Informed consent for: "The ethos and effects of data-sharing rules: Examining the history of the 'Bermuda principles' and their effects on 21st century science"

University of Adelaide
Duke University

Researchers at the University of Adelaide, Australia, and the IGSP Center for Genome Ethics, Law & Policy, Duke University, are engaged in research on the **Bermuda Principles** for sharing DNA sequence data from high-volume sequencing centers. You have been selected for an interview because we believe that the recollections you may have of your experiences with the International Strategy Meetings for Human Genome Sequencing (1996-1998) will be interesting and helpful for our project.

We expect that interviews will last from 30 minutes to much longer, but you may stop your interview at any time. Your participation is strictly voluntary, and you do not have to answer every question asked.

Your interview is being recorded and we may take written notes during the interview. After your interview, we may prepare a typed transcript of the interview. If we prepare a transcript, you will have an opportunity to review it and to make deletions and corrections.

Unless you indicate otherwise, the ***information*** that you provide in this interview will be "on the record"—that is, it can be attributed to you in the various articles and chapters that we plan to write, and thus could become public through these channels. If, however, at some point in the interview you want to provide us with information that might be useful for us to know, but which you do not want to have attributed to you, you should tell us that you wish to go "off the record" and we will stop the recording. We will, however, take notes for our own use. When you are ready to go back "on the record," we will resume recording. Anything you say while "off the record" will not be on the audio recording and therefore will not appear in the transcript.

All ***materials*** from your interview (audio recording; transcript; interviewer's notes) will be available only to members of the research team affiliated with this project, unless you consent to their wider use, as described in the paragraph below. The digital materials will be maintained in a secure, HIPPA-compliant drive at Duke University. The paper materials will be stored in a locked cabinet.

In addition to the scholarly articles and chapters that we plan to write, we also hope to create a resource for other scholars and members of the public. We plan to post some of our research data to online digital archives. While we will use your "on the record" comments to inform and write our articles, we will not post your interview transcript or audio recording online unless you give us permission to do so, in a separate agreement. At the time we send your transcript to you for review, we will also provide a consent form asking your permission to post your interview transcript and/or audio recording online. The form will provide you with different options for how, when, and with whom the materials may be shared. You will, of course, also have the option not to share the materials beyond the Duke and Adelaide researchers.

One risk of this study is that you may voluntarily disclose identifiable information that later could be requested for legal proceedings, or otherwise be used against you. Please take this into consideration when you are speaking. There may be other risks associated with your "on the record" views being made publicly available, such as having your views mischaracterized or misunderstood.

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 R H Waterstow

Printed Name R. H. Waterstow

Date 11/15/11

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

Contact information:

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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.*

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Signature _____

Printed Name JOHN SULSTON

Date 15/11/11

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

Contact information:

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Archiving Permissions Form: "The ethos and effects of data-sharing rules: Examining the history of the 'Bermuda principles' and their effects on 21st century science"
University of Adelaide
Duke University

A short while ago, you participated in an interview with investigators engaged in a research project exploring the history and consequences of the Bermuda Principles for DNA sequence data sharing. We have prepared a transcript of your recorded interview. As indicated in the Informed Consent statement for this project, you now have the opportunity to review this transcript and make deletions and corrections.

Your transcript has been sent to you in either electronic format (via Dropbox.com or e-mail communication) or hard copy format (via postal service). Please follow the instructions provided with your transcript when making any changes and when returning the document to us. These instructions are specific to the format in which you received your transcript. If you do not want to make any changes to the transcript, please let us know at the time you return this permission form to us.

In addition to the use of your interview materials in our research, we seek your permission (subject to any restrictions you impose) to place the edited, written transcript of your interview, and any related documents, on the Internet in institutionally affiliated, digital archives.

These archives may include:

- Archives affiliated with the **Institute for Genome Sciences & Policy**, Duke University.
- Archives affiliated with the **Duke University Libraries**.
- Archives affiliated with the **Genentech Center for the History of Molecular Biology and Biotechnology**, a part of the Cold Spring Harbor Laboratory (CSHL) Archives,[1] or
- Archives associated with the **Human Genome Archive** at Georgetown University.[2]

Members of the Duke University community, students, faculty and staff at other institutions, or members of the general public may access these digital archives for purposes unrelated to this research project on the Bermuda Principles. Typical research uses of interview materials include scholarly or other publications, visual presentations (i.e., powerpoint presentations), exhibits, class projects, or websites. However there may be other uses made as well, since the materials will be available to the general public. Investigative reporters and lawyers engaged in or contemplating litigation have, for example, used the Human Genome Archive at Georgetown.

Your permission to post the edited, written transcript of your interview, and any related documents, to a digital archive is completely voluntary. Unless you consent to their wider use, all materials from your interview will be available only to members of the research team affiliated with this project.

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

[2] The Human Genome Archive at Georgetown University was established in 1988 under a grant from the National Science Foundation, and was long associated with the National Reference Center for Bioethics Literature and other international resources supported by the National Library of Medicine and other components of the National Institutes of Health.

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: Robert Waterston Date of interview: 15 Nov 2011

Current institutional affiliation: University of Washington

Street Address: 3720 15th Ave NE

Phone: 206-685-7347 Email address: watersto@gs.washington.edu

Interviewer Information.

Full name(s): R. Cook-Deegan; R. Ankeny; K. 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: R. H Waterston Date: April 3, 2017

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: JOHN SULSTON Date of interview: 15 Nov 2011

Current institutional affiliation: Emeritus Wellcome Trust Sanger Institute

Street Address: (home) 39 MINCLE LANE, STAPLEFORD, CAMBRIDGE CB22 5SY

Phone: +44 1223 842248 Email address: JSULSTON@YAHOO.CO.UK

Interviewer Information.

Full name(s): R. COOK-DEEGAN; R. ANKENY; K. MAXSON

Affiliations(s): _____

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

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- [ ] NEVER: MAY NOT BE DEPOSITED IN A PUBLIC ARCHIVE

Please specify any further restrictions in the space below:

_____

Signature: JE Sulston Date: 21 Jan 2012

BCD: The informed consent documents are signed, so we have them here. Why don't we introduce ourselves so the person doing the transcription can recognize voices? I'm Bob Cook-Deegan.

RA: I'm Rachel Ankeny.

KM: I'm Kat Maxson.

RWaterston: [RWaterston].

JSulston: [JSulston].

RA: Just to confirm, both of you attended all of the Bermuda meetings. Is that right? Yep? Okay.

RWaterston: All three.

RA: I just wanted to make sure. And can you, I mean for you two it's a little bit different than it would be for a lot of people, can you talk about who you saw yourself as representing, in what capacity you were there, what you thought about when you, in your case [JSulston], started to organize the meetings and what the motivations for the meeting were? Usually one person starts and the other person can then chime in.

JSulston: Okay, so I'll start with that. So I'm [JSulston] again. I was there representing the Sanger Center, or as it then was, the Wellcome Trust MRC Sanger Center, and I was director of that, so I had a very strong motivation to have the collaboration go well. And we recognized before the first meeting, what led up to it was that we were not efficiently covering the genome at all. There was a lot of needless competition, duplication, and we needed to get people in a room together and talk about it. We also needed to share science. Now this is something that we and Washington University at St. Louis—Bob Waterston's lab—were well versed in as a result of experiences both with the nematode and the early days of what we started to do with the human. So this was not unfamiliar territory but we knew we had to go in there and do it. So I was representing my lab, I was representing the Wellcome Trust because it was their lab, or they were responsible for the part of the lab that was doing human sequencing, and I had very strong motivation to have the project go well.

BCD: And were you the two biggest labs doing sequencing at that point?

RWaterston: Yeah, I think so.

JSulston: I think so.

RWaterston: Yeah, I mean I'm not sure who else would have been operating on that scale. And Craig was clearly working on, had done flu, but that was less than two megabases, and by that point we'd done well over ten, I think. I mean that was '96. We must have been …

BCD: And where were you in the curve of sequencing?

RWaterston: We basically doubled our output every year. And so I don't know where that was in '96 but we started out with about 400 KB in the second year and a megabase in the third year. So that was '93. And that's each lab.

BCD: And were you already encountering these problems of overlap and allocation of who's going to do which thing?

JSulston: Between these two labs, you mean, or with others?

BCD: No, not just between your two labs but had that actually happened in the real world? Was that part of the reason that you would have had this meeting in 1996 as opposed to '95 or '97?

RWaterston: I'll do my bit and answer that. So [RWaterston], I was there representing the Wash U Genome Center, and we were one of the awardees for the NHGRI pilot project grant. So that was the precipitating event as it were: about a dozen labs had received funding to start human sequencing on a pilot scale. Up until that point various labs had been doing a little bit of human sequencing here and there but this was clearly a new day, and various labs had done various things. And as I mentioned yesterday, by the time Bermuda rolled around we had already sequenced a clone that had also been sequenced by Andre Rosenthal. So there were clear indications that we had to do something about the nature of the organization.

RA: Were the initial grants given out in a way that was conscious or detailed enough that they avoided duplication among the American groups or not even that?

RWaterston: No.

RA: So they were vague enough for pilot work in such a way that …

RWaterston: I was not part of the review group obviously so I can't say what actually happened. I think people were reviewed by how well they might be able to do things and [I'm not sure] how much they took into account what territories people claimed. I think the assumption was sort of that this would be taken care of.

BCD: And did you specify what you were going to sequence?

RWaterston: We did.

BCD: Presumably you did, right?

RWaterston: You had to, in part because you had to be able to say that you had the substrate to sequence. You couldn't just say, I was going to sequence a chromosome for which you had no material, no experience. So we worked with the Wash U Human Genome Center, David Schlesinger's group, and focused on chromosome seven to begin with. This was Eric Green's work at the time.

RA: So if I … to ask you then, to take it back to [JSulston], what was your understanding of what was going to go on at these meetings? Or what was your take going into it about what all was going to be covered, from just your own point of view?

RWaterston: Well as [JSulston] indicated we clearly had to compare science. There were still a number of competing ideas about how the genome should be sequenced. But we also had to deal with the issue of how to get people to play nice. And it had been a problem in the mapping phase of the project. People had used their grants. They had a chromosome in mind, but all too often the area around their favorite disease genes was very well mapped and the rest of the chromosome was much more sketchily done. And we couldn't do that obviously.

RA: And [JSulston], for you, were you fairly closely involved, I assume you were, in setting the agenda for the meetings and so on? And how much did Wellcome have a say, either through Michael Morgan …

JSulston: Yes, let me make a correction. I said I was representing Wellcome, which of course I was on the scientific side, but Michael Morgan was very much representing the Trust. And he and I talked quite a lot beforehand, and of course [RWaterston] and I were comparing all the time. So he would have been involved. I don't remember all the details. But Michael and I certainly kind of began to float the idea earlier that year, I think.

BCD: But Michael also must have been in contact with Francis.

JSulston: Then he was in contact with Francis, yeah, exactly. So the interesting thing is that we had parallel conversations going on between the scientists directing the larger sequencing labs and the funders responsible for those labs. And I think that's a very important part of the Bermuda success, is that both those sets of people came into the room.

RWaterston: Yeah, it was not only Francis and Ari, but the Japanese funders, the German funders, anybody else.

JSulston: We wanted everybody there who was seriously doing this. That was the idea.

RA: And going into it was it your intention to come up with something that came to be called the Bermuda principles? Or was that something that emerged?

JSulston: My own phrase at the time, I think it was my phrase, was the etiquette of sharing. And this again, is a sort of reflection of the quite tight informality that [RWaterston] and I'd enjoyed and built together through the map and the sequence of dealing with the worm community. And I mean advisedly 'dealing with' because you had some people who get very anxious about, for example in the mapping phase, what's going to happen to a cosmid where they have a gene? Do they own the adjacent sections in some way, and what would we do? And we sorted out as we went along rules, which really are etiquette rules. It's a matter of working out exactly, as with any commons, how to share it in an equitable way. And so that was the attitude I had. I had no idea what we were going to produce from the Bermuda meeting but I wanted to establish that etiquette.

RA: And so in that sense, I mean yesterday you said it was practical not spiritual.

JSulston: Yes.

RA: Say more about what you mean.

JSulston: Well, I was echoing [RWaterston's] words I think and confirming it. And although when I speak now as I did yesterday, I introduced what you might call a policy element, and I suppose it is spiritual a little bit because I think that these principles extend to the way we need to run the world. But it was that way round, it's starting on a bedrock of stuff you know, of stuff where you immediately see the application of this particular kind of process and then it's fanning out from there and saying, well it worked in the worm so let's try and do it in the human. Now it works with the human genome so let's try and do it with more biological information. It works for all that, let's try and do it on a larger scale still. So it's in that way that you grow, if you like, the spiritual or policy element out of that.

RA: And we've started obviously to interview people, talk to them, and it's really striking to me about the differences in people's expectations about what was talked about or what was going to be talked about. Just out of my own curiosity, was it your sense that it was going to be governed by, say, Chatham House Rule or what the norms of engagement were intended to be? Was there

an awareness of the importance of this in a moment such that people were nervous about what they were going to say and needed protection?

RWaterston: You and I were talking about this this morning, and I don't remember that being a concern going in. It was in large part a scientific meeting.

BCD: As we look through the notes, in fact, it's quite striking that session after session of, this is how much we've done, at what accuracy, here's how we were defining these technical terms. It's just presentation after presentation of what's going on.

RWaterston: And that's a lot of it, is to get clarity on what really had been done, what people's capacities really were and not just grantsmanship or puffery. Or modesty. To really figure out where things stood so that you could make a decent plan. And so the idea was that we'd spend the first sessions sort of getting clarity about what people had actually done. And then later we talked about what they could do. I think that's right, isn't it?

JSulston: Yeah, that sounds right.

KM: Did you go into the meetings knowing about the Chatham House rule?

RA: Oh, you mean at that meeting?

KM: Right.

RWaterston: I never heard of Chatham House rules until much after.

RA: Well, you'd been to a Gordon Research Conference or something like that, and lots of scientific meetings. Because that, in some way, it's not called that, but you don't cite…

RWaterston: I think it's different from, that's even more extreme … or less extreme. I mean Chatham House rules are more extreme than Gordon conferences and so forth. You go to a Gordon conference and you know you can't cite it in a publication but you can talk to your colleagues about it and you can say so and so. That's how word gets around. Chatham House rules …

RA: Are much more extreme.

RWaterston: … are more restrained as I understand them. I still haven't read them.

KM: I was wondering if there was any sort of explicit knowledge that this is how these meetings would be conducted.

JSulston: I honestly don't remember. I think the person to ask would be Michael about that if you want a clear answer. Maybe you've done that already.

RWaterston: Something might have been said. Francis and Michael jointly chaired the meeting and they made opening remarks and they would have set the ground rules.

RA: And another one that comes up a lot was when you were thinking about what you wanted on the agenda, what might get talked about formally or informally, how much was patenting a really active concern in '96?

JSulston: Well, ownership was a concern. I don't know to what extent I was thinking literally in terms of patents. We were, of course, very well aware of patents because of all the ESTs and the business around that. Whether it was really seriously coming into genetic sequencing at that point I can't remember.

BCD: You were already doing MERCK sequencing,* right? That started in '94.

RWaterston: Yeah, we'd already been doing MERCK. And I think InCyte had already started some of their stuff to glean patents. So I think we were … it was something that was going to get in the way.

JSulston: Yes, exactly. I bet I went around talking about how we had to get it out there to avoid, or break or whatever, to get the prior art in the public domain so that other people could use it. So I would have had that sense, definitely. I bet we discussed about that.

RA: So we talked quite a lot about the content of that initial meeting. What about the tenor of it? People's anxieties about what was going to be discussed, how it might limit their work. Was it a highly constructed, typical scientific meeting? Conflicts?

RWaterston: I think most people viewed it with some anxiety, certainly on the U.S. side. This was going to be a pilot phase with the idea that it was going to be winnowed down to a chosen few. That was already understood. It was a pilot phase. It was announced as a pilot phase. And there were 12, I think 12 was about the right number, I can't remember for sure. You guys could check all this. It might only be eight, I don't know. But there were certainly more centers than would be sensible to carry through on a full scale. So it probably wasn't explicit but certainly was implicit. And so we were … it was in that sense very different from what [JSulston] and I had done, where we got funding together from different sources. We (John and I) were not competing with each other, and even if we'd been in the same grant system we would have gone in together on the same grant. ******We did in fact originally hold a joint grant from NIH to get the worm sequencing started. But when we went*

* Merck's nonprofit unit funded Washington University to sequence human cDNA segments and post the sequence data publicly, starting in 1994.

*to production, Jim told us the UK had to support John's effort.**** These were all labs, on the U.S. side anyway, who were competing with each other at some level. But at the same time, clearly the expectation was that we were now going to cooperate.

****I [RWaterston] checked some things here. Some people had been funded to do human sequencing before the pilot projects and might have been at the meeting. Elson Chen and Bruce Roe might be examples. For the pilots, on our application the proposed start date for the grant was March 1, 1996, but in fact the award as issued was April 11. But people would have known by Bermuda how well they did. The study section met November 16-17 (1995) by teleconference, followed by site visits to applications deemed competitive (November 30 in our case) and a face-to-face meeting December 11-13. People would have been notified by phone shortly thereafter with summary statements a few weeks later. Watson at the meeting said the NIH had received 20 proposals. To give an idea of the competitive nature, the award was for two years, with the third year conditional upon performance. Of course in addition to the NIH funded labs, in the US there were the DOE labs at Livermore, Berkeley and Los Alamos.****

BCD: Was there any framework before this meeting? Were you doing phone calls or were there regular ways to sew this group into a community?

RWaterston: This, I think, was really the initial effort at it.

JSulston: I think so. We had this tremendously strong axis between Wash U and Cambridge. And we were really implementing that. We had lab visits going to and fro. As the labs got bigger, not everybody could go. But we had groups visiting.

RWaterston: In terms of our labs. I was just thinking and you just alluded to the fact that you had this kind of nice noncompetitive framework and then it becomes … both the funders and the sequencers in the same room.

JSulston: I was contrasting that with outside our axis. Really very different, much more casual at that point, I think.

RWaterston: I think the NIH grants were awarded that spring. It might have been a March 1st start, and so was concurrent with Bermuda. So this was the inaugural get-together, wasn't it? *[****See above for clarification of these grant start dates.****]*

JSulston: Yes, that's right. It was a curious thing that the Wellcome really jumped the gun. And in fact, I as director, my anxiety I should say, was definitely to be successful because the Wellcome had given us a lot of money already. And it

was not exactly pilot, because we were given enough money to sequence—not the whole genome, but certainly to do a chunk. There wasn't a specific sense of what the pilot was for as much as getting in there and getting on with it. We were trying to sequence the human faster and faster, exactly, and meeting obstacles now in terms of getting the big map together for the chromosomes that we'd picked and doing sequencing itself which is harder than the worm in certain respects. I think certain areas … might come to one of those in a moment … and so I was anxious that we would be showing our mettle. But on the other hand having this access with [RWaterston] was hugely reassuring because through that I was aware of what was going on in the U.S. So I didn't feel that I was walking into the unknown exactly.

RA: But it's a very different system. So your center is the only one that is being given money by Wellcome, is that right?

JSulston: That's right, that's right.

RA: It's clear that it's yours to do within a U.K. context. In the U.S., it actually explains a few people we couldn't quite figure. They did some sequencing at some point, but it explains those names on the invitee list of people who did sequencing but were never part of the human genome project. They were pilot grant winners probably that never got out. Because there are probably 12 all together. It makes a lot more sense out of something that I was puzzling over. So in the U.S. context there was a much more competitive … that's not quite the right word …

KM: When would you have heard about the recipients of the pilot grants? If it was a March 1st start date, when would you have …

RWaterston: Probably January or something like that. I don't know when we finalized the list of it. That must be how we finalized the list of invitees.

KM: And these meetings are at the end of February. So …

RWaterston: Was that when it was?

BCD: Yeah, it was the last few days. We should check though. So your recollection is basically that you went in knowing you were going to get funded and probably everybody was coming to this meeting knowing that they were about to get the grant.

**** I [RWaterston] have the dates as Feb 25th – 28th, 1996.****

RWaterston: No, I think everybody on the U.S. side was already notified as to whether they were funded, and I'm including in that I guess the DOE labs. They were not in

the same funding pool. But there was more coordination and in a sense they were competing with one another. There was LBNL and Los Alamos and … was there anybody else? Those are the two that I remember. *[See notes above for Lawrence Livermore Laboratory also.]*

JSulston: This was just the human funding, and I think the thing is you, like me, must work with basing on very solid, one of the biggest sequencing labs in the world because of the worm as well as the Merck sequencing, of course. All of that happened, so you built big, bigger than anybody else in the room, I presume, in America.

RWaterston: Pretty small still. I mean Eric [Lander] was, Eric though was already a big lab. They were doing lots of mapping.

JSulston: Yeah, no, I mean in terms of sequencing.

RWaterston: But they hadn't really turned to sequencing in a big way yet.

RA: And so, I mean this is a slight parentheses, but the Wellcome Trust then is very special in certain ways about the way it allocated its money.

JSulston: This was a particular occasion. The point, the story with the Wellcome Trust is that they reorganized their stocks in the late '80s and greatly increased their income stream and needed … Bridget Ogilvie really needed some decent flagship projects. And it happened that as a result of the machinations around extending the worm sequence and the stories of Burke and Hood and all the rest of it that we know,* I was sort of pushed forward really. It wasn't that I thought, oh it's a good idea to apply to the Wellcome for some money. No way. It was a group of people including Jim Watson, Aaron Klug and so on who were kind of hatching this together, sort of with my knowledge, but there's quite a feeling of me being the boy being put in, you see. So it's not that somehow the funding system is different in the U.K., it was that the Wellcome were in a unique time in their history and they wanted a flagship project.

BCD: It's not the overall funding system but the Wellcome system.

JSulston: But of course they had the capability of doing that as any large charity does. Howard Hughes, or whatever, they can set their own rules, in that sense there.

RA: And in that sense then it seems--it's hard to imagine the Bermuda meetings happening except in this context of the Wellcome Trust really pushing them forward. Is this a typical move for the Wellcome Trust to try and articulate policy or get involved in large-scale coordination efforts or administer …

* Sulston is referring here to the idea of forming a private company that would sequence the nematode genome, promoted initially by Frederick Bourke, who was consulting with Lee Hood. See *The Common Thread* (Sulston and Ferry), chapter 3, and *The Gene Wars* (Cook-Deegan), pp. 333-340, for more details.

JSulston: I imagine the American labs would have done it. If we hadn't existed surely these labs would have got together. You'd have probably wanted to organize it and Francis would.

RWaterston: Francis would have been doing this on his own, I'm sure. And I'm not sure, I don't know what the communication was between Michael and Francis in any detail. I know they were talking, and they together went out and made sure that the other countries were represented.

RA: Yeah, and that was actually the next question. The other countries were brought in because of the extent of the research, because of their potential importance?

JSulston: Well, they were being funded also. And bear in mind that Jean Weissenbach in France …

RWaterston: Oh, and France too.

JSulston: … tremendously contributed. His map was absolutely key, and they were already doing some sequencing. He was trying to get money for sequencing. And there were the Japanese that also had a substantial stake. The Germans. So there were a lot of players. So it wasn't just the U.K. and America axis.

RWaterston: And a lot of this is of course a legacy or an outgrowth of mapping. It's the same countries, the same groups who were involved in generating maps, both genetic and physical. And so in terms of the group knowing one another, it's not actually quite correct that they hadn't gotten together in other fora and so forth. They knew each other from mapping meetings, and the worm was sort of left aside for most of the NIH meetings. We didn't have to go.

RA: And so I mean on the axis then the anxieties obviously were in part that you might find out that someone else was doing really excellent work and you were due to be squeezed out because you weren't keeping up. But other sorts of things people thought were either going to be imposed on them or they were going to lose out on as a result of this gathering, or its outputs?

JSulston: Well, I mean obviously there had to be a sense of doing deals, of dividing things up. I say get together, sharing, but I mean that's what it boils down to, is somehow constructing agreements so that we don't duplicate and we do have something about release. Incidentally, the release thing, you mentioned patents, but more important I think than the actual patents was what might lead up to them. And that was really I think again a pragmatic reason for trying to establish, which we did, a very rapid release. You can justify it scientifically anyway, but the very rapid release was deliberately so that the members of the growing consortium would not be in a position to sit on

material and work on it in a biological way. Because that was not the job of these labs. And that was something that really had to…we were accustomed to this idea in the worm and then in the human, but other people were not. Other people were, as [RWaterston] says, it was growing out of their mapping and genetic activities and they wanted genes, and they wanted genes. Indeed it would involve patenting, but in general for development, translational research, and that was the whole idea, that the same people were doing. And it was very clear to us I would say that if this went on it would just Balkanize the whole thing and people would be competing; therefore, rapid release immediately gets around that because things go straight from the machines as far as possible into the Internet. Nobody has prior right to them.

RWaterston: It also in a way sets up the terms of the competition. You talk about it as the etiquette of sharing and that's what we focused on, but looking back on it it's sort of setting the terms. You're not competing on whether you get a gene out there and a *Cell* paper because it has something important to do with some human disease. But it has to do with how much sequence you're producing.

JSulston: And quality.

RWaterston: And the quality and the cost, yeah.

BCD: Yeah, it looked like there was a lot of talk about accuracy and how do you know …

RWaterston: Oh, yeah. No, no, that was a big deal.

JSulston: Enormously important.

RWaterston: Because you could obviously do a lot more if you didn't do it at quality. And indeed that was our initial proposal, we would do a quick run-through the genome and get stuff out there that people could use, but then we would return and make it all right. And that's what we put forward in '94, '95.

JSulston: Yeah, '94, end of '94.

RA: So once you got to the discussion of the rules that became the principles, or the idea that became the principles, what were the ones that people seemed to be convinced were good ones? What was debatable? What were the worries? Were they pragmatic? Was it actually sort of big-picture philosophical about how to do science?

RWaterston: You mean once we got to ... I think there was heavy discussion on all the points. Different people were concerned about different aspects. The Japanese and German governments, I think, were very concerned. They saw this as very

problematic. They wanted patents, and I suspect they didn't believe that the U.S. wasn't going to find some way to skirt around the issue. And the whole thing about first to publish is the first to file or whatever, what's the difference between the U.S. and the …

BCD: There's a U.S. grace period.

RWaterston: Yeah, so you can publish, you can release data in the U.S. and then up to a year later you can still patent. And in the rest of the world that's lost. And so there was a lot of concern about that. I remember that being a point of … a real sore spot with the non-U.S. They just thought they were being led down the primrose path, I'm sure. They didn't say that.

RA: And smaller centers in the U.S., did they have similar concerns about being squeezed out or somehow being subject to rules that weren't appropriate for them?

RWaterston: I'm not sure why.

RA: Yeah, I'm not sure why either. It's something people have said that I …

RWaterston: I'm sure they felt like they might be squeezed out, but not because of data release but because we had a head start.

BCD: Well, and it's hard stuff to do. It's obviously …

JSulston: And efficiencies of scale as well.

BCD: Walk us through how that whiteboard got constructed. Is this coming from a discussion you guys had over a beer that morning? Where is this coming from? Beer in the morning; excuse me, beer in the evening.

JSulston: I was writing on the board and I was … I don't know why it was me writing on the board. I like to think that [RWaterston] was conducting the meeting and I was writing. But I have no idea what happened. I really don't.

RWaterston: I can't remember if we decided that thing on the spur of the moment or whether we had thought about that ahead of time. We clearly had thought about what had to be discussed ahead of time. We had phone calls and emails ahead of time, but thinking generally about the kinds of issues that had to be raised. I don't think we had any, like [JSulston] said, I don't think we had any clear idea about what had to be the outcome or anything like that.

JSulston: I feel it is quite important we didn't, because we probably came across as actually listening, which was hugely important. I'm sure it must have been.

It's very interesting actually. One thing I do vaguely recollect, and with hindsight it must have been the case... It really was déjà vu for me because I remember being on my hooves at the worm meeting—twice, really: in the mapping episode when we gave them updates, and then as [RWaterston] said, much more at the sequencing stage. But in the beginning when people were skeptical I, and then the two of us, were definitely sort of promoting the map and explaining what we were doing. And then people would gather round and share bits of these little fragments of map and say, look at the cosmids. And then as it got bigger we pasted it on the wall of the big auditorium. And there's photos of that if you're interested.

And then same thing over sequencing. But to repeat what we were saying yesterday, there was very much anxiety when we got into sequencing of the worm about the cost. And the fact that clearly for the amount of money we were spending on sequencing people could see themselves doing all their projects in the research area. And so again, it was a real … quite tricky bit of diplomacy for us to sell this and explain how we were piloting it and we would see how it went. And until the yields started to emerge, the cross-matching in the computers, both within the worm and then between the worm and other animals, and suddenly people realized the power of this.

So it [Bermuda] was a sort of third session along the same lines in a way of trying to listen to the anxieties, to reassure people, and then somehow or another distill onto the board. And I think that photograph at the whiteboard with the scratchings out, because I think that's exactly what went up. I don't think we erased the whole thing and started again. I think I started writing, a few things got crossed out and so it wasn't a sort of pre-defined set of bullets or anything like that. I think we were just trying to get a short note of the conversation. Am I right?

RWaterston: Yeah, but I think we had topics we clearly wanted to deal with. How to share data, how to figure out …

JSulston: Oh, these were clear.

RWaterston: … who was doing what.

BCD: Is that a direct export from your experience? Those bullets, to an external observer it looks like there's a conceptual framework that's being distilled on the whiteboard. So is that derived pretty much directly from your experience with worm?

RWaterston: We had a lot of experience with worm but [JSulston] mentioned people's concern about how we were spending all this money and whether we were being heavily favored. And we wanted to bring this out at the meeting so that

that didn't become a concern because we'd certainly seen that in the human mapping phase. How do you build up the trust of the community for this? It was large sums of money.

JSulston: I really think it started as a listening thing. I do not think we walked in there saying we were going to produce a Bermuda agreement. I think it was the note of the conversation.

RWaterston: But don't you think we had the list … we had issues? We had issues.

JSulston: Absolutely. We had in our minds and we'd discussed them explicitly between us, and of course with some other colleagues as well. It wasn't like it was in a vacuum. We knew what was required in some sense to make this work. But we did not have it laid out in any form. There was no distribution, for example. There was no handout for that meeting. Typically if you had a plan you would copy it, hand it out and say, what do you think? And people would start to amend it. Absolutely not like that. It was a conversation.

KM: So that session was at the end of the 1996 …

RWaterston: I think it might have been the last session.

KM: Yeah, and you know going into it that this is going to be some discussion with people in the room about more philosophical issues. You knew that much, right?

RWaterston: There was philosophic intention … or not philosophical, but …

KM: By philosophical I just meant not like, this is the data I have and this is the accuracy…

RA: It's administrative.

KM: Right, administrative.

JSulston: More administrative than philosophical.

RA: And the kinds of issues that there needed to be compromise and debate over the length of time? Because that symbolized what to people? People were nervous about being compelled because of the practicality?

JSulston: You mean the way to release? Oh yes, there was quite a bit of discussion about that.

RA: There's a lot of crossing out in that area.

JSulston: There's lots of people would like six months. I mean, that's what the structural consortium, protein structures I think have come to, something like six months. And no doubt the Germans and Japanese would have liked that - the funders, that is. And we saw that the shorter we could push the time the better in terms of transparency, of getting over people's anxieties. Of somebody doing something that was private.

RWaterston: And then getting to that point, people started raising the practicalities. How can we get this out there? That's going to put an enormous load on our computing.

BCD: It looks like there was a little caret with an entry "into databases," which looks like it was added in? That must have been a clarification or something [about where to put the data]?

RWaterston: That's an example of something that we clearly had not gone through ahead of time at all. For the worm we posted it on our websites. And that seemed like a good model. But we recognized in that discussion that with this many people and this many interested parties and so forth it couldn't be that. It had to go into this database.

JSulston: Something a little more formal, yes, and so that was the incentive.

RA: And then the quality debate, over whether raw data were meaningful? At what point it deserved to be archived?

JSulston: We did have the notes that sequences of one KB, of one KB, isn't it? That was the compromise on that.

RWaterston: We debated on how much. It had to be how far along. That's separate from the earlier quality debate about what the product of all this was going to be in the end. But this was what you were going to have to release.

JSulston: So it was not raw reads, I think that was the point.

RWaterston: And that, the one KB was a practical thing. That if you had contamination you were just going to get one read and it was just going to be by itself. But if you got multiple reads it would assemble into something of more than one KB. And so it was more likely to be what you wanted it to be. Was it one or two?

KM: I think it was one.

RWaterston: Was it one?

JSulston: It was one. That's a reflection of the sequencing at the time.

BCD: I don't know whether that's from '96 or '97. I'm assuming …

KM: The summary of the '96 meeting was on the Internet, and it says one KB as well.

JSulston: In sequencing no machine was producing one KB reads at that time [laughter]. There was nothing magic about one KB, it just allowed us to distinguish, as [RWaterston] says, between random reads, which could have been anything, and assembled reads.

KM: Right, which were more likely to be meaningful.

RA: So another part of the vision that seems people often overlook is that you two were already coming from a background where doing this sort of sequencing or doing this kind of work, that some people might say isn't real science, it's just descriptive. That's my word. But you were in that tradition and you saw that it had certain kinds of payoffs. Were the other centers willing to buy into that vision? Did they have an idea that they were going to somehow in tandem do sequencing and do real gene-driven scientific research? Did they develop different strands? Or do you think people tended to think just doing this work was going to be a worthwhile endeavor? Because it's not something everyone would have agreed to and the human genetics people in particular.

RWaterston: You have to find out from them. But my impression was that that was one of the issues that people had to give up on. That if you're going to do this, that if you're going to get all this money, you can't be spending it on other things. This sequencing is what it's for. And we got a lot of reinforcement from Francis on that.

JSulston: Yes, it's a constant tension and it was a tension within the science. Everyone has to fence things off, and same in your lab. You want to have people … you want to have people around, smart people doing research. It has to be fenced off in a way that you'd have this project doing … and you can hire people who really love doing this stuff. And I'm one of those. I may be unique actually among the senior people, as I'm not a proper scientist in the sense that you were describing. I'm not problem-oriented really. I'm a mapper and always have been. And I didn't realize this in the beginning. It's not that I came out of the womb and said, I'm a mapper, let me at it. No, no, I discovered bit by bit that I really got a lot of gratification from getting jobs done, from finishing them and having other people use the product. And the [*C.* elegans] lineage was so satisfying to me because of that. It gave me far more satisfaction, apart from the beauty of looking at the cells, the most satisfaction I got out of that was having people use it. And then it repeated with the map and it repeated with the sequence. And so I personally had no

trouble at all and I got all my kicks out of it, but I know that in general it is a real tension in the scientific community. And we still haven't overcome it yet, and we've talked a bit about the need to have attribution and so on. But there is still this sort of emotional thing about what kind of science you're doing. I guess you just have to say horses for courses, you know. And the point about the attribution of the funding is to make sure there's room for all people in so far as when these people like me, mappers, then there should be room for them. But it's no suggestion that this is the right way to do science or that's the right way to do science. We need both interacting.

RWaterston: But I'd just add that it was [JSulston's] clear attitude that I think made people in the worm community buy into it so readily. Is that he had shared so readily and clearly gotten joy out of that and not harbored it for himself in any way. So that going into the map people knew the nature of the man.

RA: And that helped? That helped them, I assume, once you …

RWaterston: Oh, absolutely.

RA: … human, right? You sort of revisited obviously, but …

RWaterston: People in the human community didn't know [JSulston] as much but the spirit of all that came through in the discussion at the time, I'm sure.

JSulston: And of course, I mean I put myself as being further over than you are, but of course it's something we greatly share. I mean you obviously get that joy too, but you do better science than I do as well. So you do both.

RWaterston: Well I have a different funder.

RA: So then, do you think, [JSulston], at the end you felt like you'd gotten out of the meeting what you'd hoped to accomplish? At the end of the '96 meeting?

JSulston: I was thrilled.

RA: And had you expected to be able to get that much agreed upon?

JSulston: I thought it was a really tough call.

RWaterston: But it wasn't a done deal by the end of that meeting. We had pretty broad agreement to those principles. But the Germans did not sign on, and neither did the Japanese. They both, as I recall, said that they had to go home and consult.

KM: Right, so when you say sign on do you mean like …

RWaterston: I don't know about the French.

KM: … did you have … I've heard rumors of people raising their hands …

RA: And sign-up sheets.

KM: So … was there any process? What was the process of agreement? Did you just have the Germans and Japanese sitting in the corner awkwardly? I'm just trying to picture what …

JSulston: I think there probably was a bit of that. I don't know if we actually had a show of hands. The reason I say I was thrilled is that I had a very strong sense of consensus in the room, and the Germans and Japanese, the scientists, I think, wanted to participate in this way. But their national rules had to be dealt with.

RWaterston: I think that's fair. It was the administrative side.

RA: Yeah, and administrators were in attendance as well, who would have been …

RWaterston: And reading why their government was investing in this project in the first place, it wasn't to enable U.S. scientists to find disease genes.

BCD: So Craig was in the room when this final session was held?

RWaterston: I suspect he probably wasn't but I don't …

JSulston: But you recollect that Mark Adams was.

RWaterston: So what I recall is that, and this is distant, Craig went to only one of the meetings and that he did leave early. And so I think we actually had some chagrin that he wasn't going to be there. But Mark was there as well as … it wasn't only the lab heads at the meeting. From the Sanger it was Richard and David were there, and from Wash U it was Richard, Rick Wilson.

JSulston: So we were signing up as institutions not just ad hominem.

RWaterston: And so other people from TIGR were there and participated in this discussion certainly.

JSulston: That's why I was confused. I think I mistook earlier when we were talking some time ago, but that's the key, is that institutions were well represented.

RWaterston: And eventually these became terms of grants. In terms of that, there is the Bayh-Dole issue, and that came up in this discussion too.

BCD: At Bermuda?

RWaterston: I believe it did.

BCD: Wow!

RWaterston: I believe we were concerned about Bayh-Dole and whether this could be done. And you'd have to go consult the transcript to see what Francis was saying about that. But my recollection is that he thought he could maneuver it.

BCD: Based on subsequent behavior that's clearly what he concluded. But do you remember this at all?

JSulston: I think I was just at that point learning what Bayh-Dole was. I was probably listening rather than participating.

RA: And so then what's the follow-up? I mean another meeting the next year …

KM: And a meeting after that.

RA: And again a lot of practical updates. Here's how far we've gotten and so on.

JSulston: And it was understood that we rearrange the allocations depending on what people could do. And that included the technology and it included funding.

BCD: So when you say we, how does that process work?

JSulston: No, I meant we, collectively.

BCD: Yeah, I know, but that's a pretty brutal process.

RWaterston: Well it wasn't actually that bad because the territory was vast and the amount of sequence was small. There were only a few cases where people were running into each other unnecessarily because they thought they had more capacity and so they claimed more than they needed to. That's my recollection. But we revisited the German and Japanese participation and our abiding by the rules or whatever you want to call them, accepting them. That came up in certainly the second year.

RA: So does that mean they went away and in a sense got permission and signed on in retrospect in '96?

RWaterston: I believe that's right. I believe they … and I can't remember if they …

JSulston: There may have been some compromises. I'm not sure. Michael would be [the person to ask], as he was doing a lot of traveling and phoning at that time. Ended up Francis was doing it as well, but Michael was talking [with participants having trouble with the Principles], and I know it was quite hard at times. And he was just trying to bring everybody in the tent. So I'm not exactly sure.

RWaterston: Yeah, I don't know. And I don't know if it happened during the year and it just came back and people reported or how much was quiet.

RA: People start to drop off. If you look at the attendance lists there's quite a lot of people and then there is a drop-off in the labs represented over time. Australia was represented and after that they weren't. They didn't really invest in sequencing. John Mattick was there the first time. I assume they were feeling out what was going on. It's just a micro example. But you see the U.S. labs start to do that too, and of course it's hard to know, was it that they couldn't come or was it looking like they weren't going to then be a player?

******RWaterston: As I mentioned above, various labs were doing some human sequencing, regardless of whether they were funded in the pilot or not, and some of these people attended the first meeting.******

RWaterston: I'd have to look at the lists…because I think the labs knew that they were going to be funded by the time of the Bermuda meeting. It wasn't all the labs who had applied. So I can't …

RA: And they were funded for …

RWaterston: They would have been funded for the whole three years. ******Now with the caveat that the third year was apparently conditional.******

RA: For the three years.

RWaterston: But each of the labs should have had a representative, whether the head of the lab was there or not. I don't know.

RA: No, we'd have to trace that out.

BCD: So when you hear people citing this as this icon of virtuous scientific behavior, does that feel appropriate? Does it feel like it's a little bit out of place? They're missing some of the history? How does it feel to you guys as kind of the pioneers of this?

RWaterston: I don't know. It's nice, I guess, and it clearly attained more significance in '98 with Celera's entry into all this and a clearly competing model. We no longer had academic squabbling about whether they were going to get a patent on this gene or not. This was a serious commercial entity and with their attendant desire for control. And so then in contrast the early release became more significant. I'm not sure it would have had the impact it did without Celera. You think?

JSulston: The agreement?

RWaterston: Yeah. I mean it clearly … it would have governed the project and that was important of itself. But I don't know … it got more notoriety.

JSulston: Well, yes, it did get more notoriety. But on the other than hand I think it was something that people look to because the genome was a success and it would have been a success, less visibly possibly, coming to a conclusion without Celera. But it would have certainly been a success and would have gone forward and been done because it was doable and important. And other things of that ilk.

And I think, I hope nobody is falling into the trap of saying, "this is the way to do science." That's not true at all. This is a way of handling large pre-competitive pieces of science. And one of the dangers actually - you see it with this tagging of everything with "omics" on the end, because if they put "omics" on they think they'll get a grant. And sometimes maybe it's even true. It's appalling. It is not to say that all science should be large; and not all science should be pre-competitive. The point is that the Bermuda rules are a good way of handling pre-competitive science. We've seen other projects of this kind, and I alluded, for example, to the protein structures consortium, where they tried very hard with some success to introduce the same sorts of ideas. But it's difficult, with the knowledge that their structures are immediately industrially relevant and so on. So it's much harder at the edges. But it's in that context I would steer it. And I'm excited, I suppose, that it's been used in this way. But just in the sense of making science work, in this particular style of science working.

RA: And so if we back it up to just the success in the domain in which it was intended, would you say that the agreement was a success? Did it foster the kind of science you were hoping for?

JSulston: Well it made it work.

RWaterston: Yeah, I find it hard to believe that the genome project would have gone as well as it did without it.

RA: Were there those for whom the principles actually proved an impediment or problematic? Did it cause people to drop out of the game or not participate or … we know of course some people probably were …

RWaterston: For some of them it might have lessened their motivation. Depends on what their motivations were. Because we are describing a style of work that might not be attractive to everybody.

RA: And obviously, as you said earlier, it was pretty important to enforcement because it became a condition of grants for instance.

RWaterston: Yeah, there had to be some level of enforcement. Otherwise, what if somebody didn't play by the rules and everybody else was?

RA: But there weren't global rules. So that was within the U.S. granting structures for instance but relatively absent elsewhere.

RWaterston: Certainly, Jean Weissenbach followed the same rules. Andre followed the same rules. That's part of what Bermuda was, was getting the whole community …

JSulston: It was international. That's why it was held in Bermuda. So it was in the middle of the ocean.

RA: Well it was self-regulation. The thing that happens in the U.S. is that you end up with a combination of everyone agreeing and it becoming a condition that's codified.

RWaterston: I don't know that it wasn't a condition. Once the Japanese government or the executive branch went along with this, I'm not sure that it didn't become a condition.

BCD: You are describing an accountability process in having repeat meetings, among other things. I presume when you came back the next year you're supposed to say what you did and whether you set a benchmark and there's a performance measure of whether you've attained it.

RWaterston: Well, and this was all out there, right? If it wasn't out there, it didn't count.

JSulston: And everybody knew anyway it was on the web. It wasn't like everything was held back until the meeting and then suddenly produced out of a hat. Not at all, because of the release rules. And it was there. But the point behind the meeting is to have a reaffirmation really of the consortium, of the concordance between us all. That's why I use the word we. By getting people in the room, international meeting, not just U.S., not just U.S.-U.K. even, but totally

international in there in the room, face to face, and that makes it much easier to establish the sharing of the commons in the way that we have to do.

KM: So which grants are you talking about? Because you said the pilot grants from the NIH would have been for three years, right. So if those started in 1996 then could the terms of those grants be changed to reflect what was said in Bermuda? Or are you talking about in other countries?

RWaterston: So that's a good question. I don't know when the wording got added to grants. I know then that it was clearly an issue of Bayh-Dole and they had to clear all this stuff through whatever legal offices existed, the NIH and beyond. And when they were able to do that I don't know. You get a grant for three years but you only get awarded a year at a time.
****Looking in my notes, I see that by April 9 (before the awards were actually issued) NHGRI put out a policy on rapid data release that was to be applied to all the pilot projects.****

KM: Right, exactly.

RWaterston: So it could have been in the second or third years. I don't know when it was added.

KM: Okay, that's an interesting …

RA: And then, did you participate in subsequent discussions, like at Fort Lauderdale or even Toronto and so on? Or did it start to become about something else? So there's three meetings in Bermuda of a similar ilk and then that was it, right?

RWaterston: Then things changed in the human genome project. It was consolidated. The five large labs overall.

JSulston: That was extremely uncomfortable. And there was a lot of unhappiness. The only way to compete in the IP sense, because that was the key thing, was to get the data out there, to avoid privatization taking over, was to get the job done. And we used to joke, but it's a very accurate metaphor, about the Security Council and the United Nations and all that. And it's all the same issues in our microcosm. And again, it was incredibly tough and I had some very bitter conversations with people who probably really were being squeezed out at that point. And we were trying like crazy to make room for everybody to contribute as much as they could, but we were having to push to get this rough draft in once that principle was established. And we only had two years. So it was hard. And it's important to recognize that the sort of acceleration that was happening was not an acceleration, I'm pretty convinced, of the whole project. It was an acceleration towards a partial

closure which was good enough to secure the database and the IP in the public domain.

KM: Saying from ’98 to 2000.

JSulston: Yeah. In fact the battle went on a bit longer in the mouse. It wasn’t until 2001 or 2002 that we were secure with the mouse in the public domain. That was much more off the radar of the media but it was equally threatening really to biology because if the mouse had been privatized …

RA: But there’s a whole tradition that made that a very difficult … one probably could have predicted that would have been even more difficult than the human, because of the traditions of strains being proprietary. And strains of mice being proprietary, people working in that biomedical domain where there is intellectual property concerns and so on. The mouse …

RWaterston: But by that point the labs that did the mouse were the Sanger, Wash U and the Broad. We knew what the rules were and we went in and there was not a question about data release. So that just didn’t matter what the mouse community wanted. We could get mouse DNA. We didn’t need …

JSulston: These are reference genomes.

RWaterston: And Eric had actually done most of the mapping so we had the map information in house almost.

BCD: Talk a little bit about the draft sequence debate, because you alluded earlier to having basically proposed that as part of the strategy. After you answer [RA’s] question about Lauderdale.

RA: Lauderdale, yeah … or anything beyond.

JSulston: I think I went to the Airlie House meeting but not to later ones.

RWaterston: Fort Lauderdale was how to think about applying the Bermuda Principles to things beyond. How that was going to work. And that was a contentious thing because there was this … we had been taken advantage of--in ways that we didn’t anticipate--by Craig. You were saying in class yesterday, you were talking about, well why shouldn’t Craig use the data that was out in the public domain? But certainly by worm standards, that was not expected behavior.

RA: I was just trying to…

RWaterston: But on the other hand it’s true, it was out there and that was very much in people’s minds. And it’s a tension that I still haven’t resolved for myself in

terms of where things sit because you do all this work and at the end of it you want a publication that at least says you did all this work. And if somebody else can publish on it first in the same way then you lose that priority. And the database, submission to the database doesn't serve that purpose yet. And on the other hand once you put it out there in the public domain but you say people can't use it for this purpose, then it can be abused. And people are pretty slow at getting out papers sometimes and that has the reverse effect. It doesn't speed things up.

RA: So by the time, say, of the Fort Lauderdale meeting not everyone was convinced would be an understate … I mean in part because some goals are achieved but there's obviously these outliers and it's not controlling …

RWaterston: So there were codicils added in Fort Lauderdale, and I wasn't in Toronto because I couldn't go to that. So I'm not sure what transpired there.

RA: But a lot of the motivation was the perceived commercial threat and people gaming the system. Not just Craig, but just more generally.

RWaterston: Yeah, that's the tension that's created. Sorry, but you were asking …

BCD: Well I was going to go back. So it sounds like the idea of draft sequence, there was an idea in the early shift to sequencing in '94, '95 to say we should have sequence information available for people to use as you had done in worm that's drafty and not full accuracy, and then have an archival sequence pursued as the end goal. That idea goes down and then it comes back in response to Celera? Is that the story we're hearing?

RWaterston: In part I think that's right. The push to draft sequence is a pragmatic one because the draft sequence can be generated in a fairly automated fashion, right? You just need more machines and more computers. It's not quite true, but it's largely true, whereas the putting out high-quality contiguous sequence still today requires human intervention of a costly kind and we had worked at trying to make that more efficient, and we made it more efficient, but not nearly at the same rate as what shotgun data was …

JSulston: Shotgun is getting faster all the time.

RWaterston: Yeah, and we weren't the only ones working on it. There was lots of commercial investment in that as well. And so even by '94 it was clear we could go a lot faster if we dissociated one from the other. And the problem with that was whether there was a commitment ever to invest in the finished product. And that's what persuaded me to not pursue that course, was that when I looked around I did have serious doubts that if we produced a product that let people get on with their genes would the funding agencies be

motivated to fund it to get it right? Because it was going to be more expensive. And they would have already had quick utility. But you see the problems now with all these drafty whole genome assemblies and so forth. Other researchers spend an inordinate amount of time trying to figure out whether what they found is right or not. And they make mistakes because they don't understand the nature of the product and they think it's … they've seen the human genome where the worm genome had been. ******The point here is just that the worm and human are high quality.***** They compute up on one of these other genomes and bizarre things happen and they don't know what's going on. So there is a lot of downstream hidden cost in unfinished genomes, but whether it would have … how long it would have been before that was recognized and so forth it wasn't clear at all to me. And that was a big issue in going to the draft.

JSulston: Again, there was a lot of tension within the Sanger. I had big fights with the people who didn't see the political threat, which I put in terms of IP, but you understand what I'm saying, it's in terms of the danger that everything would somehow disappear into a private database. And between that perception and the people who said look, we just should keep on doing high quality stuff and people realize we're doing a good job and so on. And [RWaterston] and I were, no doubt, we were in complete agreement that we had to go there I think. And that was the … Francis and Eric, I guess, were completely on with that. But you had people I guess as well who were arguing.

RWaterston: Oh, I … yeah.

BCD: So where were Francis and Eric? They were on the other side of that debate?

JSulston: No, no, no, no.

BCD: That's what I thought. Okay.

JSulston: It was a question, it's interesting, I mean I didn't think of myself as being terribly political. I certainly was taken…we just had to do it and the world would not be impressed by our accurate sequence unless we [Inaudible – another speaking] ownership.

RWaterston: That was clearly a compromise that people weren't comfortable with in my group. And part of the discomfort was real concern about whether it would ever be completed.

RA: And then therefore there wouldn't be an output.

RWaterston: Well there would be an output but it would always be crappy.

RA: Sketchy. It would always be rough, yeah.

RWaterston: It would always be a draft and that's not what people signed up for.

BCD: So could you go back for just a second to this question of credit and how this fits into career paths and career models and pathways of doing science. [JSulston], you kind of held yourself out as a particular kind of scientist who's very happy to do something that other people use.

[Off-topic conversation]

RA: Can I just … I'll ask something slightly different and maybe you can answer Bob's question once he gets back.

RWaterston: Once I know what it is.

RA: Yeah, he can clarify what he meant. Thinking ahead to people we should interview, obviously we're making our way through the attendees list, looking at diverse perspectives, people we know who are coming from various points of view. And so, are there other people you would suggest we need to talk to or from non-invitees, or people from the invitees you think are particularly important that might not leap out to our eye? I mean looking at the list probably would make this easier but …

RWaterston: Have you talked to Jean Weissenbach?

RA: Uh-uh. So …

JSulston: Oh well, yes.

KM: I've emailed André and he hasn't written back. I'm going to email him again. And I'll email Jean as well. And also Yoshi Sakaki.

JSulston: Yoshi, yes.

KM: I've emailed him and we're kind of in an email crossfire right now because we've both been traveling.

JSulston: Yoshi would be pretty interested, I would have thought.

KM: Yeah, he is. He's just really busy. And there's a 15-hour time change.

RA: It may be better for me just to do it when I get home.

KM: Yeah. But at any rate, yeah, so Yoshi and Andre and Jean. Is there anyone else from Japan that you think …

JSulston: Yoshi's the man, really.

KM: That's kind of the sense that we've gotten.

RWaterston: If you can identify them, I'd be curious to hear what some of the German and Japanese administrators …

RA: Yeah, so are we. They're very hard to track so I think we're going to have to do that through Yoshi, for example.

KM: Probably.

RA: Because they don't come up on a website, they don't. It's just a different kind of person. But I think absolutely right, those are some broader interests we have about the diversity of views between the scientists on the ground and the policy people who were observing this go on.

KM: Exactly, and we were going to ask Yoshi in particular for people for us to speak with.

RWaterston: You have the U.S. labs, I presume.

RA: Yeah. And those are pretty obvious because of groupings.

RWaterston: You've got the DOE labs and the NIH labs.

RA: Is there anyone who you would say in a sense influenced this who wasn't at the Bermuda meetings? Or … I mean so we're going to talk to Tim Hubbard tonight just about that particular kind of issue about how these things come to be interpreted over time. That's just an example of someone not involved, right?

JSulston: You've got Richard Durbin down?

RA: Yeah, we do.

JSulston: He's important.

RA: Yeah, I think so.

JSulston: Because he batted for the accurate sequence. Quite rightly, because he argued all along that if we did not get to accurate sequence then other people would

be most worried about what [RWaterston's] describing, if we didn't get the accurate sequence, you would not be able to do the most interesting high-level computing.

RWaterston: In the human it's obvious you need it to interpret variation.

RA: That's exactly what I expected he said basically, so that's why I told you.

RWaterston: But it's true actually for other animals now. You can't do variation if it's not a decent sequence.

RA: I think Michael [Morgan] said for some reason he didn't think he was important to talk to. And we said, no, no, no, put it back on there. I think he would have something interesting to say.

RWaterston: I took a lot of heat from Maynard.

BCD: For the draft.

RWaterston: No, that was a later discussion. For Bermuda. Maynard got up at Cold Spring Harbor rather irate that this cabal had got together and set rules different from the standard norm for data release.

RA: And that's exactly the sort of … the concurrent discussions that went on. And in what context was he talking about this? The annual genome meeting at Cold Spring [Harbor Laboratory]?

RWaterston: He was a grantee and he did not go to the [Bermuda] meeting. He was represented though in the same way. He had [someone], I can't remember who from his lab was there. So he was represented and he heard what went on, and was pretty angry. He thought it was a warp of the scientific tradition, is how I would phrase it.

RA: And so it was expecting too much, or what part of it in particular, the rapid release, the quality concern, all of it?

RWaterston: It was pre-publication release.

RA: Full stuff.

RWaterston: That's what Maynard was upset about, I believe. You'll have to ask him.

RA: That's definitely worth …

BCD: We'll have plenty of time.

RWaterston: You might bring it up gingerly.

BCD: I have a long list of things to go over with Maynard.

RWaterston: It's still a sensitive topic.

RA: Did you want to repeat your question because we weren't quite sure of it so we dropped it for a minute until you came back.

BCD: I don't remember what it was.

KM: It was about scientific careers.

BCD: Yeah, you were talking about glory in people using the stuff that you've put into the world. That's kind of the indirect model of scientific creation, right? How have careers worked out for the people that did stuff that was like that as opposed to the standard model of find the gene, understand it, do the standard molecular biology?

KM: Proper science.

BCD: Have people landed on their feet? Did it feel like that at the time? How did it feel to you guys in your own careers?

RWaterston: One of the challenges in the whole system was this centralization of big labs. So that if you were a young person coming up in my lab and by the time we finished the mouse genome I had three faculty-level people in the lab I think maybe, as well as several good post-docs had been through. And with only three places in the world with $50 million a year funding each or something like that, there weren't job openings to do the same kind of thing. And so that was a challenge. But did people end up on their feet? Well, yeah, they did all right, partly because the system group was growing in spite of this concentration. There still were lab sprouts going up. John McPherson went on, is now in Toronto as part of Hudson's operation there.

BCD: The [Ontario] cancer center?

RWaterston: The cancer center. And I left so that created an opening for Rick. Marco Marra was a very successful post-doc and was lured off to the Mike Smith Center in Vancouver. So he's done very well there. Elaine [Mardis] has become co-director with Rick. So they've landed on their feet but I don't think the system was very conducive to giving them a chance. The NHGRI could have been better about thinking about its young, what kinds of pathways would be …

BCD: What about Wash U?

RWaterston: What about Wash U recognizing them? I think they felt like there was such notoriety associated with the genome project that it wasn't … I didn't experience problems in trying to get these people appointed or promoted because it was just such a high profile thing.

RA: And it brought in lots of money.

RWaterston: Bottom line. They didn't want to make me unhappy. [Laughter].

KM: One of the things we've heard was the sense that there was an understanding amongst people who mattered that getting your name on a publication with a hundred people was okay, and this was a different style of science and that was just something to be understood.

RWaterston: I don't know how wide it was, but you see a paper with a hundred people on it and if you're not familiar with the groups you don't know who did what. And so that is a problem.

RA: What we talked about earlier today, that the nature of the activities broadened probably even compared to, say, what the worm group would have considered authorship. The worm group would have included people, technicians and others often, as authors and certainly in acknowledgements, but often as authors. But it sounds as though it had even gone further because there are obviously people who had what you might call scientific administrative roles who end up being authors. So this widening definition of who needs to be acknowledged certainly, but who needs to be even considered an author.

RWaterston: But the people doing the work … I mean …

RA: So the work has got to be defined in a way.

RWaterston: Yeah, that's not an issue. Yeah, it does … I run into this clash of cultures or something on a different scale when in bioinformatics if you publish in a part of a proceedings for a conference, those are actually reviewed manuscripts and highly competitive. They may only publish 16 out of 70 papers or something like that that get submitted. But if you're in the medical community and you see somebody's got a publication in the proceedings, the meeting proceedings, you just blow it off because it's an abstract or right now it's a freebie. And so you have to know your field, I guess.

RA: So quick question before we lose you. Do you have any documents or anything that you think is worth sharing related to Bermuda?

KM: The picture of the mapping phase of the worm genome, if you've got it…

RA: Well, yeah, that would be … [JSulston] mentioned …

RWaterston: The one on the wall?

KM: Yeah.

BCD: Thank you, [RWaterston].

RA: Thank you very much, [RWaterston].

[Off-topic conversation] [Robert Waterston leaves the interview to catch his plane.]

RA: Maybe to ask [JSulston] a little bit more about the culture questions.

BCD: Yeah, I'd like to hear the type of answer.

RA: And then documentation.

JSulston: I'm not off the hook because he's leaving, huh? [Laughter].

BCD: We'll make this the final question.

JSulston: That's all right. I'm happy. I'm happy. Even without [RWaterston] to look after me.

BCD: It is something that took on a lot of extra meaning.

RA: In retrospect, yeah.

JSulston: I said to [RA] I was pretty thrilled by her remarks yesterday in the class.

RA: Which ones?

JSulston: About the concordance of the Bermuda agreement and how widespread they are, because I hadn't realized.

RA: Used and abused, yeah. I mean uses are made where there's no analog. It's not raw data, there's no good reason for rapid release, it's not a collective project. It fails on all the criteria about why you needed this, the practical criteria. It's become synonymous with something much more general.

JSulston: Like putting "omics" on the end of things to drum up …

RA: Exactly. Exactly.

[Off-topic conversation]

RA: Errol Friedberg, by the way.

JSulston: Ah, that's it, yes, yes.

RA: Sydney's biography.

BCD: Oh, you found it.

RA: I found it online. I couldn't get access to the pages I wanted. Those were the ones that were blocked. So I have to order it and look at it in more detail. I mean it looks like it's at a fairly rich level. It's somewhere between scholarly and journalistic.

JSulston: As I say, I brought it to your attention in a sort of spirit of honesty. I do not like … I wish I hadn't said so much. I said it and he put it in his book. He was entitled.

RA: Had Sydney in some way authorized him to do it?

JSulston: To do what?

RA: To do the biography.

JSulston: Oh, that I don't know. I have no idea what the connection was.

RA: He had access to lots of letters but I can't tell if it's deposited at Cold Spring or otherwise.

JSulston: I have no idea. He just came and interviewed me and I talked about this and that.

RA: So the culture of science, do you think that in some way the sequencing did foster a different kind of culture much more broadly? These norms that it's perfectly good to do descriptive science, someone has to do it, it's an important part of what you do in science? Within your own lab did it limit things for people? Did it change …

JSulston: Oh, I see. Oh, yes, so it's in parallel to what [RWaterston] was saying. Yes, so as far as I was concerned, yes, it's easy for me and totally satisfying and I don't really want anything out of all this at all, except it's nice to see things you've sewed up. That was good. And I'd extend it to at least the earlier people who came, I think, quite a bit, because people of that sort were attracted. A lot of people going through, of course. That's one thing which

you can enjoy. And it's continued with the Sanger. I mean I remember, as we hired people, because it was battle building up the team fast enough, but ours was built up for the most part less academically than [RWaterston's]. We were not part of a university. We were specifically set up, in fact I insisted on this, with our own pay scale. And we invented our own titles for people doing the sequencing. And we were able in that way to detach the skilled sequencing team from the management, because usually you only advance yourself on a pay scale by managing more people. And that's absolutely what you don't want to do if you've got somebody who's good at finishing. You want them to do more finishing. And we had one or two star finishers who just were fantastic. No way did we want them to manage a group. And of course this is a truism, it's throughout all of institutions and corporations and so on, that people get promoted according to the Peter Principle, they get promoted out of their sphere of competence. So I was aware of that when we started building up this structure, and the sorts of people we got in were not expecting to have academic careers. And I think by and large they've done fine, and in fact the thing that I found difficult for me was when they started to leave because they got offers from companies. And at first I was appalled. These people … and I said to myself: 'the project's not done, we're a team, and they're leaving'. And then I realized that we were becoming a really hot item on people's CVs, who were heading towards any kind of biotech career. It could be academic, but it could be corporate, because of the biotech openings. And so it turned out that we were therefore becoming a training center. And so long as you get better and better people in, and that was the thing, as this became known the quality of our recruits went up because people wanted to come and we were competing for posts at the Sanger Center. So I realized that we didn't miss out and you could see that anybody engaged in that is not going to lose out as the project changes and draws to a close.

There are a few people, the sort of group leaders, for whom there was a certain amount of suffering actually after I'd finished as director because under Allan Bradley the lab radically changed its style. The Wellcome Trust did not want to just continue as a sequencing center, although it remains I think the largest sequencing center in the U.K. if not in Europe. But it was doing things in a different way. It wasn't the ethos of the whole lab and the human genetics and mapping groups became dispersed at that time. So David Bentley, for example, moved to Solexa, now Illumina, and so on. He brought some people with him. Stephan Beck found a post at UCL because the Sanger didn't want to go on with epigenomics. As the ex-director I found myself distressed by these because these were people I thought were very good and I would have liked them to stay on, but the contractual arrangements were such that they didn't. I hate that that happened, that people had their lives disrupted, but on the other hand the lab was going on and they'd gone on and I'm sure five years later it makes people feel it was fine. I'm not such a competent administrator. [RWaterston] looks after his people much better than I do, if

you know what I mean. He takes a continuing responsibility for anybody he brings into his lab, and I tend to be a bit sort of, don't know what's going on here. So a bit of a mixed bag as far as that's concerned but I think, as I say, for the most part it worked out fine because people were hired on the basis that it was a job and it was a technological job.

BCD: So what's the spirit of … I can't remember if it was you or [RWaterston] who invoked the Francis Crick vs. Fred Sanger model of the doing of science. And it seems like the spiritual guru of the kind of Cambridge style, especially as you described it yourself, the lineage, the map, the sequence.

JSulston: It's a Sanger style.

BCD: That's a Sanger style. That's quintessential Sanger. Create a tool that other people do cool stuff with.

RA: And the management is Pruett style. It's combined with sort of a big picture and a sort of ethos of day-to-day life in an organization. Do you think?

JSulston: Yes, you mean in the sense of it not being very pyramidal, or the flat structure. Senior people are all equal. Everybody's as equal as possible. The coffee area, everybody encouraged to gather. That kind of thing, yes.

RA: Trade notes, that sort of thing.

JSulston: Yes, we imported the lessons of R&D.

RA: And the Sanger style doing research, is it anything beside focusing on creating resources as an important stage in the doing of science?

JSulston: Fred Sanger is an amazing figure with the things that he achieved. They're certainly not just the sorts of simple mapping things that I did, but nevertheless, you're absolutely right. The whole purpose for him was to make sequencing faster, faster, faster. First protein and then nucleic acid, DNA. And that really was the aim. And interestingly, of course, he was not highly regarded in his early career. That's one of the things that brought him to the Perutz group because biochemistry didn't want him. And it was generally thought at that time that primary sequencing was a waste of time. People knew that what really mattered about proteins was their shape and thought: 'who cares what the primary sequence is?' That's very wrong.

And of course with the nucleic acid sequencing as well, just as our whole business with mapping and sequencing genomes was pooh-poohed by the majority at first, so it was with Fred's entire sequencing operation. Why do you want to do that? It's not solving a problem, it's not even very important to

know what the sequence is. Isn't it amazing that this is how we explore, and it's interesting that there are so few places where the mapping guys in fact have managed to push through. Of course they're the ones you hear about. I think there are lots of people who might have a go. And it only works if you've hit by intuition or luck or whatever on something that really does matter. It's easy to go and do stamp collecting and get nowhere. That's the thing. So I have no idea for myself whether there was luck or judgment. I guess one just has … you just feel something's important. You say, I'm going to do this. If you're right then it's good.

Just perhaps to put it on the tape, I think we spoke about this yesterday and it's an important point, around 1990, the beginning of the big sequencing, there clearly were an awful lot of good thoughts chasing an insufficiency of data. And it was quite clear to me that I did not have to become an expert on genome analysis in order to do a great deal of good. If I could come in there, along with my colleagues, and push lots and lots of data into these big brains waiting to absorb it and do something useful with it, then it will be fantastic. So that sense was there. So in that sense it was a judgment. It wasn't an individual's stamp album, it was seeing that this would be of immediate use. And so that's why I was so associated with people like Richard Durbin, for example. Just waiting for more data. And yes, he's going to do something exciting with it. So many of them like that. And just to be a chemist and sit there and produce the data and see it flow out is something else again. We need both people, both sorts of people.

BCD: Well, I think we're at a natural ending point. We've exhausted you, or at least you've exhausted us.

RA: And do you have any documents of any sort that would go beyond notes?

JSulston: Very poor on that. Another figure, are you in touch with Alan Coulson?

RA: Haven't been in years but can be, yeah.

JSulston: Because he's a great collector of documents.

RA: Ah, that's interesting. Okay.

JSulston: Just trying to think. Now wait a minute, though, let's back off. Did he even go to Bermuda? Perhaps not?

RA: I was about to ask you why …

JSulston: He was not in the human, I'm sorry.

RA: He's good for me for other purposes.

JSulston: The thing that's confusing me, of course, is that he was part of the management team at Sanger. No, no, he probably …

RA: I don't think he was.

JSulston: … that's a red herring. I don't think he'll have anything that's relevant to this project. I'm getting confused. So who would have? I don't know. I suppose you've done David Bentley as one of the mappers.

BCD: Did he go to the meetings?

RA: Yeah.

BCD: He must be on our list. I really don't remember.

JSulston: He probably should be and might well have. What he'll do is to illustrate some of these tensions both from the academic point of view because he is running people who want to do research, and of course the generation of the map generally. Because initially we were making our own maps and we merged together as the push became stronger. But a number of the chromosomes, of course chromosome 22 was sort of fully mapped and organized by Ian Dunham there, and other chromosomes as well. But no, I would look to others for documentation. Michael Morgan in particular. I must say, if you really want to know what goes on it seems that you should really make an effort to get hold of his tape.

RA: The transcript, I know. Where that stands … I mean I explained it to [RWaterston] at breakfast … but where it stands now is that the Wellcome Library will eventually get the transcripts and then they'll make them freely available because that's their principle. But currently the Trust still owns that material. And the Trust lawyers have been pretty firm with Michael that he needs to get every single person's permission or prove that they're dead. And at one point in time it was something he needs to just make a really good effort of it and document that he's tried. And if people have been warned, then that's good enough. But the latest message when I looked at it again after talking to [RWaterston] about it looks like, if it's a different lawyer or they've gotten nervous for some reason, that there's a much stricter … they really want everyone to say yes. And we know that one person at least has said no, that the understanding was that it was Chatham House. And that person seems likely to be willing to talk to us because it's different ground rules.

JSulston: But you can't hear the tapes.

RA: Well, then they'd have to redact the transcript at least, and that person was an active participant. I would think the other thing, they're going to have the same problem we've been having, that the science administrators are on the list officially so the lawyers … but they wouldn't have talked necessarily in the meeting, they're impossible to find now because they're no longer involved in anything related. So whether they can do something more systematic like making sure they have permission of everyone who spoke and/or redacting the people who say no, but there's only one person so far who said no. So it sounds like Michael's made maybe 60-70 percent kind of inroads.

KM: There are people who just haven't responded.

RA: Right, and they felt that was going to be okay so long as there's evidence that the message was received. But now the latest sounds like they really want to make another go of non-responders. So meanwhile we're doing interviews because a lot of what we want to know is impact and background stories.

JSulston: Yeah, yeah.

RA: But we need the transcript. It would be ridiculous to do all of this and then have the transcript be released and find out there's other things that went on or different things.

JSulston: And this came so strongly into my mind because you could hear both of us were incredibly vague about some of the points.

RA: And people have had very different stories.

JSulston: It's very difficult. You make things up, not because you want to be deceitful, but because you don't remember what actually happened. You have a concept and then when you're asked a question you try and fit, visualize. And so details about exactly who was attending from a particular institution is an example of that. Correct about the institution and wrong about the particular actors.

RA: We have the list of invitees, but there are some, even Michael, isn't 100 percent clear who actually then turned up in some instances, particularly the people who didn't speak and weren't active. If they speak we know they're there.

KM: The only meeting we know who was there was 1998 because there was a piece in Genome *Research*, I think. For 1998 there was actually a retrospective that Mark Guyer wrote that had the attendees. But for '96 and '97 we just had the invitee list and …

RA: Well, and the photograph, right?

KM: The photograph, right.

RA: So one could go through and kind of at least do a count and figure out who …

BCD: … a pretty crummy photograph.

JSulston: It's rather small out at the back.

RA: Very crummy photograph. Well Jim Watson's in front.

JSulston: Jim Watson and Ed Southern. Oh, yes, Ed, have you talked to Ed, by the way?

RA: No.

JSulston: He's an interesting guy.

BCD: He'd be a great interview.

JSulston: He's a funny guy and will have his own take.

RA: Very different perspective on the stuff about the human … yeah. And I think the further question is who was actually there for any particular bit of the discussion and only getting the transcript is going to tell us that.

BCD: At some point it might make sense to just go back to them and say, as scholars we would like to go into the transcript without attribution.

RA: Well I know, and I think the Library would be willing to do that, but as we noticed, the person in charge of the Library is backing off now because the Trust hasn't transferred it from the Trust to the Library yet.

KM: Did we not even ask them that explicitly?

BCD: Yeah, we did.

RA: Yes, we did, and…Simon was in principle completely willing to do pretty much anything we asked, but it wasn't his to do because it hadn't been transferred, so we're back in the hands of the Trust. And Michael really wants to do it right, for reasons you can understand. He feels like he can't just bulldoze people. That's not the only thing he's doing at the moment. There's a hundred reasons why it's kind of gotten caught up I think. Are you looking for the photo?

JSulston: Yeah, yeah, just …

KM: It's really grainy and we tried to blow it up on Bob's computer about three days ago.

RA: Do you have it handy?

BCD: Yeah, I've got it right here.

RA: Just for the hell of it let [JSulston] look at it.

KM: Are we done with the tape?

BCD: Yes.

RA: Because anything else is going to be just names.

END OF RECORDING