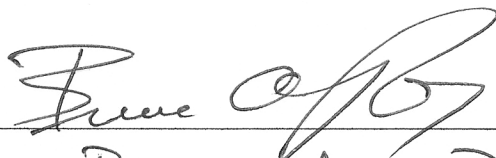


The main benefit of participating in this study is ensuring that your side of the story is properly portrayed in this history of the Bermuda Principles, which have become a model for open and collaborative research in genomics and other fields.

To help us protect the privacy of those parts of your interview that are not public, we have obtained a Certificate of Confidentiality from the U.S. National Institutes of Health. With this Certificate, we investigators cannot be forced to disclose information that may identify you, even by a court subpoena, in any U.S. federal, state, or local civil, criminal, administrative, legislative, or other proceedings. We 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 or institution obtains your written consent to receive research information, the researchers may not use the Certificate to withhold that information.

Signature  _____
Printed Name Bruce A. Roe
Date 3-29-2012

If you have read this form in its entirety and agree to the interview and its terms, please sign and date above.

Contact information:

Rachel Ankeny, Ph.D. (University of Adelaide)

rachel.ankeney@adelaide.edu.au

+61-8-8303-5570

Kathryn Maxson, B.S. (Duke University)

kat.maxson@duke.edu

(919) 668-0791

Robert Cook-Deegan, MD (Duke University)

bob.cd@duke.edu

(919) 668-0790

*If you have any questions about your rights as a research subject, you may contact the **Duke University Institutional Review Board** at 919-684-3030 or ors-info@duke.edu.*

PLEASE FILL OUT AND RETURN THIS FORM TO: Center for Public Genomics, Duke University; c/o Susan Brooks; Center for Genome Ethics, Law, and Policy; 304 Research Drive, Box 90141; Durham, NC, 27708. OR: You may fax it to us at (U.S.) 1-919-668-0799.

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

Full name: Bruce A. Roe Date of interview: 30 March 2012
Current institutional affiliation: University of Oklahoma
Street Address: 101 David L. Boren Blvd, Norman, Oklahoma 73019
Phone: 405 235-4912 Email address: broe@ou.edu

Interviewer Information.

Full name(s): Kathryn Maxson
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):

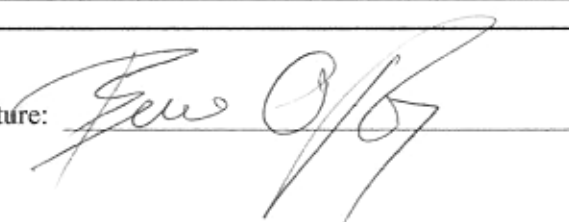
- Researchers unaffiliated with the Center for Public Genomics may **read** the interview transcript and any related documents only after obtaining my permission.
- Researchers unaffiliated with the Center for Public Genomics may **quote** from the interview only after obtaining my permission.
- Researchers 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:

- 1 year from the date of this form
- 5 years from the date of this form
- 10 years from the date of this form
- 25 years from the date of this form
- After my death
- Other: _____ (please specify a date or condition)
- NEVER: MAY NOT BE DEPOSITED IN A PUBLIC ARCHIVE

Please specify any further restrictions in the space below:

Signature: _____



Date: 26 May 2012

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

KM: We're on the record and I'm just going to explain for the tape that this is recorded, but you're going to get a chance to edit the transcript once we create it and take anything out you don't want. And basically, once you make your edits to the transcript, that edited transcript is the only thing we would ever make public for anyone else to see, other than us. And no one will ever hear the recording. It goes into our HIPAA compliant drive at Duke Medical Center and no one ever listens to that. And then once we give you the transcript and you edit it, we're going to send along a check sheet which basically lets you indicate what we're allowed to do with the transcript, everything from anyone can see it to no one can ever see it.

The reason why the Certificate of Confidentiality section is in the Informed Consent is because under this IRB protocol, Bob had done some interviews with folks who were disclosing potentially sensitive information, and it occurred to him that in any sort of litigation, he wanted to protect his research subjects to the greatest extent possible. So that Certificate of Confidentiality was not actually obtained with this project in mind. We don't envision anything with this project being something that someone would want to subpoena, but we have to include that information within all of our IRB informed consent protocols, and this project is a subset of that protocol. So that's why that's in there.

BRoe: Why do you need an IRB protocol for this? I mean I've been on IRB in Norman and at the University, so this doesn't seem to me to be something that would need IRB approval. I think our IRB would say, no, you're just interviewing people and getting what they say and letting them see the transcript and letting them do what they want with it, so I'm surprised that you've got IRB approval. But I can see that there were a couple of people that were there that could very well have...but they're pretty smart, I don't see why they would tell you anything that's proprietary or whatever.

KM: Well if you do say anything that you don't want in your transcript later, you just take it out and that's not a big deal at all.

BRoe: Okay, while we're here, just out of curiosity, have you been to my website?

KM: I have.

BRoe: Okay, I just put up on there, I just found the other day that Cold Spring Harbor released this really cool set of interviews. I don't know if you've seen that or not.

KM: Is it in relation to ...

BRoe: To the genome project.

KM: Yeah, the history of molecular biology and their Genentech project?

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

BRoe: No, this is actually...let me go to my website...I just lost your picture...

KM: I'll come back.

BRoe: You're just smaller for some reason because I'm doing something else here. Okay, so if you go to my website, there's a Cold Spring Harbor Laboratory Oral History Collection. I don't know if you see that there. Did you get to my website?

KM: Yep. I'm pulling it up. Yep. Are you on this list?

BRoe: Well ...

KM: Yes you are.

BRoe: Yep. I'm right above Jerry Rubin. And so it's sort of cool. But I remember when we did this, holy cow, it's got to be 10 years ago, I don't know when it was done, but it was done quite some time ago. This may be of interest to you to look through and see what people think.

KM: Yeah, and a lot of these folks we've interviewed as well—a very large number of them actually. So, yeah, this project, I believe it's part of a larger project that Cold Spring Harbor has gotten money from Genentech to do, to document the history of molecular biology and in particular the genome project. And we've been in contact with them because this project is part of a larger project on our grant to study the history of DNA sequencing, the implications of DNA sequencing technology. We've been in contact with Jan and Mila at Cold Spring Harbor for the potential of archiving a lot of our research resources, which is why actually when we send you that form for the transcript and what we can do with it, one of the things that we're exploring doing with the transcripts that are made completely public is sharing them with Cold Spring Harbor to become a part of their archives documenting the history of molecular biology.

BRoe: Oh, good. Yeah, I know Mila and Jan very well. And so, but she sat there and interviewed me in front of this...I don't remember what the wall was, either a fireplace or books or something...and we just sat and chatted for an hour or so. It was kind of fun. And I was too wordy probably.

KM: No worries. Anyway, well this is great, yeah, thank you. And we are going to go through and look at these. But again, we have interviewed a lot of these folks. So anyway, we have you down as being at all three Bermuda meetings. That's correct, that aligns with your recollection?

BRoe: Oh, yeah, yep. How far back do you want to go in DNA sequencing?

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

KM: Well the first question I'm going to ask is for you to talk about a little bit about who you are and what your background is, and how you came to being involved in the human genome project, why you think you were invited to the Bermuda meetings. So you can go as far back in that history as you think is relevant.

BRoe: I'd almost have to take my watch off. [Laughter.]

KM: We have as long as you want.

BRoe: So if this is being recorded, this would be kind of fun, because I actually would somehow or other like some sort of a history written down of my involvement with, my life in general I guess, when it comes to science. So I can tell you that I went to Hope College in Holland, Michigan. And after two years I decided a major. And I had taken chemistry, physics and math, and of course English and German. And then I decided that I had my best grades in chemistry and I was taking honors chemistry, so I became a chemist.

KM: My best grades were in chemistry too; I was a chemistry major for a while.

BRoe: And Hope College was at that point in time ranked very high in the country. According to the *Chicago Tribune* was ranked number three as far as chemistry majors. There were 15 of us that were ACS chemistry majors. And Hope College in Holland, Michigan is a really small school.

KM: So is it an ACS certified ...

BRoe: Oh, yeah.

KM: That's cool.

BRoe: Yeah. And it was very competitive. But anyhow I was fortunate at my junior year to be able to do undergraduate research and I studied ferrocene. And I was an organic chemist. And so that two years of blowing up Friedel Crafts reactions and everything else convinced me I wasn't going to be an organic chemist. And so I then applied to graduate school and ended up going to Western Michigan University and after the summer...during the summer I took a course called, Recent Developments in Chemistry, which was kind of cute because this was in 1963. And one of the major recent developments in chemistry that the chairman of the department, who happened to be a woman, Lillian Meyer, one of the major ones besides the discovery of Element 92 or something like that, was this paper 10 years ago by Watson and Crick on the double helix, and so science back then, 10 years was a recent development. And I read that paper and I said, "This is cool. Wow, this makes sense to me as an organic chemist that there's got to be this polymer that's involved in containing all the information a cell needs. I want to study that." And so I went to a professor there, Dr. J. Stenesh, who was studying

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

thermophiles and mesophiles and I said, “Do you have any projects on DNA?” Because the other guys were lipid chemists and enzymologists and everything else. And he said, “Yes,” and that he really wanted to compare the differences in the DNA between mesophiles and thermophiles. I said, “Okay, cool, I'll do that.” And so I actually was the first person to isolate and publish the characteristics of a thermophilic DNA polymerase from the *Bacillus stearotherophilus*. This work was part of the information that got the thermophilic DNA polymerase patent thrown out. For my PhD, I also studied nearest neighbor frequency analysis of the DNA to find out how often A was next to a C or G or T, et cetera. And lo and behold, in thermophiles Gs and Cs are clustered more than they are in mesophiles, and they have a higher melting out temperature. So anyway, they gave me a PhD for that.

Then I did a post-doc at Stony Brook where I studied transfer RNA because that was as close as you could come to sequencing nucleic acids. You could sequence RNA and as a result, we sequenced the sixth tRNA, *E. coli* alanine tRNA. From there I went to Kent State University where I set up my lab and decided that I wanted to do something with human tissues. So we isolated the first human tRNAs and we sequenced the first half dozen human transfer RNAs. Then, the year prior to my sabbatical I was at a Gordon Conference, and in a discussion, Tom Rajbandary from MIT asked me what I was going to do for my sabbatical that was coming up soon. And I said...and if you will pardon this expression...I said, I'd give an unmentionable body part to go and work for Fred Sanger. And he said, “Oh, wow, I'm going to go this coming year and work with John Smith, who's one floor below Sanger, I'll put in a good word for you.” And so lo and behold, for my sabbatical, I went to Fred Sanger's lab in Cambridge, England, and helped develop the dideoxy DNA sequencing method there. And I published five papers with Sanger.

KM: Oh, my God!

BRoe: And we sequenced the human mitochondrial genome. So I'm like, wow, this is... so I finally got to sequence DNA. And we got really excited when Steve Anderson read 100 bases on a gel and it was oral P32-labeled and we...all the sequencing was done in capillary tubes because this was before Eppendorf tubes, although we did have the Eppendorf pipettes. So we had to pipette into capillary tubes and heat seal the ends in a burner, so it was a good thing I was a chemist and had some training in glass bending making T's and other things. So yeah, it was great. And it was a really teeny weeny lab there at the MRC. Since I had isolated tRNAs and developed some methods for nucleic acid isolation, one of the first things that Sanger wanted me to do was separate the two strands of the mitochondrial genome because one strand is G rich, the other strand is C rich; it's called the heavy and the light strand. Anyway, I ended up being able to do that on a column, and I got two peaks and he came in that day and said, “Oh, I want these three tubes, which were the three around the peak, and these three tubes.” And so

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

I gave it to him. Then Alan Coulson actually used them as templates for DNA sequencing. The results proved it worked and we were very excited.

And so then I got my piece of mitochondrial DNA, about 5,700 base pairs. My entire sabbatical was sequencing that piece of DNA by shearing it into little bits and pieces, cloning into M-13 phage and doing radioactive sequencing on slab gels. And I was one of the crazy Americans that enjoyed working late and then hitting the Franklin Center with the rest of the guys. And then I'd get on my bicycle after four pints of Abbot ale and wiggle my way home. And my wife would get mad and say, "You missed dinner," and all that kind of stuff. So anyway, we had fun, the kids enjoyed it, and she enjoyed it. She ended up getting a job as a librarian at the astronomy institute there. But anyhow, that's another story. We've had fun, and we're still married after 48 years. I don't know why she puts up with me. [Laughter].

KM: So you came back from your sabbatical ...

BRoe: Right, I came back from sabbatical and my wife really was tired of winter. And she said, I really don't like the long winters in Kent, Ohio, so can we go someplace south? And I began looking for jobs in the south and ended up getting one at Oklahoma. I had a good friend who was in Charlie Cantor's lab, who also was at Oklahoma and we'd known each other for years, so that was kind of fun. And once I got to Oklahoma, I wanted to do DNA sequencing. So I hooked up with people that were interested in cancer genes and collaborated with them. And we sequenced a lot of genomic DNAs, EGF receptor, epidermal growth factor receptor, and in fact Elson Chen, one of my former students, he beat me by two weeks as he published a paper on EGF receptor before we did in *Nature*.

KM: I just emailed Elson Chen at the same time I emailed you. Maybe if you send him an email saying, "Hey, these guys are really cool, doing this project, you should do an interview with them," he'll do it.

BRoe: I don't think Elson went to Bermuda, did he?

KM: Yeah, he did.

BRoe: Oh, then Genentech must have sent him to Bermuda.

KM: He was at all three meetings.

BRoe: He was at all three, that's right, he was at all three meetings. But he was at Genentech at the time I think.

KM: So you should tell him that he should do an interview. Put in a good word.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

BRoe: Elson, only the gods know where Elson is these days. Christina, his wife, we got a Christmas card back and forth from them. But I've run into him, I've seen him from time to time. For my retirement party, he came and we had a good time. But I don't know whether he's in Taiwan most of the time or...

KM: Yeah, I think he's founded a company; he's CEO or something of a company in Taiwan.

BRoe: Right. And so they were supposedly going to map all the SNPs and Chinese DNA and things like that. And I don't know how much they're collaborating with BGI or not, but anyhow, I know he's doing well and he's over there.

KM: So you're sequencing DNAs at Oklahoma...

BRoe: Right. And then I wanted to do the human mitochondrial genome and we ended up doing...and the human was done so we ended up doing in my lab the bovine mitochondrial genome. That was actually the first thing we sequenced for DNA using dideoxy and Elson went from my lab to Bethesda Research Labs, he took the protocol that we had written in Sanger's Lab with him and he wrote the protocol or rewrote it there and they put together a kit for DNA sequencing. And that was the first DNA sequencing kit that came out from Bethesda Research Labs and Elson did that. Then Elson got a job at Genentech and they hired another post-doc of mine, Paul Armstrong, and he went to Bethesda Research Labs to continue that. And by the way, there's an argument between my two first graduate students, Elson and another fellow, Hau-Yang Tsen, as to who was the first or the second PhD. So Elson was my second PhD student, though he says he was my first. I mean they were like two weeks apart and I have no idea who, and so now remembering things like that are important. But both of them got their PhDs from me. Hau-Yang Tsen became a full professor in Taiwan, a member of the Taiwan Academy of Science and he's now retired.

So we did that and I came to Oklahoma and then in Oklahoma is where we started sequencing the mitochondrial DNA and some cDNAs and collaborated with Ira Pastan at NIH and several other groups to sequence important oncogenes by taking cancer genes and getting their cDNA sequenced. And so I was attending meetings and people were talking about maybe sequencing the human genome, or thinking about it, and they were joking, "Oh, it's going to be impossible to do." And Ray Wu, who was at Cornell, who was very famous, Ray Wu sequenced the cohesive ends of lambda, all 22 bases. And it took like three years and 17 post-docs to do. But he sat next to me at a study section and he said, "Oh, something's got to give, you can't do this." And then Wally Gilbert said, "Well maybe if we had prisoners sequence the DNA." And so he was going to have prisoners sequence the DNA because for every so many thousand bases of sequence you got a day off your sentence or something like that. All these crazy ideas about sequencing. But it became very clear that there were several groups working on

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

automating DNA sequencing. And so we ended up, sequencing some of the first human genes, the leukemia genes from the C-abl gene from chromosome 22, chromosome nine and the BCR gene on chromosome 22. And so they're, if you're familiar with leukemia, the two ends of the chromosomes 9 and 22 switched places. One of the arms for nine swaps with 22 and you get one chromosome slightly longer than the other. And that's called the Philadelphia chromosome. And that chromosome will break point when you get that translocation on chromosome nine, you get the promoter, the front part of nine of the abl gene and the BCR gene connected and then on 22 it's the BCR gene with the abl gene. Anyway, the bottom line is that you lose control of the tyrosine kinase activity. And then that fusion protein results in affecting other cell proteins, that eventually causes a loss of contact inhibition of cell growth and cells grow uncontrollably. At that point we were funded to do that in the early 1990s and we were actually...my lab and Fred Blattner's lab and somebody else were the first three...and I can't recall who...maybe Craig's lab doing cDNAs at the NIH were the first three groups that were actually funded by the NIH to do DNA sequencing for the human genome project that we were talking about. For us, what happened was after we did that radioactive sequencing for the C-abl and BCR genes, ABI came up with a sequencer and I ended up getting machine number three. It was much later that when we were at Cold Spring Harbor where Venter said that he was joining forces with ABI to do sequence the human genome.

KM: So you got the third ABI sequencer? The original?

BRoe: Right, the 360. There was the 360 ...

KM: The 370 maybe?

BRoe: Yeah, it was 370. And anyway, so I had ABI 370 number three and it was the first one outside of Foster City and Craig got his fairly soon after I got mine, although we disagree on that one too, and he says that he got his first. I really don't care who got it first. But I do remember that mine had a machine number three on it. And we worked with Cheryl Heiner at ABI.

...

And so we worked with Cheryl Heiner and we optimized the DNA concentrations that we used and we ended up publishing a paper on sequencing on the 370. And subsequently Leslie Johnson-Dow in my lab, that's who helped with automated DNA sequencing where she sequenced human endogenous retroviruses, repeat sequences present in the human genome. It was some time later, probably at a Cold Spring Harbor meeting Ian Dunham from England and Nobuyoshi Shimizu a professor from Japan, and I got together to sequence human chromosome 22. I wanted to sequence one region of chromosome 22 and they were interested in two

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

other regions of chromosome 22. Therefore we joined forces, obtained the funding and began to sequence our parts of chromosome.

As part of our efforts we collaborated with Beverly Emanuel in Philadelphia because she was interested in the Philadelphia chromosome for obvious reasons. And so that was our first real foray into sequencing more of the human DNA using fluorescent methods. And so I think that because of that and the other reasons, we then were part of the initial group of scientists funded by the NIH to sequence specific human chromosomes. And so there were...I don't know, some number of labs...but they were at least...oh, I don't know, funding maybe 15 or so human chromosome specific groups who were invited to the Bermuda meetings. But they also invited Craig of course and I remember André Rosenthal from Germany, and I don't know who was from France.

KM: Jean Weissenbach.

BRoe: Oh, Jean Weissenbach, yes, yes, yes, of course. How silly of me. Jean was there. And so anyway, it was a pretty good group. The whole meeting was pretty much organized by the Wellcome Trust.

KM: Michael Morgan?

BRoe: Michael Morgan, right. Michael Morgan at the Wellcome. And Michael wanted it in neutral territory so if we flew to Florida, well then it's the Florida meeting. And so he wanted it ... because they were paying for it, Wellcome Trust actually paid for it, and so that's why they had it in Bermuda. And so that kind of summarizes our work that led up to this meeting. Since then we completed chromosome 22, we've done a gazillion other sequences and participated in mouse and in chimp and other genomes. And then eventually the number of genome centers was reduced so that there were only a few big genome centers; they attended the meetings sponsored by the NIH after the 3 Bermuda meetings and we didn't make those conferences. So we then switched on to do mouse and then moved to plants. And so we've been happy as punch.

KM: So now just to review for the tape because it's been a little choppy, you were working with the early ABI sequencer. And which genes, for the tape, was your lab involved in sequencing before Bermuda?

BRoe: Oh, we were involved in sequencing, before the ABI, we sequenced the abl and BCR genes. And because of that interest, our group and a group in England and a group in Japan joined forces and said, we want to get funded to sequence chromosome 22. My lab was responsible for the upper quarter of human chromosome 22, the Japanese group had responsibility for the middle quarter which had the immunoglobulin gene cluster which they were interested in and the group at the Sanger Institute in the UK had the lower half.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

KM: And so you guys were involved in the human genome project in that respect.

BRoe: Right. And then ...

KM: And then when they started scaling up and they chose the bigger sequencers, you guys did not make that cut, so you continued on with non-human organisms and sequencing.

BRoe: Sure. Yeah, and fortunately for me, a couple of my former students, Lee Hood's lab was funded, he's the third I think who was funded in the beginning. And so Rick Wilson from my lab went to Lee's for a post-doc. And then Bob Waterston hired him away and Rick went to Wash U where they worked on the *C. elegans* genome. Then, once *C. elegans* was completed, in collaboration with John Sulston at the Sanger Institute, both groups moved to larger organisms. Rick subsequently hired Elaine Mardis at the Wash U genome center. Elaine, also got her PhD from work done in my group and after graduation, joined BioRad. Rick also hired Stephanie Chissoe, who also earned a PhD from my group and after a few years went to Glaxo Smith Klein. Actually I just saw Elaine a couple weeks ago because I had my annual physical checkup at Barnes Jewish Hospital where I had lung surgery five years ago. I travel there every year and we usually go out for Japanese food, which you don't get in northern Wisconsin. So we have a great time visiting and I am very proud of how well they are doing.

KM: You're at the top of quite a list of genome investigators in terms of the academic pedigree hierarchy. You've got a lot of folks that have trained with you.

BRoe: Yeah. I really have been lucky. I've had wonderful people to work with and they've bought into my crazy way of teaching and semi-micromanaging and then letting them go. I've got several people at the JGI; I have maybe six or seven people at Baylor as well as elsewhere doing genomics. So my job has been to train people. I've tried to be the teacher in the process and I actually taught the pre-med biochemistry course for almost 30 years. Prior to that, I taught nursing biochemistry, as well as nursing organic chemistry when I was at Kent State University before I moved to Oklahoma. The pre-med biochem course grew to about 400 or so students, in my last several years of teaching pre-med biochemistry in Oklahoma. When I threatened to retire because I was having lung problems my colleagues said, "We've got to hire somebody else to replace Bruce," and so they then hired someone to take over the teaching and they broke my large course into three sections. It was interesting, nobody wanted to teach that big class, but I did, I was such a ham, I had a good time doing it and because I lived it, I told lots of stories about how the science led to the discoveries that they had to learn about.

KM: Well so now you guys were, you're at this lab at OU and you guys are sequencing chromosome 22 on the ABI machines. And that's why you're invited to Bermuda.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

So you show up in Bermuda in 1996. What did you expect was going to be discussed there?

BRoe: I think that Mike told us that the purpose of the meeting was to discuss data release. And as I recall, John Sulston and Bob Waterston pretty much wanted immediate data release and I had talked with them earlier and said I would support that. So I think there were communications somehow or other ahead of time, at least for the first meeting that we talked about immediate data release.

KM: And so Michael Morgan who, just in case, because it was choppy, in case it didn't get on tape, so Michael Morgan was who invited you to the meetings. And you think that he told you that the purpose was the data release?

BRoe: Yeah, how we were going to deal with the data.

KM: Right, right, that's interesting.

BRoe: Whether he said it was data release or it was just, how are we going to deal with this massive amount of data? Relatively speaking it was a massive amount. There I am vague. And I'm not being vague for any other reason other than I don't remember, except why would he care about how we were going to deal with the data? We were in a 'publish or perish' environment, so you're going to publish. But the whole idea of getting the data out there was very interesting and it was quite fun to see the various people involved and how they felt about the data release.

KM: So just dovetailing on that, can you describe the mood and the content of the discussions that you remember from Bermuda? Were you chair of any of the sessions or moderator of any of the sessions? Or were you just an attendee?

BRoe: All three of those meetings meld into one in my head, so I really can't tell you that. Whether or not...I almost think that I chaired something, you know?

KM: Yeah, yeah.

BRoe: But I don't remember exactly what it was. I think they went around and said, "Okay, here, would you chair this?" But it was more...it wasn't a great leadership chair I don't think. It was more of a keep everybody from killing each other, keep it on track.

KM: Right, right, it was a convener.

BRoe: Yeah, more of a convener than a chair of something.

KM: Yeah, I can live with that.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

BRoe: I just don't remember. But I'm sure Michael must have all those records of who did what.

KM: Yeah, we've got the programs from all the meetings. So you mentioned people killing each other. What was the tenor like in these meetings?

BRoe: Actually, I think Michael and Bob Waterston and John Sulston were really typical British, quite calm and proper folks, okay? Waterston is probably maybe a little bit more excitable than John Sulston was, but really not very much excitable. I mean me, I'm crazy, my hands are all over the place and I'm doing all kinds of things and I'm jumping up and down and everything. But they were rather sane and sensible. But I think Mike Morgan had as an agenda that if this was going to be funded by the Wellcome Trust, that the Wellcome Trust wanted immediate data release. And somehow or other that got conveyed and Waterston and Sulston and me and I'm sure there were others who agreed with that, but initially it was I think three or four of us that said, this is a good thing.

KM: So what do you think was underlying the desire to want to release the data?

BRoe: Well I think they were...my feeling is that there were probably two reasons. The first reason is that the information was paid for by public funds and the Wellcome Trust is sort of a charity in a way and so they wanted whatever data that came from it to be publicly available, and that in turn meant that it should not be patented. And so those were two of the major reasons. One is...but the overlying theme was this is really, really an important project and we don't want it bottled up and hidden from anybody. We want this data. They had a vision that the best would come of it if we would just get it out there and people could use it. And there were some who disagreed with that. But I think that at some point in time almost everybody except maybe a handful really agreed to it. I think almost everyone who was funded by the NIH, I can't remember anyone who really spoke against it. It was sort of like ... let's see, who was there ... so I don't know, Francis Collins might have been there, I don't recall.

KM: Francis Collins was at all three meetings.

BRoe: Francis was there all three. But at some point he was...yeah, he was at the NIH then, or was he still in Michigan?

KM: He would have been at NIH by this point.

BRoe: Okay, if he was at the NIH, and the thing was if Francis said, 'We should have immediate data release,' you don't bite the hand that feeds you. And so some of us said, "Sure, yeah, we agree, we think that's a"...and so I think that ... I remember Adam Felsenfeld, Mark Guyer and Jane Peterson were there as well as I recall. And so I think that the American funding agency essentially agreed with the

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

British funding agency that there should be immediate data release, or at least that's what seemed to me to be the case. The German group was funded by some pharmaceutical house, and they actually had several who were representatives of the pharmaceutical industry in Germany. And André Rosenthal, I think felt as a scientist that he would agree for immediate data release, but the problem was that he had some constraints because of his funding, that the German group wanted to look at the data first for the obvious reasons of trying to get something proprietary out of it I guess, or make a better drug or build a better mousetrap or something. So his hands seemed tied and there was a lot of negotiation that I think went on in the background over these meetings between the American and British funding agencies, with both the German funding agency and also in Japan, where as I recall, you had to wait a year for data release.

KM: Do you know, was that federally mandated?

BRoe: Pretty much so I think. I think at the government level, they said, you can't...we have to...in fact, they were constrained...forget the genome project, I think they were constrained on any science. I think part of the reason was because they didn't want to, as a country, look bad. I think there was something to say about that. And so there was this, we're only going to show this really bright excellent stuff to the outside world. And so the scientific agencies there had to review everything that you even submitted for publication. And they got a rebellion against that at various points.

KM: And what was your stance?

BRoe: Oh, mine was immediate data release right off the bat. And I came into the meeting with immediate data release.

KM: And your motivations were just that this is the better way to do the project?

BRoe: My motivations were that...there were several reasons...first, I didn't think that you should patent genes. I think this was nature at its finest and I didn't think that they should be patented. I think that also I thought that we were funded by a federal agency and it was a massive effort and it wasn't like doing enzyme kinetics or organic chemistry or something else, I mean that anybody could do, this was really something that there was a select group of people worldwide that was maybe a couple of dozen labs that were funded with lots and lots of money and so our job was sort of to get this information and make it available to the rest of the world so they could use it. And that to me was...that goes along with my whole teaching philosophy. I mean, yeah, sure, we would publish the papers on it.

And so then I think a lot of the debate, there were two parts to the debate, but I think for me the reason was that I thought it would be just such a benefit. I only had a small lab, I mean eventually I had 60 people, but most of them were

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

undergrads, making gel mixes and looking at gels and all that kind of stuff with a roomful of these machines, cleaning plates and all that stuff. So we had a good time in that program. But I think...data and let them use it...from there and learn something about it, because I didn't care about [Inaudible - garbled]. I mean I cared, my actual real interest was, I still have an interest in, is DNA application and DNA clone traces and transfer RNAs. But my major interest these days is how to make it better, quicker, faster and more active. So I guess sometimes I consider myself to be a technocrat. I don't know. I'm interested in the technology of it and lots of methods papers, I want to get good data and then I want to give it to everybody and let them use it, because they're smarter than I am when it comes to that. I always thought it was a good thing to do.

KM: So when we've done these interviews, we've gotten several competing philosophies as to where the idea of this data release, not necessarily where it was coming from, but what the motivations were behind it and some of them are the same as yours, that this is a public works project, it's expensive, there's only so many labs in the world that are privileged enough to have the resources.

BRoe: That's one reason. That's one reason, I agree, yes.

KM: And some others were saying, coordination in terms of actually getting the project complete, in order to coordinate amongst all these disparate labs and many different countries, data release was necessary in order to get the project done.

BRoe: You must have been talking to administrators because I mean that to me that sounds like it comes right from several good friends of mine at the NIH, I mean who I deeply respect and really care for, very good people. But they were there going...and I do remember that argument, okay, now that you say it, this has got to be coordinated. And so many of us said, Well, look, I mean we're doing chromosome 22. Let the scientists who are doing chromosome 22 coordinate it amongst themselves. If somebody is doing chromosome 14, that's their job. Let them do it. Well but what about if they don't do it as well as somebody else? And then the argument would come back with, "What do you mean, you've got to tell them that they've got to do it better?" Yeah, sure, but we're all struggling with how to do this. So I think this, a lot of this coordination came from the administrative point of view. And I think that's probably, there's a justification to that, yes, because they've got to get all this money that's going to be taken out of the pockets of the poor enzymologists, how are you going to do that? And Jim Watson would argue, "No, but we have an additional allocation on top of it." And so this isn't stealing money from other researchers, this is additional funding and stuff. This is what we tried to fight for. Oh yeah, there was a coordination effort. And I think a third ... yes?

KM: Oh, well what I was going to say was the third motivation that we've heard is that it was just pure good policy and that the human genome project is something that

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

everyone shared in; and therefore, it was good policy to just have the human genome project be advertised and presented to the world as something that was publicly available. I don't know if that's what your third was going to be.

BRoe: No, my third is to stop people from patenting things.

KM: To stop people from patenting, yeah.

BRoe: Yeah, I think that your third one is pretty much encompassed by my first one.

KM: Fair enough.

BRoe: Is 'make the data immediately available to everyone because only a few people can do it, and you catch more flies with honey than with vinegar' kind of a thing. I mean that's, I think, my first one is broad enough to encompass that.

KM: Yeah, yeah, I agree.

BRoe: But I do really think that there were several people who were concerned about not immediately releasing the data. They were afraid that people would keep things secret; they were trying to get a proprietary advantage with it. And the Wellcome Trust, the NIH for all of the above reasons, reasons one and two, didn't want to have that happen. If they were patented then you would lose face, and so how can we do this? They needed to be out there.

KM: Yeah. What do you think of the implications of the Bermuda principles for science generally? By adopting this as a model for the genome project, has this set any kind of example or had effects on science in a more philosophical sense?

BRoe: I personally believe that it has. And my brain has just died because I remember there was some recent example where three or four pharmaceutical houses had collected all kinds of data that they were looking at and somehow or other they sat around a table or phone conference and said, "You know, let's just share this data between the three of us," or between some group. And here were people that were keeping data proprietary from the business community, from the scientific pharmaceutical community, and they decided to share this data. And lo and behold, my goodness, all kinds of good things came out of it. And so I think...do you remember what I'm talking about?

KM: No, I don't know the names of the companies.

BRoe: But it was a specific example of maybe...I don't know, several years ago, a couple of years ago, a handful of years ago.

KM: Bob would know, my boss. We'll ask him.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

BRoe: But there was this collaboration that GlaxoSmithKlein and Merck and somebody else I think got together and said, "Okay, we'll share this information." I think that they probably had ulterior motives because there were some other international companies probably going to scoop them on something. But this was a way that these companies could gain some advantage. Because I personally look at business as being fairly greedy that way. But that's just my own liberal bias. So I do think that lots of good stuff has come out of there.

KM: Do you think that it's fostered more coordination or collaboration within science itself, outside of business? Do you think that not just amongst biology, but amongst the broader scientific community? I'm talking much more high level, philosophically, do you think this has affected your discussions with other scientists about how data should be shared?

BRoe: First the answer is yes, but every time you talk with colleagues outside of the genome project, they go, "Well I've got a small grant, a couple hundred thousand dollars a year. I'm competing with these other five groups for this. I'm not going to tell people much about it until it gets published." And there really was no place to put enzyme kinetics. It's fine with the rest of us to wait until that comes out in *Journal of Biological Chemistry* or *Biochemistry* or something like that. But I think that the whole idea, and I just...the whole idea of these open collaborations, I think have spilled over into several important areas, none the least of which is like *PLoS ONE*, you know?

KM: Right.

BRoe: And these kinds of journals are now getting very, very high exposure because they're there for all to read. I mean you're peer reviewed and everything else, saying, how big is this whole thing about being...whoever said that ... to reinvent some part of themselves because of the Internet and how they deal with science because of how they deal with publishing of science. They're going to have to come up with different business models. I think that was originally started...I mean this project has two things going for it from the beginning.

First, it was the first project that scientists agreed, after much gnashing of teeth and everything else, to make the data freely publicly available. We were unhappy if someone were to take all of chromosome 22 and try and analyze that before we published it. I mean there were rules of how...and in fact that probably was the substance of the other meetings as well as how much of the data are you going to allow people to publish on, or how are you going to make it available to them and all that kind of stuff. Are there any constraints that you're going to put on it? Like the biggest piece you could look at was 100,000 bases or so, I don't remember, but there were some constraints that were put on. But I mean this has led to open science. To much extent, it's led to more poster sessions I think. At meetings, I mean when I was a graduate student we really didn't have poster sessions as I

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

recall. I mean we went to meetings like FASEB or something like that and we sat in these big huge lecture halls and we listened to famous scientists speak, whether they were boring or not. And that's what we did. There wasn't a free exchange.

And then all of the sudden, with this and...I think it was...there was a move afoot already before the human genome project maybe to try and open up more communication in science. I think probably because a lot of PhDs that were being produced at the major schools were going out and having their own labs and wanted to keep open lines of communication in and think a little more out of the box. But I still know places that you can't talk to your colleagues down the hall because you're afraid of them stealing your ideas. By the way, that was not the case in Oklahoma. In Oklahoma, I was very excited about Oklahoma because it was an open place and people wanted to collaborate with us. And the medical school, in fact, I got the funding for that first instrument, with the help of the chair of the Department of Microbiology, Joe Ferretti at the Oklahoma University medical school, which was 25 minutes away. The Presbyterian Hospital had just been sold to HCA or one of those groups, and so they set up a Presbyterian Foundation. Dr. Ferretti, who happened know several on their board, said we can get you the \$90,000 you need for that machine. Professor Joe Ferretti was very, very forward looking. We got that machine and in fact we sequenced several bacteria with him, some of the first bacteria that were ever sequenced. And so it was...some people had the vision and some people didn't. Also, I think that you constantly have to fight to keep these things open because the tendency of human beings is to hunker down and get their own little fiefdom.

KM: Right, right. So in retrospect, do you think that the principles for the genome project were extremely effective? And are they still relevant and valid in science today? Perhaps in...you mentioned you talk to your colleagues who are involved in smaller projects, that involved less money, but in larger projects that involve coordination amongst lots of labs and folks, you think that these types of data sharing norms are something that should be followed?

BRoe: Yeah, and in fact, NCBI wanted to close down one of their repositories and eventually reneged because we wanted that data available because we were looking at it. In this instance, the Wellcome Trust said they would store the data and make it available on the web, I think played a large role in NCBI's decision not to close this database as somehow or other David Lipman came up with the money to maintain that and keep it going. It was, "Oh, no, no, I didn't mean we were going to get rid of it. I just wanted more money somehow to keep the thing going." His budget had been cut somehow; and he had a valid point, it got through. The scientific community said, "We need this." It was essentially, "We need open access to that data." And he [Lipman] got the funding to maintain that, which I think is marvelous. So, yeah, I think it still has an effect. But you still have to fight for it because people want to hoard things; people want to patent things.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

I'm just so pleased as punch about the...although I'm afraid of what the Supreme Court is going to do now after the decision yesterday, but pleased as punch of what they did with the two decisions on patenting. I mean I have friends that were running one or two tests at Children's Hospital in Tulsa and they got a letter from Myriad that said, you can't do this, cease and desist or we're going to somehow or other try and get money out of you for doing this. But because of the threats from Myriad, he had to stop doing these tests.

KM: Yeah, we've been covering the Myriad case a lot over at Duke because my boss is pretty much obsessed with it. And we've got a big briefing page and everything. So we cover that pretty closely and we're watching it in a lot of detail. Anyway, whom do you think the Bermuda principles were the most helpful for? Do you think that it was for...what about for the smaller labs earlier in the project being able to see some of the data from the larger labs? Do you think that there was a group that benefited more than the others in this full open data release?

BRoe: All I can say is I think a lot of groups benefited from open data release. These were mostly folks that were interested in their genes. And they had already looked at cDNAs from lots of different species and now we had the gene from humans, so then they could go off and maybe get a core lab or somebody else to sequence the gene from mice and all the way down the wall. And eventually then pinpoint their work as to which mutants they were going to make. So if you were doing mutant genesis I think it helped all those folks to do that.

Plus I just think it benefits the community, the evolutionary community, the more hard core biochemical community that was interested in how enzymes worked, was making point mutations and saw a mutation that happened, it was a different one in yeast, and so they went back and made that. So I think that getting the data out there, if you're doing high throughput sequencing you've got to get the data out there, and it amazes me the number of people that are using that.

KM: Do you remember any impediments to releasing the data? Did people run into any particular roadblocks in terms of getting the data out there?

BRoe: [Laughter]. I can't speak for all the other people for roadblocks, okay, I'm sure that since they probably have...I'm laughing because one of the deans at my university who will go unnamed, actually an under-dean, not the dean of arts and sciences or something, or the dean of the college of medicine or the dean of the graduate school, one of the associate deans or assistant deans, who was interested in getting things patented at the university called me up one day and says, "My God, [BRoe], you're releasing all that data! And we haven't had a chance to look at it yet." Gee, you know, I'm sorry, but I signed off on that and you signed off on that when you accepted my NIH grant because that was the criteria for the grant. Then he said, "Oh, I didn't know that but even so can we go back and look at that data and see if there's something we can patent?" And I said, you can go back and

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

look at it all day, but I refuse to patent anything. And I wasn't alone in that. But this administrator was just taken aback that I was releasing all this data without trying to patent it.

I had been on the patent advisory board and a lot of my colleagues when they came up with stuff, compounds, they would patent it because it had made better road tar or something like that. But the university refused to patent his discovery with making some polymer for roads. When he retired he formed a company that just cleaned up on that. But university patent offices think they're going to get wealthy on these things. And they're really not. And I personally think that it just stifles science. But anyhow, I think that, yes, I think that if GlaxoSmithKlein comes up with a novel drug that of course they learned about the structure of that drug by looking at natural products, because that's where all the drugs came from, but then they made modifications to it and they find that it cures leukemia, my God, they deserve to make money. They don't deserve to make as much money as they're making. But they deserve to make some reasonable amount and I guess that's just my own opinion. However, they really should think and stop charging people so much money for new drugs. But I guess they have their reasons.

KM: Right. Did you participate in any of the discussions after Bermuda? The Fort Lauderdale or Toronto meetings about genomic data sharing?

BRoe: No, I actually didn't. By then I was out of the human genome project and those involved where the very large centers that remained. I was out of the NIH loop. When those things came out I was basically funded by NSF at that point in time. Yeah, I did not attend those meetings, and in fact, was not invited to those meetings because there was no reason for me to go.

From what I gathered, at those meetings the policy needed to be updated. It needed to be tweaked a little bit. I understand that. And I got feedback from former students of mine who went to those meetings, and sent me an email asking if the suggested changes were good ideas. Usually I then said, "Okay, let me think about it." And in pretty much all the instances I agreed with what they did. I think there's a little bit of a thing to hold back some information now that sort of disturbs me, unless I'm just misreading something. But I think there's a loophole that you can get around the 24-hour data release policy. I'm not happy with that loophole, but *c'est la vie*.

KM: Right. If the meeting were to be held today in Bermuda, do you think that it would make sense to include some groups of people who weren't there?

BRoe: My feeling is that there were a lot of women there. I think there was at least one African fellow as I recall; maybe I'm wrong. I think that all the major players at the time were there. The chromosome specific people who were...they were already forced out many of them, and it was just down to the sequencers. And so the mappers weren't included by and large, unless they moved their way into ...

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

KM: Into sequencing later, right.

BRoe: So like the group at the University of Texas, oh, what's the guy's name, oh yes, Glenn Evans. He headed the group that did chromosome seven and was involved with that. I think he came to that. I don't know if Skip Garner, who also was involved with the human genome at UT Southwestern went to any of them or not. But there were representatives from everybody that had continued on in sequencing pretty much. I can't think of any sequencing group that was left out because this was early on in sequencing and it was a pretty narrow field at that point in time. But there were a lot of women there. Jane Rogers was there, Jane Peterson was there, and a woman who represented big pharma from Germany was there, and so women were represented. But I thought it was funny the way you put that, gender balance and representation. I don't think anybody even thought of gender balance. I think we thought about the people who were invited were the best scientists.

KM: Yeah, no, where that question came from was we have the programs and the lists of people who were invited to these meetings and it just ... we were transcribing the names and we were like, wow, these are all guys. And then we came to a few women. But we decided that it might be fun to ask the question, if it seemed as if the balance was a little bit off-key. The response we've been getting from most people is yeah, the folks who were there were the best sequencers. And those are the people who were represented, and that's who should have been there. Is there anyone else that you think we should interview that we maybe not have thought of inviting to interview because maybe they weren't at the meetings? Or is there anyone who looms particularly important in your mind?

BRoe: Have you talked with André Rosenthal?

KM: Yep.

BRoe: Have you talked with any of the groups from Japan?

KM: Yes, we talked to Yoshi Sakaki.

BRoe: Okay. But you didn't talk with Professor Shimizu also from Japan, although Sakaki is good, he fought against that government on this, as he wanted immediate data release. It seems as if he felt so strongly about this that he just said, "Screw you guys, I'm going to just put this stuff out there." He went against his government. Our collaborators at Kyoto were a little bit more restrained but they still went along with it.

KM: We've also spoken to Jean Weissenbach and Gert-Jan van Ommen. And lots of British and American folks.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

BRoe: Sure, sure. I can't remember...I think that Jean was...I thought he was pretty much in favor of immediate data release but I don't really recall exactly. I think their funding was from the CNRS and I don't think that they had pharmaceutical funding. So I thought he was pretty open about that. There was no one from Italy there, there was no one from Spain or Scandinavia, but they really were more peripheral to this project. They weren't necessarily major data producers.

KM: Do you have any, just as a last question, do you have any documents or notes or anything from that time period?

BRoe: I've been asked that before, and in fact Mike Morgan sent me an email, maybe because of this study, and it was a year or so ago, wondered if I had taken any good notes there. And I thought to myself, I am a really crappy historian. The only thing that I have saved is my biochemistry lecture notes from my last year or so of lectures. And I don't know why I'm saving them; I'm not going to do it again. Except that I think about them and I go, "My God, they'd have to be all rewritten now." I mean just the way things have moved is just incredible. So it's really an exciting time.

I think it's been an exciting time to be in science and an exciting life. I read an annual review in *Biochemistry* and I can't remember who it was, he was an enzymologist I think who they always have a first few pages of somebody's biography of some sort when they sort of describe their life in science themselves and he ended it with, "The joy of science is the people you meet along the way." And I think that's very important because there are a lot of people that maintain relationships for over 20 years and we go drinking together and we have a good time together. And if I don't like everything you do but still somehow or other we got along and somehow or other respected each other. I'm sure you're visiting with Craig and I'm not going to say anything more there. But I've been on record...in a couple of books I've been quoted somehow or other, probably justified, but I don't know...about my feelings about him. But we still sit and talk when I run into him at meetings. It seems he's doing fine, he's having a good time and as long as he's enjoying himself, that's what's important. But really the joy is having known all these people, having been part of this.

I hate to say I was born in New York City and raised in northern New Jersey and Long Island, and I always kidded with people in college that my hometown newspaper was the *New York Times*. And it really was *News Day* or the *Long Island Press* or something, but I always said it was the *New York Times*. And they would always laugh at me. And so when you graduate from college they say, "What newspaper do you want this sent to?" And I'd say the *New York Times*. And they would laugh and they would say, "No, no, you're from Long Island." This will go to *News Day* or something, so it ended up going to *News Day*. But the exciting thing was, when we sequenced chromosome 22 we sequenced the first chromosome and we published that in 1999, our three groups, and I was

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

interviewed on the McNeal-Lehre report with Francis Collins, he was on from Washington DC and I was on from Oklahoma. I had my 15 minutes of fame and then some as it lasted a lot longer than that. The *NY Times* even called my sister on Long Island and they said, "What do you think about your brother being involved in this?" And she said, "Oh, he was always interested in science." And it made the *New York Times*! So it actually did make my hometown newspaper. So it was kind of fun. It's just been a delight.

I've enjoyed it. [BRoe: *I just saw a story on TV last night on personalized medicine using one of the next generation sequencing machines and just thinking about the amount of data that we collect now for next to nothing. I mean we did the human genome project essentially at a cost of a dollar a base, three billion bases, for \$3 billion. And in Sanger's lab for the mitochondrial genome, the human mitochondrial genome was probably \$10 a base. But, yeah, I mean this is really wild. And that we're still doing sequencing after all these years and I still find I'm enjoying it.*]

As I sadly told Mike, I think when we moved up here and had to clean out my office for another professor, nice guy, and so, I said fine, so I had to clean out everything, and I don't know where all my notes went. But I'm glad that you have transposed this, so now I will finally have my version of my history in science.

KM: Yeah, and if you let us, we can put it online on the Internet with our public database so anyone who's writing a paper on this will be able to find the transcript and see it.

BRoe: I don't think I've said anything that needs to be redacted, have I?

KM: Oh, no, I don't think so. But as I said before, we got that Certificate of Confidentiality independently of this project, but this project is still covered by it.

BRoe: I don't think I've said anything that's...actually putting that little blurb in there made me keep some restraint, or at least make sure not to say some things that I maybe would have said if not.

KM: Well thank you so much. This has been so useful and fun to talk with you. I really, really appreciate your contributing to our project. And we'll send you the transcript as soon as we get it, which will be in a few days. Any corrections you want to make or deletions or additions, you can feel free to do so. And we'll be in contact with you. Just thank you so much. I loved hearing also all the background of your career and everything with Fred Sanger.

So this is part of a broader project that studies DNA sequencing, as I mentioned, and last fall we went to Harvard and did an interview with George Church and Wally Gilbert, and then about three weeks later John Sulston and Bob Waterston

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

came down to Duke and gave a public lecture and we did interviews with them. It's been such a crazy ride. I was telling my boss, Bob, I said, I've interviewed two Nobel laureates in three weeks.

BRoe: Oh, yeah, I mean it's really been cool. It's been really exciting. And again I said the joy has been in knowing these people. Have you talked with Eric Lander or anybody in his group?

KM: I've got an interview with Eric Lander at the end of April. So that's when we'll be talking to him.

BRoe: Okay. And Lee Hood?

KM: Maybe not for this project. We may talk to Lee for a broader... we'll definitely talk to Lee...for our broader project on DNA sequencing.

BRoe: I mean not even ... I don't know if Lee was at that meeting or not ...

KM: He was at two of them. I think he was at '97 and '98 maybe, or maybe '96 and '97, but he wasn't at all three.

BRoe: He was at the first two I think.

KM: Okay, yeah, the first two.

BRoe: But Lee Hood, I mean who would...without him and that sequencer, his vision, and in fact, when we published the C-abl and BCR genes, they published an immunoglobulin gene cluster and those were the two biggest pieces of DNA ever sequenced at that time. And he...I don't know...he was funded by the Genome Project in some way, shape or form I think. And the Hunkapillers were there. I don't think they were at the meeting, though; I don't think either of them were at the meeting.

KM: The Hunkapillers weren't, no. So my computer battery is about to die; I'm going to have to run off on you or else I'm going to rudely cut you off.

BRoe: Okay, well thank you so very much. You will send me that as an email attachment?

KM: Yes. Thank you. And we'll be in touch. This has been so helpful and very entertaining.

BRoe: Thank you, thank you. Well that's my role in life is to be the Rodney Dangerfield of genome science.

Interviewee: Bruce Roe

Date, location, method: 30 March 2012, Durham, NC, via Skype

Interviewer: Kathryn Maxson

KM: Excellent. Well thank you so much. And we'll be in touch, okay?

BRoe: Okay. Thank you.

KM: Yes.

BRoe: Bye-bye.

KM: Bye.

END OF RECORDING