Interviewee Information. *Please list an address where we can contact you.*

Full name: PETER EDWARD LOBBAN Date of interview: _____

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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) X 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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Signature: Peter E Lobban Date: 9/7/15

Protocol 1277 Informed Consent Statement – Last revised October 2009
This statement was read at beginning of interview and Dr. Lobban gave verbal consent.

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. Peter Lobban, do you agree to the interview?**

Interview with Peter Lobban
Conducted by Stephanie Chen
Origins of Recombinant DNA Technology
11 April 2013

Chen: Dr. Lobban, you have agreed to the interview?

Lobban: Yes, I do.

Chen: Perfect. Thank you. All right. So, we'll start with the first question. Do you have the questions in front of you?

Lobban: No, I don't actually.

Chen: Then I'll read them again because the first quote is a little bit long. It talks about conception.

Lobban: Right.

Chen: The first says "conception is defined as the formation in the mind of the inventor with a definite and permanent idea of the complete and operative invention, as it is hereafter to be applied in practice."[1] And the other one says that "conception is complete when the idea is so clearly defined in the inventor's mind that only ordinary skill would be necessary to reduce the invention to practice without extensive research or experimentation."[2] Based on these two definitions of conception, do you feel that you might have a claim to have conceived an idea of new technology?

Lobban: Well, now that's sort of a difficult question to answer because that was after all the research proposal and so naturally I proposed looking into the things that weren't completely understood. But on the other hand, all of the necessary research was very straightforward and easily conceptualized in the proposal. It was just a matter of doing it. So, I certainly did conceive the technology but there could be argued that there was still some research necessary before it could be reduced to practice.

Chen: Okay, so you do feel that your proposal demonstrates that you had formed a definite and permanent idea to complete an operative invention of the technology?

Lobban: Well, yes, with qualifications. The other thing was there were a couple of things I ended up having to do that weren't in the proposal. The most important one being

[1] Hybritech Inc. v. Monoclonal Antibodies, Inc., 802 F.2d 1367 (Fed. Cir. 1986)
[2] Burroughs Wellcome Co. v. Barr Labs., Inc., 40 F.3d 1223, 1228 (Fed. Cir. 1994)

the use of exonuclease III to compensate for probably endonuclease activity that was contaminating the terminal transferase enzyme.

Chen: Okay. So there were some changes to what you have proposed.

Lobban: That is correct, yes.

Chen: All right. So, as we move on to the next question, can you talk a little bit about in what capacity you knew Professor Stanley Cohen and what kind of things you guys talked about and et cetera?

Lobban: Well, Stan Cohen came in to the biochemistry department and in particular into my lab quite frequently to discuss a variety of things. The thing that was of most interest to him was something that I used to call the calcium assay which basically was a way of treating *E. coli* with high temperatures and high concentrations of calcium chloride to make it permeable to DNA so you could introduce the naked DNA into *E. coli*,and of course he wanted to use it for restriction factors and I was interested in using it for the recombinant DNA that I was expecting to produce. Although I never actually got to use it but that's why I was interested in it. So he came in … I took the method that was published by a fellow named Mandel,[3] I believe, it's been a long time, but I took the method and improved it for my own use, to change the permeability I guess of the *E. coli* to a much higher level by changing the protocol a bit, and I discussed those things with Stan. No doubt he knew that I was working on my method of doing recombinant DNA, but we didn't … I don't remember discussing it a lot, let's put it that way.

Chen: So, just by being there he should know about basically your plan to clone in *E. coli*.

Lobban: Yes. I think so.

Chen: So would you say that he was influential in forming your proposal[4] in any way? Or did he learn about it after you already had the idea?

Lobban: He was not influential at all.

Chen: Okay. Can I ask you about how you came to the proposal? Whether your advisor, I think was Dr. Kaiser, or Dr. Kornberg influence you in any way.

Lobban: No, they … well I shouldn't say in any way because it was department policy that I had to come up with a proposal, but in terms of the details of the proposal, no, not really. Where I got the idea was from a seminar given by a fellow grad

[3] Mandel, Morton, and Akiko Higa. "Calcium-dependent bacteriophage DNA infection." *Journal of molecular biology* 53, no. 1 (1970): 159-162.
[4] Peter Lobban, "The Generation of Transducing Phage *In Vitro*," Third PhD exam, November 6, 1969, 15, courtesy of Paul Berg.

student. We had a policy in the department that grad students and post-docs would give a seminar on something in the literature. I think it was once a week. They called it the journal club.

Chen: Yeah, we have that here too.

Lobban: Oh, I see. Okay.

Chen: But anyway.

Lobban: Well it's a good idea to make sure that your young people are reading the literature and also learning how to communicate. So, one of the grad students in my year, Thomas Broker, B-R-O-K-E-R, he's now in Alabama at the university and works on human papillomavirus. Back then he was a student of Bob Lehman's, and he was doing recombination in bacteria phage T4, if I remember correctly. Anyway, he gave the seminar on the enzyme terminal transferase and described it in detail and he indicated the properties and cited a number of papers and so on, and then at the very end of the seminar he just threw off as an offhand comment that he hardly paid attention to himself, that the enzyme might be able to put cohesive ends on the ends of DNA molecules, and I said to myself, Bingo! There's my proposal. And so unfortunately, I haven't been able to induce him to look through his notes and come up with when he gave that seminar. A number of people have asked. It has to be before the date on my proposal, of course, but I don't know how much before that date or anything else about it. And he once said that he would look into his notes, but I don't think he did. They're up in an attic in his house and he just didn't want to bother.

Chen: So he was using the enzyme but nobody else was coming up with the idea of actually putting recombined DNA into *E. coli* to reproduce it?

Lobban: Well, no, actually Paul Berg came up with that idea and as far as I know, and as far as he asserts, independently of me, in about the same time frame. There's a biography[5] of his being published, you're aware of that?

Chen: Yeah I met Dr. Friedberg when I was at a San Francisco.

Lobban: Oh, okay.

Chen: So he did give me a copy so I will look through it to see what Berg says.

Lobban: Yeah, and that account in his biography that he wrote is as accurate as anything I've ever seen about the whole deal from my point of view, so that's a good reference for you. But anyway, so he came up with the idea and then there was a

[5] Errol Friedberg. *The recombinant DNA controversy revisited: a biography of Paul Berg*. Singapore: World Scientific Publishing Company, 2014.

group … I wish I had the paper - it's cited in my dissertation - but which I could get for you, and email it to you, but it was published in *Bio-Chemical and Bio-Physical Research BBRC* is what it's called. I forget what the C stands for … oh, research communications which was a kind of journal that I don't know if you are familiar with it, or whether it still exists for that matter, but to have somewhat more relaxed standards than most journals so you could publish things that weren't particularly fabulous publications but they were informative. And this group tried to use terminal transferase created cohesive ends to join T7 DNA molecules together.

Chen: Okay.

Lobban: And their results were negative. But clearly they had the idea. I don't think they had formulated it or thought it out anywhere near as well as either Paul's group or I did but they were a third group that had the idea.

Chen: All right. I think I asked you a little bit about your post-doc in Toronto through my emails. So, can you tell me a little bit about what you actually did there?

Lobban: Yes, I did two things. Previous post-doc I think I had isolated a number of mutations in Chinese hamster ovary cells, CHO cells, that were resistant to alpha-Amanitin, which is a toxin produced by a poisonous mushroom, and so I did some work to see if the RNA polymerase, which is the target of that toxin, was altered in those cells. So that was one project.

Chen: Okay.

Lobban: And another project was I isolated some mutations in the same cell line that were resistant to histidinol, which is an analog of the amino acid histidine that inhibits the enzyme that attaches histidine to transfer RNA, and I looked for mutations. Unfortunately, all the ones that I investigated were just over-producers of the enzyme; they didn't seem to be mutations in the enzymes. I didn't actually publish on that because I wrapped up my post-doc work before I did, but I turned all the mutations over to another post-doc who I assume went on and did something with it.

Chen: Okay. Can you tell me exactly kind of what you wished to learn about animal cell biology when you went, seeing as you had the idea and you were in the process of actually reducing your idea of recombinant DNA technology to practice.

Lobban: Yeah, I didn't have any specific goal in mind. I notice in your questions … I never had the least doubt that mammalian DNA or eukaryotic DNA could be replicated as part of a replica that would live in *E. coli* because we already knew enough about DNA to know that they're essentially identical. I mean they have different GC ratios but otherwise, eukaryotic DNA and prokaryotic DNA are the same molecule. Whether they could be transcribed and translated was another question

that hadn't been answered. But I already knew back then people like Charlie Yanofsky were figuring out ways to move promoters next to operons and get them expressed and things like that, so I was pretty confident that somehow or another if there was enough incentive people could get eukaryotic genes expressed in prokaryotic cells. Now, at that time, the introns had not been discovered and something that might be of interest to you, it has been mentioned in some of the histories of this time, is that my original proposals included the idea of finding reverse transcriptase. I'd done another very extensive literature search on the work of Howard Temin and his work was circumstantial, but it pretty much proved that there was such a thing as a reverse transcriptase. And what I was thinking was that if you wanted to get mammalian gene expression the concentration of a particular gene in the mammalian genome was very low, much lower than in the bacterial genome, simply because there's a lot more DNA so starting with a genome, starting with the DNA might not be the smartest thing to do. It might be rather difficult to select for what you're looking for. So instead you get cells from a tissue that is expressing the gene product and isolate the messenger RNA and then make DNA copies of it. Now that was my idea, so part of the proposal was to find that enzyme. And one of the examiners in my committee, Buzz Baldwin, said I should delete that. I think he must have thought it made the proposal too complicated. But in point of fact, since both eukaryotic genes have introns in them that bacteria can't handle, at least I don't think they can, the idea was good from another point of view because if you start with messenger RNA you're already starting with something where the introns have been taken out.

Chen: Exactly.

Lobban: And so when you put the DNA copy of the messenger RNA then you are getting a DNA that the bacterium can handle. But I didn't know that at the time. It just would have been an interesting byproduct of the whole thing.

Chen: Why I asked that was just because I thought that you could have done this a lot earlier than the other people and I was wondering why you didn't do it in Toronto or elsewhere.

Lobban: Oh well, yeah, I mean I've wondered that myself. Going to Toronto was a mistake. It was a mistake from the standpoint of job hunt I think because it made potential employers think of me as coming from Canada as opposed to being from the U.S., even though I was a U.S. citizen and that, I think, worked to my disadvantage. Canada wasn't noted as a good place to do work or anything, even though this group was well respected. And also I really didn't need to learn about somatic cell genetics in order to have some good ideas for eukaryotic genes, in particular insulin interested me, because there's an enormous demand for insulin. And finding a way to produce it inexpensively and abundantly would be an excellent idea. So what I really should have done, actually if I'd sort of known about it and had been a little bit more ambitious, was to get involved with

Genentech and things would've turned out a great deal differently, but that's the way it goes.

Chen: Yeah, well why did you choose to go to Toronto? Was it because Kaiser or somebody else thought that you should? Or maybe did you think that you should at the time?

Lobban: Well actually Dale did recommend that I consider getting into somatic cell genetics so I'm pretty sure he mentioned specifically Lou Siminovitch's group.

Chen: Okay. Interesting.

Lobban: Oh, I also should say something else. Being a grad student in California at the biochemistry department of Stanford was like being in heaven for a scientist of my sort. And so I did not feel any great pressure to finish up and go. If I had felt that pressure, I'm quite sure I could have done the whole job a year earlier.

Chen: I see.

Lobban: So, but I just didn't do it. And since then I've had some friends whose children are getting PhD's and so on and I've heard them telling their youngsters to finish up and get out. [Laughter]. Don't waste time because that time is very important for their careers.

Chen: Yeah. I see. Let's move on over to the patent. So, one, were you aware when Stanford filed the Cohen and Boyer patent on the technology?

Lobban: You know, that's a long time ago and I can't tell you exactly when I became aware of it, though one incident I remember, David Jackson called me up at my home, actually my parents' home, one time when I was visiting and I suspect that was while I was … no, it was probably after I returned to California to get a Masters in Electrical Engineering and somewhere in that time frame, so somewhere around 1976, and he said that people were considering challenging the patent and asked me some questions and asked me if I'd be willing to testify and things like that. It never came to anything, but that may have been the first time I was aware of the patent; on the other hand it may not. I can't tell you for sure. It might have been mentioned before that.

Chen: So before Jackson called around, in that general time frame, did you ever think about being an inventor of DNA technology?

Lobban: Well in terms of issuing or getting a patent, no. I'll tell you that it's hard to believe, given the way molecular biology has developed, but back then there was actually a strong prejudice against business, against getting any commercial value out of the research that was being done. People talked about working for say a pharmaceutical company as a way of selling out, a way of being untrue to the

goals of getting a PhD. So, and I was tremendously naïve, particularly about patents. So no, it didn't occur to me. The idea of starting a company didn't occur to me.

Chen: A part of the patent that kind of confused me is the claim number nine and it reads, "The method according claim six wherein said cohesive termini are formed by addition of nucleosides."[6] That to me was like what you and maybe some other people, who wanted to use terminal transferase to create these cohesive ends. Do you feel like it kind of describes your method of manually adding nucleosides?

Lobban: Yes, definitely it does. And at the time when the patent was filed I'm sure that Cohen and Boyer knew everything about both my work and Paul Berg's work, which is probably why that was included. And you had another question which maybe you are going to ask later, but I'll comment on it now.

Chen: Yeah.

Lobban: I didn't know anything about patents back then but I have since had to do … I've actually gone to the patent office and researched patents under the guidance of an attorney and so I've learned a lot about it. About what is really legally important about a patent and all those sorts of things. And if I'd known then what I know now I certainly would have had no trouble in joining an effort to try to show that the patent was invalid because of the fact that not all the inventors were included. I mean, given the claims of the patent, at least John Morrow and I should have been included, and probably Janet Mertz as well, and Paul Berg. Just simply because the claims have to do with the things that we conceptualized. And we conceptualized them before Stan Cohen did.

Chen: Yeah. I wasn't aware that Stan did any of this, adding nucleosides by terminal transferase idea. He used the EcoR1 method that Mertz came up with.

Lobban: Yes, that's correct.

Chen: Okay. So do you think …

Lobban: I think that if, if the patent had gone to court it would've been held invalid. And I've had some conversations with a number of people about the patent, people who were grad students when I was back there, mostly, including Janet, and the sort of gist of what I heard was that everyone knew the patent could be contested and rendered invalid. But it costs money to do that and effort and time, and the terms for the license were sufficiently reasonable that nobody who had the resources to challenge the patent wanted to do that. It's just simpler and easier to pay the license fee and then you can get the use of the patent right away. So that's

[6] Cohen, Stanley, and Herbert Boyer. Process for Producing Biologically Functional Molecular Chimeras. US Patent 4,237,224, filed January 4, 1979, and issued December 2, 1980.

why it was never challenged. Not because it's valid, because it isn't invalid, but because there was no point in challenging it.

Chen: Yeah, especially when they didn't know whether it would come to anything, whether it would become this big phenomenon and how they earned a lot of money in pharmaceuticals.

Lobban: Uh-huh.

Chen: When you were at Stanford biochemistry, I was under the impression from Janet and John that it was very communal in the department.

Lobban: Yes, absolutely.

Chen: So were you aware of Janet's sticky ends discovery, and did you ever think about using it to replace the terminal transferase technique which is much more complicated to ultimately realize the same kind of DNA reproduction concept that you had.

Lobban: Well, yeah, I was probably aware of Janet's discovery within a day of two of the discovery because we talked all the time. I helped her with some electron microscopy. Tom Broker taught it to me and then I taught some things that he taught me to her. And she was also an MIT graduate and so we had a kind of a bond that way and she loved to talk about research. And of course I collaborated with the rest of Paul's group too with those parts who were working on terminal transferase method - David Jackson, Bob Symons - on a very frequent basis. We shared enzymes and ideas and things like that. So yeah, I was aware of it and I thought about it too and what I told myself … of course I had a great deal of time, energy and my heart and soul invested in the terminal transferase method by then anyway, and I tend to be the kind of person that moves in a straight line, so it would've been hard to deter me, but I did think about it and the reason that I stopped might not be the best way of doing DNA joining is that you have no choice as to where the restriction enzymes break DNA.

Chen: Yeah, yeah.

Lobban: And so it could be that they break the gene you're interested in right down the middle and you'd lose the gene. So that was the thing that suggested to me that it would be nice to have also the terminal transferase method, but of course it's monumentally more difficult to do. [Laughter].

Chen: Yeah, I read your proposal and there are so many enzymes and so many steps. Janet has pointed out to me that EcoR1 cuts GAATTC and if you don't have that you can't cut it and you can't make those cohesive ends on certain other DNA.

Lobban: Yeah, and also you have no control over the spacing between these things so say you have a gene that you're interested in and it doesn't have a restriction enzyme site in it but those restriction enzyme sites are … you know … 10,000,000 base pairs apart, you get a very large piece of DNA with the DNA you're interested in and it might not fit in your vector and get into *E. coli*. So that was another possible problem. There were ways of fragmenting DNA, usually physical means, that would yield DNA of reasonable size for the terminal transferase method and the fragmentation would be more or less random. And so, your chances of getting a piece that had your particular gene in it and it wasn't too big a piece could be made very large and well DNA created by reverse transcriptase would have given you something even more useful. And I assume people do that although I don't follow the literature anymore.

Chen: Okay. So you would physically fragment the DNA but how do you select for the ones you want?

Lobban: Well actually I didn't … I didn't do it.

Chen: Okay.

Lobban: Ah, what I did, obviously, what I did for my dissertation was to create a model system.

Chen: Okay.

Lobban: One of your later questions is going to have to do with my motives for going into electrical engineering …

Chen: Yeah.

Lobban: … and by temperament I'm at least as much an engineer as a scientist.

Chen: Okay.

Lobban: And engineers do model systems. They want to develop a way of doing something so they pick a very simple system, develop a way, and then look for ways to apply it that are more useful. And so I started with T22 DNA which was absolutely ideal for the purpose because it was known to have blunt ends - no cohesive ends so I didn't have to worry about proving that my method was joining the DNA molecules together and not something that was a property of the molecules themselves. And it was also circularly permuted so you couldn't easily create cohesive ends by accident either. So it meant that it would be a very convincing model system and it was also very easy to grow. I got the T22 from Esther Lederberg over in genetics, the genetics department I guess. No, not genetics. Now I'm not sure what department she was in anymore, but anyway she gave the temperature sensitive mutant of T22 to me and that became the basis for

my model system. So I didn't have to worry about fragmenting DNA or anything at the time, but I just knew that it would be possible if I ever got to the next stage.

Chen: So would you say that your working on the model system and that Dave Jackson and Berg's group were working to create a specific recombinant using this same method?

Lobban: Yes, that is absolutely correct. And it's probably in the oral history. Paul Berg expressed some confusion, not confusion, some puzzlement as to why I did what I did because Paul wasn't an engineer at all, he's a scientist. And I don't think the idea of a model system to demonstrate the proof of concept is something that he would hit upon or even have a deeper understanding of. Instead he had a particular target in mind and he was developing basically the same method to get to that target. So it's a different mindset.

Chen: So you didn't mind that they were doing the same thing … a similar thing?

Lobban: Well, I'll tell you, I met with that committee for this research proposal twice, once for the preliminary discussion and the second time for the final discussion. And when I left the room, Dale went out with me and he said, "Well, have you talked yourself into wanting to do that for your thesis?" And I told him, "Yes." And then sometime within that time frame, maybe a week or two later, he told me, "Well, you know you ought to talk to Paul Berg because it sounds like they are doing something similar." But didn't bother me because one of the things that made the Stanford department so much nicer for grad students, for post-docs than another department is, is that there was this aura of cooperation. And that was definitely Arthur Kornberg's thing. He created that department that way. So we shared equipment, people didn't chase you off if you wanted to use their centrifuge or whatever. Even though it was bought on different people's grants, it was treated as if it belonged to the department. And the same thing with research discoveries and things like that. We talked about them freely and there wasn't any sense of competition.

Chen: Okay.

Lobban: Which doesn't seem to be true in a lot of other departments. And of course now that things have commercial uses and can be patented, and form the basis of starting new companies, I suppose things have gotten very, very secretive and competitive, but they weren't then. Now when I read the paper from these people who tried to do the same thing with T7, that really upset me until I realized that they hadn't done a very good job and it wasn't going to get in the way of my finishing my degree. But that nearly gave me a heart attack [Laughter].

Chen: Yeah, they didn't do very well so it was okay. Did you have a thesis project before this proposal?

Lobban: Yes, that was Dale's suggestion. I was attempting to find the enzyme that's now called … let's see, what do that call that anyway? The enzyme that causes insertion of λ into the *E. coli* genome.

Chen: Okay. I don't know what it is.

Lobban: Yeah, it has been discovered and its properties are being studied and so on … integrase, I think they call it, yes, of course, it is integrase. So I was going to look for integrase and I had a system all built up for doing that but I hadn't actually tried to look for it.

Chen: And when you hit upon this proposal, did you want to change it? Did your committee suggest that change your project?

Lobban: Oh no. I wanted to change my project. I mean integrase would've been very interesting and a lot of them learned from studying it, but it didn't start an entirely new branch of science and I knew that DNA joining would.

Chen: All right. I was going to ask you about whether the Stanford people and the patent attorney Bertrand Rowland ever contact you for a waiver of the inventorship?

Lobban: I don't think so. If so, I mean, one doesn't remember 100 percent of what happens so I don't ever remember any such thing and also if they did and I gave them the waiver I was a fool. [Laughter]. And also I was not … I mean I don't know what these waivers actually entail but it would not have been a true statement. You know? I mean my understanding of the way patents work is if you contributed to an invention, you had to be on the patent.

Chen: Yeah.

Lobban: You couldn't just say, oh, I'm not interested, I don't want to be on the patent.

Chen: [Laughter].

Lobban: Now you could sort of assign your rights to someone else or whatever, but I don't think you can deny having invented something if you, in fact, did invent it. I may be wrong. I'm no lawyer.

Chen: Okay. I'll look into that because there are lots of people who study patents here and I didn't think about that, but that's interesting.

Lobban: Let me say this, if Janet, John Morrow and Paul Berg were not approached, I certainly wasn't approached because I was a lot harder to find than those people were in that time frame.

Chen: I think John was definitely approached and he didn't want to waive it. With Janet and Berg I think they [patent attorney and Stanford's Office of Technology Transfer] kind of got away with it because their work was already in the public domain, so they didn't have to deal with them. But they did approach John Morrow and I don't think he agreed. [Laughter].

Lobban: Okay.

Chen: Yes?

Lobban: I was going to say if their work was already in the public domain that's another indication of the lack of validity of the patent.

Chen: Yeah, that's true. I was talking with Janet last Friday and she was saying it was like Cohen and Boyer put together the different things that were already invented and put it into one patent rather than they came up with something actually new.

Lobban: Uh-huh.

Chen: Okay, so you agree with that kind of assessment?

Lobban: Yes, I do.

Chen: Okay. Okay. There's one interesting thing I found in Bancroft's Oral History of Kornberg.[7] I think you wrote a letter to him in 1986 that said, "A key factor in the process of disillusionment that led me to abandon a career in basic research was that I found not a single person who understood the implications of being able to join DNA fragments together at will, let alone found it glamorous or even mildly interesting. If I got any reaction besides bemused silence, it typically took the form of a dismissal like, 'You'll never get expression of mammalian genes in bacteria.'" So, was that your motivation to enter engineering? Was there any reason beside this disillusionment with the academic world?

Lobban: Well when I was in junior high and high school, electronics was my hobby. I took apart my parents' radios and television sets, I wired their house. I read extensively. My grandfather had some books on how vacuum tubes worked and I consumed them in great quantities. So, I also built the usual crystal radios and things like that and I built an oscilloscope from a kit for a science fair project.

Chen: Wow.

Lobban: So electronics had interested me all along. I had a pretty good understanding of how the transistor worked by then, and also when I was in my senior year at MIT,

[7] Arthur Kornberg, interview by Sally Smith Hughes, 1997, Program in the History of the Biosciences and Biotechnology, *Biochemistry at Stanford, Biotechnology at DNAX* (California: The Regents of the University of California: 1988).

I took the first year electrical engineering circuit theory course, partly because of that interest and partly because it had the reputation of being the best taught course at the school. It was taught by the guy who founded the Bose Company, which was kind of interesting. He spent some time talking about loud speakers which seemed to fascinate him and it was quite interesting to take a course from the man who was starting a company. So, it was a natural, more of a retrogression than a transition. I just went back to the other thing that fascinated me. And it allowed me to have a very satisfying career, developing scientific and medical instruments because I knew the application from my biochemical training and biological training, and I knew the engineering from the electronics training, so I could do both.

Chen: So did you graduate from MIT with a degree in …

Lobban: Oh, they called it Life Sciences. It would be called molecular biology anywhere else but they called it Life Sciences.

Chen: Oh, I see. So, you became interested in it when you were at college?

Lobban: Yes, that's correct.

Chen: Okay. I think the topic on Kornberg is also really interesting because he, unfortunately, he's passed away so I can't talk to him, but he seemed to be a great champion of what you deserved in this whole technology, this whole thing. Were you particularly close to him when you were at the department? Or any time afterwards?

Lobban: I wouldn't say I was close to him actually. He was just a very generous person. I did talk with him once, mostly because my parents-in-law, who desperately wanted to get their daughter back into this area, arranged for me to talk with him but that was while I was hunting for a job. But I didn't come right out and say, I need your help getting a job, it's terrible out there. So, we certainly had a cordial relationship, but I wasn't his student and I guess he didn't feel at the time that he could sort of help me get a decent position. And I think later he expressed a regret that he didn't but, of course, it was quite a bit too late.

Chen: Yeah. This letter that was part of the oral history with Sally Smith Hughes on Kornberg, so were you in contact with him in the '80s? It wasn't about the time when you went to school there.

Lobban: Well actually when he wrote his book, *For the Love of Enzymes*,[8] I went over and we talked about the work I did. I don't know whether it was in the '80s or not to be honest. I think it was but I don't know. And also, oddly enough, I went to a party that was in San Francisco for I think it was for his 70th birthday, which was kind

[8] Arthur Kornberg. *For the Love of Enzymes.* Cambridge, MA: Harvard University Press, 1989.

of a lot of fun because the actress, Rita Moreno, gave a talk there and she was very funny. So I had contact, I mean I've been living in this area since 1974. You know, I went to Toronto for two years and then came back. So, I've been around and ran into various people from the department at various events all along. But the contact was not sustained or anything.

Chen: Okay, I see. There was a little anecdote that he told that he tried to nominate you for some award for DNA technology and he seemed pretty annoyed that the people wouldn't listen to his nomination. [Laughter].

Lobban: Yeah, well when I read that I was a little bit annoyed too. [Laughter]. I mean after all, Cohen and Boyer have gotten a lot of recognition.

Chen: Definitely.

Lobban: They didn't need another bit of it.

Chen: So you were aware of his efforts?

Lobban: Oh, I became aware the same way you did.

Chen: Okay, so he didn't tell you or anything.

Lobban: No.

Chen: Okay. Do you regret not pursuing or staying with academia a little bit longer? Or, looking for a job a little bit longer before entering engineering?

Lobban: Well if I have any regrets it's that I didn't get in on something like Genentech in the early years. I've kept in contact with a couple of my classmates from the Stanford biochemistry department, Tom Broker and Kim Collins in particular, and when I listen to them talk about how hard it is to get grant money and how much time has to be spent on it and what the consequences are if you don't get it, I don't envy them in the least.

Chen: Yeah, it does sound hard to get money.

Lobban: Yeah, well it wasn't back then when we were in grad school, and I guess that caused us to have unreasonable expectations, but what I enjoyed about biochemistry was doing the lab work, and the idea of having to sit in an office churning out grant proposals, while somebody else got to do the fun stuff, doesn't appeal to me, so I don't envy them at all. I don't miss that. And that was actually one of the things I liked about working for companies was that somebody else had to worry about the marketing and the finance and I could do the fun stuff. [Laughter].

Chen: Can you tell me what you actually do? Because I didn't get to see any of that in San Francisco.

Lobban: Well yeah, you might not see any of it, but my entire career I designed instruments. I definitely wrote the … they call it firmware. It's software that's embedded in read-only memory devices so that it doesn't come off a disc. When you turn this thing on the software starts right away.

Chen: Okay.

Lobban: And it's the firmware that runs the various instruments. So I've worked for Varian Associates, and the big project there was the spectrophotometer and I wrote a good part of the firmware for that. And then I moved to a company called Sequoia-Turner and I wrote firmware for blood cell analyzers and also for a chemical analyzer that we developed for another company, called Hibritech. Actually I also wrote some of the software that runs on the computers that attach to these instruments. But mostly I wrote firmware.

Chen: Wow.

Lobban: And then I spent 14 years working for Affymetrix. I don't know whether you're familiar with that company, but there is something called the fluidics station and I wrote the firmware and did the electronic design for that. I did some electronic design but mostly I wrote the firmware and software.

Chen: I see.

Lobban: That's the sort of thing I've been doing.

Chen: It sounds very interesting. When I went to visit John Morrow, we did some of the interview in his office and there was a microscope and there were all these slides and it was really interesting to get to see how he transitioned from academic biochemistry to being medical pathology. So it's interesting how you guys changed career paths.

Lobban: Yeah, well I come from a long line of men with my surname, my father and grandfather, and also my son for that matter, who changed careers at least twice during their working life. So, it's a family tradition in my case.

Chen: I see. [Laughter]. Well, I think I've covered all of my questions. Do you have anything you'd like to add about rDNA technology or the patent or even your science career in general?

Lobban: Well, I'm actually very happy that universities have started to do patents and then get some of the financial rewards. And I suspect that this is when that started. I may be wrong, but it seemed to me that until the Cohen/Boyer patent universities

weren't in the business of having patents written up where they were the beneficiary for the licensing fees. And I think that's a good thing because research needs financial support and that's a very legitimate way of getting it as long as the patent itself is legitimate. [Laughter].

Chen: Oh, that reminds me. When you were at MIT, was there not a tradition or at least a starting kind of practice in terms of writing up patents for the work that was done there?

Lobban: I don't think so. But, of course, I was an undergraduate there so was not really that closely connected with research, and definitely it wasn't true in the Life Sciences department. Now maybe in the graduate electrical engineering or engineering departments in general they may have done that, but probably not so much for the benefit of the university. The reason being that I think there are clauses in many grants that assign the intellectual property to the granting agency.

Chen: Okay.

Lobban: And so it might be that they were discouraged from trying to patent the ideas that they came up with under their research grant. But I don't know; I'm really ignorant in that area.

Chen: Yeah, that's interesting because I think there is a much longer tradition in the physical sciences or engineering with patents and biological people didn't catch up until later.

Lobban: Yeah, partly because biology until recently didn't have such sort of profitable applications.

Chen: That's right. All right. That's all I have. Do you feel like saying anything else?

Lobban: Well I assume you're going to write a paper or something.

Chen: Yes.

Lobban: I wouldn't mind having a copy of it.

Chen: Yes. [Laughter]. I will definitely let you read it. It's not a secret.

Lobban: That would be good. Thanks, Stephanie. Okay, well I think that's all that I wanted to say.

Chen: Okay. Well that's great. Thank you for speaking to me.

Lobban: Oh surely, I'm very glad to do it. Sorry I missed you over the weekend.

Chen: No, that was my fault; it was not your fault at all.

Lobban: Okay, thank you and good luck on writing it up.

Chen: All right.

Lobban: Bye-bye.

Chen: Bye-bye. Thank you.

END OF RECORDING