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Duke University

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- Archives associated with the Human Genome Archive at Georgetown University.²

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¹ The Genentech Center at Cold Spring Harbor Laboratories was established in 2006 with a gift of $2.5 million from Genentech, commemorating the 30th anniversary of the company’s founding. The mission of the Genentech Center is to identify, acquire, preserve, promote, and provide centralized access to the original papers, correspondence, and research materials of the individuals and institutions that were crucial to the development of molecular biology and biotechnology.

² The Human Genome Archive at Georgetown University was established in 1988 under a grant from the National Science Foundation, and was long associated with the National Reference Center for Bioethics Literature and other international resources supported by the National Library of Medicine and other components of the National Institutes of Health.
Interviewee Information. Please list an address where we can contact you.

Full name: John Faulkner Morrow
Current institutional affiliation: Retired from Kaiser Permanente Redwood City, CA.
Street Address: 73 Valley Club Circle, Napa, CA 94558-2058.
Phone: (650) 465-6970
Email address: johnfmorrow1@gmail.com

All interview dates including: 4/6/2013

Interviewer Information.

Full name(s): Stephanie Chen
Affiliations(s): Duke University

I, the undersigned, have read the above, and I AGREE to release my interview materials, subject to any restrictions listed below:

(A) √ I place no restrictions on my interview materials.

OR

(B) My interview materials may be reviewed, used, and quoted by the researchers affiliated with the Center for Public Genomics, Duke University; and in addition (check all that apply):

___ Researchers unaffiliated with the Center for Public Genomics may read the interview transcript and any related documents only after obtaining my permission.
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___ Researchers unaffiliated with the Center for Public Genomics DO NOT HAVE my permission to read or quote from the interview.

Posting interview materials to public digital archives: In spite of any restrictions listed above, I give permission for my interview materials to be made publicly available on the Internet by deposit in an institutionally affiliated archive:

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Please specify any further restrictions in the space below:

None.

Signature: John F. Morrow
Date: 9/25/2015
Protocol 1277 Informed Consent Statement – Last revised October 2009

The information I am about to give you and your response will now be recorded.

My name is Stephanie and I am a researcher at Duke’s Institute for Genome Sciences & Policy’s Center for Genome Ethics, Law and Policy. I am part of a team studying several aspects of genomic technologies.

The goal for these projects is to produce publicly available information, usually by publishing one or more scholarly articles or chapters. We intend to post some of our findings on a public website.

You have been selected for an interview. Your participation in this interview is strictly voluntary, and you may withdraw your consent to participate at any time. You do not have to answer every question asked.

The information that you provide will be “on the record” and attributed to you. If you want all or part of your remarks not to be public, we will turn off the recorder and not include that material in the public archive unless you give your permission.

This interview is being recorded and I will take written notes during the interview. These audio files are posted on a secure server and notes will be kept in a locked cabinet and will be available only to myself and other key personnel working on this project, unless you give us permission to make them public (in which case, we would send a transcript to you asking for permission to post it and/or the audio file).

One risk of this study is that you may voluntarily disclose identifiable information—for example about proprietary technology—that later could be requested for legal proceedings, such as patent litigation. I ask you to take this into consideration when you are speaking. The main benefit of participating in this study is ensuring that your side of the story is properly portrayed in the history of these seminal genomic technologies.

To help us protect your privacy, we have obtained a Certificate of Confidentiality from the National Institutes of Health that applies to the material we do not have your permission to post. With this Certificate, we investigators cannot be forced to disclose information that may identify you, even by a court subpoena, in any federal, state, or local civil, criminal, administrative, legislative, or other proceedings. The researchers can use the Certificate to resist any demands for information that would identify you.

The Certificate cannot be used, however, to resist a demand for information from personnel of the United States Government that is used for auditing or evaluation of federally funded projects or for information that must be disclosed in order to meet the requirements of the federal Food and Drug Administration (FDA).

A Certificate of Confidentiality does not prevent you or a member of your family from voluntarily releasing information about yourself or your involvement in this research. If an insurer, employer, or other person obtains your written consent to receive research information, the researchers may not use the Certificate to withhold that information.

Dr. John Morrow, do you agree to the interview? Yes.

John T. Morrow 4/6/2013
Interview with John F. Morrow
Conducted by Stephanie Chen
Origins of Recombinant DNA Technology
6 April 2013

Chen: How did you become involved in Cohen and Boyer's recombinant DNA technology team?

Morrow: I started to propose, anyway, to do a project with Herbert W. Boyer when we were at the Gordon Conference on Nucleic Acids in New Hampshire ... which I believe was in the summer of 1973.

Chen: June, I think.

Morrow: Okay, June of 1973. And Herb and I met ... immediately when we arrived, when I arrived, anyway at the conference site, because there was a conversational venue, it was at a prep school, as all of the Gordon Conferences are, and there was lemonade or something that a person could stand there and drink and say hello to the other scientists. And so I greeted Herb Boyer and I said, "Hi Herb, how are you? What's going on?" And he said, "John, we have a plasmid in which we can clone other DNA." And I said, "Herb, what have you cloned so far?" And he said, "Well we have cloned some restriction factor genes for ... excuse me, resistance factor genes for resistance to antibiotics," which are of course, from other bacteria. And I said, "Well you know, Herb, I want to clone animal DNA. That's my goal for the future, actually, and I have some frog ribosomal RNA genes, and I happen to know that it has EcoR1 restriction endonuclease cleavage sites that are an appropriate distance apart, i.e., about two to four ... well they were the appropriate distance apart." And I said, "You know ..." well probably before that I said, "What kind of restrictions on sites does it have?" And he said, "It has EcoR1." And I said, "Well I have a frog DNA that has EcoR1 restriction sites that are an appropriate distance apart." And ... I said, "You know, could we clone that?" And he said, "Well you know, that sounds very interesting. Let's think about that." And he said, "In fact, let's talk about that when we get back to California." So that's how ... it started with Herb and me, and actually his partner at that time, Howard Goodman, with him, Herb had done many projects at U.C. San Francisco, was there at the very same lemonade meeting and greeting place, and he said, "If Herb doesn't want to move forward with the frog DNA, I do. So come to me and we'll do it."

Chen: So did you meet them after you came back here [to Stanford]?

Morrow: I dialed up the telephone and I called Herb.

Chen: Okay. And that's how it started?
Morrow: That was how it started. I can go on from that actually. I dialed up the telephone, and I said, "Herb, I want to clone this frog DNA." And that's about what I said. [Laughter]. I said, "You know, Herb, I won't be here forever. I've finished my thesis research, my Ph.D. thesis research, and I'm writing my thesis, but I have to move to the east coast to work with Don Brown, very soon, and I want to do this. I want to do it now." And I think Herb ... I don't know his exact words, because this was a long time ago, Stephanie.

Chen: Yeah.

Morrow: This was actually 40 years ago. This was in 1973, and that was 40 years ago, so I can't remember the exact words, but I do remember what happened. I called him and I said, "When can we start working on the frog DNA?" And he said, "Well I've been talking with Stan Cohen about that and we're both interested, but we need to think about some other things first, and we'll get together and we'll have an organizational meeting sometime, and I will call you and we'll talk about it." And I think that's about the way it happened.

Chen: How did you become involved with [Donald Brown]?

Morrow: I wanted to be a postdoctoral research fellow with Donald D. Brown at the Carnegie Institution of Washington Laboratory on the Johns Hopkins Campus in Baltimore, Maryland. And I had met Don, and I asked him if I could be a postdoctoral fellow in his lab and he said, "You can be, but we need for you to apply for a postdoctoral fellowship in order to pay for this." And I did that and I received two postdoctoral fellowships, I think exactly two, at least two, from the Helen Hay Whitney Foundation and also from the National Cystic Fibrosis Research Foundation. The one that paid better was the National Cystic Fibrosis Research. [Laughter]. And so I favored that one, and actually I had accepted that.

And you have to write a research project, and of course I had talked with Don, you have to write a proposal, a research project proposal, in order to get such a fellowship funding, and of course I had talked with Don about what sorts of projects might be projected. And you see by this time, Stephanie, Berg's lab had already published that they could join unrelated DNAs together by means of making ends that were cohesive using terminal transfers to one nucleotide or another to one DNA or the other, you know. And they published, Jackson, Sanders and Burke, and I did not refer to the literature to refresh my memory about exactly when they published it. But they had published it already, it was in the public domain.

So I said to Don, "I think we can clone some genes of interest. Which genes would you like to clone?" I probably actually said, "Don, I think what you've been doing with the silk fibroin gene that encodes the major silk protein would be the best thing. If we can clone that DNA, we can start to look at promoter sequences and do interesting things. And we can study anything we want to do about the silk
fibroin gene and the nearby DNA if we can clone it. But in order to study it very well, it's very helpful to have a substantial amount of it in a test tube." And he said okay, and I wrote a cloning project in which actually I proposed to clone the silk fibroin gene, which turned out to be quite hard, by the way. But I was committed actually to exerting myself to the maximum to clone DNA from animals. Further, in order to receive my postdoctoral fellowship stipend from the National Cystic Fibrosis Foundation, and ultimately, by the way, Stephanie, this turned out to be quite productive for the National Cystic Fibrosis Research Foundation, [Laughter] because they have been able to do wonderful things with cystic fibrosis genes.

Chen: But those are animal genes?

Morrow: That's a human gene there.

Chen: Human genes, okay.

Morrow: Cystic fibrosis is actually a huge problem for human beings, and I know a person who has cystic fibrosis, and they have succeeded in making enormous headway in characterizing the genetic abnormalities that lead to cystic fibrosis. And it is very common.

Chen: Yeah, I know a person who has it too.

Morrow: Oh, okay. It's one of the most frequent ... it's a recessive hereditary disorder. So if mom has a copy of the gene, and dad has a copy of the gene, then some of the children will have cystic fibrosis statistically. It's recessive. But it is one of the most frequent hereditary disorders. So anyway, I was committed to doing my very best to clone an animal DNA and that's why I became involved with him, because I had decided that cloning animal genes was a productive avenue of research, and you know, it has turned out that it is. [Laughter].

Chen: Yeah, definitely.

Morrow: Okay. That's why.

Chen: Okay. Did you think about approaching Berg or ... Brown at your postdoc institution for the same work ... or at least Berg ... when you were still in graduate school?

Morrow: I knew that Paul Berg would not be receptive ... the answer, of course, is yes. But that gets us into several more things about the history of this research field. Of course I considered approaching Paul Berg, but it would not have fit in with my thesis project, which was mapping the SV40 genome by use of restriction endonuclease, as that is the title of my Ph.D. thesis. And in fact, this gets me to something else that we should talk about. How did you actually get into this? And I did consider the question before, but I knew that Berg would say no. And what I
proposed, though, in the research proposal for the fellowship funds, was to use the plasmid that was being used, that had been put together by Berg's group, actually, in collaboration with Douglas Berg, who I think is unrelated.

Chen: Yes.

Morrow: Right. They had made λ DV and they had ... genetically selected λ DV gal and you probably know about these things.

Chen: Yeah.

Morrow: Yeah. These things are not so very famous, but λ DV gal has one EcoR1 site, I believe. And David Hogness' lab was attempting to use λ DV gal to clone fruit fly data, and had been doing so for quite a while already by then, maybe two years. They were trying to use λ DV gal to clone animal DNA, because Drosophila DNA was a great interest, and still is. And it wasn't working, but that is what I had proposed to use, because we all thought that it was going to work. And it has been pointed out to me by Janet Mertz that if one had done a partial EcoR1 digest in order to get dimers of λ DV gal, it would have worked. That would be two copies of λ DV gal. You have to somehow make a dimer λ DV gal, I guess. Well she said they could do a partial digest, she thought it would have worked. I think it may occur, it probably occurs as dimers, but it's not easy. That could be done; she has argued more recently that it could have been done that way. But it wasn't working for Hogness' group, but that is what I had proposed to do. And I knew, because I knew David Hogness' postdocs who were trying to do it, and they are smart, effective people, but it was not working and it hadn't worked for a long time, a period of months. I don't think it ever really ...well let's not go into that, but it struck me, and it was like a bolt of lightning out of the sky when I heard Boyer say, "We have a plasmid with an EcoR1 endonuclease site, and we can clone pieces of DNA in that site." And I said, "Hey, that's what we've been looking for." And I had considered approaching Paul Berg, and I did approach Don Brown, and I said, "Don, let's do this." And we got the money to do it. So that's the story of ... the next question is actually even more interesting. I think I finished that one sort of. Yes, I did consider it.

Chen: Yes. And so, Paul Berg is quite prominent in the rDNA controversy. And he already imposed a moratorium affecting Janet's work by 1973. Did his role in that have an effect on your decision?

Morrow: Well it absolutely did have an effect on my decision. In fact, that's one reason why the frog ribosomal RNA genes were a good choice, because they carried for ribosomal RNA of frogs, which we all thought, and I think still think, are quite harmless. So actually that's probably why I got Herb Boyer's attention and Stanley Cohen's attention, and they said, "This is a good choice of DNA to clone because this is not going to violate anything about biohazard risks of recombinant DNA." That's actually why John Morrow got to do the first project, because John Morrow
happened to have some harmless DNA from animals sitting around. And John Morrow knew that it had EcoR1 endonuclease, and at some point we have to talk about how I got to that point.

Chen: Yeah.

Morrow: Don Brown sent me the frog DNA, and he did not send it to me with a request, "John, could you please clone this in Paul Berg's lab?" He would not do that, because that was work for him, it was not work for Paul Berg. Don actually did call me up and said, "John, I know that you have some restriction endonucleases there, in the Stanford Department of Biochemistry. I know you have EcoR1 and I know you have HindIII," which also creates cohesive ends by the way. And he said, "You may have some other restriction endonucleases there because you guys are involved in an industry, really of like mapping SV40 with everything you can get your hands on." And he said, "While you're there, and you have access to the restriction endonucleases at Stanford," one of which I had purified, by the way, the HindIII, I purified it with my own hands, with Janet Mertz, by the way. "While you're there, could you just check it out? Spend like an hour and see." He said, "If I send you some frog DNA," that's when I probably said, "Don, in what condition will this frog DNA be?" And he said, "It will be large. It will be long. And if you can just see if it's cleaved. And you know, a tiny bit of information about the size of the pieces would be helpful. If you can just look and see if it's cleaved, then you would know, and then you could inform us here in Baltimore, where we are not an enzymology lab and we're not a biochemistry program, you can inform us which restriction endonucleases might be productive for us." And he said, "This shouldn't take very long. You can do this by electron microscopy. Just look at the pieces. And I'm not asking you to really do anything substantial. Just see if it gets cleaved or not." And I believe that I did actually go to Paul Berg and say, "Paul, Don Brown proposes to send me a couple different kinds of DNA," and they were the 28S and 18S ribosomal RNA genes, and the 5S ribosomal RNA genes, "If Don sends me a couple tubes of DNA, is it okay if I just see if it gets cleaved by these restriction endonucleases?" I believe I asked Paul that. It was a long time ago, but I believe I did ask him that, and he said, "Okay, but keep it short. Do it fast. You've got to write your thesis. Don't waste a lot of time on this." And that's about all. So that was about the arrangement with Cohen and Boyer also and how I came to have the frog DNA in my refrigerator was that Don had asked me to see if it got cleaved by EcoR1 or Hind III. But that doesn't actually tell about the arrangement with Cohen and Boyer, because that was more complicated.

Chen: So once they contacted you, what was the arrangement with Cohen and Boyer, and how much say did you have in making that arrangement?

Morrow: Well there's more. I don't think it hurts for you to know that while I was working on my thesis, I was writing my thesis, and Stanford University required a real written thesis, not simply a collection of research papers that had already been
published, I had to actually sit down and write a book length thing. And I was doing that. I was sitting around waiting and wondering how long it was going to take Herb Boyer to get around to organizing the cloning of the frog ribosomal RNA genes and ... by the way, I'm using the word frog. Xenopus I realize is not actually a frog, it's more of a...

Chen: A toad.

Morrow: It's a South-African toad, I believe. It's a clawed toad actually, and Don Brown has told me, "It's not a frog. It's not actually a toad either, it's a third thing." But anyway, it's very useful for developmental biology research and they make extra copies of ribosomal RNA genes, which they then proceed to transcribe to make lots and lots of ribosomes for the embryo to have lots of ribosomes. When the egg gets fertilized, then the fertilized egg has lots of ribosomes that it can use to make lots and lots of proteins quickly and take off and grow into a new frog [Laughter] or toad, probably better to call it a toad but people usually call it frog usually.

Chen: Yeah.

Morrow: And that's what Paul Berg still says, he says frog data, so that's why I'm calling it frog data. Okay, before that, I was getting a little bored waiting for Herb Boyer, so I called up Stanley Cohen, and I said ... it turns out Stanley Cohen was on the second floor of the same building where I was working. I was working on the third floor in the biochemistry department.

Chen: Did you know him before this?

Morrow: Yeah, I did know him. He was around. He sometimes came to conferences in the Biochemistry Department in Stanford, and Stanford is a friendly place and the Biochemistry Department at Stanford was especially friendly, and there were lines of supervision and authority, and there was also control over money. It was not like everything hanging out but we were family. And Stanley Cohen by that time, I believe, had already learned how to ... you see, Stanley Cohen's lab worked on plasmids, that was their whole industry, I think. It was a plasmid lab and he had learned how to get DNA into E. coli from Janet Mertz, who was a graduate student of Paul Berg. So Stanley Cohen had been in Paul Berg's laboratories watching Janet and learning how to do this. Yeah, I had met Stan Cohen. And so I called up Stan Cohen and I said, "You know, Stan, I'd like to talk with you about this project on frog DNA." And Herb Boyer told me already. I had presented this to Herb Boyer, but Boyer said this will be a collaboration with Stanley Cohen. Period. He didn't say, if you'd like, we might collaborate with Stanley Cohen. He said, "This will be a collaboration with Stanley Cohen."

Chen: Because of the plasmid that they were using?
Morrow: I don't think Herb and I went into the details of why, but Herb just said to me, "This will be a collaboration with Stanley Cohen." And Herb was, I think probably at that time, an associate professor, and I was a graduate student, and I am ... Stephanie, I think you realize that I'm from the Southeastern U.S. and I try to get along with other people in a productive way, not a shy way, but a productive and cooperative way, and I'm not into arguing with associate professors. Herb said, "This will be a collaboration with Stanley Cohen," and I accepted it. I doubt that I really discussed with him why, but he probably would have said, "Well Stanley Cohen and I have been collaborating on these projects involving the re-assortment of the resistance factors genes for resistance to antibiotics. We've done this collaboration ... and we'd like to continue it." He probably would have said something like that. And I'm not sure actually that we even discussed it. He just said, "This is going to be a collaboration." That's my recollection. And so I didn't argue with Herb, and I also did not tell Herb everything that I was going to do, because he was not my thesis advisor. My thesis advisor was Paul Berg. And I didn't tell Paul everything, [Laughter], particularly at the very moment either, but I did actually, I did communicate with him.

I called Stanley Cohen and I said, "Herb Boyer has told me that any cloning of the frog DNA is going to be a collaboration with you. Can we talk about that?" And he said, "Well okay, John. Can you come to my office." And I said, "Well okay, Stan. When would be convenient?" And he named sometime in the near future. And I went to Stanley Cohen's office, which was on the second floor of the adjacent courtyard, and it was only probably 200 feet from the lab where I worked, and we talked about ... and he said he was not ... I did not actually ask him to do it alone without Herb. I'm not into stuff like that. I accepted that this would be a collaboration of Cohen and Boyer. And I said, "I'd like to be a part of that." Because I had proposed the project. They had no clue about frog DNA. They didn't have any frog DNA. They had no clue about anything regarding animals actually, neither one of them had ever done any molecular biology on an animal. So it was totally foreign to them. It was my idea, and there was no reason I should not benefit from that, in my view. Except for the fact that I was still a graduate student of Paul Berg's. [Laughter].

Okay. But now Stephanie, I think that gets us to a point in this discussion where I can point out that I actually had some conflict of loyalties actually between my duty to Paul Berg to finish my Ph.D. thesis promptly, but I also had a duty, by this time, to Don Brown to try to become a productive postdoctoral research fellow in Don Brown's laboratory. And I think what I effectively did was ... I did really well for Don Brown. [Laughter].

He had sent me some frog DNA, and before I was even a postdoctoral fellow in his lab, I had cloned it and I could produce grams of this stuff - 28S ribosomal RNA genes, 18S ribosomal RNA genes, pure, clipped by EcoR1 one, cloned in bacteria, and a publication publicizing the molecular biology that Don Brown had
done on Xenopus DNA and the ribosomal RNA genes. And he probably never imagined that all of this was going to happen before I even got to his lab.

Chen: Sure.

Morrow: And actually I never imagined that it would happen before I got to his lab until I ran into Herb Boyer and learned from Herb that they had a plasmid that actually had a usable EcoR1 one site. I thought I was going to have to get to Brown's lab and use λ DV gal. I never imagined that this would actually work until I ran into Herb. And, Stephanie, it's not really all that hard to clone DNA if you have what we call a cloning vector. In other words, either a plasmid or a phage to which you can ligate it and then a way of introducing it again into a bacteria where it will replicate. It's not really that hard. It doesn't take very long. It's more... it doesn't take long to clone it, but it takes longer to characterize exactly what you've got.

Chen: When you have clones after in a colony?

Morrow: Yeah, it takes longer to characterize the DNA of the plasmids that you have gotten into the bacteria, because sometimes the DNA from an animal, especially if it has repeated DNA sequences in it, sometimes in different copies of the plasmids it can recombine with each, and some of those, maybe even most of those repeat copies can be diluted by essentially unequal recombination, like if it makes a loop and recombinates, you can lose the codes. So it does take time to characterize it, but it doesn't take very long to clone it.

And anyway, I haven't finished on the arrangement with Cohen. There really wasn't an arrangement, although the initial arrangement was... then Herb Boyer and I talked again, and I said, "I've talked with Stanley Cohen, and he says I can be a part of collaborating on the cloning of the frog DNA." And Herb said, "Well you've got to finish your Ph.D. thesis." And I said, "Yeah, I know that, Herb." Actually everybody said, "John you have to finish your Ph.D. thesis." And I did actually finish it, although it was late, but it was not too terminally late, but it was late. It was delayed by all of this.

The initial arrangement, and I'm having to think about this a little bit. The initial arrangement, I believe, was set up with Stanley Cohen in that Stan and I then talked with one another and eventually he said, "I'm going to meet with Herb Boyer and Bob Helling in my office on a certain date in the near future." And I said, "Can I be part of the meeting, Stan?" And he said, "Yes, absolutely, you can be part of the meeting." And so I went to the meeting and I said, "I'd like to be part of this collaboration." And they probably said something like, "Well we'll see if that's possible or if that can work." They didn't make a real definite commitment to it. So there really wasn't an arrangement initially.

Chen: But if you didn't get them the frog DNA, they don't have this collaboration.
Morrow: That's a fact. [Laughter].

Morrow: And that brings me to actually an important aspect of this, which I'm not sure that Paul Berg actually knows yet.

Chen: Oh, okay.

Morrow: But it turns out that I did not have enough Xenopus ribosomal RNA genes in my refrigerator to clone, or we didn't feel like there was an adequate supply for the whole project. I called Don Brown and I said, "Don, something has come up and do you think you could send me some more of the frog ... of the Xenopus ribosomal RNA gene DNA?" And he said, "Okay, but tell me what ..." he said something like, "Okay, but tell me what it's for." And I said, "Well you know, Don, there is a group in San Francisco, and right here at Stanford, one floor down from our lab, that has a cloning plasmid that they can clone DNA with. Don, I think we can clone this DNA right here, right now." And he said, "I'll send you some Xenopus." But I said, "I don't know whether they will permit me to collaborate with them and be a part of this." I believe that Don said that it was desirable for me to actually be a co-author and to be a collaborator on part of the project so that I would know what was going on actually and I wouldn't be shut out of it, and really to be part of it. And I believe that he said, "If they won't let you be a collaborator, don't give them the DNA." And they knew that they couldn't get Xenopus DNA basically any way other than me. And this was highly desirable stuff, because it was undoubtedly from an organism that did not exchange genetic material with EcoR1, at least as far as we know.

Chen: It's a frog. [Laughter].

Morrow: It's a frog, yeah. The DNA was really far, and also it was really harmless. And also we already knew that it had EcoR1 restriction endonuclease cleavage fragments. So this was very highly desirable. [Laughter]. And also Don Brown was actually one of the leaders in molecular biology on animal genes, I think. I think it's fair to say that. And so he said, "If they won't let you be a collaborator, don't give them the DNA." I didn't have to say that though. I said, "Can I be a collaborator?" And they said, "Yeah, you can be a collaborator." And then I said, "Okay, then I'll give you the DNA." [Laughter]. And these are honorable people actually. They are honorable. I think they are honorable people, and they did actually come through and they fulfilled their commitments, their verbal commitments to me. And that was then actually the arrangement. The arrangement was I could be a collaborator, I'll give you the frog DNA.

Chen: Now are you supposed to do the cloning, or how is everybody supposed to ...

Morrow: Work together?

Chen: Yeah, work together.
Morrow: Okay, at the meeting that we had in Stanley Cohen's office, we talked about that and, Stephanie, an important aspect was how would we recognize the particular bacteria that had gotten the frog DNA, because since this frog DNA doesn't have any gene to select for. And that's actually a key aspect of the contribution that the project made. It was not selectable, and yet we were able to clone it. And we talked about this and what we arrived at was that we would simply use a small access of Xenopus DNA, EcoR1 cleavage fragments, small excess over the amount of the PSC101 plasmid, cleaved with EcoR1, and just ligate them together. And then we would transform, it's called, the bacteria with the PSC101 selection for tetracycline resistance, which is a selectable gene. And it was very possible that the vast majority of those transformants, they were called, would just be PSC101, but we reasoned that probably some of them would have gotten ligated, connected, joined, by the DNA ligase enzyme, so that they would be ligated to the Xenopus DNA, and we reasoned that they would probably replicate the Xenopus DNA fragment as part of their larger plasmid.

So we reasoned that some of the transformed plasmids would probably have Xenopus DNA connected to the PSC101 DNA. And one can screen a large number of plasmids by a quick prep on a small scale of plasmid DNA from the bacteria. I knew how to do that at the time, and I think I could still do it. And then what one does is one digests the DNA, the plasmid DNA from the transformant. You know it's a transformant because it's tetracycline resistant, but you don't know whether it has just PSC101 or whether it has something else in it.

Chen: Okay.

Morrow: And the way you find that out is you purify the plasmid DNA, which is work, and then you incubate it with EcoR1 restriction endonuclease to cleave it into whatever fragments it will. And then one electrophoresis it, and in those days an aragose gel, and one stains the gel with ethidium bromide, which makes the DNA fragments fluorescent under an ultraviolet light, and one photographs it using the fluorescence of the ethidium bound to the DNA fragments, and we did that. And a significant number ... I think actually most of the ... I'd have to go back to the paper to refresh my memory of exactly how many ... but it was not hard to find transformants that had frog DNA sized fragments.

Chen: They were bigger.

Morrow: Yeah, bigger, yeah. The plasmid itself was a bigger circle, and then when incubated with EcoR1. One fragment was the size of the PSC101 DNA, and another fragment was a different size.

Chen: Yeah, they're smaller.
Morrow: Yeah, and unfortunately, those were smaller, that's right. They were smaller than the PSC101. Unfortunately, the frog, the Xenopus ribosomal RNA genes when incubated with EcoR1 restriction in the nucleus gave rise to really a spectrum of sizes of fragments. It wasn't just two sizes we had hoped for. We had hoped for just two sizes of fragments, but we didn't get that. And that's one thing that made the whole project more difficult. But the transformants, of course all had PSC101 DNA in them, and quite a lot of them, it might have even been most of them, had frog DNA, and that was delightful actually. And that was one of the main things that we were able to express to the other molecular biologists. Typically if we told another molecular biologist, "We can clone anything." We were not really histrionic. But we would say, "We can clone animal DNA." And they would say, "Well how do you do that?" "We ligate it together with PSC101." "Does it have to have an EcoR1 restriction site?" "Yes, it does." "Oh, okay. Well then how do you select the transformants?" "We don't. We just ligate it with a smaller excess of the frog DNA and it turns out a lot of the transformants had frog DNA that they replicated." That was the best surprise factor. It's easy. It works delightfully well. It's easy. And then they would probably say something like, "Does it replicate it faithfully?" And then it was more difficult to answer that question because it was more than one size. So then I had to characterize those and show that they had some relationship, that they weren't deleting huge pieces of frog DNA. That would be bad, right? [Laughter] And they weren't actually. They were cloning it faithfully for many, many generations so that you could produce vast quantities of frog DNA at least as a sequence. Of course it was no longer a frog. [Laughter]. I mean by this time it was bacterial DNA, but it had the sequence of the frog DNA. And I showed that actually by showing that I could make heteroduplexes of the bacterial plasmids. It would hybridize as to actual long pieces of DNA from frogs that I got from Don Brown. And actually, probably one of the best figures in that paper is the one that shows the recounted DNA plasmid DNA, recounted plasmid DNA, heteroduplex analysis and the electron microscope with the long pieces of frog DNA. It was not deleting pieces of frog DNA.\(^1\) It matched and it hybridize d well and it made a nice DNA duplex.

Chen: And did you do all of this, or were they supposed to do some?

Morrow: Okay, the arrangement initially was, "Okay, John, please give the DNA to Herb Boyer, and he will incubate it with EcoR1 endonuclease and ligate it, and he will do the transformation of the bacteria in his lab, and then he will purify DNA from the plasmids," or he or Bob Helling, actually. Bob Helling was working with him as a visiting faculty member. Herb or Bob ... or Howard [Goodman], for that matter, "will purify some DNA from the plasmids and see if it's bigger, and if it has pieces of DNA of the right size to be frog DNA."

And they did that and they came up with some clones that were cleaved by EcoRI endonuclease and it looked like good candidates, but you can't actually publish that. So I gave them the frog DNA. They cleaved it, they ligated it ... by the way, the ligase they used was DNA ligase that I had asked Bob Lehman for.

Chen: Right, at Stanford?

Morrow: Yeah, but I had approached this in the appropriate way. I had asked my thesis advisor, Paul. Actually Herb Boyer called me on the phone, and he said, "John, can you get us some DNA ligase?" This was the ligase they had used for their experiment that they published on joining together the PSC101 and the pieces of DNA.

Chen: So Herb and Paul already had an exchange?

Morrow: It wasn't Herb and Paul, it was me, actually. Herb Boyer called me and asked for the DNA ligase.

Chen: What about for the first experiment?

Morrow: That was for the first experiment.

Chen: Oh.

Morrow: The only way that Herb Boyer and Stan Cohen were able to ligate DNA together was that Herb Boyer called John Morrow on the phone and said ...

Chen: Oh, so you knew each other well.

Morrow: ...Oh yeah, we do. "John, can we have some DNA ligase?" And I went to my thesis advisor, Paul, and I said, "Herb has asked me for some ligase. Paul, would it be okay with you." I said to him, "Paul, Herb has helped us enormously by providing us with EcoRI endonuclease," which Herb and his graduate student, Bob Yoshimori did actually discover, and it was a totally delightful, very important restriction endonuclease. I said, "Herb has helped us enormously. I would like to help him. Is it okay if I go to Bob Lehman and ask Bob for some DNA ligase to give to Herb Boyer?"

And Stephanie, along the way, by the say, I believe that Bob Lehman and his colleagues had worked long and hard and had produced a genetically selected strain of E. coli that produced DNA ligase. So they were actually able to produce, they were able to purify a large amount of DNA ligase. And they were probably the only lab in the world that could do that. Because Bob, I believe Bob went and discovered DNA ligase and he definitely was an expert on it and he had large amounts of DNA ligase and Paul said, "Yes, John, it's okay. Go to Bob Lehman and ask him for some DNA ligase that you can give Herb." And it turned out,
Stephanie, that Bob provided really a lot of DNA ligase. [Laughter]. It was not a tiny amount. Bob was obviously generous. Probably Paul had said to Bob Lehman, and probably Paul Berg, my advisor, had talked with Bob Lehman and said, "Bob, could you please help Herb Boyer, because he has been helpful to us." I mean I don't know actually, but I do know is that I went to Paul Berg and I said, "May I go and ask Bob Lehman?" And he said, "Yeah, it's okay. Go and ask Bob Lehman." And I did, and Bob gave me a tube, which I kept, and I was very, very carefully, I think it was just refrigerated four degrees C. And I think ...I can't remember exactly how the enzyme got into Herb's hands. I think maybe someone came down and picked it up, or maybe I went up there and carried it. I just don't remember exactly how. But anyway, I gave it to him. Bob Lehman gave it to me. I gave it to Herb Boyer. That is the enzyme that led to the Cohen and Boyer paper on ligating ...

Chen: The R-factor.

Morrow: ... the R-factor DNA fragments that use DNA ligase from Bob Lehman, and I'm pretty sure that if we go back and look at those old papers we will see that they credited him for it.

So none of this would have happened, except for the fact that Herb had been helpful, so it was an indirect exchange from Bob Lehman, and I thank Bob for being a brilliant and enormously helpful professor at Stanford. He was on my Ph.D. thesis advisory committee. Bob, thank you. Bob, you were really a wonderful advisor, and I'm very grateful. But the way that Herb Boyer and Stanley Cohen were able to even start doing any of this was they got DNA ligase from Bob Lehman's group, but the way they got it was by phoning John Morrow.

The point was that the DNA ligase that was used for all of these cloning projects was all from Bob Lehman and what was used by Herb at that time was actually from the initial batch that I gave to Herb Boyer when he asked for it. And I'm not trying to establish any debt, I'm just giving credit actually to Bob Lehman for having purified the DNA ligase and having provided it.

Anyway, I gave it to Herb Boyer who still had leftover DNA ligase that he had gotten before the first R-factor cloning project. And he ligated it together and he did the initial steps then showing that there were other fragments of DNA that were not of the same length as PSC101. But you know, plasmids can sometimes delete DNA, so theirs actually could have been pieces of DNA from altered PSC101. The PSC101 can ligate together with itself also and be transformed as a dimer or as a trimer, at least theoretically. Such events do occur in these cloning projects.

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So they had not proven that the extra DNA fragments in the transformants actually had a sequence even slightly related to the DNA sequence of the frog DNA. And I said, "You know, guys, okay, this is a mess because we have a range of lengths and fragments and we don't know what's in those fragments." And I said, "I think we need to characterize these in order to establish what we've actually done." And we talked about that and there was probably a discussion of how we could do that. And I said, "I could ask Don Brown for some radioactive 28S and 18S ribosomal RNA from Xenopus cells that I know he has." Don Brown had Xenopus cells and he was able to label them so that the ribosomal RNA would be radioactive. I said, "We could ... I can ask Don Brown for some of that, and we can see whether it hybridizes to these plasmids we have that may or may not have Xenopus sequences in it.

Anyway, the radioactive 28S and 18S ribosomal RNA was from Don Brown and it hybridize d to the plasmids. Well that was not a really simple pattern either, it was not, but it did hybridize and that's evidence they had the assigned sequence that the extra pieces of DNA and the ... it turned out to be recombinant plasmids, did actually have the sequence that we were trying to replicate in the clone. We had cloned it, and that was evidence, but it's not great evidence, because DNA/RNA hybridization is really crude too.

And then I said, "I think it would be better if I did a heteroduplex experiment where I work to see whether it's a continuous pattern of DNA duplex that's formed between these plasmids that we had that had extra DNA and the frog DNA. It was just fine. It worked excellently. It was a continuous duplex. It was not diluted or recombined. It was a continuous duplex. So that was very satisfactory, but it does work to purify enough of the various transformant genes that had [Inaudible] DNA ... or that had DNA fragments that were different in length from the PSC101 and purify those and then to incubate them with the frog DNA and to prepare nicely for the electron microscope and then to photograph it in such a way that it could be published and to [managerize?] the length of the duplex. So I did all of that stuff.

Chen: So Herb did the initial testing and then you had to basically repeat all it and then do the heteroduplex part?

Morrow: Exactly. So Herb did the cloning of the frog DNA, and then he provided those clones to the Stanley Cohen laboratory and John Morrow then actually did some work in Stanley Cohen's laboratory purifying plasmid DNA from these transformants. And we called them ... we had names for them. We had isolated numbers. I think one was CD43 and one was CD30, if I recall. C was complete digest by the way, because one of the things that we had thought about was maybe doing a partial EcoR1 digest of the frog DNA, but we decided ... I think the CD was for a complete digest.

Chen: Okay.
Morrow: And we felt that it was desirable and important to characterize whether the replication of the animal DNA, which remember, we couldn't select for this, so it was riding along as a passenger essentially in the plasmid. It wasn't doing anything for the PSC101, it was just there. In fact that was the main question we had, whether it would actually stay there and be replicated or whether the plasmid would somehow, maybe slowly or maybe quickly, somehow dilute it and the combination of bacteria can be a very fast process.

And the major question was actually whether it would be replicated stably, and it was. So the project was actually totally delightful, but it was important to characterize whether it was replicated stably and faithfully, and we needed to produce some evidence that what we had cloned was what we had set out to clone. Because after all, that's what one wants. You want to have a piece of DNA to try to clone, and then you want for the results to be very, very similar ... ideally to be identical actually to the sequence that you started with, because that enables you to study the clones and get data that will apply to the material you started with. If it's not faithful, it's not going to be very good, not very useful anymore.

Okay, so that's actually part of what happened during the experiment phase. So Herb cloned it, and then I did most of the characterization of it, I will argue.

Chen: And what did Stan do?

Morrow: Stanley Cohen provided his lab. [Laughter]. Well Stanley Cohen was supportive and ...

Chen: Well he provided the plasmid.

Morrow: Stanley Cohen did provide the plasmid, and the plasmid was not publicly available. And actually that got to be more of a problem actually during this whole project. I'm not sure how much I want to talk about that, but the plasmid was not publicly available. The only way to get that plasmid was by asking Stanley Cohen for it, and he would not say yes if ... well there were times when he said no.

But so the arrangement with Cohen and Boyer was initially that I would give them ... it was very vague … it was just that I would give them the frog DNA and Herb would ligate it together and transform and he did that in San Francisco. That was all done in San Francisco. And he got some plans that had extra pieces of DNA that was not the same length as the PSC101 and all we knew about it at that time was the size of the EcoR1 fragments, that's all we knew. He did get some plasmids that had extra fragments, extra EcoR1 fragments, and then what we did after that was free form. We had not really thought about what we were going to do in order to prove ... in fact, I don't think at that organizational meeting that we discussed at all how will we test whether the transformed recombinant clones,
that's what the ... in fact Stanley Cohen wanted to call them Chimeras, C-H-I-M-E-R-A-S, which is an imaginary, or fictional, or strange beast. He wanted to call them Chimera clones. And I think Herb and the rest of us just called them recombinant clones, because we had combined the two DNAs. We called them recombinant clones. We had not even talked about what we would do once we got the frog DNA sequences replicated in E. coli. What would we do to put together a publishable manuscript? I don't think we had gone there. We were just hoping and praying that it would work, and we were not really sure that it would. And we were actually very pleasantly surprised. Because it was unselectable DNA, you see, it didn't do anything for anybody ever except ribosomal RNA for frogs. And it wasn't in frogs, so you know, it was useless DNA. We were essentially trying to replicate useless baggage.

Chen: Yeah. Useless to E. coli.

Morrow: It was useless to E. coli, yeah. And we hadn't talked about what we were going to do to try to make a complete study. And that's actually how I wandered into trouble with Paul Berg. Because he had said that I could cleave the frog DNA with EcoRI and to tell Don Brown whether it was or was not cleaved. But by the way, Paul Berg was out of town when we began. He was out of town when we did the first work. I think he was out of town when we had the organizational meeting. And I had proposed that Paul Berg would be a collaborator on this project. I did propose it.

Chen: And did they say no?

Morrow: I said, "I can do this if Paul Berg, my thesis advisor, can be a collaborator." And they said, "Well he can't."

Chen: Why is that?

Morrow: Well I said, "Well he should be because I'm in his laboratory, and it would be very convenient to use his facilities." And they said, "No, we do not want for any of the Stanford biochemistry faculty to be collaborators on this project because ..." Stephanie, this is as well as I can remember ... because they had not been part of the initial idea, and it was felt that they were sufficiently famous that if they were collaborators that most of the credit would go to them. So they just said, "No, he can't." And I said, "Well you know, that's not very satisfactory." And they said, "Well that's the way it is. We don't want any Stanford biochemistry faculty members to be collaborators in this project." And also they said, "John, you and Herb put together this idea. Paul Berg was not involved in proposing or imagining this project."

Chen: And neither was Stan.
Morrow: That's a fact too. [Laughter]. But they said, "John, you and Herb had this idea. You cooked up this idea together and there were no Stanford faculty members involved in that." And they said, "He can't be a collaborator." And I said, "Well you know, that's a problem." But they said, "Well now you can either give us the DNA or you cannot." And I said, "Well okay. If I'm a collaborator ... if I'm not a collaborator, you don't get the DNA." I don't even think I had to say that. But I said, "Can I be a collaborator?" And they said, "Yeah, you can be a collaborator."

Chen: Just not Berg.

Morrow: "But not Paul Berg." And so I said, "Well, that's not satisfactory, but if that's the best I can get, then I guess I'll proceed with it. And I will give you the data." And we went ahead. So that was the initial arrangement, and as I have just described, it was not really a totally satisfactory arrangement.

The ideal arrangement would have been for Paul Berg, my thesis advisor, to be a collaborator. And he is a very smart guy, and he would've really been terrific and it would have been a better paper. But we produced a good paper anyway, and he has read it and he told me that he was proud of what we did eventually produce. But he's an excellent collaborator.

And anyway, that's what happened at that phase. And we had not actually considered how we would produce something that was actually publishable. Because the frog DNA didn't have phenotypic qualities. It wasn't an R-factor. It didn't have resistance to penicillin or something. There was nothing we could select. In fact that was part of the thinking, can you replicate animal DNA that you can't select? The answer is yes. And actually that was the entire basis for the human genome project where they were replicating DNA that they couldn't select. And it turned out that you can, yes. You absolutely can replicate with a cloning vector, and we were the first people that showed that actually.

Okay. I'll try to finish, hopefully concisely, what actually happened during the experiment stage. And there were things that happened that were different from what had been arranged. And the resulting paper and the proceedings at the National Academy of Sciences describes the fact that when we incubated the Xenopus ribosomal RNA gene DNA with EcoR1 endonuclease, although it was believed previously that the total repeat length of that DNA was about seven million daltons in length, that actually we observed a number of different classes of EcoR1 fragment lengths, but they clustered around 3.0 million and around 4.0 million, essentially. But there were others like 5.1 million and some that were shorter than 4.0 million, also like 3.9. So it's actually somewhat heterogeneous. It was learned later by the way, and demonstrated by Peter Wellhauer and Donald Brown and their collaborators at the Carnegie Institution that the main reason for this is different numbers of repeats of short repeating DNA sequences in the non-transcribed spacer regions of the ribosomal RNA genes, so there was an underlying biological reason that existed in frogs for the different fragment
length. But we didn't know that at the time. Consequently, it could have been that
the E. coli was deleting some of the DNA sequences in the inserts, and what I'm
calling an insert now, which is the DNA, the sequence of DNA derived from the
frog DNA that we cloned. It could have been that the E. coli was deleting some of
the cloned frog DNA sequence and then not stably replicating the frog DNA. And
we tried to establish whether the frog DNA sequences were being replicated
faithfully and stably, and it turned out that they were actually. And that was
important, because that's the whole essence of cloning is to replicate ... to insert a
DNA sequence stably and faithfully. If it doesn't do that, it's not really worth
much as a method.

So that was tough and that is why we did a heteroduplex DNA analysis, and I
think I went through the fact that it shows continuous duplex forms between the
circular DNA plasmid sequence and the very long frog DNA structures that Don
Brown had sent to us from Baltimore. Don Brown was the source of all of the
frog DNA and all of the radioactively labeled frog ribosomal RNA too in this
study. And he was an excellent postdoctoral research mentor, and I thank him
very warmly, he's also a friend, and I owe Don Brown so much. It was a pleasure
working with him. He is a delightful person.

But that was the problem, the problem was the heterogeneity of the EcoR1
fragments that we observed in the plasmids, and it turned out that the results of
the heterogeneity in the EcoR1 cleavage fragments of authentic frog DNA from
frogs, that was the biggest problem. That's what happened during the experiment
stage and it was different from what was arranged because we were thinking we
would just have ... we knew that the average length of these EcoR1 fragments of
frog DNA was about 3.5 million. And we were thinking it was just like maybe a
3.0 or a 4.0, that would've been cool, but that was not going to happen. It was
more heterogeneous.

[Morrow reading from a sheet of interview questions: What would you say to the claim that
Cohen stalled the publishing of your paper, so that Annie's\(^3\) could attain more attention?]

Okay, now I'll move on I think to what would you say to the claim that Cohen
stalled the publishing of your paper so that Annie’s could have claimed more
attention? I would really try not to say anything about or to that ... I do not know,
and actually I believe Stanley Cohen says that the publishing of this paper was not
stalled. Actually I don't know, so I'm speculating here. The fact is that the paper
on the staph. plasmid, DNA cloned and the PSC101 was published before, I think
it was the month before this. I will say, actually along those lines for an answer to
this, I would say it took us a long time to do the analytical ultracentrifugation to
establish the buoyant densities of these EcoR1 fragments of the cloned frog DNA
inserts in these recombinant plasmids and actually for Don Brown to label some

\(^3\) Chang, Annie CY, and Stanley N. Cohen. "Genome construction between bacterial species in vitro: replication
and expression of Staphylococcus plasmid genes in Escherichia coli." Proceedings of the National Academy of
frog cells with P32 and purify the ribosomal RNA and send it to us and for us to
do the hybridization experiments. And also the minicell experiments to try to
show that the frog sequences in these recombinant plasmids was transcribed. That
was not an easy experiment. I had never made minicells, but I did. And it took a
long time to do that stuff, especially the heteroduplex analysis and the
hybridization experiments with the P32 labeled frog ribosomal RNA, which we
thought was important to show that the sequences were replicated faithfully.

It took a long time to do that and probably the paper of Cohen and Chang on the
*staph*. plasmid insert ... probably the data were ready earlier, because I think that's
not a long, complicated paper, and if you look at our paper on the frog DNA
cloning, it is. I think it's at the very limit of length that was permitted in the
Proceedings of the National Academy of Sciences, and there's a lot of stuff in this
paper. It might have been able to make two papers actually if we had done just a
little bit more.

So I think probably ... my conclusion really would be that the Cohen and Chang
paper was probably ready earlier. And I'll tell you, it took a long time to get this
paper together. And another thing I would say about this idea was that the writing
of the paper on the frog DNA cloning was not totally smooth. We had a meeting
to discuss the order of authors. I don't know whether you knew.

Chen: I think Stan mentioned that in his oral history.4

Morrow: Yeah, we had a meeting to discuss the order of authors, because it was not
universally agreed or it was not a decision by acclamation that John F. Morrow
should be the first author. And in fact, the whole thing was very pretentious. And
it was decided by the way by Joshua Lederberg. Joshua Lederberg was consulted
by Stanley Cohen, and Lederberg was the Chairman of the Genetics Program, I
believe, and he was a wise old man at that point. And he said, "Well there are two
groups involved in this investigation. Let's see how much of the data was
produced by each of the groups," and he probably looked through the figures, and
all of the figures except for the aragose gel were actually produced by the group
involving Cohen, Chang and Morrow, and Lederberg said, "Okay, there are two
groups, the Cohen, Chang and Morrow group will go first in the order of authors,
and the other group will go second." And it's my understanding that Joshua
Lederberg said ... I wasn't there at this conversation between Cohen and
Lederberg, but I believe that Lederberg said, "It would not be fair to put Herb
Boyer last because that's a position of honor." And Stanley Cohen himself was
quite honest and professional and said, "That he, Stanley Cohen, could not be the
first author of this paper, because he had not conceived the idea of the project." So
he withdrew from that discussion and actually he walked out of the meeting at
which the order of authors was discussed, and he did not support me to be the first

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4 Stanley Cohen, interview by Sally Smith Hughes, 1995, Program in the History of the Biosciences and
Biotechnology, *Science, Biotechnology, and Recombinant DNA: A Personal History* (California: The Regents of the
University of California, 2009).
author. He excused himself. He said, "Guys, I can't be the first author. And also I have something that I have to do elsewhere in the medical center. Could you please excuse me?" And he left the room.

And I still remember that. But you know, he is a good man. He was honest. And then Herb and I proceeded to argue about who would be the first author. [Laughter]. And you know, I pointed out to him that I had nearly lost my Ph.D. over spending a lot of time working on this paper, and I really kind of needed to be compensated in some way, and besides, I had done most of the work. And Herb, if this hurts your feelings, I'm sorry, but actually you're not listening to this anyway I don't think. Herb, you are a genius and a very distinguished scientist and I thank you for your collaboration.

Okay, I do not, and I said already actually, that I do not think I finished the experiments. I know we didn't finish the article before the Annie Chang and Cohen paper. The experiments took a long time, but the writing then ... with the contentious nature of the order of authors, you can imagine that ... well I think the rest of the writing of the paper was actually fairly tranquil. It was not hard to agree on the wording, but it tended to be a little bit long, so we had to cut it, I think, when we turned it in. I think that Annie Chang probably finished her article first. That's what I think. Well I don't know that the consequences of that changed the order, but I think it probably did have some consequence, but that's the way it is.

[Morrow reading from a sheet of interview questions: I have learned that you discarded your notebooks. When and why did you do that?]

And now about the notebooks. It was not our custom in the Department of Biochemistry at Stanford to retain laboratory notebooks, and we were not instructed or ever even requested to use bound notebooks, so that essentially none of us used bound notebooks, we used loose-leaf notebooks, you know the kind with the steel prongs that open up. It's a binder, yeah. And my recollection is that our custom in those days was to keep the laboratory notebook leaves for a time after the publication of the resulting paper in case there might be some response or discussion about the paper after it was published. But then eventually I disposed of it, because my point of view on this I learned, I think, from my mentor, which is that the published record for us was the record. In other words, if we didn't publish it, it might as well not exist. That was our point of view.

And we published, and I published. I published not a huge amount, but I published enough as a graduate student. So we tried to get it in the literature out there, and it's not that I discarded my notebooks, it's just that I had never really kept them. And I didn't cheat on my notebooks. I didn't go back and re-date things

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with a false date or anything like that, but I didn't have bound notebooks and we simply didn't have the customs that a chemistry department would have. I think they have always used, or for a long time chemistry pros have used bound with the pages sewn in so you can't change it.

Chen: Yeah.

Morrow: But that just wasn't the way, at least for us in the Berg lab the published record was the record. And I stand by that, we published. It's not that I discarded them. Janet Mertz, by the way, was very unusual in retaining her notebooks.

Chen: Yeah, she still has all of them in her office when I was there.

Morrow: Well that's amazing. When my notebooks were most likely discarded was probably when I moved to Harvard Medical School after my postdoctoral fellowship in Baltimore with Don Brown at Johns Hopkins. I finished what I was doing, or I finished as much as I could anyway of the work at Johns Hopkins with Don Brown, actually Carnegie Institution. And then I took an assistant professorship at Harvard Medical School, and that is mostly likely when those were discarded because I was still, unfortunately, working on my Ph.D. thesis, which was not really finished. It was not finished when I moved from California to Johns Hopkins, in about the middle of December of 1973. I finished it in the spring of 1974. We typed it on acid-free paper. I shipped it to Palo Alto, it was read and approved by my thesis committee, which was Paul Berg, Robert Lehman, and I've honestly forgotten who was the other member. I think there were three members. But I'm thankful to you and I appreciate your help...it might have been Dave Hogness. Anyway, I shipped it over there and they approved it, and signed it, and sent it into Stanford University and I got my Ph.D.. And I got it in I think May of 1974.

Chen: Uh-huh, I think that's what it shows on your thesis.6

Morrow: Right, thank you. And I probably discarded those when I packed up my relatively small apartment in Baltimore, Maryland, and hired movers to transport things to Massachusetts. Actually we bought a house in Newton, Massachusetts, close enough to Harvard Medical School to commute. I'm pretty sure that's when. It's not that it was unusual, I just didn't keep notebooks. And then until I had gotten everything I needed to get out of them, and I thought I had responded to everything I needed to respond to.

[Morrow reading from a sheet of interview questions: How did you learn of the potential patent application?]

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Okay. I learned of the patent application I think probably first from Bob Lehman. And he told me, I think he probably found me up when I was with Don Brown's lab, but I'm not sure exactly how or when. It's also possible I talked to Paul Berg on the phone a couple of times from Baltimore from Don Brown’s lab I think mainly about finishing the thesis, I think. And it may have been actually first from Paul Berg. It's more plausible that it would have been first from Paul Berg that I learned there was a patent application and that Cohen and Boyer were named as the inventors, not Paul Berg.

And you know, I did not think that that was fair, and I still don't. I think that Paul Berg ... at this very moment, and I have not reflected deeply on this but I think the most fair way to do this would have been ... Berg, Cohen and Boyer would have been the inventors of this patent application. I think that would've been fair and but that's not what happened, because the publication of the method to ligate DNA using the sticky ends created by the EcoR1 endonuclease, was published by Mertz and Davis and a patent application was not made in a timely fashion within the following year. So it was not possible and there's the detail that Paul Berg was not a co-author of that paper.  

Chen: Yeah.

Morrow: Which honestly I think he should've been. He had suggested the whole project to Janet Mertz, but you know, he had to choose between being the co-author of my paper on the SV40 genome and that very issue of the *Proceedings*.  

Chen: Yeah, they were the same issue.

Morrow: And it was a competitive field, we were trying to move. Paul Berg is not a competitive person, but we did not really want to lose our position in the order of things by delaying publication longer than we had to. So he withdrew as a co-author of the Mertz and Davis paper, and that's what happened.

That's as much as I can remember about how ... it was from a Stanford professor, probably Paul, but it could have been Bob Lehman. I know I talked on the phone with Bob Lehman about this at least once. And he told me he thought that the patent application, for the patent which now is probably called Cohen and Boyer...

Chen: Yeah.

Morrow: He thought it would be withdrawn because there are actually many reasons, there were some problems with the patent application, but it was not withdrawn, and that actually had the effect of distracting me from any further attention to the

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patent application at that time. Anyway, I was a postdoctoral fellow and I did not have really a whole lot of heavy academic weight.

[Morrow reading from a sheet of interview questions: Can you describe your reaction to the inventorship waiver and any interactions with the patent lawyer Bertrand Rowland?]

Okay. Can I describe my reaction to the inventorship waiver? I did not waive; I never waived my inventorship. And any interactions with the patent attorney? I have no recollection of any interactions with the patent attorney, except I got a letter from him most likely. I think I did get a letter from him. And I have searched my files, and I don't think I still have it. If I had it, I can't find it, at this moment anyway. I know I was disappointed, obviously. I think it would have been really nice and generous if they had found some way to create some senior inventorship category and some minor, partial, or junior inventorship. But there's no such thing as a junior inventorship.

Chen: Yeah, you're either an inventor or not an inventor.

Morrow: You're either an inventor or you're not. And I would never argue that I had an equal role to Herb Boyer or Stanley Cohen, but actually I did play an original role in proposing this project and then I played an intellectual role in going through what was needed to demonstrate that we had steadily and faithfully replicated these Xenopus DNA sequences in E. coli. And I'll tell you, that was an original role, and I honestly think that it would've been more fair to have included some of the other people who worked on this. And it would've been vastly more fair if the three inventors had been Paul Berg, Stanley Cohen and Herb Boyer, in whatever order they wished to do.

Chen: It doesn't matter in the patent.

Morrow: That would've been fine. It didn't matter. And I do actually think that I myself was a partial inventor of that. That's what I think. But it was decided mainly by Stanford University, I think, that I was not. And yes, I did try to sue. I found out the patent licensing office at Johns Hopkins, and I was by that time a faculty member at Johns Hopkins, so that would've been 1978 or after. And the patent was awarded, I believe … now this is all from memory … I found out through Johns Hopkins Patent Office and I asked if they would represent me in an action to try to establish inventorship rights for me in this patent. And they said, "Dr. Morrow, thank you for calling us, but we won't." [Laughter]. "And the reason is, none of the research that you could possibly argue was involved in obtaining this patent was done at Johns Hopkins. So we have no role in this. We do not have standing at the court, and we are not going to spend our resources, and we will not stand up for you as a faculty member. No, it's not our job. We're not going to get into that."

Chen: So even if they got it corrected they wouldn't get any money out of it.
Morrow: That's a fact. It would not be beneficial for Johns Hopkins. And they said, "Was this work done at Stanford?" And I said, "Yes." And they said, "Then we will not represent you." And there was a suggestion actually by Alex Rich, who was a professor at MIT, that his brother would represent me. His brother was an attorney, Dave Rich, and this was a very kind and generous suggestion by Alex. And it can be argued, and I think my son would argue that I should have accepted his offer and permitted Dave Rich to represent me on a contingency basis that if anything were to result from this, that Dave Rich would have whatever appropriate fair share would belong to him. And you know, I probably should have done that. But you can't turn the clock back. Yes, I did try to sue, but no, I did not succeed in suing.

And I will tell you that the reason why. There are several reasons why I did not go through with suing to defend inventorship rights. Stanford University has a vastly successful patent licensing office, and it has earned more royalties, I believe, than any other university in the U.S. and I believe more than any university in the world, in patent licensing, and they know exactly how to do this. And I surmised that I would have been defeated in court. And also I was an assistant professor still. I had nearly no money, so it would've been quite a challenge for me, practically speaking, to have established my rights, even if I had excellent claims. And Stanford University is very, very successful at this sort of thing. And that is the story.

There were no explanations given to me by [Niels] Reimers [head of Stanford’s Office of Technology Transfer] or others for excluding me as an inventor. In fact, they would not even give me ... I got them on the phone, and it was probably [Bertrand] Rowland [attorney working on the patent]. I reached Rowland on the phone and I said, "Would you please send me a copy of all of the claims?" And he said, "Dr. Morrow, I won't. I have given you the broadest claim." And I said, "Okay, well I don't agree to waive my inventorship of the broadest claim." But they wouldn't even send me the rest of the claims, and in my view that was discourteous. And I became frustrated and annoyed, and I had better things to do in terms of trying to actually make progress at the scientific research that I was involved with at the time, which was quite successful, by the way.

[Morrow reading from a sheet of interview questions: Did you feel that your student status stumped your attempts to attain inventorship?]

And did I feel my student status kind of stunted my efforts to attain right of ownership? Not really. I have learned that, I believe it is true, I had it stated to me anyway, that individuals in the past before me had attained inventorship recognition by the U.S. Patent Office for work that they did as graduate students, but it's my understanding that they had to establish very clearly and distinctly to meet all of the requirements for inventorship status. It can be gotten by graduate students, I think.
Chen: Yeah.

Morrow: It can, right?

Chen: Uh-huh. There is no law guarding against graduate students, it's just whether they were often even approached by their technology licensing office.

Morrow: Exactly.

Chen: Yeah.

Morrow: No, I didn't feel my student status stunted, but getting a patent is a political endeavor. It's something that happens between generally a professor and the universities, I guess patent application office, whatever it is called. And students do not have a strong political status because they're temporary. It's a temporary status, whereas a professor has a longer-lasting status. The university has more interest in representing and promoting a professor. The student will move on and go somewhere else. I don't think my student status helped me, but no it did not stunt it, as it says.

[Morrow reading from a sheet of interview questions and claims of the patent: To which one(s) did you think your contribution was great?]

Okay, moving on to the claims of the department. I do not believe that my contribution was great to any of the claims of this department, although I did demonstrate that we could replicate DNA that was most likely absolutely foreign to E. coli, and that we could do so stably. And it would seem to be claim number 11: "A method for replicating a biologically functional DNA comprising a replicon compatible with a host unicellular organism joined to a gene derived from a source which does not exchange genetic information with said host organism, said method comprising: isolating said biologically functional DNA from transformants prepared in accordance with claim transforming unicellular microorganisms with which said replicon is compatible with said isolated DNA to provide second transformants." No, my role was before that actually. But we demonstrated that we can clone DNA from eukaryotic organisms, which was planned and believed, and I think really quite foreign to bacteria. And it was a dramatic demonstration that cloning and really recombinant DNA methods could work and did. And it was a sufficiently dramatic demonstration that it was actually reported on the front page of the *New York Times*.9

Chen: I saw that.


Morrow: And so it was a demonstration, and yes, my role in that was significant. I am the first author of the paper, but it was more of a demonstration. It's not really a mathematical part of it. And I am going to now open my mouth with some additional comments about the patent. In my humble opinion, every one of the claims of the patent had been demonstrated by Mertz and Davis, except that, of course, they had not shown a method for producing a protein foreign to a unicellular organism by expression of the gene by studying a cellular organism. Rowland said *Staphylococcus* does not exchange information with said host organism. But honestly, I think the only claim that Cohen and Boyer could have to that would be probably the *staph*. plasmid DNA. And *staphylococcus* does not, I think, naturally ... I actually don't know whether *staphylococcus* exchanges DNA with E. coli or not. I think that Stanley Cohen claims that it does not. But apart from that investigation, I'm not aware of any way that they had actually demonstrated claim number 12. Mertz and Davis really did not demonstrate claim number 12; because Mertz and Davis did not clone. They simply demonstrated the efficient and really rather easy methods for cloning.

Chen: Yeah.

Morrow: Except they didn't have a vector that was a replica into which one could insert an EcoR1 fragment and still be functional. Their vector would've been probably a DV gal, and it was no longer functional, it seems. So Mertz and Davis actually demonstrated really everything that was important about this method, except for showing that it actually worked, except for finding a vector that actually worked. And I do think that that is the main contribution of Cohen and Boyer and John Morrow and Annie Change, and any of the other collaborators, we showed that it actually worked, and that it not only worked, but it was easy. And science is incremental. Science progresses by logical...usually science progresses in small steps, and those steps are generally quite logical. And ours was, and we demonstrated that it actually worked. Okay, how did my experience with Johns Hopkins officials and Stanford officials...

[Morrow reading from a sheet of interview questions: How did your experience with John Hopkins officials, Niels Reimers, other Stanford officials, Cohen and Boyer change your career aspirations?]

It's okay... Cohen and Boyer changed my career aspirations. My interactions with Cohen and Boyer were nurturing to my career aspirations. It was actually wonderful to work with such smart scientists. Johns Hopkins was always kind to me and generous, and I am very grateful to them, and it's a wonderful university and a wonderful school of medicine, which is where I taught. And my interactions with them were quite wonderful and I enjoyed them, and it was an environment that was very productive for scientific research, and my laboratory, and I especially thank my unfortunately late chairman, Daniel Nathans, for his part in nurturing and actually financing my career and helping me to obtain a senior
investigatorship of the Howard Hughes Medical Institute. That investigatorship was the source of abundant, continuous funds for our research projects, and I will be eternally grateful to Dan for his warm, kind and encouraging support. Neil Reimers and the Stanford officials I did not find very nurturing. [Laughter]. But Cohen and Boyer ... actually Herb Boyer was a wonderful guy and he did actually once tell me that they wanted to consider me as a faculty member at UCSF, but that did not work out. And he was very kind and supportive, and he didn't say they were going to offer me a position, but he just said that they wanted to consider me. And he said he was disappointed when he learned that I had accepted a job at Harvard, but I mean I had some offers and I did accept the offer of Harvard, and I taught at Harvard Medical School for three years.

[Morrow reading from a sheet of interview questions: Doogab Yi wrote in his book\textsuperscript{11} that you received offers from Harvard and other prestigious academic postings after your postdoc at Johns Hopkins.]

Okay, that gets us on to the next question. Doogab wrote in his book that you received offers from Harvard and other prestigious academic postings after your post-doc at Johns Hopkins. That's close ... the sense of that is actually quite correct. When I was a postdoctoral fellow with Don Brown, at the Carnegie Institution in Baltimore, Maryland, on the Johns Hopkins Homewood Campus, I received invitations to come and present a seminar at several universities, including Harvard. I was invited to come and give a seminar at Harvard Medical School, and I was also invited to interview for a faculty position at the Department of Biochemistry and Molecular Biology at Harvard University in Cambridge, Massachusetts, and I went there and I believe ... I'll have to think about that, but I believe I must have given a seminar at the Biochemistry and Molecular Biology Department in Cambridge, Massachusetts at Harvard. And I did have definite offers from Harvard Medical School, from Princeton University, from Cornell University, and those are prestigious. Princeton and Cornell, they are very prestigious, and I would have been delighted, actually, to have been able to be a faculty member at Princeton University and I would have been very, very, very delighted and happy to be a faculty member at Cornell University. They have a really excellent biochemistry department. And I enjoyed my visit there, and I think my wife was dissuaded by the cold winters in Ithaca, New York. We were from the Southeastern U.S., and I loved Cornell, but I visited in the wintertime, and the ground was covered with snow, really covered, and so I decided to go to Harvard Medical School. And yes, I did receive offers from Harvard, but it was not after my post-doc at Johns Hopkins, it was actually during my postdoctoral fellowship with Johns Hopkins. Of course it took effect after the postdoctoral finished, but I interviewed, and I had an offer from Rockefeller University too, by

the way. I did not favor that. But Rockefeller University is a wonderful, fantastic institution. They all have their advantages and disadvantages, and I did receive offers from several actually very prestigious universities, and I was honored. It was fantastic. And I'm so grateful and happy actually and I do want to express gratitude and appreciation, actually, to my wonderful Ph.D. advisor, Paul Berg, who was really very kind and understanding when he learned that I had carried out this project with the Stanley Cohen Laboratory and with the Herbert Boyer Laboratory, which he had not given his permission for, although he had permitted me to do restrictions on analysis of the Xenopus ribosomal RNA genes for Don Brown, very briefly, he said, "Don't spend a lot of time on it." He was very understanding and really kind to support me and continue the application for my Ph.D. from Stanford University, and it would have been a terrible problem if he had gotten very angry and thrown me out of his lab. But he didn't. He supported me for getting my Ph.D. from Stanford, and I had finished the experiments that were the basis of my Ph.D. thesis before I started any of this work with Cohen and Boyer. Okay, I wanted to express my gratitude to Paul Berg, that I owe him really a lot.

And also to Herb Boyer, who had provided the EcoR1 restriction endonuclease, soon after its discovery, and he had provided it to Paul Berg and me, and I had done lots of experiments with it, which were very productive and I want to express my gratitude to Herb Boyer.

I also want to express my gratitude to Stanley Cohen for the hospitality of his laboratory and his office and permitting me to come there and discuss with him this really exciting project. And then he provided financial backing and materials and expensive equipment, really, for this project, cloning the frog DNA and E. coli. And he instructed his associate, Annie Cheng, to do as much work as she could manage to purify the plasmid DNA, and I believe that it was Annie Cheng who did or maybe Stanley Cohen himself ... it was probably Stanley Cohen himself, with assistance from Annie Cheng, who did the analytical ultracentrifugation of the EcoR1 cleaved recombinant plasmid DNA that is shown in our paper and the Proceedings to the National Academy of Sciences, published May, 1974.¹²

That is a challenging type of experiment, which I believe was probably done in the genetics department. Stanley Cohen's laboratory space at that time was, I think, really more associated with the Department of Medicine, but he had a joint faculty appointment in genetics, I believe. And this analytical ultracentrifugation is not easy to do, and it's not inexpensive. The ultracentrifuge with optics on the ultracentrifuge cells that can be used to eliminate the cell with ultraviolet light while it is spinning, is a very expensive instrument, and Stan was supportive of this work. And he also invited my wife and me over to his house for dinner, and it was a lovely dinner with his family, prepared I think, by his wife. It was a very

delicious and wonderful dinner at their lovely home at Portola Valley, and Stan, I thank you for that too.

And I also want to thank, as I said, Dan Nathans, and I want to thank Thomas J. Kelly, Jr., my beloved friend really who was the chairman who succeeded Nathans in the Department of Molecular Biology and Genetics at Johns Hopkins, who was always very, very supportive. And I have really had a very successful scientific career, and it was really wonderful, and then you asked some more questions about that. [Laughter].

[Morrow reading from a sheet of interview questions: What caused you to abandon academia?]

What caused me to abandon academia? Okay, I can deal with that. I did not abandon academia. I finished my M.D. degree, which I had started before going to Stanford. I was in the medical school, in the M.D./Ph.D. program actually at Duke University for one full year before I moved to Stanford University in Palo Alto, California. And I had completed all of the pre-clinical courses at Duke University School of Medicine to permit me to apply for, and be enrolled for, and examined in the National Boards Part I Examination, which I passed before I enrolled in the Ph.D. program at Stanford.

Okay, so I started in medical school immediately after college, in an M.D./Ph.D. program. In my humble opinion, I am an M.D./Ph.D. type of person. I love scientific research. I have great enthusiasm for it, and I have done it until two or three o'clock in the morning. I have great enthusiasm for scientific research, and I'm an M.D./Ph.D. type of person, but I did want to finish the M.D. I felt that the time was running out in which any medical school would permit me to finish my M.D. when I reached the age of 39. And at 39 I was able to apply to the University of Miami to their M.D./Ph.D. Program, and I am very grateful to them for considering my application and for admitting me to their program, which is a very delightful, wonderful, high-level program that moves very quickly through what was necessary to function as a doctor. It's a challenge, but I worked very hard and I learned everything that is necessary to be an internal medicine intern at the University of Maryland Medical Center. And I finished the M.D.

So I didn't abandon academia then. I found that internal medicine did not really agree with me terribly well. [Laughter]. I was pretty old at the age of I think 41 when I started my internship, and that's pretty old for an intern, and interns sometimes have to stay awake all night, and function all the next day in those days, presenting the cases at rounds and taking care of the hospitalized patients, usually for all of the next day. And it was pretty hard, and I went into pathology. [Laughter]. I love medicine. I love being a physician and helping to take care of patients, but as a pathologist, I do not have the direct responsibility for the lives of the hospitalized patients as they have any adverse events that maybe follow them in their serious illnesses in the hospital.
So being a pathologist is quite delightful. And I taught pathology then as a faculty member at Albert Einstein College of Medicine in New York City, and then for four years at the Brown University School of Medicine in Providence, Rhode Island. I taught medical students pathology and I taught young pathology resident physicians how to become pathologists, how to make diagnoses, how to write reports, and actually how to do autopsies, too, for that matter. And I taught them that, but I am not the world's best pathology faculty member. I was functional, but not really as outstanding as I might wish to be. And eventually I went to work at a private laboratory, which I found is not manageable, because it's a challenge to stay up with all of the latest findings, to be able to give up-to-date lectures to medical students and pathology residents, and also you practice your pathology so quickly that you can have time left over to do research and publish original investigations, it's not easy. And I was really pretty old by that time to try to do all of that, and I went to work at a private laboratory, and it agrees with me better. And I practice presently at Kaiser Permanente Health Care in Northern California, and I find this to be quite a delightful organization which I believe delivers high-quality medical care.

In other words, I did not abandon academia, but I moved from the biochemistry department at Harvard, that was called actually the Department of Biological Chemistry at Harvard Medical School, in any case, it's the biochemistry program, and then at Johns Hopkins, my department was Molecular Biology and Genetics, which was what we taught. I moved from that to the Pathology Department. I continued to teach, and I continued to publish, but I didn't publish as much as I would have been optimal because I just wasn't fast enough and I didn't have quickness and energy, maybe, to be able to carry out a really active investigation while at the same time that I was keeping up with the literature and doing all the teaching and the clinical practice that's needed for a really top-notch academic pathology faculty member. So eventually I did actually go to work for a private laboratory, but people do that, and they find it to be a satisfying career.

[Morrow reading from a sheet of interview questions: Do you regret it?]

Do I regret it? No, but I do love scientific research and I wish I could find a way to do it right now, and maybe I'll work on that actually. I guess the answer to the last question, do you regret it is yes or somewhat anyway. I do somewhat regret it, but you know, it's hard work. Yeah, I regret it. I would really rather be able to function in the laboratory and also in the hospital. But you know, it's not easy and actually most of the pathologists in the world are not teaching at medical schools, most of them are just out there to practice and have hospitals. But yeah, I do regret it, at least somewhat, yeah, I do. And yeah I think my son probably wishes that I was still a university faculty member, but it's my life.

Chen: I want to revisit your Xenopus experiment. How did Berg find out and what did he do after he found out?
Morrow: Well he never told me how he found out, except it turns out that I made an appointment with him to meet with him in his office, and I said, "You know, Don, and I have been doing this project, cloning of these frog ribosomal RNA genes." And he said, "Well who are you doing that with?" And I said, "Well I'm doing it with Stanley Cohen and Herb Boyer." And he said, "Well you're finished with that, aren't you?" And I said, "Well you know, I'm not really quite finished with it yet, but I should be finished pretty soon." And he said, "Well you know you need to stop doing that and get your Ph.D. thesis finished." And I said, "Well, okay, I'll start doing that as soon as I can." I did not really knuckle under at that point. And I went to him and I told him that I was doing that but he told me later that someone else had told him that I was doing this. And I said, "Who told you that?" And he said, "Well someone told me." He would never tell me who told him, but I suspect that it was Bob Lehman, because Bob was on my Ph.D. thesis advisory committee, and Bob and Paul are friends. And Bob was occasionally critical of me, but I don't know that Bob Lehman told him, but somebody told him. And there were actually a number of possibilities.

Okay, so that's how he reacted, and he said, "Well you need to stop that as soon as possible and finish what you're supposed to be doing." And I said, "Well I am working on finishing my Ph.D. thesis. I'm doing both things." And he said, "Well are you sleeping?" I remember that. And I said, "Well not very much, and I'm working all the time." And he said, "Well you know you better get your Ph.D. thesis done." And I think that's the way he expressed it.

Chen: And then you moved away to start your post-doc while you were still finishing up the thesis?

Morrow: Yeah, I had to move to start my postdoctoral fellowship because the National Cystic Fibrosis Research Foundation informed me that if I did not show up at Don Brown's laboratory in Baltimore, Maryland, by December 31, or least January 1 ... they said December 31, I think. I had to show up in the year 1973, in his laboratory, ready to start work.

Chen: But basically you weren't a Ph.D. yet, because you hadn't finished.

Morrow: I didn't have the Ph.D. yet, but that's not very unusual for a person to finish the Ph.D. research, that happens first, then you finish the Ph.D. thesis later, then you submit it, and then it often takes months at some universities for the various people on your advisory committee to comb through it, and actually read it. These are book-length documents ... to read it, and ascertain or decide, really, whether it's sufficient, in their view, for the degree of Doctor of Philosophy, and that takes months oftentimes. And in the interim, the Ph.D. candidate doesn't want to be just sitting around doing nothing, and the university where he or she ... a lot of the graduate students are women and Janet Mertz was not the first Ph.D. candidate in the Department of Biochemistry at Stanford. Sally Seaver was several years before Janet, and Sally Seaver is a graduate of Harvard University, and she was a
biochemistry graduate student at Stanford, and her thesis advisor was George Stark. And I am pretty sure that she obtained her Ph.D. from Stanford University, and that would've been probably three or four years before Janet Mertz got her Ph.D. So Janet was early, but she was not the only woman in the department, by any means.

In any case, the graduate student would be left doing nothing, and the university where the Ph.D. work had been done really has no use for a researcher at that point because the project has been finished, it's been written up. Meanwhile they are reviewing the thesis. It's actually the rule more than the exception for a person to show up at the postdoctoral laboratory, or place, wherever it is, to do work before the Ph.D. degree has actually been awarded. And even after the Ph.D. thesis advisory committee has approved the thesis, it has to be sent to the university, and the university then has people who impose a certain set of standards. For instance, it has to be on acid-free paper. And the university can announce or they can state a reason why the work submitted for the Ph.D. is not sufficient, or it needs to be revised anyway. And then it often takes several months for the university to make its approval. And then after the university has approved, the Ph.D. degree is only awarded by Stanford once a year in May. So you can't get your Ph.D. degree in December, let's say. And that's relevant because I had to show up by the end of December, and I couldn't actually get the Ph.D. by the end of December. Stanford would not award it until May of the next year.

Morrow: And that's the rule of most universities, I think. And that was the story.

And maybe it turns out that individuals who have Ph.D. degrees are prized by a number of organizations, and I have read actually in some of the excellent writings of Atul Gawande, who is a surgery faculty member at Harvard Medical School, that one reason that the Ph.D. degree is prized is that holders of Ph.D. degrees are prized, at least by universities, and also by medical departments, surgical departments, et cetera is that having completed and received the Ph.D. degree demonstrates sustained and practical and effective pursuit and really devotion and commitment to a single goal over a period of a long time. It's not an easy thing, and people who can do that are people who can accomplish big projects, and you're probably one of those people.

Morrow: A Ph.D. has a lot of value because it shows that you have accomplished a big project. And not everybody can do that. So I'm very grateful to Paul Berg and Bob Lehman, and my other Ph.D. committee member.

Chen: Did you say it was Hogness?
Morrow: I think it was Dave Hogness. I'm positive it was not Arthur Kornberg. I don't think it was Arthur Kornberg. I don't think Arthur was on my thesis committee. In any case, thank you very much, distinguished scholars. And I have seen Paul Berg and Bob Lehman in the last few years at the Beckman Center for Molecular Medicine at Stanford University.

Chen: They are still teaching?

Morrow: I think [Lehman's] still teaching. In any case, I'm grateful to them and I know they are active, and Paul Berg is still publishing, which is amazing and it's wonderful actually. Okay, thank you.