CHEMICAL HERITAGE FOUNDATION

BARBARA PANNING

The Pew Scholars Program in the Biomedical Sciences

Transcript of an Interview
Conducted by

Hilary Domush

at

University of California, San Francisco
San Francisco, California

on

27 and 28 August 2008

(With Subsequent Corrections and Additions)

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BARBARA PANNING

1963 Born in Chur, Switzerland on 17 August

Education

1986 BSc., McMaster University, Ontario, Canada, Biology
1994 PhD, McMaster University, Ontario, Canada, Medical Sciences

Professional Experience

Whitehead Institute, MIT, Boston, Massachusetts
1994-1997 Postdoctorate, Developmental Genetics

Center for Cancer Research, MIT, Boston, Massachusetts
1997-1999 Postdoctorate, Biochemistry

University of California, San Francisco, San Francisco, California
1999-2008 Assistant Professor, Biochemistry & Biophysics
2008-present Associate Professor, Biochemistry & Biophysics

Honors

1985-1986 McMaster University Senate Scholarship
1989-1993 McMaster University Centennial Fellowship
1989-1993 National Cancer Institute of Canada, Steve Fonyo Studentship
1994-1997 Canadian Medical Research Council Research Fellowship
1997-1999 Canadian Medical Research Council Centennial Fellowship
2002-2006 Pew Scholar in the Biomedical Sciences

ABSTRACT

Barbara Panning was born in Switzerland, one of three children. The family moved to Toronto, Canada, when Barbara was a young child, but Barbara continued to summer in Switzerland. She had an idyllic childhood and always loved science. Panning attended McMaster University, majoring in biology and anthropology. Her senior project involved work on Roberts syndrome in Darrell Tomkins's lab. She continued at McMaster for her PhD, advised by James Smiley, working on herpes virus and adenovirus and publishing a number of papers. Panning began work on X-inactivation in Rudolf Jaenisch's lab at the Whitehead Institute for Biomedical Research at Massachusetts Institute of Technology (MIT), moving to Philip Sharp's lab at MIT to complete her postdoctoral work. She also married during this time.

Panning accepted an assistant professorship at the University of California, San Francisco, where the complexity of X-inactivation continues to intrigue her: X chromosomes seem to talk to each other so that they know how to silence one of them; people don't end up with zero or two X chromosomes. How does this happen? When? How does an X chromosome get silenced in the proper twelve-hour time frame? How does DNA get packaged into chromatin for regulation of gene expression? These are the questions with which Panning still wrestles.

Panning discusses funding in general and her funding, specifically the Pew Scholars award; the necessity for publishing in top-tier journals versus public-access sites; teaching and other administrative duties and their effect on her lab time; balancing family life with work; lab management and the composition of her lab; and women in science and accommodations for children. She laments the decline in funding for science and the general science illiteracy, ruminating on the possibility of a science "ambassador." In addition to long hours in the lab, Panning is involved in outreach to minority students and helps at her child's school.

INTERVIEWER

Hilary Domush was a Program Associate in the Center for Oral History at CHF from 2007-2015. Previously, she earned a BS in chemistry from Bates College in Lewiston, Maine in 2003. She then completed an MS in chemistry and an MA in history of science both from the University of Wisconsin-Madison. Her graduate work in the history of science focused on early nineteenth-century chemistry in the city of Edinburgh, while her work in the chemistry was in a total synthesis laboratory. At CHF, she worked on projects such as the Pew Biomedical Scholars, Women in Chemistry, Atmospheric Science, and Catalysis.

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INTERVIEWER: **Hilary Domush**

LOCATION: **University of California, San Francisco**
San Francisco, California

DATE: **27 August 2008**

DOMUSH: Today is 27 August. I'm Hilary Domush, and I'm here with Dr. Barbara Panning at UCSF [University of California, San Francisco]. Did I say your last name correctly?

PANNING: That's correct.

DOMUSH: Okay. Now, I read in a UCSF publication that you were born in Switzerland. Is that correct? Did you also grow up there?

PANNING: My family stayed there until I was three and then we moved to Canada. But I went back to Switzerland every summer while I was growing up, so I grew up in both countries.

DOMUSH: Did you have family that you were going back to in Switzerland?

PANNING: Visiting my grandparents in the Alps. So they, they ran a small hotel and it was Heidi-esque.

DOMUSH: Did you have siblings as well that were traveling back and forth with you?

PANNING: So, [it is] my brother and sister and I. I was ten years old the first time my parents put us on an airplane by ourselves, I had a six-year old sister and an eight-year old brother, and we got picked up at the other end in Switzerland. We did change in Montreal, [Canada], because it was Toronto, [Canada] to Montreal to change in Montreal by ourselves. A little tag around my neck that said, "My name is Barbara. I speak English and German. Please make sure I get on this flight."

DOMUSH: It went okay. You got on the correct flight?

PANNING: We got on the correct flight, yeah. My parents worried a bit because a ten-year-old, a six-year-old, and an eight-year-old traveling by themselves internationally was a little scary, but there was no problem. We did just fine. And then after, that, we did just fine every year on our own.

DOMUSH: So, you were speaking German when you were in Switzerland?

PANNING: German. They talk Deutsch-Romansh, which is the fourth national language and it's only spoken by people high up in the Alps and it's fairly rare.

DOMUSH: Did you speak that in Canada as well?

PANNING: Mainly English in Canada. My parents . . . my father needed to learn English when we arrived in Canada, so we started speaking English around the home.

DOMUSH: Did you move to Canada because of a work situation?

PANNING: Yeah, my mother's parents hated my dad and my dad's parents didn't like my mother, so they decided to move as far away as they possibly could. [laughter] I'm exaggerating. There were other [issues]. The cost of living in Switzerland was very high, and it was clear my parents would never be able to own a home with just one working parent. We had one set of relatives in Canada that told us the situation, and told them the situation was different in Canada, so they decided to move to Canada.

DOMUSH: How was it when you were in Canada? You said you moved when you were very young, so the transition must not have been [too difficult].

PANNING: Yeah. I was three. It wasn't, it wasn't. I don't remember being traumatized by it or upset by it or anything like that. And we grew up in suburban Toronto and had a house. It was a nice neighborhood. There were lots of . . . it was a recently built subdivision. Every other family in the neighborhood had children our ages. You know, after school my parents would just open the door and let us go out and play and pick us up, call for us a couple hours later knowing that there was someone watching us, no matter where we were, we were going to be close by, some parent would be watching us. So, it was a pretty idyllic childhood. I now have a six-year old and a three-year old, and it's a different world. I have to arrange play dates.

DOMUSH: So, when you were after school kind of running around with your siblings and all these kids in the neighborhood . . . some of the other Pew Scholars [Program in the Biomedical Sciences] that I've talked to have talked about being very interested in the outdoors and that being kind of their first exposure to science. Was that your experience?

PANNING: I have a very clear recollection of my friend, Ken Ball's fifth birthday party. And in the goodie bag that they took home . . . so, he was five. I was five. In the goodie bag we were given to take home and—maybe he was six—we were given these little puzzle cubes. So, you moved the little squares inside a big square with one square missing and you had to move the squares around to recreate a picture or a number sequence. And I remember I was fascinated by it. I sat down and I did it. I would do it over and over again. And that was sort of the first time that it hit me that I really liked figuring things out. I liked these challenges, and I liked figuring these things out. I wanted more of those, and I wanted puzzles and stuff like that, so that was one-half of it.

And then the other half of it is I remember being teased in public school. I was the nature nut. Our neighborhood was built close to farmers' fields and a creek bed that had a forest all around it, and some friends and I would go into the forest during the summer. That would be, we'd disappear in the morning and wander around and come out for lunch and go back in and disappear the afternoon. We'd explore. And I, yeah, I'd bring stuff home. And I don't know how many frogs my mother had to get me to bring back—tadpoles and frogs and things that were glittery and beautiful but hatched into millions of little worms.

DOMUSH: Were your siblings also interested in bringing these nature things back home?

PANNING: Not really. My brother has a learning disability and my sister was just a completely different person. She was a ballerina-artist kind of person. And I'm sure my parents inadvertently or purposefully directed us in different ways.

DOMUSH: So were your parents encouraging of you being outside—not necessarily bringing these things back home.

PANNING: I think they were. Yeah. I don't ever recall my parents trying to guide me in anything. In high school you choose your classes. I always chose my own courses. I was never…I never had a curfew, I think because I never needed one because I had common sense to come home at particular times. So I don't, there wasn't much guidance sort of in any aspect from my parents. They just let us be who we wanted to be and do what we wanted to do. They

tried with my brother to give more guidance because of his learning disability, and I don't think that worked in the end anyway, but . . .

DOMUSH: So, once you were kind of moving along in school and choosing your own courses in high school, were the science courses the ones that were more interesting or were there other subjects that were?

PANNING: The science courses were the ones I gravitated towards. They were the most interesting. I took every science course I could. There were two tracks—an advanced track, and a non-advanced track. So, I took all the maths, all the sciences, lots of languages. I graduated with way more credits in the fast track—upper courses. In fact, the only thing I didn't get was a couple of arts courses and phys-ed. So, it wasn't…you know, when you could have taken four courses, I took eight. [laughter] It's just because I was interested, and I wanted to learn it all and, it was worth it to me.

DOMUSH: Okay. So now, the whole time you're going through high school and things like that are you continuing to go back to Switzerland in the summers?

PANNING: I was going back to Switzerland in the summers, yes, throughout high school until my last year of high school when I stayed and got a job in downtown Toronto […] being a file clerk for an insurance company. And what I learned then was that I hate to commute and that I never want to be a file clerk.

DOMUSH: Good things to learn.

PANNING: Yes. And I learnt them young.

DOMUSH: So, now from having read other Scholars' [oral] histories and spoken with a couple of people, one of whom is also a Canadian or grew up in Canada, I know that applying to college is very different than in the United States and most people [in Canada] stay close to home. So, was McMaster University [Hamilton, Ontario, Canada] close to your home and that's where you went.

PANNING: It was close to home, and at this point my sister was having some kind of recurrent kidney problems, and we just didn't know what the cause was, what was happening. It had gotten to the point where I was typed and I was a match for her. So, I just wanted to be [close] and McMaster University happened to have a medical center where they were doing a lot of

that. So, I wanted to stay close to home to be there just in case something, which was why I chose to do graduate school at McMaster University, as well. As well as, it had a top-tier cancer research group, you know, the best in Canada, and that was absolutely wonderful, as well.

DOMUSH: How far was it from home?

PANNING: Thirty kilometers, quite close to home.

DOMUSH: Okay. Did you live at home?

PANNING: So, I had to put myself through college. The first two years I had to live at home because I just couldn't make enough money to pay tuition and for rent someplace. I saved money so I was able to live away from home for the last two years of college. My parents are old-school Europeans and all their money was saved for my brother's education, so two girls and my brother, so there was no money for my education. I had to do it myself. And then my brother eventually didn't go to college. So, my sister got to…

DOMUSH: When you'd already put yourself through college at that point.

PANNING: I'd already put myself through college. [laughter] There wasn't anything much they could do for me. And by that point, I was in graduate school and I was getting paid. And they didn't…I mean, they didn't object to the point. They didn't understand why I wasn't getting married and having children. I just seemed to be staying in school forever. And every visit home all through graduate school and the postdoctoral years was, "When are you going to get a real job?" I mean, this was the recurring theme. They didn't understand it. They just didn't understand it, didn't know why I was doing it, didn't get the point of it. And they wouldn't stop me, and if I needed help—financial help or something like that—they were certainly happy enough to provide it, but they just couldn't see why I was doing this. There was no purpose to it.

DOMUSH: Did they not understand what it was about science or just not understand why school was taking [so long]?

PANNING: They couldn't understand why school was taking so long and then after I graduated with a PhD, I had to go on and do, it still wasn't done.

DOMUSH: More school.

PANNING: I still had to go into more school. And when I was going to start my real life like the important things like bringing home grandchildren, the things that mattered more to them I think. [laughter]

DOMUSH: Okay. So, when you started at McMaster you're commuting from home. And were there other people in your neighborhood that were also going to McMaster?

PANNING: A group of people that I graduated with all, and that were brothers and sisters that all ended up going to McMaster. So, it was a group of about ten of us that all ended up going there. So, we had rotating [carpool], the ten of us and three cars. It was like that. And we worked out a schedule to get us back and forth.

DOMUSH: So, how…I'm not very familiar with the university system there, but do you start taking…did you have to pick a major when you applied . . . ?

PANNING: Right, no, so you started at general [. . .] program where you choose first year science programs, and one non-science elective. And then after that you begin to focus in on what you wanted to do. I was already pretty clear I wanted to do something biology- or chemistry-related going into high school.

DOMUSH: Oh, already going into high school you knew that?

PANNING: Sorry, going into college. I already knew in high school. In high school it had become very clear that I liked . . . we had one very talented teacher who taught some basic molecular biology at the high school—genetics and molecular biology. I loved it, and that's what I wanted to do. I think in my high school yearbook I had future career it was genetic engineer. So, I knew already then that I wanted to do the biology track. I took a basic first-year sciences mix, and then I think as my non-science elective I took anthropology. I ended up getting a double degree in anthropology and biology. There's a considerable overlap in physical anthropology. We use a lot of biological tools.

DOMUSH: Had you had any exposure to anthropology before going in to university?

PANNING: No, other than reading, you know, Heinrich Schliemann [about the ancient city of] Troy and sort of liking the idea of archeology. No, not really. The archeology courses involved

senior thesis project in a dig in the far north [of] Ontario, close to the Arctic Circle. In that dig I learned I don't think pottery is that interesting. [laughter] Digging up little shards of pottery in the freezing cold. So, it wasn't that cold. I mean it was warm enough that we could do the dig, but it got really cold at night. I learned this wasn't for me.

So that was when I pretty quickly switched over to the molecular biology. And I was getting that, so in the first year I took general course. The second year you could try taking mainly biology courses and chemistry courses., and by third and fourth year you're in…even within biology you're tracked either in ecology or molecular biology. And I was clearly going, that was what I was interested in. And I never did anything with some plan in the future of getting a PhD or becoming a professor; I did it all because I thought it was the most interesting thing to do. So, you graduate with a biology degree. In fourth year, you have a chance to do projects, and it was very clear to me that I really liked doing the science. And there were some courses that introduced me to some things that I thought I would really enjoy doing. So, I decided to do a PhD and got into the PhD program and just kept going. Again, every step because I found something interesting that I really wanted to do, not because I had some dream I was following.

DOMUSH: So, what were the classes like that kept you so interested that really helped you kind of realize, you know, even if I don't have a plan per se that this is something I want to stick with?

PANNING: Developmental biology courses, cell biology, molecular biology . . . basically all the kind of stuff that I am continuing to do today, where we'd be introduced to not only having a problem . . . okay, I'll just provide an example. DNA replication. So, a really good teacher broke it down and said you know the DNA's got to copy itself. Well, these are the kind of things it's got to do, including you know twist and writhe and doing it faithfully. So, what intrigued me was not only the process of figuring out all, you know, actually doing the experiments and the elegant experiments that were done to figure out how you get rid of twist and writhe and what do you replicate, but also, understanding that these are the problems that these biological machines face. So there were many levels that I just found really intriguing. Identifying the problem, you know, really figuring out what was interesting about it, how could you solve that problem? I think in high school I was also in "The Thinkable," which I enjoyed a lot too, which was this problem-solving Olympics that I thought was great. And none of the problems were scientific. They were all sort of socioeconomic problems, but it was…

DOMUSH: Still problem solving. So, when you were taking these really interesting courses and really getting into this idea of biological problem solving, was there also a lab component?

PANNING: There was a lab component as well, and I did well in that, so that was, I knew that that wouldn't be a problem. I enjoyed it.

DOMUSH: Did you do any additional laboratory work outside of the laboratory courses?

PANNING: Right, so there was in the second and third year, there were laboratory courses. And in fourth year, you could join the lab and do a project.

DOMUSH: Okay. So, what, how did you go about joining a lab or was it . . . since it was part of the program was it kind of picked for you?

PANNING: No . . . it was a, I think it was a course if I remember correctly. You interviewed. You asked around, found a lab that had room for you and then you worked a few hours a day in the lab and wrote a senior thesis on the work in the lab.

DOMUSH: So, what did you work on?

PANNING: I worked with a woman named [Darrell J. Tomkins] . . . last name escapes me right now. I could Pub-Med it and get it for you. She was studying Roberts syndrome, which is a syndrome—a human growth syndrome. So, the babies were born small and never really grew particularly—didn't grow normally—and in the end died prematurely with all sorts of growth defects. It was autosomal recessive, and the interesting thing from Darrell's perspective . . . so, she was a cytogeneticist [and] there was this very striking genetic abnormality of the chromosomes where they clearly weren't condensed. Part of the chromosomes were not properly condensed . . . blocks of heterochromatin which are not only really, really condensed were not condensed and the chromosomes had a very abnormal morphology. And the slow growth of the individuals was also mimicked by really slow growth of the cells in culture. So, the kind of research that was done in that lab was growing a wild type, and mutant cells, fibroblast with different drugs to see how it affected the growth of the chromosomes. In the end, I didn't think the research was all that interesting because it was just throwing drugs at some cells and it wasn't all that exciting. But in that year, I was introduced to stuff that I thought was really exciting. And that was what I set out to do my PhD on.

DOMUSH: So, how did that laboratory experience compare to the laboratory experiences you'd had in the courses?

PANNING: How did they compare?

DOMUSH: Were they . . . were there techniques that you learned in the courses that you could now use in the research?

PANNING: No. I had to learn all sorts of new stuff that I had never done before in that laboratory experience. The courses actually turned out to be . . . the developmental biology was removing frog's eyes from their eye sockets to their sides where the optical lobe was before it formed and getting a frog's eye to develop in the side. The molecular biology and cell biology courses were more pouring gels and separating proteins and none of that was, didn't do any of that in this lab. That was the kind of stuff, so I didn't . . . . I enjoyed the molecular or I enjoyed the labs and I did really well in the labs, and I got all the techniques to work, whereas the rest of the class was usually not doing so well. So that was, I knew I had the hands for it, and I could follow the instructions, and I could figure this stuff out. But I had to learn a whole new skill set for my fourth-year project lab, and then a whole new skill set when I started graduate school.

DOMUSH: So, in the fourth year project lab were you learning from . . . did that lab have graduate students?

PANNING: That lab had graduate students and postdocs, and I was learning predominantly from a graduate student.

DOMUSH: Okay. And was there something . . . was it just that, you know, you really liked research and just kept being really interested in the different aspects of biology, or was there something also appealing about…was there a camaraderie in her research group that was appealing in any way?

PANNING: I guess so. I don't . . . the people were real nice. They weren't particularly driven. They weren't highly motivated by . . . you know, that research experience was told me that that wasn't the lab I wanted to be in and that wasn't the kind of research I wanted to be doing. But I found . . . . I talked to other people in different labs and in the end it was the guy whose course I had taken. He really blew me away with how he explained things, the kind of problems he was trying to solve in his lab, and that's where I went for graduate school, and it was wonderful.

There in the graduate lab it was a lot of grad students and postdocs and a lot of camaraderie; and it was the cancer research group that was funded by the Canadian NCI [National Cancer Institute of Canada] at the time. There were eight lab heads in a group and each one had a lab of five or six people, and we were all together, so there was like this large group that worked, all of us working on DNA tumor viruses. So, it was a commonality I guess— common tools and skill sets—and the problems were all kind of related, even if we were working on different viruses, the same ideas were coming up. So, it was wonderful. It was absolutely fantastic. I loved it. And it was a great place for training. My PhD advisor really

wanted to, he challenged us. He would come in and talk about things he'd read that he thought were interesting, and there was always some lively discussion going on, and it was wonderful.

DOMUSH: And now your PhD advisor was James [R.] Smiley.

PANNING: [Yes].

DOMUSH: So, the course that you took, okay, is that was when you first started the graduate program?

PANNING: No, the course that I took, so graduate programs are different in Canada. In Canada, you have to have organized your graduate supervisor before you even start graduate school, so I applied to the graduate program with the intention of going to his lab. And once I got there I could have switched labs, but it's very different from what is done in the States where you come in. You do a year of coursework, and then you decide on a lab. You do some rotations and decide on a lab. There you have to be accepted into a lab before you can get into the program.

DOMUSH: So, had you taken his class then as an undergraduate?

PANNING: I had taken his class as an undergraduate, and I'd known people who had done projects, their fourth-year projects in his lab and really liked him. And I think he got . . . the year I started graduate school three other people went to his lab. [. . .] He's a wonderful guy. And then you take coursework. So, we still do coursework. We just, it was two years of coursework while you're doing graduate school and teaching…in my case in the nursing school and in the medical school. So, for three hours a day, twice a week I was teaching. It was a pretty demanding graduate program. A lot of teaching . . .

DOMUSH: What course were you teaching?

PANNING: Usually physiology or microbiology, immunology for nurses, so it involved two three-hour sessions, two days a week. And it was a semester system, two or three semesters. So I was gone. And then I had to prepare for it. I didn't know about physiology, so I needed to really bone up on that. And then I was taking my own course load which was also, probably two three-hour sessions a week and then working in the lab. Then the coursework was for two years, the graduate work. The coursework was for two years. The teaching was for all six years of graduate school. Here at UCSF the students teach one semester.

DOMUSH: Oh, wow.

PANNING: Yeah, I did teaching for all . . . that teaching load for all six years of graduate school. After two years of graduate school, you do a qualifying exam that lets [you] transfer to a PhD program. And when you're two years into your PhD program you do your comprehensive exams. And your comprehensive exams involved three fifteen-page essays and one on a medical science project, one on a cell biology project and one on a molecular biology project. Then you take three months off and write the three essays and then have a day-long oral exam with committee members that you have chosen and worked with them writing the essays. The year after, the class after mine, it was cut down to two exams. And two years after that it was cut down to one exam. [laughter] And I think two years before I had started they had gotten rid of [the] language. And you used to have to also demonstrate proficiency in another language.

DOMUSH: Oh, wow. Do you think they were cutting things down because professors were tired of grading?

PANNING: I think they were cutting things down because it was just interfering with the research. So, the main thing you were supposed to be doing is learning how to do research, and you're spending all this time teaching and in courses. I mean effectively my PhD supervisor was paying me for two years when I have to admit I didn't get a hell of a lot done because there was just so much coursework and the teaching load was high. And then in the third year you finally begin to accomplish something because you at least have gotten rid of the coursework, and you're a little more familiar with the teaching. If you get the same course a few years in a row it wasn't quite so onerous by the third time you'd done it.

DOMUSH: So, tell me a little bit about the project that you worked on, the research project that you worked on in the lab.

PANNING: Okay. So, Jim studied herpes viruses, and herpes viruses fell under the mantle of DNA tumor viruses, and they have a very specific gene expression program. He was interested in how that worked. So, basically, the virus infects the cell and then the first group of genes turn on and they produce gene products that then turn on a second group of genes. And that second group of genes are basically the genes that are necessary for replication of the virus. And then amongst those genes there's replication genes or things that allow the virus to replicate its DNA, copy itself, and then factors that turn on the third group of genes. They're a group of genes that package the virus and allow it to egress. And the very first thing that it needs to do when it gets in before it can start this domino effect is strip the host machinery that's used for transcription away from the host and the machinery that's used for translation away from the host transcripts

and host DNA and get it off the virus. And it packaged [some] proteins right into the cell, right into the virus that move into the cell with the virus that let it do that. And we were trying to understand how those proteins worked.

DOMUSH: When you applied to graduate school and you applied to work in his lab was this a project that you knew you'd be working on or that you proposed in some way as fitting with what he was already doing?

PANNING: So, when you apply . . . right, I knew the general area of what was going on in his lab. He definitely said I think this would be a good project to start with. And then it evolved into something else. So, by the end we were trying to figure out what these viral proteins were doing. And the project evolved into my understanding how the viral…or working on trying to understand how the viral proteins affected the host, not only the, not only stripped things like polymerase, right, that takes the RNA away from the host, but actually factored the way the host DNA was packaged. So, it was, it was really a great learning experience because I came in with some guidance because I just didn't know the field. I didn't know it well enough to have come up with a reasonable project on my own. I hadn't even working, been working on it for my fourth-year project. Someone in the lab had been working on it, and he was a bit further ahead, but it gave me the opportunity to learn and evolve and figure out what I wanted to do in this, in that area. And then, having sort of been bitten by the understanding how DNA is organized and packaged and how that affects how genes are turned on and off, I decided that's what I wanted to do for graduate school…oh sorry, for postdoc. And so, I looked around for a postdoc that would let me do that.

DOMUSH: So, in the lab as your research is evolving, your understanding and excitement about the project [are evolving], what was the interaction like between you and Jim? Was he the type of boss that was kind of constantly looking over your shoulder.

PANNING: No, you were, I worked very independently. He wasn't a micromanager. He didn't tell me what to do. Especially [with] the initial project that he wanted done there he was [. . .] I could always go to him for advice, but that was his project. But in doing that initial project, I made some interesting observations, and he would say well, that's really cool. You could follow up on that if you want, go ahead. And I would. And it was more . . . his guidance was more like, well, that's an interesting problem. Why don't you try and figure that out or sort of suggestions along those lines without any, these are the experiments you need to do this. So, I was left on my own to figure out how to do it, gather the tools, learn the new techniques. It was great.

DOMUSH: Was there any competition within the lab? Were other people working on the same project?

PANNING: There were other people [who] were working on the same project. There was no competition in the lab, and I had no competition in the world because no one had made this observation, okay. So, I wasn't particularly worried. And I still remember going to my very first scientific meeting which was the Thirteenth International Congress in Herpes Virology in Irvine, California, with Jim and most of the other people in the lab, and Jim presenting all my new observations as part of a talk. And then I had poster at the meeting. And this person who was the . . . had a horrible reputation in the field of . . . . I didn't know it at the time but, it turns out after the fact because I was young and naïve and knew nothing about anyone in the field, personally, right…had a horrible reputation of quickly and dirtily going after other people's experiments if he thought they were interesting. So, he hung out at my poster and scooped me. Published it within and I don't think there's anyone who could have had the observation that I had. And I don't think, I really to this day think that he rushed it out. First of all he got it wrong. It was incorrect. And secondly, it was just, it just was not pretty, I mean it wasn't a good job. And he didn't publish in a great journal, but he rushed it out. So, it made it harder for me to publish my story that I had to correct [and] do like a million more controls and figure, and rebut, point out how wrong he was, wrong to the point where he identified this protein of the virus being necessary for this function. And the virus he used wasn't even a mutant in that protein. And that was well known, you know. So, it was an example of poor, shoddy, refereeing for his initial manuscript, like his and then my having to deal with it all. So, I learnt some, I certainly learnt some stuff from that.

DOMUSH: So, was this the first paper that you were going to write then?

PANNING: It was the…so, I already had written the paper, my first paper.[1] And this was sort of this was the stuff that was mine that the observations I had made that I was going to follow up on.

DOMUSH: So, did you . . . you said you had to do a rebuttal. Was that just in response to kind of his publication or to the reviews that you got?

PANNING: Well, so basically, when I was ready to put my story together, I had to take into account his was already there and mine said something completely different. So, in writing mine I had to basically make it clear that our results were different, why I thought they were different, and why I thought mine was interesting enough to publish given that . . .

[1] B. Panning and J.R. Smiley, "Regulation of cellular genes transduced by herpes simplex virus," *Journal of Virology*, **63** (1989):1929-1937.

DOMUSH: Given that he had published already.

PANNING: Yeah, has published already. So, I learnt some things about . . . it's good to know the community. Yeah. I learnt some stuff that meeting, that I hope has stayed with me. [laughter] I don't know if it always has, but I hope it stayed with me.

DOMUSH: But you were able to publish that.

PANNING: And I was able to publish in the end. In the end because he was so wrong, I mean, the primary observation was . . . so, we noticed that the virus does this. And then we tried to identify the protein in the virus that's responsible for the activity. So, I had presented, or Jim had presented at the meeting, that the virus does this. So, everyone at the meeting already knew that Jim's lab had done it. And it's a big meeting and the entire community was there, so most people would . . . anyone who needed to know would. [. . .] But then he tried all these different mutants and identified one particular protein that was doing it. But he was wrong. So, I was able to publish it and say this is the one that does do it. So, it was okay. I mean, I learnt that yeah, and I learnt that there's crappy science because, I mean, he didn't do a particularly good job. He didn't control things. And I learnt that people will step on you to get where they want to go, and that there are people like that out there and hopefully to identify them and stay away from them. It can be hard to identify them. If you're relying on word of mouth, when you're setting up a collaboration, you know, you can't . . . and I actually did get burnt quite badly early in my career by someone who turned out to be really quite horrible. But, yeah, for the most part . . .

DOMUSH: When this other group came out with this publication, did Jim say anything like, "Oh, that's why he was hanging around your poster so long"? [laughter]

PANNING: Jim was furious. But then he was, he said, "You know, I'm not surprised that this guy would do that. He is that kind of a guy. And I should maybe have not presented your unpublished results." So, Jim presented part of it as part of his talk in a keynote address at the meeting, and then there was the poster.

DOMUSH: Did he know . . . he knew though that you were presenting the poster.

PANNING: He knew that it was going to be in the poster, yeah. He had discussed it what would go on the poster. So, yeah, but it wasn't devastating. It wasn't awful. Jim was really supportive. When I was in graduate school I was blissfully unaware of publish or perish. Or even that it mattered what journal we published in. I was just oblivious to all that. All I was working on was trying to get these experiments done and solve this problem, and I just really

focused on that. And it didn't occur to me that any of this would have any impact on your career somewhere down the line. All I wanted to do was get this step done, and in the end I did quite well at graduate school. In Canada in this particular area, the best you can publish in usually is *Journal of Virology,* and *Molecular Cellular Biology,* and stuff like that. And that's where I published. And I did fine. And I got lots of papers in graduate school.

DOMUSH: Now where were you able to publish this paper?

PANNING: This stuff, I think it went to a journal in general virology. I actually can't remember, probably the *Journal of General Virology,* which is one of the lower tier, not the top-tier virology journal.

DOMUSH: Okay. But it did still get published?

PANNING: But, it did still get published. Yeah. I don't know if I could have gotten it published if this guy had been right, in which . . . but I got it published. And when this guy's came out, I rapidly shifted focus to basically this unusual change in the way the cell responded to the virus. It turned out that every DNA tumor virus did it, so I jumped onto a different tumor virus that I was able to really figure out what was going on pretty quickly there.

DOMUSH: So you moved away from herpes?

PANNING: So, I moved away from herpes. I still finished up the herpes project, but my focus kind of switched away from herpes to this adenovirus where these other oncogenic proteins were doing the same thing that herpes virus proteins were doing. So, it was related, but it was a different system that I could use to figure it out and it was different enough that I was able to publish it without having to worry about this other guy's work. And then sort of the herpes stuff wasn't high priority, so I think it was even two years after his paper came out that we got out. So, it just wasn't my number one priority at that point.

DOMUSH: Was switching viruses difficult? I mean is that, are the techniques from virus to virus, at least within tumor viruses, [similar]?

PANNING: Bit different, a little bit. So, if you were to ask, what do I think part of what makes a good scientist? It's a fearlessness about technique. I want to understand this problem, so I'm going to figure out what I need to do it. And, you know, if it had been a lethal virus, I would have figured out how to work with it so that I could solve the problem or probably I would have

decided maybe that wasn't worth it. [laughter] You just figure it out. You read. You talk to people who have done similar things. You figure it all out, so . . .

DOMUSH: Okay. So, how, at what point in graduate school did you decide "I think I'm ready to move on"? Or, did Jim come and say to you, you know, you're getting ready to move on now?

PANNING: Jim certainly didn't tell me. Other people in my graduate committee told me I was ready to move on. I think PhD supervisors very rarely tell their students they need to move on because they're really productive and they don't particularly want to see them go. [laughter] So Jim certainly didn't fight it when I said it's time. I think I had this realization when Jim and I had a discussion about how we could interpret a particular experiment, and I turned out to be right. And it was then that I realized that I may not be as smart as he is because he's like the smartest person I know in the whole wide world, he's just so incredible, but I'm smart enough that I'll get by, and that I know more about this particular area than he does. And that makes sense. He's juggling ten projects in the lab and I've just got my own little project, so that's when I realized okay, I can handle one thing; maybe I can do three things, and it's time to move on.

DOMUSH: Okay. So, you said that you had stayed at McMaster for both undergrad and graduate school because your sister was having a recurring kidney problem. But you moved away?

PANNING: Right. So, that was resolved. So, her kidney problem…they figured out what it was. There was no reason to hang around anymore.

DOMUSH: And you didn't have to do anything with being a match?

PANNING: I didn't have to. In the end it didn't matter. It didn't. Correct.

DOMUSH: So, then how did you decide . . . did you apply to postdoctoral groups in Canada and in the United States?

PANNING: No. So, the advice I had gotten from the people at McMaster University was go to the States, or Europe, and then it'll be easier for you to get a job back in Canada, so I decided I was going to use my postdoc application thing as a chance to do a world tour. So I decided this is my opportunity to go live someplace different anyway, so I chose labs that were doing work that I was really interested in but that were in different parts of the world. So, I went to London,

[England] and I interviewed in London and Cambridge, [England]. I interviewed in San Diego, [California] and LA [Los Angeles, California] and Boston, [Massachusetts]. In fact, I had six interviews and seven postdoc offers because when I was flying back from Boston to Hamilton, [Canada] after my interview at Boston with Rudolf [Jaenisch]. And I knew I was late getting to the airport. I knew I was going to be late and I was sitting on the "T" and I didn't know what time it was because I never wear a watch, and I wanted to know if I even had a chance of making my flight, so I looked around, and there was this guy sitting two seats down with a white cooler, you know, [marked] "biohazard," that, you know, very, very clear. So I went and sat down beside him, and I said, "You're a scientist. I can trust you. Can you please tell the time?" And he looks at me and goes, "How did you know I'm a scientist?" [laughter] And just like, and we're laughing. So, it turns out he was Nathaniel Heintz [Pew Scholar class of 1985], and he was at Rockefeller [University] and offered me a postdoc by the time we got to the airport. So that was pretty good.

DOMUSH: I don't think most people in the "T" choose to go sit next to the person carrying a "biohazard" box.

PANNING: Maybe. In Boston it's different because there's just so many [scientists]. I remember it was a pretty exciting place to be because I could be sitting on the "T" listening in on conversations and you could find out what's going on in other labs just by listening to what was going on. It was pretty interesting that way.

DOMUSH: So, what made you choose then to go to MIT [Massachusetts Institute of Technology] and go to Boston as opposed to San Diego, or LA, or London, and Cambridge?

PANNING: I liked the research the best. The problem intrigued me the most in the end, yeah. I think that was pretty much it. And I really liked it the best. The other labs were great. There was nothing wrong with them, but I really liked what was going on in Rudolf's lab at the time, the best. They were doing…you know, knockout mice were just coming in to be and Rudolf was really involved in sort of epigenetics which I found most interesting—understanding how DNA is packaged and how the packaging of the DNA affects its expression. The problem he was beginning to work on his lab at the time that I arrived was X-inactivation—the silencing of one chromosome. I just thought the whole thing was really fascinating, and that's where I decided to go.

DOMUSH: Was there anything . . . . I mean as you just said, you know, you're on the "T" and you're running into people carrying biohazard boxes that are very clearly scientists and overhearing these other scientific discussions. I mean, did that play into the decision at all that Boston has this . . . ?

PANNING: I knew that there was a lot there. I'd also been getting advice from people [. . .] at McMaster who had been in different parts of the world. And so, I knew that Boston had this vital science environment and it was, obviously my first impression was—are you familiar with Boston?

DOMUSH: A little bit.

PANNING: A little bit, okay. So, my first . . . . I arrived the night before my interview and I took the "T" to Central Square, which was not the nicest environment. This is a person coming from Canada. The streets are clean. It's safe. And I come up in Central Square and the first thing I hear is, "He's got a gun. Duck." [laughter] But I, you know managed to figure out to get to the bus that I needed to get to my hotel or walk to the hotel which was like a mile walk from Central Square. But then I checked into my hotel and I was right, I was close to, the hotel turned out to be relatively close to Harvard Square. I walked to Harvard Square and it was just an exciting city. It was clear it was an exciting city and would be fun to live in and, yeah. In the end, I lived in Central Square and I wasn't scared by it at all. [laughter] But, that first day was certainly, but it was a wonderful, young, exciting place to be.

DOMUSH: I'm not familiar at all with Hamilton, Ontario. How big is that? I know how big Boston is and so I can kind of picture what it was like, but what was that transition like?

PANNING: I can't, it was intimidating moving to . . . moving by myself to a new city, but I don't remember being intimidated by the size or anything like that. Just the thought of…I always lived close to home. I'd always had groups of people that I knew embarking sort of on the next stage. So, the people from high school went to the same university, even though we took completely different courses. A group of people in my fourth year class stayed on and did graduate school even if we ended up in different labs. So, there was always other people experiencing the same thing, that I knew. And, no one from my graduate class went to Boston for a postdoc, I was the only one. I was the first. After that, people would come and stay with me and get postdocs, but I was kind of the first one to go do that. So, it was scary and it was intimidating, and yeah, I had anxiety about it before I left, but, you know, you got to go. I did it. I sort of made the transition a little bit easier for myself by going for a month and then coming . . . . I think I must have arrived at U.S., just before U.S. Thanksgiving staying until Christmas and coming home for Christmas. And staying an extra three weeks at Christmas and writing a review, a paper with my PhD supervisor and then going back to Boston for three months that time and then coming back and defending my thesis and then going back to Boston. Then I stayed.

DOMUSH: So, you eased in a little bit? [knocking]

PANNING: Just one second.

DOMUSH: All right, so we're back. And we were talking . . .

PANNING: About starting, was I intimidated or upset or scared about . . .

DOMUSH: About moving to Boston.

PANNING: Transition. And it was intimidating, but I did fine. So, when I first arrived at Boston, I realized that I wasn't going to be able to find or, I went to find a place to live two months before I actually arrived. And then realized I wasn't going to be able to find anything that I could afford because I was going to be there on a Canadian postdoc salary because I had Canadian fellowship that translated to actually less than graduate students were earning at MIT.

So, I decided to move into graduate housing and get the lay of the land and figure out where I wanted to live and what . . . if I could find a roommate situation that I'd be happy with. So, I ended up being the third person in a room with two other graduate students in a three-bedroom apartment, two other graduate students. And one of my distinct memories at MIT is moving in on a Saturday when they were playing "Star Trek, Next Generation" or something like that. I'm walking down this long dormitory-like hallway with "Star Trek, Next Generation" blaring from everywhere. I mean, it was just, and then as I'm moving in and putting my stuff in my room, they switched it over to some local MIT channel where they deconstructed the episode of *Star Trek*. It was like an actual, you know, *Star Trek* TV where they discussed it in great earnestness. Yeah. This place is like as geeky as it's reputed to be.

And at this point, I was semi-goth. I had toned down goth because they'd advised me not to be quite so scary looking when I was looking for postdocs as a graduate student. And I'd spend my evenings—evenings that I wasn't spending in the lab—I would spend at either at slam poetry readings or Manray [Nightclub] which was like the closest goth club to MIT. But yeah, I kind of became more mainstream as [my] postdoc progressed and spent more and more time in the lab. [. . .]

DOMUSH: Were the people that you were meeting in either Rudy's lab at MIT or maybe the surrounding labs were . . . you know, you had always come from the stuff before kind of with a group of people that you knew and now you were going somewhere where you didn't necessarily know anyone. So, were you finding some people to hang out with?

PANNING: Hang out with, yeah. Yeah, absolutely. Certainly, the nice thing about the Whitehead and one of the things that attracted me to it is that there's this large international community, and everyone pretty much who's there has just gone through what I went through. They arrived recently. Now they might be there a year by now, but it's a pile of people who were there transiently from somewhere else in the world. Often some couples came, but a lot of single people came. So, it was pretty easy to hook up with groups of people to do things together.

DOMUSH: Scientifically, what was it like in the lab? I mean, you had said that in your first, in your fourth-year project, the people in the lab were not particularly driven, not necessarily so interested and, you know, you said you didn't want a laboratory experience like that. And I assume that in Jim's group it was different than that.

PANNING: It was different, yeah.

DOMUSH: So, how did Rudy's lab compare?

PANNING: Rudy's lab had some very driven [people], yeah. Rudy's lab when I was there it was I think an absolute great place. For the most part, I think it's continually a great place. Every now and then, he had a personality who was . . . who can mess it up for people. But everyone was motivated, hardworking.

There were multiple areas of research in the lab, but every one area had enough people that you could have . . . so, for instance you had some people that were studying neural crest cells, people studying muscle development and some people studying DNA methylation, and some people studying X-inactivation. Every group, those groups don't have a lot in common, but every group had enough people in it that you could have a discussion, a reasonable discussion about your topic area and then you learned all this interesting stuff about things that you weren't normally thinking about. So, it was, I thought it was a wonderful experience. About thirty people in my lab and it was great because I was exposed to so much stuff and I loved Rudolf. He was a wonderful mentor, a great guy. Left you completely on your own, you had to figure it out on your own. But that was fine with me because I knew I could do that.

DOMUSH: Okay. Was being in a lab with thirty people a little bit overwhelming sometimes? I mean that sounds very large.

PANNING: It never occurred to me to be overwhelmed.

DOMUSH: It was just what it was.

PANNING: Just what it was. So, we just, yeah.

DOMUSH: In a lab of thirty people are there still . . . you know, you said people are working on projects that are very different. Are there still group meetings and things?

PANNING: There were still group meetings and that's where you get most of your exposure to all these areas that you're not as familiar with when you hear it at a group meeting. And then you get to talk to them and make friends with people studying neural development and learn a little bit about neural development. It was just this really, really wonderful, enlightening, I just learned so much. And it was fun to be there.

You know, at McMaster University it was a relatively small place. We'd get, there was a weekly seminars series, a couple of seminar series and every once in a while you'd get someone really good. But at MIT, it was a weekly seminar series and every week there was someone great, you know. It was pretty exciting just to hear all these fantastic talks and the science that I thought was amazing. It was just wonderful. It was, yeah, so, I was happy being there and pretty excited about what I was being exposed to, not only locally in the lab, but sort of in a lot of the community.

DOMUSH: Right. Did you have to do any teaching? Because you said all the way through your postdoc or all the way through your graduate career, you had to teach.

PANNING: No teaching. It was great, other than helping grad students in the lab.

DOMUSH: Was that something that you enjoyed, that, you know, teaching technique and things like that?

PANNING: Yeah. Or, yeah, or being the person who kind of mentored a graduate student and helped with their project and defined their project. It was fun.

DOMUSH: Okay. So, now you were a postdoc with Rudy for about three years?

PANNING: About, I think three-and-a-half, four years nearly and then I switched over and did a postdoc with Phil [Phillip A.] Sharp. And that was because I realized everything that I had

done in Rudolf's lab was wonderful, but it made me . . . . I realized I needed to use completely different approaches to get at the heart of the problem that I wanted to solve, and I needed to go to a place that I could learn that.

DOMUSH: And this was because you wanted to get to the heart of the problem of X-inactivation. And so that was what you started in Rudy's lab. And you were going to take it . . . ?

PANNING: With me, right.

DOMUSH: And how did he feel about that?

PANNING: Rudolf was fine with it. Rudolf is, again he's very supportive. He doesn't . . . there's no, I don't know of anyone who ever had a conversation with him saying you can't do this, I can't do that. It's always been fine. And as people generally do really well because I think he doesn't step on their toes or just believing is painless enough that you can have a productive conversation about, you know, these are the things I'm going to do, and these are the things he does and no one really competes.

DOMUSH: Okay, did you have any issues, competition issues about maybe getting scooped or maybe being too open with your own research, as you had had in graduate school?

PANNING: Not with Rudolf. Not in Rudolf's lab. With Rudolf . . . okay, there was one paper I published in Rudolf's lab that Rudolf had presented the results in Britain, a meeting in Britain.[2] And a few months later…and Rudolf's archenemy in X-inactivation, Neil Brockdorff was at the meeting and came up to Rudolf and said we have very similar results. How is, how are things going or do you think your close to submission? And Rudolf said no. And then he came back and said we have to submit it because Brockdorff has similar results.

So within a week I had it submitted, and I was writing it anyway. It wasn't . . . within a week I had submitted it, but it was because we could. It was together enough that we could. So, then a month later I went to a meeting, I gave a talk at a meeting for Rudolf. So, Rudolf decided not to go, so I gave the talk for him. And Brockdorff was there, evil Brockdorff. I might need to edit that later. So, I gave the talk. And Neil, I can remember I gave the talk and an editor from *Cell* came up to me right after the talk, and this is where we had submitted the manuscript. The

[2] B. Panning, J. Dausman, and R. Jaenish, "X chromosome inactivation is mediated by *Xist* RNA stabilization," *Cell*, **90** (1997):907-916.

editor came up and introduced herself to me and Neil, I didn't know who Neil was, what he looked like. He happened to be standing close by. Introduced herself to me and said we just got the manuscript. "We're really excited about it," you know, and "That was a great talk." "Thank you." And before she was like putting her hand out to say this and before she could even make contact with my hand, he physically shoves me aside and stands in front of her going, "Hello, my name is Neil Brockdorff and we have a very similar story. I hope *Cell* will consider it." [laughter]

DOMUSH: Wow.

PANNING: So then he disappears for the rest of the meeting . . . like, completely gone except for when he had to be there for his talk. And of course, we were talking to other people at the meeting, and I happened to be talking to someone who said, "Yeah, I don't have my laptop because Neil is borrowing it to write a manuscript that he has to get out really fast."

So mine was accepted by *Cell* before he actually got his submitted. And *Cell* emailed us and asked whether we would consider delaying publication of our manuscript so that his could come out. And of course we said no, so ours came out. And then, three months later his came out that was essentially exactly the same story, even though he had heard the whole story twice, now; we all had told it, and he had seen me tell it at the meeting.

DOMUSH: Wow.

PANNING: Yeah. So, that was kind of . . . but at least, we didn't get scooped, and they both came out in the same year. It was okay.

DOMUSH: And yours came out first.

PANNING: Ours came out first. Despite that experience, I still would rather talk about unpublished data at a meeting than talk about stuff that's been in press for a year. That's not what those meetings are about. They're supposed to be pushing science forward, and you don't do that by talking about stuff that everyone already knows.

DOMUSH: Right. So when you went to work with Phil Sharp you said that it was because you could learn something in his lab to help you with your study of X-inactivation. What could you do in his lab that you hadn't learned previously?

PANNING: So, some biochemistry. I'd done basically biochemistry. So, in Rudolf's lab I had done developmental genetics, and in Jim's lab I had been doing a lot of virology, but I'd never really isolated a complex metric . . . isolated a group of proteins stuck together in a complex that does something. And that was what Phil's lab had expertise in, so I went there to learn how to purify a protein, and specifically proteins that had an RNA component or complexes that had a RNA complex because that's what his lab specialized in. So, Phil had no interest in studying X-inactivation. He was happy to have me in his lab for a couple of years so I could learn, but there wasn't even any thought of competition after I left because I was clearly coming there to learn this and move on.

DOMUSH: Were there other labs that you considered going to, to learn that?

PANNING: I didn't even seriously look at any other labs because at this point I had met someone in Rudolf's lab and married him, and he had taken a job in a biotech company in the Boston area, so I didn't want to leave Boston at that point.

DOMUSH: Okay. Now, was Phil Sharp also at the Whitehead?

PANNING: He was at MIT. So, the Whitehead is an independent research institute that's affiliated with MIT, but it's basically independently governed and administered. The faculty have cross-appointments at MIT, so they can get MIT graduate students, but it's a separate entity. Phil Sharp is at MIT itself in the biology department. So, the Whitehead was two hundred meters away. The building that the Whitehead was housed in was two hundred meters away from the building.

DOMUSH: Your commute didn't change very much.

PANNING: So, my commute didn't change at all. [laughter] Nothing, I mean, it was a whole new lab, a whole new environment, whole new techniques, whole new groups of people, but it wasn't . . . . I don't consider moving from one biology lab to another biology lab being [terribly difficult].

DOMUSH: Right. How did learning these new techniques go? Did it go smoothly? Did it go . . . ?

PANNING: It went reasonably smoothly, yeah. I learned enough that I needed to, to move on. Yeah.

DOMUSH: And it served its purpose.

PANNING: It served its purpose and Phil was a wonderful man. And really . . . . I learned a lot watching him and learning from him, so that was great, too.

DOMUSH: Were you learning directly from him about the techniques?

PANNING: No. No, not the techniques, more learning about the politics of science.

DOMUSH: Okay. You said that while you were in Rudy's lab you got married. Was he also in Rudy's lab?

PANNING: He was also in Rudy's lab. So, we didn't meet until . . . he's Canadian as well, from a different part of Canada. So, I've got Swiss and Canadian citizenship, but he's Canadian from a different part of Canada, and we probably would have never met except that we ended up postdoc'ing in the same lab. And [we] got married pretty quickly.

DOMUSH: And so, then he moved to a biotech company in the area?

PANNING: He moved to a biotech company in the area. He didn't really want to do research. He didn't want to go that track. It didn't appeal to him. So, he went into biotech, and I continued on. And then, I would say he was in his job for about a year and a half or two years when…probably a year and a half when I started looking for jobs . . . when I started doing an actual job search. And I did a really, really restricted search. Actually, I had a job lined up at Brandeis University in the Boston area. And they told me if you really, seriously want this job you should do . . . you know, you'll get the offer, but you need to do a search to get offers at other places. So, I did a search; I applied to twelve places.

DOMUSH: All in the United States?

PANNING: All in the United States. Twelve places that looked interesting that had . . . what defined the twelve places was the fact that none of this came, I didn't want to do a huge search and knowing that I had to do a search . . . . I found out that I had to do a search in the end of October, and by then most of the searches were closed. So, I didn't have an option of applying

to a hundred places anyway; which thirty, forty, fifty, sixty places is not unusual now. So, I just took twelve that seemed reasonable and went out and got, I think, six or seven interviews and six or seven offers and chose, and then chose UCSF because it was wonderful. I just knew that this was where I thought I wanted to be doing my science.

DOMUSH: When you started looking for jobs, did you have any consideration that maybe you didn't want to go into academic research, maybe you also wanted to be in biotech?

PANNING: No. I knew that I wanted to keep studying X-inactivation. And I had done well enough in grad or in a postdoc that I could get a reasonably good job.

DOMUSH: So, once you found out that you did have to do the search and you applied to these six or seven places and got . . . or got six or seven interviews, how did you decide that UCSF was the best one, especially since you already this offer in Boston where you could have stayed, presumably very easily?

PANNING: The environment. The research environment there reminded me a lot of the environment back in my graduate school, where it was these smaller labs that work really closely together, and I really liked that. That was a wonderful environment that [. . .] and it is like that here. So, that was kind of what made it…decided me. The other places were all really good, too. They wouldn't . . . . I just liked it here the best because of that the intensely collaborative culture.

DOMUSH: Was your husband open to moving to any of these places or did that take some convincing?

PANNING: It took a little convincing, but in the end he . . . there's biotech here, so it worked out.

DOMUSH: Now, when you moved here were you moving into this building?

PANNING: No. I was . . . this building was built . . . we moved in here in 2003, so spring of 2003, and I arrived in winter of 2000. So, it was about three years before.

DOMUSH: Okay. So, were you up at [the UCSF] Parnassus [Campus] then?

PANNING: We were up at Parnassus first, yeah.

DOMUSH: Okay. So, what was it like coming then? I've talked to some other people that say they came and there was nothing. Did you have anything here that you could start with right away, or did you really have to build up everything in your lab from scratch?

PANNING: From here. We took about everything from Parnassus. I mean it really wasn't . . .

DOMUSH: No, no, when you came from Boston.

PANNING: When I came from Boston. Because this is such . . . the way that the labs are organized here, you fit into a group of - everything is done in groups of four labs. So, you fit into a group of four labs, and most of the infrastructure is in place, so any major equipment is usually there. There wasn't . . . it wasn't nothing. It wasn't that I had nothing. I had access to just about everything I needed. I needed to buy a few small pieces of equipment, and then the important part was beginning to build up the lab, was getting people, getting good people to get the research started.

DOMUSH: And how did getting students or postdocs go at the beginning?

PANNING: It was fine. It wasn't difficult. Most of the time students are really drawn to the new labs, so it wasn't hard getting students. Postdocs . . . . I think I made a mistake in the first person I hired as a postdoc, and I had to fire her very quickly. I think when I hired her, I was just [thinking] why, you know why would anyone want to come and work for me? I mean this place where all these absolutely brilliant, well-known scientists; like, I didn't know if anyone would ever actually work for me. I got an application from someone that looked good on paper. I had doubts when I . . . she was from Russia. I had doubts when I interviewed her. I thought, you know, I'll give it a shot and then she got here and she was absolutely dreadful.

DOMUSH: Was it just a personality conflict or was her science dreadful?

PANNING: It was not a personality conflict. She had decided she worked hard enough . . . to summarize it she decided she'd worked hard enough getting her PhD in Russia and was here to take it easy. So, she wasn't . . . and I'm not a slave driver by any means, but she was not willing to work to learn English. So, she couldn't even understand what I was asking her to do half the time and the things that even the simple things I was asking her, she wasn't doing correctly. She

didn't want to take the time to or the energy to learn English well enough to read the protocols, and it was a very frustrating experience. I got her into English as a Second Language programs and did everything I could to help, but it just wasn't . . .

DOMUSH: She wasn't working.

PANNING: She wasn't. So I had to fire her after a year-and-a-half. But it was, she served her purpose in that you know when the graduate students came there was a postdoc and there was a technician. You know, there were the beginnings of a lab, and then graduate students come, and then more graduate students come, and it's okay.

DOMUSH: So, how did it go? I mean you had this not very good experience with your first postdoc, but how did it go to kind of learn how to be the PI?

PANNING: I don't know that I've ever learned that yet. The good people are good no matter what. And if I can provide an environment where they can be themselves and work on something interesting then I think I've done my job. So, that environment defines, of course, the experiments, but, you know, you put into their ideas, discussion, bringing up new approaches, stuff like that. If I can foster that, I think I've done my job. So, I'm never going to be someone that can take an absolutely horrible person and drive them and turn them into someone who's doing a great job, right. I can take someone who's overly good and work with them, to help them be successful, but I'm not, I mean, I honestly don't want to be the kind of person that micromanages or forces or, you know. But if the person I'm working with isn't interested enough to take ownership of their project and develop it on their own then there's not going to be anything I can do for them. There are students that I've had that turned out not to be very talented. I mean people who were really smart that don't have good hands and the kind of thing I've done with them is try different projects until I figure out something that they're good at and they can do well. And then develop a project that they can do over and over again, so that they can get that paper and get their PhD. But I can't say since then, I've had anyone who was like that—who just really had no interest in the science, wasn't motivated at all, didn't want to get anything done, and I'm hoping it's because I know what to look out for now, but I think it's possible I've just been lucky because I've had some really great postdoctoral fellows and graduate students, just stellar.

DOMUSH: How big is your lab?

PANNING: Right now, it's quite small, but I've been downsizing because I have two small children and just don't have the kind of time to spend with people. So, there's five people right now. Yeah. At its peak, it had twelve, which was . . . that's when I started having children. It

was particularly tough when I had one three-year-old and a son one year older, it was really, really tough going.

DOMUSH: So, do you have postdocs and graduate students right now?

PANNING: Yeah.

DOMUSH: Do you also have . . . well, I guess, UCSF doesn't really have undergrads, so no undergrads in your lab.

PANNING: No undergrads, no.

DOMUSH: Okay. When you first got here and you're kind of working with this postdoc or trying to work with this postdoc and getting new graduate students in the lab, did you have the projects laid out for them that they were going to do at the beginning . . . that this is how I want to work on X-inactivation and this is what you're going to do as part of that project?

PANNING: Right. So, my approach has generally been these are the areas we're interested in. These are the kind of experiments that you could do, do a rotation in a lab, and figure out what appeals to you and if that's something you'd like to do. So, the graduate students sort of get a menu of options. They come in and they say, "This is what I'm most interested in, what I'd like to try." They get started on that. Then they do rotation. And then that can turn into a whole project or they can completely switch projects once they start. So, the projects were, it was less a definition of a project, more a definition of a problem. This is what we'd like to try to figure out. This is what we know. What can we do to get closer to understanding how that works? Part of that is because of the complexity of the problems. So, X-inactivation isn't, it's more than just one problem. So, everything is complex, so I don't want to say that what I am doing is more difficult than anyone else's or anything like that, but there's multiple things going on. So, it's like…can I just step back and explain that, is that okay? Is it an appropriate time?

DOMUSH: Yeah.

PANNING: So, females have two X chromosomes and males have one. They silence one to equalize X and the gene dosage between males and females. Females are conceived with two active X chromosomes. You can't, there's essentially genes on the X chromosome. You can't undergo spermatogenesis genesis with inactive X chromosome. It doesn't work. So, you can't inherit . . . it actually turns out oogenesis are haploid half the time. So, you reactivate your

inactive X chromosome and so you have two active X chromosomes in your germ genesis as well. So, you inherit one active X chromosome from your mom, one active X chromosome from your dad. And then early in development, just around the time of implantation you silence one chromosome. So, what's involved in silencing one chromosome? It's not only do silencing. Well, it's that you have to know how many X chromosomes you have. You don't want to silence an X chromosome if you only have one.

DOMUSH: Right.

PANNING: So, you need to know you have more than one, at the very least, there's some system in place that lets you know you have one or more than one. In humans and, actually, in every organism except for a subset, X-inactivation is random. So, 50 percent of these cells, the ones you got from your mom and then the other 50 percent of the cells we saw were the ones you got from your dad. So, superimposed on this system that lets you know how many X chromosomes you have, there has to be another system that allows it to make this random choice so that, right, and the random choice is . . . it's robust. It happens all the time and it's, there's mutual exclusivity. We can't find evidence for cells in which two X chromosomes are inactive in females or two X chromosomes are active. It really looks like it happens all the time robustly. And it happens always so that one X chromosome is the active and the other X chromosome is the inactive.

You don't . . . somehow there seems to be some crosstalk between the two chromosomes so that one knows what the other one is doing. And understanding how that crosstalk could happen within the confines of a nucleus with two pieces of DNA forming apart from each other, that's also an interesting question. The whole thing is developmentally regulated. It's happening at a very specific time. What are the inputs? How do you know when in time to do this process? And once you finally make the decision about which one is going to be active X chromosome and which one's going to be an inactive X chromosome, how do you physically silence five percent of your DNA in what looks to be a twelve-hour window in development? How in the world do you do that? You know these are . . . every one of those questions is itself a pretty interesting problem, and I'm fascinated by all of them. And there are labs working exclusively on every one of them. I've been lucky enough to get groups of people that have wanted to work on more than one of them as well.

So, that's the type . . . when a graduate student comes in, I say these are the big issues within the, if you're interested in understanding randomness this is what our data say is going on. These are some follow-up experiments that could be done. That's kind of how things work. So, with people in the lab understanding randomness, with people in the lab understanding working on projects, understanding how chromatin are regulated, how the DNA is packaged into chromatin and that regulates gene expression.

DOMUSH: So, do people come in then for rotations. You said UCSF students do rotations. So, people come in for rotations and they try one of these projects; then if they decide that they want to work in your lab, do people often switch to some other aspect [of the research]?

PANNING: Normally, normally people by the time they're doing rotations have a pretty good idea of the kind of things that interest them, and I very rarely have anyone that switched to another kind of project. Usually people fall into, they're either fascinated by this whole randomness, and how is all this going on? Or, they're really interested in the chromatin aspect of it. How is the DNA packaged?

DOMUSH: What do you think of rotations? When you were a graduate student you didn't have rotations and you picked your advisor before you even got into the graduate program. So, from your perspective now as a professor with graduate students, how do you think that works?

PANNING: I think the rotations are an excellent idea only because it gives you an opportunity to get to know your PhD supervisor before you actually commit to the lab. And there was one person where I did graduate school, she was utterly a charming woman, absolutely wonderful to people who weren't in her lab but horrible to people who were in her lab, just awful, and no one ever got out of there with a PhD. People left with a master's . . . just disheartened. Great people, who I think could have done really good science, just gave up. All went to medical school, teachers college, they were just, it was a horrible experience. Every year, a new group of people would interview for positions, who interviewed for graduate school, and you come around and you interview people before you commit to a lab. She would be her charming best, and people in the lab would take the interviewers aside and say, "Don't come here. It's hell. Don't do it." And every year, a group of people, there'd be a subset of people that would say, "No, it'll be different for me." [laughter] And it never was. The postdocs were destroyed. The grad students were emotionally awful, but people kept coming because she was so very nice in the interview and so incredibly charming in the interview. And, you know, you'd say, "No one's ever gotten a PhD." They all leave. They're miserable. And a person like that could never function in a system [with rotations] because the students would know. They could still get postdocs because the postdoc you choose on the basis of one interview, but the students would know.

DOMUSH: So, how does it work? You said that researchers here are kind of organized into groups of four, and I know, I mean even your offices are kind of organized into these groups of four. So, do your labs work together, these other three professors near or is it just that your labs are right next to each other? How does that work here?

PANNING: Our labs don't work together. We actually did work very closely with lab for one project. No. You just, you find, again it's where the project takes you, who do you need to talk to? What do you need to do? So, I have collaborations or interactions with labs that aren't on

this floor that are in different buildings, but these are the people we talk to day-to-day and discuss, bounce ideas off of and it's nice. I like my colleagues. They're really good for that.

DOMUSH: Do you have joint group meetings or anything like that for your students?

PANNING: Not with anyone here, but certainly with other groups. I've got, one of my…so, you never know when you're sort of embarking on a discovery. You never know what you're going to find. Luckily there's enough people here that often you'll run into, you'll find something that someone here knows a lot about, so if a student stumbles onto something that is relevant to what another lab studies we now start doing joint groupings with that lab, and that student and I will start doing joint groupings with that lab, and anyone who comes onto that project will start doing joint groupings with that lab.

DOMUSH: So, are you doing that currently—joint group meetings?

PANNING: Yeah.

DOMUSH: With which labs?

PANNING: So, Christine Guthrie's lab, one student is interacting with hers because it turns out there's some splicing regulation that looks interesting, and the student just graduated but we did a joint project on chromatin regulation, and we're starting up some projects on epistasis mapping.

DOMUSH: Do you have any collaborations or any . . . maybe not a formal collaboration but kind of scientific interaction . . . you know, there's such a large community here that's not just UCSF, but [University of California] Berkeley and Stanford [University] are obviously, very close. There's so much biotech in the area. Does that play a role in how your lab works at all?

PANNING: It has on occasion. We worked with people in Berkeley when they've had a technique or skill that we could use in our set of experiments. There's . . . in the end no, surprisingly not. If a student or postdoc in the lab starts up a collaboration, I certainly support it, but there hasn't been anyone here whom we have really done anything with, outside of the people at UCSF. And I don't know why that is.

DOMUSH: I mean, you're certainly not lacking for people here at UCSF.

PANNING: Yeah, exactly. And there's not, the areas that we study which is epigenetics and chromatin structure that it's not huge in the Bay Area. There's not a lot of labs in this area that study this stuff. In the beginning, for dosage compensation . . . there's one other dosage compensation lab here and that's Barbara J. Meyer. She's in Berkeley. But she studies in *C. elegans,* and I certainly benefited from having the other Barbara here because people who are interested in studying dosage compensation in the Bay Area would email both of us for postdocs, and some of them would come here, so that was good.

DOMUSH: So, you said for the specifics of what your lab does, there aren't many other groups in the Bay Area. Are those groups, are there many other groups in general though throughout the . . .

PANNING: There are other, so for instance I have a collaboration with someone in Italy.

DOMUSH: Oh, wow. Does that mean you get to go to Italy?

PANNING: I haven't gone yet. He keeps coming to the States. But I will someday. And I'm a collaborator in Japan because we're more . . . from the actual X-inactivation stuff there are people that are interested in the kind of experiments we're doing that I just work with more closely than people around here, because there's no other X-inactivation lab here. There's stuff that's peripheral in X-inactivation, but not sort of the really, really guts of the problem. No one else here does it.

DOMUSH: So, is the X-inactivation community a more open community then or is it kind of more closed and competitive?

PANNING: It's very competitive. There are some people who are very powerful who I'm not . . . . I don't particularly like or I don't respect would be another way of thinking of it. And there's . . . it can be closed minded I would say is one way of thinking of it. I think its opening up. More young people are joining and it's becoming more open-minded. But there was a while where you were only allowed to think of things sort of one way. These are some, because we don't really have a grip on for instance, how you're doing something, how you're making the random choice, and how you know how many X chromosomes we have. At best, it's models. We don't have molecules. We don't have proteins for DNA sequencing. Some . . . a lot of thinking that goes into the field is theoretical, this is the way it could be done. For instance, many years ago, the founder of the field—Mary Lyon—said, "This is the way I think it's happening." And in the field, you're not allowed to come up with other ideas for the way it

could be happening. It's kind of frowned upon because that's what Mary Lyon said forty years ago. But in fact, there is a favored model that most people cite, but it's not the only one, and it may even turn out to be the wrong one. But it can be extraordinarily difficult to publish something that is not incubated that well.

DOMUSH: Is that something that you've come up against?

PANNING: Yes. It's something that I've come up against, and we figure out how to get around it. It can be, yeah, challenging and frustrating.

DOMUSH: But you got the impression that it is starting to change

PANNING: It's changing. Yeah, absolutely. It's starting to change.

DOMUSH: And is that because there are more young professors?

PANNING: I think more [that] the field is growing. More young people are coming in. I'd say in the last five years there's been . . . where there used to be maybe ten labs studying it, now I think there's thirty labs studying it.

DOMUSH: So, you said that there's kind of the model from forty years ago. I would imagine that there have been a lot of changes in how research is done on X-inactivation in the last forty years. In your own career of, you know, graduate school, and postdoc, and coming here, have you seen any technology breakthroughs or things that you need in your lab today that you can't even imagine working without that didn't really exist when you were . . .

PANNING: Well, I mean, I'm sure you get the same answer to this, PCRs [polymerase chain reaction], microarrays, homologous recombination to target. Those are all things that are mainstays of what we do and it would be difficult to . . . RNA, discovery of RNAi, being able to [do RNA interference in mammalian cells . . . those are all enormous breakthroughs that we couldn't, we need those techniques. We use them every day. So, it's been, yeah. And you have to, every time something new gets discovered you have to figure out whether it's something you can use and incorporate into your research.

DOMUSH: So, does that play into the conversation about . . . maybe we need to rethink this forty-year-old model? You know, there's a whole lot that's gone on since then.

PANNING: There's a whole lot of new tools we can apply to get up the model. I agree that the . . . to get at it you almost, you have to think of . . . we don't even know enough about it to test it. So, the basic model would be…I'll describe the model and then I'll explain it. The idea is that there are two—one active and one inactive—X chromosomes. And then based on analysis of cells with extra X chromosomes . . . humans show up in the fertility clinics with fifty chromosomes, instead of forty-six and the four extra are the X chromosomes. And those are all inactive. So, as long as you're normal…as long as all your autosomes . . . you have a known number of autosomes, you just silence every X chromosome beyond one. So, there are males out there that have four X chromosomes and they have four of them are, three of them are silent and they have one active X chromosome, even though they're males. It has nothing to do with the sex determination. It's about everything to do with measuring what looks to be the relative number of X chromosomes to your autosome contact.

So, the model was the autosomes produced one of something. And that one of something finds one chromosome, and that one chromosome is then designated to be the active X chromosome, and all the others are silent. And to be able to test that model you need to identify the one of something that the autosomes produce and you need to identify the site on the X chromosome that it binds to. And we don't know. We just don't. And so, we have all the tools. If we had any one of those, we could get at it. No one's been able to find this . . . the one site on the chromosome that something binds to, but if you don't know what's binding, you don't even know what the site looks like. So, without the handles to go and get at it we can't tell with that model is what it comes down to, unless you can find some other handles that let you develop a new model that's also consistent with the data. So, it's complicated. And until we get what we call this *cis*-inactive sequence—the DNA element that something's binding or until we get that something that's binding there's no . . . it's still a black box. It really is. This is like one of the ultimate black boxes. We still actually have no idea how it's done. I have some ideas.

DOMUSH: But I can see why this would appeal to the problem-solving nature that you described.

PANNING: Yeah.

DOMUSH: I mean this is very much a complex problem and as you say to put it simply. One of the things that you have to do now that you're a PI is, you know, help your students who are interested become kind of the best researcher that they can be. Do you also do things like help them to write their papers?

PANNING: So, students write their first draft of their paper themselves with . . . we'll discuss what goes in the discussion, and then I go through it and make comments and sit down with

them and explain why I made every comment. And it's a very collaborative process, so that I try to make them understand why changes are being made, what's being accomplished by the changes and, you know, what we're trying to do and why we write this paper. I've been lucky in that most students have been really good at writing, so it hasn't been a difficult process by any means.

DOMUSH: Do students here have a scientific writing course as part of their coursework?

PANNING: I don't think so, no. It's not [. . .] you've gotten to the point where you're writing the result, you've done a series of experiments. You can put them in a linear order that make some kind of sense. And then, the discussion is what, where you put in the context of everything else that's going on in the world. That sometimes needs a little more work. It's not . . . in the end it's not that difficult, maybe. There's stylistic differences; for instance, I hate using "interestingly" in a paper, a primary research publication because I'm just reporting results. I'm not reporting how I feel about it, right?

DOMUSH: Right. Those don't sound like very scientific terms.

PANNING: I'm perfectly comfortable with having those in a review that you're writing of someone's paper, but I just don't feel it's appropriate in a manuscript, and I know my students purposely stick it in there to see if I actually read it. Will I catch them? Will I take that out? [laughter]

DOMUSH: Have you had to do anything with your students . . . other professors that I've talked to have talked about when they get reviews back or reviews that they don't want published . . . teaching their students about writing a rebuttal. Have you had to do that with any of your students?

PANNING: So, with postdocs it's the same exercise, okay? Let's break down these comments. Let's figure out how we write a letter back to get this in or don't get it in. It's the same. And there'll be lots of discussion to mentally work out the final version and, yeah. But it's a very collaborative, I feel like it's a very collaborative effort always. Usually they can think of a couple of exceptions, but generally, the student or the postdoc—and I see a postdoc as a training experience, too—write the first draft. And I invite students and postdocs—whenever I get an invitation to write a review, I always ask a student or a postdoc if they want to help me and get to write the first draft, and then we work through it together.

DOMUSH: Wow, that's great. That's actually different than a lot of other Pew Scholars that I've talked to, who said that they, that's something that they don't expose their students to.

PANNING: So I guess it's your perception of what your job is. My job is to train these people. And so, these are the things they need to learn: one of them is writing. One of them is identifying an interesting problem, breaking it down to doable experiments or doable sub-problems that can be addressed experimentally and communicating all that; and also, making other people realize how interesting the problem is. So, choosing something that's interesting, and sort of reinforcing that it's interesting for other people. I had one technician who desperately wanted to go to grad school, and he couldn't identify an interesting problem if it hit him in the face. I mean he was just a very sweet guy, but he would get caught by stuff that was really peripheral and not—at least to me seemed very peripheral—maybe it was fascinating, but he didn't make a case for it being fascinating to me or other people. And that was just not the kind of things he would want to follow up on, were not going anywhere.

DOMUSH: When you write grants, do you also expose your students to that type of writing?

PANNING: Yeah. I get their comments on the grants. You depend more on my colleagues for input on grants because they have more experience with the study sections and the politics. The grantsmanship is probably more political than it is scientific, so go to the right people for the right help.

DOMUSH: Right. You had a student come in just now asking about if you'd heard back or if you heard back from *Genes and Development.* Have you had an experience like you had in graduate school where someone stole your data and ended up scooping you? Have you had to rush out a publication?

PANNING: We've not ever had to rush out publications. That's not true. There was one publication that we knew we had competition on and I was collaborating with another lab, and we did get that out very quickly. But for the most part, we haven't had to rush anything out. What we do is different enough from the rest of the world that we haven't been particularly worried. Hold on, that is twice. There was also another with Meter. We had been working on the story for a long time, and we thought we had no competition. Then Meter was just taking his time. And there was one experiment that would have moved it into a really top tier journal. And he just wasn't working hard on it and luckily, we got an advanced online, like from *Genes and Development* back then, a paper that had significant overlap with ours was coming out and it wasn't in press, yet. We just knew it was coming out in a month's time. So, Meter just wrote

his up really quickly and sent it to *Journal of Biological Chemistry*.[3] It probably would have gone to like a *Genes and Development* or *Cell* if we had time to do that last experiment. But since his, since we could have tried something a bit trickier which would have been going to another pretty good journal, *Nature Structural & Molecular Biology* with the semi complete story and hope that while it was under review they'd gotten that last experiment done. But Meter just didn't want to risk it. He just wanted to get it out, and he had a postdoc, lined up and he was ready to go. So, that's the . . . it was his . . . there it was his choice. I would have been happy to go either route, but he just wanted to get it out.

DOMUSH: Normally how do you decide where to submit a paper to which journal?

PANNING: I talk to people, figure out how, what journal's appropriate for us. I talk to people outside the field, what kinds of things are they publishing? If you know a little bit about the editors, you know what their expertise is, so that particular editor did graduate work or postdoctoral work in this field, so maybe they're more interested in this, than another editor. You can, the journals sort of publish a spectrum of things, and you know where your stuff lies within that spectrum, so there are multiple tiers of journals from good to bad that all publish cell biology. So, you decide how good your paper is, how exciting is it and try and get it into the best journal that it possibly can. Some people just send everything to a top-tier journal, and that's definitely not the . . . . I like to think I instill in my students and I too have a little more sense than, you know, just wasting time if it really doesn't have a chance.

DOMUSH: Right. But stuff that doesn't have a chance in the top-tier journal but does in the next tier.

PANNING: The next tier level, yeah, right.

DOMUSH: Okay. Aside from, you know, teaching your students about writing and guiding them through their research work, are there other kind of duties . . . what kind of things do you do here at UCSF that maybe take you away from the lab?

PANNING: So, there's medical school teaching which involves—it's not didactic—it's small-group teaching, problem-based learning with groups of fourteen medical students. I think I have six or, I think, seven sessions, two-hour sessions, and it's a lot of work. The fact that it starts next month and I've got two months of it that take me away a lot, just the standard stuff. I'm

[3] D. A. Nusinow, I. Hernández-Muñoz, T. G. Fazzio, G. M. Shah, W. L. Kraus, and B. Panning, "Poly (ADP-ribose) polymerase 1 is inhibited by a histone H2A variant, macroH2A, and contributes to silencing of the X chromosome." *Journal of Biological Chemistry* **282** (2007): 1285-1289.

sure you've heard this from all the other Scholars reviewing manuscripts for journals, being on thesis committees, being on different university committees from the graduate admissions committee, it's the students of the committee, the curriculum committee for the graduate students. I'm not even naming them all, there's just lots of them, so you're always helping, doing sort of community work, just going to scientific meetings and giving talks that takes you away. And I haven't been doing that so much for the last year. I'm finally at the point now, where my husband can look after the kids by himself, and I can start going away more than once a year.

DOMUSH: That sounds great.

PANNING: Yeah. This year should be fun. Yeah. Writing grants, writing review papers like other manuscripts takes quite a while when I get one, which is frequently.

DOMUSH: Do you ever have an opportunity to actually be in the lab yourself now?

PANNING: [Yes], I do. I go in quite a bit. I can't follow one project through to completion, but I can walk in and say, "Look, I've got a free afternoon today or a couple of free afternoons this week; is there something I can do for you?" And then, if someone has an experiment that I can help them with I'll do it.

DOMUSH: Okay. When you first got to UCSF as a PI were you . . . ?

PANNING: I was in the lab all the time. Yeah. Now, it's a lot less, but I'm still there. I'm hoping I never have to give it up completely, but we'll see.

DOMUSH: Does the summer afford more opportunity for that or . . . ?

PANNING: The summers a little more . . . right, I don't have as many teaching responsibilities. Because also, I teach in graduate courses, so we have a first-year graduate curriculum. I teach there, as well, yeah, all that. I don't do that during the summer, so that makes it a little easier.

DOMUSH: A little bit more time.

PANNING: Little bit more time. But this summer I have to admit I've used the time to do things like take my six-year-old to Raging Waters for a day [and] do Mommy stuff which I'm trying to juggle in amongst the work stuff as well. [laughter]

DOMUSH: Right. I can imagine that we'll probably discuss some of that more tomorrow. The way you phrased the work that you do at UCSF, a lot of the committee work you termed it as the "community." Is some of that work for UCSF actually going out to the community and doing anything?

PANNING: Not so often. There are, there's teaching that you can do. Well, they're always needing people to help with the scientific outreach teaching programs. But that's mainly graduate students and postdocs that can take a few hours a day for ten weeks to go teach fifth grade students, and I just don't have the time to do that. Every now and then people from UCSF are asked to give lectures as community outreach. I'll go and give talks at conferences or universities with large minority populations to try and increase the visibility of UCSF and science education, graduate school in particular amongst minorities. There's not that much science outreach that the university does. We offer a medical school . . . a mini medical school [for the public] and sometimes . . . one year that I helped with that. I helped develop some lectures for that where people can learn a little bit about medical school or areas of medicine. Every year it's a different theme.

DOMUSH: Okay. I think we're almost at a good stopping point for today, and I think that tomorrow we'll pick up and start talking a little bit about the Pew fellowship and then go off from there into some of these larger issues about science. But I did have one question and, of course, if there's anything that you wanted to bring up it's more than okay. When you [. . .] started your job search and you and your husband were thinking about places you could go if you chose not to stay in Boston, did you think about going back to Canada?

PANNING: No, there isn't so much in the way of biotech opportunity in Canada, so my husband is actually the one who decided, no, we don't want to go to Canada. When we got together, our original dream had been to get jobs in British Columbia, back in Canada, and when he decided to take the move to biotech there was, that was kind of a no-brainer, like it wasn't going to happen. So when I got the job on the west coast it was pretty exciting because we wanted, it's a place he loves more. The reason we wanted . . . he grew up in Edmonton, Alberta which is landlocked. It's Montana and right north really to the Arctic Circle. [laughter] It's the middle of nowhere. He never wants to go back there. But his family had always taken vacations down the West Coast in Oregon and Washington and very rarely into northern California, but those are where all his best memories were, and that's why he wanted to settle in British Columbia at some point. So this was the next best thing.

DOMUSH: Okay. Is that changing at all, or do you have any sense of whether that's changing about biotech being a more in the United States thing than a Canadian thing?

PANNING: There are biotech companies that are now starting up in Canada, more of them in the Toronto area, so, yeah, it's changing. There are and will be opportunities there in the future and now.

DOMUSH: Interesting. Did you think about going to Europe at all or was that just kind of too far away now?

PANNING: So, because I had Swiss citizenship it would have been, I probably could have gone anywhere, but it would have been easy for me to get a job in Switzerland…being a female. But I didn't . . . my husband had done a postdoc in Switzerland before he came to Boston and really didn't like it, so he didn't want to be that far away. He didn't enjoy it. And for me, Switzerland is a very . . . people are very staid and slow to change. So, you can be living there for ten years before you get invited to someone's home for dinner.

DOMUSH: Oh, wow.

PANNING: They're a very closed culture, and I didn't want to go back and work my way back into that. That didn't appeal to me at all, and I wanted to be a little closer to my family. Not that it's, my family's all in Toronto or Europe, but my parents and brother and sister are all in Toronto, so that's . . .

DOMUSH: Maybe it's a little bit closer than . . .

PANNING: Six of one, half a dozen of the other. It's the same plane ride basically.

DOMUSH: Do you get any opportunities still to go back to Switzerland?

PANNING: We try and go back. I've gone back maybe three times with my husband. I try and go back as much as I can. It's just getting awfully expensive with two children.

DOMUSH: Right.

PANNING: And double the airfare now, right. And airfare's not cheap.

DOMUSH: No, certainly not. Since he did a postdoc in Switzerland, does he speak German?

PANNING: He was in Geneva, [Switzerland], and he learned a little bit of French.

DOMUSH: Little bit of French.

PANNING: It is, you know, you're again in this microcosm where everyone's speaking English because it's international, and that's the language of science. So, he didn't get that much of an opportunity. He didn't like it, so he came back the States pretty quickly.

DOMUSH: Right, okay. Well, unless there's anything that you want to add today . . .

PANNING: No. I'll ponder this tonight and maybe I'll have some questions tomorrow.

DOMUSH: Okay.

PANNING: All right, great.

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

INTERVIEWEE: **Barbara Panning**

INTERVIEWER: **Hilary Domush**

LOCATION: **University of California, San Francisco**
San Francisco, California

DATE: **28 August 2008**

DOMUSH: Today is 28 August. Again, I'm Hilary Domush and I'm here at UCSF with Dr. Barbara Panning. And yesterday we finished off for the day talking about when you began as a PI. And so, today I wanted to begin talking a little bit about the Pew fellowship. And first of all, I'm interested to know if you were familiar with the Pew fellowship before you applied?

PANNING: I had not heard of it before I applied. I knew that there were . . . when I arrived at UCSF I was told about fellowships that I could apply for, and Pew was amongst them. But up to that point, I hadn't known about it.

DOMUSH: Okay. So, when you got here and they told you about the fellowships that you could apply for or that you were encouraged to apply for, did you just apply for all of them or did some seem more appealing?

PANNING: They all seemed pretty appealing. So, what happens is you apply for all of them, and an internal committee decides who to put forward for each individual function. So, you go through an internal competition first, and then if your application is selected it was forwarded. So, it happened that Pew was the first one in the calendar that I applied for it, and I was put forward for it.

DOMUSH: How did writing the application go? I've been told that writing a Pew application is very different than other funding agencies that you're used to writing for.

PANNING: The one thing that's different is highlighting the research that you propose to do with the Pew, how unique, different, and challenging, possibly not fundable by other mechanisms. You see that in some applications, but you don't see that so often. So, that component of it didn't—couldn't—be done on autopilot that you would write every other grant.

DOMUSH: Was that something that . . . was it easy to describe the ways in which your research was risky or possibly not fundable by . . . ?

PANNING: In short, no problem. [laughter]

DOMUSH: Okay. What about it was risky?

PANNING: So, again, I described about X-inactivation and how we basically don't even have molecular handles on any of the events. And some of them we do have handles on and that allows us to move forward a little bit. What I wrote the fellowship on was something that we didn't have such a great handle on, and it was certainly nothing that would have been fundable from a place like the NIH [National Institutes of Health] because there just wasn't enough what you call preliminary results . . . preliminary data that NIH would have had confidence that this would have yielded publications somewhere down the line.

DOMUSH: Okay. And were you able to achieve your Pew goals that you set out?

PANNING: I would have to go back and look at the original fellowship application to figure out if I did everything. I did a good portion of it.

DOMUSH: Okay. So, when you got the Pew fellowship, obviously, compared to an NIH grant the money is not that substantial, particularly compared to NIH. What did you do with that money?

PANNING: I used it to pay for other technicians or graduate students for the duration of the fellowship. So, it allowed me to hire someone that I wouldn't have been able to hire otherwise, which allowed [. . .] projects to move forward that would never have even gotten off the ground. So, that was big. I mean that's not just . . .

DOMUSH: Do you remember how big your lab was at that time?

PANNING: When I first got the fellowship, I think I had four people, two graduate students, a technician, and a postdoc. And by the time with the fellowship, I think I had twelve.

DOMUSH: Oh, wow. So, that really . . . it really took off.

PANNING: It helped, yeah. So, it certainly helped, the other things helped as well, projects coming together and stuff like that.

DOMUSH: Okay. Can you talk a little bit about your experience at the Pew meetings?

PANNING: So, you don't need introduction to the Pew meetings here, I'm sure. They're held at beautiful, balmy locations and in the nicest suites I've ever been to in my entire life. And great opportunity to get to meet other scientists and talk about life and science in no particular order in an environment where it's not…where it's more relaxing. So, it was . . . they were wonderful. [. . .] I made it to the first retreat. The second retreat I'd had a baby in [. . .] I'm not sure if I missed one or two retreats. I'd have to go back and look…because of recently having had babies, or having a newborn. Certainly, my second child was born in December, and I couldn't make it to the following March retreat, so I didn't even get the full retreat experience or benefit from the full four years of retreats. But the ones that I went to I remember being a . . . part of me thinking, I wonder if the Pew could put this money into research instead of the retreat. [laughter] Part of me thinking this is a really enjoyable break and I really got a chance to meet people that were doing research in a field that was outside of mine that I wouldn't have run into in the course of the normal set of conferences that I go to.

So, I feel like my experience was really broadened. I made connections with people that I wouldn't have made connections with otherwise, and it's been great. And every now and then I'll need something and I'll look it up, look up who can provide it, like a construct or something like that and it's an ex-Pew fellow. And it's a great way to start the email you know saying we were in Pew together and now I'm asking you for something. And you usually get a response that says oh, those meetings were great. I wish that we keep going to those meetings.

DOMUSH: I mean, aside from, you know, being able to email someone and say remember that meeting, can I borrow such and such . . . did any type of collaboration come out of it?

PANNING: A couple. I don't know if they went very far. Sort of ideas, and we sent reagents back and forth. And then they never really went anywhere. So, it was kind of, wouldn't this it be cool if this was true? Let's test it. And it wasn't and we didn't go any further.

DOMUSH: Okay, but the idea and the conversation was out there. I know you said that at the Pew meetings you were being exposed to people, you know, kind of all walks of biology that you don't really run into at most meetings that you're going to. Did it make you want to seek out kind of a broader swath of scientists after those meetings were over? I mean there's so much to be gained from going to a conference where everyone knows what you're talking about.

PANNING: Right.

DOMUSH: But what is there to be gained, I guess, from going to a meeting where you can't . . . you're not necessarily speaking the same terminology?

PANNING: So, you learn about tools that are emerging in other fields that you could apply to your own field. Sometimes you learn about different ways of thinking about things, about problems. So, the problem of one. How do you choose one of something? This does it many, many different times and it does it in the context of X-inactivation and also does it in a context of just making one centriole. So, talking to centriole people to see how they think about a problem, let me figure out different ways of thinking about a problem that I was working with. So, it broadens your approach, your thinking, so that's good. Did I go out and purposefully seek that in my choice of meetings or collaborators after Pew meetings or as a result of Pew meetings? Not particularly.

DOMUSH: I mean, I'm not sure. When I ask the question I'm not sure how one would even do that, so I don't know if it's . . .

PANNING: So, one example would be to try to go to a scientific meeting just outside of your field and it's kind of peripheral [or] tangential, and hope that you learn something there. But meetings are expensive. I have two small children, limited amount of time you can travel. So, realistically it's not really a possibility.

DOMUSH: Are there opportunities on campus where maybe you could be doing something?

PANNING: Right, exactly. So, I'm exposed to that all the time. We have retreats where virtually every researcher at UCSF heads out to [Lake] Tahoe for the long weekend. And we - and not everyone talks every year, but in the rotation of every two or three years, everyone gets a chance to speak. We have . . .

DOMUSH: Those sound like mini Pew retreats being in a very . . .

PANNING: Mini Pew retreats.

DOMUSH: Pretty location.

PANNING: The accommodations aren't quite as luxurious but it's still pretty nice. And we have a Friday and a departmental meeting and an interdepartmental meeting once a week. And in the departmental [meeting] everyone in the department gets up once a week and talks about what they're doing. And the research in the department ranges from neurodevelopment and flies to purifying complexes and to the basic, very biochemistry, biophysics to development and genetics. So, you get exposed to a lot there. And there's an interdepartmental meeting where all the departments, neuroscience, physiology, cell systems biology departments are present. So, you get a really broad swath of what's going on.

DOMUSH: These things that you just mentioned, are graduate students and postdocs at these as well or is it just faculty.

PANNING: This is faculty. And then there's the departmental seminar series which brings in people from, that are doing, a huge variety of science, every Tuesday afternoon. And so the graduate students and everyone goes to those. The departmental retreat or the huge retreat in Tahoe, everyone is invited to, grad students, postdocs, everyone, but those weekly meetings were just faculty. They're more . . . we think of them more as faculty development than development of everyone.

DOMUSH: Do you find that graduate students or postdocs . . . . I mean, UCSF is a very rigorously academic place, are graduate students and postdocs inclined to go to talks and symposia that are not directly related, but that they see the benefit of going a little bit tangential or is that something that you kind of have to . . .

PANNING: I think if it looks like the talk will be interesting enough, they'll go. So, even if it's completely unrelated to what they're doing if either the title is interesting or we've talked to them about the speaker and saying they'll give a really interesting talk, often if it's a high profile, really high-profile paper that's come out in the last year, pretty much everyone in the building will go. There are days when there are talks done in the auditorium when, if you're late, you're not getting a seat.

DOMUSH: Okay. And before you went to your first Pew meeting were you excited about interacting with biomedical scientists that weren't quite in your field or did you . . . ?

PANNING: Oh, yeah, absolutely. I had already, coming from Rudolf's lab where it was great to be learning something about neurosciences. I completely see the benefits of that, so oh, yeah. It's wonderful to be interacting with a disparate group of people.

DOMUSH: So often outside of science or depending on who you talk to, it's so easy to get *so* focused on what you're doing and not see the benefit or not even realize that there are kind of these other important things . . .

PANNING: There's other things out there.

DOMUSH: And, you know, especially like you said that really could apply or help you think about something in a new way. So, that was something you were looking forward to going to Pew?

PANNING: Yes.

DOMUSH: Okay. Did your husband get to go with you to the meetings?

PANNING: One meeting my husband and son went. So, my first child went with and we were, and then it was a Puerto Rico meeting and then we stayed [and] turned it into a vacation as well. I discovered at that meeting and a few other things that I attempted that you can't do meetings with them. It just divides your attention and you don't do either of them justice. So, it was a learning experience.

DOMUSH: Well, he was probably excited for it to be a learning experience and go to Puerto Rico anyway, maybe.

PANNING: Yeah. And we stayed an extra week in Puerto Rico after that. So, that was good. That was nice.

DOMUSH: Do you think that it's just that it's too hard to split up the time between family and scientific meeting or was there more that Pew could have done to help with that?

PANNING: I realistically don't think there's anything that Pew can do. When you have . . . in my case that he was just under two—twenty-four, twenty-three, twenty-two months old—and he

sees Mommy somewhere in the building and he wants . . . the child needs your attention. It's very difficult for my husband to look after a child alone in a hotel room without the normal things you have at home to distract them. So, it's just, it's very . . . and the child knows you're there, so why isn't Mommy here at eight o'clock to put me to sleep? Because there's an evening meeting. And Pew can't . . . basically there would be comfortably maybe two blocks during the day where I could leave the kid with my husband. And there's some times when it's more important that I'm around. [. . .] It's just hard to juggle it all. It's very hard to juggle.

I've seen other meetings where there are more families there, where the families and the children know each other and then you can take advantage of kids playing together and enjoying each other and then the fact that one parent isn't there isn't quite so difficult on them or hard on them. But that's not really possible with the Pew, I don't believe. People are there for four years and come in and out, and I don't think that's going to work. Who knows that maybe there's a creative way to think about it.

DOMUSH: I spoke with another Scholar recently, who said that she also missed some of the Pew meetings because of when her children were born, either they were just born, or just about to be born, and, you know, she couldn't go. And she said she really wished that Pew had let her make up the meetings. Is that something that you also would have liked to take advantage of?

PANNING: I wouldn't have said no. [laughter] Certainly, I wouldn't have said no. But it didn't occur to me. You're in a class. So, when you're, it's kind of, it also would be bizarre to be sort of disconnected. You're sort of one group of people and you go there four years together. And those are people you get to know the best because you overlap with them the most. So, it would be kind of bizarre to be there an extra year and effectively maybe know some of the people…

DOMUSH: But you wouldn't have that same class feeling. Do you think that . . . the Pew fellowship is a fellowship for young investigators, and many people are starting families, is there . . . . I mean, what can be done [. . .] by Pew to help with that? You know, I mean people talk about how great it is to have the meetings in Puerto Rico, but maybe if they were somewhere in the United States or continental United States maybe it would be easier?

PANNING: I think the aim of the Pew is to promote . . . in addition to biomedical research in the United States, they also have this Latin American Fellows [Program in the Biomedical Sciences], and the two meetings are held together. I think it's quite important for them to have it in some Latin American country which kind of lets out the United States. So, granted it would be easier in the United States, but I think what the Pew is trying to accomplish makes it difficult to do that. So, I had to miss a meeting or two and a few other people had to miss a few meetings. It was, you know, it wasn't devastating. I mean, you move on. I think what the Pew is trying to accomplish I would argue is more important than the inconvenience I had because I couldn't go to a couple of meetings.

DOMUSH: Did you get to interact with the Latin American Fellows at all?

PANNING: Yeah. You get a chance to interact with anyone that you feel like basically that you're willing to go up and talk to and interact with. And the Latin American Fellows at the meeting have poster sessions specifically where we all go around and talk to them about their science. So, there's events organized so that you can talk, events organized to facilitate interaction between the Latin American Fellows the Scholars. They're a different population because the Latin American Fellows are postdoctoral fellows and Pew Scholars are faculty—junior faculty. I mean, there's different concerns and certainly there was discussion between the two groups, probably more of a mentoring from the faculty to the Fellows.

DOMUSH: Did you get to interact with the Advisory Committee members?

PANNING: Absolutely. Yeah. You see them. They're everywhere, you know. They were in the bars at night and they were at dinner times. So, yeah, you got, I got lots of chances to talk to them, too.

DOMUSH: Was there anyone in particular that you were kind of excited to go and meet or maybe a little wary of walking up to them and on the beach.

PANNING: Yeah. I don't think I ever . . . when the opportunity presented itself I would certainly talk to people, but I wouldn't seek anyone out. But I had lots of chances to talk to people [. . .] whose science I've always admired and got and it was very interesting to interact with them, ask what was happening. You know interact with them and ask what was going on in their labs at this time. So, everyone like Tobin and Gerry [Gerald R.] Crabtree, Larry, his name is [Larry Prisky] I had some fascinating discussions. So, it was really good. It was wonderful.

DOMUSH: Okay. Just before we move on to something else, is there anything about the Pew fellowship maybe in comparison to some other kind of young investigator fellowships that you know of or anything that the Pew could do differently? I mean, aside from funding you forever, you know, something like that, but anything that would have made that experience or that program more beneficial?

PANNING: So, I didn't have any other fellowships so I can't compare the experience. I think the Pew, when I've spoken to people who had a Searle [Scholars] or a Packard [Fellowship] I think the Pew did things a little bit differently. There was maybe a bit more attention paid to, for

instance, developing managerial skills and Ed [Edward H. O'Neil] did his talks about that, which were always entertaining and very informative. So, I think that was unusual about the Pew and maybe something that could be developed more. They're certainly . . . they were aware of issues and would have mini sessions on them like grant-writing where people could talk to members of the [SIB] or other Pew Scholars and discuss things like that. And in the context of and these occurred in the context of the retreat and I don't know if anything more could be done, anything more structured, for example, because of the amount of time that they . . . this was what they were trying to stick in, in between all the talks that are given and all the other things that are going on. I never really thought about it deeply enough, and off the top of my head, I think they do more than most other people do and can't think of anything else. But I may have an epiphany at midnight tonight and I can get back to you. [laughter]

DOMUSH: Okay. You said that they do things like talk about managerial skills or about grant-writing; is that something that . . . you know, everyone that's there is a relatively young PI, you know, in various stages of being junior faculty. Are those the type of things that you're all talking about with each other or conversations kind of more like this is the research I'm doing . . . ?

PANNING: There would be the whole gamut. There would be experiences at study sections. The kind of things you can do to help grants. Have you got your first grant yet? Many of the people at Pew [meetings] don't have their first grant yet. Certainly the whole gamut but that from your research, what we're trying to address, to how you like living in the city that you're in. Common friends. Many people know other people—that's, you know, just from social to work-related. They all sort of happened in a very casual environment.

DOMUSH: So, do you think then that because the Pew meeting really has kind of this wide swath of biological sciences that it makes it easier to talk about not just science but some of those other things, whereas maybe at a more specialized meeting where everyone can really talk about the same aspect of science you get so focused and maybe you don't talk about some of those other really important issues?

PANNING: I think the Pew meeting also explicitly states that you want to talk about some of these issues. They have small forums on grant-writing. They have management, understanding management, or what you can do to manage your lab better. Those are all issues that are brought up, and at other scientific meetings, they just wouldn't really be brought up. So, independently people can try to discuss those things with others that they meet at the meeting and it may be appropriate in some circumstances and not others.

DOMUSH: Okay. So, what are some of the other meetings that you go to? I know you said that you've been going to fewer since your children.

PANNING: Mainly meetings on epigenetics or chromatin transcription. Those would be sort of the key areas. Every decade there's one meeting on X-inactivation that I would go to. But no, you know, chromatin regulation and epigenetics-related fields, transcriptional regulation. Those would be the kind of meetings I would go to. And they would take place in, they would be the Keystone Meetings, or FASEB [Federation of American Societies for Experimental Biology] meetings which often go on in Colorado in the summer, or Gordon Conferences which were often on the east coast at the small schools in the summer. The Keystone Meetings . . . . I organized one a year ago, about a year ago now, that was a ski meeting in Ontario [Canada], so . . .

DOMUSH: How was it organizing a meeting? Is that a . . . ?

PANNING: Little bit stressful. But it was okay. You write a grant to pay for it, and you invite speakers, and some say no, and then you invite more speakers, and then you hope people think you put together a meeting that's interesting enough that they want to come. That happened, so it was good. Yes.

DOMUSH: Did you get to expose your students to anything about kind of planning a meeting?

PANNING: No, that really, wasn't really an option. I guess, every now and then, I would walk in and say do you think we should…have you ever heard this person speak? Do you think we should invite them to a meeting? So, my students come from places like Harvard [University] and MIT, and they've been exposed. And the postdocs in particular did graduate school, and got to [go], and had been to meetings and heard all sorts of speakers. So, I'd ask for advice, but not really the nitty-gritty of organizing the whole thing.

DOMUSH: Okay. So, when you first got your Pew fellowship you didn't have NIH funding?

PANNING: I did have. I had gotten NIH funding.

DOMUSH: Okay. So, when you started the Pew you had your NIH funding. How do you think then the Pew and the NIH at the same time kind of helped to reinforce each other? You know, that all of a sudden you must have had, must have seemed like there was a lot going for your lab kind of money or prestige-wise. So, what changed all of a sudden?

PANNING: It just gave me the flexibility to expand the lab, to have more graduate students and postdocs, so to do projects that I wouldn't have otherwise started, because they were . . . the Pew in particular, because they were a little bit risky. The NIH is a little bit more constrained. That was the main thing I think. Again, just more opportunity because the more money you have, the more people you can hire and the more opportunity you have, the more hands you have working on projects.

DOMUSH: And it just feeds into it, you know the more . . .

PANNING: And, it just feeds into it, right.

DOMUSH: Okay. Can we talk a little bit now about NIH funding, and this has been a big topic of discussion with many of the other Scholars. So, one thing that I found out earlier this week is that I was told that the NIH grants are dropping in size—the actual size of the grant from I think twenty-five pages to fifteen.

PANNING: Yes. They will be getting shorter.

DOMUSH: Which to me sounds like a huge decrease in the amount of space you have to talk about your research. So, I don't know when this goes into effect or when it will start to affect you, but are you excited about this decrease in the amount to write, or is it going to be harder to say what you need to say in fifteen pages?

PANNING: It will not be harder if you have enough published results that you can justify the research. So, basically, right now the way the grants are structured there's one page is specific aims, where you clearly state what you want to do, very precisely. The next page is background and significance or the next two pages are background and significance, relatively briefly providing the background. And then the next . . . so, it's a twenty-five page . . . the next ten pages—ten to eleven pages are preliminary results. And the next ten to eleven pages are experimental findings. So, if you can just get rid of all the preliminary results section which you can do by referring to the published papers, then this isn't too difficult. If you don't have a lot of published papers then it becomes tricky because you have to use six of your pages to present preliminary results, or if you want something new that you haven't published yet—breaking out into an area you haven't published yet—you're in the same quandary. You have to present some preliminary results to justify the fact that you have ideas, that you can do these experiments, that there's the kernel of something interesting there.

So, then you have to cut away from your experimental plan and there you're trying then to balance how much detail do you go into in the experimental plan to justify getting money to

continue doing work for the next four or five years. And I'll suspect there'll be burps and difficulties for everyone as they make the transition. In the end, my guess is the study sections are going to be so grateful that they don't have to read ten grants of twenty-five pages in great detail to comment on.

DOMUSH: Do you think though that it might make . . . . I mean, people are talking all the time about, you know, fewer grants are being funded because there's less funding available. But, if you cut ten pages out of the grant, is there a concern that the choice for who does get funding might be more arbitrary or is it arbitrary now?

PANNING: It's already very arbitrary or there's certainly a huge arbitrary component to it and enormous political considerations. And I don't think that's going to go away as they get shorter. I think right now the feeling that everything is funding at five or at least five percent fewer grants than they used to, at least, if not more. And senior investigators are losing their grants or not getting them renewed the first time. And the feeling is you're just waiting your turn. So, depending on where you are in the lineup for waiting your turn, you may have to go through a couple rounds of going, of not . . .

DOMUSH: Resubmission.

PANNING: Resubmission before you get funding.

DOMUSH: Have you had to go through . . . ?

PANNING: I had to go through rounds of resubmission, yeah, and the political things that come into play. So, I just had an interesting conversation with the person who used to be head of my steady section. And she stepped [down]. She was finished, done her tenure, and we were talking and she said, "Oh, yeah, you should never send any grants to the study section. I couldn't tell you that before because I was head, but there's one person on that study section who knows your field, has very firm ideas of what's going on, and you will never get a grant funded."

DOMUSH: Wow. Are there other study sections that you can submit to?

PANNING: Well . . . so, certainly now, I've started looking for other study sections that I can repackage the research to go to different sections are funding that area of the research and you know in NIH-based mechanisms.

DOMUSH: Wow. I guess that's good information to know.

PANNING: It was good information.

DOMUSH: It would have been . . .

PANNING: But, you know, politically, you know, I've been banging my head against the wall at that study section before.

DOMUSH: Right. Is there any way to kind of take that aspect out of NIH funding, do you think, or is it just that people are making these decisions and people have their own agenda?

PANNING: Right.

DOMUSH: Now I remember when I was in chemistry as a graduate student we were forced to do a lot of work by our professor that when it came time for grant submission. Do your students do work to help you? I mean written work as opposed to their research work.

PANNING: No, I have them read over the grants, but I don't have them write any of the grants. I know some people who have been very successful here to have their students and postdocs write portions of the grant. But I feel like that's such an inappropriate use, though. I mean, it's good for them to help and to think about it, and I certainly talk about . . . . I could structure it this way and these are the, you know, do these experiments make sense? So, a lot of discussion, but the actual sitting down and writing is not really their job. I certainly take input, but I don't feel like I should be taking time away from them getting their thesis or their papers done to write the grant. On the other hand, it could be considered really good training experience, so . . .

DOMUSH: But they do get to read through the grants.

PANNING: Read through it and they make comments on it. And they . . . specific aims have changed completely because of their input, so . . .

DOMUSH: So they're, they are taking . . .

PANNING: They're involved.

DOMUSH: Have you had any students that have come to you and said, "You know, there's just not enough funding in general, and I don't want to do this anymore because I don't think that it's a fundable job"?

PANNING: I've had postdocs that have decided to go into editing and students that have decided to go into scientific editing and students that have gone to biotech because . . . they were all females, and I don't think it was strictly the funding. It was also [that] they all wanted to have families and it was just the thought of balancing this job with a family and in the best of times, much less in this environment where the NIH isn't terribly forthcoming for the most part they just decided that that wasn't the way they wanted their lives to be.

DOMUSH: Do you think that there's any sort of—bias isn't necessarily the right word—but that maybe women in science are put at some sort of disadvantage in funding because you know if they are having a family as you said yesterday, you know the earlier . . . again "burden" is not the right word, but there's so much . . .

PANNING: Demands.

DOMUSH: Yes. There's so much more demand on the woman early on and maybe that's keeping them from publishing as much preliminary data or I don't know. Do you think that . . . ?

PANNING: I think that it's situation dependent. I have some friends who either can afford a nanny or have family close by, and in that instance with that much support having children isn't that difficult. If you're in a situation where you can't afford a nanny and there's no family close by, those are the people who I see having the most difficult time. And [. . .] is there anything that can be done about that? I honestly don't know. I can't even think of a fair way of addressing it because every situation is so different. So, is there one rule that could apply to all?

DOMUSH: Should institutions do more to help? I mean, does UCSF do anything like offer day care or anything like that, something that might help?

PANNING: Again, it's difficult to [say]. How much is UCSF required, how much should UCSF have to do, and why should I be treated differently from a graduate student? So, UCSF has day care, but it's so expensive that with two children I can't afford it. So, I have my children in a day care, but it's also a little bit closer to my house and considerably less expensive, six hundred dollars a month cheaper for my younger one. My older one is in public school now. So, it's not…I don't know why I should benefit, where does the economic burden lie? And I don't know. Should grad students be supported in having children? Should they get free day care? At Berkeley they do. At UCSF they don't. Again, it's a really tricky situation. How do you decide that? My husband and I earn a certain amount of money; therefore, we do get day care support at UCSF, and someone doesn't. First of all, I don't think the money's even there in a state school to accomplish that.

DOMUSH: Right.

PANNING: But if it was doable. I don't even know how you would fairly proportionate it. It could be very tricky. So, maybe the fairest thing is just to have everyone fend for themselves. [laughter] Yeah. It's tough these social-management type things are very difficult. I don't know how they're best administered without, for them, how they can best reach their goals.

DOMUSH: Have you had any . . . you said that you've had students who have decided to go into other aspects of science away from bench work and it just so happened that they were all women.

PANNING: Right. Of my, the eight students I've had, six are female and two are male, so statistically speaking it was going to happen that way anyway.

DOMUSH: Right. Have you had any students, though, that have come to you and said, you know, you have two young children and you still seem to be doing a job you love that you wanted to do, do you have any advice for me?

PANNING: So, I get hit up to be mentor for postdoc mentoring events, graduate student mentoring events. I'm always brought forward as the one with two small kids that runs a lab. And yes, I am constantly asked for advice. At this latest meeting that I went—the chromatin transcription meetings, the FASEB meeting—they even organized a women's tea where that was to bring together the young female researchers or the graduate students and postdocs with the more senior women to provide a forum in which you could get advice about, again, life issues or whatever you wanted.

But it turned out most of the discussion was how do we balance? But the concern for the postdocs for the most part is how do you get two careers or how do we balance sort of all the major transitions and survive? How do two people get jobs together if you're both scientists? How then do you get a family started when you're both trying to write NIH grants? Those are the issues.

DOMUSH: Do you enjoy going to those meetings and kind of being put forth as . . . . I mean, I made the assumption that you know I see you and you seem successful in doing these things. And you do have two children that you've mentioned multiple times and you know that's . . .

PANNING: I'm sorry. Do I mention them that many times? [. . .]

DOMUSH: No, but you have mentioned them quite a bit, which is great. And so, I assumed that people would come to you and in fact, that's true.

PANNING: It does happen.

DOMUSH: So do you, do you enjoy it?

PANNING: Do I enjoy it? I'm certainly happy to help out wherever I can. It's I think nice for people to feel like they have someone they can come and talk to.

DOMUSH: Did you have anyone that you could go and talk to or any . . . were there any forums such as the ones you've mentioned?

PANNING: So, in my department and actually around UCSF there are not too many women that have kids. So, there was certainly . . . when I had my first there was not really anyone around my age that I could talk to that had children. Now more of the young people in my department including one of the women that had children, so there's more of a community, but then there wasn't.

DOMUSH: So, was it, when you did have your first child was it . . . did people say anything? Did they respond in any sort of way?

PANNING: So, for the first six months I brought him to the office with me every day. I brought him to every meeting. He came to all these lunchtime meetings where people gave talks. He fell asleep. After everyone eats, you know the senior investigators kind of fall asleep. He fell asleep with them.

DOMUSH: He fit right in.

PANNING: He fit right in. It was great. I think I got . . . sometimes people were taken aback or upset or not upset, I think maybe taken aback that a child was appearing everywhere. I didn't breast-feed him in public that often. But for the most part it was just accepted and that's the way it was. But I don't think anyone would have known. If they felt it was inappropriate I don't even know if they would know how to approach it with me, right. And since he was a pretty well-behaved child and didn't disrupt anything. The graduate students had a ball with him. And every year at this big retreat in Tahoe there's skits where they make fun of faculty and of course, that year it was a woman walking around with a baby in a baby carrier all the time. So, it was funny.

DOMUSH: Since then have other . . . you said that there's another female faculty member that has had a child. Did she bring her child to meetings and things like that as well?

PANNING: She brought her child . . . didn't bring to quite as many meetings but she certainly brought her child to work every day the first few months until a nanny was arranged . . .

DOMUSH: Well, you certainly have a big enough office that that must have been very easy.

PANNING: Yeah, that was not a problem. Yeah. But when they get to be a little bit older than the first two months, they start being more demanding of your attention, so the fact the way I did it, there were probably three or four months there, when I got nothing done until I could finally get them into a day care. So, there are very few places where you can take infants under five months old. So, effectively I don't know how women manage it who only get six weeks' leave. There's just very few places that you can leave your child until four or five months when the first, when they'll start to take them.

DOMUSH: I have no idea. I've never . . . had never thought about that.

PANNING: Yeah. You don't think about it until you're pregnant and you're looking into day care, right.

DOMUSH: So, you said though that you're . . . now your youngest child is in day care relatively near your house.

PANNING: Right.

DOMUSH: Is the commute that you have, or that your husband has as well . . . is that a difficult commute? Does it take up much time?

PANNING: No. Remember I told you yesterday that one of the things I learned when I worked in downtown Toronto is that I hate commuting.

DOMUSH: Yes.

PANNING: I took that to heart. And we made sure that we bought a house that was less than a ten-minute drive—ten- to fifteen-minute drive. So, it's a bit of detour for the day care, but it's not so horrible.

DOMUSH: Okay. So, now you said your older son . . . . I think you said he's five or six.

PANNING: He's just started first grade on Monday.

DOMUSH: Oh, wow. How exciting.

PANNING: Very exciting.

DOMUSH: And you said when I mentioned the parafilm on the table the other day you said that you had gone, I guess, to his classroom to do some experiments.

PANNING: Kindergarten, yeah

DOMUSH: Is that something that you would like to repeat?

PANNING: And I will. So, this is, he's in public school in San Francisco, [California], and the schools are good, especially if you have one where there's a significant parental involvement. So, one of the things I do is I go in one day a week and help out with what they call "centers." So, in the morning, one morning a week I'm there from 7:50 a.m. when school starts till 9:30 a.m. And I help out with some activity that the teacher organizes, which is usually either art or poetry activity with a small group of children, when the children are sort of rotated to the centers. So, every group, they'll be divided into groups. Every group does something at the poetry center where I'll be at on one, do on Monday with a different mom on, one will then do, Tuesday's a different mom. So, we do centers. And then on top of that I do a science unit for the last quarter of the year. And I think I went in for five weeks and did—or six weeks—and did scientific experiments with them.

DOMUSH: Oh wow, that sounds so exciting.

PANNING: It was lots of fun. It was great. It was great.

DOMUSH: Do you have any sense that . . . . I mean, you've gone in now to a classroom. Did you do anything reaching out into the community like that you know before your son was in school?

PANNING: So, what I did . . . UCSF has a lot of outreach to minority colleges. I would go and give talks at minority institutions that had a strong minority presence to be involved in the recruiting of minority people to UCSF graduate school. So, I think that's what I did the most of. There wasn't . . . . I always encourage the students in the lab to take part in the teaching . . .

DOMUSH: Right, you mentioned that.

PANNING: Science education program. But that's a big commitment. That is essentially five hours, the better part of a day, one day a week for ten to fifteen weeks straight. And that's just not an option for my life right now.

DOMUSH: There's so much discussion in the media right now about the numbers of students at various points in their academic career choosing to not pursue science. And is it your feeling that, you know, scientists like yourself going into a public school and doing scientific experiments even with kids who are in kindergarten . . . you know, that very early on that that's what's going to get kids to stay and keep going in science?

PANNING: I guess anything that engages children in learning will encourage them to stay in school and encourage them to follow that area. So, I guess what I hope is that children that have some innate interest in science that that can be fostered enough that they'll continue by having experiences like this. I don't know if I'd ever turn someone who is clearly an artist into a scientist. But if they, if the kernel is there I would hope that we can promote it so that it gets a chance to grow and maybe get a little scientist out. Or, if it's . . . or some will learn that this is what they don't want to do and I guess that's useful too.

DOMUSH: Do any of your colleagues, did any of them hear about your experience about going into the kindergarten, and say that sounds really neat; I never thought to go do something with kindergarteners?

PANNING: Honestly I never . . . . I haven't talked about it with my colleagues. It wouldn't [. . .] it falls under the parenting [category]. So, the world is divided into Mommy and work. And they don't really mix together too much. I'm certain that when the colleagues I have that have younger children and their kids in public school this will be something, once they've experienced it this will be something I'll discuss with them.

DOMUSH: Okay. [. . .] You said yesterday that since you've had children you've not gone to as many meetings, you've kind of cut your lab size down a little bit. So, now you have a six-year-old and I think you said you have a kid who's three…

PANNING: Three-and-a-half.

DOMUSH: Do you think you're at a point where you can start going to some more meetings?

PANNING: Yeah. So, now they're old enough that I can leave them alone with my husband and he's not completely overwhelmed. Two small children don't need me quite as badly, right. It's just a lot easier now that . . . . I'd have to explain that my three year old had some health issues that made life extraordinarily difficult for the first two-and-a-half years of his life, and we're just coming through that now, and coming out where he's okay. Everyone's life is just easier sort of all around.

DOMUSH: Okay.

PANNING: So that's been part of it, as well.

DOMUSH: Okay. But he's doing, he's better.

PANNING: He's doing fine. It was never anything life-threatening. Do you want to hear it?

DOMUSH: Sure. [. . .] If you want to talk about it.

PANNING: I don't know if the Pew people want to hear it. No, it was never anything life-threatening. It was more incredibly inconvenient. It was life-threatening in a way but not horribly . . . not imminently to worry about; it just made it incredibly difficult for anyone to get any sleep for two-and-a-half years.

DOMUSH: And now . . .

PANNING: Now we're through that.

DOMUSH: You can get some sleep.

PANNING: Now, we're through, now we can get sleep, and one person can handle the two kids alone.

DOMUSH: So, how much time in trying to balance being a mom, being a professor do you find yourself bringing more work home or maybe not staying as long at the office?

PANNING: So, I have to be home in the evening for the kids. It's not an option. So I have to leave here every night by 5:00 p.m. at the latest. So, what I do is when I finally get my children to sleep I pick up my computer and usually begin working unless I'm sick and so tired that I can't work anymore. Unfortunately, that's usually not till 10:30 p.m. at night because the younger one gets a nap at school every day, so it's not unusual for me to begin work at 10:30 p.m. and work till 2:00 a.m. or 3:00 a.m. in the morning. So, many nights I will do that. I'm usually so exhausted that I take a couple of nights off and start up again. So, it just, I can be here from 9:00 a.m. to 5:00 p.m. and then I have to make up for not doing a 9:00 a.m. to 5:00 p.m. job after.

DOMUSH: Right, okay. We talked a little bit about funding and, you know, some problems with NIH funding and how they can be so political or arbitrary, and I'm curious, you know, you went through graduate school in Canada and your husband's Canadian, but I don't know if he also went to graduate school in Canada.

PANNING: He didn't. He went to graduate school in Britain.

DOMUSH: Do you guys have . . . since you have kind of this international outlook on science, do you have friends who are research scientists in other places in the world that maybe you get an insight into what funding is like in other places?

PANNING: We certainly have friends in Canada and Britain and I mean it's not great anywhere is what it comes down to. It's always there's never enough to go around. And I don't think things are contracting as quickly in other countries as they are in the United States. It doesn't seem quite as bad there, but it's not great. And there's certainly not as much money, so. The United States went through—the NIH—went through a nearly a doubling and there's a lot more money. And a lot of buildings, like this one, were built. A lot of people were hired. And then it started contracting pretty rapidly, too, and much of what was done on speculation [occurred] on the assumption that NIH's growth would at least, even if it didn't continue to double, it would at least stay the same.

In other countries, that didn't happen. And then the amount of money given out for a single Canadian grant doesn't, it doesn't compare to what an NIH grant does. So, it's just a completely different, there's not the shell shock in Canada and in Britain because you never went through strategic expansion and people just never had so much money in the first place. So, there's always been the complaint that there's not enough. But I think in America it's just worse because . . .

DOMUSH: There was enough.

PANNING: There was. There was certainly more. I don't know if any, actually, when there was more, I don't think that people thought there was enough. But there was certainly more. And I think the problem then is that at the time when there was more there was a lot of expansion going on under the assumption that there always would be more. And that's now, we have institutes that are empty like it's there are places that I've gone to give talks where one person in the department has funding. It's really . . . . I can't describe how awful it is. It's really bad.

DOMUSH: Do you know professors that have had to find something else to do because they just can't get funding?

PANNING: I even know a Pew Scholar who gave up science.

DOMUSH: Oh, wow.

PANNING: She was in my class. She was in the class in behind mine. So, yeah. It hits all. It's just more often than not, what's happening now is it's almost forced attrition at the retirement, at the older . . . many older people or older researchers are not getting granting and deciding then at sixty-five or seventy, yeah, I've had enough, just retire.

DOMUSH: But there's no need to keep resubmitting grants when you're . . .

PANNING: Yeah.

DOMUSH: Is it particularly hard hitting for new faculty or is it kind of middle . . . ?

PANNING: I think it's really hard. It's pretty much bad everywhere. I mean when you're looking at cuts of 5 percent, you're hitting across the board. But it can be harder to get that first grant and it can be harder when . . . sort of, anytime. It's just hard. It's, yeah, so I think that's intimidating. Many people are staying in postdocs longer in hopes of getting a job when this all ends. You know, the hope is that the whirlwind and more money will fund to the NIH or changing, a change in government may change the NIH funding. So, it's not clear to me that that change in government will necessarily happen and if it does, how much money will actually be free and available to the NIH. Or how many years will more funds go into the NIH, take to turn things around, but, in fact, that's my advice to people right now who are in postdocs just stay as long as you can. Wait until this passes.

DOMUSH: When you talk to some of your friends or colleagues in Britain or Canada it didn't really sound like a situation though where kind of the grass is greener on the other side. But is there ever any temptation to say well, maybe if we go back to Canada it'll be easier?

PANNING: The only thing that would make going back to Canada, the only thing that would be easier about going back to Canada is I would have a lot of family support. If we went to the

right spot in Canada, which would have helped two or three years ago, but I don't know if that would help so much now.

The other thing that would bring me back . . . so, it wouldn't be scientific concerns, the kind of thing that would send me back is or take me back is family health issues. So, for instance my father has prostate cancer right now. I've been spending a considerable amount of time on the phone, going back to Toronto to be involved in the treatment. That would be much easier if I was there instead of here.

DOMUSH: Right. Is most of your family then in Toronto?

PANNING: My immediate family—my brother, and sister, and my parents. [. . .]

DOMUSH: In talking a little bit more about funding and kind of the international whatnot, you said that your husband went to graduate school in Britain and did his first postdoc in Switzerland.

PANNING: Right.

DOMUSH: And even though you came to the United States for your postdoc when you guys were looking for jobs or thinking about looking for jobs outside of the Boston area, at that time were you considering going really you know you said that you kind of looked in some larger cities like London or Cambridge?

PANNING: When I was looking for postdocs.

DOMUSH: Okay, I'm sorry.

PANNING: Right.

DOMUSH: Would you have considered at that time do you think looking for faculty positions maybe in Europe?

PANNING: So, by the time I was looking for faculty positions I was looking at areas that had good biotechs and so that my husband would be able to get a job. And I honestly didn't know

enough about biotech in Britain or Europe, but I had the feeling that it just, there wasn't quite as much there. So, it would have been difficult. So, in the end, I'm sure if we, if I had been unable to get a job on the first pass which was not . . .

DOMUSH: Not the case.

PANNING: Well, not that it was not the case, it wasn't even, it wasn't even a real search. I knew I had a job and I was just looking at, none of these things really came into play. I imagine they could if turned out differently, but it didn't.

DOMUSH: Do you have students or postdocs in your group that are from kind of a more international academic community than just the United States?

PANNING: I had a Canadian postdoc and she's now editor at *Cell,* but everyone else has been U.S., now that I think of it. The graduate students are all U.S. citizens, and the postdocs are—oh, [inaudible] is Chinese, but she's a permanent resident as well, so.

DOMUSH: I was just curious. I actually heard someone else in the hallway, when I was standing in the hallway, saying how they were about to start a job in, I think Montreal. And I was just curious if anyone came to you and said, you know, you have this, scientific experiences other places, is science different in these other places? [. . .] Or maybe that's a question that's just, you know, is science different in Seattle, [Washington] as opposed to Europe?

PANNING: Yeah. So, there's, so the day-to-day doing the experiments I imagine is the same just about anywhere. But the experience is probably different depending on who you're surrounded by, what you have access to that all makes it a very, you know - I can imagine being quite frustrated if you don't have a piece of equipment or even access to a piece of equipment to do what you need to do. And there may be different levels of prestige in different countries associated with being a scientist.

DOMUSH: Issues of having instrumentation that's . . . . I mean you could go many places in the United States and not have a community or not have the instruments.

PANNING: Right. Well, I was thinking for instance, one of the things that the Pew organizes is old pieces of equipment that we can't use anymore or graciously and gratefully accepted in South America and we send them down there. So, if you're trying to establish a lab in South America, which is one of the things Pew is trying to promote science in Latin America having

stuff that we discarded is a big plus. So, it's a completely different sort of level way of doing it, way of doing science because you can't do some of the things that people would be doing here. You just simply don't have the instrumentation. And, for instance, there's some things that are published in top-tier journals simply because they used the latest technology or the best instrumentation, right, and you'll never compete in that. It's like a portion of what you can do is cut off from you.

DOMUSH: Have you sent anything? Have you been able to send anything to South America or anyone in the department maybe that has?

PANNING: Yeah. I think a lot of stuff has gone from UCSF. The way that it works in my particular group is I didn't . . . when I joined I had people surrounding me that had everything, so I didn't have to buy anything, so it's not really anything to pass on. But when stuff becomes available and let them know.

DOMUSH: Wow. I've never heard anyone mention that. That sounds very interesting. And there must be so much kind of logistical networking information that's involved in transporting these huge pieces of instrumentation.

PANNING: And the Pew takes care of it. I don't know how many of them have been huge because a lot of the stuff we do is small. The instruments aren't always enormous. A lot of them are quite small, relatively easy. They're still big enough, but they're bigger than a bread box, but not as big as a refrigerator. [laughter] So, yes, there is some effort involved but nothing that FedEx can't do.

DOMUSH: Well, good to know. You mentioned just now that there are scientific publications that get into top-tier journals simply because they have the newest instrumentation and not everyone has that yet, and so they seem far ahead. I'm curious about kind of publishing and the publishing processing yesterday we mentioned a little bit about this paper you submitted to *Genes and Development*. And I'm wondering if you think in the same way for funding that you have to resubmit multiple times for an NIH grant if there's a similar process going on in publishing right now that you're having to resubmit or choose other journals?

PANNING: I think we've been pretty lucky because we generally have gone to one, maybe two, journals. So, depending on where you're going. If you're aiming at the very top-tier journals which I consider *Science*, *Nature*, and *Cell* there's a crapshoot. It really is a crapshoot every time. You don't know that you'll get the right alignment of three good reviewers to get a paper that might deserve to be or at least be competitive for one of those journals. And you're rolling the dice every time you take it from one to the other.

But when you go down to the less competitive, less tabloid maybe journals which is one way of thinking of it, then my experience has generally been that [. . .] it will go for review. You get comments. Every reviewer feels like . . . reviewers fall into different categories. There are reviewers that read your manuscript and say the data are interesting, the conclusions you draw from the data substantially defy the data and it's a reasonable article and is or isn't appropriate for this journal. And then there are the reviewers that feel like they have to make their special stamp on your story. [laughter] And that what they will always request at least one experiment or something. And if you're lucky you get more of the former, but in fact, it's my experience, having the experience of talking to people, is that the sort of standard is the latter. They'll always ask for something. So, invariably your paper's rejected. But it's the kind of rejection they say if you can deal with these reviewers' issues, we'd be happy to see it again.

And sort of I'm of the first reviewer set. I really feel like someone has written a paper and if the data substantiate the conclusions they draw and if the paper's interesting then I don't need to ask for more experiments to make it interesting or make it relevant for the journal, right. If it's a good match, then it's fine. So, I get very frustrated in dealing with the people who want to put their own special touch on it or ask for essentially a pile of experiments that end up in supplemental [data] and turn your supplemental data into an eighteen-piece add-on that doesn't really say anything that substantively change the original manuscript they put out.

DOMUSH: Right. I think from looking at your CV that you've published in *Public Library of Science* as well as the not-open-access journals. And from talking with other people in the Bay Area, I've been told that the Bay Area in particular is biased in some ways in favor of *Public Library of Science* and the open-access journals. And I'm wondering about how you decide if you're going to submit something to…

PANNING: An open-access journal versus a not-open-access journal. So, the decisions about where to submit manuscripts are done in collaboration with the people who are on the manuscript. And the graduate student or postdoc will have very different feelings about where the journal goes. [. . .] So the paper that went to *Genes & Development*, we probably could have sent it someplace a little bit better. But the graduate student and postdoc who worked on it, the postdoc who has a job in *Cell* and she's working as a manager, and the graduate student wants to go into editing, it really doesn't matter to their CVs where it goes. So, they decided to send it to there.

There's other people where they want a shot at the top journal, and if I think that the paper is a reasonable fit for that journal, I'll send it there. And there's other people I can convince that open access is the way to go because it just seems like the appropriate thing to do. So, everyone and because it's not just my career riding on this, it's the career of the person, people, authors on that paper, I take their feelings into consideration as well. If I had my way, I'd probably send a lot of, just about everything, to an open-access journal. But you know, *PLoS*

Biology and *PLoS Genetics* have…they're hard to get into now. They're . . . so sometimes you send it not there because you don't know that you get in to there. [laughter]

DOMUSH: You just said that if you had it kind of your way you would send everything to an open-access journal, and is that just because science should be more open?

PANNING: Yeah.

DOMUSH: That these should be available.

PANNING: Exactly. It's, right. These are, how are we going to disseminate ideas if only the people who can afford the three-hundred-dollar subscription to *Cell* can read them?

DOMUSH: Do you have any students that . . . you know, students or postdocs who feel really strongly that maybe I have a paper that could be submitted somewhere else, but I only want it submitted in an open-access journal?

PANNING: No. It's always the other way around.

DOMUSH: Okay. Is it a prestige [issue]?

PANNING: Absolutely. So, especially there's some places . . . in Europe I know of some institutes, but I suspect it's the same in the States where you or when they're considering hiring you, there's an actual equation where one *Science* paper gets you ten points. And a *Nature* paper gets you ten points. And a *Cell* paper gets you ten points. But, a paper in a lower journal doesn't give you any points and they decide who to interview on the basis or largely on the basis of where you published.

DOMUSH: And it's really an equation?

PANNING: It's really an equation, yeah. So, getting a publication in a top-tier journal can make it or break it for postdocs when it comes time to look for a faculty position.

DOMUSH: Is there room . . . . I mean, *Public Library of Science* is a relatively new publication compared to so many of the other journals. Is there room for it to gain prestige or gain that . . . ?

PANNING: I mean it's already gained prestige. It already went up. So, I don't know if it's ever going to get to what the *Science* and *Nature* are. So, *Science* and *Nature* are basically tabloids. The people who read them . . . the science is important, but there's science news in there. So, they're [is a] journalistic aspect of them that something that publishes, sure *Science* just a basic research paper doesn't really have, and I suspect that may stop it from getting quite as prestigious. But it's certainly getting [there] It's amongst the top journals in biology you get today, so. I think it's certainly doing just fine. The model has worked.

DOMUSH: But maybe the equation that that type of equation of *Science* equally ten points and *Nature* equally ten points that maybe that's what's flawed.

PANNING: I would say that that's certainly flawed. That some people, very talented people who do good work that's in really solid journals, but just they didn't have the dumb luck, forces didn't align the right way when they submitted it to *Science* And they didn't get the three reviewers that they needed, that that's just, that just happened. There's nothing, especially in this environment where everyone…the review process is almost adversarial, right.

DOMUSH: When you call it adversarial is it as political as the way you described NIH funding where someone can just say, "You know, this may be good science, but I just disagree kind of fundamentally with it?"

PANNING: No. So, what will kill you in a review is, the paper's fine. The science is fine. It's just not interesting enough for this journal. And you can't fight that. So, you have to, so, when you're trying to get into these journals you have to make a case that what you've done is in fact interesting and new and innovative enough. And some stuff clearly is, but you're competing with so many people who are also doing stuff that's new and innovative and why would yours get one of the ten coveted positions this week in this journal? All it takes is one person to say it's very good, but it's not that interesting or . . .

DOMUSH: Not for here.

PANNING: Not for here. And it's virtually impossible to fight that. So, you really need to get at least two people expressing strong enthusiasm for your manuscript to get it in. And different people have different agendas, especially when you do research that [. . .] crosses more than one

area. So, if you do something that brings two fields together, you'll often get reviewers from Field A or Field B and they have very different agendas. They look at it differently. They have completely different sets of experiments they would like to see done. They have different focuses they'd like to put a manuscript. And it can be very difficult to balance those two together to produce a product that they're both interested in, that both groups think is interesting.

DOMUSH: Is it sometimes a terminology issue? I mean, I would imagine that if you're involved in the scientific field, it's got crossing between Field A and Field B or Discipline A and Discipline B, that a lot of times the terminology and the way you phrase things is slightly different?

PANNING: I think that because . . .

DOMUSH: And I don't know if that plays into how that the article is written.

PANNING: I think that can certainly play into it. But I think things are just . . . maybe it's easiest if I give an example of what we're doing right now. So, we found that splicing of this particular RNA is playing a role in regulation of which chromosome will be active and which one will be inactive. So, there's splicing regulation. And we identified a splicing factor that binds that RNA. So, now this paper is interesting, that we're just sending out to, have just sent out to *Genes and Development*, is interesting to the X-inactivation community, the people that think about what's going on to allow one X to be active and one to be inactive. But it's now interesting to the splicing community, particularly the people that are interested in this particular splicing factor. The kind of experiments that the splicing community does is move the RNA that binds the splicing factor into different genes, and show it still works as a splicing regulator. The kinds of experiments that X-inactivation people do are delete it, mutate it, look to see what effect it has. They're completely different mindsets, completely different kinds of experiments. And one reviewer I know will tell us to do one set of things. The other set of reviewers will tell us to do the other set of things. It's like they'll look at the paper with completely different world views and then try to produce something that's interesting enough for both of them.

DOMUSH: It's like walking a tightrope.

PANNING: Right, exactly. It's like basically getting enough data in there that both will be made happy. So, another option would be just to separate them all out. But then you don't . . . it doesn't work either because the conclusions and the discussions and the new ideas you want to raise require that both pieces of data be together. One idea would be to publish one first on its own. But then that would have to be a splicing paper. And I don't, no one in my lab wants to do

a bona fide splicing paper, right. So, it's kind of a, so no one in my lab wants to do a bona fide splicing paper and doesn't want to go through all the hoops, hurdles, and [more] hoops that people would ordinarily do if we were a hardcore splicing lab to put this paper out. Because we don't think that's so interesting.

DOMUSH: So, what happens if . . . . I mean, if you have a reviewer from the splicing community and a reviewer from the X-inactivation community and they do have these very opposing viewpoints, is it then up to the third reviewer to . . . ?

PANNING: The third reviewer could be from either community and put things . . . mix things either way. If you have a talented editor, she'll see that . . . she'll see. It really . . . that there could be another splicer and then they could require lots of splicing experiments. And what we would do is do a ton of the splicing experiments and resubmit it. and hope and then try and address both sets of reviewers' issues. It's really again, it depends on the reviewers. You just don't know what will be asked of you. But normally if something is asked of you, you do it and you resubmit. That's kind of the [system]. But you can just never predict in advance what will be asked of you. And if what you packaged together, you've done a good enough job that it'll appease . . . the X-inactivation part is sufficient to appease, making the splicing people interested and the splicing aspect of it . . . you just don't know.

DOMUSH: Well, especially in something . . . in the way you describe it, if you tried to separate them it wouldn't necessarily be a complete story.

PANNING: Right. It wouldn't fall together the way.

DOMUSH: Right. It wouldn't necessarily be as interesting. Since so much about publishing sounds so arbitrary, you know, [it is] totally dependent on who your reviewer is or kind of what they might ask of you that day, and then so much of funding is dependent on have you published this preliminary data—is it better from your perspective to just try and get a paper out? You know, do what they recommend, not necessarily write a rebuttal and say, no, I think that this is a complete research story and just try and get that out there so that when you do submit your next grant it's there.

PANNING: Published paper. By the time you've gone through one round of review, if the experiments they require are not unreasonable and not terribly time-consuming, it's far faster to do them then send it back. Even on occasion, I've had manuscripts that haven't gone back to review again. We've done the experiments they requested, written a cover letter saying we've done them, addressed whatever issues we could without experiments and the text changes requested and resubmitted it and two days later, I've got an acceptance letter.

So, it really depends on other times that there's more experiments; then you have to kind of gauge the interest of the editor, and if it looks like they basically say that we can accept this now if you do the experiments that justify the reviewers we'd be very interested. So, then you know that as long as you do what they say, what the reviewers requested, if it's doable, you'll get it in. So, again, every instance is different. There's other times you just . . . there's no way we're going to be able to turn into a paper that will convince this reviewer, so then what you do is you take that as a learning experience and make changes. If it was a misunderstanding or if it was, you can proactively deal with the issues that the reviewer has when you send it to another journal. But basically, if you've gotten a review once at one journal, the fastest way of getting it in is probably just to stick with that journal, depending on how much is required. If an enormous amount of work is required then just drop it a couple of tiers, and if the same reviewer sees it he or she will say well, it's fine for this journal, but I wouldn't have let it into [the higher tier journal].

DOMUSH: Have you ever had an experience where you get reviews back where their suggestions are actually really helpful? They're not just kind of time-consuming.

PANNING: Yes. I certainly had suggestions that were . . . yes, that were helpful. And it's more often than not they're just time-consuming, but we've had helpful ones, ones that I think changed the paper, made it better. In no instance has it completely undermined what our initial model, what we thought was going on that had made it . . .

DOMUSH: But strengthened it maybe.

PANNING: A little bit stronger. Yeah.

DOMUSH: Okay. Sorry. I lost my train of thought again. Just completely changing subjects, we touched very briefly on the idea of students either choosing to go into science or choosing to not go into science. We were discussing the outreach work that you do at your son's school. And what I wanted to ask you about was if you have any sense for kind of the public perception of science? Just kind of, not necessarily someone with any scientific background, but just the average person out on the street and that they might not necessarily understand what you do or understand what the outcry amongst scientists is about the funding situation, and if it's worth it as a scientist to do something to help that person understand?

PANNING: So, one on one I'm certainly happy to explain why I think scientific research is important to the United States and to the world at large. I hope that I can make someone understand that. Sort of in a bigger picture, do I think we need a science ambassador? Carl

Sagan was an example of someone who brought science to many people and made them think it was quite interesting, and I think also helped people understand the significance of it. I think in Canada, there's a guy who does an equivalent of a PBS [Public Broadcasting System] show, whose name escapes me right now.

DOMUSH: [David] Suzuki?

PANNING: David Suzuki. I think every Canadian knows David Suzuki, and they love his science shows and he branches . . . he's a biochemist at the University of BC [British Columbia], but he doesn't have research anymore. His life is completely, basically, I think, as a science ambassador and doing shows and, you know, basically keeping . . . bringing different aspects of science into public awareness. Like, there's no one like that in the United States that I can think of. We could desperately use somebody, and it would be nice to have someone charismatic and interesting to fill that role, but I have no idea how you would go about doing it. And I don't know if many of the scientific organizations in the United States are more focused on kind of lobbying; certainly the National Academy of Sciences is essentially a lobbying group. Is there a better, more clever way of doing this by making science more popular, less scary to people, trying to uncouple the religious concerns, the concerns of many religious people about the stem cell research that's going on today, I don't know. I think it would be great if we could find a scientist who was charismatic enough and who could explain themselves, explain science well enough to most people that they were less afraid of it, intimidated by it, and could see the importance of it.

DOMUSH: I guess in thinking about kind of the role of science ambassador. You know, you're very busy and you're not going to take that on right now. I mean you know it's wonderful to go into your son's school, but you are not going to . . .

PANNING: It would have to be a full-time job. It would be very appropriate to begin to train people for this job. And I don't know if working . . . so I think we need the credibility of someone who's actually a working scientist. But it needs a working scientist who can take the time. What was his name? [Kenneth R. Miller] The man who really fought the Roman Catholic [viewpoint] and is a professor at Brown University who does basic research and who wrote books kind of debunking, [. . .] scientific creationism, remember that was all the rage a few years ago.

DOMUSH: Yes. I'm not going to be able to think of his name either. We'll come up with it later.

PANNING: He actually spoke, I think, at a Pew meeting one of the years I was at a Pew . . .

DOMUSH: Right. And I've heard a couple of the other Scholars mention it, but I'm not going to be able to think of it right now.

PANNING: His name escapes me right now, but he was kind of briefly there. I think it never really took off, and I don't know, I guess the problem is how do you even, where do you even put such a person? Do they appear on *Good Morning, America* once a week with some interesting science? I just, PBS they have science programs, but it's not, maybe what we need is someone on some incredibly popular show like *Oprah.* Shows I don't even watch because I don't have time. [laughter]

DOMUSH: There, you know there are so many senior professors that maybe their labs are kind of winding down.

PANNING: Might be appropriate for something like that. Or maybe, someone young and dynamic would be appropriate as well.

DOMUSH: It would have to be someone dynamic, have to be someone that appealed to a general audience. You brought up this issue of kind of stem cells and religion. And do you ever worry that . . . now I have no idea balancing two very busy jobs that you have, how much news you get to listen to . . . but do you ever worry that perhaps the media is creating some sort of controversy where maybe if there wasn't a need to create a new news story every minute because there's twenty-four hour news, that maybe not as many people would really be that concerned about an issue? So, I think about evolution debates in schools, and maybe it's not that much of an issue and the media just needs something to talk about, and then it seems like a really big issue.

PANNING: Because they're . . . right.

DOMUSH: You know, or another example is there have been a lot of articles that I've read recently about global warming debates in the media. And about the media's effort to be showing both sides of the story, perhaps some media outlets show the other side too much. That there's a minority of scientists that don't think global warming exists and . . .

PANNING: Or they don't think AIDS is caused by HIV and those gets as much air playing as much . . .

DOMUSH: I mean, is the media to blame then for some of this kind of anti-science feeling?

PANNING: I don't [know] or are individuals to blame because they don't seek, they don't critically observe the media and think about . . . there's a lot more than the media. There's the blogs. There's the web. You can go to any and you need to critically evaluate what you've been told. And that involves figuring out what consensus is and whether this one person who happened to be on FOX News is an outlier or not, I don't know. I would say it's difficult because it's up to the individual to learn as much as they possibly can and how much time do you have to actually learn. Maybe some selectivity into your media outlet is appropriate. But other than to imperil the time . . . and maybe I'm making the wrong choice too.

DOMUSH: Do you think it's also an issue of . . . . I mean, no matter what if someone's listening to news there's going to be some type of bias even if AIDS and HIV or global warming, even if we're presented both sides of the story, those both sides might not necessarily be equal sides. So, if it's up to the person to make some sort of educated decision about what they've heard, does that then assume that that person's had some sort of scientific education?

PANNING: That they will take the time to educate themselves or that they're in a position that's . . . that may not be a fair assumption.

DOMUSH: Not everyone understands the scientific process or even that the scientific process is about progress. You know you were talking yesterday about this forty-year-old model of X-inactivation, and while I would bet that the average person on the street doesn't have any idea what that is, this idea of a fact in forty years . . .

PANNING: Can change.

DOMUSH: Can change, you know that's not what people think facts are.

PANNING: It's difficult to process. So, I guess we're back to that charismatic individual who can explain that to people and explain that science is like real life. Things change as you get new information. Your perceptions or your ideas change. Basically, it always goes back to explaining why what we're discovering is important for their day-to-day lives, as well as the functioning of the country.

DOMUSH: You said yesterday that when you were going through university and graduate school that your parents just really didn't . . . they didn't get it.

PANNING: No.

DOMUSH: Have you had any luck of being able to explain to them better what you do?

PANNING: So, they're interested enough in the research and like it when I take the time to explain it to them. They just don't know why. And they're much happier now that I have children and married, right. So, now they can tolerate it because the stuff that was important to them is out. So, now I think they're, but I think they were very confused for a long time. So, my dad loves reading popular science books and magazines, and he always questions me about what's going on. I'm in the school, my son's school all the time, and people talk about what you do, and they want to know what I'm researching. So, I've gotten quite good at explaining it in a way that someone without an enormous amount of scientific background can understand, and it still makes it seem interesting. And more scientists need to be able to do that. I think in many grants nowadays they ask for a lay person's statement, but that still turns out to be filled with scientific gobbledy-gook because you just can't imagine when you're writing a lay person's statement that you're actually talking to some guy you meet on the street and try to explain what you do.

DOMUSH: Right. They're not bringing in lay people into NIH to judge your grant. Do you ever bring up with your students this idea of it being important for them to be able to explain what they do? You know, kind of at a dinner party, if someone says what are you doing? You know, I can tell you from my own experience—many of my friends are chemistry PhDs—people look at them, and their eyes glaze over and say oh, and then they kind of walk away. [laughter]

PANNING: Yeah. You have to make it make sense in the context of someone's real life, which I don't know if it's harder with chemistry. Do I ever advise my graduate students that that's important? I can't say that I ever have. I talk to them about having done it with other people, so if anything they may learn by example. There's enough people here . . . there's enough of a science education . . . it's part of the culture. So, it's maybe not something that you need to explicitly discuss. Also, a lot of the graduate students go on to science writing courses. They're involved in science outreach. That's one of the things that the graduate students do, if they don't want to do basic research, it's pretty common here.

DOMUSH: I guess I'm wondering if you bring it up with your students because you had said you had students that have gone into editing and writing. But I'm wondering if that person, that science ambassador is just sitting in a lab somewhere and doesn't even know . . .

PANNING: Doesn't even know.

DOMUSH: Doesn't even know that . . . doesn't even realize because they're so involved that that's something that's needed.

PANNING: Right. I wonder if, even if the scientific community, I'm sure if you brought it up there would be an acknowledgment that it's needed. What I think isn't certain is how to manufacture the opportunity; how to do it. So we certainly…we have the systems that we work well in, like the National Academy of Sciences and the Federation of Scientific and Experimental Biologists and all sorts of these groups. But how do you get the talented and charismatic individuals from those groups to talk to non- . . . to talk, right, what's the right forum? Should we get interviewed on NPR [National Public Radio] when we publish something that's reasonably hot? And sort of you get your fifteen minutes. What we almost need is someone who . . . it's not just fifteen minutes, it's a lifetime endeavor, and they need to be in an essentially high-profile position that people sort of know who they are and what they're trying to accomplish. I guess, in the example of Canada it worked because it's a small country and because the Canadian content of what's on television stations probably everyone watches David Suzuki. About 30 or 40 percent of the population watch David Suzuki. I don't know if that's . . . if you could engineer something like that in the States. You would need someone for FOX. As well as someone for, right, I'd . . .

DOMUSH: Well, one person that I was talking to made the interesting point that you know when Carl Sagan was on PBS [Public Broadcasting System] there were fewer channels. You know there are . . . . I flip through channels at home. It takes a while. There are a lot of channels. And there's a lot of options for people to say why would I watch this right now?

PANNING: Why would I watch this? Which is why I think it might need to go over something like *Good Morning, America*. You know, something that is done in five-minute bits that are palatable.

DOMUSH: In your opinion, you know, there's a lot of scientific endeavor that makes a five-minute news clip here or there, but they're not necessarily the basic science things. So, a lot of pharmaceutical research that went wrong. It's news. So, you hear constantly about the Vioxx law cases and you hear if someone thinks that they have a possible vaccine for diseases A, B, or

C. Would it help kind of the general public if instead of those aspects of science maybe people were talking about really basic research, you know, kind of the publications that go . . . ?

PANNING: When they solved Fermat's Last Theorem, that was all over the news. It was a basic mathematical postulate that had been around for a long time and no one had been able to solve. So, there was the novelty of it. But no one actually explained the details of the solution to anyone, but people were interested and excited in it. So, you're right, if it could be other scientific—basic scientific—achievements could be turned around into something that seems interesting. But I guess the general interest here was something, a problem that had been around for a while that no one had solved. That applies every day to what scientists do.

DOMUSH: Right. And then maybe more people would understand why things take so long, why it needs so much funding. Or what it is if they understand that all of these really major breakthroughs that kind of do you get the five-minutes in the news. But for all of that there's so much day-to-day work.

PANNING: I think the other thing is the NIH budget, of the United States budget is nothing, really. We're not throwing . . . we're not putting that much money into it, and even if we doubled it we wouldn't be putting that much money into it, so there's . . . . I don't know if there's the general understanding about it, as well. It could be, I mean for most people that's not, it's not an issue. It's not. I don't think people perceive that we're spending a lot of money on research. I think you're right there is a perception that it's moving too slowly. Richard [M.] Nixon said we would cure cancer in the next five years, and we haven't yet. People are still dying of cancer. Why? If we can find someone to explain to people that cancer is not one disease it's a pretty complicated one, and we have made some pretty major breakthroughs. Also, it's sort of the time-scale of understanding it, developing the drugs, getting them to market is about twenty-five years. And we are now beginning to see stuff bearing the fruits of research that was started twenty-five years ago. I don't know how many people really understand that.

DOMUSH: I mean, because of a background in chemistry, I've been able to explain to some of my friends when they've expressed dismay again things like Vioxx lawsuits and things. And they say well, don't do they do testing? And I said well, of course they do testing. You can't, you know, you can't keep a drug in clinical trials for twenty-five years. You know, there's no money and there's too much demand for it. And then they kind of think about it and say well, no, you can't. And there's always going to be something. Hopefully it's not something terrible.

PANNING: All the estrogens for women going through menopause. Who knew that they would cause heart disease or increase your chance of having cancer? But they provided a lot of relief for many women for many years. And I'm sure some wouldn't give it up even though it

increased their risks of heart attack. Sometimes you just need to know. I know people who . . . Vioxx—if they could get it today they still would.

DOMUSH: They still would, yeah. So, maybe it's just that we really need to find the science ambassador.

PANNING: I think we need to find a science ambassador, and we need to find a forum for the science ambassador to speak in. And the ambassador could also be talking to the government or talking to the people. So, there is no one talking to the people. There are potentially science ambassadors talking to the government; the effectiveness of that varies from government to government. And so, I wonder if the most effective thing would be to talk to the people and there's a groundswell of support amongst the people and assist.

DOMUSH: Do you have any sense of whether or not some of these debates about science and religion or kind of what science should be done? So much of the public seems to think that stem cell research should no longer be conducted even though they might not really understand anything about it, and then maybe the government goes and restricts more aspects of that. Do you have a sense if—from friends in Canada or Britain—that there's a similar kind of disconnect between the public perception of science or the public having an influence on what science has done?

PANNING: They just think we're nuts. [laughter] I mean, I think that summarizes it. They think there's a perception of this, and I think that Americans being self-centered and not terribly educated that unfortunately recent political developments tend to . . .

DOMUSH: Nothing has changed their mind about that.

PANNING: Yeah, exactly. I mean, you know from simple things like understanding geography . . . countries outside the United States. The perception of the average American couldn't name the capital of Mexico, much less point it out on a map. So, it's all part and parcel of the same thing. So, that would be my call. And they think it's funny.

I had one meeting I went to was in, it was the every decade X-inactivation meeting and it was in the Institute Curie—so, it was in Paris [France]. And they had their closing gala in the library, the Museum of Natural History. And in this beautiful big hall where they showed evolution of like so many different organisms—you know, the big stuffed animals, but showing the sort of development. There was no doubt, right, that evolution happened. It wasn't even brought up. And there were people there just, ha-ha, the United States. So you guys think that you all just came out of nowhere, you know.

DOMUSH: It was kind of a laughingstock in some ways.

PANNING: It's seemed like a laughingstock, right, that a good portion of the population felt this way, and there's not really anything you can do about it except try and reach that portion of our population and make them realize that their beliefs are not necessarily at odds with what science tells us.

DOMUSH: Right. Okay. Well, going again kind of switching topics, the Pew Biomedical Scholars Program is a biomedical program. And I'm just curious if you have a definition for biomedicine?

PANNING: Do I have . . . . I've never thought of it deeply but I would say any biology at all, any biological or chemistry . . . anything that can be applied directly or indirectly to medicine and so, very broad.

DOMUSH: Okay. Great. Well and that's something, I think that's how Pew sees it. One of the last things that I want to ask you about, of course, unless you have anything further that you want to add, is where you would like to see your lab's research going in the next handful of years. And I know that depends on a lot of things; I'm sure funding is obviously one of them, but if you could just look at the research and kind of where the research is going and where you want it to go.

PANNING: Where do I want it to go?

DOMUSH: Or, where do you think it may lead you?

PANNING: So, very specific or big.

DOMUSH: Either way, you can be specific. You can be big. You can do both.

PANNING: I guess I would hope that we have a better understanding of the kinds of things I talked about yesterday. The things that we don't even have handles for, that we don't even have any molecules or proteins or RNAs or anything involved in yet, I would like . . . . I would hope

that we made some steps forward and getting handles or use the handles that we found recently to actually make some steps forward into understanding mechanisms.

DOMUSH: Specifically, though.

PANNING: Like very specifically.

DOMUSH: You can be as specific as you want.

PANNING: I mean, I would hope that we understand how the two X chromosomes talk to each other, so that they can make these random and mutually exclusive, random decisions and get mutual exclusive bates. And I would hope we understand how the non-coding RNA plays a role in that is also, simultaneously, then used and silences the chromosome. So, what's the connection between the silencing and the chromosomes talking to each other? The activity, the silencing activity of this RNA, what is actually shutting the chromosome off, how is that same activity being used to . . . involved in making these random choices and establishing mutual exclusivity.

And that's part of the bigger field of understanding what non-coding RNA are doing, which an emerging field right now. And we're getting a lot more genomic information, understanding what portions of the genome are read by the transcription machinery. It's becoming clear that there are a lot of these RNAs. So, RNA is the messenger. A lot of these messengers that don't make proteins, they just make RNAs and the RNAs themselves do something, usually to regulate gene expression. So, this is, the amount of them there was completely unanticipated. There's a huge number of them, and that's a big burgeoning field right now. And they've been identified but no one knows what they really do, and I'm hoping that we can make some, contribute some, contributions to understanding what they actually do at a molecular level.

DOMUSH: Great. Well, unless there's anything else that you want to add, I'm not sure that I have anything else to ask about.

PANNING: I can't think of anything to add, so.

DOMUSH: Okay, great. Well, thank you so much for your time and really appreciate it.

PANNING: Thank you. It's been a pleasure.

DOMUSH: Good.

[END OF AUDIO, FILE 2.1]

[END OF INTERVIEW]

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