that you would give to a graduate student or even a postdoc. The project was too big and required too much cooperation and sharing, so it seemed like a perfect project for Collaborative.

JONES: Well, on the other hand, there's . . . it's not so clear what the commercial path is.

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[4] David Baltimore, interview by Sondra Schlesinger at New York, New York, and Cambridge and Boston, Massachusetts, 7 February 1994, and 13 and 29 April 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0198).

DONIS-KELLER: Exactly although I think we made a really good case for once you had a genetic map of the genome, then you could apply that to inherited diseases and probably common diseases had some genetic basis. And we put together, I think, a really good plan for that. And, you know, the more common diseases—heart disease, schizophrenia, bipolar—affected a lot of people. And so, there was a—you know, especially heart disease—a potentially huge market, and that seemed to raise, you know, some interest. So, we put together our proposals, and we actually got started on it without any funding. And the scientific advisory board was . . . endorsed it. [noise in background] Par Par! [dog] She's rearranging her bed. [laughter] This would have been around . . . oh, I guess, [1982, '83]. And so we decided—

JONES: How many people are on the project?

DONIS-KELLER: We started out with just maybe two or three. And then, I actually have all of the data on how the project grew. I was surprised I kept everything—all kinds of notes on meetings and things.

JONES: You know, we'd love to scan these things if . . .

DONIS-KELLER: I'll put it in order.

JONES: Okay. And maybe we could come back some time and go scan them.

DONIS-KELLER: Yeah, that would be fine. There's just a huge amount of time. I've got the old advisory board meetings. Like this is the kind of thing I had to put together.

JONES: For the meetings?

DONIS-KELLER: For the scientific advisory board meetings.

JONES: Ah, that's a pretty hefty document.

DONIS-KELLER: Yeah. So, my section would be all the molecular biology [and genetic mapping], and then there were a couple of other people who would, you know, bring their scientists together. So, I think at the—

JONES: So this was done on the old word processor?

DONIS-KELLER: Oh yeah, and then just Xeroxed and oh goodness. Yeah. So, I think at the largest I probably had twenty people working with me on this—people in cell culture and then developing markers and doing the probing and the linkage mapping calculations and things. So, we started a few people on this project, probably maybe three or four: Jim Schumm, Rob Knowlton, [Jeff Braman, Cindy Helms], me, maybe one other person, Tim Keith.

JONES: And you're collaborating . . . is this . . . what's this? **<T: 55 min>**

DONIS-KELLER: What's that one?

JONES: Working on . . . you've got a marker for this and you're doing something with it?

DONIS-KELLER: Oh yeah. Neurofibromatosis, [but that came later in 1986-87].

JONES: You've got a lot of big, big names on the . . .

DONIS-KELLER: Yeah, right. So, yeah, that was Karen [Stephens, a senior scientist in my group]. That was her project, so yeah, this must be around ['86-'87 or something]. So we got started so we would have some credibility, some markers to be able to show, and we made a lambda library and started looking for polymorphisms using these lambda probes, which we had screened out through repetitive sequences and things. [We first used the markers to follow the progress of bone marrow transplantation and that gave us some credibility.] And [for the genome mapping project] we had developed Southern panels of unrelated people that we would do an initial test to see if we saw some size variation in these restriction fragment lengths. And then at that time the CEPH [Laboratorie] in Paris, [France], was getting started. And also Ray White came on as a consultant and collaborator, and he had been . . . well, basically, to do the map you needed a set of family resources, you needed a set of markers, and you needed linkage mapping methods. And Ray White was interested in this idea too, and he had been a postdoc at MIT, and he and David Botstein knew each other. And Ray was really convinced that this would be a good idea. He was on that paper—the 1980 paper—along with Skolnick. And he went out to Utah. I mean, he was at UMass [University of Massachusetts] and went out to Utah because the family resources were there. And he would take his bloodmobile to family picnics and collect these family resources. The Utah Mormons had large sibships and availability of grandparents, parents, and then the large sibships. And so he would establish lymphoblastoid

cell lines. And so, he developed the family resource, and he had twenty-one really good families for mapping. And so we talked about a collaboration and we would, you know, exchange probes and he would give us families and we would all be mapping together.

JONES: Were there any problems with the commercial, the academic . . . ?

DONIS-KELLER: Oh, yeah. Huge. Again, this arrogance on the part of academics and administrators at these various colleges and universities. We'd have to have a collaboration agreement. The lawyers would have to vet the thing, and it became impossible in some cases. In fact, we never got a cystic fibrosis collaboration going with Ray because the Howard Hughes Medical Institute was funding him by that time and we just couldn't get it past the lawyers at Utah or Howard Hughes. And so, finally, we never got to even use his family materials, although he had sent us things, we'd made some blots, but we could never come to agreement. So, anyway, sort of, getting ahead.

So, we started this, and we then . . . this was when limited R&D partnership were the thing to do. All of the biotech companies were going and getting money from Wall Street. And so, we made the rounds to all of these investment banking places, and we had our dog and pony show. I would give the presentation on the science, and then the president would, you know, talk about the company and all that.

JONES: Who was the president at the time?

DONIS-KELLER: Oh, again, we had, sort of, a revolving door of those people. Jim [James] Wimbush was one. And, oh gosh. We had several of them. I have to think about that too and then we also had revolving vice presidents for research too, and the last one was Tom Oesterling, and he's the one I worked with the most, I guess. Alison Tauton-Rigby was . . . I don't know, she had some dust-up with management and she left and went to Genzyme. She was a scientific leader. And then . . .

JONES: How was it working with her?

DONIS-KELLER: Oh, extremely difficult. Nobody could work with Alison very well. She was a very tough person to work with.

JONES: Wait, how so?

DONIS-KELLER: Personality. Just she would . . . you would have a conversation—you would come in with an idea and you would have a conversation—and by the time you left, she was convinced that it was her idea, that she had thought of it. And this is pretty well-known. And she just wasn't a good manager. Just wasn't . . . you know how some people can work with others and develop them and support them? She wasn't one of those kinds of people. But I don't envy her having to work with Collaborative Management. **<T: 60 min>** They were all men I'm sure biased against women. And she had a tough time. But she gave us a very tough time, and there was a vote of no confidence by the whole group of us, and that was it for her. So she left. I think she did very well at Genzyme from what I heard.

JONES: And the revolving door with vice presidents and presidents is that a reflection of the . . .

DONIS-KELLER: I think—

JONES: The company?

DONIS-KELLER: I think the difficulty to try to get money in the door and get support for projects and just a lack of leadership vision. Orrie certainly was . . . didn't have the vision. He didn't have the leadership skills, so he probably made it very difficult for any president. I can't imagine what it would be like to work with him at that level. So there's . . . you know, that was always changing. It was sort of "president of the month" and "VP for research of the month" too. At least, it felt like that.

Anyway, so we made the rounds of all the investment bankers. And we did . . . finally, we got twenty million dollars, and let's see . . . I'll have to go back and look. We put together a whole marketing plan, and they did an R&D partnership, and we got twenty million dollars for this project, for the mapping project, and for some other things too. I'll have to go back and look at the details on that. But I remember just going through all those documents that we . . . that had to be drafted and all the lawyers and how much it cost to do that. It was impressive. But we got . . . I think Jim Wimbush was responsible for us getting that twenty million dollars initially. But then, I don't think we were able to get any more other than that.

JONES: Well, when you're doing the road shows, or would you call it a road show for those?

DONIS-KELLER: It was a road show, definitely.

JONES: And were you . . . the R&D partnerships, were these to individuals, selling to individuals at that time?

DONIS-KELLER: No, it was like Merrill Lynch would put one out and get people to buy into it.

JONES: So, you're giving the presentation to the . . . to bankers, and then they'd go off . . .

DONIS-KELLER: Right. And they would structure it and get their investors and then it would . . . you know, there would be an offering.

JONES: So you'd have to convey the science to them. Did they get it? Did they understand?

DONIS-KELLER: No. [laughter] So, you know, we had to develop a way of talking about the science in layman's terms that made it all logical, that it would make sense, that it was understandable. And I have to say we were able to do that, I think, with analogies and with good diagrams and explain crossing over and how you could do a diagnostic test with RFLP markers, you wouldn't have to know anything about the disorder in order to map and to be able to test for it because what you were testing for was linkage to it. And I think we made a really good case, for that. And even though some of these disorders were rare, it was clear that, you know, there would be other ones that we could work on once we got these. So, I thought it was a really good package, and we did get some money for it. And, let's see . . . so that brings us to, like, '83, '84. And, let's see . . . we went, started going to these human genome mapping meetings, and there was one in Helsinki, [Finland], I think, and that would have been '85.

JONES: And this is . . . these are the kind of things where, for example, your advisory board members are showing up there? Skolnick . . . was Skolnick on the board? I'm not sure.

DONIS-KELLER: Skolnick was a consultant. He really never had much to do with the company. Ray White would have been at one of those meetings. He was a paid consultant as well. Ron Davis might have gone to those meetings. But they were essentially mapping meetings where you would report, you know, progress and see what other people were doing on the other chromosomes. So it was the genome mapping meetings that we went to and we'd show some of our data. [chime] Do you need to answer that?

JONES: No, I don't.

DONIS-KELLER: So we had a pretty good set of markers at the ’85 meeting. And this is also a place for us to talk to potential collaborators. Before that we had talked to Lap-Chee Tsui and Manuel Buchwald in Toronto, [Canada], because they had cystic fibrosis families, and Ray White had some cystic fibrosis families. So we were trying to identify **<T: 65 min>** academics who had these families collected so that we could test our markers. Even though we didn’t have a full map we could still, you know, get these things in place and start doing some testing.

JONES: Were you excited about this? Did you think . . . ?

DONIS-KELLER: I loved it. I thought it was great. I absolutely was just practically religious about it. Yeah, I totally believed in it, and, you know, we did a lot of really good work, I think. A lot of useful work.

JONES: How hard were you working at this time? How old is your daughter during this period?

DONIS-KELLER: Oh, let’s see . . . she would have been [mid-teenage] years [sixteen in 1985]. So I was working really hard. But, you know, I could take my work home at night too. And so I wouldn’t be in the lab at night while she was at home, so I would always take my work home. By this time I was out . . . I was not working at the bench anymore; I was really a project manager completely, so there was always things I could do at night at home. But it was really, really hard work. It was just like being in academics—in competitive academics—where you just live and breathe that work and you’re working on it on weekends, but it’s interesting, and you want to. So, amazingly enough, we set up this collaboration with Lap-Chee in the—I was just looking at the notes—in July of ’85, we got the collaboration agreement. SickKids Hospital in Toronto gave us no trouble with setting up an agreement whereas these other places, you know, we’d go back and forth with the lawyers. It was just hideous.

JONES: What do you think accounts for the difference?

DONIS-KELLER: I don’t know. A hospital is more of a business. I don’t know. Beats me. But so Lap-Chee sent us families. And we thought we would exchange families and exchange probes and we’d both just get busy mapping. And we sat down with him at the human genome meeting in ’85—in August of ’85—and we just, you know, figured out, well . . . or decided a LOD score of 3 would have to be the threshold for linkage, and -2 would be non-linkage. So we worked out the ground rules of what would make sense in terms of the data. And amazingly enough, like three . . . and then he went back and I went back. And three weeks later, he found linkage—he was the first to find linkage to this one marker, 917. And I was just looking at the

calculations . . . I even . . . I still have all of those data, and the LOD score was 3.22. It was unbelievable. It was like, "No, this can't be happening. We just started." [laughter] And we thought it would be years. You know, we'd work our way through the genome, and we'd finally figure out what was on what chromosome. But in the linkage . . .

JONES: Did you just get lucky with this?

DONIS-KELLER: Yes. We just got lucky. It was an informative marker, a reasonably informative marker, and it was in the first batch of probes that we gave to Lap-Chee. He was just working his way through. I think he had probably tried maybe forty or more markers on his own and then some of ours before this linkage happened. And so, that was incredibly exciting. But we didn't know what chromosome that marker was on because we had markers but no chromosome localizations for those. So then it had to be determined what chromosome. And Lap-Chee had some chromosome panels that could be used, but he mixed them up the first time and so we didn't trust his result. [laughter] And then so we got another collaborator involved, and this was [Jean] Frezal and Odile Cohen-Haguenauer in [Paris], France. And they had some panels that could do chromosome localization of probes. And so they came up with chromosome 7, which Lap-Chee had, but he also had, I think, chromosome 2. You know, it wasn't completely clear. But Odile's work showed that it was 7. So then there was a meeting coming up in Utah, the American Society for Human Genetics, and their work had not yet been published. And, you know, it's funny, the . . . it gets out immediately. People knew immediately that something was going on, that we had something.

JONES: You . . . had you talked about, you know . . . ?

DONIS-KELLER: Oh, of course. But Lap-Chee was irrepressible. He told Ray; Ray told people. Everybody was telling everybody and not admitting it, so . . . . [I heard later that one of our scientific advisors spilled the beans]. And the journals, I . . . the journals have these rules. You cannot have a prepublication. You can't talk about this publicly until it's been accepted for publication. And so I was very observant of those [rules]. And so we had written up the 917 linkage and submitted that to *Science*—that's another good place for leaks, the **<T: 70 min>** reviewers.

JONES: Wait, is that— Yeah.

DONIS-KELLER: Sure and then we had the chromosome localization when we went to this meeting at the American Society for Human Genetics, but we couldn't really talk about it. And then, of course, all the academics were "Well, who do you think you are, you company people?" And "you have to tell us, you have to get this out in the world. You know, you're depriving . . ."

Whatever. So, it was very acrimonious, I have to say. People were pretty nasty. And Lap-Chee knew, but . . . And then, at the same time, sort of, a rival company, Kathy Klinger at Integrated Genetics, said that they had a linkage to cystic fibrosis on chromosome 21, which again made me wonder: are there . . . is there more than one locus for this disorder? It looked like it was a single gene disorder, but it wasn't completely clear what was going on. It turned out hers was not true, but she presented that at the same meeting, and I didn't know what to make of her results, but . . .

JONES: Did you go back and say, "Oh, we need to look at 21"?

DONIS-KELLER: We didn't have any markers on 21. We didn't even know if we had anything on 21. So, it's not like we could just go and repeat the experiment and test. We couldn't. So, anyway, our first paper in *Science* got published, and then the second one for chromosome localization was submitted to *Nature*. And then, all of a sudden Ray White had linkage to chromosome 7, a different marker, and amazingly enough, it was even closer than our marker. It was just astounding how serendipitous this was for everybody. And then there was a competitor in England, Bob [Robert] Williamson, and he had reported linkage to an enzyme marker, which you couldn't really do. It never really worked out, but he did . . . this thing was called (PON)-1 [Human serum Paraoxonase]. He said he had linkage, but it wasn't really a practical marker to use. It wasn't a DNA marker; it was an enzymatic assay kind of thing.

So the whole field really heated up with respect to cystic fibrosis. And then, the Cystic Fibrosis Foundation got involved too because they wanted to be on top and be seen as the ones who were really sponsoring the research, and they made Ray White some, kind of, fancy CF investigator or something. And they really . . . they were not very nice to us.

JONES: Is that right?

DONIS-KELLER: Yeah. We didn't—

JONES: They didn't think that you would be good . . . the same kind of publicity going to . . . ?

DONIS-KELLER: Yeah, they didn't want to be affiliated with a company. And we applied—they were giving out grants they said—so we applied. We thought, "Well, maybe we could get some funding." Because we didn't have much funding. And they basically said, "No, not to companies." And, you know, they were happy to take our data and claim that the Cystic Fibrosis Foundation was . . . [It was] a real awakening for me to see how these disease foundations, who I naively thought were just interested in the welfare of their patients and finding the truth and getting to farther advance in the science, but, you know, they were very

small-minded, interested in their own prestige. And there was some opposition we could feel for doing prenatal testing for cystic fibrosis because of the decisions people would make not to bring that pregnancy to term.

JONES: What was the position of the foundation?

DONIS-KELLER: They didn't have, they didn't say outright what their position was. But, because they relied so heavily on people who are parents of children with cystic fibrosis that the notion of sponsoring, you know, abortions was something that they didn't want to be affiliated with. We certainly got the sense—that sense—very strongly from them. And, you know, it wasn't really a big ethical dilemma for some families, but we were deluged with questions from people—could we do prenatal testing for them?—even before we had flanking markers, so very early on. People were desperate. They had already had a couple of children with cystic fibrosis, a pretty horrible terminal disease. Just awful in those days. I mean, now people survive much longer, but in those days, it was just horrible. So, then we started thinking about making a reference laboratory so we could actually do genetic testing. So, that was another effort that we undertook.

JONES: Were they doing that in Framingham? Did they . . . ?

DONIS-KELLER: I don't know if they did . . . they were definitely interested in testing. I don't know if they did cystic fibrosis or not. They had some other rare-squared diseases **<T: 75 min>** they were working on, I think. So we started a reference laboratory.

JONES: And that's a pretty good-sized investment as well?

DONIS-KELLER: Yeah. And so cystic fibrosis was the big one that we worked on. And it was a very expensive test to do because it relied on Southern blots and, you know, we hired somebody who was experienced with reference laboratories, but, you know, it was a lot of labor and calibration and you had to be extremely careful in the, sort of, chain of evidence or whatever with the sample when it came in. And it wasn't like being an academic lab, you know? There were a lot of different rules and regulations that reference laboratories needed to adhere to, although people in academic labs were just doing this testing themselves without the safeguards that reference laboratories put in place. But we got started on that. Cystic fibrosis was the first one. But we continued on the genetic mapping, and we did in 1987 have our whole genome map together, and that was another sort of controversial and acrimonious—

JONES: Why was it controversial?

DONIS-KELLER: Well, Ray White was our on-again, off-again collaborator, and he had his own, I would say, personality issues. And he didn't think very highly of companies. He saw us as probe suppliers, not as scientists, I think. We could do something for him, basically. And I think he was—because we were not able to set up the cystic fibrosis collaboration because of all these . . . I was looking at all the letters from the lawyers and this and that—I think he just was angry with us about that because he missed out on linkage. And I think he missed out, he felt like he missed out on other things. I think when he was a postdoc or a graduate student in Maury Fox's lab, he missed out on the introns that Phil Sharp saw. He saw that in the TEM [Transmission Electron Microscope], but he misread it. And so, he . . . this is my own, sort of, mental thinking about Ray feeling like "Oh, I missed that again" or "I missed this" or "I missed that." So I think he was unhappy with us over cystic fibrosis. And he was in Utah, and we were, you know, where we were.

So, he . . . the other thing that happened while we were developing our mapping work was the CEPH collaboration in Paris, in France with Jean Dausset, who had done a lot with HLA. And he wanted to form a genome mapping, linkage mapping group of people in Europe and in US worldwide. I mean, it was a great notion, and we would bring together all the families. He had collected some French families, and a schizophrenia Amish pedigree had been contributed by Ken [Kenneth K.] Kidd. And all together—on paper at least—there were forty families that would be the mapping resource, and then we were supposed to share data and share probes. And I was one of the founding members of this CEPH collaboration. And I think it was a wonderful idea. It was a grand idea. And Dausset had the money to actually grow up huge amounts of [cell lines from which DNA was isolated], and they would send out DNA to people, anyone who wanted it and wanted to do mapping. So, that was just a wonderful thought because for each individual lab to do that, the cost was just prohibitive. We ended up getting the cell lines ourselves because we were just using so much DNA. It was silly for them to produce it for us; we could do it ourselves. So that was going on, and Ray was a founding member of the CEPH collaboration as well.

So we continued to develop more markers, and we had about five hundred markers that were ready for mapping. And our strategy was to construct linkage groups and then tie one member of the linkage group to a chromosome by *in situ* hybridization or using blots because then we would know what chromosome the group was on, instead of painstakingly physically mapping one by one. And we also didn't have those resources or that technology in our laboratory. So, that was our strategy.

The linkage mapping methods had not been developed at all well. If you can imagine four hundred markers, doing two-point analysis to see which two markers were linked to each other, that's something like 81,000 calculations **<T: 80 min>** that would have to be done. And then, to get the order of markers you'd have to have some new kind of algorithm developed called multi-point linkage analysis. And Ray White had a collaborator in Utah, Jean-Marc Lalouel—he was the computer guy. It was a disaster. He never got anything done. He was impossible to talk to. We never got anywhere with this guy, and so we hired our own guy, [Phil

Green]. And Eric [S.] Lander had just then become interested. He came to the gene mapping meeting in '85. He had been at . . . he was a mathematician, was interested in doing something else besides pure mathematics, and he—I think—he said through his brother [Arthur Lander], had learned about David Botstein and the . . . what was going on at MIT. And so, Eric went and spent some time in David Page's lab at MIT, but mouse genetics wasn't something he was interested in, I guess. I don't know. So he came over and, and he and David Botstein really hit it off. And so, Eric became really interested in human genetics and came . . . he wanted to learn about it. And he had no reputation, no . . . nothing. No . . . nothing had been done. So, he just came to learn about it at the '85 meeting. And just to show you how insular people are, they . . . he was not warmly received. He was this outsider. But that's where I remember meeting Eric.

JONES: Yeah. By the way, he's—at this June 4 event—he's . . . there's a panel discussion, and he's going to be the moderator.

DONIS-KELLER: Oh yeah.

JONES: I'm not sure what the theme is. It might just be a general . . .

DONIS-KELLER: Genome thing?

JONES: Well, it's not genome; it's just biotech, Boston . . . I'm not sure, exactly .

DONIS-KELLER: Oh yeah. Well, he was a big shot with Millennium [Pharmaceuticals], right?

JONES: Yeah.

DONIS-KELLER: Yeah. I tried not to follow those things after a while. It's like, "Enough." He was an interesting character. Anyway, so Eric then, sort of, was a consultant to us to help us hire somebody who could really develop the algorithms that we needed because we were just stuck. Lalouel wasn't helping at all. We knew we needed better methods than what was out there, totally. I mean, our cystic fibrosis stuff, we did essentially by hand. It was ridiculous. So I got a letter from Phil [Green]—we put out an advertisement—and I got a letter from Phil [Philip Palmer] Green. This was in the spring of '86. I still have his letter where he asked, you know, he said he was interested; he had this kind of background. So, we brought him in. Eric interviewed him, and I interviewed him. And I can remember Eric and I walking around in the parking lot [at Collaborative in Waltham] talking about this guy, Phil Green, and decided he was the kind of

guy who could develop the methods, the methods that we needed. And he and Eric hit it off, and so we hired Phil, and he got right to work, and he started working on the cystic fibrosis stuff. By this time, we had a number of markers, and we needed to have a good way to do the calculations to know what the probabilities for crossovers were and stuff like that and then just start to build up some linkage maps of chromosome 7 by linkage to this little group.

And so he and Eric talked about how to develop these [multi-locus linkage] algorithms, and they ended up using something called the EM algorithm. And so, Eric developed one way of doing it, and Phil Green developed another way. And so, we would then just give them our data and when we put together the whole map, Eric did a run with his students and Phil Green would do runs and then compare notes. And we got essentially the same map, so that was really nice to have that confirmatory data. But the methods worked really well.

JONES: And all this time Orrie Friedman is—

DONIS-KELLER: He's out there.

JONES: "It's great, and go ahead, go ahead and do whatever you need to do"?

DONIS-KELLER: Yeah. Yeah. Very enthusiastic about it. We added more people. We needed more people, so we were given the go-ahead to do it. And I think Orrie really bought into the idea of being able to apply this to all kinds of diseases. I mean, very, very few had been mapped, much less cloned. I mean, the first one that—besides muscular dystrophy, which, you know, it was a special case on the X chromosome— the first one was Huntington's disease.

I remember actually being called into the office of the . . . I think it . . . the then-president. "Why didn't we know about this?" "How come we weren't on this?" It was just absurd; we had no families. So, I remember that.

And we had some collaborators **<T: 85 min>** to tie these things to various chromosomes. Odile, I think, was the main one. And so, we started to develop these linkage groups on different chromosomes. And we ended up with about four hundred markers, and we incorporated some data that we had gotten from CEPH because people would report their data to CEPH and developed this map. And that was in the summer of '87. And Eric and I set to work writing the paper. We had a linkage map for the human genome, and it was pretty decent. It excluded—I mean—it included 95 percent of the genome; I think that's what we say in the paper. And we had a map for every chromosome, except the Y, of course, which doesn't have recombination. And we had male maps and female maps and an average map because females have more recombination than males, so we end up with these different kinds of maps.

And we had an incredibly dedicated group. These folks just worked so hard. Doing Southern blots was always tricky if you've been in the lab and done things like that. And it just took a lot of work and very careful work in the genotyping. And it was like a little family. We just worked really hard, and we were taking all-nighters to write the paper and to get the data and make the figures and all that. And as Eric and I were starting to write this up, Eric had the idea that it would—

JONES: [phone rings] Sorry.

DONIS-KELLER: Oops. Got to get that one?

JONES: [side conversation on phone] Sorry.

DONIS-KELLER: Sure.

JONES: I should turn this off.

DONIS-KELLER: You have two cell phones?

JONES: No, I've just got the one. No, this is . . .

DONIS-KELLER: Oh, the recorder.

JONES: Okay. Sorry.

DONIS-KELLER: Let's see.

JONES: We were . . . you were talking—

DONIS-KELLER: Oh, Eric had the idea that "Wouldn't it be cool if we got this published in *Cell*"? Because *Cell* had no page limitations. We could write this whole thing up, and we could have, like, a centerfold with all of our maps and everything. And so, I think he called up Ben Lewin, and we had lunch with Ben Lewin, who was then the editor of *Cell* and told him what

we had, and he was pretty excited about it and said, "Yeah, by all means. Write it up for *Cell*." And so we did that, and it was . . . we submitted it right before we went to the next—that was two years later, so it was '87, fall of '87—it's the next human gene mapping meeting. And that was the beginning of September, and so we submitted this paper to *Cell* in the end of August. And I remember just we took, like, three all-nighters to finish it, writing it up, and we finished writing it up actually in Eric's lab because we were still doing some more calculations and things.

And then, of course the company wanted to capitalize on this as best they could, and they decided we would have a press conference. Only, it would be embargoed. [laughter] So, here we had done all these all-nighters; I was just totally bleary-eyed. And Eric and me and Phil Green met with these press people. I even wrote down who . . . Howard Schmeck was one of them from *The Times*, and then I wrote down the names of the other ones. I still have that; I was looking at that. And so we gave this little press conference, and they were embargoed and we went off to this meeting in Paris. And then there was a whole big tussle about authorship and order of authors. Eric, of course, realized that this was important, and he was, kind of, buried in the middle because he came in pretty late in the project. We had been working on this for five years, and the people in the lab had really been working hard. And Phil Green, this was his day-and-night, you know, that's all he did, so he was . . . played a really major role. And so—

JONES: So, these, these were tough negotiations?

DONIS-KELLER: Tough. Very tough. Yeah. And . . .

JONES: Whose, whose call was it ultimately?

DONIS-KELLER: [I was in charge of the research group so it was mainly my call, but], you know, in the end, the scientific advisors got involved. The way it started out in the beginning, Phil Green was first on the paper, and I was last because I was the senior author. That's sort of the tradition. And then, Eric wanted to move up. And then, some of the scientific advisors thought that they should be on the paper. Ron Davis thought he should be on the paper. He thought all the scientific advisors should be on the paper; at least so I heard. **<T: 90 min>** And so, it became a whole big hubbub. And we had a meeting with David Baltimore. So then, David Baltimore said, "Well, you know, if Botstein and . . . goes on the paper, if the other scientific advisors go on the paper, then [you] have to be first." So, he insisted I be first, which . . . hey, fine, no problem with that. [laughter] And so, Botstein was buried there in the middle to satisfy him, I guess. Ron Davis decided he didn't need to be in the paper, and Gerry Fink could care less.

JONES: And what about Phil Green? He's been bumped.

DONIS-KELLER: He was number two. He was happy with that. No, he had no problem with that at all. I had led the project for five years and brought him in, and he was so happy to have a job and a career. And he prospered tremendously from this.

JONES: Yeah. Well, how did you feel about the scientific advisors coming in and putting their names on it?

DONIS-KELLER: They hadn't done a thing, one. [laughter] Botstein had not done a thing. He never . . . he didn't look at anything, our [calculations]—nothing. They would show up for the advisory board meetings; they would take their nice big fat paychecks. Thanks. They got stock; we didn't. You know, so there were all those kinds of things. So, yeah, I wasn't happy about that. But, you know, you have to compromise. And, you know, he . . . it was his idea in the beginning that this would be a good thing to do, so fine. He was in there in the middle. And then, Eric Lander's undergraduates and—I think he just had undergraduates at that time—Steve Lincoln and a few others were in there. It was a cast of—I don't know—twenty authors or something. Quite a few.

JONES: Big science.

DONIS-KELLER: So that's where the mapping . . . that came out. And when that came out, it was big news and we were on the front page of *The New York Times* with our pictures and everything. And Ray White was just livid. You know, competitive—we got our map out, and he was nowhere near with his map. And so he called it, "Premature and presumptuous." That's what we were: premature and presumptuous. I think he was quoted in *The Wall Street Journal* for having said that about us. And so my lab folks had a sweatshirt made for me that said "Premature and Presumptuous" in quotes—very wide.

So, you know, we always felt somewhat beleaguered that the academics never really accepted us. There was a lot of "Well, this company, they won't share their probes now" and "Who's going to have access?" And so, there was a lot swirling around. And in fact, the company was trying to figure out how can they make some money off of this. Can they make a service, or can they rent the probes out or . . . and so, they were just trying to figure out what to do. And then after the paper came out and all the sort of fanfare kind of died down, they decided they were done with the mapping—like, essentially cancelled the project. So, this was like '88.

And said, "We've done enough mapping. We're not going to fund it." And so that was kind of . . . that was very depressing for us. You know, when all of a sudden . . . this is what happens in industry, though. You know, one day you're working on a project and then some higher-ups decide that you're not going to do it anymore and you're done. You get, you know,

reprogrammed into some other project. And so then everything, sort of, became more on the reference lab side of things. We had been working also with other collaborators on different diseases.

JONES: Did they figure that there was enough there to work with to come up with tests or . . . ?

DONIS-KELLER: It was never clear that that was a way to make money. And we had some of the best people. Sam Berkman was an advisor. He had, you know, done quite a lot of lab testing and made it all automated, and he was a very smart guy. But this was not a blood test. This was not a simple, you know, diagnostic. This stuff just couldn't be automated in the way that it . . . you know, using Southern blots. When things turned to PCR, I think it got a lot easier, and it is . . . . I think it still doesn't make money for people; it's still very expensive depending on how many different mutations there are and things like that. But they were thinking and hoping that somehow they could make money in the reference laboratory. They were convincing themselves. So at that point I just . . . . I wanted to keep mapping, you know? I wanted to keep working on cystic fibrosis. We did get some grants, interestingly enough, from the NIH [National Institutes of Health]. We got one for cystic fibrosis—

JONES: So, the company said, "If you can get your money to fund the program, go ahead"?

DONIS-KELLER: Yeah. **<T: 95 min>** And we did. And we had a mapping grant, but it wasn't big, but it was something. But they weren't going to support it other than that. It's funny. We put in an SBIR grant way back in '84, and it got, like, a priority score of 359 or something. They thought it wasn't doable. So, that was probably . . . [laughter]

JONES: Whose idea was that to do the SBIR?

DONIS-KELLER: Oh, at the company we were always putting in SBIRs.

JONES: Yeah?

DONIS-KELLER: Yeah.

JONES: Was that Orrie Friedman? Or whose . . . ?

DONIS-KELLER: Oh, probably Alison.

JONES: Alison.

DONIS-KELLER: Yeah. Yeah. So—

JONES: When Alison left, then how did things get reconfigured? Somebody else did . . . ?

DONIS-KELLER: Well, then they . . . I don't remember after Alison—maybe it was Oesterling after Alison. I thought there was some intermediate person before that. I'll have to think about it because I thought we did have somebody else before him. I don't remember now. Maybe . . . No, I thought maybe Gerald Vovis took over. He was the other sort of senior scientist. I don't know. I'll have to think about that. But Oesterling, when he was . . . see, when Alison was, sort of, the head of research, she was in our building. When Oesterling took over, he was over at world headquarters.

JONES: He was at world headquarters, yeah.

DONIS-KELLER: With all those people. And he, he was not a scientist. He was from . . . was it J&J [Johnson & Johnson] maybe? And he didn't know any science at all. In fact, the first meeting I had with him, he asked me if plants had DNA too. And I thought, "Is this a test? He's testing me to see if I can keep a straight face?" I think he really didn't know. So he did, sort of, the business side of science. But he couldn't understand the science at all. Anyway . . .

JONES: So, does the mapping project wind down at some point then?

DONIS-KELLER: There's less people working on it, and at that point, I just . . . Maynard Olson came and gave a seminar at our little place in Waltham, and I was starting to think about, "Well, maybe I can go somewhere else to do this work." Because I just . . . I loved the mapping, and he suggested that there might be a position at Washington University. And so, I went out there—

JONES: So, this work had really transformed your options?

DONIS-KELLER: Oh, absolutely.

JONES: Or your range of options.

DONIS-KELLER: Yeah. And there were so many different opportunities. You know, there were different diseases that people hadn't mapped yet. There's just . . . I looked at Johns Hopkins, and there were some real possibilities there working on bipolar and unipolar and autism. But they really couldn't put together much in the way of a group or any, you know, way to get started, or space because Hopkins is Hopkins; they just didn't have any space. And I would be in the psychiatry department, which wasn't such a good fit. But they were really nice people there. Susan [E.] Folstein and her husband [Marshal F. Folstein]—what was his name?—we had . . . I had gotten to know them a little bit. And then Washington University, I went out there and gave a seminar, and it was amazing. They had just beaten the bushes, and there were just a ton of people there for my seminar. They couldn't have made me feel better. And they offered a lot of space and a huge start-up package because they wanted to position themselves for human genome mapping, and doing the linkage map was a really good start. And Maynard had this grand plan for having a genome center. And this is . . . so now we're into '87, '88, and there's a lot of talk about starting the genome project.

JONES: Is there confidence that it's going to happen, that the government will . . . ?

DONIS-KELLER: Yeah, he was certainly an insider on study groups and things. And there had been some meetings—I went to a few of them— talking about the feasibility of developing a map of the genome—a physical map of the genome—and then finally a sequence of the genome. A lot of people who were at the first meetings were completely unsure that that could be done, even for three billion dollars, that it was just . . . and it was a very expensive technology, and it was hard to imagine how you could do the whole genome with all the repetitive sequences and everything. But the physical map seemed like it was approachable. The genetic map, if we kept working on it, we could get really high-resolution maps. That was . . . it was clear that that was really going to work. So, the idea was to have **<T: 100 min>** the genetic map, and I would, sort of, run that, and then Maynard's folks and David Schlessinger would do the physical mapping, and then Waterstone would do the sequencing. And so, that was the grand idea. And it wasn't hard to convince me to relocate, and, and it was terrific. We got a lot done. And then, I relocated with Phil Green and Cindy Helms, my two biggest helpers and wonderful people.

JONES: They went to St. Louis, [Missouri]?

DONIS-KELLER: Yeah, which incensed Orrie to no end.

JONES: Did it?

DONIS-KELLER: Well, I mean, Phil wanted to keep mapping. And Cindy was . . . she's an amazing person. She was the lab manager. A very sophisticated sense of genetics and just . . . she was just wonderful. And so, I had the opportunity to bring more than myself. And I talked to Phil and to Cindy, and they were really happy about coming and getting into academics. So, I went to tell . . . I went specifically just to tell Orrie by myself—I think at his house even. And he said, "You can't quit!" [laughter] But it was okay. They . . . once they, sort of, got over it, they were fine with it really.

JONES: Who else did you have to tell?

DONIS-KELLER: Oh, Oesterling, I guess. He was still there. He left maybe a year after I left. I don't know who they got in after I left. I haven't talked to those people in a long time. So then I went to Wash U and continued genetic mapping and worked on . . . there were a whole lot of people to collaborate with at Wash U on various diseases. We worked on diabetes. We worked on multiple endocrine neoplasia. All kinds of . . . and other collaborators at different places too. So we really had a good time. It was great. So . . .

JONES: And you liked living in St. Louis?

DONIS-KELLER: Well, you don't move there for the climate, that's for sure.

JONES: Right. Okay.

DONIS-KELLER: And it wasn't a very interesting city to be in. But, you know, again, if you're in the lab seven days a week, what difference does it make? [laughter]

JONES: You were still working at that kind of . . . ?

DONIS-KELLER: Oh yeah. Sure. I loved it, and by this time my daughter was in college, so, you know, she'd gone through high school, and me making a move was fine.

JONES: Where, where did she go to school?

DONIS-KELLER: Barnard in Columbia [Barnard College of Columbia University].

JONES: Okay. So, you split up at that point?

DONIS-KELLER: Yeah, she would come back in the summers. But yeah, so, it was . . . I think when she went to college, then it was okay for me to think about moving somewhere else.

JONES: And how did you . . . I don't know . . . is "adapt" the right word? I mean, it's an academic environment. Was it a big switch, or not?

DONIS-KELLER: Well, it was, sort of, hard to get the geography or the lay of the land, to understand how people behaved and everything. But I was so buried in getting the lab up and running and getting things going and forming collaborations, I was just busy, busy, busy, and then later, you know, you find out that there's a lot of politics. There was a lot of—

JONES: Politics at the university?

DONIS-KELLER: Oh yes. If you think backbiting is bad in industry, I mean, it's rampant in academics. So . . .

JONES: Although there's not that much to . . .

DONIS-KELLER: Fight over?

JONES: Well, you know . . .

DONIS-KELLER: Space.

JONES: Yeah, I guess.

DONIS-KELLER: Space. Resources. Students. Graduate students. But, you know, I made some really good friendships there too. Some really good people to work with. So, all in all I was much happier there ever than working in the companies I think in part because I was really running my own show. You know, if I wanted to work on a project, I would work on a project as long as I could get the funding or somehow make it work, but I wasn't at the mercy of the higher-ups deciding to stop that project and start something else, and I'd switch into something else. So, I think that was good for a peace of mind. I just loved the work.

And St. Louis had a lot to offer in terms of . . . the art museum was free; it was right close to the campus. They had a halfway decent symphony. You know, so I could take some time out to do things like that. And I met some really interesting people. We worked on breast cancer tissue repository. We even did some work with Genentech on Herceptin. So, you know, it became, you know, known that we had this mapping facility, and it worked out real well.

JONES: And do you feel like you took the mapping further there than you could have done at . . . ?

DONIS-KELLER: Oh, hugely. Oh, absolutely. And, you know, it didn't **<T: 105 min>** make sense for Collaborative to keep putting money in because it wasn't . . . they weren't getting return on the investment, and with these limited R&D partnerships they want, you know, two times payback in three years or something. I mean, they, they didn't get that. So, it made sense for them.

JONES: And you maintained your connections with all these other academics working in the field? How was that . . . ?

DONIS-KELLER: Yeah, I mean, then it was easier, so much easier to form collaborations. You just agreed amongst yourselves and you just, sort of, talked over what the, what the ground rules were and how you were going to do it and you did it. There were no collaboration agreements, no lawyers. It worked very well. We got a huge amount done. It was just like a load being lifted off. And, you know, as time went on, all these companies became much more accepted and everyone was working with companies too, so my past didn't . . . wasn't seen as a negative either.

JONES: Right. Did you have much involvement with the human genome project as that was . . . ?

DONIS-KELLER: Oh, we got a human genome project grant—a big one—and that was . . . David Schlessinger was the PI on that, and I was a co-PI for the linkage mapping.

JONES: Okay, you were . . .

DONIS-KELLER: And Maynard Olson was for the physical mapping. Yeah. So, we got a big . . . yeah.

JONES: So, that . . . did you feel like, "Oh, I've got plenty of money to do whatever needs to be done"?

DONIS-KELLER: We did.

JONES: Yeah, that's a nice . . .

DONIS-KELLER: We did. It was a very good situation to be in in those days, yeah.

JONES: So, you were there until 2001. That's about the time the genome is . . .

DONIS-KELLER: Well, actually '98.

JONES: Oh, '98?

DONIS-KELLER: Yeah. I mean, I was officially . . . I took a leave of absence, and I think the maximum is like a three-year, so I was still officially a professor there. But I think in '97, I was eligible for sabbatical. This is a novelty that you can actually take a sabbatical. I didn't want to go anywhere because I had grant applications due, so I decided I would go across to the Hilltop campus and take some art courses. And I had never really given up art all the way through.

JONES: Had you done . . . had you worked on stuff?

DONIS-KELLER: I had done some photography, and even when I was here in Boston, I would take a figure drawing class or a pastel class or something just to have something, some connection, some thread to art. So, when I was on sabbatical besides rewriting grant applications, I went over and took some drawing courses and printmaking, and I made some

really good friends over there. It was just . . . they were really very welcoming, very interesting people. And an artist photographer, Catherine Wagner, actually came to my lab to do her science project, and through her I met some people as well. And when that sabbatical was over, I just didn't want to give up art. I just didn't want to give it up. It filled some need or something, and so I just decided I was going to go and get a master's in fine arts. You can teach with a master's, and I wanted to really be immersed in a company of artists. And so, this was just, you know, outrageous for most people to think. So, yeah, I closed the lab. I had a huge lab.

JONES: Did it take . . . was it trouble to close it down? I mean, you've got a lot of people there, I imagine. You have to place them, and, you know, do all that stuff. Did it . . . ?

DONIS-KELLER: They all got jobs. At the height of my lab, I think I had forty people all together.

JONES: Wow, that's—

DONIS-KELLER: Graduate students, postdocs, a lot of technicians because we were doing our own oligo synthesis, and we were doing our own cell culture. We were doing essentially everything. And I had my own dishwasher people and media people and yeah, so it was a big lab. I think that last year of sabbatical and the year before that when people finished up and left, I just didn't bring other people on. So, the lab was starting to contract. And then, I was just starting to think about "Well, what should I do next?" I mean, the linkage map was essentially . . . just really no point in doing more, and the physical map was really developing pretty nicely. I didn't see a place for myself in sequencing. It just . . . . I mean, that was Waterstone and that was all, sort of, mechanized. And I wasn't at all interested in the computational side, which is really the most interesting part, but I didn't have the background. So, I was really, kind of, thinking, "Well, you know, what should I do? Maybe I'll work on another disease or something."

So I was, kind of, at a crossroads too, so I think that played into the thinking. And I think I would have had to have started something new. I would have had to do a sabbatical and go somewhere and learn something and then come back **<T: 110 min>** and essentially, kind of, start things up in a new way, which is what people do all the time. But I just loved the art. And, you know, there's some point in your life where you start to think . . .

JONES: If not now . . .

DONIS-KELLER: "Maybe this . . ." If not now . . . I was close to fifty. If not now, when? So people were just stunned that you could give up tenure and close down the lab. [laughter] But it

was, sort of, like a yard sale, only nobody had to pay for anything. So people just came, "Oh, yeah, I'll take that freezer. I'll take that refrigerator." It was, kind of, fun. We gave everything away. [laughter] I mean, technically, the university owns everything, but they just let us decide who would get what, so we gave it all away. And every single person got a job, all the postdocs got placed, and I was happy about that. So, that was good.

JONES: And then you elected to return here.

DONIS-KELLER: Yeah. By this time, I had gotten to know . . . I had known Boris for more than twenty years. He was on the board of tutors at Harvard when I was on the board of tutors. So, I got to know him and his wife Adele. And through David Botstein, I had gotten to know quite a number of people in the biology department [at MIT], so I would go to their parties and all that sort of thing, and Boris' wife died of cancer in 1990 or '91. By that time we were very friendly. They would come for Thanksgiving and, you know, I really liked Boris and Adele. They were part of our circle. And then I just became closer and closer friends with Boris, and we got married in '95 . . . '96. Ninety-six. And I just commuted, you know? He would come out to St. Louis about once a month or I would go there. There are a lot of advantages to having that kind of relationship. [laughter] It works well because you can still work like crazy, and then you go for a weekend or a long weekend and you just, kind of, enjoy each other. It's, kind of, perfect. But as time went on—as you can see, Boris is a good bit older than me—he was having some difficulties with falls and things, and he really needed me to be there. So I decided . . . I applied to the Museum School [School of the Museum of Fine Arts at Tufts University] and to MassArt [Massachusetts College of Art and Design] and decided to go to the Museum School here in Boston. And so we bought this house and renovated it, and I moved back here.

JONES: Very nice. You've got a nice . . .

DONIS-KELLER: It's a good old house.

JONES: A very nice space here.

DONIS-KELLER: Yeah. We had a nice architect, and it's a nice little—

JONES: Is that a park or is it . . . what is . . . ?

DONIS-KELLER: It's Sacramento Field; it belongs to the City of Cambridge.

JONES: I see.

DONIS-KELLER: And so, they do soccer and baseball, and people walk their dogs there.

JONES: Well, that's nice. That life.

DONIS-KELLER: Yeah. Well, this house was owned by Harvard, and they owned the one next door, and I think a couple more when they were in the real estate business. And what they wanted to do was to change this whole area into graduate student housing, and they wanted Sacramento Field. And the City wouldn't give it to them, so then, you know, time passes, and they decide to get out of the real estate business, and they sold this house to us. So, they still have the right of first refusal. But they sold all the houses in this area. So we got this house. A good deal. It was six apartments. It had been broken up into six apartments, and we got it renovated.

JONES: Was that, a big deal to undo that?

DONIS-KELLER: Major. Oh, yeah. Took it down to the studs. It was . . . Moved stairways. It was really fun. But it brought the house back to what . . . a single family house and tried to preserve as much of the character as possible. So, it was good.

JONES: And your art? You got in . . . what's . . . can you describe it? Can you . . . ?

DONIS-KELLER: Yeah. Well, you know, I think all artists draw from their background and training, and, for me, it was science. So, the first . . . I always loved drawing. I just love drawing. And the first project was actually what I started on in sabbatical: the genotype/phenotype. And I wanted a metaphor for the relationship between genotype and phenotype, and it seemed to me self-portraiture would be a good one because as people grow, they change. There's, you know, all these different sort of views of a person and what the person is, and those could be similar to phenotype differences. So I then needed a beginning image just to mutate from, and so **<T: 115 min>** I just chose my Sam's card picture because it was like a mug shot. I decided I didn't want anything that's flattering, you know, although I probably would have. But anyway, so I took that and scanned that at different resolutions and then using the facilities at Wash U's printmaking, I made aluminum plate lithographs, and I would just print different resolutions on top of each other and make all these sort of different versions of myself. And I made this big installation that you see on the website, and I think I made 176 different Helen heads. And, you know, I started weaving them and cutting them and

just doing all kinds of manipulations. And I'm still working . . . I still do them. So, they're all woven now, and I've started making some bigger ones.

But that caught the attention of . . . oh, there was some interest in sort of genetic art, and I was in a couple of shows. There was one in New York. And Karen Sinsheimer, who was the curator of—in Santa Barbara, [California]—Santa Barbara Museum of Art, got interested in the work. And she invited me to come out there because she was . . . there was [an] art collector who might have been interested in the work. And so, I went out there, and I took, you know . . . I know, that's what it was. She was organizing a show, and I was going to be in it. And she took me over to meet this art collector, Howard Stein, and he loved the work and he bought it. I hadn't even gotten my master's degree yet, and he bought the whole thing. So, that was pretty exciting. And so, it's been shown . . . now I think it's in his warehouse. He has tons and tons of art. But yeah, so that was the first art/science thing.

And then, I did . . . I dabbled a little bit in videos, and I made a video about art and science. And I think I'll go back and work on that some more when I get another sabbatical. And I've, kind of, moved on from the direct genetic metaphor-making, and I'm really interested in . . . my work has always been pretty much photo-based, and so now I'm working on a project of extreme environments that are at boundaries of tectonic plates. So, Iceland is one of those places—

JONES: So, are you . . . ?

DONIS-KELLER: And Death Valley is another one of those places.

JONES: So, you're making trips to Iceland and going up to Death Valley, [California], and . . . ?

DONIS-KELLER: Yeah, up to Death Valley. I've been to Death Valley now twice? Three times? Twice, I guess. And to Iceland three times.

JONES: Yeah, Iceland. I've never been to Iceland, but it looks like a fascinating place.

DONIS-KELLER: It's fabulous.

JONES: And the whole middle of the island, or the—

DONIS-KELLER: It's uninhabited—totally. It's amazing. Yeah, it's . . . you know, you see glaciers and steam vents, and it's always changing, so it's also, kind of, a metaphor for stability and change. And so is Death Valley. It's . . . there's been, you know, so many upheavals in Death Valley. And it once was very volcanic, and the geology is really, really interesting. And it's, you know, another desolate looking place. Iceland can look quite desolate as well. So, I like the mood that I feel when I'm there and what I hope to capture. So, I've done a little portfolio book of Iceland and Death Valley. I have some images of that, and I'm working on a larger portfolio book now.

JONES: Well, we'd love to put some of that in the magazine. I mean, that would be . . .

DONIS-KELLER: Great.

JONES: Yeah, if it's okay with you.

DONIS-KELLER: Yeah.

JONES: And maybe we'll find a—

DONIS-KELLER: Let me show you the book.

JONES: Okay, yeah.

DONIS-KELLER: I'll just show you the book real quick. You can . . . [background noise]

JONES: This is interesting: thoroughbred horse paternity. Do you remember that one?

DONIS-KELLER: Oh, yeah. Oh, that's a funny one.

JONES: Whose idea was that? **<T: 120 min>**

DONIS-KELLER: Oh gosh. I don't know who came up with that in the first place. But I think actually the jockey club, you know, because paternity matters so much to them. And so, I didn't

go on that trip, but they went down and talked to these people, and they weren't interested at all. They had no interest in DNA.

JONES: Really?

DONIS-KELLER: Nope.

JONES: They hadn't figured it out yet?

DONIS-KELLER: I think they thought that there was more potential for fraud in mixing up samples and things. It never went anywhere. So yeah, actually, a couple of us went out to some horse farm and collected some blood from horses. They put this thing to pinch the nose, and they get blood right out of his neck. I was like, "Whoa." [laughter] But yeah, that didn't go very far.

JONES: Yeah? How do you select these images? How many images did you . . . ?

DONIS-KELLER: Oh, a lot.

JONES: Yeah?

DONIS-KELLER: A lot. Yeah. But . . .

JONES: Can . . . ?

DONIS-KELLER: You know, you can just . . . when you're . . .

JONES: You can just see it?

DONIS-KELLER: Reviewing your work at night, you can just see which ones have potential.

JONES: It's just the eye or whatever.

DONIS-KELLER: Yeah, yeah. I love Iceland. It's . . . I've got to go back.

JONES: Well, maybe we can do . . . would you want to do something? Should we do Iceland? Should we reproduce some of these things for . . . ?

DONIS-KELLER: Yep. Or, we could do . . . well, Iceland and Death Valley.

JONES: Yeah, yeah. That sounds great.

DONIS-KELLER: Yeah.

JONES: And this is the inner part of the . . . ?

DONIS-KELLER: Yeah, that's at Central Highlands. And that's called Villains Falls, and the story is that they don't have any trees there, and so they would toss the villains over the falls. That was the story. [laughter] It was an amazing place.

JONES: But nobody lives there, right? Or did they have outlaws?

DONIS-KELLER: It's very sparsely populated.

JONES: Yeah?

DONIS-KELLER: Yeah. I think probably 90 percent of the population lives in Reykjavik, [Iceland].

JONES: Right, yeah.

DONIS-KELLER: There's some . . . a little bit of farm—

JONES: Which is not a big town.

DONIS-KELLER: No. You can just walk it pretty easily. The people are really interesting and nice there too. Well-read. It's just an amazing place.

JONES: Well, it's so different than . . . well, I'm sure it's unique in a lot of ways. So, this is Death Valley.

DONIS-KELLER: That's Death Valley. Yeah, that's Zabriskie Point.

JONES: Yeah, that is definitely earthquake topography, that . . . I love to fly over these areas and . . .

DONIS-KELLER: Oh, yeah.

JONES: But, you know, millions of years, how many earthquakes does it take to, you know, produce that?

DONIS-KELLER: Yeah.

JONES: Or maybe it was . . . maybe there was some, kind of, volcanic origin to it now.

DONIS-KELLER: Yeah, the shifting of the plates and pushing old rocks up and new ones down. Yeah, that's Salt Creek. Yeah, I just went to Death Valley in January, and I haven't had a chance to work up any of those images yet, but I've got some pretty . . . ones I'm pretty excited about.

JONES: And this looks like it's been treated somehow.

DONIS-KELLER: No.

JONES: No?

DONIS-KELLER: It's just . . . the colors are just unbelievable.

JONES: That is, like . . .

DONIS-KELLER: That's a real sunset that was just stunning. Yeah, I guess the air is just so clear. I don't know. It's amazing. And the rocks are, you know, they've got all those different colors from the various metals in the rocks. There's one place called Artists Palette that has, you know, all sorts of just manganese and all kinds of different colors. That's Titus Canyon. That has a polarizing filter, so the sky's so blue. That's Artists Palette. It's amazing. The rocks are just those pinks, and it looks sort of coppery and . . .

JONES: And there's a green and blue.

DONIS-KELLER: Yeah. And green, yeah.

JONES: Bluish-green.

DONIS-KELLER: It's amazing.

JONES: Now it would be good . . . maybe we could . . . we'll call you up on the phone and get some statements about the photos and . . .

DONIS-KELLER: Sure.

JONES: You could help us with that.

DONIS-KELLER: Yeah, I can send you some low-res images that you can just then use.

JONES: Well, hi-res images would be better for—

DONIS-KELLER: Oh, okay.

JONES: Printing in the magazine.

DONIS-KELLER: Oh, you actually print it? Oh.

JONES: Yeah, we do print it.

DONIS-KELLER: I just thought it was like a web magazine or something.

JONES: No, no. No, we print it.

DONIS-KELLER: Oh, really?

JONES: Yeah.

DONIS-KELLER: Wow. Wow, that would be cool.

JONES: Yeah.

DONIS-KELLER: Nice, yeah.

JONES: So, yeah, we could make it look good.

DONIS-KELLER: Okay. Hmm. Wow.

JONES: Very good.

DONIS-KELLER: Okay. All right.

JONES: Well, thank you so much.

DONIS-KELLER: Sure.

JONES: Is there anything else we need to cover?

DONIS-KELLER: No, I mean, the **<T: 125 min>** biotechnology part was like a ten-year period.

JONES: And at the . . . your position as professor here, I have biology and art. Is that . . . ?

DONIS-KELLER: That's right. Yeah, that was another, sort of, lucky break. you know, in most academic settings you cannot be in two fields like that. It's just . . . they just can't do it.

You can have a joint appointment—you know, genetics and then in psychiatry or something where it's, kind of, related. But so, when I finished my master's, I really didn't know what I was going to do. I really didn't want to go back and start a lab up again because I'd been out of it for three years, and I just didn't want that life anymore. So I looked around and on the web I saw that this new school, Olin [College of Engineering], was advertising for artists-in-residence. So, I thought, "Well, I'll do that. You know, that's a good way to sort of get started and, sort of, think." And so, I put in my application. This was, like, I'm in my last six months of my master's. And then, I didn't hear anything from them. I mean, I don't think I even got an acknowledgement. And so, then I looked later, and they were advertising for somebody to teach biology. So I thought, "Well, I can do that." So, I just changed my cover letter and sent the thing in. [laughter] And I got a call from them and they were actually interested in people who do more than one thing. They were looking for, you know, creativity or people who have more than one dimension. So, yeah.

So, I teach drawing. I teach digital photography. I used to teach video, but can't really afford to do video there anymore, so I haven't done video in quite a while. And I teach introductory biology, and I teach some upper level genetics. Oh, and I got to teach this really interesting engineering course called "User-Oriented Collaborative Design." And it's a course that all sophomores have to take, and it, sort of, harkens back to my design training. And a group of students, a team will select a user group that they want to develop a paradigm-shifting product or service for, and they go out and they do research. They interview users, they come back, they do all sorts of ideation techniques, and they come up with these amazing products or services. They don't take it to the prototype stage; they just take it to the idea stage and show that they've . . . that it would be feasible, say, in a ten-year time frame. So, it was really, really interesting to teach that course. I've done that—it's a team-taught—there are six instructors since we have, you know, the whole sophomore class. So that's been very interesting to teach as well.

So, it's a small school. We just graduated our eighth class, I think. We just got . . . I was signed on there in the summer of 2001, and we graduated our first class in 2006, I think, because the first year was called a partner year, and we recruited a set of students who . . . to, sort of, help us define the curriculum. It was another, you know, totally new and innovative thing to work on. And it's done very well.

JONES: Good.

DONIS-KELLER: The school's done well.

JONES: And teaching was something that you . . .

DONIS-KELLER: I had never done before, no.

JONES: Yeah? How did . . . ?

DONIS-KELLER: So, it was hard.

JONES: Well, yeah.

DONIS-KELLER: Yeah. It's . . . and our school, it's not like you develop multiple choice tests or anything. It's very project-oriented, and there's a lot of hands-on, a lot of working with students.

JONES: Where do the students come from? It sounds like—

DONIS-KELLER: Every state in the union except . . . I think now we have every state in the union. We were lacking North Dakota, but we got North Dakota, Hawaii—all over the place. And some from Europe.

JONES: And they come because of the, sort of, unique curriculum?

DONIS-KELLER: Uniqueness. Yeah, the project-oriented nature of the school. We had a full scholarship for our first class, the first several classes, which was a pretty good draw and really gave us ability to choose the very best students. So, you know, they graduated with zero debt. but then we had to—with the stock market crash and our endowment not going anywhere—we had to start charging tuition. So, we charge half-tuition now. And then if students need it, we help them make it up with loans.

But yeah, it's done . . . the school's done remarkably well. We just won the highest, engineering honor . . . the executives, the president and the vice presidents from the Engineering Society just . . .

JONES: How did it get started? Whose idea was it?

DONIS-KELLER: There was this Olin Foundation, and all the people on the board of trustees **<T: 130 min>** were, I think, essentially lawyers, and they would fund an Olin building on this campus or that campus. [Franklin W.] Olin was an engineer and entrepreneur who developed a way of making gunpowder, keeping it dry or something like that, and he made this foundation. And the chair of the board of trustees, Larry [Lawrence W.] Milas, was starting to get older and was not satisfied with just making an Olin building here or there, and he just decided he wanted to make his own engineering school, college, and he wanted it to be freestanding. He went and looked at a number of different places where they could have, you know, some add-on to existing places, but he decided he wanted a freestanding one. So, the foundation, incorporated here in Massachusetts. He had a connection to Babson College in the past, and so he bought some land from Babson. That's why it got located there, and I think the charter was formed in 1997. And we offer mechanical engineering, electrical and computing, and then a basic engineering degree where you can sort of configure your own engineering degree. So, it's small. We have a total of almost four hundred students. It's a four-year undergraduate institution.

JONES: Well, that sounds like fun.

DONIS-KELLER: It is. It is. Yeah, I love teaching drawing there. I get students who have never drawn before, and it's amazing what they can do.

JONES: Yeah. Have you trained any biologist artists, people who are like double majors?

DONIS-KELLER: Not so far. No, not yet. But I think by example it shows them that there are lots of opportunities for a person. And the students we get are interested in a lot of things besides engineering. They have . . . we actually fund and sponsor things called "passionate pursuits." And so, they write a proposal; they can get some funding. For example, one student

wanted to learn the trapeze. So he did that. [laughter] Other students do oil painting, or, you know, one did . . . oh, there's this, kind of, silk . . . you get silk and you can suspend yourself. It's some sort of . . . it's like gymnastics in a way. It's really hard. So different . . . dancing, all . . . glass blowing, all sorts of different things. But our students have varied interests.

JONES: Yeah. Yeah. It sounds like a very human place—

DONIS-KELLER: It is.

JONES: Where you can—

DONIS-KELLER: It is. And small. Everyone knows each other. We take care of everybody. So yeah, they seem happy. We just don't have enough alums yet to give us money. [laughter]

JONES: Well, that takes some time, right? Yeah.

DONIS-KELLER: Yeah. But our students have been successful.

JONES: Good. Yeah.

DONIS-KELLER: Yeah. A lot of them go to Microsoft, lots of big companies, and then they make their own start-ups as well. So . . .

JONES: Well, maybe it won't take too long then for them to—

DONIS-KELLER: Let's hope.

JONES: Start coming back.

DONIS-KELLER: But, you know, the . . . I guess my thing that I try to instill is just an appreciation for art and sponsoring art and realizing that, you know, professional artists work very hard and having respect for that and for what they're trying to say. So, yeah.

JONES: Well, also the . . . it's just . . . the same thing is true of history. I mean, we're trying to promote history. We do the history of science. But how do you impress upon people the importance of it? You know, it's . . . well, think about a world without history. Think about a world without art.

DONIS-KELLER: Yeah, really.

JONES: You know, it could be . . . it really is *really* important, you know?

DONIS-KELLER: Knowing the foundations in science too.

JONES: Thank you, Helen.

DONIS-KELLER: Very good.

JONES: It was wonderful.

DONIS-KELLER: Okay, it was nice to talk to you.

JONES: So, a number of things. We will get a transcript and get it back to you for review.

DONIS-KELLER: Okay.

JONES: I'm thinking when I go back to San Francisco, [California], I'm going to have Breanna Rego, who is one of our research associates, she'll give you a call and we'll get the . . . see if we can get a magazine thing together.

DONIS-KELLER: Great.

JONES: And if you have . . . I'm sure there's lots of great stuff in there.

DONIS-KELLER: Oh, I have lots of stuff, yeah.

JONES: So, maybe we can come back and make arrangements.

DONIS-KELLER: Yeah.

JONES: I'll have somebody call about that.

DONIS-KELLER: Yeah, this has really motivated me to finally get this stuff in order.

JONES: Well, good. Yeah, I think, you know, it's important to . . .

DONIS-KELLER: It's, kind of, nice to see how do these things sort of develop, you know? **<T: 135 min>** There's sort of landmarks here and there, but how did it come to be, I think that is an interesting process.

JONES: Yeah, absolutely. And, you know, a lot of important stuff has been done. How do you feel about the . . . you know, you did this important mapping work and people are, you know, trying to develop . . . ?

DONIS-KELLER: I feel really good about it. When we first did the cystic fibrosis, that was my first kind of connection with patients and families, and just the appreciation they have for when you find out something that somewhere down the line is going to help them, their health. It's a tremendous feeling when you feel like you really did something that was worth it.

JONES: Absolutely. That's why I think, you know, people are doing that kind of work, important work where you record it. "This was done. This is how it was done." And people are interested in that.

DONIS-KELLER: Yeah. And also, I think . . . I looked on your web page of the oral histories and things. It was all men. There's just—

JONES: It is, it is. That's right.

DONIS-KELLER: So few women. It was awful.

JONES: Absolutely. Yeah.

DONIS-KELLER: I know I recently got a book from Dave [David A.] Micklos, *DNA Science*, from Cold Spring Harbor. There are, like, two women mentioned. There *are* women—

JONES: I think it's changing slowly.

DONIS-KELLER: Yeah.

JONES: It's changing slowly, but it will take some time. I think, you know, in biology today more than half of new PhDs are women.

DONIS-KELLER: Yeah.

JONES: So . . . and in San Francisco, you know, we've got Sue Desmond Hellmann. She developed Herceptin; she's chancellor of the university [University of California San Francisco] out there. You know, so . . .

DONIS-KELLER: Absolutely.

JONES: So, people are getting there.

DONIS-KELLER: Yeah.

JONES: It's—

DONIS-KELLER: Slow.

JONES: Yeah.

DONIS-KELLER: But still, the people writing the books are not that sensitive to, "Well, maybe we should include, you know, Mary-Claire King in this *DNA Science* book." And she's not, you know?

JONES: Yeah, well, I don't know how to account for that. Actually, we were thinking about doing a magazine story on the . . . on Las Abuelas. The . . . do you know that story?

DONIS-KELLER: No.

JONES: She went down to and I guess it was the seventies in Argentina—

DONIS-KELLER: Oh, in Argentina.

JONES: Yeah.

DONIS-KELLER: Yes, I teach from her—

JONES: Yeah?

DONIS-KELLER: Yeah. That was amazing.

JONES: It's a great story.

DONIS-KELLER: Oh yeah.

JONES: So, we want to do something on it in the magazine.

DONIS-KELLER: That's terrific.

JONES: You know?

DONIS-KELLER: Yeah.

JONES: Yeah. So, that's the kind of—

DONIS-KELLER: And it continues. I think she's probably still doing some things with . . .

JONES: I didn't know that. I should probably call her up and . . . yeah.

DONIS-KELLER: She has a very nice paper that was dedicated to a former mentor. They had, like, a symposium, and she wrote a paper that's really, you know, something that I think almost anybody could understand. I'll search that out because I teach that in my "Intro to Biology" class. And it really has a good effect on them. They realize, "Well, this DNA stuff, you know, can really be important."

JONES: Yeah.

DONIS-KELLER: So, that's good. Oh yeah. Yeah, she's terrific.

JONES: Yeah. Thanks very much.

DONIS-KELLER: Sure. This was good to talk—

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

PUBLICATION LIST

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MUSIC/SOUND COMPOSITIONS AND SPONSORED VIDEO WORK

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HDK Artwork Piece Used to Illustrate Article on Human Genetics in German Newspaper, Die Zeit, November 2, 2000
HDK Music Composition (Excerpts from Erehwon Variations 1999) Used in "Paradise Now – Picturing the Genetic Revolution", A Video by Kathy Brew and Roberto Guerra, Mosquito Productions. Exhibited at Exit Art, New York, NY, 9/2000
HDK Artwork Illustrations for scientific paper by Alistair MacDonald and Rolf Sattler. Floral development of Myrica gale and the controversy over floral concepts (1973) Canadian Journal of Botany DOI: 10.1139/b73-251 https://www.researchgate.net/publication/249543331