January 18, 2007

I. Early and Family Life; Leaving Austria for England and the United States

Q: It is January 18th, 2007, and I'm Andrea Maestrejuan with Liane Brauch Russell for her interview for the UCLA Human Genetics Oral History Project. We are at her office at the Oak Ridge National Laboratory[1]. I'll start at the very beginning and ask you when and where you were born.

A: I was born in Vienna, Austria in August 1923 and lived there until March of 1938. So, I spent my first almost fifteen years in Vienna.

Q: Tell me a little bit about your parents. Were they Austrians as well?

A: Yes, they were. My father was a chemist. I think he was more what we would now call a chemical engineer, but he was a chemist. By the time I really became conscious of what he was doing, he was mostly at that time representing a bunch of chemical companies, non-Austrian companies. He was their representative in Austria, particularly ICI -- Imperial Chemical Industries -- I remember was one of the companies he represented.

Q: That was a British firm?

A: That was a British firm. That had something to do with my subsequent life too. My mother was not a professional, she was a housewife. A little later, she had a strong interest in music. She sang and took singing lessons. And a little later in life, after we left Austria, she studied to be a speech therapist, but she never really, I think, did it professionally. So she was essentially not a professional person.

My father always wanted me to be a chemist, so I was brought up with that in the back of his mind. He loved the story of Madame Curie[2]. He was always telling me that. Also, because I think he may have wanted a boy, I was sort of brought up as a boy. I eventually did have a brother, but I was very consciously raised away from female frills.

In fact, I never wanted any dolls, I never had any dolls. I had animals, I had lots of animal toys, and I was very crazy about animals and very strongly convinced that they should have the same rights as people. I know when I was a little kid and we went on a hike, I would walk in front of everybody and get all the little critters out of the way so people wouldn't step on them. So that was my relation to animals.

I don't know whether I showed any strong scientific interest as a child, but I know that my father told me lots of stories of what scientists had done. Our education, again, it was pretty rigorous. It was at a much higher level of learning at an equivalent age than what they would get in school here. Elementary school went for four years, and then you got into high school, so you got into high school at the age of ten, ten or eleven.

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[1] Oak Ridge National Laboratory; A Department of Energy science and technology laboratory in Oak Ridge, Tennessee founded in 1943. It was initially created to separate plutonium for the Manhattan Project. See http://www.ornl.gov/ornlhome/about.shtml for more information.

[2] Marie Curie (1867-1934); A famous Polish physician and chemist who was awarded two Nobel Prizes, one for physics and one for chemistry. She discovered two elements and worked extensively on radioactivity and isotopes. See http://nobelprize.org/nobel_prizes/chemistry/laureates/1911/marie-curie-bio.html for her life story.

Q: So did you start at the gymnasium[3] before you left for the United States?

A: Yes, I started there -- I'm not sure, I would have been about ten, so it was probably around 1933. So my last -- I think I had five years, almost five years at the gymnasium before I left. You know, that started with pretty rigorous science and geography and all these things.

Q: What were your parents' expectations for you, especially in a system in which you kind of have to know fairly early on what educational --

A: I think my father wanted me to be another Madame Curie. (laughs) I mean, he always dreamed that I would go into some kind of science. But he didn't push me in any way.

Q: Did he bring anything home in terms of chemistry to demonstrate to you chemical principles or the kind of fun tricks --

A: One thing I remember, and I don't know why, is the term thixotropic[4], and I don't know how you spell that. He showed me that yogurt, when yogurt sits, it separates into sort of a fairly hard phase, and the liquid, and then you stir it. And that's a physical chemical property that's also exhibited, for instance, by certain suspensions of sand, and so on. So I remember that as a demonstration of something scientific. Physical chemistry, I guess.

He always said that he wanted to publish a book on the chemistry of cooking, the general principles of changes in steak that occur, and so on.

Q: And did he help in that regard around the house?

A: He was the worst cook in the world. Very famous family story of how he tried to make scrambled eggs when my mother was sick and ended up with a totally inedible scrambled egg. He was not good around the house.

Q: And had your father come from a well-educated background?

A: No, I don't think they were particularly well -- I mean, I think they were. They had sort of normal middle-class education, but they were not in science or anything like that. I don't even know for sure what my grandfather did because he was dead when I was a kid. He had died sometime before.

Q: Did you receive any music training?

A: Yes. I had piano lessons. And we went to virtually every single opera I know and a lot of concerts. The schools were very good that way. They had subscriptions at reduced prices for students. You had to go certain days of the week, but every

[3] Gymnasium; Equivalent to high school in the United States, it is the secondary part of one's education in some parts of Europe.
[4] Thixotropic; Certain fluids become less viscous when they are shaken or disturbed. Such a fluid is thixotropic.

Wednesday, or whatever, you could go to a Philharmonic concert. So we had a lot of music.

Q: You said you have one sibling?

A: No, I have two. I had a sister who was two years younger than me and a brother who was ten years younger than me. But neither of them went into science.

Q: Okay. And did your sister get the same kind of Madame Curie --

A: She may not have gotten it because she exhibited very early a strong artistic sense. She was a wonderful -- she could do caricatures and all sorts of things. She even caricatured people's body motions and things like that. She was very artistic, so I don't think she was exposed to that.

Q: And what did you think about getting involved in the sciences? How much thought did you give to --

A: To what it meant?

Q: Yeah, and taking the science track, or the higher --

A: I didn't really think about it. I think I was too young to think what it really meant in terms of what kind of a career you would have. I just never thought about that. I just sort of took it for granted that I'd be taking a lot of science courses. And I did not have any strong artistic abilities. I wrote well, but not like my sister at all, so we started going on different tracks.

Q: And what kind of religious traditions did you grow up with?

A: I grew up with very few. My father was a total nonbeliever and we never went to anything. I think my mother was wishing and sort of blaming him for bringing us up in a fairly non-religious way. I don't really remember going to any kind of religious services much.

But in school -- I don't know how much you know about the political history of Austria, but around 1933 -- before 1933 we had a Social Democratic[5] government, and in '33 there was a *putsch*[6] and a guy by the name of [Englebert] Dollfuss[7] became the chancellor, a short guy with a club foot. He was Christian Democrat[8], and after the Christian Democrats took over the government, there was a lot of religion introduced into the schools. I think we had to have religious education in the schools.

Q: Primarily Catholicism?

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[5] Social Democracy; A left-leaning political philosophy which stresses political action over economic determinism. Historically this philosophy originated from the socialist movement.
[6] Putsch; A German word meaning a sudden attempt to overthrow the government.
[7] Englebert Dollfuss; The Chancellor of Austria from 1932 to 1934 who lead an authoritarian style of government based on conservative Roman Catholic ideas.
[8] Christian Socialist Party; An Austrian right wing political party from 1893 to 1933 that drew most of its constituents from the rural peasantry.

A: I would say the great majority of the kids were Catholic, but they would ask us at the beginning of the school year, or maybe even the beginning of entry into whatever school we were in, we had to all get up and say what religion we were.

There were two kinds of Protestants. I mean, it wasn't like Presbyterians and Episcopalians and stuff, just two kinds. They were HB and AB. AB stands for Augsburger Bekenntnis and HB stands for Helvetische Bekenntnis. So that was Calvin[ism] vs. Luther[anism], and those were the only two kinds of Protestants. Then there were the Catholics and they were all just one. And then there were Jews.

At that point -- I really don't know the ratio, but the great majority, probably 70 percent, were Catholic. Maybe 25 percent were Protestant, one or the other, and then 5 percent maybe Jewish.

Q: And after '33 was there a point in which your parents had to declare a religion for their children?

A: Yes. We had to -- I was Jewish, but I knew very little until we had religious education in school. So that's how I learned something. Also, my mother was always very, I think, secretly religious, sort of almost resented that she couldn't do it. So she would go to relatives that had various observances. I had a cousin to whose house we'd go for things like Passover and -- what was the thing in the fall ?-- the harvest festival[9], Purim[10], and things like that. That was really the extent of my religious education: what I got in school and through that cousin, and also some friends.

Q: So tell me what it was like to grow up in Vienna in the late twenties and early thirties.

A: Well, it was physically wonderful. Also, the countryside was so close and so beautiful. We would take long vacations. The summer was pretty long. The school year, I don't know when it ended, there was like two or three months of summer vacation. We'd often rent a house in the country somewhere, often fairly close to Vienna so my dad could commute. Sometimes if it was a little farther, he'd spend the week in Vienna and then he came out for the weekends. So I grew very, very fond of the countryside.

He was quite a hiker, so even during the winter we would go out into the Vienna woods and hike, or to some nearby mountains that were just an hour away by train. I was really brought up a lot in the outdoors, which explains something about my later life, I think.

Q: In your circle of friends, were there other kids who were -- particularly other girlfriends -- who were being kind of trained to go to college and to do science?

A: Yes, at my schools. I had forgotten that, but a friend of mine reminded me. I knew that my high school - my gymnasium - was all female, but I couldn't remember about my elementary school, believe it or not. I just couldn't. But this friend said it was, too. So I had a totally non-coed education the whole time I was in Vienna. I don't know

[9] The festival of Sukkoth celebrates the harvest with the construction of “booths”, temporary shelters which are then used as an outdoor room, often the site of a family meal.

[10] The festival of Purim celebrates the deliverance of the Jewish people from a plot to destroy them. The full story is told in the biblical book of Esther. Today the festival celebration includes dressing up as characters in the story, feasting, and the exchange of gifts among other things.

whether that made -- but I think that was apparently more or less the rule and the norm. My mother was concerned that we should meet boys the proper way, so we had to go to dancing school, and that's how we met boys, in dancing school and stuff like that.

Q: And how well do you think your school in Vienna trained you in the sciences?

A: I think my science courses were very good. They didn't really train me. I don't think I even took any biology classes. I remember chemistry and physics. And I don't know if you can call it a science, but I had a lot of geography, some of which was geology. But I don't remember any biology, so I was very excited when I had it later. I always looked down on botany because I always thought that all you have to do is identify the flowers, which I didn't think was very scientific, just to learn the names and to identify them. So I kind of looked down on that, which I'm very sorry [about].

Q: Well, Vienna had been a large center for medical training, the universities there. How much thought did you give, before '38, to where you would go to the university and what kind of things --

A: Not before I left. I think maybe if I had stayed on – because I had three more years to go in high school, three and a half more years to go. So in the first four and a half years, I really -- it was very far from my mind what the university was going to be like. Then, of course, things started to really fall apart in terms of the political situation, which influenced the social situation in the schools.

I didn't really have a great circle of friends, and the friends that I did have, I had two very close girlfriends and we would spend after school at each other's houses. I abandoned those pretty early. I broke up with them because they got a crush on Clark Gable, and the whole conversation, day after day, would be -- and they would collect his pictures and they'd collect all sorts of stuff from him. So I quit that, and those had been my closest social friends. I think I spent a lot of time not with any friends.

Q: What would you spend your time doing then?

A: What would I do?

Q: Yeah.

A: Actually we got a lot of homework. The school day ended at one o'clock, so it was a morning. It was eight to one, and then we had tons of homework in the afternoon. The other thing is -- I don't know whether it was in high school, but certainly before that, we had a companion, a lady that would come in and took us for walks or took us to museums, and so on. We were supposed to speak French to her, so that was my French training. And we had a succession of these mademoiselles, I think they got a pretty horrible life out of us. We really sort of tortured them, (laughs) my sister and I. There was one we convinced we would like to play cards in French, so we spent the afternoon playing cards.

Q: Well, during the thirties, things were changing rapidly, just the rejection of the Social Democratic governments that had taken control after World War I. And then the rise of anti-Semitism and fascism in Europe. In what ways did your family experience these political events? Was it talked about at the kitchen table? How did anti-Semitism enter your life?

A: My own [experience] was through school. I really don't know how my parents -- my father, for instance, whether he encountered much in his business. I know that he had working in his office a young man who had lived in one of the new housing developments that was put up by the Social Democratic government, and he was thought to be a Social Democrat. With the Nazis [arrival], he became a prime Nazi. I mean, that kind of thing.

But really it didn't start until they invaded, until they marched into Austria, and that was in March of 1938. That totally changed our lives. In fact, I was the person who was responsible for the fact, indirectly, that my family didn't leave that very day because they had planned to take the car and drive to a place called Bratislava, which is on the border of Hungary and Czechoslovakia, very close, only an hour's drive from Vienna.

We were going to do that, just pick up and go. But we couldn't because I had just become old enough to have my own passport. Before that I had been on my mother's passport, and I think at fourteen you had to have your own passport. I had applied for one and not got it. So here I was without a passport and they couldn't leave. It turned out to be very, very fortuitous because a lot of people who did that got picked up right at the border when they were trying to get out.

So we stayed. My dad's business was taken over by a guy who was an SS [Schutzstaffel][11] man, and I very well remember when he came to the apartment. My dad was at work, and he came to the apartment and rang the bell. I opened the door and there was this -- he was about six foot four or something. This six foot four guy in a totally black uniform came looking for my dad.

He turned out to be a pretty good guy. I don't know whether he was pretty good, but he wanted to take over my dad's business without giving up the representational foreign company, so he wanted to do well for my dad. I don't know how much of that was it, or how much he was just a pretty decent guy. But he fixed it up so he would take the business, he would take the apartment, and in return he was arranging for our orderly exit. So we had about four or five weeks in which to pack everything and decide what we were taking.

But, you know, there were strange things. For instance, because of the book burnings -- and we didn't have a fireplace, and we wanted to get rid of them. We felt we had to get rid of the books that might incriminate us. I remember spending days flushing books down the toilet, tearing out pages and flushing them down the toilet.

Q: What kind of books at that point were going to incriminate you?

A: There were even fairly well-known authors, like Thomas Mann[12] and Franz Werfel[13] and people that we wouldn't even consider political today. Then my dad had some -- I think my dad had been a Social Democrat, so we had some of those books. But it's hard work tearing up books and flushing them down the toilet. (laughs)

It was scary, a really scary time, because they would -- one of the favorite tricks they had was to get people whom they wanted to humiliate to scrub the sidewalks. So we would see them. I mean, we were up on the third floor and look down, and they would drag some shopkeeper out of his shop and made him scrub the sidewalks.

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[11] The SS; The notorious Nazi police force which originally served as the personal bodyguard for Adolf Hitler.

[12] Thomas Mann; A German Nobel Laureate who was famous for his deeply profound novels.

[13] Franz Werfel; An Austrian novelist and playwright.

Q: Did you live in a particularly Jewish area of Vienna?

A: No, not particularly so. It was in the Fourth District, and most of the Jewish area was the Second District. But my dad was also -- and his accountant, who was my mother's brother-in-law. He was married to her sister, and the two of them, who worked in the same office, had to go and scrub something. But they didn't scrub the sidewalks. My dad said that was really insult added to injury because they had to scrub a monument which he had hated all of his life. He thought it was the ugliest monument in Vienna and he had to scrub that.

Q: Do you remember which monument that was?

A: It was the guy -- Radetsky [Tegetthof Denkmal], I think. I think so. The guy who won a naval battle when Austria still owned Trieste as a seaport. And it had the front ends of boats coming out of a big column, and the guy was on top of it, and the bows of the boats came out of the side of this column. It was really ugly, and that's the one he had to scrub.

Q: Well, clearly, your parents, it seems to me, had been planning --

A: Yes, they had been.

Q: Was this something that they discussed?

A: No. I think they must have discussed where we were going and also, mostly, what we were going to take. I spent those five weeks mostly packing, weeding out stuff. The irony of it was that nothing of what we packed ever came. We went through five weeks of packing, and it all ended up right in Vienna. My sister went back to Vienna in the fifties at one point and visited the guy who had taken over my dad's company, and he still had the same office that my dad had. And there were the rugs that had been in our apartment, and the pictures, everything. So it all stayed right there. We really just came over with what we carried.

But we flew, which was very unusual in those days, very unusual. We went to the Vienna airport, which is about the size of this area right here. (chuckles) I remember being patted down by a matron to make sure I didn't take anything with me. Then the plane was not pressurized, and I think the toilet had a hole in the bottom. My dad was determined to use it while we flew out of Vienna. We flew to Prague, spent only about a day there, I think, and then flew on to Brussels. My mother and the three kids, three of us, stayed in Brussels for maybe four weeks or so, five weeks.

My dad went on to England and started getting himself a job, and I think that was with the ICI, or maybe some other English company that he represented. He got a job, and it was a strange job. He was running a little factory outside London, in a place that was very heavily bombed later during the war. And it was making a special quality of soot that could be used -- it's called carbon black. It was a much smaller particle than regular soot. They used it in darkening the surface of cement highways, and also heavily used in tires, rubber tires. So he ran this little factory. But he had a job before we came on and joined him.

And then I went to school in England for three years. Also, their education was pretty high level.

Q: Was this in London?

A: It was in London to start with. I started that in the fall of '38, and it was called the Southampstead High School, which was part of a set of schools for girls which was supposed to emulate the standards for boys' schools. What was it called? The Girls' Public Day School Trust or something. Anyhow, there were several schools like that.

We lived fairly close to that, and on my way to school there lived Sigmund Freud. So I saw him every day I walked to school, he was out walking.

We stayed in London. See, in 1938 was when Munich happened. That's when [Neville] Chamberlain[14] went to Munich. At that time, before he came back with "Peace in our Time",[15] all the schools in England -- the whole country was convinced that war would start before he went to Munich, so the schools all developed evacuation plans. Our school took a vote among the parents between about three or four places that they would like to evacuate to. I think we were supposed to evacuate to Shrewsbury. Well, we didn't because Chamberlain came back.

When war actually did start, I remember we walked to school, my sister and I, with our little bitty suitcases. The whole school got walked by the teachers. We walked to the nearest railroad station, which was probably maybe ten minutes walk or something, and all got on the first train, wherever it went. They really didn't know. The school didn't know where it was --

Q: Had the bombing already started?

A: No, it hadn't. In fact, we were evacuated the day before the war started, when it looked extremely imminent. Our train -- they all went -- this particular railroad station served the north of England, so we just went north. The train stopped in a place called Northampton, and we all got out. And then groups of girls, maybe a dozen girls, would walk with a teacher and walk up and down the street, and the teacher would knock on doors and ask people if they would take in some of the kids to stay there. And the people were just wonderful. Everybody took in kids.

My sister and I were always together, which was a big comfort, but we were not alone. We were taken in by this family. Then the school tried to bring the kids together for activities during the day, so the next day the whole school went to this big indoor swimming pool. And it was while I was in the swimming pool that war started, because I heard the big P.A. system in the pool booming, reflecting from the surface of the water. So that's when war started.

We stayed in Northampton maybe a couple of days, and then the school probably negotiated for a permanent place. The permanent place turned out to be Berkhamsted, which is in Hertfordshire. So we went back south again, but not all the way to London. Berkhamsted isn't really all that far out of London. There, they had evidently negotiated for billets[16] before we walked up and down the street.

My sister and I ended up -- most billets would take in one or maybe two of the kids. But the one we got taken into took in ten, and it was a guy by the name of Sir

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[14] Neville Chamberlain; British Prime Minister from 1937 to 1940. He is best known for signing the Munich Agreement, thereby giving the Sudetenland to Adolf Hitler and buying temporary peace with Germany.

[15] "Peace in our time"; A phrase from the speech Neville Chamberlain gave following the Munich Agreement.

[16] Historically billets were the sleeping quarters that a soldier was assigned to. In this context, billets designated the arrangements made for a place to sleep for children to keep them safe in the country while British cities were being heavily bombed.

Richard Cooper, who it turned out also ran some kind of chemical company, I guess, but I'm not sure what. They had a big house on top of a hill, really lovely, lots of yard, tennis courts, all sorts of things. So they took in the ten kids. I was the oldest, so I was supposed to help keep discipline among the others. I still correspond with one of the girls who was there.

Q: What happened to your brother? Where did he go?

A: My brother was too young to -- I think he was five at the time. Yeah, he was five. He was in some kind of -- I'm not sure, but I know he also ended up in Berkhamsted, through whatever my parents arranged. I don't think his whole school went out, I'm not sure. But he was in the local boys school. I'm trying to remember. It was called Berkhamsted School, something like that. He was only five. He didn't know very much English. And they changed his name because the kids couldn't pronounce it. His name was B-r-a-u-c-h, and they changed it to Brooks, B-r-o-o-k-s. I know he had what they called a tuck box, where the boys are allowed to keep their belongings. And his tuck box had “Brooks” on it.

Then eventually, after we'd been there for a year or so, then my parents also moved there. But after my first year there, I went home to London for summer holidays, and that's when the Blitz[17] started. So I experienced some of the bombings while I was in London for the summer holidays.

Q: I want to take you back just a bit because I find it remarkable that your parents kind of were prepared with the *Anschluss*[18] to leave. At what point did they start discussing where they would go? How early did they know that being even nominally Jewish in Vienna in the thirties was going to be -- you were going to have to flee.

A: I think they did because of what was happening in Germany already. Although it wasn't really as bad in Germany as it got to be in Austria. I think Austria is a beautiful country, but the people are much nastier. I mean, I disliked the Germans, or did at that time, for various [reasons]of their rigidness and so one, but they turned out not to be -- and the Viennese were supposed to be *gemütlich*[19] and they turned out to be really bad, especially in the country.

Q: Had your parents decided then to go to Britain? Or how did they decide where they would try and --

A: I think they decided to go to Britain because of my dad's [connections]-- he was pretty sure he could get a job, and I think that's probably why they decided. And then eventually they decided to come to the States. So we must have fairly early -- I'm not sure of this, but you have to apply for a visa and you get on a long, long list. I don't know at what point they started to apply for a U.S. visa, but I know that we didn't get it until the spring of '41. So then we left.

[17] The Blitz; A period of heavy and destructive bombing by the Germans of British cities (especially London) from 1940 to 1941 during World War II.
[18] The Anschluss; The Nazi occupation and annexation of Austria by Germany in 1938.
[19] *Gemütlich;* German for warm and friendly.

At that time, the U-boat[20] activity was at its peak, so we went with a convoy. We went in what had been a liner of the Cunard Blue Star line, White Star? - maybe both. Anyhow, they had all these fancy liners, like the *Queen Elizabeth* and the *Queen Mary*. We went in one called the *Andalucía Star*, and it had been converted into [supply ships]-- they went down to Argentina to pick up beef and ship beef back to Britain, so that's what the *Andalucía Star* was doing. But they went empty down to Argentina, so they took passengers. But that was secondary. Their main function was bringing back the beef.

We started out with a convoy, went as far as Iceland with a convoy, and then broke from the convoy and went straight south. There was one place where the U-boat activity was particularly hot, and that was off the coast where Africa sticks out to the west, a place called Dakar. So when we were off the coast of Dakar, the boat started going a zigzag course to avoid being hit by a torpedo. But that was really the only really warlike symptom of that particular trip.

Then we went down to Montevideo, Uruguay, and went across and picked a U.S. ship up from Buenos Aires called the *SS Brazil* and went up to New York. I remember that as a fun trip. It was the first ocean trip I'd ever had, it was wonderful.

II. College Education in the US; Beginning Work at the Jackson Lab

Q: It always strikes me because, for this generation of geneticists, they have these incredible stories of fleeing various parts of Europe. So you flee Austria, which has become unlivable, and then you get to England, and suddenly bombs are starting to drop. You're then on a route again, this circuitous route to get to the United States. How was this normal for you? What was normal for you?

A: I really didn't have any expectations of what was normal, other than way back when I could have stayed on in Austria. And I sure am glad I didn't.

Q: Was that a choice of yours to stay behind while your family went to England?

A: Oh, no. No, there was never any question of us parting. I think we were always going to. Because, I mean, they could have gone without me because I didn't have a passport. But they always stayed together.

Q: How do you end up in New York?

A: I remember New York as -- we arrived in June, and I immediately knew I was going to go to college. I was going to go to Hunter College, which is one of the New York City colleges. There are five of them. They were free to New York City residents, which is the only kind of college I could have gone to. I had to go to something that was free or very cheap. And entered as a freshman. I had to convince them that I had enough educational background, since I had not graduated.

Oh, wait a moment. I should say that in England I finished the end of high school, which was upper fifth, and took the school certificate. I know they have a totally different system of tests now, but the school certificate would have let me enter any university in England, except for some of them you had to take extra stuff. But I stayed

[20] U-boats; Military submarines used by the Germans in World War I and World War II. They particularly targeted non-military merchant convoys in order to break the supply lines to the Allied powers.

on to what they call the sixth form, which is essentially past normal high school education and will be in lieu of -- it's like getting advanced standing, so I would have had advanced standing if I had gone on to university in England.

I was in sixth form. There were just two of us who took science. By that time, you had to decide whether you were science or non-science, and there were probably ten or so that were non-science. And the two of us who took science had a wonderful time because we got virtually individual instruction. Some of the science we took, they did not have teachers for the high school, so we borrowed - I remember at least one - a man, from the local boys school.

One thing I remember was when he had us use a microscope, and what we looked at was the stone cells in pears. If you take a pear and you smear the meat of the pear, make a thin smear on a slide, you see these absolutely beautiful stone cells. They have geometric -- they're like snowflakes almost. I was absolutely thrilled, and they couldn't get me away from that microscope. So that was probably a turn-on.

Q: Had you made plans to go to the university in England?

A: Yes. I would have if we'd stayed on.

Q: Which one?

A: It probably would depend on what I was able to afford. To get into Oxford or Cambridge, you had to take special exams and it cost a lot. But you could take scholarship exams and get in without having to pay. So I think I was working toward taking the scholarship exam but never got to the point. I do remember the school certificates, which I took at the end of the last regular year. They were pretty tough. But you could decide at the -- you had to take a certain number of fixed things, like English and algebra and that kind of thing.

But then you could take exams in electives, and I decided at the last moment, never having had it, to take it in art. I hadn't had any art courses. But I didn't need it -- I could fail and it wouldn't influence my getting in -- so I took the art. We had to do a still life, a person, and a water color. I passed two out of three, I was very proud of myself. (laughter) I flunked the watercolor. (laughs)

Q: Well, clearly you have natural ability then. Did you take any certificates in science?

A: Oh, yeah. I would have had chemistry and physics. Again I don't think I would have had biology.

Q: And how did science education in Vienna compare to the science education you were getting in England?

A: I think they were probably pretty equivalent. And again my memory is so lousy on that. I don't remember much lab work in either place. But then at Hunter -- because at Hunter you didn't have to declare a major until your sophomore year, so I don't know what I declared as a freshman. But in my sophomore [year] I declared a chemistry major with a biology minor, which would have -- at that time, I was convinced I wanted to go to med school, and that would have been sort of equivalent to a premed. I don't think they called it premed, but if you took the right courses, it would have been.

In my freshman year, I took a lot of required courses, like civics and speech and lots of English, that kind of thing. But I think I did have the elementary chemistry course in my freshman year.

But then I had a very interesting thing happen in one of my English courses. I had a teacher who decided that I could write well, so she had me submit an essay to the *Atlantic Monthly*. They had a national contest every year for college students, and you could send a story or an essay or a poem. So I sent an essay, and believe it or not, I got the national essay prize, which carried some money with it. So for the first time, I had some money. By today's standards it would be nothing. I think it was like a hundred dollars, but that would be equivalent to maybe five or six hundred dollars now.

In the summer after my freshman year, I used that money to take a vacation in New Hampshire by myself. I didn't take any of my family with me. That was my first time to get out in the country, and I just loved New Hampshire, it was wonderful.

Because I won this prize, I was immediately beseeched by a whole lot of agents who wanted to be my agent to see if I could sell things to magazines. I remember visiting some of these magazine offices, and I got really turned off by seeing the kind of world I would be living in if that's what I went into. But I was really tempted for a while after I got that prize.

Q: What was your essay on?

A: It was called "Refugee."

Q: And it was about your experience?

A: It was much about my experience, but general thoughts.

Q: Well, again to go back just a little bit. How well was your English when you got to England to do well in school?

A: I had had one or two years, maybe a year and a half of English in high school in Vienna, so I really knew a fair amount. I remember the first class of English I took in Vienna. We had to say “this is, this is” probably a thousand times because there were all these -- two kinds of s's and -- but I didn't have much trouble, and I always thought English was very easy. Because the language I'd had before was French, and that was not really in school, that was -- so I don't remember having any problem with English.

Before I was admitted to Hunter, I had to take all sorts of entrance tests because they didn't think school in England was equivalent to school in the States, which is just the opposite. In England they kept laughing about the level of college -- you know, college in the United States was like high school in England kind of thing. Two different sets of opinions. So I had to take all these entrance tests.

Q: And where did you fit in when you got to Hunter? How well did your education prepare you for American college?

A: I think it did, very well. I didn't have any difficulty really. I think it had been more rigorous.

Oh, the other thing I meant to tell you, the summer before I entered Hunter, while I was taking all these tests, I had to take a job and the job I took was in a sample card factory. My sister and I -- sample cards, pieces of fabric that they stick on cards for people. And they actually have people who do that, who stick those samples on. We

traveled in a crowded subway in the heat wave in New York and go up to the fifth floor of some downtown building and clock in and clock out. It was an education.

Q: You arrived in the United States in 1941, in the summer of 1941?

A: June I think it was.

Q: And in December then, the United States gets attacked. What were you thinking? Was this another situation where you were going to have to leave?

A: No, I didn't think we'd have to leave, but I thought here we go again, you know, kind of thing.

Q: And how difficult was it for your father to find work in America?

A: He again, I think, worked for ICI, I believe. They had some facilities in New Jersey, so he worked there. And he eventually started his own little business. He started his own carbon black thing, and they made carbon black, mostly for paints. They made suspensions of various kinds.

Q: But you lived in New York all that time, with your family?

A: I lived with my family -- yes, I think all the time I went to college I lived with my family, but not always in the same -- we started somewhere in midtown and moved up to Washington Heights. So, yeah, I think it was the whole time.

Now this is where my career really started. I took some biology courses and was very much turned on by an embryology course, with a wonderful, wonderful teacher. One of the things that she did a lot of was have us model things out of modeling clay. I remember the germ layers[21], the ectoderm was blue and the mesoderm was red and the endoderm was yellow. So we modeled the relations of the germ layers in different stages of the embryo. I really liked that course a lot. I don't know what else did I took.

The chemistry was very circumscribed. The first year was sort of general inorganic, and the second year was – no, it was qualitative analysis, quantitative analysis, organic and physical. So those were four years of chemistry.

The biology, I had comparative anatomy and embryology. And then the embryology was -- Hunter College is downtown, but it wasn't originally. Originally, there was an uptown campus, in the Bronx. It was a real campus with green grass and stuff. But it was taken by the navy as a facility for some naval training, so Hunter had to give up their campus, and so I moved downtown for my sophomore year. I normally wouldn't have moved till my junior year. And it's a tall building, like seven or eight or maybe more floors.

I remember after one of my embryology classes, I went out to catch the elevator, and the elevator wasn't coming. So I looked at the bulletin board and started reading everything, and there was this announcement of the Jackson Laboratory[22] summer

[21] Germ layers; A collection of cells formed during the embryonic stage. There are three germ layers, the ectoderm, mesoderm, and the endoderm. They give rise to an animal's tissues and organs.

[22] Jackson Laboratory in Bar Harbor, Maine is well known for its long history training scientists in genetics, cancer research, and fostering excellence in scientific research.

course. So that elevator changed my life. I wrote and was accepted. The summer after my sophomore year, I went to Jackson.

Nowadays, everybody and his brother's got summer courses, but this was really new. There were very few places that had summer research courses, and it turned out to be that Bill [William L. Russell][23] was the one who had instituted that summer course. It was done in a wonderful way. Every student was assigned to a staff member and sort of didn't act as a technician, but did an independent little project.

In addition to that, you also had a tutor. You went to a tutorial. The individual students went one-to-one with a staff member, and they would read certain papers, or whatever.

Then the third thing they had was a technique class, which all the students took, where the different staff members demonstrated whatever techniques they were doing.

And this being in the summer of '43, still in the war, so there weren't any male students. So I was stuck in this totally -- half the college were all females. The summer course at Jackson turned out to be, not by rule, but we happened to be *all* females. There were about fifteen or so summer students. We lived in tents. They had platforms, wooden platforms. The students were in tents. And it was just down from the lab. Are you familiar with Jackson at all?

Q: No, I have not been to Jackson.

A: It's totally different now. You'd never recognize it. It was just one big brick building, and it was in the woods, and the tents were in the woods, and we'd go up to the lab. We could go up at night and work on our experiment, or whatever.

That's when I got turned on. I really got turned on. Because I remember at one point looking down a microscope and seeing a fertilized mouse egg and thinking, this fertilized mouse egg is going to be a whole mouse. Just less than three weeks from now it's going to be a whole mouse. That was just a really inspiring thought.

Q: What funding was available to go up to Jackson? How did you afford to do that?

A: It was quite very reasonable because --

Q: You lived in tents.

A: We lived in tents, and we all ate -- they had one of the mouse caretakers, his wife cooked lunches for all fifteen of us, or whatever. We really weren't much cost to the laboratory, except the time that the staff members were doing stuff. But I guess they got something out of us, too, because some of the little projects turned out to be very interesting.

The first year I paid what everybody paid, which is not much. The second year I got a scholarship, so the second year I went for free. I think the project I did my second year had something to do with the anterior chamber of the eye was supposed to not have any antibodies in it, so you didn't have any tissue specificity. It didn't affect what you put into the anterior chamber.

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[23] William L. Russell (1927 – 2003); Liane Russell's husband and a well known scientist. He is most noted for his work on acceptable levels of radiation. He is a recipient of the prestigious Enrico Fermi Award. For an article written about him in the New York Times, see http://www.nytimes.com/2003/07/25/us/william-l-russell-92-radiation-pioneer.html.

I remember putting -- and that really took a lot, for me to be able to do that. I had to cut through the cornea and stick something into the anterior chamber with a very fine pipette, and then see what happened to it. That was one of my projects.

The other project was a dopa reaction[24] of pigment cell in a follicle and in a dermis. You did frozen sections of a piece of skin, and they all started looking like - [Dr. Russell draws] -- you know, like this. Take a wedge of skin and then cut -- on the microscope we'd cut frozen sections and then you'd put them in the reagents -- anyhow, I'd go to sleep, I'd close my eyes, and I'd see these things floating. Every night I'd see these things floating. But it really -- I decided at that point I really, really wanted to -- this is what I really wanted to do. I mean, not necessarily this, but --

Q: But experimental work?

A: Experimental work, yeah. So where do we go from there? I still wanted to be an M.D.

Q: Okay. And I wanted to ask you -- you had mentioned that by this time you'd decided to go to medical school. When did you decide that you wanted to go to medical school?

A: That I didn't? Bill talked me out of it. (chuckles) He said it would not be intellectually satisfying to me. I think nowadays people do -- a lot more medical [students] do research, but not in those days. You became a practicing physician. So I wouldn't be doing research.

And the other thing, he thought doctors were the most unscientific people in the world, the way they used evidence. They had not had the kind of training to really evaluate evidence.

Q: What was it about medicine that attracted you to begin with?

A: Well, you know, for one thing, I just wanted to help people. Like I had taken the little critters out of the way, I was going to do the same thing for people. I think that was mostly it, to do something for people. In a way, that was sort of on the edge of science, if it wasn't really science.

At one point, I know, I had this really crazy idea. After I had won this essay prize and I had been considering doing writing, I thought, hey, I could still do that. Oh, I was also taking some psychology courses, electives. I can combine all this. I can go to medical school and become a psychiatrist and then I can write about it. So I could combine all those things.

Q: Okay. Hunter College, the history of the college was as a normal school; a teaching school to train teachers.

A: That's right.

Q: How unusual was it for women at Hunter to pursue more of a scientific -- ?

[24] Dopa Reaction; A staining seen in tissue when dopa has been administered. This may indicate dopa oxidase in the cells.

A: Well, the normal school was a long time ago. By the time I went, it was really a pretty high level college. I think also city colleges -- there was Hunter College, City College [of New York], Brooklyn College, and one other that I've forgotten. Of the city colleges, I think it was the next to highest in academic performance.

Q: Okay. I'm going to pop in a new tape. Do you want to take a break?

A: I don't need to take a break, but at some point I'm going to go to the ladies room.

Q: Do you want to do it now?

A: Yeah, I'll do it now.

Q: Okay. After a brief pause, we're back. One question I wanted to follow up on is, you had mentioned as a child you were very concerned with animal welfare, and then at the Jackson Lab, during the summer, you were able to do actual experimental work where you're using animals in a completely different way. How did you reconcile your --

A: Well, I didn't actually kill the animals. The only thing was this anterior chamber. I mean, they survived and they were fine. But it was really hard just to work around the eye, I think. But the skin work, the dopa reaction. We just took a very small piece of skin for that.

Q: And was Bill at that point, we he a full-time staff member at the Jackson lab?

A: Yes, he was. Actually, that summer school had been started in an informal way by C.C. [Clarence Cook] Little[25], who had started the Jackson Lab. And it started in the thirties, when he was doing a lot of raising money, and so on. So as a favor to some of his donors, he had their sons come and spend summer. So that was sort of the beginning of the school. They didn't have any rigorous standards about whom to admit. Then many of these people became M.D.s, and so on.

But anyhow, when -- we called him Prexy -- Prexy Little gave up the summer school, then Bill took it over and he just totally changed it. He had all the things that I mentioned, the tutors and their own experiments. I think even Prexy had -- the summer students were mostly like technicians, they were mostly helping some staff member. But when I was there, you were not supposed to be doing that, you were supposed to be doing your own little experiment.

That was so exciting also, to me, to think that whatever I found, nobody would have known before if I hadn't found it out. It was something totally new. It may be very small, but…

Q: Okay. And you had mentioned that you wanted to go into medicine for the practical reason that you can help people. And how aware were you that for people interested in science, you could go on to graduate school and do nothing but this kind of academic-type research-oriented lifestyle?

[25] Clarence Cook Little (1888 – 1971); An esteemed American geneticist who studied mouse and Mendelian genetics. He went on to found the Jackson Lab.

A: Well, I certainly became aware of that when I was at Jackson, but I really wasn't thinking about it very much before that. I was always thinking in terms of after I get through college, I'll be in medical school. I wasn't really thinking about graduate school, but I became very much aware of it, even my first summer.

Q: How much exposure did you have to genetics before you got to the Jackson Lab?

A: You know, that's something else I cannot remember. I'm going to have to look up my course record, whether I had a genetics course at Hunter before I went there or not. I must have had some. Or whether it was just a segment of some broader course.

Q: Certainly nothing remarkable.

A: No, nothing that in itself turned me on, I think.

Q: You mentioned that you became very interested in embryological work. What kind of science was the Jackson Lab summer program teaching?

A: Well, the Jackson Lab, in itself, was supposed to be a cancer research lab, but there were only, I think altogether maybe only about eight staff members when my first summer course happened. I would say out of those, only maybe two or three were very directly into cancer research.

Certainly, Bill wasn't. Bill had been -- his mentor in graduate school was Sewall Wright[26], and he became very much interested in phenotypic[27] variations within inbred strains. This was the time when many of the now well-known inbred strains were being created. Bill's work was essentially directed towards finding out the causes of phenotypic variations in populations which were genotypically[28] identical. So he was working at that time with vertebral variations. I mean, this was not because it was in itself that interesting, but because it was an index of variability. So some strains have five presacral vertebrae and others have six.

Then there are other strains that have a certain ratio of five and six, that may have 80 percent five and 20 percent six. Whether you breed from the animals that have six or from the animals who have five, they would all give the same distribution, regardless of what they themselves are. So the genotype fixes them, the causes of an invariable character. I mean, you can only have five or six, you can't have five point five or something. But the causes are distributed and the final character is fixed. I'll have to draw this. [Dr. Russell takes a sheet and draws]

Q: Okay.

A: So this may be a scale of say prevertebral material. You just have a continuum. You have the distribution of the amount of prevertebral material, or whatever you call it. It's like that. But then there is a threshold that anybody who has this much has got to

[26] Sewall Wright (1889 – 1988); A noteworthy American geneticist who was one of the fathers of theoretical population genetics. See http://www.harvardsquarelibrary.org/unitarians/wright-sewall.html for more information.
[27] Phenotype; The outward characteristics of a cell or an organism. It includes appearance, behavior, and development.
[28] Genotype; The genetic makeup of a cell or an organism, as opposed to its appearance.

have six, and everybody who has this much has got to have five. So you have a continuous variation, but you have a discontinuous final expression. Whether you breed from these guys, or whether you breed from these guys, they're all going to have offspring with that same distribution.

That's essentially the kind of system that he was working with, and specifically looking into the prenatal environmental as to where they got shifted relative to the threshold. They would still be there, but the threshold would shift, this would still be the same.

In connection with that, he developed ovarian transplants, which are still used now. He originated the ovarian transplant so you can get from one -- but you can't transplant from one strain to another because of the rejection problem. But if you transplant from an inbred strain to an F1 hybrid made from that strain, it will accept the ovary.

I know one of his papers was called “Pure Strain Mice From Hybrid Mothers” [Russell WL, Hurst JG. Pure Strain Mice Born to Hybrid Mothers Following Ovarian Transplantation. *Proceedings of the National Academies of Sciences of the United States of America*. 1945; 31(9):267-73]. Then he had another one, “Offspring of Unborn Mothers” [Russell WL, Douglass PM. Offspring from Unborn Mothers. *Proceedings of the National Academies of Sciences of the United States of America.* 1945; 31(12):402-4] because he would take embryonic ovaries and stick them into an adult host.

This was really fascinating. He would get this ovary, which is absolutely tiny. You have to wait until day thirteen of embryonic development before you can tell an ovary from a testis. And the ovary was almost invisible. But he was very, very nearsighted, so he could see things extremely well. He would put it into a watch glass and take his glasses off and hold it right there and get it out of the watch glass. That was not cancer research.

Other people were working on what was then called the "mammary factor," which turned out to be virus. The "milk factor"[29] they called it, which meant that certain strains develop mammary cancers, but if you foster-nursed them on another strain, they didn't. That kind of thing. So that was getting partly into cancer research, but the continuum of what people were working on.

Q: Sewall Wright is seen a population geneticist now or a mathematical geneticist/ theoretical geneticist, and how did Bill describe himself at that point? Was he a cancer geneticist?

A: He described himself as a physiological geneticist, because -- yeah, very much. In fact, Sewall Wright did that to some extent, although that's not what he's known for. As you probably know, I ended up with Wright also. He was a very non-human person. I mean, he was that much of a genius that he was really unable to relate socially with ordinary people.

III. Graduate Studies and Thesis Work

Q: Okay. Well, why don't you tell me about how you go about deciding going to graduate school and why you choose the University of Chicago.

[29] Milk factor; A substance transmitted from mother to offspring in some inbred mice strains via milk or tissues transmitted through nursing. It inhibits production of mammalian cholesterol.

A; I decided, probably after my second summer at Jackson, that that was what I was going to do. I finished college in three and a half years because I had taken one summer semester to get my credits together, so I graduated in the middle of the year instead of at the end. I graduated in January of '45, and it was before that that I had decided to go to -- it must have been probably in '44, which was after my second year.

I applied to Curt Stern[30] at Rochester and Sewall Wright, and who was the third person? Herman [B.] Chase[31] maybe, I don't know, who was at Brown [University]. I know I applied three places. And then I got accepted to all of them, so I had -- I think I got in again because Bill had been with Sewall, but I decided to go with Sewall.

It really didn't do that much for me because I finally started at [University of] Chicago in the fall of '45. The very first thing that happened is -- have you ever heard of [Herluf H.] Strandskov?[32] He was a human geneticist at Chicago. He was *the* human geneticist in the department. I think he had been a student of Wright's. But he came up to me and he said, "I think you'll find that Sewall is not going to be too much help in practical things." So he found space for me. He found a desk. He did everything. Because Sewall didn't really much know who his students were. He was very much detached.

But his courses were really amazing. Although I didn't really absorb as much as I would have liked to from his biometry course when he had all his statistical stuff. I don't think I myself would have gravitated into that area. It was too detached from real things. I wanted to do the type of work where I could test the hypothesis I had, and that's hard to do in quantitative genetics, I think. You just go and look what somebody else has found.

But his physiological genetics course was very, very fascinating. I think there were three -- he taught a basic genetics, physiological. I still have my notes from those courses. In fact, Bill paid me to write notes and send them to him because it was thirteen years since he'd taken those courses, so he really wanted to know.

Q: Did you know of Curt Stern's reputation before?

A: I knew him slightly from -- I mean, not very deeply or anything. I really liked him, and I think I would have very much liked being his student. Did you ever meet him?

Q: No.

A: He was really a great person.

Q: Yeah, he's clearly very important to the history of human genetics, and genetics in general.

A: Yeah. I don't know when he died, but it was some time ago.

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[30] Curt Stern (1902 – 1981); A German-American geneticist most noted for his work on homologous chromosomes and mitosis research. He also was head of the group that declared that there was no “safe” level of radiation.

[31] Herman B. Chase; For an example of this geneticist’s work see http://bjr.birjournals.org/cgi/content/abstract/31/362/65.

[32] Herluf H. Strandskov; For an example of this geneticist’s work see http://www.genetics.org/cgi/reprint/24/5/722.pdf.

Q: Right. Before we go into how you got on to the content for your dissertation -- I haven't looked at it, but I know what the title is -- how aware were you, being at the University of Chicago, about what was going on with the Manhattan Project?[33]

A: I was aware of it because Bill had a consultantship at -- was it Rochester or Buffalo? I don't know. It was with somebody called Don [Donald R.] Charles. Don Charles was a mouse geneticist, and he had a contract from Atomic Energy Commission[34] I guess it was at the time -- oh, no, it was the predecessor, so it was the Manhattan Project. His contract was to sort of basically do radiation effects, mostly clastogeneic[35] effects, chromosome breakage, and Bill was a consultant to that project. So while he was still at Jackson, he would several times go to -- I think it was Rochester, and consult with Don Charles.

So he knew what it was. I wasn't too aware, other than the fact that he was going to Rochester to consult somebody. I wasn't too sure what the guy was doing. And anyhow, it wasn't secret. I mean, the work he was doing was not secret, because there had been other people had been doing radiation stuff before. Paula Hertwig[36] in Germany and George [D.] Snell[37] had been doing it. Of course, [Hermann J.] Muller[38] had been -- so the fact that people were doing radiation work wasn't secret. I think what was secret was who was funding it and why.

But that's not how -- Don Charles was not our connection to Oak Ridge at all. I didn't know too much. And Bill was looking for a job, so Oak Ridge was one of the places that offered. His condition for a job was that I should also be able to work wherever he worked. So several of his offers did not offer that. I know Brown was one of the places he would have gone to if he hadn't made that condition, but they had nepotism rulings. So when he interviewed here [Oak Ridge Laboratory], that was the first thing - they said, "Oh, yes, no problem." So we almost automatically ended up here.

I think it was the second time I had had an indirect influence on fate, because the first time was my passport, and then the fact that he wouldn't take any other job. He very likely would have ended up somewhere else.

Q: Yeah. Who did you end up finishing your Ph.D. with?

A: That has a very interesting story. I left Chicago after two years. I had only gone there from '45-'46 and then '46-'47, and I'd finished all the course requirements for the Ph.D. I had not really gotten started on a thesis because Dr. Wright, being totally out of this world, didn't know that what he was assigning me was not a possible project. He had been interested for some years - mostly via his students because he himself never got into the lab - so he had been interested in physiological genetics of pigments.

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[33] Manhattan Project; Created in 1942, this was a secret project established by the US Government which developed the first atomic bomb. It ended in 1946.
[34] Atomic Energy Commission; A commission created by the Atomic Energy Act of 1946 to regulate nuclear energy development and use during peacetime.
[35] Clastogens; Substances that cause breakage within chromosomes thereby causing various genetic disorders.
[36] Paula Hertwig (1889 – 1983); A German biologist who was a pioneer in radiation genetics and one of the first to see the dangers of radium and roentgen radiation.
[37] George D. Snell (1903 – 1996); An American geneticist and immunologist who shared the Nobel Peace Prize in Medicine for his work on immunological markers on the cell membrane.
[38] Hermann J. Muller (1890 – 1967), A noted American geneticist who was awarded the Nobel Prize in Physiology or Medicine for proving artificial mutations can occur as a result of X-ray manipulation. See http://nobelprize.org/nobel_prizes/medicine/laureates/1946/muller-bio.html.

Bill's thesis had been on the dopa reaction in guinea pigs, which Sewall Wright had done all the analysis of the genetics of pigmentation essentially from the phenotypes. Bill did this dopa reaction, and he did it in frozen sections, which is a project I later had at Jackson. Because it was very hard to do in solution. Dr. Wright wanted him to do it in solution, and then Bill thought he could do it in the frozen sections, so that ended up being his dissertation.

By the time I got there, Dr. Wright was very disappointed that he hadn't had any students in the interim who really were able to do the biochemistry of melanin formation. Nobody could do it at that time, and I was certainly not an organic chemist, so I was very frustrated. I sort of started out in various ways and nothing really worked. By the time I left Chicago -- I mean, I was a student still, but I wasn't in residence -- I had done all my course work and really not started my thesis at all.

After I got here, I was going to do my thesis -- I had something I later picked up again, but then went off on a sidetrack for my thesis. What I was going to do was to get somatic mutations. Bill's project was essentially to do the germ line mutations from radiation, and I wanted to do somatic mutations on the same loci, which meant that you had to irradiate embryos in utero while the pigments also were being -- you know, at birth, migrating and multiplying, and so on.

I had to do this preliminary work to find out what would be the best stage because you want to do it when there are enough pigment -- well, when there are a few pigment cells so you can end up with a big spot from the one that you mutated, it's going to multiply and make a big spot, so you really, on the one hand, want to do it when there are very few. On the other hand, you want to have a large enough population so you can do some quantitative -- because otherwise, if you do it from one cell, every embryo essentially becomes one. But if you do it from an embryo that has two hundred cells, then you're essentially doing mutagenesis[39] in a population of two hundred. So you want to find something in between.

I was doing all this preliminary work simply to find the right stage for the right kind of spot. And in the course of doing that, I'd come up with all these abnormal malformed embryos. So I digressed away from my original project, which I came back to again ten years later and did the teratogenesis[40] of radiation. So that's the cause of my thesis, and it turned out that nobody had done anything systematic. People knew that radiation caused malformations in embryos, but I essentially decided to outline the different critical periods to possibly relate it to normal events that were going on at the time.

It became quite a big thesis, and for the next maybe three or four years, I stayed with it, various aspects, after the thesis was done, and then went back into genetics that I never really had wanted to leave, but it was a good thesis project because it was a sure thing. And it was something that Dr. Wright did know something about because he had been interested in malformations, of other people's creations. He was actually a good long distance advisor essentially, telling me what really was a critical question, and things like that. But I never saw more of him than maybe twice a year. He'd come here, and I might go up there.

I had started that shortly after I came to Oak Ridge, which was late in '47, and I finished it in '49, so it was a two year research project. Consequently, I never did a postdoc, because I was already working.

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[39] Mutagenesis; The process by which chemicals or radiation causes a genetic mutation.

[40] Teratogenesis; The study of atypical processes in physiological development, especially those occurring in a fetus.

Q: In those early years, how common would it have been to do a postdoc in genetics?

A: Not very common, not very common at all. Have you found that also with other people?

Q: Yes. For this period, postdocs were [unusual], but I worked on another project that interviewed people who got their Ph.D.'s in the eighties and nineties, and one postdoc is mandatory and to have a second one is not unusual before getting an appointment.

A: And it's really too bad in a way. I mean, I wouldn't have been able to do a postdoc. But in a way, I missed having had that experience.

Q: What do you see as the advantages of having a postdoc?

A: Well, because you broaden your experience so much, particularly in the way of techniques. I mean, techniques are so important now, and were much less so then.

IV. Work with Radiation and Embryology; Chromosome Research at Oak Ridge Lab in Mice and Drosophila

Q: What techniques could -- let me ask another question first. What was driving your science? Was it just circumstance that you had a situation in which you could create -- to look at the effects of radiation on embryos or embryological development? Was it an interest in embryology? Was it an interest in basic genetic principles. What was it that was driving your interest at this point?

A: Well, at that point, for my thesis, which was not typical of my later work at all, it was certainly the fact of relating it to events of normal embryology. Very soon, it became a very practical thing because of the hazard, the human hazard. Several of my papers were devoted to that and trying to find out also, just in library research, what kind of doses people were normally getting in medical practice for diagnostic radiation, and what type of precautions were taken.

Then I came up with this fourteen day rule that if you -- and that roused the ire of the radiological community. The fourteen day rule essentially says that if a patient has to have diagnostic radiation that's not urgent, that's not an emergency or anything like that, and you can time it, you should time it in relation to the menstrual cycle and give the radiation at a time when there's no possibility of -- in other words, the first half of the cycle, before ovulation, when you don't have the possibility of an unsuspected pregnancy. As soon as I published that, I was horribly attacked by all radiologists, I think mostly because they were worried about being sued. I'll get you that publication.

Q: If this is [the publication] from *Radiology*, I may have it here with me. Okay. Yes, I do have a copy of that one. [Russell, LB and Russell, WL. Radiation hazards to the embryo and fetus. *Radiology.* 1952;58(3):369-77.]

A: You may not have all these letters to the editor.

Q: Oh, no. I don't have the letters to the editor.

A: Somebody by the name of Chamberlain? No, it was President of the Radiologists of America or something. Richard [H.] Chamberlain. There were several letters of all of the -- anyhow, you're welcome to read them. [handing Maestrejuan the letters]

Q: Okay. Well, who was your audience at this point? Who did you think your audience was?

A: Well, the funding audience was the AEC [Atomic Energy Commission]. At that point, it was the medical community.

Q: Okay. And in this particular article, you were very explicit that, okay, these are experimental results from mice, but this is what it would look like in humans. And you made the direct connection on how applicable this would be for human medicine. What was motivating to make that connection between the mouse and basic genetic principles in mammals?

A: This wasn't really genetics. This was teratogenesis. I found one case in the literature. There were lots of human medical publications about various odd babies after radiation, but the time of radiation was usually not very well known or so, it wasn't published. I found one paper where it was exactly known when conception occurred because the woman was married to a guy who was in the army, and he came home for one weekend, or something like that. So it was a very exact timing of conception, and the radiation too. And the abnormality, which was an upper arm abnormality, was exactly like it would been in the mouse at a certain stage. So there was a stage comparison.

Then -- I don't know if that's in this paper somewhere. No, it must be somewhere else. Somebody had done a mouse and human comparison of embryonic events.

Q: I think you cite that in that work.

A: I have it somewhere. So when you do that, then you can tell when the major organogenesis[41] stages would be in humans. In the mouse, they're between day six and thirteen. In humans, it's from week two to week seven or something. It gives you a very good cross-reference point. The stages that I was never very happy with knowing more about were the post major organogenesis ones, when they were much more subtle things than gross malformation.

I did come back -- this was my main excursion into teratogenesis, but later on I came back to the area via developing a test for teratogenic effects that would pick up very small doses of either radiation or chemicals. It was based on what I drew for you, what I call the homeotic shifts. I'm not sure, I'll have to find out what publications, but you may already have those.

I just really loved it, but I didn't do much more with it, other than publishing it as a relatively easy method. What it was based on was, again, the axial formula, so many thoracic, so many lumbar, so many sacral, and also things like the number of components of the sternum. In the mouse, you have five little bones, and then you have the base of the sternum, and a number of ribs. So easily quantitatable, very easily countable changes.

[41] Organogenesis; The process by which organs form from embryonic cells.

If you pick the strains that straddle a threshold like that, then very small disturbances would push them one side or the other of the threshold, and you count the number of the vertebrae and you see if you've had a small disturbance. I call that the homeotic shift method, and I have about four or five papers on that. But that was my only subsequent way back into embryology. Except, of course, the spot test[42] which is also involved in embryosis, but it's purely genetic.

Q: At this point in the late forties, how were you identifying yourself? By the 1950s, say. What kind of scientist were you?

A: A geneticist. As I said, the only other time I got into teratogenesis was probably in the late fifties or early sixties when I did the homeotic shift.

The other thing that I profited from was the mutations that were being induced as part of the mutagenesis tests. Bill's work was essentially counting the mutations and finding things that affected the rate of induction, whereas I grabbed them. So I was very fortunate that he had decided to keep propagating them as stock, not just counting them but setting up -- so by the time I got into sort of milking the results of his labors, there were a lot of stocks to work with.

I think I got back in a big way into genetics when I started doing the complementation tests[43] with the mutations, because, as you probably know, you probably didn't read too much of his stuff, but he was using the specific locus test[44] and it was essentially using seven loci, not for their own sake, but because they were a good indicator, an easy indicator of mutation rates. But this resulted in our having multiple mutations at any one locus. They may have twenty or thirty at the B locus, the may have fifty or sixty at some other locus. So that gives you a lot of good genetic material to do complementation[45] with.

And I milked his weird cases in other ways. For instance, a lot of the X-chromosome work I did came out of that. And really, the best thing that ever happened to me was finding the variegated mutants. I would say in the genetic area, a lot of things that I did had a common interest, and that was in mosaicism[46] coming from different directions.

And you know, the book that I edited, *Genetic Mosaics and Chimeras*? [Russell, Liane B., ed. *Genetic mosaics and chimeras in mammals*. New York: Plenum Press, 1978.] They had found -- I've forgotten in what order now, but anyhow, that some of the specific locus mutations were not whole-body, but they were mottled or variegated. So those are the ones I picked up for special interest.

Q: And why did you choose those?

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[42] Spot Test; Dr. Russell's fur-spot test identified chemicals likely to be mutagenic in reproductive cells.

[43] Complementation Tests; A test which determines if two different strains of an organism both have homozygous recessive mutations by seeing whether the two organisms produce the same wild-type phenotype when cross bred (for example, a change in wing structure in flies). This test is useful because it shows what a specific gene's function is. For more detail, see http://www.ndsu.edu/pubweb/~mcclean/plsc431/mutation/mutation5.htm.

[44] Specific Locus Test; A test that shows recessive mutations in offspring when one parent is normal and the other is recessive at various loci.

[45] Complementation; Where two different homozygous recessive strains of an organism result in the same phenotype and where the cross of both genotypes results in the wild type.

[46] Mosaicism; A mutation where there are different sets of chromosomes within the cells of a single organism. This difference can result in genetic disorders such as Downs syndrome.

A: Why did I do that?

Q: Yes.

A: Because they were unusual and potentially of genetic interest. I was very much, at that time -- I had mentioned Ed [Edward B.] Lewis.[47] I thought he was great, and he had done quite a bit of work on position effect[48] on Drosophila[49], and he had a review on position effect, which I read every word of and I thought it was great.

But anyway, I was pretty sure that there was some kind of position effect because in Drosophila, if you have heterochromatin[50] next to a gene, it makes the -- not just next, but near -- it will make the action of that gene, quote, "uncertain." So I thought that was what was happening here.

Well, I soon found -- and they're not that common. To start with we had one, and then a year later we had two more. I think eventually, even over all the years, we may have had twenty, which is not a big number. Because those aberrations turn out not to be as frequent. What we found was that they were -- the females were the only ones that were the variegated. And also, the females were what we then called semi-sterile. It means the litter size was roughly half of what it would have been. So immediately the suspicion is [that] they carry a translocation.[51] Of course, this is before the days of very good cytogenetics[52] but we did pretty soon get the cytogenetics, too. So, genetically, it was very clear they were translocations, and they were translocations of an X chromosome to an autosome.[53] I really don't know how much you want me to talk about this.

Q: Yes, continue.

A: I've got it all written down.

Q: Right. Well, one question I wanted to ask before, and I'll just insert now, because you're discussing really the *Science* article in '61.

A: Was it '59? Something like that, yeah.

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[47] Edward B. Lewis (1918 – 2004); A Nobel Prize co-recipient in medicine for demonstrating how genes direct embryonic development, Lewis was a well-respected geneticist. He also developed the complementation test and studied radiation effects.

[48] Position Effect; The effect of a gene's position in the chromosome upon phenotype. For instance genes translocated to regions of heterochromatin are often not expressed.

[49] Drosophila; A genus of fruit fly, most commonly *drosophila melanogaster,* used as a model organism in many scientific experiments since it is relatively inexpensive, has a high fecundity and a short life span, and has a discernable phenotype and genotype.

[50] Heterochromatin; An intricately coiled piece form of DNA which is known for its relative genetic inactivity.

[51] Chromosomal translocation; Movement of gene segments between nonhomologous chromosomes.

[52] Cytogenetics; The study of the cell's chromosomes.

[53] Autosome; A chromosome that is not a sex chromosome. Autosomes carry genes which determine the somatic characteristics and do not have any influence on determining the sex of the organism, unlike the sex chromosomes.

Q: And we'll get back to that, because you and Mary [F.] Lyon[54] start to at the same time simultaneously develop a hypothesis on the --

A: See, because I came from the side of the translocation.

Q: Okay.

A: And she didn't. She came from some sex-linked mutations.

Q: Well, what genetic tools did you have to work with at this point?

A: Just making crosses. Just really progeny where the translocation bearing females were variegated, but the translocation bearing males were not. Then we found -- this is something else that I was working on at the same time was spontaneous and induced losses of sex chromosomes. Once in a while we would find a female that had a translocation but was *not* variegated, and she would turn out to have lost one of her sex chromosomes. So the only X chromosome she had would be in two pieces, each one attached to the --

[moving the microphone]

Q: Continue.

A: And whatever the marker gene was wild type allele[55] of that particular marker gene, for instance, brown or pink or whatever, that piece of autosome was attached to a piece of X. But it was only when she had another X that she was variegated, and when she lacked another X, or when it was a male that only had that one broken X, was also not variegated.

So from that, I came at it from a very different way from Mary [Lyon]. I said that one X was needed to do its job, and the other X was surplus and was able to be on and off and induced the position effect, and I called it the position effect, if the X was inducing a position effect.

Q: And what I was reading is it would be more of an activation of one of the X's, and she described it as inactivation of one of the X's. Or is that too simplistic?

A: Well, I essentially said one X is needed to be active, and the rest of them are junk, I mean they're not needed. So that immediately brought the idea that the reason that it was variegated was because in some cells the one X was doing its job, and in others, the other. In the cells where the broken X was active, then that wild type marker was also active, and in the other cells, the intact X was active. So the broken X was not, and the piece of autosome attached to it also was not, so it was off, and that's what gave you the variegation.

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[54] Mary F. Lyon, FRS (Fellow of the Royal Society); An English geneticist famous for her work on radiation and its effect on mutation. She also formulated the Lyon Hypothesis, stating that an X chromosome can be inactive.

[55] Wild Type Allele; The most prevalent allele, the one with the highest gene frequency is the one deemed as wild type allele. The allele that is thought to contribute the typical phenotype seen in "wild" populations of organisms.

Q: Okay. And were you aware of Lyon, what she was doing in Britain at that point?

A: No, because she was essentially -- the whole XO[56] thing started here with the scurfy[57] back in -- I don't know, but two or three years before that. That was interesting because scurfy was a mutation which was not one of our markers, it turned out to be an X- chromatization. At the time we didn't publish it, otherwise we would have published the first X-linked mutation, but we didn't.

Q: And why didn't you?

A: Because Bill was lazy, or busy or something. But scurfy ended up with the same situation. Once in a while you'd find only the males with scurfy. I'm trying to remember - I know that Bill was doing ovarian translocation at that time to rule out one of the possibilities, and the upshot was that we found the XO, because of the situation in the scurfy strain where you sometimes got the wild type females, and they turned out to be XOs.

So that was published. Bill published that, and at the same time we published two papers together. The other one, I was also doing it with tabby,[58] which was a well known X-linked mutation. Then shortly thereafter, we found XXYs, and they would be variegated if they had the translocations or if they had whatever the mutant was.

Because if it had been like Drosophila, it would have been different because, in Drosophila, the Y chromosome will suppress the variegation. Because I had been reading the E.B. Lewis stuff, that's why I was pretty sure to start with that there was something to do with the Y, but then when we had the translocation both as XOs and as XXYs, the XXYs were much later -- not much later, maybe a year later, it turned out that it wasn't whether or not you had a Y but it was how many Xs you had that made a difference, so it wasn't like Drosophila. The Y didn't have anything to do with it.

Q: So Drosophila had been used as a model organism for genetics for quite a long time. How much of the understanding of genetics based on the Drosophila model influenced the way you thought genetics would work?

A: It did. Immediately, we found the XOs and everything. It turned out that sex determination was *not* like Drosophila. In Drosophila, it's the ratio of autosomes to X chromosomes, and that's how I was conditioned to think. But it wasn't. It was whether you had a Y or not. So the Y determination shattered the parallelism with Drosophila, and so did the position effect. The Y had nothing to do with whether or not you got the position effect.

Q: And how long did it take you to figure this out? Or was it just that when you identified the first XXY mouse that you realized that things were different than flies?

A: No, I think it was even before we had the XXY, when we just had the XO and XX that we figured it out. But, you know, the relation to the human -- because just about

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[56] XO Genotype; A case where there is only one sex hormone instead of two. XO individuals are females with Turner's Syndrome.
[57] Scurfy; The scurfy mutation affects the autoimmune system. This mutation in mice is used to study the immune system. It causes mice to have scabby skin, open their eyes late, and die early.
[58] Tabby; Abbreviated as (*Ta*), is a mutation used as a marker gene in strains carrying the X-linked mutations jimpy (*jp*) and testicular feminization (*Tfm*) and in the XO chromosomal stock.

that time Charlie [Charles E.] Ford and others were doing XO humans. But in the humans, it was all cytogenetic evidence. There was no genetic evidence. In the mouse, we had the genetic *and* the cytogenetic, so I think that was a nice clincher. I mean, that showed when modeled organisms are important. Also, human XOs are non-fertile, but mouse XOs are, so you're able to do genetic work with them.

Q: And where were you picking up the cytogenetics techniques?

A: We had a cytogeneticist by the name of Ernie [Ernest H.Yi] Chu, who is still at [University of] Michigan, I think. He was pretty good. I think that the mouse cytogenetics was never as good as the humans because the mouse chromosomes are all acrocentric, so before the banding came in you really could not identify individual mouse chromosomes. They make a continuum in length, and the centromeres[59] are all at the end, or near the end. And humans, you could identify individual chromosomes. So I think the mouse cytogenetics was lagging at the time, but it certainly was good enough to tell whether you had an X or not.

Q: So the model here at Oak Ridge, rather than to do postdocs, was to bring in people with different expertise?

A: Yes, it was. And Dr. [Alexander] Hollaender[60] was a great genius that way. He was the division director. He was the one that hired Bill, but he was here not very long before Bill came, I think, maybe a year or so. He wanted to make the division very genetics-oriented. Before he came here, whoever had been in charge of biology, it was mostly radiation effects on different organ systems, that kind of stuff.

But Hollaender wanted to make a genetics division. He built up some really great young people. Drosophila, Neurospora,[61] maize.[62] He had all sorts of organisms represented. He also went out and started to become international. He got our group, our division people, invited along to foreign meetings, or to take sabbaticals abroad. And then he would bring in foreign investigators for a year or two years. He brought in a great many people for short durations. I think that's where we got our broad experience. Bill and I never got a sabbatical because we had to stay with the -- there's no one else has a mouse colony like that size. But we had a lot of people come in.

Q: Okay. I think we're going to get back to that topic, but I wanted to -- just because this is an interesting issue for priority debates in science, yours and Lyon's work come out the same year. One comes out in *Nature* -- they're simultaneous. How do you account for the fact that you basically came up with the same conclusions at the same time working completely independently of each other?

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[59] Centromeres; The point where two sister chromatids meet is the centromere. From this point, the cell can engage in cell division.

[60] Alexander Hollaender (1898 – 1986); A radiation biologist who theorized that nucleic acids were the main component of genetic information. He is a recipient of the Enrico Fermi award. For more information see http://www.genetics.org/cgi/reprint/143/3/1051.

[61] Neurospora; *Neurospora crassa* is a model organism; it is a fungus of the phylum Ascomycot and the genus Neurospora. It is easy to grow and only has seven chromosomes making it ideal for genetics research.

[62] Maize; Also known as corn, maize is a model plant organism often used in genetics research from the genus Zea.

A: And working totally different projects, yeah. It's maybe because she had got at that time the fact that sex-linked mutants came to be known. There really weren't any sex-linked mutants in a mouse. Well, the scurfy was there, but it hadn't been published, and it wasn't a good thing to work with because it was so lethal. Tabby was really the one that became a good mutant to work with. That wasn't very long, and then the whole XO situation, which we published, but that she made use of.

Q: And why was she able to make use of it?

A: Just that it had been published.

Q: How did you respond that this idea now is known as the Lyon --

A: It's associated with her.

Q: The Lyon Hypothesis,[63] yeah.

A: Well, I think it was because I didn't capitalize on it, and I think that was -- I was not very good at publicizing myself. And also, I really didn't do all the work I could have done on it because of other things in my life. My kids were like eight and ten, and I had a lot of things that involved them. We were talking last night. I was never able to become single focused on something because I was too diffused, and I didn't do as much work as I could have.

Q: And what do you think the implications have been for you that it's not the Russell-Lyon Hypothesis or the Russell Hypothesis?

A: It did bother me some, but it didn't really make me sick or anything. It did bother me.

V. Relationship with husband William Russell and his work

Q: Okay. Well, one area I want to kind of go back to so that we can talk more about your work here at Oak Ridge is this relationship you have with your husband and just to get the timing of that down a little bit. So you're an undergraduate at Hunter when you go to Jackson Lab, and he's an instructor at the summer program.

A: Yeah, he headed it up more or less.

Q: And at what point did this relationship become more than just mentor-student?

A: Well, I don't know how much you want to go into my private life, but he was married and he had, at that time -- by the time I got there in '43 he had two kids, eventually had four. So it took quite a bit of personal things before he finally got divorced in '47, and then we married.

[63] Lyon Hypothesis; The hypothesis articulated by Mary Lyon maintaining that in organisms with more than one X chromosome, the other X chromosome may become inactive. The other X chromosome condenses into chromatin.

Q: So at the University of Chicago you were still going back to the Jackson Labs, or you were maintaining a relationship?

A: Yes. When I was at Chicago, in '45, and then in '46, I went back to Jackson, but this time not as a student. I had a summer job as a technician. I was finishing up at the time the pigment work, the frozen section stuff that I originally started when I first met him. And that was my first publication, actually. [Russell LB, Russell WL. A Study of the Physiological genetics of Coat Color in the Mouse by Means of the Dopa Reaction in Frozen Sections of Skin. *Genetics*. 1948; 33(3):237-62.]

So I worked on that the summer of '46, then went back to Chicago for my second year. By '47 I was no longer welcome at the lab because at that time the divorce was in progress. I was not even allowed to show my face at the Jackson Lab. Bill found that the Jackson Lab had use of another facility where they were doing some dog psychology. It was maybe fifteen miles from the lab and he found they were willing to give me a room in there where I could at least write. So I wrote up some of that pigment paper there.

Then we got married in September of '47, and he knew before that -- before that he knew, early in '47, that he would have to leave the lab. So that's when he started looking for jobs.

Q: Because the lab was going through financial difficulties or because --

A: No, because they just didn't want him to work there because of the divorce.

Q: His first wife was also a staff scientist?

A: She was on the staff, yeah. I mean, that's why -- if she hadn't been, they probably wouldn't have cared less, but because she was working there --

Q: And after you had done your course -- this kind of corresponds with the timing that you completed your course work. Was there a thought that you wouldn't complete a Ph.D.?

A: I'm sorry, was there what?

Q: Did you think that you might not complete a Ph.D.?

A: I think I was very much directed toward completing it, and I think that was one reason that he wanted to make sure that I would be able to get into the lab wherever he had a job. I actually was an employee when I did my thesis, which was double-dipping. I think I got two thousand dollars a year was my salary. So while I was doing my thesis research, I was actually an employee. My publications are ORNL publications.

Q: Okay. And Bill did not have any other job offers that would include you with some kind of --

A: I think there were three job offers he had, and this was the only one that would have included me.

Q: Okay. At that point, how did you see your role as a professional scientist when you got here?

A: Well, his job was set up on the idea that the Atomic Energy Commission wanted to know something about the genetic effects of radiation on human populations. So that was the rationale for his job. So it was very much directed from the beginning toward developing good mutagenesis tests, and then actually measuring radiation effects on mutations.

He took it very much from a point of view of exploring the factors that influenced mutation rate and mutation type. He wanted to explore the factors. He was not so much interested in getting an absolute mutation rate because it would have been meaningful to get that for seven loci so he was using the seven loci essentially as a system that he can use on two sides of a comparison -- males versus females or high dose rate versus low dose rate. It was a good objective endpoint that you could use to measure the effects of different factors, both physical and biological variables.

That was, I think, the main direction of his work, which I participated in mostly just to know what he was doing, and so on. But from the beginning, I was always doing different things. First the teratogenesis, and then I got into somatic mutations and ended up developing the spot test. And then essentially using the mutations that he had generated for genetic work. I think the way we worked together was that we were using the same system, taking different parts out of it for what we were finding out.

Q: Okay. And this was because the specific locus test basically was a project in which to generate a large population from which to study mutations. Where was that developed, and how --

A: You mean the test?

Q: Yeah. Where did the idea come from?

A: Oh, he developed that.

Q: While he was still at Jackson or…?

A: No, when he first was hired. Dr. Hollaender wanted to very much to have him consult in developing systems, because it was going to be a big expansive thing. So his main consultants were H.J. Muller and Sewall Wright, and they both had very different ideas of what they thought should be done. H.J. Muller wanted essentially -- what's the Drosophila test for mutagenesis? I've forgotten the name of it.

Q: Yeah, I know what you're talking about.

A: Anyhow, it's essentially measuring inductions of recessives. XL -- not it's not the XL – I've forgotten what it is. You take a piece of chromosome, that is marked by an inversion, I think, and then you measure the recessives in that chromosome.

It involved making backcrosses.[64] You can't get the recessives in the first generation. You have to have the individual that carries the recessive generate another one and back cross them to pick up a recessive. It involved three crosses. Of course, we did all the bookkeeping and keeping track and all that kind of stuff. So that wasn't very attractive to Bill.

[64] Backcrosses; Breeding a hybrid offspring with its parent or an organism close in genetic makeup to its parent. This method is used to help identify the genotypes of the hybrid offspring.

What Dr. Wright wanted him to do was essentially pick up dominance, just anything, just look at the first generation and whatever looked different, or whatever was small, or didn't live, and measure dominance that way. Not at any specific chromosome region or any particular locus, but any dominant you could think of. That wasn't attractive to him because it was very dependent on the acuity of the observer. It was not an objective thing.

Then he developed the specific locus tests on his own. So he didn't go with either of the advisors.

Q: Although from what I understand from the history is that Muller was concerned that the scale wasn't big enough to catch enough mutations.

A: What wasn't big enough?

Q: The scale of the project. And that then ultimately you scaled the number of readings up quite a bit.

A: Yes, and the thing is that on a per locus basis, the mouse turned out to be more mutable than Drosophila, so the calculations that were made on the Drosophila rate were, in a sense, pessimistic in terms of what you could get with a certain number of animals. But the attractiveness of the specific locus test was that you didn't have to go farther than F1 [generation], you take them all in F1. So from a point of view of laboriousness, you didn't have to keep track of three generations of two and a back cross. You didn't have to do that.

I think he probably was instrumental in trying to get the scale enlarged because originally -- I wish you could see our original building, but it was three stories high. Originally, we had the first floor. This was at Y-12 [National Security Complex].[65] It was a building that was built during the war for something and never used for whatever it was built for, so it's just chock full of this machinery, huge things that were so big that they had to build up a little pedestal of extra concrete on the floor. In order to get into the building, not only did they have to get all the machinery out, but they had to drill off all these raised places. In fact, they finally gave up on that and decided instead of getting them off, they would build up the rest of the floor to the same level, which meant that in two of our corridors we had little ramps, just about this big.

Anyway, we had originally just the first floor, and that was, I believe, supposed to be sufficient. Then I think Muller did argue for more. So we gradually moved into the second, and maybe five years later we got the third. By that time - this was after Bill had found the dose rate effect[66] - we did need a lot of space, just to pursue the dose rate effect if nothing else.

So whatever I did was always much smaller scale because Bill had to have maybe a dozen technicians, and I never had more than one. I was always working with a single technician.

Q: Muller comes across as a very enigmatic personality.

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[65] Y-12 National Security Complex; A building by the Oak Ridge Labs owned by the US Department of Energy. It deals with uranium and nuclear weapon parts production.

[66] Dose Rate Effect; Dose rate is the amount of radiation absorbed over time. The Dose Rate effect that William Russell wrote about in his paper *Radiation Dose Rate and Mutation Frequency* says that the amount of radiation received is correlated to the number of mutations. See http://www.sciencemag.org/cgi/content/abstract/128/3338/1546 for a link to the full text.

A: Oh, he's great, he's just really great.

Q: Because he does all this fundamental work. He worked with [Thomas Hunt] Morgan[67] and he's going to start a program at [University of] Texas and he ends up leaving for the Soviet Union which he quickly has to leave. Then he has a hard time trying to find a position in the United States again. What was your interaction?

A: Somewhere I have a folder which you might like to see. When they had the first Atoms for Peace conference in 1951, it was in Geneva, and Muller was denied access because they considered him a Communist. Bill got very much involved in trying to get that reversed. So I have a whole folder on Muller and the Atoms for Peace conference. It's fascinating stuff. He got so excited over the thing. It was wonderful to talk to him. He'd get all heated up.

Q: Was he finally --

A: He went to Indiana University.

Q: Yeah, but did he finally get into the conference, the Atoms for Peace conference?

A: Yes, I think he did.

Q: Okay. To go back, because we may come up with this topic in a different way, but I wanted to ask, when you came to Oak Ridge, how important was it to you that you have a professional identity independent of your husband's?

A: Yes, and I think it wasn't too difficult because I was doing something so different. Because my first project was not anything like what he was doing. But at the same time, it was something that Hollaender liked because it was -- I don't know what the word for it is -- project-oriented, because it was something that you can show practical importance, and radiation had something to do with it. So from the funding point of view, Hollaender really liked it. And at the same time, it was very different from anything Bill was doing. So I did have a separate identity.

In fact, some of the lab with the division would organize an annual big conference, maybe the third big one that was already organized around this teratogenesis. So I think I had a very separate identity from Bill within the division and within the whole scientific community.

Although a lot of things we did get sort of lumped together. For instance, he had Roentgen Medal, which is given by the town of Remscheid-Lennep, and they give that annually, internationally. So we got that together, and I don't think they really too much cared that we were doing different things. They sort of lumped us together for doing something with radiation on mice.

Q: Okay. I want to go back -- one question I wanted to ask was, with the project as it began here at Oak Ridge and the requirements to have a large facility to handle the

[67] Thomas Hunt Morgan (1866 – 1945); An esteemed American geneticist awarded the Nobel Prize in Physiology or Medicine for his work demonstrating how genes are the units of heredity in his work with Drosophila. See http://nobelprize.org/nobel_prizes/medicine/laureates/1933/morgan-bio.html.

mice because it was going to be a large-scale project, in what ways were you going to become competitors then to the Jackson Labs, which also had large mouse stocks?

A: I don't think we were because at that time they did little radiation work. Tom [Thomas H.] Roderick[68] for example, who was using inversions as a way to pick up radiation inducement. That was funded by AEC or DOE [Department of Energy], whoever it was at the time. But the lab as a whole, the Jackson Lab didn't really go into radiation, or other types of mutagenesis very much.

Then, of course, in '79 when Bill found ENU [*N*-ethyl-*N*-nitrosourea][69] to be a supermutagen, then labs all over the place would pick up ENU mutagenesis just as a tool for making mutations, including Jackson. But I don't think we were competing with them on that.

VI. The Oak Ridge Labs; Balancing Children and Research

Q: Okay. Describe when you got here at Oak Ridge, which at that point was still primarily a production facility for the Manhattan Project?

A: When we first came here, it was very much oriented toward the Manhattan Project. I mean, it was emerging from the Manhattan Project. Very shortly after we arrived -- in fact, when Bill accepted the job, he was under the impression that the lab as a whole would be administered by the University of Chicago. At the time we came here, it was Monsanto [Company], and the lab is always run by some other group for the DOE or for the AEC. It was Monsanto when we came. And then, the end of that year, it was going to be University of Chicago. That really pleased Bill a lot, that we were going to be under an academic institution. Like Brookhaven [National Laboratory], for example, is under - what?

Q: SUNY [State University of New York]. Isn't that one of the SUNYs? Stony Brook.

A: Yeah, SUNY. And Argonne [National Laboratory] is under the University of Chicago. So that was the idea, and all of a sudden they hit us with Union Carbide [Corporation].

Q: And just entering the facility to get into your work environment, and knowing this was really a military installation that was going to be transferred as a private industry, or at least kind of sponsored by this private industry, what were your concerns about the kinds of science that you would be able to conduct here?

A: Well, I think because we were buffered by Hollaender, and I think he would never have gone into that kind of thing. We were never worried that we would somehow be grouped with a bunch of military work. We were the only division -- at one point, believe it or not, we were the largest division in Oak Ridge National Lab. It's very hard to believe. Because we were doing only basic genetics work. I mean, not we personally

[68] Thomas H. Roderick; A geneticist who worked at the Jackson Labs. He is most well known for using genetics in genealogy.

[69] *N*-ethyl-*N*-nitrosourea; Also known as ENU, this is a very strong mutagen that usually focuses on spermatogonial stem cells. The effect is genome wide. For William Russell's paper on this topic see http://www.pnas.org/content/76/11/5818.abstract.

but the whole division was doing basic genetics work, and more than 70 percent of it had nothing to do with radiation or chemicals or anything. It really was like an academic institution all along, and that was because that's the way he conceived it, and he was strong enough -- and he was really not a very nice person, but he was very strong and very able and very important to have as a guy who went and got the money.

Q: Okay. And since this was pretty much an experiment for him, and Bill was a key hire in order to enact this vision of his, for this facility, how confident were you that Hollaender -- before you even got here, that Hollaender could be able to create an institution?

A: Not before I came here, because really - I had not met him, I didn't know what his vision was, and I really didn't care at that point. I just wanted a place that Bill could have a job and I could have a job. So my vision didn't go very far.

Q: Okay. And just briefly, tell me a little bit about -- I imagine Oak Ridge has grown somewhat since then because it was just a wide place in the road, from what I understand, before the nuclear facilities were built here. Tell me a little bit about what it was like to live here, and then to have children here, where the National Laboratory is about the only game in town, it seems like.

A: Well, there are three facilities. The National Lab was essentially the research facility, in lots of areas; solid state and metallurgy, and there were all sorts of different areas. Then there was the one we passed this morning, Y-12 [Y-12 National Security Complex].

I should go back farther. When the Manhattan district first decided to set up in Oak Ridge, at that time, they were going to make uranium hexafluoride, but they didn't know how. So each facility was going at it in a different way. The Y-12, which we passed today, was electromagnetic separation. They were taking this mixture of uranium isotopes and then with a very strong magnet, they would collect it in different -- it was like a mailbox, it was like a stack of mail slots, and the whole -- you may have seen pictures -- the whole facility, they called it a racetrack, was set around an oval and the magnet was in the middle, and the different isotopes would go into the different slots. So this is a magnetic thing.

Then the one that you have not seen yet, and I should take you on a tour, was K-25, also called the Gaseous Diffusion Plant, and that's where the uranium hexafluoride went around a huge long series of filters that kept separating it by slow degrees. I mean, it was a gigantic facility.

What became Oak Ridge National Lab [original name of Oak Ridge National Laboratory was X-10] was a reactor, which we passed this morning, the original. It's now a museum. They decided -- I'm not sure when, but certainly before the first bomb was built, that the gaseous diffusion would be the one they would stick with. I believe that the material for the first bomb was actually made in Y-12 rather than the gaseous diffusion.

But then they gave up the reactor thing, and it immediately ceased to have any kind of production interest. Then it really became, probably in 1945 right after the bombs went off, it became a research facility. So it never had any kind of production implications. I shouldn't say never, it was after they made the decision that -- so from the time Hollaender was hired, it must have already been in other areas, not just biology, a research facility.

And the town was really very wonderful. When I first came, I was very, very disappointed because we came in from the east end of town. We drove in, we drove down from Maine. At that time, the town was not really finished, and there was a bunch of what was called hutments set up where we first came into town. They were like square plywood buildings with shutters instead of windows, and like the first two miles of town I drove through, that's all I saw, and I almost said let's go back.

The other thing, I was very stupidly worried about getting exposed to radiation.

Q: There's just a bit of irony in that.

A: Yeah. I thought it was in the dust and everything around. So the very first thing I did was buy a vacuum cleaner, and I very carefully vacuumed the house every morning. I did like about probably a hundred times more vacuuming then than I have ever done since then. I was very worried. But I soon got disabused of that.

Q: And who disabused you of this idea?

A: Well, because they were very concerned about limiting radiation exposure anywhere outside of the facility, even within the facilities. And it's probably the safest place in the country to be for not getting exposed. And also, I don't know if you have ever seen pictures of early Oak Ridge. I have a book at home I'll have to show you. Originally, when it was being constructed, there were a hundred thousand people living in this town. Then by the time I got here, it was pretty well on its way to shrinking, but it hadn't -- the current population is a little less than thirty thousand, and it's been that for a long, long time. But the original hundred thousand -- and it had that many. It had all these hutments and all sorts of temporary structures.

At the same time, by the time we came, already it was a very interactive, friendly, and intellectual atmosphere. Not everywhere, but maybe just people that were working at the labs. They started an orchestra. One of the division’s biochemists started the orchestra, and the orchestra is still going and it's wonderful. They started an excellent playhouse, and they had all sorts of councils and God knows what. People were very interactive because there were very few, and still are very few, restaurants. So people did a lot of eating at each other's houses.

The schools were wonderful. They still are. I think they’re among the first hundred in the country, the school system here. It's a top school system. So some people -- Dabney [K. Johnson] whom you met last night, she moved to Oak Ridge originally after her husband died, to have her daughter brought up in the Oak Ridge school system. And there are a lot of people who come here just because the school system is so good.

In recent years, it's become much more sort of a general town. It's no longer so much centered around the people that work at the lab. It's got a bunch of developers wanting to grab the land. I mean, the reservation, the original Oak Ridge Reservation, was fifty-eight thousand acres. It is now thirty-three thousand because of various things that have been built. Even at thirty-three thousand, in satellite pictures, it's the greenest spot anywhere around.

One of the things now is a big concern to preserve the undisturbed reservation. There's some of the environmental scientist division that's done some research into the reservation, too, but lots of it is not disturbed. Just a whole bunch of strong development interest constantly wanting to grab land from DOE to put another subdivision over there. So it's become very different.

Q: Okay. Well, you basically finish up your Ph.D. thesis when you're here, and in 1950 you have your first child? Is that correct?

A: Yes, I did my finals at Chicago, some time in the late summer of '49. And I decided not to have any children until I'd finished my thesis. So then I had my first one in late '50. And I was fired! I mean, not fired but they did not have maternity leave. You had to be terminated and rehired.

Q: And was it a guarantee that you'd be rehired?

A: Well, the funny thing is, not with my first one, but my second one they did have a maternity leave, but the idea was it was dangerous to have an embryo around radiation. I could have told them that they didn't need to worry about the first three months, but they didn't care about the first three months. I didn't have to leave until like four weeks before due date. But I lost three months of my accumulated work credit because I was fired – terminated - and had to be rehired.

Q: Well, you were concerned when you moved here about if the radiation would be in the dust and in the air. Were you concerned whether there was enough radiation in the environment, or that the nature of your work would expose you more or less than the -?

A: These things [showing a radiation badge] measure how much radiation there is. Over all the years, I've had much less than the average population.

Q: I used to work where I collected those radiation badges. When did radiation badges begin to be used?

A: They've had some, not this kind, but they've had some ever since I worked here.

Q: Okay. And how did you decide when would be a good time to have children?

A: Well, I wanted to have them -- if I hadn't had to finish my thesis, I probably would have had them a little earlier. This is also a very good place to find people to stay with the children. I never had to send them to nursery school because I always had someone in the home, a really, really wonderful woman. We had a little bit of trouble to start with and had turnover, but from the time that my first child was about three months old until my youngest child was sixteen, we had the same wonderful woman. They just loved her. It was great.

Q: At this point, the work here, the specific locus test was generating a lot of mutations, and there was a lot of data to be analyzed, or might be analyzed. How did you see this impact having your child bearing years come at this very -- which could be a very productive time scientifically for you as well. How did you think you could balance?

A: I didn't hear the last sentence.

Q: How well did you think you could balance both child rearing and a scientific career?

A: Well, I think that probably both careers were somewhat impacted by having both of them. I'm sure that I would have done a lot more work if I had not had the children, because I very much put them first, particularly when they were little, even though I had Inez at home all the time. I would be the one to take them to piano lessons, and I would be the one to take them to school events and all these things. Of course, if they were sick, I would not come in to work. So it did take a fair amount of my time. And probably during a period when I was doing some of the more interesting things. But I don't regret it. That's where Mary [Lyon] had an advantage over me because she never had a family, so she could really single-mindedly pursue something.

Q: Well, and what concerns did your husband have, or did you discuss these kinds of concerns of what children would mean for his career, and balancing the responsibility of fatherhood with --?

A: I think he's pretty much -- I mean, he's a wonderful father, and he's really devoted a lot of time to them also. I think the nature of the work was different to the extent that he could afford more time of not doing things, because once he started one of these big experiments, he had a dozen technicians taking over, and then you had to analyze the data. Of course, it wasn't just one experiment, there were a stack. Even so, it was much easier in a way for him not to be so involved in the work directly.

He also was very much involved in -- because of the human implications, he was on a lot of committees that he got put on to, and also that he felt he should be on. I mentioned the United Nations [Scientific] Committee [on the Effects of Atomic Radiation],[70] which was working quite a lot. Then he was on the BEAR Committee, Biological Effects of Atomic Radiation.[71] That was a National Research Council[72] committee, and it became the BEIR [Biological Effects of Ionizing Radiation] Committee. I don't know how many installations of that, but he was on all of those.

I actually got on a lot of those later on, but that was after the kids were grown. I was on several of the National Research Council committees, and I was on ICPEMC [International Commission for Protection against Environmental Mutagens and Carcinogens], believe it or not. Ever heard of that?

Q: No.

A: Oh God, what did it stand for? International Committee for something mutagens and environmental carcinogens, or something. That gave me a lot of nice interesting trips to Europe. So committees took also much more time out of my work than I would have wanted to. But the worst thing that happened was when I had to go into administration, because in 1975 I became Section Head. That was really a damper. And I didn't do very much interesting stuff after that. I still did a lot of work, but it was not as exciting as the stuff before that.

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[70] United Nations Scientific Committee on the Effects of Atomic Radiation; Created in 1955 by the UN, this committee analyzes global levels of ionizing radiation and its effects. See http://www.unscear.org/.

[71] Biological Effects of Atomic Radiation, aka BEAR (1954 – 1964); An organization established to study the effects of radiation on living organism and to make sense of the data already published on radiation. There were six subcommittees under BEAR studying particular topics.

[72] National Research Council; A US governmental organization which seeks to educate public figures on matters of scientific policy.

My original section was over a hundred people, so I got involved into all those little fights and conflicts between people, and getting the funding, and having to talk to visiting Washington people. It was awfully time consuming.

Q: And when children are young, that probably precludes many women from getting involved in these administrative duties.

A: Yeah. It did take so much time, but on the other hand, I didn't have to do so many of the physical things that other women have to do. I didn't have to do the cooking and cleaning the house because I had Inez to do that. So that was really great.

Q: And what expectations did you have for your children? Were they able to escape this very unique situation?

A: I didn't have any real -- not like my dad had with me, I don't think. On the other hand, I should have probably. I really wanted them to do what they wanted. Neither of them had gone into science. I guess my daughter's the closest to it because she got her master's in physical therapy, so she got a fair amount of sort of medical-type courses in connection with that.

And my son was really a musical genius, but he never did anything professionally with that, mostly because it's too hard to do anything professionally and earn a living. But he got -- his original college degree was in music, and then he went back and got a master's in computer science. That's not really science.

So neither of them did -- and we've always wondered why. I think Bill's hypothesis was that when we came home and talked at the dinner table, we would spend much time bitching about Hollaender or bitching about what was going on in the lab that was ornery. Like if you had to renew your badge, and you had to fill out this and that. We would spend our dinner times bitching about these things instead of talking about the exciting things we were doing. So that's his theory of why they didn't.

Q: Okay. Well, I think we've covered a lot of ground today, and we're at a good place to stop. We'll pick up again tomorrow.

A: Okay.

VII. Thoughts on Women in Science; Further Discussion on the Relationship of Home and Work Life; Collaborations with William Russell and Scientific Writing

Q: It is January 19th, 2007, and I am with Liane Brauch Russell at her office here at Oak Ridge National Laboratory. I'm Andrea Maestrejuan and we're here to conduct the last session for her oral history interview for the UCLA Human Genetics Oral History Project.

I wanted to follow up with some topics that we talked about a little bit yesterday. One of them is just a general question about women in science, because we do have these figures, like Rosalind Franklin[73] and Barbara McClintock[74] who, as you said before

[73] Rosalind Franklin (1928 – 1958); An English scientist and x-ray crystallographer who determined the structure of DNA.

in talking about Mary Lyon, have made some sacrifices in their personal lives. So we have these women who are loners, working alone, as role models. How accurate do you think that is for women, that they either have to choose between a very rich professional life or family life, or settle for something in between the two if they want both?

A: I do think that the ones that were loners and did not have families really worked a lot harder, and maybe *had* to work harder in the type of environment they were in. That surely was true of Rosalind Franklin and Barbara McClintock. I don't know whether they made the choices purposely, if they had not chosen to work so hard whether they would have had families. There is a trade-off. You do not work so hard, you do not pursue your goals so single-mindedly if you do have other interests. Maybe they are families, but sometimes they're just some other outside interest that somebody also has an avocation or something like that.

I think the other thing also is that when you do go single-mindedly, you also become more competitive and you're looked upon as being -- at least you used to be looked upon as being bitchy or abrasive or something because you were making your way as a woman. I think some of them really were bitchy, and certainly not the ones that you have mentioned. I don't know Rosalind Franklin. I have met Barbara McClintock. In *no way* was she bitchy or abrasive. But some of the geneticists I knew, a few of them, were.

Really, many of the ones I know – women - did not have families. I'm trying to search my memory here. Well, in the division, we had, very interestingly, a lot of husband-wife teams, in the biology division. There were the Popps [Raymond A. and Diana M.]. They were not really -- I mean, she was not really a geneticist.

There was Rhoda [F.] Grell. Now, Rhoda Grell was a special case. She's still alive. She is really very brilliant. I think she's extremely brilliant. She was definitely paranoid about how she was being suppressed by male colleagues. She did not have the conflict of a family. She had a son, I think, from an earlier marriage, who was pretty well grown, and I think she did not really become a research person until after he had left home, so she did not have the family conflict.

She was not really bitchy, but she was paranoid, and she had reason to some extent. They really *were* trying to suppress her. She was working essentially on gene duplication in relation to pachytene[75] stages, when did crossing over occur, things like that. People were really trying to sort of push that idea into the background. I think she was exonerated later on, but I don't know how much credit she got. So I think paranoia was justified in some cases, at least maybe in the sixties or seventies or something like that.

There is a trade-off with having a family. You do not go to the lab at all hours of the day and night, you do not read all the literature, and you don't push yourself, you don't go to the meetings that would advance you and get your message spread to the extent.

Q: How much do you think that these behavior types in women are typical of all scientists, or specific to women scientists. This kind of competitiveness and what you call bitchiness, is that something peculiar to females working within the sciences?

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[74] Barbara McClintock (1902 – 1992); An American cytogeneticist who was awarded the Nobel Prize in Physiology or Medicine for her work on maize cytogenetics.

[75] Pachytene; The third stage of prophase in meiosis, the cell division that results in gametes. It involves crossing over of genetic information.

A: No, it isn't. But on the other hand, the males don't have the family conflicts to the same extent. They do make the trade-off, but I think they're more often in a trade-off in the direction of the work, because they're able to forego some of the family obligations.

I don't know to what extent that's still true, but -- well, I had a colleague here who really did most of the caretaking of their child because his wife was an M.D. I don't think that he did very much at the expense of his work.

Q: We kind of mentioned this on Wednesday evening, that the life sciences, and genetics in particular, seem to have a striking number of women who made significant work in our part of the canon of the history of genetics. You might argue that that's true for the life sciences as opposed to the physical sciences, to a lesser extent chemistry. Why do you think that was the case for say your generation of women?

A: Are you asking why I think that's true?

Q: Or if it is true, if that's just an anecdote that I'm emphasizing, which may or may not be true. And if is true, what do you think accounts for the number of women?

A: Honestly, I think it is true. I think there are a large number of women in genetics. Particularly in -- I was going to say in mammalian genetics, but that may not be totally true. I know quite a few in mammalian genetics, but it's probably true of other model organisms too. I don't know how it stacks up in human genetics. I don't know why that happened, whether it's just a role model that occurred early. It's not because it's easier, that's for sure.

In many ways I think genetics is much more intellectual challenging than say field biology or something like that. It's really very mathematically oriented, and according to some ideas, the female brain is not as mathematically oriented. I don't think that's true, but it certainly would argue in the opposite direction. Genetics requires so much logic and so much inference from data that are not -- it isn't that you do something and find something, but you make an interesting inference from vaguely related findings, and it requires a lot of that kind of thinking. I think that's more of a mathematic type of thinking. You certainly can't account for it from that argument.

Even in the mutagenesis area, Charlotte Auerbach[76] - I don't know if you know her, she worked during the war, and she made quite a few contributions. I don't know whether she was a role model. I don't think so, because her work was fairly secret. Barbara McClintock. I'm trying to think of the people in the late forties and early fifties. Mary Lyon started working around that time.

It still is not a huge percentage. Has anybody done the statistics on it?

Q: No, I don't think so. What changes have you seen over the course of your career in regards to the work environment for women who want to do genetics research?

A: Well, certainly the environment I was in, and it may not have been typical, was very favorable. I mean, there was really -- I don't think there was much discrimination, sex discrimination. For one thing, as I mentioned, we had a whole lot of -- in the division -- of couples working, not always both in genetics. It could have been two different fields.

[76] Charlotte Auerbach FRS (1899 – 1994); A Jewish German scientist whose work on the effects of mustard gas on fruit flies established the field of mutagenesis.

Well, Dr. Hollaender, bless his heart, who was always very careful about saving money and using it for some other things. That was really one discrimination, and that was salary, that, at least in our case, the female member of the pair got a *much* lower salary, because Hollaender said, "You don't need it, your husband's making all that money." I mean, he said it quite openly. So that was a discrimination.

But certainly in terms of what you could do to advance your research, and so on, there wasn't any discrimination in that way, other than the type of -- the women had the same struggle the men had in trying to make their case for funding within the division. Of course, the outside, I don't know -- for grants, I don't know whether there was any in those days or not. But within the division, everybody had to fight for intradivisional funding.

Q: And what opportunities did you have to address these differences in salary?

A: I think you could probably go and scream at him and you probably would have been successful. He was very susceptible to people who stood up for themselves. It's something I didn't do, and probably should have done. And even Bill didn't do, because he was always very nebulous. He probably couldn't tell you what his salary was, and that kind of thing. If you didn't go and scream for a raise, you didn't get it.

Nowadays, after Hollaender left, and this is lab-wide, this was no longer the case because they had a performance appraisal process put in place, which was very -- you know, in a performance appraisal everybody had the same chance. But before that, you had to go and scream for raises. Bill didn't do it because as long as he had enough to get by on, he didn't really care that much.

Q: Okay. Well, we talked a little bit about this yesterday, but how well were you able -- because you and your husband had separate research projects but you did publish together, and certainly the research was coordinated, how well do you think you drew the lines between what's at work stays at work and what's in the family stays -- separating the home life from your lab life.

A: I don't know how you would have had the family in your lab life, other than allowing the kids in the lab, which was not allowed for external reasons. So I don't know how -- we had an open day when you could get them through in the lab, but other than that, there was really no way to get your home life into the laboratory. Of course, the other way you could do, you could take your work home, and we did a lot of that. A lot of the writing was done at home, a lot of the talking about the work was done at home.

So it worked one way and didn't work the other way, but I don't know whether we would have consciously avoided taking it home. I mean, the tendency was actually to do more of it at home because the kids were there. So if you had a choice of writing a paper in your office or writing it at home, you would write it at home, after they'd gone to bed, that kind of thing.

Q: It seems like it wasn't so unusual that there were working couples here at Oak Ridge, but certainly in science in general it's unusual. If you were collaborating on a paper together, for instance, how would you coordinate your collaboration?

A: You mean how Bill and I…?

Q: Yes.

A: Well, we knew at all stages of the work what the other person was doing. They would talk about data as they were coming in, and what was interesting. As far as the writing went, one of us would always do the first draft. I don't think we'd ever say you write this part and I write this part. I think one of us would write the whole first draft, but then we had a lot of input from each other in subsequent drafts. So by the time I had a paper that I considered finished, it had really been through the mill. I mean, it was very polished because Bill was a very good critic. I found out I didn't have to send it to an outside review, but of course we did. We had a very rigorous intradivision reviewing process before it even went outside. So that was really the collaboration on the papers, that one would write the whole thing. And it was just very natural as to who would do it.

I did write the first drafts for lots of Bill's data because he was not a heavy publisher, and it harmed his career not to publish. So I would write the first drafts for quite a few of his things. I think the reason he wasn't a heavy publisher because he was such a perfectionist. He always thought it wasn't quite done. For instance, on some mutation rate thing, he'd say, "Well, all the mutants aren't tested yet." It didn't really matter so much if they were all tested or not, but he was a real perfectionist. He wanted to wait until the last possible piece of data was in. I would say, "Hey, let's get it out, and then later on if you write a review paper or something, you can add the additional things." So from that point of view, I think there was some conscious taking over.

The dose rate paper went through incredible back and forth. I mean, that was his data. But it was very important to get it out right because it was so totally going against dogma. We must have had that in the back and forth stage for a couple months at least.

Q: And how did you handle disagreements in either style and/or content?

A: No problem. [I] usually accepted things as good suggestions. I think I objected to some of the style changes because I maybe put a little more emotion into my style. (chuckles) But I think I really just learned a lot from the criticisms. I learned to be very rigorous and very sure that what I was saying was not easy to misconstrue because of the words that were used.

Q: And what was it about you that you were able to take some of his data -- he was reluctant to put together until he had the final experiment done. What was it about you that you were able to just go ahead and draft something?

A: Because I write very easily. We had a staff member who had to write one paragraph on something. It was not a scientific paper. I watched him, and he was physically in agony writing this paragraph. And I just dashed things off very easily. As I say, Bill was such a perfectionist, he didn't.

What I would sometimes do -- and this was pre-word processing, pre-computers. He might sit in an armchair and say a sentence that he was going to [write]-- and then he would go off on something. It was really in the very early stages of thinking out writing. And I would write down everything he said. I had all these embryonic fragments of thoughts down on paper, and then later on we would find them very useful. Because I did not want him to wait until he had the perfect sentence before he got it down, because I knew that he would go back and forth and think, “No, this is not really right,” and in the meantime, nothing would get written down and he might lose the thought. So I was just writing down all sorts of junk thoughts sometimes, but very embryonic thoughts and parts of sentences. That we did a lot of.

Q: You mentioned that early in your undergraduate career, or high school career, you had won an essay contest.

A: Yeah, when I was a freshman.

Q: And you had seriously considered -- you had gone around publishing houses and seriously considered a job in writing. How did that idea of writing contrast with the kind of writing that one does to get scientific papers published?

A: Well, it's very different, because I also, at that same time in my freshman year, maybe the next year, I did write a few short stories, which I did not send off anywhere. That, of course, would be very, very different. But the essay type thing was not that different in a way because I had to have logical thoughts and consider different possibilities. So that wasn't that different. But the fact that I had written them for non-scientific things I think just made it a lot easier for me to write without agonizing over it. I think this is where the word processors are so wonderful. You just write whatever's in your head and it doesn't matter, you can come back to it, you can move it around. And that's the kind of thing that held people up from writing, that they couldn't do it when they were writing it down.

Q: And how do you teach in your own staff and research assistants and whoever else you might have had come through the lab -- postdocs I'm not sure about but we can talk about that -- how do you teach one to write well?

A: This is a very real problem. When I was in high school in England in sixth form, that I mentioned yesterday, which is really post high school, every other week we had to write an essay on anything, and the alternate weeks we would write what we called a précis. A précis is essentially like an abstract. The précis had to be a certain number of words, and it was great training. I wish they would make students in high school do that, to take the essence out of information and get it down in logical sequence. So writing a précis every other week, boy, that's the only way.

But I think by the time they have come here and have got their Ph.D. and everything, it may be too late, I don't know. Or else take a year off and write précis, you know. The newsletter I gave you yesterday, that's essentially a series of articles. I don't know how much chance you had to look at it, but for each article I have probably ten, twelve information sources, and I try to get it all together into a paragraph this long.

So I have continuous practice in writing, and I will dash it off. The really hard thing is to get my information sources together, and once I have those, it's very easy to write.

VIII. Comparing Oak Ridge to a University and to Jackson Labs; Funding; Role as Division Head at Oak Ridge

Q: To get back to when you came here to Oak Ridge, you had basically come here with this project in mind, to create a large number of breeding experiments to study mutation. In terms of your own work as a scientist and your own identity as a scientist, what did you foresee the kind of the results of the breeding experience?

A: How much did I foresee?

Q: Yeah. Did you foresee that this would shape the content of your research for the next couple of decades?

A: No. It went along as each thing came up sort of. For instance, I did the teratogenesis and nothing else for maybe three or four years, or even longer than that. Then I did the somatic mutation I always wanted to do. I had that in mind from -- in fact, that's why I got in to the teratogenesis, accidentally, because I was doing the preliminary. So that was a direction I had very much in mind, and it was something that was going to be different from the germ line mutagenesis, very different.

But then the other things arose. For instance, we would get mottled mutants, and that took me into the whole area of the variegated position effect and into the X chromosome area. And spontaneous XOs occurred, so that took me into the area of just finding out more about induced ones, and also about spontaneous ones. I mean, the first ones were spontaneous, and we had not really thought -- I just went off and decided what is a good way to find out when they can be induced in relation to the meiotic cycle[77] or post-fertilization events and things like that.

So there was this back and forth between doing this from the genesis of the thing, from sort of the basic information. That then became a test system, and once I got the test system developed, then the tests would supply things that again would be worthy of basic work. So there was a constant back and forth between the programmatic and the basic, I think.

Q: And how much do you account for your success in being able to build and build and build upon the initial work that you started here that has lasted a lifetime of research work? How much is that a unique situation that was here at Oak Ridge, as opposed -- and as you stayed here, this might be a counterfactual question -- say, for instance, if you were at a university and not in a laboratory setting like this?

A: If I had been at a university, I probably would not have had the mouse facility. I mean, in another field this might not be true, you might have the same kind of facilities in a university and here. And, of course, the teaching load would have taken away from my research time. On the other hand, I would have kept up with the literature a lot better. I probably would not have the pain of having to go and get to the literature.

In our mouse facility over at Y-12, which was a very clean but conventional facility, I could walk from my office across the hall, literally. There was a mouse room right across the hall. I didn't have to put on a gown, I didn't have to take a shower. And any time a thought occurred to me, I'd go over there and say, well, let's look at these litters and see what's -- you know, whatever. If I decided to make a particular cross, we would probably have all the strains right there. We wouldn't have to import anything.

And we'd have other mutants that would be very important, like for instance, with the translocation stocks that had the X-linked translocations, they were pretty hard to maintain, the stocks, to propagate. The females were semi-sterile because of the translocation, and not only are they semi-sterile but they're worse than semi-sterile because physiological impact on the mother of the translocation. So you get litter sizes of maybe two or three or something like that instead of a normal litter of seven or eight. They're pretty hard to maintain.

Probably, if I had not had all the choice of stocks, I could pick from any number of stocks to cross the translocations that would be the right genotype to give you phenotypic recognition -- you know, it would tell the segregants and at the same time

[77] Meiotic Cycle; A cell division cycle where the number of chromosomes is reduced by half.

might make the stock easier to keep, I could just go pick and choose. In a university, I would have to write to people and ask them to send me different stocks and all this kind of stuff.

I think as a mouse geneticist it was ideal, it was really ideal. I would not have been able to do half of what I did. Plus the fact, of course, the mutants came out of Bill's experiments.

But going back to the university thing, the thing that some of us missed was having students, graduate students. Then when the lab instituted the collaborative project, the UT [University of Tennessee]-ORNL Graduate School of Biomedical Sciences [now called the Graduate School of Genome Science and Technology], then we were able to have students and take them through -- Dabney, whom you met on Wednesday night, she went through the UT Biomed when she was in her forties. She was a great student. She was not my student. I was able to have two or three students that way, and that was help[ful]. And also, we were able to give lectures.

Q: Was this out here at Oak Ridge, or did you have to go to the university campus?

A: It was physically at Oak Ridge, but they got a UT degree. They had to take some of their course work over there because we didn't offer the whole basic Ph.D. type course work. The courses we gave out at the laboratory were pretty specialized, so they had to take much of their basic work at UT.

Q: I'm going to be jumping around a little bit as we try to cover some different areas, but to continue along in this vein, typically in an academic setting, you would have a phalanx of technicians, graduate students and postdocs, and maybe visiting scholars. How did you get the hands in your lab to help you, and how easy was it to attract competent technicians and research assistants here at Oak Ridge?

A: Bill's work required a lot of technicians. It was not so technically demanding in terms of wet labs type of work, but it was very demanding in terms of recognizing mutants and that kind of thing. The technicians that we started to attract right in the early fifties were mostly -- I was going to say out of southern colleges, but that's not entirely true, but many of them were out of southern colleges.

And they were almost entirely females. I don't know whether that was a conscious choice or whether most of the science majors in these colleges, if they were male, most of them might have gone on to graduate school and most of the females went on to look for jobs. That could be part of the reason. But somehow we ended up with almost entirely female technicians. Not until later, in the sixties, did we get a few male technicians.

They got quite a bit of training on site, not only in terms of what they had to look for, but the whole system of record keeping was very well developed by Bill, and they had to learn the record keeping. For instance, every mouse had a pen tag, a breeding card that recorded the litter of a particular female, plus the distribution of segregants, and a ledger record. So everything was cross-referenced three ways. You could look at any one mouse -- I sometimes demonstrated that to visitors. We'd pick up a mouse, read the earmarks, and I could go back and find that mouse's ancestry like six generations ago in a space of ten minutes, by going through the regular ledger records and the card records. So they had to learn that.

And they had to learn some very general things, which were very important, like if you make a mistake, own up to it, you're not going to be punished, but if you cover over

it you might screw up the breeding for generations to come. Very general things like that.

We had a great, great collection of technicians. We never had trouble really. Maybe in the course of the years we had maybe three real lemons, but most of them were just really good. In fact, upstairs is Pat [Patricia R.] Hunsicker, who was -- it was like a little army, and there was usually one general, one of the technicians. Pat Hunsicker became the general in the maybe eighties, and she's now the only one that's still working here. She is in charge -- we have all of our mutants and stuff on a database, which is accessible to the world, and people are often asking. They're all frozen, unfortunately. When we moved, we had to freeze them all. So she's the person who will know most about any of these things.

You were asking about students. I mentioned the graduate school, but also we had a lot of visiting scientists, a lot of them from abroad, but not always. Some of them stayed a long time, really became part of the program. There was no difficulty because most of them got some of their pay from their home institutions because it was considered good for them to be working in a group. I don't think they cost us terribly much.

In the old days, we used to be pretty affluent. Hollaender was great at getting the money. The AEC had lots of money in those days, and they were very willing to support basic research. I don't remember ever having to apply for an NIH [National Institutes of Health][78] grant or anything like that. In those days, it was very easy to get money. Then in the eighties, things started shutting down.

Q: And then what were your funding opportunities for your work?

A: I had never had to apply for -- I did have a large interagency agreement, a very large one, that I got through NIEHS, that's the National Institute of Environmental Health Sciences.[79] They supported a lot of the chemical mutagenesis work, and they were very good also at supporting what came out of it in terms of analysis of say germ-cell stages and things like that. But that was most of the fundraising work that I had to do. That was a fair amount of work. I had to really put myself out to get the agency contract and to keep getting it renewed and to have it broadened so it would support more and more people in the section.

Q: Well, how would you contrast Jackson Labs and the lab here at Oak Ridge?

A: Well, Jackson didn't do any mutagenesis really, except for what Tom Roderick was doing, and that was a small project. It was a really important one, but it was not expensive or a big part of what the lab did. And I think he had some AEC funding.

We were really, in terms of the funding, totally mutagenesis-oriented, and Jackson probably maybe 3 percent mutagenesis-oriented. Their big income came from selling mice, so that was a really big part of Jackson, and we did not. We distributed a lot of mice, we had a huge distribution, people wanting this and that mutant, but we never charged for anything.

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[78] National Institutes of Health; The United States governmental organization in charge of regulating scientific research. It operates under the Department of Health and Human Services. See their website for more information: http://www.nih.gov/about/index.html.

[79] National Institute of Environmental Health Sciences; This institute is focuses on how the environment influences disease. It is part of the US Department of Health and Human Services.

We were able to also -- but that was only in later years -- I think get some industry funding for specific contracts, like Waldi [Walderico M.] Generoso, who was our chromosome aberration[80] guy, got some for some artificial sweetener testing, I can't remember which one.

And I got one small one for triclosan.[81] Do you know what that is? It's in Dial soap, in the antibacterial soaps. So I tested that with a spot test, I think. The spot test was fairly popular for industry to test thing, and I was able to get this outside funding. It may have been the first time we ever got outside funding, and it took a bit of red tape. I didn't like it. I didn't like the requirements, how I had to report to them and how they were sort of pushing me and showing it really wasn't a bad thing. I did not like it, and I did not encourage it. I think it's the only one that I remember having.

Other than that, it was all government funding. I don't know to what extent Jackson had government funding. I know they would get individual grants, NIH grants, or so. In later years, of course, most recently, everybody who works here now *has* to have an NIH grant or they don't get anything, [not] enough to support them. And they've become much more program-oriented now, the DOE is almost totally program-oriented, and they don't want to fund anything that isn't related to Homeland Security.

Q: Is any salary support given by the national lab now, or does it all have to come out of your NIH grants?

A: To the people who work here now?

Q: Uh-huh [yes].

A: I do think they get it, but I really don't know because I have very consciously divorced myself from anything administrative, which I'm so happy to do. I get all these e-mail messages and I press “erase”. I don't know. I really probably should. I'm sorry.

Q: That's okay. Why have you shied away from administrative duties?

A: Well, I was section head from '75 to '95, so those are long twenty years during which I really did much less research than I would have wanted to, and I really did the kind of research that I could more or less assign to technicians. It was not the most exciting period of my research career, and I was very glad to drop that in '95. It was taken over by Rick [Richard P.] Woychik[82] at that time, and then subsequently by other people when he left. But I was still working.

I was still getting paid until 2002 I think is when I finally retired. I felt I had to know something, but I didn't have to *do* very much. Since then, of course, I guess I'm no longer employed, so I can do essentially as much or as little -- in terms of knowing. I'm certainly not expected to *do* anything.

Q: As division head, what were your goals?

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[80] Chromosome Aberration; Any abnormality in a chromosome’s structure or the chromosome count. It can be deleterious, beneficial, or have no effect.

[81] Triclosan; An antibacterial substance commonly found in cleaning supplies.

[82] Richard P. Woychik; A noted geneticist who worked at the Jackson Labs. He is best known for his work on obesity-related genes. For a biographical sketch see http://www.jax.org/news/archives/2002/woychik_outline.html.

A: My goals were really to get us into the molecular era. I think I was fairly successful in that because it all started -- well, we hadn't talked about this yesterday. It was really the complementation studies with the mutants. Because there were so many mutants at each locus, the complementation was very fruitful and we were able to generate what would then be considered almost fine structure maps, just from the complementation.

So through the complementation studies, we were able to, just by the breeding results, identify new lethals or sub-lethals or even visibles that were in the whole complex. And the complexes were of different sizes and at the different loci. They ranged from maybe two or three centimorgans to ten or twelve centimorgans.[83] So there were fairly long stretches of different chromosomes.

When we generated the genetic complementation maps[84], it immediately seemed, well, this is something that you can really go a lot farther with if you can characterize some of the new mutants and also get a much more detailed map, and we had to go into molecular.

I collaborated with -- and this was through the dilute-short ear region.[85] The dilute-short ears, right from the start, Bill thought it was a good idea to have a couple of linked loci. We actually had two sets of link loci among the seven. And the dilute-short ear are very close. They're .6 centimorgans apart. So really early, like in his very first paper, he was able to tell whether he was inducing a broad proportion of smaller deletions, or even large deletions he was inducing, as compared to maybe smaller lesions.

Because of that, the first complementation test I did was in the dilute-short ear region. I did that actually in the early seventies. Then Nancy [A.] Jenkins and Neal [G.] Copeland,[86] who were interested in dilute because the dilute mutation itself was due to a viral integration, and they came at it from that direction. So they were interested in dilute. I started collaborating with them with the mutants that we had characterized on this complementation map, and many that we had identified as being overlapping deletions of different lengths.

Then, after I started collaborating with them, they had a postdoc at the time, who was Gene [Eugene M.] Rinchik, so Gene came to work for us after he had finished his postdoc with them. So he started out on the molecular characterization of some of the dilute-short ears, and later on got into brown [locus-region].

Well, he also characterized a class of mutants that we had not been able to do much with genetically because they are hypermorphs[87] as far as the phenotype is concerned. But they're viable, and that particularly applied to the albino mutant. So he, for instance, characterized a group of viable albinos that yet some of them turned out to be very small deletions, intralocus deletions. So he was the first molecular geneticist we had.

Then he and I together knocked ourselves out to convince DOE that we really needed more people. The man who was in charge at that time of -- I don't know what

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[83] Centimorgans; A measurement that signifies distance along a chromosome. Usually 1cM is equal to about 1 million base pairs

[84] Genetic Complementation Maps; A gene map that describes each mutation and shows whether it complements with other mutations.

[85] Dilute Short-Ear Region (d-se); A region of the mouse loci that is integral to development. For a sample of Liane Russell's paper in this topic see http://www.ncbi.nlm.nih.gov/pmc/articles/PMC1202704/.

[86] Nancy A. Jenkins and Neal G. Copeland; A husband and wife duo of geneticists noted for studying human diseases, particularly cancer, in mice.

[87] Hypermorphic mutation; a mutation which causes an increase in normal gene function.

they call it. It's part of administration of the biological research. His name is [Charles] DeLisi.[88] He's a molecular geneticist himself, but he was working purely administratively.

Anyhow, we went up and we gave him a long propaganda presentation. We ended up to get funding for an additional one, and that's when we got Rick Woycik, and Rick hugely expanded the type of thing we were doing because he was doing insertional mutagenesis. Well, he himself wasn't. We got another one, [J. Frederic] Mushinsky, who was doing insertional mutagenesis. Rick was doing -- well, he was shooting pieces of DNA essentially into fertilized eggs. So that's just a totally different area.

So really, through Gene and then Rick -- and Rick ended up with a whole slew of students who were very good students and very much broadened the whole area. They were doing the (a) [agouti] locus and some of the non-specific -- we also had a lot of mutants that were not specific locus mutants, things that just popped up at other loci, non-marked loci. One of his students, for instance, decided to go into skin mutants.

I think the molecular expansion probably started from the time I collaborated with Nancy Jenkins and Neal Copeland, and then really got going after Gene Rinchik came here. And then Rick, fortunately, wanted to be section head. That killed him too eventually. [laughter]

IX. Modern Authorship and Today's Research Environment; Spontaneous Mutation; Research Techniques

Q: How did molecular techniques change the nature of your work? Because in the fifties and sixties and seventies you really collaborated with other members in the lab. It wouldn't be unusual for you to have a single author publication, or maybe one or two other people. But, for instance, in the shorter mutation when you're cloning out the gene, or your collaborators are, in the *Cell* paper [Kingsley, DM, et. al. The mouse short ear skeletal morphogenesis locus is associated with defects in a bone morphogenetic member of the TGFß superfamily. *Cell*. 1992 Oct 30;71(3):399-410.], there's three different institutions, there are six or seven different authors on this paper. How did molecular techniques begin to change the way in which you conducted your research?

A: My own? I never did any of those molecular techniques. These were my collaborators. I would essentially furnish the genetic information and do what other things I thought were necessary to do from a genetic evidence point of view. And, of course, we had the mutants.

Recently, last year, I communicated a paper to *PNAS*[89] [Smyth, IM, et al. Genomic anatomy of the Tyrpl (brown) deletion complex. Proceedings of the National Academy of Sciences of the United States of America. 2006 March 7; 103(10):3704-3709.] that's on the brown locus. It's really based originally on the complementation record of the brown locus. That has sixty authors. Ian [J.] Jackson is the corresponding author - sixty authors.

On that one, I really didn't do much other than -- I was not an author on that one, I communicated the paper. But Dabney Johnson was an author, and she essentially had characterized a couple of the mutants they used.

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[88] Charles DeLisi; Currently Professor of Science and Engineering at Boston University, DeLisi was head of the Department of Energy's Health and Environmental Research Programs from 1985 to 1987.

[89] Proceedings of the National Academy of Sciences of the United States of America; *PNAS* is the journal of the US National Academy of Sciences specializing in printing biomedical sciences articles.

Nowadays, you pick these papers, everybody's a specialist in something. No one person knows even at least half the techniques that have gone into the paper.

Q: And what accounts for this dramatic change, it seems to me, in the course of a few decades, that you essentially could do all the experiments and write the paper with you and one or two other people in your own lab to this consortium of research?

A: I think in a way I'm glad that I grew up in the former era, because you have much less of a part in anything now. I don't even know whether most of these people don't even know each other. The paper that Dabney and I were talking about on Wednesday night that I communicated, which was rejected, it was due mostly to the collaborators, the guy who works here who was the corresponding author. I think his part was pretty good, but I think some of the collaborators' stuff was awful, and I don't think he even knew some of them. They certainly don't get together and talk very much. There wasn't anything like Bill and I, for instance. And I would consider it much less satisfying. It's a good way to get your list of publications greatly extended without doing a hell of a lot of work, but it's not really your baby.

Q: So, do you have a different sense of value to the work that you do in this later era than you did in the former period?

A: I think the original complementation work I felt very satisfied by. I thought that really was my baby, and some really interesting conclusions one could draw, what grew out of it. By that time, it was no longer -- I was renting my baby out to a lot of other people.

That doesn't mean that there wasn't some other things I did that I wasn't very interested in. But that was mostly analysis of -- well, of our findings without having to go into the lab and do anything. And some of them I wrote after I no longer had access. I don't have any research funds.

The analysis, for example, of certain mutagens that will -- first of all, the stages at which they're most effective. At that time, there was nothing that was most effective in between meiotic divisions and basic -- the mitotically dividing spermatogonia.[90] In between, nothing was particular effective until we found etoposide.[91]

So I wasn't as crazy about testing a bunch of chemicals, but once in a while you'd come up with something that was very unusual. So etoposide was affected in packaging. And we said, okay, if it does that, it may have influenced crossing over. So I got into this whole area of chemical effects and crossing over, which really has not been very much known, so I considered that pretty satisfactory.

That was probably due to the fact that, first of all, I had access to data on dozens of other chemicals that I was able to put together, able to relate germ-cell stage to chemical. And that we were lucky in finding one that acted on crossing over.[92] Just like Bill found ENU. That comes out of testing dozens of uninteresting chemicals, and all of a sudden you find ENU.

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[90] Spermatogonia; A male germ layer that has not yet differentiated. It is the precursor to stem cells.

[91] Etoposide; A semi-synthetic product which can cause cell death, breaks in DNA, as well as a stop to DNA transcription.

[92] Crossing over; The process of exchanging genetic material between homologous chromosomes which happens in the prophase stage of meiosis.

And then the other thing that I found really satisfying in the later years when I was busy as an administrator was looking at spontaneous mutations. Because we had run such a large operation, we had accumulated data on spontaneous mutations, which normally no one -- if you put together everybody's work all over the world, you would still have very few. So I had this -- I don't know if you got to read it, this thing in *PNAS* on spontaneous mutations? [Russell LB, Russell WL. Spontaneous mutations recovered as mosaics in the mouse specific-locus test. *PNAS* 1996 Nov 12; 93(23):13072-7.]. I think it was in '96. That had -- let me pull it out.

[pause]

Q: To kind of bridge from our brief pause to your *PNAS* articles in which you were just showing me that most of the spontaneous mutations in that paper were from your lab, but a few others from two other labs. Would this kind of work been possible in today's research environment?

A: I don't think -- certainly, you would have had to have raised and looked at huge numbers of mice to make that possible. And I think I pointed out that even from the other two labs, which are pretty large, each one, each got around ten, and by the time you put them all together, there was about sixty. Of course, we were looking at specific loci, but unless you look at specific loci, you really cannot get a mutation, spontaneous mutation information.

There were two kinds of things that were happening. Some of the specific locus spontaneous mutations were mosaics, visible mosaics. But then also some whole body occurred in clusters, and that was only possible because from any one male we would raise a huge progeny. I mentioned the seven shell thing yesterday. Because of the seven shell thing, we got a maximum production from each male. Hundreds or more was not unusual from any given male, and it's only when you have a large progeny from an individual animal that you're able to detect clusters. Instead of one mutation, you might get five or six.

So these two circumstances made it possible to detect mosaicisms that occurred in the previous generation. Either it occurred in what I call the score generation, where they were visibly mosaic, or if the same mutation occurred one generation back, then the gonad of the male that was having these progenies was mosaic. And you were able to detect that by getting a cluster of mutants.

So these two circumstances, once we got others analyzed, it made it, first of all, appear that they were mostly fifty-fifty, the distribution. It's like this, you know, with a mean just about fifty. And also, that indicated that they occurred between the last pre-meiotic mitosis and the first post-meiotic mitosis, which would be in the pro-nucleus[93] stage. We called this the perigametic interval[94] and most spontaneous mutations was a higher mutation rate in the perigametic interval than the spontaneous mutations that were picked up as singletons, were whole body and were just one to a sibship.[95] So that had all sorts of implications, why the perigametic interval?

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[93] Pronucleus; A sperm or egg nucleus with only half the normal number of chromosomes. Pronuclei appear after the sperm enters the ovum but before the respective nuclei fuse.

[94] Perigametic Interval; The interval between the last pre-meiotic mitosis and the first post-meiotic mitosis. It contains a very high mutation rate. For an article by William and Liane Russell on the perigametic interval see: http://www.pnas.org/content/93/23/13072.full.pdf (*Spontaneous mutations recovered as mosaics in the mouse specific-locus test*).

[95] Sibship; A group of persons all descended from a common ancestor, often siblings.

Then in this other paper [Russell WL, Bangham JW, Russell LB. Differential response of mouse male germ-cell stages to radiation-induced specific-locus and dominant mutations. *Genetics*. 1998 Apr;148(4):1567-78.] we showed that they had a different distribution among the loci. These were the ones that occurred in the perigametic interval, and these were the ones that were singletons.

Things like that that you do with just existing data, if you just look at it the right way. I think even not having laboratory funding there's lots of things that you can do. And that's very satisfying to me.

[pause]

Q: How has the pace of science changed? I have interviewed some biomedical researchers who are in their late thirties, early forties, just getting started in their career, and they would say that they were so busy generating data, with all the new techniques available, PCR, microarrays, that just sitting around and thinking about the larger implications of this data, they just didn't have time for. How do you think that has changed in the course of your career, the pace at which data is generated?

A: I think I agree with what these other people were saying. I was thinking that I would *hate* right now to be entering the career. I mean the process of getting a Ph.D. and doing a postdoc is so much more difficult. I couldn't do it, I probably couldn't do it.

Q: And why is that?

A: Well, you'd have to know all these millions of techniques and do them. And as you say, if you do that, you probably don't have much time to think about the broader picture. You have to be a real whiz at maybe ten different, at least, complicated techniques.

The other thing is that you are expected by the time you get your Ph.D. to have maybe three or four publications. In my generation, your first publication usually was your thesis. It didn't happen to be in my case because I had a couple before that because of the work on the pigment genetics at Jackson. But most people who got their Ph.D., their thesis was the first thing they published. Then it was some time before they had enough data wherever they went to work before they -- now you're expected to have all these publications. For your thesis, you have to have all these techniques. You have to be so competitive before you get anywhere. I think it would be too hard for me to do. I probably would chicken out.

Q: And how do you think that affects the quality of the science that's being produced?

A: I think you really weed out a lot of people, but I don't know whether you're doing the right weeding out. You certainly are weeding out the non-technically adept, who might be very good thinkers.

Q: Okay. I want to get back to this issue of techniques, because you develop this mouse spot test, and it becomes pretty much a standard technique to easily screen. Tell me a little bit about that. What was driving your science at that time? Was it purely a technical issue, in order to deal with all these generations of mice that you had to analyze? Or how much was being driven by just kind of the practicalities that there were limited techniques available and you were really after a larger theoretical problem?

A: Well, I think there was always a pressure for us to do something that was practical. Really, the people who funded us wanted -- needed something by which you could assess the mutagenicity of something. This is particularly after the chemical mutagenesis started. The first decade or so of our work was unencumbered by the problem of chemical mutagenesis. It didn't really start going until the sixties. And then people said, “God, there's tens of thousands of chemicals out there that really need to be looked at for their mutagenic potential.” So all these different committees came up looking at short-term tests.

There was something called a Gene Talks Program. It was funded by the EPA[96] [Environmental Protection Agency]. They assembled committees of about maybe a dozen people for each test, for each short-term test, to gather all the literature and to assess the test. But that was not the primary thing that really pertains to your question. That sort of came farther along.

But there was the constant pressure or encouragement to have short-term tests, so the big -- the things that were used at that time were Salmonella.[97] We were very leery of whether this really predicted mutagenicity in mammals. Because it was chemicals rather than radiation, and you had to have the metabolism -- I mean, radiation you can really compare organisms a lot better, but you have to have the physiology of the animal that's getting hit with the mutagen.

Plus the fact that germ-cell stage is so important. We now know that it's more important even than we thought to start with. For instance, on the dose rate effect with radiation, we departed from Drosophila because -- but it turned out after -- this was not really something basic probably, but almost all Drosophila mutagenesis data came from mature oocytes[98] or in some cases sperm. But the dose rate effect happened only in mitotic spermatagonia. The fact that the short-term tests to screen for chemical mutagenicity we thought were not applicable really. So we ourselves had the pressure on ourselves to see if we can't have a short-term mammalian test. Of course, the specific locus test is far from short-term. I mean, it can be done quickly, but you need a lot of mice.

So having done the basic work on the somatic mutations with the same loci, that seemed like that's the way to go. And it is a short-term test. So that's when I really got to marketing it as a short-term test and doing quite a bit of it myself.

The thing that I wish had been also adopted, although it was not mutagenicity, but it was teratogenicity, would be the homeotic shift. I think that would be an ideal test, and it has not been widely used, maybe because teratogenicity is not so much of a human problem.

Q: Okay. Well, I was reading one article that was comparing the mouse spot test with transgenic animal assays, and basically they were -- this goes to the question of what have recombinant DNA techniques in light of this revolution in molecular biology really done to the field of mutagenesis? And the argument was that the mouse spot test was coming out -- it had to be large-scale, it was very intensive, where the transgenic approach would be less costly and less space intensive. So how have these revolutions changed the field?

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[96] Environmental Protection Agency; A US agency that administers the environmental policy passed by the government. See http://www.epa.gov/.

[97] Salmonella; A bacterium which causes many different afflictions in humans and animals.

[98] Oocytes; The female germ cell which is an integral part of reproduction. Also known as the egg cell.

A: I really have not had that with the Blue Mouse® thing. But I know that there were a lot of instances when I was still more up to date in it where it was not that predictive. It's bacterial genes, for one thing, it's not mammalian genes. Plus, it doesn't really do much for larger lesions.[99] It'll pick up point mutations, but it won't even pick up deletions, and the spot test does. So it picks up a broader spectrum. And a lot of these chemicals probably are clastogenic as well as mutagenic, or maybe instead of mutagenic. So you want to have the capability of picking up larger lesions, which the Blue Mouse® does not.

And the spot test is not -- it's not that expensive. I mean, you don't need a very large population of mice. Well, it's a little labor intensive because you have to have timed pregnancies, so from that point of view it's more -- the triclosan that I told you about, the soap, that was done with a spot test.

X. Teratogenesis; Collaboration with Harwell Lab; Privatized Funding; Patents

Q: I want to kind of pursue a tangent here which I thought about when you said that teratogenesis really isn't a hot thing anymore. Many critics of the Human Genome Project said the primacy of the gene has made all other kinds of genetic approaches -- and particularly privileges the gene over say environment influences and expression of phenotype. You can approach this from what chemicals or what other factors, radiation, influence mutation rates. How do you see this thing that had culminated, this ideology of the primacy of the gene that culminated in the human genome project affecting the field and the kinds of attention that's drawn to your work, the kinds of funding that was brought to your work?

A: Well, teratogenicity is largely avoided, I think, by just telling pregnant women not to do certain things. But, of course, if you don't know what normal chemicals do. It is a human problem. But you would be surprised at how many people confuse a genetic cause for the teratogens. I would have to explain it over and over that -- even to people -- I even had to instruct Irene [A.] Uchida[100] about that sometimes, just the idea that something is a birth defect does not mean it's genetic necessarily, does not mean it's a mutation-caused thing. But people don't often think about that, I mean, if you come at it empirically, if you're not testing something. Just because you pick it up as a birth defect, it's not a mutation necessarily.

Q: Right. Are you familiar with F. Clarke Fraser's[101] work?

A: Yes, but not recently. I have not kept up. He's in Canada still?

Q: Yes, and he's fairly retired. He also had very similar things to say about teratogens, because that's what he considers himself to be.

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[99] Genetic Lesions; Injuries or loss of function which occur in the DNA molecules which control a cell's proper operation, including the accuracy and appropriate rate of the cell's division. Genetic lesions in a cell can occur at any age.

[100] Irene Uchida; A Canadian scientist who is best known for her work on Down Syndrome. She has also studied the effects of radiation on mice.

[101] Frank Clarke Fraser, FRSC; A very important Canadian geneticist who is currently a Professor Emeritus at McGill University. An interview with Dr. Fraser is available in this collection.

A: What kind of things does -- he says that a birth defect could be --?

Q: Yes. He approaches it from a clinical genetics point of view, being trained as a pediatrician. I want to go back just a little bit to something you said at the very beginning when we started taping, and that was there's been some work done on [Medical Research Council Radiobiology Unit at] Harwell, the specific locus test work that they were trying to set up at the same time that you were trying, and you were collaborators. They also had the same similar mandate from the British government to explore the effects of radiation on genes. How well did that collaboration [work]?

A: Well, it was very interesting. The group that started out at Harwell had been at [University of] Edinburgh [Institute of Animal Genetics], and they had started some radiation mutagenesis while they were still at Edinburgh, mostly clastogenic stuff. But then they got T.[Toby] C. Carter, who headed up the mutagenesis. He was a very competitive guy. At one point -- well, when they did set up in Harwell, we sent them our specific locus stock so we would all be working on the same inbred strains for the radiation.

I can't remember exactly, but that's also in those files of storage. I think they were still at Edinburgh when he accused Bill of stealing the idea for some of their experiments. Of course, you didn't have to have any great ideas, you had to have a past and that was already there. But then your ideas were whether you should do low dose, or whatever. You didn't have to be a genius to have any idea of what to do.

Anyhow, somehow he accused Bill, and that got into all sorts of bureaucratic levels, practically a war between -- the, whatever it is, the MST that funds – that communicated with the Atomic Energy Commission and they had all these things at bureaucratic levels. Bill really had almost a nervous breakdown over this. He was very thin-skinned about stuff like that. I remember sometime during that time that I decided the only way to get him out of his near-depression was to -- I went back and researched all the correspondence and all the meeting there had been and got it all documented, and I finally was able to document the fact that Bill didn't steal any thoughts.

But that was a real to-do. That was just Toby Carter, and when he left, everything was fine. We all got along fine. He went into some kind of a strange industry, I can't remember what it was. It was some breeding-related industry. But that was really a one-person thing. Everybody else at Harwell was fine.

Q: What happened to the projects at Harwell?

A: Well, the person who did some of the specific locus mouse work was Tony [Anthony G.] Searle, who is a wonderful guy. We've been the best of friends. We're still friends now. The last time I was in England I went to see him. We always had these very amicable relations with Tony Searle. Now, Mary Lyon did some specific locus work, too, and she was much more competitive, but not at the level of T.C. Carter.

Something else at Harwell that I was going to tell you about, but I can't remember. Oh, this is not Harwell but Neuherberg. Udo [H.] Ehling, who set up the big mutagenesis work in Neuherberg, had been here in the sixties, at the beginning of the chemical mutagenesis work. In fact, he was the one who did most of the chemical mutagenesis work here. The idea was that he was probably going to join our staff, but then he had this great offer. They offered him the earth. They're really generous in Germany, they have tons of money. So he took that job there.

Again, he took our stocks so we would have a -- unfortunately, the treated parent is a hybrid between two inbred strains, which is a C3H [Coumarate 3 Hydroxylase

strain], which is a long established strain, and the 101 [strain]. Well, something happened in Neuherberg. They made the wrong matings, and the 101 got contaminated from the C3H. They discovered that sometime later. So they ended up calling that stock the 102 [strain], but it's not anything -- it's got the C3H genes in there, and it's been inbred since then, but God knows whether it's more like the 101. And they're probably not even doing specific locus work anymore.

Q: Okay. A couple other areas I wanted to talk about briefly, and that was, it's not unusual now to see at the bottom of the page who provided the funding for the work, but at the time that you were initially coming out in the late forties and early fifties, and you have Union Carbide on all of your papers and the Atomic Energy Commission, what impact did you think that would have on the reception of your work, that you were being sponsored by one, basically an extension of the military. It's seen differently now, but at that point it was certainly seen as an extension of the military. And that you were being funded or sponsored by a private or corporation.

A: I personally didn't worry too much about it, but I think it did give that impression to some people. At meetings, they would sometimes look down upon -- not so much the fact that it was the military, but the fact that here we had all this funding from government, and here they were struggling at some academic institution. So you sometimes got this thing that said, well, no wonder you guys can do good work, you're just loaded with money.

I think it was more that kind of thing. I don't think most people thought that our work was influenced by the fact that the funding came from these sources, but there was this sort of -- you know, it's a lower level kind of work because they don't have to worry about being good because they're going to have all that money from the government.

Q: And because of the specific methodologies in the specific locus tests and the mouse spot tests, have either of you considered taking out patents or getting some kind of proprietary --

A: We never thought of that, and I think if we had, the patent would have gone to the AEC. But you don't do that in science, you don't take out a patent.

Q: Well, they do now, right?

A: They do now, right.

Q: That's what I wanted to ask. It's not unusual now to have large corporate labs publishing in the same journals as academic institutions. And there's still a perception that the kind of work that's done in the private sector is different than the public sector, like an academic institution. Have you experienced a difference in reception from at the time when you were unusual by having these private sector connections--

A: Oh, like when we had the triclosan?

Q: Yes. And now when it's quite common and not unusual to have private-public collaboration.

A: I think at the time it was more me. I was a little leery. I didn't want to have private sector funding. I thought it would taint us. I think it was -- I don't know how the triclosan thing came to me, probably through one of the committees I was on. But I didn't want to do anymore. I didn't really get very much pressure from them, only in the sense that they wanted it finished in a hurry. They didn't try to influence the outcomes or anything. They would call me every week, how are you coming, how's it going, that kind of thing. I didn't even want that anymore. But they certainly didn't try to influence the outcomes. I just didn't want to have anymore private sector funding. But Waldi Generoso had to take quite a bit because it was his bread and butter.

Q: One of the hallmarks in the history of mammalian genetics is the creation of the OncoMouse®[102] by Harvard [University]. How did you react to the ability of researchers to take out a patent on a mouse?

A: I don't like it very much, but I'm probably very old-fashioned, because you said that's done a lot now.

Q: What is it specifically could you say that is not good about it?

A: Because I think that anything that's developed through a scientific experiment belongs to science, without anybody becoming rich on it.

Q: Well, you've witnessed many changes over the course of your career. We were just talking about the revolutions in techniques and the kind of science that's produced and the pace at which it's produced. What impact do you think the privatization, so to speak, or -- privatization of scientific knowledge, I'll use that term, has had on the field?

A: I honestly don't know. I don't know whether it has any influence. I'm looking at it mostly from an ethical or moral point of view rather than from a practical. Whether it would have a practical impact in increasing the rate at which science progresses or not -- I mean, it does in the sense that it has some positive effect in the way it gets funding more easily. But whether it's subsequent to the patent, whether it slows things down, I don't know.

XI. Miscellaneous; Efforts in Environmental Conservation

Q: Okay. We'll begin to wrap things up. I'm going to be jumping back and forth across time. When the project here at Oak Ridge was set up, there was a bit of a debate because the ABCC [Atomic Bomb Casualty Commission][103] which became part of the AEC --

A: Oh, yes. Oh, wait a minute. I'm thinking of something else. What were you talking about there?

[102] OncoMouse; A laboratory mouse that has been genetically altered to be more susceptible to cancer.
[103] Atomic Bomb Casualty Commission (ABCC); An organization created by Harry Truman in 1948 to study the effects of radiation from the atomic bombings at Nagasaki and Hiroshima.

Q: [James V.] Neel's[104] and [W. Jack] Schull's[105] and [Newton E.] Morton's and [Duncan J.] McDonald's[106] work in Hiroshima and Nagasaki looking at radiation effects on the survivors of the atomic bombs.

A: Oh, yes.

Q: There was some argument as to whether those would be funded. I wanted to know -- because the other was this project here, which I think Jim [James F.] Crow[107] and Bill [Russell] and several others said this would be a better approach to it because it was more experimentally rigorous, and the results would be more reliable. How involved were you in these debates between whether the AEC should be funding different projects, particularly this one on a human populations, which was questionable what kind of results they would get.

A: Bill was much more involved than I. I was never so much into the hazard thing. He was on a lot of committees that were trying to work out the risk levels and allowable doses, and so on. So he was very much more into the ABCC. I think the thing that always impressed us was that so little came out of it, that we had to go around explaining that really radiation did cause mutations, because they didn't for a long time detect any effect, any genetic effect on the human population, and it seemed likely that they wouldn't. You can tell people that with the level of mutagenicity that we found in the mouse, that level would be very hard to detect in humans. So it wasn't that humans were insusceptible to it, and that was really a frequent part of Bill's talk. There really are mutations being induced in humans. It's just awfully hard to show.

Of course, Neel was pushing the -- oh, gosh -- the 2D gel [two dimensional gel electrophoresis][108] thing, so for some time there was some pressure on us to use 2D gels, but it's just very time consuming and very little positive outcome as to what kind of mutation would really move a spot from here to here.

I don't know what really turned out to be the most fruitful approach in the Japanese studies, because then Seymour Abrahamson went and headed that. As far as I know -- do you know what happened while Seymour was there?

Q: No, I don't.

A: But Bill had had a conflict also that I was totally out of, on what was called the ABCW [hypothesis][109] [Seymour] Abrahamson, [Michael A.] Bender, [Alan D.] Conger, and [Sheldon] Wolff, because of their interpretation of the dose rate effect was that it was a two-hit phenomenon, and that's why you were getting essentially a quadratic. Bill

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[104] James V. Neel (1915 – 2000); An American geneticist who researched the genetics of sickle cell anemia and radiation from atomic bombings. Dr. Neel established the first human genetics department in the US at the University of Michigan and was very influential in the field.
[105] William J. Schull; A geneticist who studying the effects of ionizing radiation. See http://www.schullinstitute.org/index.php?option=com_content&view=article&id=4&Itemid=8
[106] Duncan J. McDonald; The former chief of human genetics at the Atomic Bomb Casualty Commission.
[107] James F. Crow; A noted geneticist who is currently Professor Emeritus in Genetics at the University of Wisconsin-Madison. See James Crow's interview in this collection.
[108] 2-Dimensional Gel Electrophoresis; A technique that allows infrequent proteins to be identified.
[109] ABCW Hypothesis; This postulate maintains that the rate of mutation (from radiation) for each locus is correlated directly to haploid DNA content.

was saying, no, it was the repair effect in the mitotic cells, and these are not two-hit phenomenon. So they had really contentious debates.

Q: Put this in a context for me as how revolutionary was it to think that there were DNA repair mechanisms?

A: Well, at the time that he postulated it, there really hadn't been any. I mean, it's since become a huge field, but that was really the first time to say this looks like it's a premutational repair. And whether it's a two-hit thing or not, it's true that a fair percentage of mutations are deletions, but most of them are such small deletions that they do not require two-hit phenomenon. So I think from that point of view they were probably not right.

Q: Then just to return to this whole period, the beginning period, when you were doing your work, how conscious were you about the political implications of your work?

A: Only in the sense that I aroused the ire of all these radiologists when I showed the teratogenetic effects. But other than that, my own work really had not very many political implications.

Q: Okay. And when you began your genetic studies, because you looked at it from an embryological developmental biology point of view but also a genetic point of view, how much did developmental -- let me put it this way. What was the relationship between the field of genetics and developmental biology at the time you were doing your early work, and how has that changed?

A: I think there was quite a lot of interrelation. There was all the work of L.C. Dunn[110] and Salome [Gluecksohn-] Waelsh.[111] There were a lot of mammalian geneticists whose work had strong developmental components, and of course in Drosophila, too. As far as I know, that has stayed very strong. And even more so now with the ability to analyze specific genes, effects of specific genes at various developmental stages, the whole Hox [gene][112] clusters, for example. It's become, in a way, even more developmentally oriented.

Q: Okay. Well, a couple more questions and I can wrap it up. Given that you kind of grew up in a period, or you worked in an area of genetics that was highly politicized at the end of World War II, and even the environment in which you worked was somewhat politicized, what do you think now is the nature of social and/or ethical issues confronting the field of mammalian genetics?

A: What is the major political issue of what?

Q: Ethical or social issue affecting mammalian genetics, the field of mammalian genetics.

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[110] Leslie Clarence Dunn (1893-1974); A geneticist and former Professor of Zoology at Columbia.
[111] Salome Gluecksohn-Waelsch; A German geneticist noted for her work on developmental genetics.
[112] Hox Genes; A group of developmental genes that are responsible for the spatial arrangement of the body.

A: Oh. [pause] Well, I suppose you would be talking about things like stem cell cloning. Probably that's -- I wouldn't call it social, I would call it benighted. I think that is a big political influence on what kind of research is done.

Q: And how have you personally experienced that?

A: Only in the sense of Rick Woychik's work, because that was really the first -- not the first but the early parts of us doing stem cell work. And people here are doing it. I don't know how much government funding they're getting. I think it's all on NIH grants. I personally have not, only through my colleagues. But I think that's where you get most political influence on the work.

Q: It seems to me that environment has been an important factor in where you have done your research. You started at the Jackson Labs and now you're at Oak Ridge. They're both very isolated, very much integrated into a nice environment. They try not to be such a blight within the natural landscape. But they have been relatively isolated from say major centers, like Boston or New York or Chicago, where many universities are doing much research. How important is environment for you in conducting science?

A: Well, of course, I don't know -- this is something I haven't touched on at all, but since the mid-sixties I have had a major outside work, and this is when we started this group that I gave you the newsletter of. That began in the mid-1960s. It probably took as much time away from research as my kids did. By that time, they were pretty grown. So that took the place of having young kids and really taking off a great deal of time. Because we were mostly fighting emergencies and crises. We were fighting dam proposals and strip mining and things like that. So it isn't something you can say, oh, I'm going to do that next year because it was a crisis right now, and you really had to be very actively involved. So that took a great deal of my time, but I think it's an environment that's very worth saving. It is really, really unusual.

Q: And what is the connection between your passion for science and your passion for the environment?

A: My passion?

Q: Yeah. For science and for the environment.

A: Well, I don't know. I think, in a sense, the environment is a stronger passion because some of these things probably couldn't survive without me, and I think science could probably get along without me. Certainly now. I certainly cannot contribute very much to science at this point.

Q: Although you did retire, I think, formally in 2002, and yet you're still publishing papers as early as last month.

A: Yeah, I'm still publishing. After getting that contributed paper rejected, I'm wondering whether I should anymore.

Q: Okay. Well, my last question for you is, looking back over your career, what has been the most personally interesting work you've done, and why was it so interesting?

A: There're so many. I think I would start probably with the X chromosome and the sex determination. That was very interesting. Then I think the things that got out of the complementation work, the genetics small structure mapping. My interest in mosaics and the various ways in which I have approached that. I would say those are probably the three most -- to me, the most satisfying phases of my work.

Q: Okay. Well, I've come to the end of my questions. I'll turn it over to you and what would you like to talk about that we haven't?

A: I was just going to ask you. Did I ever send you a complete list of publications?

Q: No. You sent me an abbreviated form, but I went on PubMed and --

A: I will Xerox that for you. Are there any that you would like the reprints of? Because if you do, they're in storage, but I can get them.

Q: Okay. Actually, we do need to talk about that a little bit before I leave, but we can do that after we finish taping. Anything else you'd like to talk about?

A: No, it's just that I think that your project is fascinating to me, because so many of the people that you have already interviewed are people that I know, know of at least. It'll be great to see the whole thing.

Q: Well, can I insert one last question that I meant to ask and I didn't, and that was, could you compare and contrast C.C. Little's style of administration with Hollaender's? I know that you had very little -- you were basically a graduate student research assistant at Jackson Lab, but could you compare and contrast their styles?

A: You know, I personally didn't experience so much of C.C. Little, because by the time I became a student, he was fairly -- I was a student, I didn't have that much to do with him. But I had the feeling that he was much more congenial as far as the people who worked there. He as more trying to be one of the guys, and Hollaender was trying to be more like -- he was German. He was trying to be more like a Prussian -- not even a general, but a sergeant -- he would do things like trying to find out who was pregnant so he could be sure that that person wasn't going to be working too long and that there would be a replacement, and all this kind of stuff.

There was a famous pink room revolution when he -- there was a ladies room in the Y-12 facility that had pink walls, and he was keeping track of how long people stayed in there, the technicians, because that was time spent away from work. He had the idea that they were in there reading or gossiping or something, instead of working. So he kept track of how long people spent in the pink room. And people got very upset. That was the pink room revolution.

He was very much into people's personal lives from the point of view of whether they really did the work, and very upset with people who didn't publish much, but instead of trying to find out why, he would sort of give them hell. He was very, very different that way from C.C. Little. But he was a genius at building up the division, making it very internationally known, and really encouraging good people, and getting money. It was just his personal style that was very different.

Q: So Oak Ridge was essentially a town in and of itself. The laboratory was a city in and of itself, it sounds like.

A: Yeah.

Q: In which he tried to organize both the private lives and the professional lives of everybody who worked there?

A: Oh, yeah. They were all like his children, and he was really going to be a strict father to make sure they did well in the world. Very interesting guy. That magazine has a picture of him. I don't know if you saw it.

Q: Yes, I did. Okay. Well, I think that should wrap it up.

A: Okay.

Q: I really appreciate you taking the time to do this interview, and it's been certainly my pleasure to talk to you.

A: Well it as fun. Thank you.

End interview