

**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: Eric Green, M.D., Ph.D. Date of interview: Dec. 8, 2011  
Current institutional affiliation: National Human Genome Research Institute  
Street Address: 31 Center Drive, Building 31, Room 4B09, Bethesda, Maryland 20892  
Phone: (301) 496-0844 Email address: egreen@nhgri.nih.gov

**Interviewer Information.**

Full name(s): Robert Cook-Deegan, M.D.; Kathryn Maxson, B.S.  
Affiliations(s): Duke University

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

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

**OR**

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

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

Researchers unaffiliated with the Center for Public Genomics may **quote** from the interview only after obtaining my permission.

Researchers unaffiliated with the Center for Public Genomics **DO NOT HAVE** my permission to **read or quote** from the interview.

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

1 year from the date of this form

5 years from the date of this form

10 years from the date of this form

25 years from the date of this form

After my death

Other: \_\_\_\_\_ (please specify a date or condition)

NEVER: MAY NOT BE DEPOSITED IN A PUBLIC ARCHIVE

**Please specify any further restrictions in the space below:**

Signature: 

Date: FEB 27 2012

**Informed consent for: “The ethos and effects of data-sharing rules: Examining the history of the ‘Bermuda principles’ and their effects on 21<sup>st</sup> 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  \_\_\_\_\_

Printed Name Eric Green, M.D., Ph.D. \_\_\_\_\_

Date 12/8/11 \_\_\_\_\_

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

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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](mailto:ors-info@duke.edu).*

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Date, location, method: 08 December 2011, Rockville, MD, in person

Interviewers: Kathryn Maxson, Robert Cook-Deegan

KM: So we should just reiterate that you have read the informed consent and signed it.

EGreen: Yeah, especially the signing part.

KM: Awesome. So you're [EGreen], and you have seen the informed consent.

BCD: So we don't have to read through it.

EGreen: No.

BCD: So [EGreen], if you could just situate yourself in relation to this story. Both where you are now and where you were in 1996, '97, '98. And we'll just start there.

EGreen: Yeah. So because I do think that it's probably all part of your study to get people's worldviews. At the time it was very different. So I'm sure it's very different for different people. So you want me to start with where I am now and then tell about where I was?

BCD: Either one. Just make sure we're covering both ends. Why we're talking to you as director of NHGRI and where you were then and intervening.

EGreen: So where I am now is Director of the National Human Genome Research Institute, which as I'm sure you know, is the funding agency that was responsible for the largest effort of the United States in the human genome project. And because my predecessor was Francis Collins, who ended up serving as the de facto head leader, if you will, of the international multi-funding agency Human Genome Project, there's obviously a huge historic footprint of this institute with respect to all things human genome project related. So, and obviously now our role at NIH is to continue to lead research efforts in genomics, broadly defined.

But in 1996, I guess is where you want to start, when the first Bermuda meeting took place, and I participated in all three, I was a very different person then. I was younger and I was much more junior. So to contextualize my role in all this is that I graduated with MD/PhD degree in 1987, and I went into a clinical training program in pathology that basically required one full-time clinical year, but then immediately allowed you to go essentially do a full-time post-doc for several years. So on July 1 of 1988 I had the ability to go into a lab full-time as a post-doctoral fellow and decided to make a fairly major shift in my scientific background, having gotten a PhD in cell biology. I heard about this new emerging field of genomics, which was about the time the phrase was actually being used, and looked around Washington University where I was and wanted to really find somebody who was a technological innovator. I had at least a belief then, and I think it was the right instinct, to pursue training with a technological innovator in a field as somebody who might catapult my career forward. And once you came up with that logic it was quickly apparent that a great individual to work with was

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Maynard Olson. But I didn't go and work with him thinking human genome project because that was just some gossip kind of stuff that somebody like me was reading about, but I was a little aware of it. But I really went to him as a technical innovator in this new emerging area of genomics. And I'm setting the table for that to recognize that I entered his lab in July of 1988 just when there was fairly intensive planning going on for the launching the genome project and quickly found my way on a very exciting project that was instrumental ...

BCD: Where is this relative to discovery of the YACs gel tab?

EGreen: No, no, no, you're going to really dig me into history. That's great. Probably better that of my memory of the Bermuda meetings. But YAC, so this is also key to this story at a personal level, maybe not for you, but the personal level, is that YACs were invented along with major advances in pulse field gel electrophoresis by two of my graduate school colleagues, David Burke and Georges Carle, and we were all ... both of those guys were graduate students but I was an MD/PhD student, but we were all friends. And it even gets more incestuous than that because Georges was a particularly good friend, met my wife, was a technician in somebody's lab and one of her graduate students in that lab at that time was a woman going out with Georges Carle, so we were social. I got to know Georges in particular extremely well and sort of got to know David Burke. So I just sort of knew about them even though I was working scientifically over here, they were doing this crazy, wild stuff of YAC cloning and pulse field gels. And they were working with Maynard and had said great things about him, which naturally gravitated me towards him. So when I arrived in Maynard's lab, Georges had just left. In fact, I inherited some of his scientific accoutrements, tweezers, pipette, and pencils because I literally sat at his bench. He had just left. And meanwhile David Burke was just in the process of boxing up to move to Shirley Tilghman's lab as a post-doctoral fellow. It's part of what made me believe in some ways in the world, sometimes in your life, it's sometimes better to be lucky than good. I couldn't have timed it any better because I walked into a lab where there was actually a pretty big exodus of some superstars with these incredible opportunities of, my God, you have this YAC technology and you have this idea of the human genome project and you have this brilliant person, Maynard Olson. I mean an idiot could have been successful and that's why I was successful. I'm happy to say it. [Laughter] I was in the right place at the right time. I walked into that.

So that was 1988 and intense planning stuff, Maynard fueling me with great ideas of some of the earliest things to do with YACs to try to map the human genome, how this will set up a circumstance to launch the human genome project. And there was always method to his madness and the madness was, get some of this stuff preliminarily plotted out, which I did in '88 so that Washington University was poised to submit a major application for the human genome, first, one of the first human genome centers, which was the one that actually David Schlessinger was the head of but Maynard was involved.

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BCD: Did you write part of that grant?

EGreen: Not only did I write part of that grant proposal, Maynard made me defend it at the site visit as a second-year post-doc. This is prototypic Maynard. It is absolutely prototypic Maynard, is that he just said, go. And I'm so ... in retrospect I can't believe I maintained bladder control during that. I mean if I knew ... and there was a lot of, we can get to Bermuda ... if I knew then what I knew now, I would have been much more awestruck or freaked out or nervous. I was just so naïve, because that's really where I'm trying to set this table. You just hit on something else. Is that, I really didn't understand the full ramifications of what I was doing because I was just so into the moment of, this is just cool science and I'm pretty extrovert, I don't mind talking to anybody. And if I pulled out that site visit I can even remember the room at Washington University where we had the site visit. And I know it was Hunt Willard ... and Hunt probably remembers this. It's the first time Hunt met me. Hunt Willard was on the site visit team. I remember that. Who else? There were some other superstars on that site visit team, and I should have been just incredibly intimidated but I was so new to the field. I did not come into this from human genetics. There was no genomics. I was ... so I didn't even know of these superstars. And yet I just sort of walked in there, and Maynard made me, he said ... so I wrote this section with him all about STSs and YACs, put that in the first grant. And he said, I want you to defend it. I may have been the only post-doc that did it at this major site visit. And there was a lot of hoopla with this, right? It was the first set of site visits for the human genome project. So they were big and they were big deals and they were bringing in the provost and they were bringing in the dean, and I was going, why are those big shots here? And I'm just sitting there going, and here you have a YAC clone. And that's what I was told to do, so I did it. But in that perspective Maynard told me that was key. That was part of the key. I'm not saying I was key, but part of what they really liked about the Wash U center was that they had a whole bunch of sort of a youth movement going on of people that wasn't going to just be Maynard. They really saw how they were putting people together that were really ready to be intense about this. Which is exactly what we were.

And so, yeah, so I deviated a little, but the point is that was ... so the point was, without even knowing truly what I was doing I found myself working on the human genome project. Front line. First funded center. All these press releases going out. I just didn't pay attention. I just wanted to make maps. I had these YAC clones. I had this idea how to do it. I wanted to build maps. And it was actually very funny because another thing I remember. In the middle of this ... and I'd have to go back and look at what year it was, it must have been in 1989, maybe the end of '88 or '89 ... Maynard of course wanted me and I wanted to apply for post-doctoral fellowship. So I applied for a Helen Hay Whitney fellowship and I remember being flown out to Stanford for the interview with David Hogness, who was on the selection committee. And I go in there, I know my YACs and I know all of our ideas, and we have all these ideas about how

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we're going to do this and what I had proposed in this, in my fellowship application. And we talked about it awhile and then he looked at me and goes, "So what do you think" ... I really remember this question ... he goes, "What do you think about this? Should we really do the human genome project or not?" And I was sort of stunned by that question because I was just so ...

BCD: Why wouldn't you?

EGreen: Well, why wouldn't you? I guess I had read about some of the controversy, but I was just so narrow in my focus and I just want to make these damned maps because I'm a post-doc. That's what I'm supposed to do. Well, of course, why would you not to this? This is working. That was sort of my answer. And I'm sure it was the most superficial answer. And I think what it illustrated to him, that I was not thinking big picture. I was so thinking focused picture. But that's what post-docs should do, right? But I look back now and I think that nowadays in my job all I'm supposed to think about is big picture. I don't have time to think about little pictures. And yet then that's all I thought about.

So in any case, this is all story time, but on the other hand it does illustrate some of the things about Bermuda. So by the way so there, genome center, Maynard ends up leaving, I end up getting the faculty position, we fast-forward. I mean I was his post-doc, but then just the time that I needed to go look for a job I went out looking for jobs and tried to get recruited. Francis tried to recruit me to Michigan and I think that was the first time I turned him down. I haven't turned him down ever since, but it was the only time I've turned Francis down for anything. And decided to stay at Wash U. And it was just the time that Maynard decided to leave. And Maynard left. I became the head of that project for the center. I took over his office. I took over his life. He just gave me everything. But the genome project obviously was in full tilt. I took a faculty position at Wash U in 1992. And then Francis successfully recruited me here in '94.

BCD: So what was your role in that original grant? Did you become one of the major [Inaudible – another speaking] of the grant?

EGreen: Yeah, so the grant is ... David was the head of it but then there were several projects within the grant. David Schlessinger ran the X-chromosome map. The Wash U genome center, the first one...there was a separate one, obviously, that eventually grew up for sequencing...but that one was about mapping the X-chromosome, mapping chromosome 7. And David Schlessinger ran the X-chromosome and Maynard was supposed to run chromosome 7, but he completely delegated it to me.

BCD: You then inherited that?

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EGreen: Yeah, so the whole deal that kept me at Wash U was Maynard said, I'm leaving. And he had already made an independent decision to leave, and he says, I think, Wash U, I think you're crazy not to convince Eric to stay and just let him take over this project. So it even made sense, everybody wanted me to do it. Turned out that Michael Gottesman, of all people, had to get involved in it at the time because Jim Watson had already left and they hadn't recruited Francis. So this was at a time...so all this had to sort of be dealt with because they were changing the P.I. of a major component of a major center. But everybody agreed. They even brought a site visit team in and they all agreed it was fine. So it was just interesting because Wash U's startup package to me was actually a regular startup package but also an immediate ascendancy to a major project.

BCD: You were doing chromosome 7?

EGreen: Yeah.

BCD: So maybe when you come down to Durham we should do a thing of CF or something, because I don't know that history. I'd be interested ...

EGreen: That's how I got to know...Francis.

BCD: [Inaudible – another speaking] collaborative research story there.

EGreen: Yeah, although there's some bad soap opera activity there.

BCD: I know.

EGreen: Oh, you know about that. So that will get ...

BCD: That's why I don't want to do it on tape.

EGreen: Don't do it on tape. Although we could do it on tape if you wanted, that was how I first got to know Francis, because Maynard ...

BCD: Through CF?

EGreen: No, before they found the gene. Maynard said, "You could pull out any region of the human genome big block." He goes, "We need to get human geneticists working with you so you can help them find genes. You should talk to Francis Collins because he's trying to find the CF gene." So I have a whole story I could tell you sometime about the first time I met Francis at Cold Spring Harbor at the genome meeting. Francis had found the gene but didn't ... this was in May of '89; he published in September. They had found the gene but they weren't talking about it. But we forged a collaboration with him to help him. And the first time I met Francis, he was talking in a group and Maynard said, that's him. I'd heard



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about this Francis Collins, white coat, physician-scientist star, and all I could ... think he pointed to some guy who was in these disgusting, dirty blue jeans and some big cowboy boots. And I looked, and I went back to Maynard. I said, "That's Francis?" I couldn't believe it. Yeah, that was him, because he had dressed down. So anyway, that's actually the history ...

BCD: ... Cold Spring Harbor.

EGreen: He was dressed in Cold ...

BCD: It's reverse snobbism.

EGreen: I agree, but it was a little dressed further down than I ever thought. But, yeah, so that's what catapulted me even to Bermuda. I was leading the chromosome 7 mapping effort for the human genome project. And it was designated. And then what happened was when Francis recruited me here ... so I took a faculty position in '92 ... and then by a few months later practically Francis decided he was coming here, called me and said, "Will you come?" And I agreed to come here. But I couldn't because of a spousal issue having to get synchronized to move. She was in the middle of clinical training. We had to wait a year. So I didn't arrive until '94. And I sort of just ...

BCD: '94 is when you come ...

EGreen: Came here, yeah. And move the chromosome 7 mapping project with me. That was all part of it.

BCD: Oh, really?

EGreen: Yeah, that was all part of the negotiation. And it was fine. Wash U was fine with it because David couldn't handle doing that as well. And they knew it was completely intertwined in my early career. And they probably weren't going to argue with Francis. It was sort of an awkward thing but everybody seemed to agree with it pretty quickly. So I moved it here to the intramural program.

BCD: So you'd been here for about a year and a half.

EGreen: Correct. But realize that I was ... so '96 was actually just the year I was coming up for tenure. I was tenured in 1996 here in the intramural program. But I'm still a fairly junior investigator. I'm just sort of a frontline, we have got to get this map of this human chromosome done, high quality, perfect, etc. And then ironically...and here is the irony...is that since 7's map was so good because we had one of the better maps for a real-sized chromosome, not one of those junior teeny chromosomes that surged ahead in the early days of the human genome project because they were small, but a real, average-sized chromosome, our map

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was so good, when Waterston in particular said, “All right, this nematode stuff’s going pretty well. We’ve got to start turning our attention to how we’re going to figure out how we’re going to sequence human DNA.” Bob was my chairman for a while and my buddy, even though I left. And we immediately said, Wash U wants to do chromosome 7. Let’s work with Eric even though he’s not here anymore, we’ll just work with him. So that put me at one of these early partnerships between the mappers and the sequencers. And I was the mapper with a darn good map and they were a sequencing center starting to work with ...

BCD: So chromosome 7 is becoming one of the early targets for sequencing.

EGreen: Yeah, yeah.

BCD: Because the map is in good shape.

EGreen: Because the map is in good shape. That’s the reason why. And yes, there were other chromosomes, like 21 and 22 in particular, because they were so small and they got big-focus efforts early on and lots of things, but Bob Waterston recognized that, and Rick Wilson recognized, that 7 was going to be really where they could cut their teeth on how we were sort of going to tackle this.

BCD: Were you there mainly because of the session that David Cox was doing on mapping?

EGreen: Yep.

BCD: Okay.

EGreen: Well, I think I was there ... if you sort of look at the list of folks...I should also point out that...I think, if I can get into Francis’s head back then...is Francis is head of this institute and the intramural program was a bit of a Noah’s Ark. When he and Jeff Trent built it there was a Noah’s Ark. They said, you know what, we need two cancer guys, two cancer researchers and we should probably get two good developmental people and two really good mouse people. Oh, we should really get two genome jocks, of sorts, and that was me and Nick Dracopoli. I think, by then I’m not even sure Nick was still there. He only stayed a little bit. And he was sort of doing a lot of linkage mapping but was sort of peripherally involved in some of the mapping and sequencing. So I was the genome project guy. I was really, if you really look at the history of the institute, I was really the only person in our intramural program that was heavily involved in the human genome project. And that was my ...

BCD: Everybody else was doing human genetics, basically.

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EGreen: Right, right, and that was all by design. But it was the design of a Noah's Ark and I was his token. And that was fine; I knew that was my role. But as a result, I think Francis did rely on me a lot. He would sort of have his extramural program staff, all of which worked with me, ironically. But, sort of fast forward, which is funny. But they were off over there and I was sort of his reality check back to what it's like on the front line. That's the funding agency side and the coordination side, but then I'm the frontline guy that has to sort of really deal with how is the science really being done. So I think a lot of times I got invited to things because I could provide that perspective. But I think even independent of that, legitimately, they invited all the...if you sort of look at this, of all this stuff...they had the chromosome 21 guys, the chromosome 22 guys, they had the X- chromosome folks and they had the chromosome 7 folks. And it was sort of the front suite of chromosomes that were going to be the earliest targets.

KM: For the sequencing.

EGreen: For the sequencing. And it became a whole issue around how are we going to release the maps? It was all intertwined in terms of data release issues and access and things like that.

BCD: So what did you expect to happen there?

EGreen: So in thinking about it, that's the great irony is that having not come from the genetics community I don't think I came into this with a whole lot of angst around this idea of sharing. And I think there was a lot of issues around, which I think a lot of it, in my view, I might be wrong, but my view was that a lot of the consternation revolved around, or maybe it was amplified by, the perception of the human genetics community which had traditionally not been a very sharing-oriented group.

KM: We have students writing papers on this for our class.

EGreen: Right. So going into it I didn't have that concern. And so my attitude was, well this is all about getting the data out. So to me, I sort of went into there saying, well, why not? This should all be relatively easy to do. With that said, I would say the only angst I probably had coming into this is the recognition that however we do it, we, and having just been tenured and just accumulating a lot of trainees who were working on this, is wanting to make sure that whatever was going to be done that we don't totally disassemble things to a point that people can't get credit. And so, and I was probably a little influenced, it was interesting that...and this may be relevant, let me deviate just for a second because it might be relevant to get into my head for some of these things...is that I and a small set of other people that would probably include people like Geoff Duyk, Nick Dracopoli, maybe somebody like Ken Buetow, I can think there were probably a few others...there was that first set of post-docs that really got involved in the genome

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project and in genomics that all went out looking for jobs in about 1992. And there was a lot of sociology going on with this because it was a two-edged sword, that everybody said, we need genomicists. That was just the realm. Oh my God, we've got to get young talent in genomics. But every one of the folk, every place you would run into consternation of, how the hell are we ever going to tenure you? You're just contributing to this public good. Or, you're not doing hypothesis driven research; you're building maps. Where's your hypothesis—that the maps can't be built? So there was a lot of that. So it was very interesting that at the same time it was a hot-cold thing. It was like they desperately needed you and wanted you, but then as you would interview...I'd go to places like the University of Pennsylvania, for example. Even a place like the University of Michigan with Francis. He tried to protect me from the voices that he probably...I didn't want to hear. But it was interesting. A lot of people, oh, this will be good. And a couple people would say, well what are you going to really do when you grow up and you become a P.I.? I would get a little bit of that. Like, when you go to get tenure, you're not going to be building maps, are you, because you're not going to get tenured building maps. So there was a little bit of, how are we going to do that? I think that was a little bit of cold water in the face. Or it certainly quickly did. And I got a little bit of that at the University of Utah. I only looked at four places, I think ultimately. And it was a little bit of cold water. It was like, this is the most important stuff going on. How could you not think this is tenurable? That gave me a quick wake-up that there are forces out there that would conspire against the professional development of young faculty if they got too heavily invested. And I think we still face this even today.

BCD: It didn't feel like that in the intramural program here?

EGreen: I think to a large extent I was incredibly naïve. But part of it is I don't worry that much about stuff like that. So I guess I wasn't that worried because it was just a groundswell of excitement. Maybe. But on the other hand, maybe this was sort of in the back of my mind, I remember consciously thinking that if I stayed at Wash U...when Francis offered this to me, when I thought about it, I think I thought about it for 30 seconds...so for the first 15 seconds of that 30 seconds before I said yes, I probably thought, what would my life be like if I stay at Wash U? And my criticism of Wash U at the time was that they really weren't building human genetics. They were building genomics but no human. And partially to link me back to my clinical involvement in pathology, I always said I like this genomics stuff but I really am getting into this because I really ultimately want to do human genetics. And then the second 15 seconds before I decided to say yes to Francis, was the recognition that, wow, I go there to this Noah's Ark, I get to work with Bob Nussbaum and David Ledbetter and Francis Collins and Jeff Trent. Holy crap! I will learn a lot, and that will be...I will bring genomics, they will teach me genetics and then I will have a much more diversified thing. And it might have been, I don't know if I thought this all the way through, but there probably might have a little bit of a desire to broaden my game because of maybe getting the cold

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water on my face a year earlier, saying maybe you'll never get tenure. That maybe the idea of coming here and being able to both do genomics, but then...and that's exactly what I did because I finished the chromosome 7 stuff and then I built up a whole part of my lab that did human genetics. We cloned genes and we did...and then we eventually got into comparative genomics. But the point was, there was no question I could do tenurable work even outside of my human genome project stuff once I got here. And I was not totally sure I was going to be able to do that at Wash U as easily. But doing it here was like cutting butter with a hot knife because I had great colleagues to help me.

BCD: I just want to make a connection here. You're doing the genome project among a cluster of intramural researchers, many of whom are doing classic human genetics and had the classic norms of data sharing from that field as part of their ...

EGreen: Yep.

BCD: And yet you're going into ... so how are you thinking about that?

EGreen: Yeah, that goes back a long way. I'm not sure I had been influenced by very much. But on the other hand, some of those folks, the Bob Nussbaums of the world, are incredibly altruistic people. So I'm not sure they would have dissuaded me from any of this. I mean I think they probably viewed the world to the point the human genome project was to get the darned data out. I don't think they would have ever dissuaded me against that.

BCD: That's actually a different cluster of human geneticists ...

EGreen: All sorts of different ...

BCD: ... from animals, that were doing ...

EGreen: Right.

BCD: ... Alzheimer's or the incredible tawdry stuff that was happening. Well, neurofibromatosis had a little bit of that in but that was actually more Francis and Ray White.

EGreen: Right, right. So I guess my point is, as I entered into these Bermuda meetings I don't think I brought the baggage or the consternation of the ... I'm not sure I knew enough about the bad stories about human genetics because I hadn't been living in that field. But what I think I was bringing was, and the more I think about it, was that whatever we do let's just try to make sure we preserve careers. I was always very sensitive, especially back then, about the career paths for young genomicists to make sure, because it's easy ... and I remember thinking this, and I may have even said this, is I remember saying, Bob Waterston and John Sulston

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don't have to worry about tenure anymore. They don't even have to publish any more papers. But I do think there was an issue that we need to find the right balance, the right way to preserve things, to make sure that folks who were making contributions ... to some extent, I'll tell you, I continue to carry that torch. I can even point to, I could actually show you today the criteria for tenure in the NIH intramural program. And there are two bullets in there, they don't have my name, but it was when I was the scientific director of the institute, I got them changed at the NIH level. I got them added to reflect the fact that part of tenurability, among the criteria for tenure at the NIH intramural program should be the deposition of large data sets in publicly available repositories as one of the criteria. And then similarly and then saying in another bullet for software, the development of software, other analytical tools, and make them publicly available, at a similar scale as publishing really great papers in high-profile journals.

BCD: Just to mark it. It would be wonderful to have that document that we can do a cross tab with out of this interview.

EGreen: Sure. You can pull that out of the NIH source book I'm sure. Or boy, lots of people who have [Inaudible – another speaking] would have it.

BCD: [Inaudible – another speaking] would have the document, I don't know.

EGreen: But there should still be, unless they changed it, because I've not been an SD for two years, but there definitely were two bullets that I ended up getting added and passionately argued about and pushed for. Because I just wanted to make sure that folks coming up, and even now, because the human genome project is not the last consortium-based project. And we constantly have issues about people being worried about being too invested than being the middle author of 50 and never getting ... and so I think that was one of the flags I carried and continue to carry.

BCD: I want to loop this back to Bermuda, but before we do that I don't want to lose this point. So how do you think about this ... you gave us two examples of things you can do about that tenure criteria for credit. But I'm going to fast-forward this here basically and think about genomics as a field. How did the field ... a lot of people seemed to have had pretty good careers who were part of this process. So tell us a little bit about how that worked. Is this like high-energy physics where even though there are 100 people on the paper everybody kind of knows what everybody has done and who's good and who isn't good? How are ... instead of being the lead author, because that can't be your criterion, how do you know who's good and bad at that and who should be promoted and who shouldn't be promoted in a field that is this ...

KM: You mean now?

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BCD: ... as this field is evolving, a lot of the papers that you presumably are part of have got a lot of names on them.

EGreen: Yep. So I can point to several things and I would also remind you that ... I can think of many fights that I've fought over the years. And I would also remind you that the other hat I wear and have had lots of involvement in is the hat of a founding editor of *Genome Research*. And we have seen a lot of ... a group of editors, many of which are involved in the genome project, practically everyone, and very involved in thinking about these same issues. So I bring lots of different experiences to this. Here's an interesting thought immediately: Is this doable? Can I find a student to do this? I think one of the things that has grown considerably, catalyzed by the genome project, are co-first authors, co-senior authors. I don't think in '85, you go back to the early '80s, were there as many papers with co-, two and three first authors with an asterisk or two or three or four senior authors with an asterisk? So I think we pushed that.

The other thing clearly that's happened, and although some journals are better about this than others, and I fight fights all the time for this, is a journal's willingness to use consortium names. Journals follow some ridiculous rules and I've had to fight them at various stages. Oh well, you can't mix individual authors and a consortium. Well of course you can. And then you win with the journal and then you have a fight with NCBI because they won't index everybody individually. They'll only index the consortium so there's all sorts of things. But what I would tell you is I have fought like stink over many different circumstances to just have whatever these tools are in place to be able to make sure that people can adequately identify individual contributions. Now meanwhile, some journals have gone even a step farther, like I think *Nature* is one of them, but some journals are now saying when you have lists of authors you need to identify author by author what their contributions are. And some even have like a checklist where you can sort of say for every person you have 10 categories, what did they do. And you check they did this, this and this.

And then I will also tell you that, I don't know if it's in place here yet or not for the tenure process, but I've heard about institutions requiring that when you submit a tenure package on somebody that in addition to whatever the number is, five or 10 papers that you put as part of that package, you need to for that individual give a two- or three-sentence explanation of what their contribution was to that paper. Which gives you a platform for being able to stick your nose up above a sea of authors and be able to explain what your unique contribution was. And in some cases there are overwhelming contributions. I developed the algorithm among 50 authors that generated a crap-load of data. I figured out the algorithm that allowed us to sift this, that and the other. And it was a year of work but without it even though I'm a middle author, without it they could never have done these analyses. So I think there's a lot of things we have done, and I still don't think we have it perfect, but I think there's been a cultural change. I think

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those cultural changes are tied to this in many ways because I do think that if we would never have had data release the way it was, we would have gone back to very obvious single papers. It would have just been the kind of thing that would have held the field back but would have kept us in those traditional molds.

BCD: So I'll stop getting in the way. Back to 1996.

EGreen: So what's the question? So I guess I entered the Bermuda meeting.

BCD: What did you expect to happen there? What did happen there, from your perspective?

EGreen: Yeah, so I have to admit, I don't know if you've found this with other people, I find the three meetings blur together. I have a lot of trouble remembering what happened. I have a vague memory that the third meeting seemed anticlimactic after the first two. It seemed like everything was solved after two but I think it was the Wellcome Trust just more than anybody I think just wanted to make sure everything was tied in a bow and we have this momentum of meetings two years in a row. But I remember just sort of going and wondering, is there really a critical agenda?

BCD: You guys had no wind at that last meeting of Celera in the offing.

EGreen: Wow, I don't remember that, yeah.

BCD: I think that was May.

KM: Well yeah, the meeting was February. So ...

EGreen: And by the way, I did dig back. I have all these pictures and, do you have my digital pictures from that?

BCD: No.

EGreen: So I had just gotten a digital camera and I think it was the third meeting, and I'm the one that has a few pictures.

KM: Yeah, that'd be wonderful.

EGreen: If you give me a thumb drive I'll just hand these to you. So let's make sure we don't forget because I actually pulled them up. I actually pulled them up.

KM: Along with his pocket protector he's got a thumb drive and ...

EGreen: You think he's a nerd? [Laughter].



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KM: Yeah, he might be a little bit of a nerd.

EGreen: Yeah, but you're speaking to one. I don't have a pocket protector.

KM: That's okay, I'm a nerd too.

EGreen: So in any case, let me give you a ... I only have about four or five photos, I think.

KM: That's wonderful, thank you.

EGreen: But I think that was from the third meeting. And then I looked to see whether I had any just regular photos and I don't think I ever took them in the first two meetings. So what is my other memory? I guess my other memory, I guess I remember going into this, I mean I knew there was going to be some drama, just because it was as much a drama about coordinating as it was going to be a drama around actual data release. But I also remember probably not fully appreciating the sociological barriers we were breaking down on the one hand, but being very aware of the ... having just been up for tenure, although not being worried, but being probably more paranoid about the young view than I was about the map review. Does that make sense? In other words I think Waterston and Sulston were there to have a paradigm change, to create a paradigm change for all of science. Okay, and I respected that. I thought that was cool. I don't think I appreciated the magnitude of what they were trying to accomplish. And I was probably much, as I often am, much more practical focused around on the one hand yes, we have to do this because of the genome project, it's the only way to do this, and then my own worldview sort of said, but let's make sure we do this and protect young investigators and don't make this so that it's not an appealing thing to be. Because I think I was sensitive to the idea of making sure that the career path for genomics was not a scary one but an exciting one because of having cold water splashed in my face.

BCD: So you just did an allusion to a framework for thinking about science, which is a shift to the structure of scientific revolutions and paradigm shift. And you attributed some of that paradigm shift to ... I just want to make sure that I'm following your logic here ... to the data sharing policies or something that happened at these meetings that was part of what seems to you in your career to have been a shift. That's a term that you use in retrospect. So let's focus on what is it that shifted. What is the difference here? In what way is this way of doing science different from ...

EGreen: Well, the fact that data was going to be given out before it was published. To me, that was, my memory, maybe I should look in my memory, my memory was that coming up with the criteria, sitting there, and I think there are these acetate sheets somewhere, of how they were going to...what were the terms under which data...

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BCD: I want you to explain...I know, but I want to hear you say it...what does this mean? Why is that a big deal? How does that work, and what does it mean to the conduct of the science? Why does it matter?

EGreen: So it was against the grain of, you generate your data, you publish your results and then you share everything all at once. But that you hold everything until you have gotten your credit, credit defined by a citable publication. That was, that's the traditional way, and since I had grown up in the halls of McDonnell Sciences Building at Wash U, hearing Bob Waterston talking about this other world of especially nematode community where they were more interested in sharing and not worrying about getting credit, and that was the antithesis of what I had heard caricatured as the human genetics community, that it was very clear that this was going to be a challenge because this was against not only the traditional grain of science, not medical science in general, but it was against the traditional grain of human genetics in particular.

BCD: So there are two general ideas about where these ideas of open science are coming from. And I want to ask you from your perspective where it feels like. One idea is this is coming out of the way that a certain set of biologists are organizing their projects, yeast and nematode. Maynard being an exemplar of yeast and John and Bob being the exemplars of nematode. And an open sharing norm that's evolved out of a way of studying a model organism that doesn't have a lot of financial stakes associated with it. Disease implications are there but they're indirect. The other direction that people talk about is what's going on in software in the Microsoft wars and people doing computational stuff that's also becoming important in genomics in bringing those ideas in. From your perspective do both of those seem to make sense or is one of those more important than the other? Or...

EGreen: So my memory of the circumstances that were framing the Bermuda meetings were much more the former. If it was anything around the latter...God, this is hard to go back...it wasn't so much software development. It might have been some of the early EST shenanigans that were going on.

BCD: But actually in software what's going on in those days is there's a movement that's quite aggressive and explicit about Stallman and a countermovement to the Microsoft version of high intellectual property fences around software.

EGreen: Yeah, but I have no memory of those arguments permeating it. But I do have memory of that some of this whole thing around intellectual property concerns were more because the EST wars.

BCD: So it's EST wars and, just to inject another...I don't know the degree that it was important but remember the *BRCA1* is a story of 1994 and the *BRCA2* is a story of 1995 and you're meeting eight months later.

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EGreen: Yep. So I think the whole idea of gene patents were floating, right. So that clearly contributed. So I think both of those things ... so it's sort of the white knight, this is the way it should be done because it works well, and one of the reasons why this should be done well is because then you avoid the dark knights over here. So I think there was a little bit of that. Wow, that sort of reminds me of the cover of Cold Spring Harbor meeting, that I got in trouble because of that. But that's a whole other story too.

BCD: So now let's take your perspective now. Having established a norm and actually a practice, it becomes actually embedded in practices, how does that matter and does that change the way that you do your guys' business around here these days?

EGreen: Today? Oh absolutely. It was a total paradigm shift that is pointed to repeatedly. And now it's no longer the, wow, that's the weird way of doing it. It's like that's the way large projects should be done. Every once in a while it requires a little bit of tweaking, whether we did it in, had to go to Fort Lauderdale and tweak it a little, had to go back to Toronto fairly recently, tweak it a little more. But there's no questioning the fundamentals. The only issue now becomes which projects does it apply to, how do you practically do certain things, sort of bum-bum-bum-bum-bum. So it was probably sort of obvious that, and I can't imagine you even thought about it, but it would be interesting to talk to 30-year-old scientists now, young trainees, and they probably would think it was horrific, the idea that human genome project data wasn't being released in real time because I think we've changed, we've just sort of changed the cultural norm. I don't know if you're doing any of that but it would be interesting to do that. Sort of like talking to them and they just said, "You once lived in a world where you couldn't just bring up the human genome sequence on the Internet? You must be kidding." I gave a lecture last night to some high school students and you talk to some of them about, even in high school they realize, they just...they just said, there was once a day where you didn't have the genome sequence? Like wow, I guess that's when you didn't have flush toilets either, right? [Laughter]. It wasn't that long ago and yet the new generation coming up, they can't even comprehend how you existed without it. I just think some of them would say, well if it's one of those big genomics projects of course you're going to release data. You're just not releasing it fast enough as far as I'm concerned because I'm so hungry for the data. It's what they expect.

BCD: We should look at our script to make sure that we haven't ...

KM: That's what I'm doing. So do you remember in, well it was in 1996 and I know the meetings are kind of conflated, but there was a whiteboard, it was the last session in the 1996 meeting, and it was John Sulston who wrote these principles and he's scratching a bunch of stuff out. Do you remember that and what were your impressions of that meeting? Because actually that meeting was almost all

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scientific. Kind of like what do you have and what's the other guy have and what kind of quality standards are you going to use for releasing your data?

EGreen: This was the file I just found this morning.

KM: I'm just wondering what your impressions of that were, if you remember it.

EGreen: Oh, no, I remember it was...was it a whiteboard or was it an acetate sheet in an overhead? Do you know?

KM: I think it was an overhead.

BCD: It was a whiteboard.

KM: Oh, it was a whiteboard?

BCD: I think it was a whiteboard but we don't know. What we have is a picture of ...

KM: Well and then Francis ...

BCD: I guess it could be projected either way, but somebody's got a photograph ...

KM: Well, yeah, but it was on a projector, right, because I think Francis gave a talk at one point where he put it on a projector maybe?

BCD: No, the picture that we have of it is actually from Francis' GenBank at 25.

KM: Exactly, yeah.

BCD: Where he has a picture of whatever it is up on the wall. I don't know; we don't know.

EGreen: Yeah, no, and here's the...Bob and John proposal, just two bullets, immediate data release; second bullet, no protection before release. I don't know if that was off my notes from that white or whatever, or that was something earlier in the meeting. But in any case, I have a memory of that because I remember and it was in some ways it seemed so simple but I also knew because the way it was being presented that it was sort of the heart of the issue. And the fact that John and Bob were presenting it and crafting it in the way they were.

BCD: So what's the role of the NIH intramural program in the sequencing part?

EGreen: None. Because basically it was very clear that by the time we were ready to start sequencing 7 it would require a real industrial scale. So there was never any thought that I was ever going to sequence 7. That's why I immediately forged the

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collaboration. Ironically, in '97 I created the center here and then eventually got into production sequencing, but it was not for human. It was always for comparative stuff.

BCD: Is there any relationship between what's going on at NIH and what's going on with Mark Adams and Craig at TIGR at that point?

EGreen: No. No. I remember sharing a cab ride for one of those meetings with Mark and Craig, but it was also, I think it was already sort of getting a little strained. I could tell. I knew it was. Those guys were just, yeah, it was strained even at the mapping level, the way they were handling things. Even while they were in the genome project it was clear they were ...

BCD: You mean when they were still here...

EGreen: Yeah, yeah.

KM: So how was this enforced, this data release?

EGreen: How was it enforced? How was it, or how was it envisioned? I'm not sure I have the answer to either but I'm just ...

KM: Both, I think.

EGreen: Yeah, because one of the issues I know that came up was, there was more clarity how to release the sequence data than there was about the mapping data. Nobody at that moment in time knew the pace at which the sequence was going to be generated. And so ... in fact that may have been, now that I think about it, it may be another one of the reasons why I was there and they invited some of the mapping groups, is because they also realized that we don't know what the shelf life of these maps are going to be, how are we going to get those maps out? And so I'm not sure I have a memory of how it was going to be enforced other than there was, at least in the case of NHGRI groups, there were obviously a lot of progress reports being written and a lot of feedback constantly given to program staff as there even is now. But there was always the issue around a lot of this stuff that it was an honor system.

KM: Right, exactly. So you were at Fort Lauderdale and you were at Toronto; you went to both of those meetings. And in your impression do you think that those meetings were more important for bringing what happened in Bermuda to scientists outside genomics than the actual Bermuda meetings were?

EGreen: Oh, yeah, no, I mean the Bermuda meetings were squarely on the genome project and on some of the earliest pioneers. And there's no question that once that got to be the norm, quote/unquote, that Fort Lauderdale and then Toronto was much

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more about sort of moving out to the boundary, what projects it applied to. There was much more about expanding it beyond the genome project. No question.

KM: Right. Exactly. Interesting.

EGreen: And also especially Bermuda, no, especially Fort Lauderdale, what I said, I saw was happening both as a data producer but then as a journal editor, and especially at a place like *Genome Research* where we would get a lot of the manuscripts I mean we were sort of the lead specialty journal or the lead journal among the specialty journals in genomics, not so much genetics but certainly in genomics, we would get a lot, we would get a lot of papers where immediately the editors would know something's wrong here, that you're not giving credit to the people who generated the data. And then the question became what is the role of the journals in all this? We would sit there and have these things of, well should we be telling NHGRI staff that somebody is being unfair to the Wash U sequencing center? What's our role? Do we go in and enforce this or should they go in and enforce this? So that's why it almost became like, I think Fort Lauderdale, I think so much about we have a community. And what are the different people's roles in this community to make sure that this cultural norm is kept within the lines and is not abused?

KM: So speaking of cultural norms, you're speaking of culture more in terms of scientific cultural norms.

EGreen: Scientific culture.

KM: Right. Well just to push on that a little bit what was your impression of the attitudes of scientists from outside the U.K. and the U.S. labs that were at the Bermuda meetings?

EGreen: Yeah, that's ... actually I remember that was an issue. And I'm not even sure ... and it always has been an issue. It continues even I think even today as I've done some world travels to be an issue because those folks are individuals who started to get into the genomics field, countries sticking their toe in the water for the first time. Rightly or wrongly they were going to not be able to keep up in terms of the amount of money that they could put in as the U.K. and the U.S., and probably were so worried about never getting any credit because they could never get their voice louder than Lander's or Sulston's. I mean, it's just hard and I can sympathize with them. And so their worry would be that the contribution of Country X would never be recognized and so they would go to their governments to try to raise the money. And I think they were very concerned that if they didn't somehow say, oh, and our scientists will be able to take advantage of this in a way that will give them some advance head start over the big countries. So I think they faced a lot of issues. And I think the people that were at the Bermuda meeting and similar meetings were just caught in the middle, right, because they're saying, we

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want to join your club and we actually agree with you, but when we go back and tell this to our governments they're saying no, because we think that the reason the government wants to invest is they want to get intellectual property, they want to get recognition, they want to get all these things so they can become capitalist bullies like the U.S. kind of thing. I remember having a conversation, I can't remember with whom, but I remember always just sitting there thinking, I feel sorry for these people. They were getting beat up a little bit, right, because they were saying ... there was a lot of, you've got to buy in, right. It was sort of a little bit like Bob and John saying, you've got to buy in and then eventually it was Francis, you've got to buy in. At the same time they wanted to but they couldn't speak, they didn't have the authority.

KM: They had to go back and ...

EGreen: They had a responsibility but not the authority.

BCD: A lot of this stuff, and I think we've covered almost everything we need to do. The residual questions are about any documents that come to mind, but I think we've have already kind of covered that. There is one interesting question which is, in retrospect, are there constituencies that should have been there that weren't?

EGreen: At Bermuda?

BCD: At Bermuda.

EGreen: So I would say one constituency I remember, and maybe I'm not saying it needed to be there, but the fact that the one thing about Bermuda was that because it was trying to do something so historic it had to do it a little naïve. And so one example I would give would be it was never very well defined how any of this was going to get enforced or what the checks and balances were going to be. And I don't have a memory of recognizing it, but I think if we would have paused for a minute we would have realized that the journals were going to get caught in the middle of this. It was just no question they were. And I think in retrospect, maybe some of us were there who were journal editors, but we really weren't wearing that hat there. And it was the one big difference between Bermuda and Fort Lauderdale was getting journal editors involved and discussing this. Because it was very clear that they were going to end up getting in the crosshairs.

KM: So between Bermuda and Fort Lauderdale ...

EGreen: What year was Fort Lauderdale?

KM: 2003. So you have this span of seven years between the first Bermuda meeting and the Fort Lauderdale meeting in 2003. Between then you have journal editors

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like you who were just kind of dealing with this on an *ad hoc* basis. I have this problem we have to figure out how to ...

EGreen: Do we approach this group that may not be giving credit to this sequence center? Do we go tattletale on somebody and go tell it to the funding agencies? I can remember phone calls. I can remember weekly *Genome Research* phone calls where occasionally we would have to dissect something and ...

KM: So you did all these options.

EGreen: Yeah, no, we would do different things. And I sometimes would be, I can remember a couple times being a little uncomfortable because I was, yes, I was at NHGRI but I was on the intramural side of the house. And in fact I would oftentimes ... in fact, I know I never got involved because I felt like it was a little awkward for me to be involved. In the case of *Genome Research* we always had an in-house executive editor who was either Laurie Goodman and then it became Hillary Sussman. They were always the ones that would have to make the contacts because they didn't want those of us who were scientists doing that.

KM: So I have one more question, if we have time.

EGreen: Yeah, we have time.

KM: How did this feel like there was a movement towards small labs making sure that they were recognized? Wow, we want to see this data, we don't really have the money or the resources to be generating a lot of it, but we want to make sure that we have access and participate in this too even though we're smaller, we're not one of the G-5. What role did the small lab play?

EGreen: Well I think in the case of the small labs that were actually participating in the genome project, again I think it was this idea of how are they going to be recognized as their contribution, especially if they've just got some chromosomal band.

KM: Right, exactly.

EGreen: They're not going to publish that. And it keeps coming back to this whole thing about credit. It's all about credit. And some of it was based on ego and some of it's based on practical things, like I need to get tenure or I need to get my next grant. And the social norms are publications and senior authorships. Some of it was that.

BCD: So on that credit point, let's loop back to that for just a second because I think that's important to some of the stuff that ... things like the Sage Bionetwork and places where they're developing things that aren't necessarily going to fall out



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into traditionally publishable things. These are algorithms or models or whatever. How do you think about that credit issue and career paths and how ... this must be coming up in your work all the time now.

EGreen: Yeah, and so I guess what I would say is I think it partially depends on where you are ... I mean I can just think about the advice I give my internal investigators here ... it depends on where you are in your career and what hurdles you face and when you face them. And you have to be a little practical, but you also have to use every tool available to you to make sure ... you can step a little bit out of tradition ... just use every tool available to you to make sure that you get the proper credit that you deserve. So what do I mean by that? I mean I tell our tenure track investigators here who are interested in doing consortium-based science to only spend upwards of 50 percent of their time doing consortium-based science. So I just think it would be bad if they go for tenure if every one of their papers they're a middle author in a sea of 50 authors. But if half their papers are of that but they also ... I say, some of the stuff you're doing go publish something on your own so you can get the traditional stuff to convince people that you can do the ... because you're never going to win everybody over. And then meanwhile on your consortium papers you absolutely have to document, and you can just do it as part of your tenure package, but make sure that you have a unique contribution. And you need in your two or three sentences to say what it is that you contributed. And hopefully, and even if you have to help orchestrate it, make sure that that's agreed upon by people who might be approached writing letters on your behalf.

BCD: So how's that incorporated into your ... so that's partly a documentation process ... but that has to be coupled to a process of review that incorporates that documentation and matches it. So you had to change both of those features.

EGreen: Yeah, but some of this is doable. So for example, when we put our final tenure package together that goes into the NIH for tenure, the direct supervisor, the branch chief in this case, writes a very detailed memo on why they are putting this person up for tenure.

...

So I think you have to be your advocate. Maybe another way to say it is, I think if you're going to get, as a young investigator, if you're going to get immersed in consortium-based science you need to be a little bit maybe ... right now when we're in a shifting, maybe a shifting culture, maybe 20 years from now all this will melt away but maybe it won't. Maybe we'll always be in this little bit of an environment of some people always wondering. I just think you have to be your own best advocate. That means ... and you have to build up a network of advocates. It's part of what you have to do.

BCD: Are we done with this interview part?

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KM: Yeah, I think so. Should we turn off ...

BCD: Thank you.

KM: Thank you so much.

EGreen: Sure.

END OF RECORDING