JAMES CROW INTERVIEW
Session 1, June 1, 2005

I. Childhood, Religion, and Family; Undergraduate Education at Friends University

AM: OK. It is June 1st, 2005, and I'm with James Crow at his office at the University of Wisconsin, Madison. I'm Andrea Maestrejuan and we're here to conduct his interview for the UCLA Human Genetics Oral History Project. We'll start off at the very beginning and I'll ask you when and where you were born.

JC: I was born in a Philadelphia suburb, Phoenixville, Pennsylvania.

AM: I know from other works that I've read about you that your father [Hollie Ernest Crow] was also a scientist, a biologist.

JC: Yes, he was.

AM: Why don't you tell me a little bit about your father's background? Was he also from the Pennsylvania area, and did he come from an educated family?

JC: No, my father did not come from an educated family. He was born in Missouri near a town called Bourbon, which is a great name for a town in Missouri, and was on a farm. He was born in a log cabin, actually. The family had built a frame house by that time, but he was born before they got it finished, or at least he was born in the cabin. He spent his early years in the Missouri Ozarks. Then about the time of the Cherokee Strip,[1] around the turn of the century, the family moved to Oklahoma. He was actually a part of the Cherokee Strip. Then they staked out a farm, and the rest of his childhood and middle adolescence was spent in Oklahoma. Then he went to an academy there, mainly because of its location. It was a Quaker academy, called Stella [Friends] Academy, in the northern part of Oklahoma, close to the border of Kansas, and went to that academy. There's when he met his wife [Lena Whitaker Crow], and I'll tell you her story in just a minute. They met, though, in this academy.

Well, let me start off with her. She was born in Texas and was orphaned at the age of about twelve, in good Texas fashion; her father was murdered. Her mother died shortly afterward, perhaps in grief, I don't know. Anyhow, at age twelve. It was a large family. I've forgotten the exact number, but eight or ten children, and they were put out in groups of three or four to different families. She was with a group of four. Moved to Oklahoma, not too far from where my father was living, and grew up there. Then she went to this same academy in northern Oklahoma. So the two of them met there. This was a Quaker academy, so they both became Quakers at that time. Then the natural place to go to college was in Wichita [Kansas] at Friends University, which was an institution just getting started at that time.[2] So they both migrated to Friends and went to school there.

My father went through college. He worked his way all the way, carried newspapers and painted. He actually built a house, or helped to build it. It's significant because later we lived in that house, many years later. He went through college – well, in an orderly way, but he must have worked awfully hard. Those were tough days. But he did win a scholarship to go to Haverford College in Pennsylvania, near Philadelphia.[3] Spent a year at Haverford. They were married by that time, and it was nip and tuck financially, but they got by. He did odd jobs, and so did my mother.

Then he was, for a while, a graduate student at the University of Pennsylvania. I'm not sure about the order of things here. Actually, at some stage in this he was back in Kansas again and went to the University of Kansas and got a masters degree there. He worked with [Clarence

E.] McClung.[4] That's the famous grasshopper geneticist. He liked McClung, and as near as I can tell, he was a favorite of McClung, too. Then when McClung moved to [University of] Pennsylvania, somehow or other he moved at the same time. I'm quite vague about this. It's all written down, but I don't remember all – I wasn't there, of course. He did the standard kind of grasshopper cytology that McClung did, and he was an associate with [David H.] Wenrich[5] and with [Eleanor E.] Carothers.[6] Both of them went on to become quite well known.

Then he got a teaching job at Ursinus College in Collegeville [Pennsylvania], which again is next door to Phoenixville.[7] Actually, I think our family lived in Collegeville and when I gave my birthplace as Phoenixville, that literally is where I was born, but I wasn't born in the city where my parents were. He taught there for two or three years and did some more graduate work at Pennsylvania and I think might have been headed toward – he always liked to teach. He may have been headed toward a university-type job where he was both teaching and research. But it didn't work out that way. He had the chance to move to Wichita, which is where his parents were living, and he felt he should move back there and take care of his mother and father, so he did. So they moved back, and they moved into this very house that he'd built some years before. By this time I had been born. I was born in 1916 while he was teaching at Ursinus College. Then he moved to this college in Wichita and taught there for the rest of his life.

AM: This academy that he went to in Oklahoma, was that like a prep school? Was it a high school?

JC: It was at high school level, and it was quite small. It must have been just two dozen people or so there. Actually, I think they learned quite a bit. They talked about the courses they took. I don't know how rigorous they were, but they did study Latin, for example, and biology and languages and literature.

AM: Did he have any siblings?

JC: Yes, he did. He was the youngest of perhaps twelve, a large number anyhow. They were about equally split between boys and girls. The boys all died, and I never saw any of his older brothers. Several of them died of tuberculosis,[8] and my father was worried about having tuberculosis for his whole life, but he never did. I don't think he even had the germs.

Among the girls – they're hardier, as you well know. (chuckles) All of them survived, and one of them, my Aunt Clara [Clara Crow Gorsage] had tuberculosis all her life, but she lived well into her nineties. I remember her hacking cough and [she was] very thin, but she was a tough woman.

AM: Did they all stay in the Oklahoma area?

JC: They scattered, but several of them stayed in Oklahoma. Two or three of them moved to Kansas. One moved to Florida.

AM: And your mother, she went to Friends University as well.

JC: Mm-hmm.

AM: And what did she study?

JC: She didn't graduate, so I don't think she studied any particular thing. She had two years at the time they got married and then went off to Philadelphia with my father. I believe she never did graduate. I'm not quite sure about this.

AM: Did she work at home then after moving to Pennsylvania?

JC: In Pennsylvania, she had two or three part-time jobs. One was at *Ladies Home Journal* or *McCall's,* a ladies magazine anyhow, in Philadelphia. It was a great thing for them. They'd both grown up in Oklahoma and Kansas, and then to get to Philadelphia was the cultural event of their lifetimes. They heard a symphony orchestra for the first time. There's no doubt, because my mother talked about it a great deal later on, what a big experience it was moving to the East, Philadelphia in particular.

AM: And were they musically trained?

JC: No. They were musical, but neither of them had any training. My mother had a nice voice, and my father, if he had decided to go into music, would have done all right. He had the talent for it. He played the saxophone. Nobody told him how, but he learned how.

AM: Do you have any siblings?

JC: I have a brother [Ernest W. Crow]. He's four years younger than I am, and he went into medicine. When he went into medicine and I went into genetics, we had very little in common, but of course nowadays, medicine is full of genetics, so we have a great time together now. He still lives in Wichita.

AM: Did he go to –

JC: He went to the University of Kansas.

AM: It's kind of asking the obvious, but to get at your interest in science, what were your parents' expectations for their children in terms of educational achievement and professional achievement?

JC: Well, since my father was the only educated member of his family, the only one who finished college anyhow, and I think that's almost true of my mother's – some of them went further and had some years in college. I wouldn't say they had specific expectations for me. I was always a good student, and they expected that I'd be going to college and maybe graduate school, but I was never pushed, nor especially encouraged. I was self-motivated, largely.

AM: There seems to be a tradition of the Friends Church, the Quaker Church, in your family. What kind of religious traditions did you grow up with?

JC: The Quaker Church in Wichita was an interesting mixture. I grew up in it and was a very regular attender. My father and mother both were very serious about their religion. It was a mixture, an unholy mixture I would say, of traditional Quakerism, George Fox type Quakerism,[9] and Midwestern or Southern conservatism. It was an uneasy battle. Most uneasy truce, I should say, the whole time. The church did some things in the Quaker way and some things in the standard Evangelical tradition. I didn't really take to it. I remember not believing the story of Jonah and the whale, for example.

Later on – I'm getting ahead of the story, but later on, after I'd graduated from graduate school and was already teaching at Dartmouth College, I decided I should resign from the church, which I did. It was very hard on my parents. I'm sorry now that I did it. There was no point -- I didn't need to do it, but I did, I resigned. And it bothered them a great deal to have their – they remained deeply religious even after I had given it up.

AM:	And why did you resign from the church?

JC:	I just decided I didn't believe in God. It just wasn't a sudden decision on my part. I read about people who gave up their religion and had a terrible time finding something to substitute for it, and that was never a problem to me. Undoubtedly, this was a gradual thing. I'm sure even by the time I was in high school I was having doubts, but I didn't want to break with my parents. I was in the daily routine of going to church on Sundays. So I didn't make any kind of a fuss, but I might – just because I didn't want to make a fuss, I think. I was beginning to have doubts. Then after getting in to graduate school, I ran into other people who were not religious, and it didn't take me very long to just sort of decide this was not for me.

AM:	Kansas has come up again in the news, especially in terms of scientific education.

JC:	It certainly has.

AM:	Some of your research interest is in the field of evolution and selection, and your father was a scientist in a religious institution, or university. What kinds of discussions did you have, even in the early period when these debates were just as alive and active between the role of religion and science, particularly evolution and the kind of science – the Darwinian revolution?

JC:	Well, I can't remember personally ever having any doubts about evolution. I don't remember being taught it particularly, but natural selection just – I think I would have invented it myself if I hadn't known it already existed. And my father certainly believed in evolution, but he saw no incompatibility between being religious and being an evolutionist. He certainly didn't take the first chapters of Genesis literally, but he did believe in the supernatural, as did my mother. But they didn't see any conflict between that and evolution.

AM:	Would you say that's true for the Friends, Quaker religion in general, that they don't take say such an active stance as some others?

JC:	I think so. I'm not that close to the Quaker church now. But if I were going to join a religion, I think it would be the Quakers. I do admire the social conscience of the Quakers, and I believe in their kind of pacifism, for the most part. I do have great admiration for it in every regard except the supernatural. And I've even been tempted to join the congregation here. I never have. I could fit into a Unitarian [Universalist] society perfectly well.[10] There's a group of people just like me. These are confused liberals, slightly left of center, and do-gooders. I'm clearly in that category.

AM:	OK, great. And you have three children.

JC:	Mm-hmm.

AM:	Did you bring them up with any religious tradition?

JC:	No. I didn't try to influence them. They knew that I was a non-believer. My wife didn't say much one way or the other. She was largely influenced by my views, I think. Among my children, one of them has become quite religious, so whatever I might have said had an opposite effect on her. The other two, I don't really know much – we didn't have much talk about religion in the home. They did know that I was a non-believer from the beginning, though. That was apparent. I was reading Bertrand Russell,[11] and my son looked over my shoulder and read a little bit of it one time. I remember that.

AM: Well, again this might be asking the obvious, but how important was your father's position as a teacher of biology? How important a role did that play in your scientific interests?

JC: Well, it was an important role. Let me start out a little earlier in my life, and then we'll get to him. I was very interested in music as a child. I have a story that I think is of some interest as to how I got violin lessons. When I was not yet in school but must have been pretty close to it, there was a house fire in our neighborhood at night, and it made a big impression on me. It had big flames shooting up, and just a short distance away.

Somehow or other, I associated that fire with a piece of music that was on our old Victrola.[12] We had an old wind-up Victrola with records on it. And it included Eddy Brown, who was a noted violinist at the time, playing [Edward] McDowell's "To a Wild Rose." Pretty corny music, actually. Whenever the family played that record, I would think of this fire somehow. Maybe the record was going on when the fire broke out. Anyhow, I associated it. [When the record was played,] I'd go over in the corner of the room and cry. You know how parents are. They said that child has deep musical feelings, and so they gave me violin lessons.

They started out by – I had wanted to play violin. Maybe these sort of came together. They started me taking piano lessons, and I started on my sixth birthday, I remember. I had my first piano lesson when I was six years old. I did that for about two years and then took up violin. I was quite serious about music, not serious enough to practice religiously, but serious enough to talk about it. Through high school, I even thought maybe I might like to be a musician. Then probably in high school, I vacillated. But anyhow, I discovered that I really wasn't talented enough, nor had I worked hard enough in my early years to ever make it as a professional musician. So I decided I'd better do something else. But I kept it up as a hobby, and I still play.

I've always liked sciences. In high school, I enjoyed physics and chemistry. I had perhaps the world's worst physics teacher in high school, but despite that, I still learned something. The teacher I don't think knew very much. I remember vividly one time his going to demonstrate that sound doesn't carry through a vacuum. He had a bell jar with a bell inside it and was going to pump the air out of there and we shouldn't hear the bell ringing. Well, half the class – certainly including me – realized he'd hooked it up backwards and it was pumping air into it. I remember waiting for the explosion, which fortunately never happened. Anyhow, that was my high school physics teacher. Nonetheless, I liked the subject, and I learned as much as students in that class were expected to learn.

At the end of high school, I did get a scholarship to the University of Kansas, but it didn't pay for everything. We sort of figured out that I'd save money by just staying home in Wichita and going to school at Friends [University]. I think – well, I can't say it was a mistake. Everything's worked fine for me. But I would have learned more if I had taken that scholarship and gone to the University of Kansas.

All during college I worked. Well, during high school, I worked in a drugstore, worked most nights there in the soda fountain. Then in my senior year, I got a better job. I worked there in the public library. Actually, it was a good job to have, for me or anyone else, because I worked in the reference department, and much of my time was spent looking up what people wanted to know. And of course, when I looked it up, I learned it myself. So I think I learned a comparable amount to what I was learning in school by this. It was five hours every afternoon and all day Saturday, so I didn't have much time for anything else. I went to classes in the morning, worked in the afternoon, and in the evening I often had something, oftentimes a musical activity. So really there was very little study. I think I learned the fine art of doing the minimum amount of work to get an A, and usually succeeded, not always.

AM: So school came easy for you?

JC: It was easy for me, and I enjoyed it always. Of course, this was a school where my father had taught. I first started out to major in physics. We had a very charismatic physics teacher.

AM: In college?

JC: College now. A poor teacher in high school, but a good teacher in college.

AM: This poor teacher in high school didn't turn you off to physics.

JC: Didn't turn me off the subject, no. He didn't at all. In college I started right off taking physics in my first year. I don't know why I didn't go on with it. I certainly liked it and could very well have done so. But I didn't take it the second year. I took a chemistry course the second year, and took biology too. Then I finally ended up majoring in both chemistry and biology. And all the biology courses were taught by my father. You asked a while ago whether he had an influence. He certainly did. But most of what I learned, I think I learned by reading rather than by my father's lectures.

AM: Well, at home, even in high school, did your father have like the latest textbooks or journals lying around? How well was your science knowledge compared to your classmates?

JC: It was a little better. My father subscribed to the *Journal of Heredity*,[13] I remember, and I liked to look at it, even younger than high school, I think. It had spectacular pictures. I've recently gone back and looked at some of those issues, and I remember some of them. He also subscribed to Science. I don't think I read much of that. I can't say, though, that I had a very deep interest in anything beyond my classes at that time.

My father and I had very few serious discussions. Of course, we talked all the time, but it was more likely about – it might have been current affairs – he was interested in politics – or about some purely family question. So I can't say that there was – I must have learned quite a bit from him by example, and I do admire him, of course. He was hard working and conscientious.

I'm sort of sorry that he didn't – I think he would have had a good research career if he had gone into that. He certainly was smart enough. And I got the impression that maybe he was McClung's favorite, too, from just some of the things in his autobiography. But he got started on this teaching path, and in a small college there was no chance for any research. Later on, he insisted that he really didn't want to do research. I don't know now and didn't know then whether this was sort of a defense mechanism rather than a real conviction. Anyhow, he had a happy life, and he enjoyed teaching.

AM: The community that you grew up in, were most of your classmates or after-school mates all affiliated with the university?

JC: My next door neighbor's father was the chemistry teacher in the same institution. [His son and I were close friends.] There was a period of about three years in which our family moved to a small farm near Wichita. My father had grown up on a farm, as I've said, and he thought that was a nice place for kids to grow up. I'm not so sure now, but anyhow, we did. It was a small farm. It must have been less than ten acres. Really only two acres that we worked on. But I did spend three years in this rural school. It was a two-room school. I started in the fifth grade, half way through the fifth grade, actually, and then went through the sixth and seventh. I'm glad I didn't stay any longer because in a rural school, everybody hears everything that's going on. I knew everything that the eighth graders were learning, so it was just as well that I didn't stay for the eighth grade.

This was in the suburbs of Wichita. And my father had never completed his Ph.D. but he decided to, and so we spent a year in Lawrence, Kansas, at the University of Kansas. That's when I was in the eighth grade. Then we moved back to Wichita, and the rest of my education was there, up through college, that is.

AM: Did you (get introduced) to the rigors of farm life during those three years?

JC: I wouldn't say it was very rigorous. It was a small truck farm. Once in a while I did some plowing with a horse, and I liked to do that. I even thought at the time maybe I really wanted to be a farmer. Then I had a period where I thought maybe I'd like to be a veterinarian. But I did like animals. I'm sort of glad to have lived on a farm, and I certainly picked up the farm vocabulary and that sort of thing. There were 4-H clubs that I was a member of. Most of the students in this school, their parents were part-time farmers. Most of them had some kind of a job in the city and farmed on the side. It was a little too close to town to be real farmland. Right now it's in the center part of Wichita. The city's grown up around it.

AM: What did you think you'd do with physics? You said you were interested in physics and were going to go off to study physics at the university.

JC: I just liked the subject. I don't know that I had any clear idea about it. I just thoroughly enjoyed physics. No small amount of the enjoyment was this charismatic teacher. He had all sorts of gadgetry that he'd worked up himself, mostly analogies. I remember he had a series of mousetraps and rattraps to explain how vacuum tubes worked. I vividly remember those mousetraps, but I haven't the slightest idea how a vacuum tube works. (chuckles) So I remember the analogy better than what it was supposed to be teaching.

There's an interesting consequence. Physics was a popular major at this school, and yet not everybody's talented in physics. There were several people who majored in physics and then even went off to graduate school and then either dropped out or went into something else. So I think the net effect of having this too spectacular teacher was to attract people into the field who probably should have gone elsewhere.

AM: Was the interest in physics then at this university because of this charismatic teacher, or was there some –

JC: No. It's this teacher. There was just one physics teacher. There was just one teacher of physics, one biology, one chemistry.

AM: It must have been really odd. I can't even imagine having my parent as an instructor.

JC: It was slightly embarrassing, in a way. But then I did it. Aside from the very first elementary class, they were all small classes with just three or four students. And to a large extent, I worked on my own. For example, comparative anatomy class where we dissected all sorts of things. There were four of us in the class. My father was there to answer questions, but most of the time I didn't see him at all. We just dissected away and learned on our own.

AM: During this period in most people's lives, there's disagreements with parents. Everybody's trying to move and be an independent. Did you have the same problem, maybe in intellectual content or in work habits?

JC:	No, I never did. I was never a rebellious child. I was well behaved. Although I'm sure I disagreed with my parents. But mostly I kept it to myself. These are mostly social views or religious views or something like that. But it was a smooth childhood. My own children were more rebellious, but I wasn't. I had bigger differences with my father considerably later on, after I'd started teaching at Dartmouth College. I think they were mostly political discussions. I can't even remember what the differences were. I do remember in college, though, I flirted with left wing politics. Just possibly, if there had been a Communist Party on the campus, I might even have joined it. I certainly was tempted to go in that direction.

AM:	At Dartmouth?

JC:	No, this was back at Friends in Wichita. There was a lot of socialism in that school. There was a lot of socialism in the United States. This was in the depths of the depression, and there was good reason to think the capitalist system wasn't working very well. Our hero was Norman Thomas, really, who was running for president relentlessly and repeatedly.[14] This was in 1936-37, the fall of '36. Anyhow, the election of '37, I guess it would be. Thirty-six was the election. I was all for [Franklin D.] Roosevelt.[15] I wanted to hear the Communist candidate for president. It was Earl [R.] Browder.[16] He was from Wichita, actually, and he spoke. I wanted to go. My father didn't really want me to go. Finally, we sort of compromised that we'd both go. Actually, I think he enjoyed it just as much as I did.

That's the only time I'd ever heard – at least at that age – an out-and-out Communist. I rather liked what he had to say. I remember he was – oh, just a few things from the lecture itself. He was speaking about the Great Depression, and he said, "Even capitalists are jumping out of the windows," and somebody in the room said, "Let 'em jump!" (laughs) It was good rabble rousing.

AM:	Did your family come from a tradition of political social liberalism?

JC:	Only what they picked up in Wichita. They certainly both came from very conservative families in their childhood. Maybe this Quaker academy in Oklahoma may have furnished some liberal ideas. I'm not sure about that.

AM:	Were both parents Quaker growing up?

JC:	No. They got it at this academy. I should say one other thing. I was very much into pacifism, as I said, and in socialism, too. Everybody was a socialist. Everybody I knew that had any ideas at all was a socialist then.

AM:	How times have changed.

JC:	Yeah. (chuckles) Well, I think it's totally understandable. The capitalist system was really falling apart then, in the Depression. One of the things I did at that time, I spent the summer of 1936 in what was called the Emergency Peace Campaign.[17] It was a group of people sponsored by – the Quakers had quite a bit to do with it, but the money didn't come from there. I spent the summer going around various places in western Kansas preaching pacifism. A group of four of us worked together. I'm very glad I did it. I don't think I made any difference in the world, but it was good for me. I got in the habit of public speaking, and I've never been afraid of an audience since that time. So I think it was a good learning experience. Of course, soon after that we were in the war and I changed my views when I discovered what was going on in the world. But in 1936 I was very much a pacifist.

During the next year, the next college year, there was a meeting between a group of people in Wichita and a group of people from Oklahoma that got together. We had the Kansas-Oklahoma Peace Agitation Action Committee. We used all the buzz words of the Left. Can't remember all of them now. I was president of this thing. I think we had only one meeting, it was in Oklahoma. I came back home and never attended another meeting, and it got taken over by a Communist front organization. I never did anything about it. And I worried all during the McCarthy period, to jump way ahead of the story, that my name would turn up on some list. It never did, but I just worried – either it wasn't that important or –

AM: I'm sure there's a file on you in the FBI.

JC: I've been tempted, under the Freedom of Information Act,[18] to – I may do it yet. I'd like to know if anything this trivial, actually, is in the files.

AM: You were always interested in science academically. Did you have any other aspirations toward activism?

JC: In science, you mean.

AM: Yes.

JC: I didn't realize then that you could do research as an undergraduate. That was just never a thought that entered anybody's mind. I was content to learn what my courses taught. I certainly was not the kind that you think of when you think of scientists who are doing one kind of a gadgetry or something in their childhood, or blowing up something in the kitchen sink. I didn't do anything like that. I was just sort of a model student. I went along and learned everything in the course and nothing else.

I was involved mostly in extracurricular activity, much of which was musical. I played in the string quartet, for example, that had a regular radio program during those years. There was a Wichita Symphony, too, that I played in, that rehearsed once every week. So two or three nights a week went to music. And after I got home I'd study for an hour maybe, not very much.

AM: Did your brother – did school come as easily to him?

JC: Not quite, because – my poor brother. (chuckles) Every class he went into, his teacher would ask him if he was as smart as I was, and he got to being rather – well, he found it more amusing. I think he did. At least, later on he found it so. Especially the Latin teacher. She really wanted him to take Latin because she remembered me as a good Latin student. He didn't want to take Latin, and didn't. He's told about that a few times.

AM: Did he go to Friends University as well?

JC: Yes, he did.

AM: Oh, my goodness.

JC: Then he went on to medical school at the University of Kansas. We were [not in graduate school] at the same time, though. We were four years apart in age, and we were five years apart in school because I skipped a grade.

AM: What year did you skip a grade?

JC: It came in two halves. I skipped from two and a half to three, and then when the fifth grade came along, we moved in the middle of the year, so I was put a semester ahead by the way it worked. So the net effect, I had a year ahead of him. So the end effect, it was a year.

AM: So your brother did have big shoes to fill.

JC: (chuckles) He talks about it once in a while. (laughs) He was actually a good student and got through medical school perfectly well. But he didn't impress his teachers as much as I had, is what it amounts to.

II. Graduate Fellowship at University of Texas at Austin; Beginning work with *Drosophila*

AM: OK. Well, why did you decide to switch to biology and chemistry from physics?

JC: I don't know. Not any particularly good reason. I suspect it was the circumstances of enrollment that particular year, and things like that. I know if I'd gone on with physics, I'd be a very happy physicist. I sort of liked the subject.

AM: What kind of physics?

JC: Well, then I didn't know enough, and the course never did get into the interesting parts of physics. There was no relativity, no quantum mechanics, or anything like that. All I know about that subject I've gotten just by reading later on.

AM: In the biology classes, were they teaching any genetics?

JC: There was a genetics class.

AM: What were they teaching?

JC: It was [Edmund C.] Sinnott and [Leslie C.] Dunn, [*Principles of Genetics]* was the standard textbook at that time.[19] The course, taught by my father of course consisted essentially of reading that book pretty much in order. And I loved it. I was perfectly happy to go further into genetics. I thought of medicine a few times during college and decided not to for two reasons. One is, I don't think I could have done it financially, which essentially made the decision. But by that time, I decided I'd rather go to graduate school anyhow, so when it came time to continue on – I don't think I could have – those were days when it was almost impossible to find a job, and in a sense, graduate school was the course of least resistance. If I got a fellowship, at least I was secure for the next few years.

AM: Was it competitive then? Were other students like you trying to get into graduate school?

JC: Yes. Two or three of my closest friends got into graduate school. Not the same one that I did. So actually, it wasn't too hard, at least for me – only the ones who were pretty good students tried this, of course. This is not very relevant to anything, but I remember it. This college required a course in religion in order to graduate, which I didn't really want to take. I was already slightly rebellious. Quietly, not publicly. But anyhow, I found the course I wanted to take, which was the history of the Christian church. It actually included the Jewish church, too. That history is essentially the history of Europe and I chose the course for that reason, and I'm glad I did. I learned quite a bit of history.

In that class – it was about ten people, I suppose – three of us were science majors and the rest were preachers, or future preachers. The three of us got far and away the best grades. I guess if you've learned the bones of the ankle and the Krebs Cycle,[20] it's no trouble remembering the names of the Popes. (chuckles) I've been amused by that ever since, that the three of us who were not interested in religion got by the best grades in this course in religion, in church history.

AM: When it came to deciding about going to graduate school, what area did you want to –

JC: I considered both biology and chemistry, and I wasn't quite sure which direction I wanted to go. I have an interesting history that Jim [James D.] Watson[21] and I share. We don't share very much, but we share one thing. We both were turned down by both Harvard [University] and Cal

Tech [California Institute of Technology] for entering graduate school. I applied to a dozen or so schools and, as I said, got turned down by those two. I was accepted to a few places. But the first acceptance was from University of Texas [at Austin], and about that same time I got an offer from [University of] Wisconsin [at Madison] – there were two kinds of fellowships at Wisconsin. There was a good kind and a not so good kind. One paid three hundred dollars and the other five hundred dollars. Well, I got the three hundred dollar scholarship. If I'd have gotten the five hundred dollar scholarship, I think I would have gone to Wisconsin, but I didn't. So I went to Texas instead. Now, if I had gone to Wisconsin, it would have been in biochemistry.

AM: And why was that?

JC: Just because that's where this particular fellowship would have been. And if I went to Texas, it would have been in biology, or genetics. By that time, I think – I wasn't unhappy that I made this choice, and I have never regretted since, either. Also, the Texas offer came first, and I was so insecure that I accepted it almost immediately. I expect if I had been offered at Wisconsin, I'd have accepted that first and would have been a biochemist.

AM: OK. Well, let's talk a little bit then about going to Texas. What were some of the other places you said you got accepted to?

JC: Iowa was one, the University of Iowa. Eleanor Carothers was at the University of Iowa. She knew my father of course very well, and that may have had something to do with them offering me a place there. And I would have studied grasshopper cytology if I had gone there, sort of followed my father's path. Those are the only ones I can remember now.

AM: Now, were you identifying places by their programs in genetics? Texas had just started their program.

JC: Texas was strong in genetics. I'd pretty well decided that if I didn't go into biochemistry – I had almost decided that really genetics is what I wanted to do. I was nominated by my college for a Rhodes Scholarship,[22] and I went to Lawrence, Kansas, to meet with a group of people. They asked me where I'd go if I really had my choice, and instead of saying Oxford [University], I said Cal Tech [in Pasadena, CA], which was probably the wrong thing to say. But that's how I felt about that time. I felt that's the place I really wanted to go. At that time, the [Thomas H.] Morgan group were all there.[23]

But Texas seemed good, too, and of course [Hermann J.] Muller was at Texas,[24] and he'd done his radiation experiments. This would be 1937, and I did know then that Muller had left and gone to Russia, but what I didn't know was that he had no intention of coming back. I thought that while I was there I could probably study with Muller. So I was delighted to get the Texas fellowship. Delighted to get anything, really. I was so insecure about finding employment.

AM: So you were keeping up with the literature in genetics to know that Muller was working on?

JC: I knew that much about it. I was not reading journals. Maybe once in a great while. Most of my knowledge was textbook knowledge at that time. And I had no clear idea at all about the nature of research or that I'd have any real interest in research. I thought I might end up doing pretty much what my father had, go to graduate school, get a degree, and teach.

AM: What kind of laboratory experimentalist opportunities did you have at your college, at the Friends University? Was there much hands-on?

JC: There was quite a bit of hands-on work, but it wasn't research, it was the kind of laboratory you have that goes with courses.

AM: Well, when you got to Texas you mentioned that you wanted to work with Muller. You knew that he was gone, so where were you – Describe the process of who you were going to choose to work with at that point.

JC: Right. Well, when I got there, I wasn't sure who I'd be working with. But the fellowship was not with any particular person. It was a teaching assistantship. But the person who more or less picked me up, I think it would be fair to say, was [John T.] Patterson, and he became my major professor.[25] He was an interesting character, what in those days you would call earthy. He had a semi-vulgar, funny actually, sense of humor. I remember when I came in, he was gruff. He was always puffing on his pipe and seemed gruff, although he was really kindly. He said, "You're blonder and skinnier than I thought you'd be." Those were his first words.

AM: And why did he have an image of you?

JC: I had sent a photograph, which you did in those days. He then became a major professor, and I started out just as a graduate student. I was mostly taking courses, and then eventually found my way into the fly lab, but not until a month or so after I'd been there. Mostly I was taking courses, and doing this teaching, which I'd never done before.

The three people who were central to my education were – there was Patterson, as I just mentioned. [Theophilus S.] Painter was the cytologist.[26] But the one that I interacted most with is a man named Wilson [S.] Stone.[27]

AM: Was [H.] Bentley Glass there?[28]

JC: He'd left by that time. He and Stone were contemporaries, but Stone stayed on as a faculty member. I didn't meet Bentley Glass until several years later.

AM: OK. Now, how was genetics organized at Texas? Did it have its own department at that point, or was it still kind of divided between zoology –

JC: It was part of zoology. There wasn't any genetics department itself. But there was a *Drosophila* laboratory,[29] which was all the genetics there was. It was a very popular place, incidentally, because it was a room that was cooled, and in Texas summers, that's the thing you look for, of course. I don't want to say I went into genetics because of temperature control, but that didn't slow me from going that way.

This was an interesting period in the history of genetics at Texas because they were making a transition from studying cytogenetic problems[30] into studying speciation[31] and natural populations. I got there just as the change was being made. Patterson and Stone were working on a problem, trying to infer what happened at meiosis[32] from breeding results. Now it seems like a trivial problem, but at that time, it was difficult for me and for them.

There was also a much more mathematically minded student in the lab. He had taken a crack at this and didn't get anywhere, but I thought I'd look at it. I solved the problem, to my satisfaction. I had one of these – first time I ever had, and it hasn't really happened since. I was stuck on this crazy thing, and I thought about it for two or three days off and on. Finally, I gave up and went to the beer tavern and had a beer and then had a sudden realization of what I had been doing wrong. The kind of thing you read about that happens once in a while. That's happened maybe a couple of other times, but this was the first time and the most striking in my life. So then I went back and solved this problem immediately.

At that time, I hadn't had much math, but Stone hadn't had any math either and was therefore more respectful of it probably than he would have been if he'd had more. He said, "You really ought to take math courses and you ought to read Sewall [G.] Wright,[33] if you have this kind of mathematical skills." It really wasn't much, but I acted on it anyhow and started – so I then took statistics and probability courses and math courses and started reading Wright, which I found very difficult to read, but I tried at it anyhow. That's all I did in this kind of thing.

Incidentally, I didn't know about maximum likelihood method in those days. That would be the way I would do it now, but I had invented my own little way, which was not nearly so good.

I was more or less assigned what to work on in that Patterson was starting studying different species, and there were three or four different graduate students and each of us got a *Drosophila* group to work on. Mine was the *mulleri* group,[34] so my thesis was on isolating mechanisms principally in that group.

One difference between the attitude that I had, at least, and I think the University of Texas in general – the Ph.D. here nowadays is really a research degree, and you primarily spend a lot of time on research and you're successful or not in accordance with that research. At Texas at that time, there was more emphasis on learning, and the thesis was something you did all right, but my thesis that I wrote then certainly wouldn't pass muster here now. It was done quickly and didn't have very much data. I spent a lot of time taking courses. In a way, I'm glad I did.

As I said, Stone was more influential with me than Patterson was, and he encouraged me – and Patterson didn't disagree – encouraged me to take courses that I wasn't really prepared for. He said, "If you get a low mark, don't worry about it." So I have some B's on my record that one shouldn't have as a graduate student. I took a course in radiation physics, for example, that I was totally unprepared for, because other students were physics majors. But I learned a hell of a lot in this course, and I've been glad I did it ever since. I took physical chemistry, too, and that I thoroughly enjoyed. I might have become a physical chemist if I'd had taken it earlier in life. (chuckles)

AM: Physical chemistry stopped me cold.

JC: Well, I took to it. I even had occasion to use this later. I'll again jump ahead. The book we used was by [Frederick H.] Getman and [Farrington] Daniels [*Outlines of Physical Chemistry*].[35] I remember the authors very well. Daniels was at the University of Wisconsin, and I later met him. It was a pleasure to meet the man whose textbook I'd used. But since I did like it – I worked all the problems in the book. Some year later, after coming to be on the [Wisconsin] faculty, Joshua Lederberg[36] had a student, Norton Zinder[37] – he's become very distinguished since that time. He was taking physical chemistry, which he didn't like. He was in your category. (chuckles) But Josh had more or less insisted on his taking it. Then I found out he was using the same book I had, so when he had any trouble with problems, I would help him with those problems. I don't know how much responsibility to take for his getting through physical chemistry and ultimately becoming a big success, but I'll take any amount of credit that I can get. (laughs)

AM: Where there any corn geneticists or any geneticists in the botany department at Texas at this time?

JC: There weren't at that time. There were some corn genetics going on, but it wasn't at Austin, it was at Texas A & M [University]. I don't believe there was any other genetics in Texas then. There was cytology, and there was a course in fungi, which I took, but I think this was all of the genetics. It's an interesting question which I never thought of before. But I certainly didn't get any genetics anywhere else.

AM: OK. How much thought did you give to model organisms, or was that even an issue?

JC: No thought at all. They were using *Drosophila* and that was it. I enjoyed actually working with *Drosophila,* so this wasn't any hardship. I've never been the kind of person who goes to the laboratory and just enjoys every minute of laboratory manipulation. I'm really more interested in getting an answer of some sort and thinking about it than I am of the day-to-day details. And that was true then, too. I probably did less work per thesis than any of my contemporaries did.

AM: How were you with your hands in the lab?

JC: I wasn't particularly bad, or good either. There wasn't anything that called for laboratory skills. Well, I did learn how to blow glass, for example. After I moved to my first job at Dartmouth, I was the only one in the department who knew how to do this, so I made equipment for other people, but I never thought of it as being a particular skill.

AM: You kept the fly populations alive.

JC: Mm-hmm. I remember my father told me once that, "Whatever else you go into, don't go into *Drosophila* genetics because you don't want to spend all your time counting millions of flies." Then of course, that's what I did. But I usually managed to find a way of working on a problem that didn't demand an awful lot of counting work.

Let me mention one more thing while I was going to graduate school, because I did keep up music during those graduate school years, which was hard to do because graduate school is a full-time job. But I sort of disciplined myself to keep up by playing in the university orchestra. Also, you could rent a practice room – not rent it, I didn't have to pay for it, but get assigned to a practice room. I did that from five to six every day, for several years. Not all four years, but much of the time. That just meant that I had to go to that practice room every afternoon or else I couldn't have kept it. So that kept me from going totally stale.

I did a little bit of freelance playing while I was there. During my last year before getting a degree, I changed from the teaching appointment to a research appointment, which paid a little bit less. There was an opportunity as a musician for playing in a little orchestra that made up the difference, so I did that. I enjoyed doing it. It was corny and terrible, but I made a little money that way. On the lighter side, I was a victim of one of the diabolically cleverest jokes that anybody's ever perpetrated. I was playing in the pit orchestra for kind of a light comedy show of some sort. I can't remember what it was now. I stopped to get a hamburger for supper and left my viola in the fly lab and came back and started playing, and fruit flies kept buzzing up out of the f-holes. And one of my dear colleagues had anesthetized thousands of flies and put them inside the viola. (laughs)

AM: I bet you didn't leave your viola in the lab anymore.

JC: I didn't do it again, no. (laughs)

AM: Well, great colleagues. Was the Austin blues scene going at this time?

JC: The skies, you mean? Or the music?

AM: The music.

JC: There probably was. I did play in a dance band for a little while, but I had no contact with the blues, any of that kind of music. It was mostly classical.

AM: OK. Well, you just mentioned that your dad said whatever you do in genetics, don't end up spending all your time counting flies.

JC: Right.

AM: And that's what you do. But maybe you can talk about at Texas what techniques and methodologies did you have available to you?

JC: The main technique was a microscope, and I did use salivary gland chromosomes.[38] I learned to [analyze] them. Other than that, the technique was simply that of growing flies. Sometimes I collected my own flies, but other people were doing collecting and they usually -- so I was interested in studying flies collected from nature. They did the collecting for me essentially, so I got more flies than I could work with.

I did enjoy this. I found four different closely related species that hybridized to some extent,[39] and I was able to, in some cases, do a genetic analysis of what caused the hybrid sterility. And I found an interesting mutant that had just a slight effect within the species but had a gross effect between species. Nowadays, that's commonplace, but that was a little bit novel at the time.

I also discovered something that's become very much talked about recently. I didn't establish it very well, nor did I realize the significance. It's what is now called reinforcement. That is, if you take two species that are partially incompatible in the same environment, they'll become totally incompatible; otherwise, the system breaks down. And that building up of incompatibility is called reinforcement.[40] I found that flies from the same locality, different species from the same locality, were very reluctant to mate. Whereas, those from different localities would mate, at least in the laboratory. But I didn't make much of it. My thesis was finally published at the University of Texas publication. They had a series of papers on [*Drosophila* genetics].

AM: While you were there?

JC: No. It was a year or so later. I wrote it while I was there. I actually did have one paper published while I was still there. I wasn't that excited about doing publication, and I wasn't even sure that I wanted a research career. I liked it, but I wasn't sure that I would be any good at it.

AM: I've interviewed quite a few young scientists, within the first five years of their first faculty tenure track appointment, biomedical researchers, and to them, it would be anathema to think that they could leave the graduate program without having not just publications, but some significant publications, as perhaps judged by where they were published. How different was that –

JC: This is just a different time. When it came time to begin looking – Well, I had hoped to do a postdoctoral fellowship, and I really did want to study with Sewall Wright. Patterson did ask Wright at the meeting if it's possible to work out something, but it didn't work out. I don't think it would have been practical anyhow, because I graduated in 1941 and it was pretty apparent we were going to be in a war before the year was out, and I don't think a fellowship was the right thing at that time. So I wasn't really disappointed not to. I had not even met Wright at that time. I admired him greatly by just what he wrote. I also read [Ronald A.] Fisher, too.[41]

Then I just decided to look for a job. I applied to quite a number of different places, and the one I got was at Dartmouth College. I believe that was almost the only significant opening that year. There weren't very many graduates, either, but jobs were scarce then. My rival was a good friend then, and still, for the position at Dartmouth. It's interesting why I got that job. It was to replace Jim [James V.] Neel.[42] Sorry you didn't interview him. He would have been a good one. You're a little too late. It was to replace him while he went on leave for a year. I was reasonably sure he wasn't coming back, but anyhow, the appointment was for one year and nobody knew what would happen after that year.

Another person that was considered for this appointment was Harrison [D.] Stalker, who maybe you have heard of.[43] His mentor was Jim Neel. It wasn't his mentor, I'm sorry. Neel knew him, though, and recommended him. When I got there, I discovered what the story was. The man that taught elementary biology, which Neel had assisted and then I was to assist, just didn't get along with Jim Neel at all. Well, the long and the short of it is, if Neel recommended someone, he wasn't going to have him. (chuckles) So the reason I got this job was because Neel didn't recommend me. I think the department thought that they didn't have much choice between these two people, and they might as well appease the fellow. So that's why I came.

AM: Before we move on to Dartmouth, I want to talk a little bit more about Texas, and right off the bat, how common was it for graduate students to do postdocs then?

JC: Hardly ever. It certainly wasn't the routine thing that it is now. Maybe I graduated at the wrong time, but I think the people that were ahead of me – the ones that had postdocs, at Texas at least, were the ones that stayed on at Texas because they couldn't find a job. So somebody scrounged around and found an appointment for them. But this was not a postdoc in the sense that you would seek it out, it was a temporizing affair.

AM: And for you, the importance of taking a postdoc with Wright would be –

JC: Would be really to learn something from him. I would have profited by it, I'm sure, but of course I got him as a colleague later on, which is even better.

AM: You had mentioned that [Wilson] Stone had encouraged you to take a more mathematical statistical approach to a problem as a means to solving the problem. At that time, how important did you think mathematic statistics was going to be in your work?

JC: Well, I just liked it, and so I went in for that reason. I wasn't sure that I would ever be good enough to make significant accomplishments, because it was all I could do to understand either Fisher or Wright, and I had no feeling that I could carry on or do anything [comparable to what] they had done. Mostly to me it was a matter of just enjoying learning it without giving too much thought as to what I would do with it. That sort of came later.

AM: How important was it to read Wright and Fisher among the other graduate students?

JC: I was the only one who did it. I talked with Stone about it, but I was the only one who had any real understanding of Wright's work. Stone and I talked a great deal in general about evolutionary problems, but the truly mathematical part, he didn't understand what I was doing. And I barely understood what was Wright was doing. I kept on even after I moved to Dartmouth reading Wright's papers and spent many evenings doing that. But I'll talk about Dartmouth later.

I can't see much else to say about graduate student life. The group of students who were working in genetics were all good friends, and we rented a house and lived together one year. Each of us had our own thesis we were working on. We were doing pretty much the same thing. So it was a pleasant kind of companionship.

AM: One last question and we'll leave Texas and we'll leave off for the day. That is, the program at Texas when you arrived there, [Hermann J.] Muller was gone, but they had kind of brought him in to build the department up. Muller is an interesting character for a variety of reasons, and he left to go to Russia, and his political consciousness is part of the kind of mystique of him as a geneticist as well. When you got there, what was the legacy at that point being formed around Muller's tenure at Texas?

JC: Well, he certainly left a legacy, and later on I can tell you quite a bit about him because I got well acquainted with Muller later on. But what did I know then? I knew that he had been leftist, maybe Communist, I didn't know for sure. And I knew that he'd gone to Russia for idealistic reasons. And I knew that he had been associated with various kinds of leftist student politics at the university and had essentially burned his bridges. I discovered it after getting there, I didn't know it till I got there. But then I soon learned he wasn't coming back.

There was a lot of talk about Muller. He was certainly the most famous of the Texas people. It was clear enough that Patterson didn't like Muller, but he respected him. And Stone had been Muller's student. He both liked Muller and respected him. But neither Patterson nor [Theophilus S.] Painter really liked Muller. Muller thought they were stealing his ideas, and I think that's – stealing is the wrong word. They were using them [freely] without [thinking about] stealing.

The problem that Patterson was working on, just finishing up when I got there, was one that was suggested by Muller. I'll tell you what it was. There was a question, raised by Muller, I'm sure, and that is whether the X chromosome determines femaleness by a single gene on the X chromosome, or whether it's a whole series of genes along the chromosome. Well, Patterson had started working on that problem, I'm sure instigated by Muller, by using translocations[44] and combining left and right halves of translocations and systematically covering the X chromosome this way.

But there was one region he couldn't get. The flies all died. I remember – and it was a suggestion from Stone, the technique for this – that instead of using translocations, to start out with a fly that had a long duplication, which included that region and wouldn't have lived with such a long duplication, but to radiate the fly, and hope to delete much of this [chromosome] and leave just the right region. So I worked on that, and it worked. And I finally solved this problem; when the fly came through, it was not a female. No, it was a male. Which means there was not a single female sex determining system. I did that while I was still a graduate student. I didn't write it until after I got out, but it was one of the first papers that I wrote from Dartmouth. That finished up this problem, but to answer the question you raised initially, this was the Muller heritage, and clearly this was a problem that had been suggested by Muller.

I know from later conversations with Muller, but even more from reading, that he felt that the group in Texas was simply capitalizing on his ideas, which was true. But I've never thought that was so bad. Muller had far more ideas than he could carry through himself. But Patterson clearly was – although, as I said, he really didn't like Muller, but he respected him.

Painter was working on Muller's ideas because, with the salivary gland chromosomes, clearly the idea of using them was a way of discovering the fine structure of genetic mapping. [45]That came after Muller had left. Well, I can't be too specific about what Muller's interests on Painter were. Even before he worked on salivary gland chromosomes, he did the same kind of question with other chromosomes, essentially to show that when chromosomes broke, the genetic consequences were what you would expect from that.

AM: So Muller's scientific legacy pretty much remained intact at Texas.

JC: Mm-hmm.

AM: Did his persona as activist-scientist [transmit to] any of the graduate students?

JC: No, it didn't.

AM: Including your own –

JC: Well, I would have been receptive. But there was no political activism among graduate students, among us graduate students at least, at Texas. I tried a little bit to join some societies, but nothing came of it. So essentially, I was just a graduate student and forgot all my political activism. There were stories about Muller and about his leftist views. Patterson was right wing in his own views, so he didn't approve of Muller at all in his politics. Stone, I think, was sympathetic, and he'd been a student of Muller.

They had left by the time I got there, but Muller had two Russian people come to work with him. One was named [Israel I.] Agol and the other was named [Solomon] Levit.[46] They had left at the same time, or before Muller left actually. I remember several people talking about these Russians there who kept trying to propagandize and proselytize these people and convert them into Communism. But that's the wrong thing to do in Texas (chuckles), and I don't think it got anywhere. The students I was with came later, so they didn't know any of this, but among the faculty members and the people who had been around a long time, most of the talk about these two Russians was the gossipy and amusement sort, rather than whether they were influenced by the politics.

One of the things that distressed me is that both of those two were liquidated pretty shortly after they went back. They were both charged, before they were killed, by being anti-Communist. Well, they certainly weren't when they were in Texas. They made a nuisance of themselves at Texas by preaching Communism.

AM: OK. I think we're at a good place to stop for the day.

JC: OK

Session 2, June 2, 2005
III. Ann Crockett Crow; Teaching at Dartmouth; WWII; Parasitology Training in Guatemala

AM: It is June 2nd, 2005. I'm Andrea Maestrejuan with James Crow in his office at the University of Wisconsin to conduct the second session of his interview for the UCLA Human Genetics Oral History Project. What I'd like to start off with was to follow up on something you said yesterday about the University of Texas [at Austin], that it was shifting away when you were there, from a research program that emphasized cytogenetics to one that emphasized speciation. I wanted to ask what was driving this shift?

JC: I don't really know what was driving it. It was very popular at the time, largely because of [Theodosius G.] Dobzhansky,[47] who sort of set the tone for this. Patterson and Dobzhansky were good friends, so my guess – it's just a guess – is that that was the influence. Anyhow, it was an interesting time to be there because I got, in some ways, the best of both of these worlds. I worked enough in classical *Drosophila* work to learn quite a bit about it, and then directly into speciation.

AM: You just went to something off camera that you got married in Texas. Last we heard you were living with a bunch of graduate students in Austin, so that must have changed at some point.

JC: Well, I lived with a bunch of graduate students right up till the time I got my degree. But I met my future wife [Ann Crockett Crow] roughly a year and a half before graduating, a little over a year anyhow. We met in the university orchestra. She was a clarinet player. She had the nice Texas name of Crockett, which is hard to improve on in Texas. (chuckles)

AM: Was she actually descended from [Davy Crockett]? I thought his lineage died in the Alamo.

JC: I think that's right, although he may have had prior children. (chuckles) Anyhow, there was one of her relatives that was very interested in proving his relationship to Crockett and hired a genealogist to see if she could do this. She didn't succeed, but she wrote the – I can't quote it now, but a beautiful equivocal letter that just didn't say much. Not the slightest evidence that he was related to David Crockett.

She was a clarinet player in the orchestra, and we knew each other casually in the orchestra. But the place we really first got acquainted was, we were both playing in an orchestra pit. I told you about this practical joke with the fruit flies.

AM: Right, with the fruit flies.

JC: This wasn't the same occasion, but it was that kind of thing. It just happened that I was seated next to her. That's one way to get acquainted. If you're in an orchestra pit, it's crowded anyhow, and it's hard to avoid your neighbor. So we went out afterward, and that was the beginning of it. That was pretty close to the end of the year before my last year. Then we dated regularly during the following year. We got married in the summer. But I had my Ph.D. by that time.

AM: Did you have a job by that time?

JC: By that time I had a job. In my thesis, I remember writing in the introduction that I wasn't married and I didn't have a job, but I hoped to remedy both. And I succeeded. (chuckles) I started to say that jobs were very scarce at that time. The Depression wasn't over, which was one reason. But there was all the uncertainty about the European war that discouraged universities from hiring people. The only other opportunity I had was a one-year appointment at Centenary College [of Louisiana], which is not a very high standard school. The emphasis was on the football team, and I really –

AM: And where was this?

JC: Louisiana somewhere. I was prepared to go there if I couldn't find anything else, and then the Dartmouth job came, which pleased me enormously, of course.

AM: Did your future wife go to school in Texas as well?

JC: Yes, she did. She was at the university. She only went through two years, though, when we got married, which wasn't too pleasing to her family. Her mother thought that I was robbing the cradle, you might say. I was six or seven years older than she was. We were married sixty-two years, perfect marriage.

AM: Another question I wanted to ask –

JC: I mean sixty years, excuse me, sixty years and two days.

AM: Well, still.

JC: (chuckles) We don't quibble over that.

AM: That's fine. Most of my friends who got married when we graduated from school are no longer married to each other, so sixty years is pretty phenomenal. We kind of left off yesterday with you going off to Dartmouth and basically getting the job by default, so to speak. Were you hired specifically to teach genetics? Were they looking for a geneticist, or were they looking for somebody in biology?

JC: They were looking for a geneticist, but it was expected that whoever taught the genetics course would also be the assistant in the general zoology course, which is what Jim Neel had done, and which is what I did too. Within a few weeks, possibly months, I discovered why Jim Neel didn't get along with this person. I didn't either. So I only taught in that course for one year and managed to finagle things so that I taught some other courses.

AM: Did they have a genetics department?

JC: No, it was just in zoology, and [genetics] was just a course. There was no second or third course really. There were opportunities for students to do little undergraduate research projects. A few of them did, but there wasn't an awful lot of that.

AM: If I remember right at Dartmouth, and this was just a couple years ago when I was there, that the medical school is located near the basic sciences, the complex of buildings. Had clinical genetics at the medical school been started there yet? Or did you have any –

JC: Well, I helped start it. It was after I'd been there two or three years. I was first asked to teach a course in statistics. It was decided then that medical students should learn statistics. So I taught statistics to the -- the first course the medical students took was statistics, and I could see the disappointment written on their faces. They came here expecting, at last I'm going to see the human body, or push pills, and why do I have to learn the chi square distribution?[48] I think if I hadn't known these students already – because many of them attended my undergraduate courses – I probably would have been tarred and feathered and chased out of town. They also started teaching genetics at that time, so I gave both statistics and genetics to the medical students. These were both rather short courses.

Later on, one of the students told me something interesting. He said he hated statistics when he took it and he loved genetics when he took it, but when he got to the third and fourth years of his medical school, which was not in Hanover any longer, he was the leading student in the class because he knew about the chi square test. He wrote back to thank me for teaching him that.

One thing, Dartmouth was entirely male students at that time and became coeducational quite a bit later. The medical school at that time was a two-year school, and it was considerably closer to the campus than nowadays. It was just really right on the campus. I enjoyed the medical school teaching, which I did every year. But that's only a small part of the teaching story.

I started out teaching general biology, and then in the spring term taught genetics. By that time, the war had started, so the university decided to train its faculty. Those that knew any math at all could teach math. So I got dragged into teaching math for two or three years to the students. Rather soon, Dartmouth became a V-12 unit [United States Navy Training School].[49] It was V-12 in those days, training for naval officers. So I taught math courses virtually the whole duration of the war.

For me it was great. If everybody's war experience was as nice as mine, wars wouldn't be so bad. Since I was brought in more or less to teach math, I could pretty much choose the courses I wanted to teach. So I just started with analytical geometry and went straight through three semesters of calculus with the students. I knew the material before but not well enough to teach the course. I've always been thankful because – it's gone now, but for many years, I had at my fingertips the kind of techniques that otherwise I'd have had to look up. So I've always been thankful for that. But that's only part of my teaching.

AM: So you didn't have to worry about being drafted at this point.

JC: I didn't worry, but I came awfully close to being drafted. I'll tell another story first. I thought maybe I would be drafted, so I decided to take navigation. I thought that would be something I could do. I had for no good reason taken a course in spherical trigonometry, which is what navigation's all about. So I took a course specifically designed for Dartmouth teachers. It was fun because I was way ahead of the rest of the class, knowing this stuff in advance. And it's kind of fun to be the bright boy among my colleagues. Anyhow, when I got through, I took the exam for being a navigator, and if you got a sufficiently high score, that enabled you to be a teacher of navigation, which I did. So I was immediately qualified to teach navigation. I did that for a year or two, along with these math courses.

Then something quite different happened. I was fairly high on the draft priority list. In Texas when the draft first came, I still had some pacifist instincts and I registered as a conscientious objector. Then I changed my mind about that and wrote the draft board.

AM: And why did you change your mind?

JC: I just decided Hitler was something to go after, and my earlier pacifism was all right in the abstract but not when Hitler was involved. So it was the state of affairs in Germany that determined it. I should have decided earlier, but as I told you yesterday, I didn't pay much attention to the news as a graduate student. I shifted from very much of a political activist as an undergraduate to just a full-time student, more or less ignoring politics throughout graduate school. Then I took it up again when I went to Dartmouth.

Either the first or the second year I was at Dartmouth, I decided that I might someday want to teach medical genetics, I wasn't sure. So I took a course in parasitology,[50] which was offered for the medical students. It was in the building where I taught. It was very easy for me to go there every day. So I did take that course. The very next year, the man that taught the course, but also a malarial surveyor, at least he was a professional parasitologist, went into the army doing parasitological work. I was the only person in Hanover, New Hampshire, that had ever had a course in parasitology, or at least the only one among the Dartmouth faculty. So I immediately started teaching parasitology.

Then – this could go on forever. Then the [John and Mary R.] Markle Foundation[51] – I believe that's what it was – decided that the medical students, many of whom when they graduated were going to be sent to the South Seas and various tropical places, didn't know anything about tropical diseases, which are often from parasites. So they arranged for people who were teaching parasitology to get a course in tropical diseases. This was in 1943. It involved a six- or maybe eight-week course at Tulane University. They got someone else to teach my math course for me. So I took this course in tropical diseases, mostly parasitology, partly insects and partly viruses the ordinary things.

I was one of the few non-MDs in the class and was pretty badly lost for a while. On the other hand, I knew more biology than they did, and I found it easy to distinguish one worm from another (chuckles), or even more of the insects. Part of the course consisted of a total of four weeks in Costa Rica and Guatemala, partly in the field and partly in the laboratory just as a training run. I loved it, I had a great time there. That was just a little bit over a year later.

One happy thing that happened – I think it was still 1943, could have been '44. I started to go to – on the way to Guatemala, I almost overslept the train and had to run and catch it, almost the way they do in the movies, running with my suitcase and jumping on the back car. But in the course of all this excitement, I forgot my glasses. I wrote Ann. Somehow she knew I'd forgotten them, and she sent them to me in New Orleans. Fortunately, my flight to Guatemala was postponed day after day, so the glasses came before I left.

There was just one flight from New Orleans to Guatemala, and it left at midnight. So I went to the airport that night, and the flight was canceled because of the weather. So I went to the French Quarter, naturally. Then the next night, the same thing happened again. And the third night, again it was canceled. By that evening, the Budapest String Quartet was playing in New Orleans, so I went to their concert and then out to the airport afterwards, and the flight again was canceled. I don't remember how many nights this went on, but eventually, the flight took off.

The experience in Guatemala was really a very pleasant one. I learned a lot about parasites and tropical diseases. Then went on down to Costa Rica and worked in the hospital mostly there. They had a saying in the hospital that every patient in the hospital has three diseases. He has malaria and he has hookworm, and then whatever he came to the hospital for, which was not too bad a description really.

On the way back, or even before leaving, I knew exactly the days I'd be in New York, so I arranged to buy tickets to two Broadway plays. In those war years, getting tickets to Broadway was almost an impossibility, except months in advance. Anyhow, I did know long enough to do this. The two were the high points in my theater-going experience. The first one was *Othello*. The Moor was played by Paul Robeson, and Iago was José Ferrer. It's just a dream cast. I've never seen anything like it. I saw *Othello* recently this summer, and although it was a good performance, it seemed pretty tame. No voice like Paul Robeson's. And Desdemona was played by a Madison girl, that I later got acquainted with, Uta Hagen.[52]

AM: Oh, she was from Madison. Didn't know that.

JC: Uh-huh. I knew her mother well. I don't know whether this is useful for an oral history or not.

AM: Well, it certainly contrasts. I imagine your kids, when they ask you what you did during the war, you have quite different stories to tell. How did a Ph.D. graduate in *Drosophila* classical genetics – what were you doing in a hospital in Guatemala? I mean, were you learning –

JC: It was mostly laboratory aspects. But I did go around with the physicians on their clinical rounds. One time – of course you remember your successes and not the failures, or at least I'll talk about the successes. We were asked to diagnose what these different patients had, and I recognized this one as a drunkard. I don't know why, but he just looked like an alcoholic. I knew that alcoholics frequently had vitamin deficiencies, and he had a way of walking that's characteristic of neuropathy, that happens with a Vitamin B1 deficiency.[53] So I made this guess and immediately increased my standing in the course. Not permanently because I made mistakes later, but – (chuckles)

AM: OK. At this point – maybe this is the wrong way to think about it, but when you went back to Dartmouth, was this a tenure track job?

JC: It started out as a temporary appointment to replace Jim Neel. As I told you yesterday, I think, I strongly suspected he wasn't coming back. He probably would have a permanent job [elsewhere]. I was an instructor for two or three years and then got promoted to assistant professor. I don't think Dartmouth had tenure rules. I'm not sure about this. I was never conscious of having tenure, but I never had the slightest doubt that I could stay there as long as I wanted to.

Oh, as far as the draft is concerned, I did get called up for the draft and went through the physical examination and all the rest of it, but the college appealed, appealed it for me. By that time, teaching all these courses, they decided I was worth more there than in the army, which I think is true, actually. I would have been a pretty poor soldier.

AM: It sounds like you were teaching a bunch of courses that were relevant to training young recruits.

JC: Very much so. Of course, research was essentially non-existent during that time. And I taught long hours. I taught three or four classes every day. So I just spent most of my time either teaching or preparing to teach. I did fiddle around a little bit with research problems, and I wrote up the paper on sex determining genes while I was there.

I also did something that's far and away the most popular thing I've ever done in the sense of the number of copies and reprints requested. Everybody does the chi square test and looks up the values in the table, and I decided that it would be nice to see if I could find a way of presenting that graphically. I played around with the distribution a little bit, and I found a transformation that made these values essentially linear, so I didn't have to compute so many points to draw to draw the graph. But I still had to look – and many of the points I did get out of the literature, too.

So I started drawing this graph. There were a few points that I did have to compute. I'm sure we do better now, but in those days, you couldn't integrate chi square distribution. You had to do it by expanding it into a series, which I did. But it was a very slowly converging series, and it took me all afternoon with [not a] hand operated, electric, but that old-fashioned kind of chunk, chunk, chunk calculator. Anyhow, I finally finished this graph and published it in the Journal of the American Statistical Association. It's still being used in various textbooks.

IV. Getting to Know Muller, Fisher, Wright, and Lederberg; Children

AM: So by about 1945, what were you doing, and where was your interest in genetics, and where was your focus? What was driving you?

JC: Well, genetics was pretty largely in abeyance during that time, but there was one aspect of it that wasn't. This has been very important in my future life. While I was teaching at Dartmouth, H. J. Muller was teaching at Amherst College. He was on a temporary appointment there and didn't have a job, a permanent job. I remember being enormously incensed that here was the world's greatest geneticist that didn't have a permanent job.

I'll jump ahead a little bit. Two things about him at Amherst. One was that he had just been told that he couldn't stay beyond that year. This was about the time the war was ending, '45 or '46, '45 it was. He had just been told that he couldn't stay there permanently. Then there was a woman named Dorothy [M.] Wrinch, who was a theoretical protein chemist, and she was making three dimensional molecules of proteins.[54] I have no idea if it turned out to be useful or not, but it attracted a great deal of attention at the time. It was her husband who taught biology at Amherst, and I don't think she even had a job there, but she did have a place where she could work. The same day that Muller was essentially given his walking papers, she was told she couldn't keep that room after the end of the year.

That very week, I happened to be visiting Muller, and there was a South American geneticist named [Andre] Dreyfus,[55] who was distinguished enough that he was taken to visit the president. The first thing he did when he met the president was to congratulate him on having two such distinguished scientists as Muller and Wrinch. I didn't know then and don't know now whether he did that innocently or archly, but in any case, he did.

Then just a few months later, Muller got this job at Indiana [University], which is where he was the rest of his life. Later on, I visited Indiana, spent a semester there, and talked to the man who had hired Muller. He said that several people advised him not to take Muller on, that he was cantankerous and hard to get along with, and a prima donna. But everybody knew he was a great scientist. This man said, "Well, I already have three prima donnas on my faculty, one more won't make any difference." So they hired Muller. And then he got the Nobel Prize within a few weeks after moving to Indiana. I've often wondered what Amherst thought about dismissing a person who later won the Nobel Prize.

All this was going on probably in – I'm a little vague about the years, but it was about the time the war was ending, or beginning to end, at least. There was one time in which I even thought I could get away for a semester from Dartmouth, and I thought of going to Amherst and spending it with Muller. Then I decided for reasons of idealism and patriotic purpose that it really wasn't right to take a research appointment during the war. So I stayed and did my teaching job.

AM: And what did you want to do with Muller?

JC: I didn't have a very clear idea. I knew I wanted to continue in *Drosophila* genetics. To back up just a little bit, my first visit to Muller, which was probably 1945 or '44. It was while I was doing all the rest of this teaching, but I did sneak away to Amherst once in a while, several times, actually. But the first time I visited him, it was my first contact with that kind of a mind, and I was tremendously excited by it. Stone had told me how smart Muller was, so I wasn't surprised at that, but I was still taken aback. I think this is the only time in my life I was so excited about the various new ideas that I got from him that I didn't sleep at all that night, and that's the only time in my life I think – I've been awake all night, but never because of sheer scientific interest in a subject before or since.

Muller suggested a couple of things that I might do, both of which would have been good if they'd worked. So I did putter around in the laboratory a little bit. But Dartmouth was not an ideal place to do research. I had to make my own media and wash my own bottles, and doing all this heavy teaching. So I had relatively little time for research, and neither of these things that I'd planned to do that Muller had suggested, neither of them worked out. So actually, as far as research is concerned, I got very little done during the four years at Dartmouth. Let's see if there's anything else to say about that period.

AM: And this little bit of research that you were able to do, was it with *Drosophila*?

JC: Yeah. I also did a little bit of theoretical work later, but I'll tell you about that in a minute. Let's see, before I get too far ahead of the story. I think I told you most of the things that were going on at Dartmouth. I had three children.

AM: I was going to say, in '43, if you were going to – I forgot where you said you went for the semester course on tropical diseases.

JC: New Orleans.

AM: New Orleans and then Guatemala and then Costa Rica. I think your first child was born in '43 as well. So you were quite busy that year.

JC: He was an infant when I left. Then I had two more at roughly two-year intervals.

AM: So they were all born in Hanover.

JC: They were all born in Hanover.

AM: OK. Just briefly – because I don't know if it will come up again. Did you encourage any of them to become scientists?

JC: I didn't try to influence them. It wouldn't have made a bit of difference if I had. They went their own ways. I'll just tell you a little story right now. My son, the oldest, firstborn, he became a computer graphics expert ultimately and now lives in Silicon Valley. The second child was a daughter [Laura Crow] and she was theater-struck and was going to be an actress, and then a dancer, and ended up as a costume designer, at which she's been quite successful. She teaches at the University of Connecticut now. My younger daughter got a Ph.D. in English literature, in Victorian novels, and taught Jane Austen, et cetera, for a while. She's mostly just a housewife now and takes care of her children, does some teaching.

Her husband got his Ph.D. at roughly the same time. His was in English literature, but his thesis was on [Edmund] Spenser's *Faerie Queene*, and there was no demand for Spenser scholars.[56] (chuckles) He tried for two years to get an academic job without finding one that he really would like to take. So he went to law school. He's been a successful lawyer. On the way to law school, he announced that he was going to be the richest Spenser scholar in the United States.

AM: (chuckles) Has he gotten there?

JC: (chuckles) Probably is, I don't know.

AM: Where are they?

JC: They live in Madison [Wisconsin].

AM: So no scientists and no musicians? Or are they musicians as well?

JC: Two of them are musically gifted. My son plays bass and plays quite well. Either classical or jazz but especially jazz bass. Then he got married to a young woman whose father was a physicist here on the campus and later in the University of Chicago. Then they moved to San Francisco. I thought he was going to become a San Francisco bum. He was playing in a rock band. But he also sang in a Bach chorus, so it wasn't all bad. He stayed out of the draft – this was Vietnam – he stayed out of the draft by being a computer programmer. He always liked art and finally found what he wanted to do, which was computer graphics. Then once he found that, everything was beautiful from then on.

Back in Hanover. By the time the war was over, I stopped teaching all this math and went back to teaching genetics again and biology courses and teaching considerably less, too, so then I did have a little time for some research work. Then I took a chance to get a semester off, and I went back to Texas spent it there again and sort of tuned in again on *Drosophila* research. I did little projects that didn't amount to much, on ether resistance in *Drosophila*.

At the end of that time, there was a summer course in Raleigh, North Carolina, in statistics. The star attraction was R. A. Fisher, and I of course wanted to go to this. It was a great course. I learned lots and lots of statistics and loved it. It was also a chance to meet Fisher. I'll tell you how I met him. He gave a lecture -- they had regular courses, and I attended his courses, along with other people. They had an evening lecture that Fisher gave, and it was on the Rh factor. The three-locus model of that was new at that time.[57] I had never heard of it. Fisher presented that, and I just thought that was the most exciting thing I'd ever heard, so I was quite eager to learn more.

After the lecture was over, they asked for questions, and someone in the audience asked a question about the chi square procedure. Fisher didn't really answer the question. He said in effect that I'm the person who straightened out degrees of freedom with chi square, and why challenge my ability to do this, or something. Anyhow, it was a pretty adverse answer to what was probably a perfectly simple question. But that did stop most of the questioning. There were no further statistical questions. Then finally I screwed up my courage and asked a genetic question, and of course that's what he wanted. So he answered that.

AM: What did you ask?

JC: I can't remember. It had something to do with the three loci. I can't remember exactly what it was. Anyhow, after the lecture, I went up to the front and asked some more questions. I was about the only one – I probably was the only one – this was an audience of statisticians, and I was probably one of small number that had any idea at all what he was talking about. Anyhow, we did have this short conference afterward. He said, "Do you want to have a glass of beer?" There's a bar pretty close, across the street, so we went to the bar and sat down, ordered beer, and they didn't have any. We still had wartime shortages. This was in 1946. So we said, "Well, let's try wine," and they didn't have any wine, either. It wasn't much of a bar, I must say, although there were a lot of people there. I don't know what they were drinking.

But they did have champagne, so we ordered a bottle of champagne. I got it uncorked, and then the waiter came up and said, "I'm sorry. You can't drink it here. There's a North Carolina law against drinking champagne in public places." I think it was meant for wine, too. So we corked up the champagne and went back to my dorm room and drank the champagne. It was a wonderful way to meet Fisher. And he was a friend for the rest of his life. I saw him, one way or the other, probably an average of once a year. Then his daughter moved to Madison, so he came regularly to visit her. We saved problems for him. He always visited me when he came. He really became a very close friend.

AM: Had you met [Sewall] Wright by this time?

JC: Yes, I had met him.

AM: Did you talk to Fisher about Wright?

JC: Yes, I did. And Fisher made it quite clear he had no respect for Wright, in genetics, that is. By this time, I had not talked to Wright about Fisher. There is an interesting story about this, though, about comparing Wright with Fisher. While I was at this summer course in Raleigh, I had an idea. It turned out to be something that had already invented by [J.B.S.] Haldane,[58] but I didn't know this. It had to do with the idea that the impact of mutation on the population depends on the mutation rate rather than the severity of the mutation of the genes. Once that's pointed out, it's sort of obvious, but anyhow, I discovered it for myself. So I took it to Fisher. By this time, I was acquainted with him. After that one meeting, we talked quite a bit down there in North Carolina. I showed it to him, and he actually did a little scribbling on the paper to verify that I was correct. He said, "That's a good idea. I think you should publish it," which I eventually decided to do.

I didn't get around to it very soon, and around Christmastime of 1946, I guess it would have been – I did meet Wright then, at a Genetics Society [of America] meeting.[59] I asked him about this, did he agree with it? He said, "Yes, I agree with it. I have ever since I read it in Haldane a few years ago." So I realized that it was something Haldane had done. But it's interesting that Fisher did not know that Haldane did it, because he didn't read much. And Wright, of course, read everything. I probably have these years wrong, but I guess it doesn't matter too much. It can be off a year or two as to when these various things happened.

AM: Yes, right.

JC: I did have a short talk with Wright at that time, mainly about this particular problem. When I wrote my paper, which was just a few months later in the spring of '48, when I wrote the paper, Wright was a reviewer of it, and he made a couple of very useful suggestions. So that was my first theoretical paper, and it attracted quite a bit of attention. It provided evidence for what was beginning to be called the overdominance theory of hybrid vigor.[60] I thought this time that this offered evidence for it. Later on, I changed my mind, not because of [the theory being wrong], but because new data came in. It wasn't published till I got to Madison, so I'm getting a little ahead of the story. But I wrote it while I was still at Dartmouth.

AM: OK. Just to get to your publication record, because I may be missing a little bit. The earliest [paper] I pulled off of PubMed was 1950. Would that be about correct?

JC: This came out in '48, and the statistics paper was in '45. And I think the sex determination paper was '45. Might have been '46. My thesis was '42. I actually wrote a little paper that was published in '39. I was one of three authors of a little paper on the technique of studying embryos by making them transparent and then staining the bones. I worked on that a little while as my first year as a graduate student. It's nothing I'm really proud of, but it was my first. (chuckles)

AM: Well, during the sabbatical at Texas, were Patterson and Stone and all the people, your mentors, still there?

JC: Yeah, they were all there still, so I sort of picked up where I'd left off. Stone and I had talked a great deal previously, and we did then. Actually, our roles were slightly reversed because Stone was in the war during the whole time, so he was totally out of touch with what was happening in genetics. Although I was busy doing other things, I did do a pretty good job at Dartmouth of keeping up with the literature. Actually, in that year, I was telling Stone things more than he was telling me things, but we, as always, had a good time together.

AM: And your purpose for going back to Texas was to?

JC: I thought I'd start some sort of a *Drosophila* research problem, and I did this little study on ether resistance, which never did get published.[61] I should have published it because it was not totally uninteresting. It was supported by the American Philosophical Society, so it's in their annual report.[62]

AM: And this was to kind of jumpstart a research program at Dartmouth?

JC: Mm-hmm. At that time, I was expecting to stay at Dartmouth. I was beginning to get restless. I like to teach, and I think I could have spent the rest of my life teaching at Dartmouth. They were smart students, and everything was pleasant about it. But I began to think that maybe I don't really want to become a Mr. Chips,[63] so I sort of thought that if an opportunity came to go to a research institution, I'd give it a try.

We're getting sort of toward the end of my Dartmouth career. As I said, I was getting just a little bit restless, but I wasn't unhappy there. It was a great life. The family loved it. We'd moved into a bigger apartment. So everything was just going swimmingly. But I decided to read a little bit about – this was 1947 – I decided to do a little more systematic reading in genetics than I'd had a chance to do previously. You could get a carrel in the library, so I then had two or three hours of uninterrupted reading. I did that almost every day for a while, and that sort of brought me up to date in genetics literature, which is one reason why I could talk to Stone. I knew more than he did.

Then I decided to go to the Cold Spring Harbor Symposium. It was the summer of 1947. It was on nucleic acids. I'd pretty well decided by that time that nucleic acids were likely to be important, although I certainly didn't have the insight that thought it was the gene. I thought it was high time I learned something about it, so I did go to the Cold Spring Harbor Symposium. And it was the turning point in my life, I think it's fair to say, because for the first time I got acquainted with several people who later became close friends. Tracy [M.] Sonneborn was one.[64] I saw a lot of him there. He said I should get out of Dartmouth, I remember that. He said, "You'll never get anywhere if you stay up there. You should go to a research university," which I was going to do, but there wasn't any chance yet. The important point was, I met Joshua Lederberg at that conference. The way I met him –

AM: And he was at [University of] Wisconsin at that point?

JC: He was on the way to Wisconsin. It was in the summer of '47 and he was coming here in the fall. He wasn't on the regular program. To back up a bit, I'd already read his paper pointing out there really is sex in bacteria. I read the paper and enjoyed it. I was immediately convinced. Not everybody was. But it was clear to me. So I thought this was just great. That meant that *E. coli [Escherichia coli]*[65] could be studied genetically just like higher organisms.

Lederberg was at this conference, but he wasn't on the program. Then sort of by popular demand – maybe coming from himself, I'm not sure, but it would have come from me certainly – he gave an evening lecture on these topics. It was a vivid time for me. He talked for two or three hours, it seems. Maybe it wasn't that long. A long time, but I was interested the whole time. Then there was quite an animated discussion afterwards.

What got us acquainted – or what threw us together anyway – was that I asked a question that showed some genetic perception. It had to do with linkage and interference, and the details were kind of interesting, but anyhow, it showed him that I knew what he was talking about. Then that got us started talking regularly. He said then he was going off to Wisconsin. And we talked a little bit about how to teach genetics because he thought he'd be teaching undergraduate courses. It turned out he didn't, but he thought he was going to be.

Then I met him again that same summer. The Biometric Society[66] had a meeting at Woods Hole [Marine Biological Laboratory in Massachusetts], a two or three day meeting. So I saw him again there, and again we talked, and really became very compatible. I decided then if I ever had a chance to be with Lederberg, I'd certainly take it.

Then the next Christmastime, I was invited to come to Wisconsin for an interview. I don't know this, but I'm sure he was the instigator of it. No one else here knew me. So I came for the interview and was offered a job.

V. Teaching Statistics and Human Genetics; Research Work with *Drosophila*; Agriculture and Genetics at Wisconsin-Madison

AM: Did you know Wisconsin?

JC: Didn't know anything about it at all, but I had a sentimental attachment for it even when I was in college, I remember. Mainly for this great liberal tradition that we talked about the other day. There were several things about it. I would have come under any circumstances, but that was one. The other thing that interested me was the string quartet in residence here, and I'd have a chance to hear a world class string quartet. So as I said, I would have come anyhow, but that was gravy.

AM: To go back just a little bit. When you said you got a carrel in the library and were starting to read more in genetics, or at least to try to keep up, how did you do that? What kind of genetics? Or was it so specialized at that point that you could go to genetics and get –

JC: I read rather widely. I knew I was mainly interested in population genetics, but I decided to learn what they now call molecular genetics, too, so I read quite a bit about proteins and DNA, which is what whetted my appetite really to go to this summer conference.

AM: What were you thinking at the time? I guess it would be considered biochemical approaches to genetics would offer more than population geneticists had to at that point. What was catching your eye?

JC: I wasn't really expecting to do research in that area. I just wanted to be – partly, I thought I'd be a better teacher, but that wasn't the whole reason. I've always had a broad interest, considerably broader than I'd ever make use of, in a research way. And that's still true. I've been dilettantish all along. It's not good advice for anybody else. It's worked for me, but it's usually not the way to make a name for yourself. It's better to pick something and stay with it.

AM: Would you consider that an individual thing, or maybe a generational thing?

JC: It's an individual thing. It's just my personality.

AM: OK. You had mentioned that when you spoke to Lederberg, he wanted to know how to teach genetics. What did you tell him? How does one teach genetics?

JC: I don't remember now what I told him, but I told him the kind of subject matter that I taught, because I did try to teach the latest thing. One student – again it was this Rh factor. I told him the latest business on that. He went home and talked to his father, who was a physician. He came back and told me his father was just astounded that his teacher at Dartmouth would know that much about what was happening the day before yesterday, which pleased me, of course. But it was also a statement about the kind of students I had at Dartmouth. They were very bright.

I told you earlier about this teaching statistics to the medical students. This was a two-year medical school with twenty students in each class. The students could save a year by going to medical school because they counted the medical school year as their senior year in college, so it was a sought after position. There were probably two hundred premed students at Dartmouth, so these were the twenty selected ones, and probably the best. If the selection system were perfect, they'd be the best. Anyhow, they were very, very bright. I think that's the reason I could teach chi square without being shot. (chuckles)

AM: Just to go back a little bit, what was the justification? Why did Dartmouth think that statistics and genetics needed to be taught to medical students?

JC: I don't think the initiative came from Dartmouth. I think it was a nationwide trend. I had nothing to do with the decision to do it. I don't really know where it came from, but I did know that all medical schools, most of them at least, started teaching a little bit of statistics at that time. Actually, when I was teaching this, I met a couple of people who were starting statistics courses at other medical schools, and we compared notes. I found I was teaching a lot more, actually much too much, but I had these brilliant students, so why not?

AM: You said you also taught the first classes in medical genetics there. How does a classically trained geneticist teach medical genetics?

JC: I read. There wasn't much clinical genetics. It was mostly general principles. So it wasn't an awful lot different from an undergraduate course in genetics, except that I deliberately chose examples from human genetics, whereas, otherwise I would have chosen fruit flies.

AM: What impact, if any, did this experience in kind of the clinical setting, because of your need to teach tropical diseases and parasitology, have on your classical genetic thinking?

JC: I don't think it had much effect on classical genetics thinking, but it did make me interested in human genetics.

AM: In what ways and why?

JC: Just because I was teaching medical students, and I couldn't help but take an interest in – well, I keep coming back to the Rh factor, but it was new at that time, and exciting. Also during this period of biochemical genetics and *Neurospora*,[67] which is not human but came to the fore, I learned about [Archibald E.] Garrod and the inborn errors of metabolism,[68] and I taught quite a bit about that. So there was quite a bit of what is now called – or maybe then was called biochemical genetics – that I put into the course. But I had population genetics, too. There, I was teaching what I already knew rather than learning much of it on the spot.

AM: But there was an undergraduate course in genetics that the medical school students probably took if they came in from –

JC: I think so. I think all these students in my medical class had already had general genetics, and they probably had – I also taught comparative anatomy and embryology, so some of these students had had three or four courses with me before they ever got to medical school. This one – probably more than one, but at least one I could identify, a math student that took the four courses in math that I taught in sequence, and he also took a biology course. Essentially, all the science he had at Dartmouth was from one teacher, namely me. I didn't mind then. I didn't mind teaching a lot of courses because I felt maybe that's the way I was doing my bit for the war effort.

I certainly had a kind of bravado that I don't have now, that is, in being willing to teach something that I'd just learned the day before, or in a course where by the end of the course the students knew everything that I knew, which was true of much of the parasitology. Nobody does that any longer. Everybody wants to teach only his own specialty. But in those days, most people taught lots of things they knew that they weren't that well acquainted with.

AM: Particularly in science, it seems to me, in biomedical research that you teach to your specialty and you don't teach the general biology classes anymore.

JC:	I think that's a career mistake, for the students as well as for the person doing the teaching. I've always been happy that – the whole period I've been at Wisconsin, I taught genetics every year, I think. And that does force you to keep up with the field.

Going back to Dartmouth, I learned a kind of a lesson. There was a team taught course in which I taught two sections. I taught endocrinology[69] and I taught genetics. At the end of the semester, there were these reports on how they liked the teacher, and my endocrinology scores were much better than my genetics scores. That got me to thinking a little bit about it, and I know the answer. The endocrinology, I was teaching most of what I'd just learned and I didn't know the complications. (chuckles) In genetics I knew too much really.

AM:	How difficult was it to decide to move to Wisconsin?

JC:	Not difficult at all. It was a chance. I had some reservations about whether I was really cut out for a high-pressure research life. Before I left Dartmouth, I didn't have any ironclad guarantee or anything like this, but I was pretty sure that I could go back and that if I got to the university and found out I wasn't good for it, I'd go back and enjoy my teaching. But after a couple of weeks here, or a month at most, there was no temptation to leave.

AM:	Before you got here, what were you anticipating you would be doing in terms of research, what kind of space you would have? What were your expectations before you got here?

JC:	I wasn't sure what research I'd do. That was more or less up in the air. I knew it'd be something in *Drosophila*. And I also knew that research was expected, that I needed to do something and I would be expected to. I was grossly disappointed in the kind of equipment that was available for me here. It was a lab in a temporary building that I shared with a class that was meeting there. I could hear their lectures and they could smell my flies. It was a very uncomfortable situation. Our genetics department at that time was what's now the agriculture-journalism building and was just very, very crowded. So anyhow, that was a disappointment.

I noticed Joshua Lederberg was crowded too, although not quite as crowded as I was. When I came here to just join the faculty – he within a few weeks, actually, moved to something better, but he was in a room not much bigger than this room. He and his wife and one student, Norton Zinder, who I mentioned yesterday, and a couple of young women who were doing bottle washing and assistants of various kinds, they were all crowded in that one room. I came in July and it was hot, and there was no air conditioning. I just thought that was – here was a person doing this magnificent work that later won the Nobel Prize under these horrible circumstances. I admired his getting this done under those circumstances as much as anything else. For some reason, I was inspired by this. If you can do world class work, at least you can do third rate work under crowded conditions.

AM:	So Lederberg had been here just a year.

JC:	Yes.

AM:	Because Wisconsin does have a pretty famous history in genetics, how was genetics organized here as a basic science and also as a clinical science, if there was any?

JC:	There wasn't any clinical science at that time. At that time, genetics was in the Agriculture College. Just a few years before, they [had] reorganized the teaching so that the genetics course was taken by students [from] all over the campus, so although the department was [in] Agriculture, the course was general, and that was the course I was brought to teach.

AM: Currently, the laboratory of genetics is still in the School of Agricultural Sciences.

JC: Yes, but I'll come a little later to the early days of medical genetics, because the medical school didn't get involved at first.

AM: OK. So how much teaching were you going to do, and how much research time were you going to get?

JC: When I came here, I was to teach just this one course in the fall and another course in the spring, so I taught one course each semester. That left lots of time for research, so I did start out with a research laboratory. Of course, I have the ability – disability – to get involved in too many things, so I was quick in tutoring some students that needed help, or participating in various kinds of seminars, getting on committees.

I remember Dr. [R. Alexander] Brink, who was chairman at the time I came here.[70] He protected me, he tried to keep me from doing things. Among other things, he let me know in a very subtle way, but I got the message, that he wouldn't really think it's too good an idea for me to play in the Madison Symphony [Orchestra]. That's time consuming business. Later on, we found out I could do that and still get research done. He changed his mind totally. But it was very clear at first that he thought I was spreading myself too thin. And I was.

As far as research is concerned, though, I decided the first thing to do in *Drosophila* research was to study the evolution of insecticide resistance, so I studied DDT resistance for about two years or so.[71] The kinds of things I found out were fairly predictable. One of the things that had some intellectual interest about it was that I found that the genes that governed resistance in *Drosophila* were mostly just simple and additive, cumulative. At the same time, I was reading the literature on the resistance to antibiotics in bacteria – and I'm talking about resistance that depends on many genes rather than one big gene – and this polygenic resistance in bacteria showed very extreme kinds of interaction.

I was puzzled by this difference and talked about it with Josh [Joshua Lederberg] and we came up with the answer. I think it probably came from him, I don't remember for sure, but most things did come from him, so it's a good chance this did, too. Anyhow, here's the idea, that if you're selecting for drug resistance in bacteria, you have a mutant that produces slight resistance, and then you get a second mutant that carries it on. Well, the second mutant is quite likely to be a modifier of the first and work only in the presence of the first. Then the third on the second, and so on, so this accumulates and you end up with a sequence of highly interdependent processes.

On the other hand, in *Drosophila*, you're working with a big sexually reproducing population -- I forgot to say, the bacteria were non-sexual. In a sexual population, the genes are scrambled every generation, so the genes that persist are the ones that mix the best with all sorts of other genes, so they're essentially additive in nature. This is the part of my study that attracted the least attention, but intellectually, to me this was the most interesting part about it.

I also started out then with two graduate students, both of which turned out to be failures. The first one was – I had three, excuse me. One went through. He worked on the embryology of *Drosophila*. He was a Nisei,[72] my first encounter with that. His name, if you want to know is [Frank] Seto.[73] He was a straightforward embryologist – he eventually taught at the University of Oklahoma, and I think still does. Probably retired by now.

The next two students were failures, and they're interesting. One of them turned out – he'd already been to dental school. He was a very nice guy, I enjoyed him very much. He was smart, but he was scared of the exams. When it came time for the Masters exam -- he didn't show up for the exam. Then this became a pattern, and finally he dropped out.

My other student at that same time, he did an experiment in insecticide resistance. It turned out he falsified some data. I won't tell you the whole story how I discovered it, but that turned out to be the case. But it does have an amusing end. As soon as this was discovered – it turned out he was just sort of a – I guess I'd say pathological liar, without being very much emotionally involved. Because when I confronted him, I thought there would be an explosion, and there wasn't. He just quickly admitted it and dropped out of school and went to work in Madison Avenue, and he's had quite a success as an advertising writer, which fits. (chuckles) I was pretty discouraged at that time.

AM: How are you interpreting this as [reflecting on] your skills as an educator and as a mentor?

JC: Well, I was beginning to doubt myself as a mentor, I must say. I certainly thought graduate school was a disillusioning experience. Then everything became wonderful, because Newton [E.] Morton appeared about that time.[74] The reason he came here was that one of my classmates at Texas taught at the University of Hawaii, and he wrote a letter and said he had a student that was the smartest student he'd ever had, and also the most obstreperous student he'd ever had. He thought I was equal to the task of dealing with someone like that.

AM: (laughs) Why would he say that?

JC: (laughs) I don't know why. Anyhow, he thought that I could handle it. It seemed like a good challenge to me. Actually, it was nothing. He was highly pleasant the whole time. And he really was very close to a genius. Then we did a rather nice problem on random drift in *Drosophila*, an experimental problem, and eventually wrote an article about it, a couple of articles. But what he really wanted to do was human genetics. I had some interest in that direction too, at least enough to encourage him. I thought just the right thing for him to do would be to go to Japan. He had a Japanese wife, furthermore. The right thing to do was to go to Japan and work for Jim Neel in this Hiroshima-Nagasaki project. So he did. I arranged for him to get an appointment there, so he went over to work with Neel.

It turned out he didn't stay very long, that he and Neel just didn't get along. Nobody told me this, but I knew perfectly well what happened. Neel had these plans for the next twenty years, and Morton came in with fifty new ideas that would have been improvements, but you don't like to change the protocol on something that's already going. So it turned out he and Neel didn't get along.

In the course of his being in Japan, he sent me some reprints from a man named [Motoo] Kimura, that I thought was an older person, and probably a professional mathematician.[75] He told me this seemed like awfully good work, and I certainly agreed. It was just astonishing what this was, and it was considerably better than anything Wright had been able to do. But I didn't think much more about it. I read these papers and couldn't fully understand them, but I could understand the significance, and didn't give much more thought about this.

Then Morton came back, and he started work in theoretical aspects of human genetics. I shouldn't say theoretical. These were really the mathematical trickery for doing experiments in human genetics, for doing what substituted for experiments in human genetics. Among other things, he invented what's now called the LOD [logarithm of the odds] score.[76] Is that part of your vocabulary? For studying linkage. Well, there was no real chance to study linkage at that time. There just weren't enough genes. So this whole thing lay in abeyance for twenty, thirty, forty -- forty years, at least. But now that we have all these markers, everybody uses LOD scores, and nobody knows where they came from, but they originated right here in Madison. Then he did quite a number of other studies. After he got his degree, he stayed on as a postdoc and then as a faculty member. This would have been '52 or so when he finished, maybe '53.

The Genetics Society met in Madison that year, and I was walking through the Student Union, which is where the meetings were held, and there was a Japanese man there who was clearly lost. I stopped and asked him if I could help him, and I found his place for him. He told me his name was Kimura. I asked, "By any chance is he the Kimura that wrote these papers that I had read?" And he was. I think I was one of a small handful of people in the United States that ever would have heard of him. He'd brought along a paper that he wanted to have published. At that time I was assistant editor of *Genetics*, so I took his paper, and I realized it was something good and sent it to Sewall Wright to review, and he said it's outstanding. He very rarely said that, so I realized we really had something wonderful here.

He was on his way to Iowa State [University] at Ames. He had applied to study with Wright, and Wright told him he was retiring and would probably be moving. I think Wright had in the back of his mind moving to Wisconsin. That was being negotiated at the time. But he didn't say that to Kimura. He said instead he should go study at Iowa State, where a man named [Jay Laurence] Lush,[77] who was sort of a Wright disciple, was a teacher there. There was also a strong statistics program there.

So Kimura was on his way to Iowa State, or had just arrived at Iowa State, when I met him here in Madison. We hit it off immediately, and I very much enjoyed the little bit of discourse that I had with him. Part of what I did was to translate his paper into idiomatic English, for one thing. He went on off to Iowa, and then about Christmastime wrote and said could he come here and work. It turned out that he wasn't satisfied with the direction of research there. He wanted to work in stochastic processes; he did not want to work with analysis of variance. So he came here, and Wright came the same year. It was a great year for Wisconsin. So I had simultaneously Wright as a colleague and Kimura and Morton as students.

AM: How did the collaboration begin between Morton, you, and Muller, who at this time was at Indiana [University]?

JC: When Morton came back, he had an idea about using consanguinity,[78] matings, to get some estimate about hidden genes. I had a rather similar idea, and we sort of worked this out together. Then I went to a meeting of what was then called the BEAR committee, Biological Effects of Atomic Radiation,[79] to talk to Muller about this. He'd had the same idea. So Muller said, "Well, let's just the three of us write a paper, and then we can help Morton by making him the first author of this paper and give him a start." I agreed to do this, and I came back and I didn't like Muller's writing style. I thought this is someplace where I'll try to take charge. So I came home as fast as I could and wrote this paper. I had some help from Morton, but I essentially wrote it, then mailed it to Muller and said to make whatever changes he wanted. Well, he was busy, so he made very few changes. So essentially that paper was written by me, and I'm glad of that because I think it was more readable than most of what Muller would have written. It's a paper I'm very proud of, actually. The first use of inbreeding theory to try to estimate what would cause genetic loads and indirect estimation of the mutation rate.[80]

AM: What were the expectations that you would – this was theoretical human genetics was the label on it, but you would actually be able to apply any kind of human genetic data to these theories?

JC: Quite a number of other people made similar measurements from the effects of consanguineous marriages, cousin marriages mainly. The results were rather disappointing because they were just all over the map as far as the numbers were concerned. I think it's fair to say, although this is an awfully nice theory on paper, it hasn't really been that useful in practice. I think – well, human beings do not behave in a very systematic way, and I guess that's the problem here.

We were measuring essentially lethal effects, and what's lethal in Japan – or rural France where most of our data come from – is not going to be the same as it is in Brazil or in some other country. What's lethal in a poor environment is [sometimes] not even an impediment in a good environment. So for all these reasons, it hasn't been very useful. I still make use of it, and other people do. The theory's still good, and it's been applied more and more to natural populations, wild populations of endangered species and things like that.

AM: While you were doing this, what was going on in your lab? The fly lab experimental work.

JC: We were starting to study – by this time I'd stopped the DDT work, and we were doing experiments in *Drosophila* that somewhat paralleled these studies in the human. But in *Drosophila*, you can make chromosomes homozygous by using markers and crossover suppressors. So the first of these papers was written by Rayla Greenberg [Temin].[81] In that case, we worked out the distinction between whether the inbreeding effect is a few genes with a large effect or a lot of genes with small effect. We were able to make that distinction and divide it up into those groups.

This has an interesting anecdote associated with it. This paper became rather popular in some circles, and it was used as essentially a text reading in a course in genetics in California, at [University of California] Davis, [University of California] Berkeley. Just last year, a former student from Davis came here, and the first question he asked is, "Whatever happened to Greenberg?" What happened to Greenberg is she married Howard Temin and changed her name, so Greenberg disappeared, as far as the literature is concerned.

AM: When did Temin get here? It was after you arrived.

JC: Yes, quite a bit after, and then he married this student of mine. Maybe around 1960 somewhere.

AM: So genetics was, at this point, in zoology within the school?

JC: Within the College of Agriculture. The genetics department was in the College of Agriculture, and was all this time.

AM: How many geneticists were there across the campus?

JC: In the department, there were probably fifteen or so people, [perhaps] twenty. Not all of those in one building. Then in 1963, we moved across the street into one of these buildings back [on Henry Mall]. It's now called the Old Genetics Building. Much of the work that I'm now talking about was done there.

Now we're getting up into '58, '59, '60, along in there. About that time is when Josh Lederberg won the Nobel Prize. I believe it was '58. There was a change of deans in the medical school at about that time, and the incoming dean, whose name was [John Z.] Bowers, was interested in genetics. He'd been associated with the Hiroshima project in Japan.[82] He and Josh knew each other before he ever came here, so it was very natural that he and Josh got together and decided we should really have medical genetics.

AM: To this point, there was no genetics being taught to medical students?

JC: Very little. Actually, I did a little teaching. I gave maybe four or five lectures to the medical students as part of a preventive medicine course. But it was just a few lectures in a course. Anyhow, Bowers and Lederberg together decided they really should have a human genetics course and that they should just start a Department of Genetics in the medical school. So they did. The department consisted of two people, Joshua Lederberg and Newton Morton.

I didn't join it for political reasons. When I discussed it with Josh, we decided [that] if both of us sort of moved out of the Agriculture Genetics department, it just wouldn't look good, or wouldn't be a good thing to do.

AM: To the College?

JC: To the College, yeah. That didn't change anybody's relationship, but organizationally then this medical school department had just two members in it. Eventually, Morton left. In the other order, however, but first Lederberg left to go to Stanford [University]. When he left, he was hardly on his plane before the dean called me up and said why didn't I come in and replace Lederberg as chairman of that department? So I did, while still retaining my membership in the agriculture department.

Essentially, by just agreement among ourselves, we started behaving not as two departments but as one big department. And by coincidence, they always managed to each elect the same person as chairman. That was a contrived coincidence, I should say.

AM: Yes. So there was always – as chair of genetics, you had dual roles.

JC: Mm-hmm. So I did that for a few years. Later became acting dean of the medical school. During that period, my colleague, Klaus Patau, was the chairman.[83] But for quite a long period of time I served as the chairman of both departments. And it still operates that way.

AM: So is there a lot of synergy then between these two departments? I think that would be kind of unique.

JC: Yes it is, I think, unique. At first, we thought we might try to get just one department and call it one department, but that didn't fit very well with the hierarchy of the organization of the university and colleges. But this has worked very well. The individual faculty members know which department they're in, but the students don't have much idea. We're all together in one building, interspersed. It's been very successful.

AM: So physically they're also in the same place.

JC: Mm-hmm.

AM: So the classical geneticists that were in the zoology department --

JC: Well, there never were any in the zoology department. This was always in Agriculture. The only connection zoology had was that the course was listed in zoology. Actually, my appointment for half of the time was in zoology, but that's not where I spent my time.

AM: So the other geneticists who really didn't consider themselves much of human or medical genetics –

JC: Oh, this was all either just pure genetics or agriculture.

AM: Did they have any conflicts with creating this kind of dual program?

JC: Not anything serious. I know Dr. Brink felt as if maybe we were subtracting from his department by adding over there, but once we had this agreement to all work together, it's been amicable all along. As in any group, different kind of schisms arise, but it's never been because of the organization. I think it's one of the more successful experiments.

AM: This is jumping forward a little bit, but maybe it's a good place to stop for the day. How well do you think, with this kind of esprit de corps that came with creating these two departments – How successfully do you think you have maintained that?

JC: I think it's been very successful. I think we have a good genetics program here. Sometimes we now call it the genetics laboratory, to encompass these two departments. So it has come to be the name over the last few years. Almost everybody now is in this building or the adjacent building. For a while, we were a little bit more scattered, especially in the early days when I first came here. There wasn't room enough in that building for everyone. Some of the people had their offices in the animal husbandry building or some other place like that.

Then we had one period in which – it was partly personal differences, but mainly it was just the intellectual differences. The department became more and more basic, and that meant that the people who were in animal breeding and plant breeding felt less and less at home. So the animal breeders pulled out and joined the animal husbandry department. It was an amicable divorce.

AM: And the plant geneticists?

JC: Some convenient retirements (chuckles). That was the problem. Although we still have plant genetics going on, the emphasis is pretty basic for the most part. The one person who was doing rather practical – very clever, actually – potato genetics, had a joint appointment. In fact, Wisconsin has a congregation of joint appointments so people can have two different homes. It has both pluses and minuses, but the plus is that you have two avenues for research funds and for all the other goodies that a university has to offer.

AM: And the minuses?

JC: Two sets of meetings. That was at least for me. I finally learned not to go to every meeting, but it did – not quite double, but essentially increased the administrative job.

AM: So the university – it's not unusual to have dual appointments?

JC: No, it's not unusual. They're all over the campus.

AM: OK. Well, I think we're at a good place to stop today, and we'll pick up tomorrow. Thanks.

Session III - June 3, 2005

AM: It is June 3rd, 2005. I'm Andrea Maestrejuan with James Crow in his office at the University of Wisconsin to conduct the last session of his interview for the UCLA Human Genetics Oral History Project. I thought I'd start off kind of where we ended up yesterday, and that was talking about the combination – creating a medical genetics department which was actually kind of spatially the same place as the basic science department of genetics. To go back a little bit before that, how many medical students were interested in taking courses in genetics from the basic science part of the campus?

JC: Not very many. The little segment of genetics that I taught for half a dozen lectures or so, all the medical students took that. There were occasional MD-PhD students, but not very many in those days. Most pre-medical students were headed straight for medical school.

AM: Besides Newton Morton, how many graduate students here were interested in doing more human genetics?

JC: I believe of my graduate students, he's almost the only one. More recently, I've had a couple of postdocs who were interested in human genetics, most recently a man named [Taisei] Nomura, who just retired in Japan this year. He was in radiation effects. But most of my students have been in pure genetics, sometimes mathematical and sometimes experimental.

AM: So where was the emphasis coming from to create a separate department for medical genetics at the medical school?

JC: I think it came jointly. Joshua Lederberg was always interested in human genetics, and he thought that that ought to be a part of the medical school. He also thought that medical genetics ought to include not only human genetics but the genetics of the human parasites, too, so he thought bacterial genetics, which was his specialty, properly belonged in such a department. I don't know whether he convinced the dean or the dean convinced him, or whether this was an interaction. In any case, the then new-coming dean was very eager to have a genetics department in the medical school. So it was formulated fairly soon after John Bowers, the dean, came here.

AM: In your own mind, how were you defining the two separate entities of medical genetics, human genetics, within this department that had other –

JC: The new medical department, yes. When I became chairman of it, which I did when Lederberg left, the idea I had – and the department consisted of four people – Newton Morton, myself, Klaus Patau, and Oliver Smithies were in it.[84] Our view then was that we would be a basic science department, and we would try to get geneticists into the various clinical departments, who could have joint appointments possibly, but we wouldn't, ourselves, do much clinical genetics.

That was only partially successful. We did have one such person, John [M.] Opitz,[85] who was in the pediatrics department, and he was active both in pediatrics and genetics. But the idea of getting a geneticist in each of the other clinical departments just didn't pan out for quite a while, so that part of it was a failure. Meanwhile, the basic genetics thrived.

AM: Have you been able to generate any interest in medical students to learn more genetics? Or was that forced upon them by the curriculum?

JC: I haven't had much to do with it, but there are more and more medical students who will do research projects, and a fair share of those are in genetics. We now have – considerably later now – we do have a genetic counseling service, which operates mainly through the pediatrics department. But these people all have an affiliation with genetics.

AM: After your experience in Guatemala and Costa Rica during the forties when you were kind of recruited, so to speak, to teach more clinically oriented genetics, have you had much experience after that with clinical genetics?

JC: Very little. For a while, I did a little bit of genetic counseling long ago, but I soon discovered that the problems were not technical genetics as much as they were problems in the disease itself, or in medicine, and I decided I'm not the right person to do this. All our genetic counseling now is done by people with MDs, [or a specific interest in genetic diseases,] and these are people who understand the disease that they're counseling about.

AM: Does Wisconsin have a program for the Masters?

JC: Yes, it does. It's quite a popular program.

AM: Where did you get the training to become a genetic counselor?

JC: I never had any. I just used my knowledge of genetics. I don't think I offered any counseling, except where it was a disease whose genetic pattern was already well known and didn't demand any thought on my part other than just routine analysis.

AM: This is characteristic of so many of your cohorts, that they became genetic counselors whether they were trained as PhDs or MDs, and basically learned by doing.

JC: I know that Curt Stern,[86] for example, did some genetic counseling, and he had no medical special knowledge. And there are probably a large number of such.

AM: Right. There seem to be two ways of doing it – directive and non-directive, take a more direct approach and tell the people what to do. Would you consider –

JC: I was extremely non-directive, and I wrote a little about it, some of which got published. Anyhow, it was mostly among ourselves around here. Our business was to provide information. At that time, it was possible to confuse counseling with eugenics.[87] Although I had some eugenics sympathies, [they were] rather weak ones. But I did take a strong view that if we were counseling, our business was this particular family, and the overall interests of society came second.

VI. The BEAR (Biological Effects of Atomic Radiation) Committee; Working in an Administrative Role

AM: OK. A couple of more questions on what we briefly mentioned yesterday, and then we can move on to talk more about your role as a geneticist administrator as much as a geneticist researcher, or however we can describe that. One was, you briefly talked about your role with the BEAR, the Biological Effects of Atomic Radiation. Again, this seems to be a defining factor in many of your generation of geneticists in that they had some affiliation with commissions by the Atomic Energy Committee[88] and Neel's work. Your student went to work with Neel. And your own role and [Hermann] Muller's role. We certainly hear about atomic energy and nuclear energy from the physicist's point of view, but what was the impact on these new roles for geneticists in atomic radiation testing and the effects of it on the discipline?

JC: Well, my part of this sort of started with the committee that's called the BEAR committee, Biological Effects of Atomic Radiation. The instigating thing that caused that committee to be formed were the arguments over nuclear testing, above ground testing, which generated a lot of fallout, not very much in any one spot but worldwide it amounted to considerable radiation. I don't know who instigated getting a report done, but in any case, the National Academy of Sciences (which had created BEAR) decided to do such a report, so it included other aspects than just genetics.[89]

There was a genetics committee, the chairman of which was Warren Weaver, a mathematician, and it was a very good choice.[90] He was a good diplomat and a very quick study, so he understood things very rapidly. The committee itself consisted of almost a Who's Who in genetics in those days. It had Sonneborn, who dropped out after a while, and Muller and [Sewall] Wright, [Theodosius] Dobzhansky, [James] Neel, four or five others, [William L.] Russell.[91]

I wasn't immediately on the committee, although I did come to the first meeting. I think that Muller said that they needed more population geneticists and recommended me. So that's how I got on the committee. I played a useful role in this committee because rather soon in the deliberations there was a real impasse, or difference of opinion – a real impasse, I guess I should say, between Muller, who had this principle of genetic load that I told you about last time, and he wanted to use that in a quantitative way to assess radiation risks.[92] Wright thought that the subject was considerably more complicated than that and that it was misleading to count genetic deaths. So the two of them really had quite a strong argument. I don't want to say heated, they were gentle about it, but it was very firm in both cases. It came close to splitting the committee, and I did play a role in this because I understood both of them, and there weren't many of the committee members who really understood Wright's viewpoint on this very well, and also Muller's, because I was closely in touch with both of them. So I played a little role of coaching Warren Weaver. He would call me up by telephone, and we had a few sessions, with questions.

At the very end, when the committee was about ready to have a report, he sent me a letter – it would be nice if I could get a copy of it – he said that he'd just heard from Wright that he wouldn't sign this report unless some of the words were changed so they were less like Muller's urgings. He wrote to me and asked me if I wouldn't please persuade Wright to weaken on this point. I told him I couldn't persuade Wright but I would talk to him. I think Weaver -- I didn't do it. I think Weaver himself worked out some compromise wording that both of them would finally accept, in both cases reluctantly. The interesting point, really, is that this had nothing to do with the substance of the report because the main viewpoint of this committee, which everybody *did* agree on, is that we should try to keep manmade radiation at levels comparable to natural radiation levels and use that as the guideline. That part was uncontroversial.

My admiration for Warren Weaver, incidentally, was great. He was so quick to understand all this. He talked an awful lot maybe, but he had sensible things to say. Partly on the basis of this friendship with Weaver that I developed, I finally – after this committee report had been finally set in, I had the bright idea I would like to go to Japan and work with [Motoo] Kimura, so I just wrote a personal letter to Warren Weaver and said would he consider just supporting me for a summer in Japan to write a book on population genetics. And he did.

Actually, serving on that committee probably had a lot to do with establishing my reputation because one of the consequences of serving on the committee was being invited two or three times to testify before the Congress, and that got some newspaper attention. It wasn't too long after that that I got elected to the National Academy [of Sciences of the United States of America]. I think an objective view is that, on the basis of my research, I wouldn't have done it alone, and I think it had to do with this public persona that I had developed by that time, and name recognition.

AM: Would that characterize your other colleagues at that time, that profile?

JC: Perhaps. I was the only one who was not already known. There was quite a bit of publicity about – Well, at the time of this congressional hearing, [Alfred H.] Sturtevant[93] and Muller also testified. But these people were already well known.

AM: Well, Bentley Glass –

JC: And Bentley Glass did, too. [pause]

I think I was probably the only one who began to get name recognition who didn't already have it, as a result of the BEAR committee. Bill Russell, who did these big mouse experiments, was already known. He didn't have so much to say in the committee, however. Most of the people who did the talking in this committee were Wright and Muller. Dobzhansky was one on Wright's side, but the argument was a little too mathematical for Dobzhansky to take a part in it.

AM: Austin, when Muller came, he brought with him all his radiation experiments and the effects of radiation on mutagenesis to get all his mutants. When you were there, was there much work going on with X-rays?

JC: Yes, we did.

AM: And was there a lot of talk about the effects? If this is causing some of the mutations in *Drosophila*, what is it doing to the technicians and the lab workers?

JC: Well, it was taken for granted that there would be harmful effects. Nobody tried to quantify it. But we were all careful. There was an old X ray machine that must have scattered radiation in directions, but it had a lead shield around it, so you could go inside and turn on the machine and then go out. Actually, the various people in the room mostly left while we were doing radiation. I don't have any idea what the level of radiation was outside [the protected area], but people were careful about it, as far as their own health was concerned. There wasn't much public discussion of it. I think it didn't happen until nuclear testing became an issue. Muller had been preaching this all along, but he didn't get a lot of attention, I think, until after the bomb testing began.

AM: This is going to seem like a tangent, but I wanted to bring back – because you mentioned it yesterday – you did a little research on DDT resistance. I know that Alan [P.] Poland was here at the cancer center and was working on chemical mutagenesis and dioxin.[94] I know that he's a bit of a controversial figure here, but I wanted to know, in the context of your own work, how much was this idea about environmental impact on mutagenesis in humans playing a role? Because for a while, that was a specialty here.

JC:	It was in the back of my mind, but it wasn't the reason for doing these studies. The reason for doing the studies -- my personal interest in it was more or less basic. I thought this was the way to study evolution in the laboratory. But of course, the practical part was that the insects were developing resistance, and I certainly showed that that would happen in a rather short time.

The kind of experiments I did, I had the flies growing in cages, and I just scattered DDT in the cages, intentionally in a careless manner because I thought I'd mimic nature better this way. This was not systematically done at all. Whenever the fly population built up, I'd add more DDT. After not very long, actually, a year maybe, these flies would just walk on DDT, and it didn't bother them in the least.

A sidelight of this is personal. Nobody at that time thought of DDT as having any effect on humans, or any warm-blooded animals, so I was very careless about DDT. I didn't pay any attention to whether I was eating it and breathing it or not. I noticed that flies, if I treated them with DDT, not quite enough to kill them, and then starved them afterwards, then they died after they were starved. I could starve them quite a bit later. And I deduced, and almost certainly correctly, that what was happening is the DDT was stored in the fat, and then when they were starved, they started using this fat and poisoned themselves from that point. I've used this as an excuse to myself never to go on a diet. (chuckles)

AM:	Were you aware of Poland's work?

JC:	No. I know the name, but I'm not aware of his work.

AM:	Also, I wanted to just maybe pursue this a little bit more if we could. Your first experience with graduate students was less than stellar, but then you had some very good graduate students, spectacular graduate students. Then you had a whole tradition of bringing in Japanese graduate students. What would you account for your ability to bring in these stellar graduate students, and also the attraction of Japanese students to your lab?

JC:	Well, the way the Japanese students worked is easily described. When Kimura got his degree, he was very happy here, so he said that he'd find a successor, so he did. Of course, he would find a good one, so his successor was a man named [Yuichiro] Hiraizumi,[95] and then *he* found a successor. Although this is not a beautifully unbroken chain, it's roughly that. So I had eight graduate students, most of which were chosen by their predecessor.

As far as English and European students, mostly American, I think having Morton and Kimura as students really started me off with a reputation. I didn't do anything particular as a lab director. If there was an experimental project, usually it was something I thought of, but I didn't pay much attention to the hands-on day-to-day business. A good many of my – well, I'm now thinking more of postdocs, but a good many of my postdocs were really self-generators, and they could have been somewhere else perfectly well as far as the work being done. We talked every day and I was a good listener, but I think it's fair to say that some of my very best students really didn't need me, they generated themselves.

I had a policy – if it doesn't sound like excessive false modesty. I tried to pick students that I thought were smarter than I am, for the reason that if you don't do that, the field isn't getting any better. So I really have tried, when I had an opportunity, to pick students who were bright, if I could judge that, and you can to some extent. I have had good students. All along I think I've had the reputation, when I was actively teaching, of having the best students. That sort of is a self-perpetuating system.

AM:	Yeah. Now, would you say there's something to Japanese genetics that tends to develop good population geneticists, or theoretical population geneticists? Is there a style of Japanese genetics?

JC: I don't think there's a style particularly, but Japanese are better trained in mathematics than American students are. And of course, the ones that came here to work with me tended to have that kind of an interest.

AM: You mentioned this yesterday, and you just told another story in which you served as a mediator between two very –

JC: Contentious? (chuckles)

AM: Contentious individuals and how you'd been called in to mediate, or translate for other people. Why don't we talk a little bit about your role as administrator? Was this something that you – because you were dean of the medical school, you were chair of the department here, and you've been vice president of GSA [Genetics Society of America], president of GSA, and president of the ASHG [American Society of Human Genetics][96] – Is that something that you aspired to, or because of innate talents were kind of pushed into?

JC: I was pushed into it, I think. I've mentioned most of the conspicuous examples of mediation. I haven't done very many more than I've told you about. But my role in the medical school was clearly as a mediator. We had this very knock-down, drag-out fight over a dean, and I was on the committee elected by the faculty to represent the faculty. We had many meetings, and about that time the dean was fired. So the reason I was named acting dean, I think, was because of having served on that committee. But I did know the president. He knew I wouldn't want to do it, but he called me up and said, "This is an order." With tongue in cheek, of course. Anyhow, I agreed. He essentially promised me that the job would last only six months, so quite reluctantly I accepted this, not only reluctantly but with apprehension. I wasn't sure I could do this job. And it lasted two and a half years, not just six months.

Much of the job really was mending fences, because the school was really divided deeply into two camps. I tried to get along with both of them and bring them together, and to a pretty large extent, succeeded. Things were better when I left than when I started, although it took a few more years for all the wounds to get healed. So I would say my main contribution to the medical school was really nothing very innovative; it was just simply peacemaker. I wanted to get out of the job. But I can't say that I hated it. I sort of found it stimulating.

I did carry on the laboratory program. I had Rayla Greenberg, by then named Temin, and she was in charge of the laboratory. And I had three or four graduate students doing problems. These were population genetics experiments that had been planned to last a year or so, so they didn't need day-to-day attention. I think an outside observer didn't know my research program slowed up, but I did. And the ideas certainly slowed down.

The way I planned my day was to spend the morning in the dean's office and the afternoon in the lab. Most of the days I was successful in dividing the time this way, but it didn't really work because I'd go to the lab in the afternoon and find myself thinking about these issues that arose in the dean's office.

I have one other thing to say, and I may have told you before. Anyhow, I was apprehensive over the fact that I don't have an MD. I thought that would be a major problem, but it was a non-problem. I stayed out of clinical questions, but they don't come to the dean's office anyhow. The kind of things that came to me were salaries and parking and disputes over space. We were chronically short of space at that time, and that was a major issue always. When the time came up to – when they finally found a successor, I was pretty strongly involved emotionally, and although I was glad to stop, it wasn't totally easy.

AM: So this new department had been created, and now this basic person is coming in to be dean of the medical school. Was there any resentment by other medical specialties? Because at this same time, there's the movement toward creating a Medical Board for medical genetics. Was there any tension --

JC: There may have been. I wasn't particularly aware of it. I made one appointment that was disagreed with by a medical group. I decided, with some advice of course, that the direction of laboratory medicine was going away from looking in a microscope, traditional pathology, and more toward using chemistry. So I appointed a lab director with an M.D. all right, but his prime motives were chemistry. He wasn't even competent in classical pathology. So I got a strong letter from the Society of Pathologists, which I answered in much the way that I told you now, that I thought this indicated the way the subject was going.

I can't even think of any other controversy with any medical group that I had. I don't doubt for a moment that there was considerable gossip behind the scenes, that I was ignorant of medicine and what was I doing being a dean. But nobody told me this.

It's another anecdote, but I suppose I'll mention this. There was a woman named Helen Dickey who taught medicine, a very sharp-tongued woman. She was telling her class in clinical medicine, "That's not really hard to understand. Even Dr. Crow could understand this." So when it was my turn teaching my class, I had a genetics problem, and I told the class, "Even Dr. Dickey can understand this one." (chuckles) Between her and me this was friendly [banter] all along.

AM: Well, you've kind of seen the field evolve from the creation of GSA, although you must have been a very young person when the GSA was formed, and then you saw Muller --

JC: Actually, [the journal *Genetics* was founded in 1916,] the very year I was born. [The GSA, Genetics Society of America, was founded in 1931.] The Human Genetics Society [American Society of Human Genetics] was born within my consciousness [in 1948], yes.

AM: The American Genetics Association[97] –

JC: That's a different society. That publishes the *Journal of Heredity*, which goes back a little bit earlier.

AM: OK. How, as a geneticist, did you see – you're president of both organizations, how did you see the distinction between these two professional genetics groups?

JC: I don't think there was a very big distinction. The Human Genetics Society – there was clinical genetics, of course, probably somewhat, but there was also considerable interest in basic genetics using human subjects, and to some extent there were people in the Human Genetics Society who were doing *Drosophila* genetics too.

The way this got started in the first place is that, for a long time Muller, who was very influential on the whole field, and on me in particular, he said that his ultimate interest was human genetics, but he thought that the subject was so poorly established that this was not a time to do much in human genetics. You couldn't do much. So he continued to work with *Drosophila*. Then in whatever year the Human Genetics Society [ASHG] was founded, I believe it was '48, he finally decided – and many other people along with him at the same time, he wasn't the only one – decided really the time was right to treat human genetics as a separate subject.

Part of the reason for making it a separate subject is it was beginning to take up a lot of room in the journal *Genetics,* which was having a crowding problem anyhow. So what might have been a disagreement really wasn't. It was good riddance, in a sense. And I'd say the Society for the Study of Evolution[98] grew out of the Genetics Society in much the same way. So I think both of these were amicable separations.

AM: What was your own personal identification with the ASHG and the GSA? You become obviously active members in both.

JC: I became a member [of the ASHG] from the very beginning and did attend the meetings. I was very interested in this famous paper by Muller called "Our Load of Mutations."[99] I already knew Muller by this time, as I've told you previously. This paper that you mentioned last time, the Morton, Crow, and Muller, it may be my first serious venture into human genetics.[100] It's never been first on my list of interests. It's been something I've always had in the back of my mind, and if there's an opportunity do an interesting problem in humans, I'll do it. But I have never been wanting to study humans just because they're human.

VII. Implications of Science and Research; The Status of Genetics

AM: What drives your science then?

JC: I'd say curiosity. And maybe even more important is opportunism. The problems sort of come up. I thought last night a little about this, and if I really were writing my history, there isn't any coherent pattern to it. It's essentially something interesting comes up and I work on it, and something else follows up.

One of the things I did in human genetics, which I found interesting, it's been useful, the idea of using names as a way of studying heredity. As Muller put it, the person's name is linked to the Y chromosome so we can trace what amounts to Y chromosome ancestry by just following names. I did have a student, whose name is [Arthur P.] Mange, who was interested in human genetics at that time.[101] I forgot him when I was talking a while ago. He studied the Hutterites, which are an isolated religious group,[102] and this method worked very well for that. You could measure the inbreeding coefficient and the various ramifications thereof by just going to a phonebook, or public records in this case. I remember thinking to myself at the time that if we ever have good markers on the Y chromosome, this method will become obsolete. And that's happened. Of course, it's more expensive to study Y chromosomes than it is to just go to public records, so maybe it'll stay for that reason

AM: Before we leave the sixties, which is when you become president of GSA and ASHG, let's talk a little bit – because you mentioned it briefly off camera – about the sixties here in Madison, which was as turbulent as the sixties pretty much on many college campuses, and your role.

JC: Well, it was a turbulent period. The student unrest, and the Dow Chemical Company was here [recruiting].[103] It was a focal point for antagonism. I smelled tear gas for the first time during this period. Just after I got through being dean, which would be in 1965, not more than a couple of months after that, I was just settling into normal life again and was asked to chair this committee dealing with student unrest, which I thought about and finally agreed to do, against my better judgment. But that turned out to be an interesting committee, and it was influential. We made a number of recommendations about the organization of student affairs on the campus, but the main thing was this committee essentially took the university out of the business of being parents of the children and left the personal lives of the students pretty much in their own hands. We said very directly that the university's business is teaching and not regulating personal lives. That had a big influence; it changed the whole university pattern at that time. The report came out – it must have started in '65 and took us two or three years to do it, so in the late sixties. I was just reviewing it last night and had forgotten most of what we said.

AM: Considering you were a bit of an activist yourself as a college student in Kansas, how did you identify with what was going on with the students here?

JC: I was sympathetic with their cause all along. I sort of had the view – and I think this is a very popular view among faculty members – that these kids may be a bit rambunctious but their heart's in the right place, an issue that I'm personally interested in. Then we had this bombing in the [Army] Math Research Center here that killed a person, and that just changed the scene. Then these people were no longer innocent merrymakers. Not merrymakers, zealots. They were doing something dangerous then. I think that one event, of course, changed a lot here on the campus. I wasn't on the campus at the time. I was in Japan when that event happened. But even the Japanese newspapers covered it, so I was aware of it.

AM: Were your own children at that point college age? Were they here?

JC: They weren't involved here. My youngest daughter, when she was in Oberlin [College in Ohio], went to a sit-in that just had to do with anti-segregation laws in the South. She was involved in that and went to Washington and sat in. My other children were in this direction in their views, but neither of them, as far as I knew at least, was much of an activist.

AM: A little bit of a tangent here. You remind me that part of what happened at Madison was sparked by Dow Chemical recruiting on campus. When you enter the building here, this new building, it's the genetics and biotechnology building. We don't seem to think too much anymore about the melding between university research and private research done by corporations. As a geneticist, you certainly have seen this transformation of genetics.

JC: I certainly have. In the past, I thought of academic research and private companies as just totally distinct with no communication one from the other, except the companies made use of basic research. When some of the departments, Harvard [University] among others, some of the faculty members started forming their own corporations, I wasn't at all sure that was a good thing. I'm still not sure. But it's of course so commonplace now, there's no changing. I guess, in general, it's been a good thing.

The controversy here that affects people in this department really is genetically engineered foods, and of course stem cell research and cloning.

AM: Right. And you mentioned yesterday that there's been a problem, that there are threats of violence against the stem cell research.

JC: Yeah.

AM: Has the university taken any active position on this?

JC: It's defended doing stem cell research. I don't know that the university as such has taken a position on genetically engineered foods, but there are a lot of people who are doing it. I haven't seen very much by way of – it's not that controversial around the university, around the state. It's controversial nationally. It's especially controversial in Germany.

AM: How do you feel yourself personally about the ethical issues and the stance of geneticists to be involved in these discussions about genetically engineered food to DNA sequences?

JC: I guess I think the profession owes a responsibility to society to look into these questions. I don't want to say that every practicing geneticist has to be socially conscious. It's impractical to wish that anyhow, nor do I really think that's best. But I think there should be, and the Human Genetics Society, and especially the Clinical Genetics Society,[104] does give considerable thought to genetic counseling and the extent to which this should be directive. I haven't been involved in any of this, but the question of whether you should do tests on a person when there's no consequence to the outcome of the test. I think that makes sense.

Just guessing what's going to happen in the future, I think we're going to have to have something that's equivalent of rationing of medical care. Techniques get better and better, and inevitably they get more expensive as they get better. We ration it now, except we ration it by income. I would much rather see a more rational rationing system. I don't know that that's likely to come, but I think it's inevitable that not everybody will get all the medical care that he would prefer to have, or else we'll be bankrupt as a nation, which we seem to be going toward anyhow.

AM: In your own words in which you were describing your curiosity with taking advantage of these opportunities in defining your research program, and your curiosity followed these opportunities, to what extent were you thinking about the larger implications of the research that you were doing?

JC: Usually not at all, because most of the things I've done don't have an obvious long range implication, or societal implication. The DDT work had some practical applications, or could have, but really it just told us that insects are going to become resistant and you'd better expect that to happen. In that sense, I think it played some useful role, but a lot of other people were doing the same thing at the time.

AM: OK. I might be jumping around a little bit as we start to wrap things up, and I wanted to ask about a couple of things that you said in your oral history interview for the Genetics Society of America project. One was that you mentioned that population geneticists in the United States really weren't interested for a long time in human genetics, and it was really others – particularly the English, [Lionel S.] Penrose, [J.B.S.] Haldane – who really picked it up. Why do you think that was the case in the United States that population geneticists did not move toward human genetics as soon?

JC: It's a good question, but I don't think I know the answer. It just may have reflected the personalities. Now, Sewall Wright, who was of course the leading American population geneticist, he didn't write anything about human genetics, except when it was purely incidental. He certainly didn't do it the way Haldane and [R. A.] Fisher and Penrose[105] did.

Aside from Wright – well, let me say it another way around. There were no human geneticists in the United States that were as distinguished as those three in England were. We had some human geneticists, and they were all right, but they weren't in the world class that Fisher, Haldane, and Penrose were. So this may just have been a coincidence that they were three of the very best, who happened to be interested in humans, [and they were in England].

Penrose especially. He in many ways got human genetics going in Britain, and had a big influence on the United States, too, because for a while, if you were going into medical genetics, part of the tour of duty was to spend some time with Penrose. As near as I can tell, the people that went over there didn't go there with any specific idea. They were just going to have tea with other people and with Penrose and pick up words of wisdom from the master. I've known two or three people, some of whom you've interviewed, who had this experience.

AM: Besides going to Japan, did you ever go to Europe to do any –

JC: Never did any work there. I've been to conferences there, but I've never done anything useful.

AM: We talked again a little bit off tape about this. Do you see national styles of science emerge? They may not persist, but could you say that within genetics there are these national differences in terms of what kind of research gets pushed, or what kind of approaches get taken up?

JC: Well, I think these are less than they were in the past. When genetics was just getting started in this country, the best genetics, the [Thomas H.] Morgan school, was the United States monopoly. And although there was good genetics being done in England, the really basic work on *Drosophila* and corn was done here. English people were doing good work, but it was the Morgan school that really developed it, along with the people working with maize. Germany was pretty far behind in that regard. On the other hand, Germany was ahead in the overall thinking and, in a generation still earlier, by producing [Hans] Spemann, [Richard B.] Goldschmidt,[106] [and] the rest of these, it was the leader.

I did sense there was a real difference, and I was aware of it, of style. The American genetics style was to be mechanistic and map genes and postpone until we knew more the question of how genes work and developmental genetics. And the Germans were thinking about this all along, even though they didn't get very far, I think. I don't think developmental genetics really got anywhere to speak of until molecular techniques came. Now, of course, it's magnificent.

AM: Just to follow up on that, because this emerged and I don't have the quote exactly, but there was a sense when you were being asked about directions and the future that there was a sense of kind of nostalgia about classical genetics, that it was being kind of pushed to the side, or I think you even used the words in danger of becoming extinct because of molecular genetics. I just wanted to talk a little bit about your feelings on classical genetics versus molecular genetics.

JC: Well, first about just at Wisconsin. I was chairman for quite a while and had some influence over the directions that we went, although we voted, of course. I wasn't a dictator. When molecular biology first became apparent, right after the [James D.] Watson and [Francis H.C.] Crick [DNA] model,[107] it was just clear to me that that was the direction genetics ought to go. So I pushed very hard for the next few appointments in molecular genetics and pretty near largely neglected to follow other aspects, including population genetics. But I think as a result of this that we quickly got a critical mass in molecular genetics, and the population genetics got neglected. It may have been a tactical mistake from population genetics' standpoint to push that direction.

As far as the national picture is concerned, I think when bacterial genetics came, with Lederberg and the virus genetics, the people at Cal Tech, that that was so much better than any other kind of genetics that people gravitated toward it. Not everybody worked in it, but everybody was conscious of it. Then we had the Operon model,[108] and we had all these beautiful things coming out of molecular genetics, of course the greatest of which was the Watson and Crick DNA model. So it seems inevitable to me that that would become the center of genetics. And of course population genetics was, to some extent, thrust side, but everything else was thrust aside, too, so I don't regard it as discriminatory against population genetics. It's just the growth of molecular technology.

I see, as a population geneticist, a resurgence of the field now, partly as a result of Kimura and the neutral theory [of molecular evolution],[109] based entirely on some molecular techniques and higher mathematics, that population genetics has changed from a subject in which we had this magnificent theory and no data worthy of it to a data-rich situation right now. And we need some more theory now. It's a role reversal, and that's been very interesting to me.

AM: OK. Again, I'll be jumping around. We've talked a little bit about your own ideas on evolution and natural selection. We were talking about, because you were friends or colleagues with both Wright and Fisher and they had, toward the latter part of their careers, contradictory views on selection. How did you mediate between these two, because you clearly were friends were both and spoke to both?

JC: Right. Well, I didn't really mediate between them. I actually played games, you might say. When Fisher came here to visit, I would ask him questions. The questions often had originated with Wright, but I didn't say so, and get an answer from Fisher. Then a few days later I'd talk to Wright and tell him I'd had this thought. Of course, the thought came from Fisher. So I played that game quite a bit. In my day-to-day contact with Wright – of course, I saw him every day, or virtually so – I came to admire the man very much, but I have to say in my personal view, I think my evolutionary views are closer to Fisher's.

I think Wright's shifting balance theory is an exceedingly clever idea,[110] and it's been very, very popular with biologists, but it's been less popular with mathematicians. I think the reason it's popular with biologists is that it's appealing. Biologists like to think of things as being complex. I think it's a disease in biologists to look for complexity. And mathematicians look for simplicity.

Biologists didn't realize as well as the mathematicians did – I guess I'm including other population geneticists as mathematicians in this regard – that the Wright theory ought to work, but it requires a concatenation of several improbable events. The population structure has to be about right, and the mutation rate, and the selection has to be within rather narrow limits.

Then there's the question about whether the peaks and valleys metaphor that Wright talks about, how real that is. One thing Fisher said that appealed to me, he said that Wright's peaks and valleys, they ought to be replaced by – not a rugged landscape, but an ocean with troughs and valleys and crests. In other words, it's changing all the time, and what's a peak now may not be a peak tomorrow, and that therefore –

I'll back up a bit. The reason for having this shifting balance theory is that Wright was bothered by the fact – as many biologists had been – with the fact that you might have a combination of genes that work well together, and there might be another combination of genes that's still better, but the intermediate stages are worse, and there's no way for natural selection to go through a bad stage to get to something better. And Wright's model is a way of getting around that. It requires some improbable circumstances, but it does provide that.

What Fisher says in contrast is that, if you ask the question, can I go from here to here, the second here being a specific place, that's probably quite correct, but that's not what natural selection is trying to do. Natural selection is simply trying to get better in any way it can. Fisher says that if you just say here's a population at a present state, that it can always do something that improves it. It may not be what you have in mind. And that appeals to me.

I never did question Wright when I was talking to him personally, and we often talked about his theory in detail. I did write one paper with a couple of colleagues that has been taken as supportive of Wright, and it is. It says that one criticism of Wright is probably wrong. But on the whole, I think probably the totality is in Fisher's favor.

I must say I feel enormously privileged having been very close to both Wright and Fisher, and Muller. Very few people have had that good luck.

VIII. Progress in the Evolution Debate; Eugenics; Miscellaneous

AM: Well, to just briefly discuss – It's a clear theme in a lot of your writing, the writing you've done on the history of genetics, and certainly we've been – you read about the year of the gene, or the decade of the gene, and now the century of the gene. The twentieth century is really about bringing Darwin and Mendel together and taking it much further, and that evolutionary theory has really progressed, or been better defined than ever. Yet, on the outside, outside of these walls, the debate against teaching evolution seems to just have gotten bigger. We have all kinds of states now either labeling their textbooks that this is a theory, insisting they create an intelligent design.[111] It's not just Louisiana, it's Kansas, it's Ohio. How do you feel as a geneticist, and as your role as educator, that so much strides have been made in understanding evolution and natural selection, and yet culturally, no progress has been made?

JC: Well, I am puzzled by this. I'm especially puzzled by literate, intelligent, often scientifically trained people who are into intelligent design – these people don't accept the first chapter of Genesis literally. They do think evolution occurred, but they think there was some sort of a supernatural force that was involved in it. The argument of so-called irreducible complexity[112] that the intelligent design people make such a to-do over, I think that's a non-issue. I can invent ways of getting complicated biochemical sequences by an orderly process, although I have to admit that I don't yet know how to make a flagellum by putting together component parts in an orderly way.

So perhaps that's the strongest arguing point that the intelligent design people have, just things seem to be too complicated and intermediate steps too likely to be disastrous for there to be an orderly progress from a non-eye to an eye. I think I just have to say that case after case we can analyze, and the glass is half full now. But many of my creationist friends – not that I have many, I have a couple, though – they would say the glass is half empty. I have one person I discuss this with fairly regularly, and he just takes the view that he just doesn't think natural selection is sufficient, it requires a more orderly process than natural selection can do. My response to him is always – and we know what each other's going to say by now – that each year we learn more and more, and it becomes more and more ordinary random mutation followed by selection.

I'm bothered by the intelligent designers who concentrate now on intracellular products. They say that the blood coagulating mechanism, or the flagellum, is too complicated. That to me is a very, very old argument. I'd say the elephant trunk is complicated, too, and a lot more complicated than the bacterial flagellum. So what's new in this argument?

Well, I've given you my sermon, but unfortunately, I don't think that carries much weight in the Kansas legislature right now. I am worried about creationism. I told you at the very early day that my father had this view that there was no necessary incompatibility between religion and science. There is between Old Testament details and science but not between religion and science. My own views are atheistic, but I don't go around preaching atheism. You don't get very far trying to do this. And I do accept the fact that people can be religious and still be evolutionists.

Among the four principle evolution people in this country, or in Britain, R. A. Fisher was religious. Sewall Wright, I think he was agnostic. He was a Unitarian, though, but he went to the Unitarian Society for social reasons. Muller was clearly an agnostic, and Haldane was probably an out-and-out atheist. So was Muller for that matter. Oh, Dobzhansky was religious. So the leaders of this field are pretty evenly split between those that are religious and those that aren't, and yet they agree on evolutionary views. All the arguments among Muller, Fisher, Wright, the rest of these, none of them are changed one whit by whether the person's own views are religious or not. So I've decided I don't care whether a person is personally religious. I want to believe in evolution.

AM: Do you think these kind of Scopes Trial stories[113] that we read in the paper today are something peculiar to the United States?

JC: It seems to be, to me.

AM: Do the Japanese population geneticists have to deal with that?

JC: I've never had many discussions of religion with population geneticists. Several of my Japanese friends have said to me, “I was educated in a Christian school but I'm not a Christian.” Two or three said this, as if I cared much about which they were. But the Japanese people that I dealt with, religion – they may have been Buddhists. Certainly, none of them followed the Christian religion, as far as I'm aware.

In England, I don't think there's this much argument about intelligent design, and certainly not on the continent. The French, I don't know what to say about them. They'll have a unique view on whatever the subject is.

AM: This is going back a little bit to something we just talked about, and that is, [Jonathan] Beckwith, and [Richard C.] Lewontin to a certain extent,[114] have discussed or talked about the hegemony of the gene, that molecular genetics has now kind of reduced everything down to genetic explanations for everything and argue that there are other things, and maybe this isn't going to bring us to a better understanding of the human condition. So relate this back to this kind of fear that population genetics, or other kinds of fields, biology or science or whatever, as being subsumed by molecular genetics. How do you look at this?

JC: Well, I don't agree with them on this, but it does remind me to say a little more. I had my classes picketed as being – nobody said anything as strong as racist, but of being too much of a hereditarian, which I really wasn't. I wrote an article that was more or less defending Arthur [R.] Jensen,[115] and this was taken up by the same committee that Lewontin and Beckwith are on. I don't think they did it, but their Madison counterparts took that up from me, and we had quite a little to-do around here, people demanding that I be fired. Of course, nothing happened. It was all over in a couple of weeks. But I did find my classes being picketed. An interesting experience, fun to look back on. I don't take it seriously now.

Beckwith I don't know very well, but I do know Lewontin very well . I know what his views are, and I know he says things that just strike me as absurd. I think he must think they're absurd, too, when he asserts that there's no genetic basis for intellectual differences, and he almost goes that far.

The one thing that I heard about him, and I asked him whether this was true and he said yes, so it augurs well for him. Somebody asked him, "Dick, I know you say that there's no genetic basis for behavior, intelligence in particular, and especially there are no racial differences. Aren't you just a little bit curious as to whether there are genes involved with intelligence?" Dick's answer was, "If we had a more just society, I would permit myself to be interested." So he takes the view, now I think still, and it's the same view that [Hermann] Muller had in the twenties. Muller said we shouldn't be talking about eugenics until we solve our social problems. I think Dick feels, and I presume Beckwith, who I don't know so well, feels that if somebody shows a strong genetic basis for some important social trait, that that could be socially problematic. But especially he has the view we need to clear up a lot about society first.

I'm not totally unsympathetic with that, but I disagree with the idea – Oh, there's another way of saying all of this. I had a little personal discussion with Dick Lewontin about this once. We both agree that sooner or later we'll reach a point where we have to lie, but we place it at a different point. If the social gain is important enough, he's willing to dissemble before I am. To me, I place the truth on a higher pedestal.

AM: And he does lie?

JC: Not very often. He does. Well, that's too strong a statement for me to make, but I know he must surely say things that he doesn't really believe, and with a noble social purpose.

AM: This is a good bridge to the area I'd like to talk about now. [pause]

That's kind of the eugenics legacy that, particularly in the United States, geneticists carry with them, because in the early part of the twentieth century, there was certainly a melding between journals and societies and figures of sciences. You had mentioned – I think you had mentioned, and I just want to make this – that you used the [Edmund C.] Sinnott and [Leslie C.] Dunn [*Principles of Genetics*] textbook, and they did have a chapter in their first edition on eugenics. Were you aware of it when you were learning –

JC: I was aware of it. I didn't question it then. I have since. I'm not horrified by eugenics the way some people are. I think the early eugenics movement was both naive and sometimes cruel, and it was good riddance. But the idea – it wasn't a right wing organization initially because a lot of people that we admire for other reasons, like Bertrand Russell and [Harold J.] Laski,[116] British people, were eugenicists at that time. It was just a popular thing to do, whatever your political motivations were.

I can see that if we hadn't made all these discoveries in molecular biology so that any eugenics steps as far as the population was concerned seemed rather futile, one could still argue that -- and I think one might have, I mean, if genetics was the same state of knowledge that it was in the 1920s, that we might worry about – well, one question was sperm banks. There have been advocates of selecting males for sperm banks. Well, if that were practiced on a nationwide scale, it would make a difference, but it's done with such a pitifully small number that – and the view I take about this is to be humanitarian. I'm all for using artificial insemination,[117] or any other genetic trick, but I want the interest to be the interest of this particular family and not – I've said the same thing before. I want the interest of this particular family and not any considerations of the race as a whole, the population as a whole.

Muller is interesting in this regard because he was very opposed to the American eugenics movement, just because it was futile and the social problems were so great. But even from the beginning, he's been advocating judicious use of artificial insemination. He thought that people could be persuaded, women in particular could be persuaded, to choose good males for artificial insemination. I always found this problematic, even slightly amusing, that Muller had this – he just thought that people could be inspired to improve the population by this means. And he was strongly opposed to coercion. He said he wanted people to be inspired rather than coerced. That seemed, then and now, as strikingly naive. And if Muller were still alive, with all the techniques that are now available, [he would be an advocate].

I think we're going to have these questions come up. People can now choose, more often by aborting the wrong embryos or something like this than by choosing mates. But we can have a child that's free of a good many kinds of diseases, and it's not a very big step to choosing a child that's more intelligent or a child that's more healthy, or taller, other things that – I know some people react very strongly against this. I think my view is that I'll worry about this as a social problem when it becomes a social problem, but not too long in advance. So I'm not a very good medical ethicist. I choose not to worry about problems until it's pretty clear that they're happening. I realize people can say, well, by the time you've waited that long, you're halfway down the slippery slope. I'm aware of that argument.

AM: OK. I wanted to just take a couple of things out of some writings you did. One's from the seventies, '72 in *Birth Defects,* in which you were writing an introduction and a conclusion to a volume on human genetics, medical genetics. You bring up Muller, and it seems to me in this one paragraph – and I'll just read it. "Muller was not willing, as many geneticists are, to let the determination of future gene frequencies be the byproduct of social, economic, and medical decisions that through their effects on reproduction determine which kinds of genes are perpetuated." It seems to me that you were kind of indicting – maybe indicting is a little bit too strong of a word – your fellow geneticists in a certain kind of failure of will, maybe.

JC: I think that's correct. I have a weaker view now. (chuckles)

AM: OK. So just tell me why you had such a strong view then and why –

JC: Mostly because I'd been talking a lot with Muller, and it's hard to talk with Muller and not be partly convinced. I don't think that's – of course, it's not out of context, but I don't think it's unrepresentative of what I would have been thinking forty years ago.

AM: Then more recently, in *Mutations Research* in 1999, [reading] "We live in a time of continually improving living conditions. Traits that would have been weeded out by selection in the past are being preserved. This can only mean a greater rate of mutation accumulation. We do not notice any ill effects of this because of the improved living environment, but can we keep improving living conditions forever? Or will there be a day of reckoning and we find ourselves devoting an increasing fraction of our resources to taking care of each other's genetics defects?" Now, I'm thinking this is the slippery slope, and that you have now kind of come down. How do you distinguish what you wrote then with some other earlier –

JC: I still pretty largely agree with what I said then. That's one of the reasons – (chuckles) I'd be pretty inconsistent if I – what I say when I give lectures on mutation rates is pretty much what's in there. Then I say that we know how to deal with genes of major effect, usually by non-breeding methods, molecular tricks, so most of the problem is going to be with genes with very minor effects that will accumulatively add up to something. I do think that if we don't do anything any different that that will happen. But that's not going to happen for several hundred years. We've learned so much in the last twenty or thirty years that I'm just not in favor of taking any kind of action right now. So I cop out when I talk about this. (chuckles)

AM: OK. One final question, which is completely in a different direction. Did you participate in the decision by the Genetics Society of America to boycott the 1978 International Congress?

JC: Mm-hmm, I did. In Moscow, you mean, in Russia. Yes, I did. I was president of the American delegation, so I thought that I could exert an influence that way.

AM: What were your reasons for boycotting?

JC: [Natan] Sharansky was one,[118] and the other name I've forgotten, but these two people were imprisoned right at that time, and I thought that was particularly cruel. There were also rumors – some of which turned out to be correct – that some geneticists, especially Jewish geneticists, were going to be badly treated when they got there, which turned out to be right. So I just thought that this was a good thing to do.

I'm not at all sure now if this was the right decision. Some of my Russian friends have told me quite openly that they think it was the wrong decision, that the role I might have played by interceding, or at least translating between Russian genetics and American genetics, would be worth a lot more than any possible political impact I could have had on a regime that wasn't very responsive to political impacts in this country. I won't argue at all that this was the right thing to do, but I did it nonetheless.

This group from Harvard challenged me on that. I was out of the country or I would have responded. They had an article saying why would I boycott Russia when I didn't approve of the Chile regime and I wasn't boycotting a meeting in Chile. I know how I would have answered. I would have said there wasn't any Genetics Society meeting in Chile. I might have boycotted that, too.

IX. Crow's Research Pursuits; The State of Science

AM: Well, I've come to the end of my questions. Are there some other areas you'd like to talk about that we haven't touched on yet?

JC: The one area we haven't really touched very much on is research. Now, is that something that's important to include in this?

AM: Yes, definitely.

JC: I wrote a little outline for myself so I can go roughly chronological, the kind of things that I've been interested in as research projects, sometimes with graduate students, sometimes with postdocs, sometimes on my own. I did talk about the DDT studies, which I did at first. Then for several years I spent quite a bit of experimental – one way or the other – studying the question of genetic load, how to make use of inbreeding data to reach any kind of inferences. That was done with three or four different graduate students, not all at the same time.

Something else I did at that time – it started really when Newton Morton was a student – was trying to decide ways to measure Wright's effective population number [size] in real populations. I set myself to ask this question, What can I tell by examining everything except genetics – birth and death rates, population size – how much from that can I infer about what the genetically effect population number is? So in a sense, I opened up this field and wrote several papers on it. Almost all of them have a mistake in it somewhere, so I've corrected one mistake. Then finally I gave up correcting mistakes, and there are a couple of people in Europe – Bill [William G.] Hill in [University of] Edinburgh and [Armando] Caballero in [Universidad de Vigo] Spain, both of whom found detail mistakes. They don't change the whole picture very much, but they've made substantial improvements in this. If I were writing a paper today, I'd simply copy what Caballero said.

Then I told you about using the names as a way of studying genetics. That turned out to be very fruitful in studying the Hutterites. It was very popular among anthropologists, who took it up with a vengeance. In some cases, I thought – in ways that I didn't thoroughly approve of – I thought they were looser in the use of this than I would have been. But I thought it was a useful contribution. It's worth saying that the basic idea there, I thought, came from Muller. He once mentioned that names are linked to the Y chromosome, and when I first worked out the theory of this, I wrote Muller and said the idea came from him, maybe I could acknowledge him somehow. He said he'd never heard of such an idea. It probably means that he tossed it off and never gave it another thought. But I took it seriously.

AM: For the guy who always was afraid people were stealing his ideas, I would have thought he would have latched on there.

JC: I'm probably the only person in the world who's had a reverse priority dispute with Muller. I claim quite a bit of credit for this. (chuckles)

And I worked out a little theory about how to use again demographic data to infer how much selection's taking place, or how much selection could take place. I called it an index of opportunity for selection. That too was taken up by several ecological groups. I didn't do much with it. I just formulated the initial theory.

I've been interested in the fact that hardly ever are genes completely recessive. There's usually a little bit of a heterozygous effect. I've done with my students several experiments, sometimes in the laboratory, sometimes using natural populations, to get at this point, and I think established beyond any reasonable doubt that at least in *Drosophila* there's hardly ever such a thing as a completely recessive gene.

Oh, and Fisher had a concept that he called reproductive value.[119] It's a way of, when a population has not reached age stability, of taking that into account. Fisher left it halfway done, and then I discovered that, in fact, Fisher had solved the problem completely. I'm being vague here, but with details it doesn't matter. It's done for a population without any sex, so I thought one ought to try to carry this over to a Mendelian population,[120] which is a much tougher problem. I never did solve it in any neat analytical way, but I did formulate it as a problem in calculus, in other words, as a continuous problem, and partly numerical and partly theory, I satisfied myself that Fisher's theory really does apply to Mendelian populations too.

This had a happy counterpart, because Paul [A.] Samuelson, the economist,[121] was interested in the same problem. I don't know why because it isn't really economics, but he's a man of wide interests. He was working on the same thing, but his approach was finite, using matrices, whereas mine was using calculus. We worked more or less in parallel, exchanged conclusions when we had them. And I could expect a phone call from him every Saturday morning. He knew I worked at home. He did economics during the week and then did this on weekends. So we had this delightful correspondence. Then after we both stopped, we didn't do anything with each other for several years. Then finally we had the occasion for a phone call, and I remember asking him how he was enjoying retirement. He said, "It's all right. But I'm not as grossly overpaid as I used to be." Which I loved.

AM: This is the extent to which evolutionary theory has been moved into other disciplines, economists currently, it's very trendy right now to use evolutionary theory to explain economic development and economic structure. As somebody who has worked in this field and developed it from the genetics point of view, what do you think about this?

JC: I enjoy it. Of course, I don't know how good it is in economics, but I read a paper a year or so ago with mathematics in it, and I could have just changed the words and left the mathematics intact and it would have been a good paper in population genetics. Clearly, the methodology is carried over. I'm not surprised. This is characteristic of mathematics that it has many uses. I rather like that. And some of the vocabulary of population genetics has been carried into economics.

Oh, I told you about doing one problem about one of the phases of Wright's theory, and we did quite an elaborate simulation computer study of that. It got beyond my ordinary mathematical capacity. I worked on it with a couple of colleagues here.

What I'm doing right now – it's always with other people's data because I don't have students any longer, and I don't have a lab. I'm interested in the relationship between mutation and paternal age. It's been known, remarkably, ever since [Wilhelm] Weinberg[122] in 1912 found out that the children with achondroplasia, the short-limbed dwarfism, that they tended to be late born in the sibship.[123] So he inferred from that, that probably means there's a mutation, and I think for 1912, that's an astonishingly accurate observation. Anyhow, it turned out to be quite correct. There are more cell divisions in the male than in the female, and in older males than in younger males, so this all makes sense. But, as usual, that's not the whole story.

I've gotten interested in some of these diseases, particularly one called Apert's syndrome,[124] which is neither here nor there, but it's a well known genetic disease. It shows an extreme paternal age effect, non-linear. All the mutations take place at one nucleotide, or maybe two, but in one codon. It's been suggested that that probably, rather than representing a high mutation rate, really represents some sort of selection working before meiosis in the male. At first, I sort of rejected that idea and had a long argument with the person who was – in correspondence, that is, not public. Then I finally came around to think this is really worth taking seriously. I'm trying to think of ways to accept or reject that particular notion, with no success so far. But I lecture on it frequently. (chuckles)

And there's a topic called purging that is the idea that if an individual is inbred right now, he's going to be homozygous for some genes. And if there's an inbred ancestor way back somewhere, that some of those genes might have gotten removed by natural selection in the ancestor, and that's what's been called purging. There's paper after paper in the ecological field now of somebody who does an experiment and they either find purging or they don't find it, and they write a paper.

It occurred to me there is a way this can be quantified and made into a quantitative subject. The place I'm applying this, of all places, is in dairy cattle. But there are these extensive herd records that involve hundreds of thousands of cows. There's a young student here who's been – these are all computerized and I don't understand for an instant how they do all this, but I'll trust it. So I worked it out with her, an idea of how one can measure the purging effect in a quantitative way, rather than just saying it exists or doesn't exist. So that's a preoccupation of mine right now.

AM: I imagine the Wisconsin Alumni Research Foundation is very pleased that you're working with dairy.

JC: They probably would be if they knew it, right. (chuckles) The data already exist. I've done just a little more theoretical study of the kind of thing that would have attracted some attention thirty-five or forty years ago, but the field changed in the meantime, so there's little corners of the theory that I've tidied up a little bit, which amuses me and nobody else cares. (chuckles)

AM: Well, you clearly have done a lot in many different areas across your career. If you had to sum up who you are as a geneticist, what would you say?

JC: Well, I'd say most of all I've been opportunistic. I tend to be other directed, to use what used to be a popular expression, and I tend to follow something that just comes up. Some of my work has been planned and organized, it just isn't totally that way. But it's usually started off with a more or less chance or circumstance and then I've followed it up, sometimes developed it quite a way and sometimes abandoned it after a short while.

I don't think this is the best way to get research done. I think it's better to pick an area and really become very good at it, and I think most people do that. I probably would have made a bigger impact on the field if I had picked something and stayed with it. It would have been a lot less interesting life, and I have no regrets at all about dabbling here and there.

The other question that people frequently ask me is whether I would have done more research if I hadn't spent so much time teaching and on committees and deaning. Of course I would have done more, but I think it's a fair statement, I don't know that I would have done anything any more important, because I think most of the good ideas that I've had I've found a way to work out. I guess I can't say whether if I'd maximized my research that I would have had correspondingly more ideas, but I sort of doubt it.

[pause]

AM: How is the field of genetics , in all of its definitions, driven? Is it by narrowly focused people, or is it by people like you who can do many things and tidy up the corners, tidy up the edges, and be a good educator for the next generations?

JC: I think the main progress is by people who stay close to their last. I don't sense anybody right now that plays the central thinking role that Muller did at one time in the past, Lederberg did more recently, and Fisher. There probably are such people now. There certainly must be. And maybe we have to wait a few years to see who did. But I don't sense anybody who was thinking about the whole field and sort of pointing out that this is where we need to know more, and here's where it doesn't matter so much. I think nowadays people are largely choosing problems of their own interest, or where they can get an answer. And the techniques are so good. I wish I were starting over, because it is an exciting time to do genetic research.

AM: Is it an advantage or a disadvantage to have a field in which there is no group of thinkers, like a Muller or a Lederberg?

JC: I don't think now it makes an awful lot of difference. Muller certainly did influence the course of research of several different people. Most of them, though, were in his own laboratory. But I think it's extended beyond that. And clearly Lederberg did, he just opened up a field. Well, of course, Watson and Crick did, too. I mustn't forget them.

My favorite – I wrote a little article on the anniversary of Watson and Crick, and I was interested in the fact that just at the time when this famous photograph by Rosalind Franklin that tipped Jim Watson off as to what the structure was.[125] That if [Linus C.] Pauling had been permitted to go to England, he would have probably seen that photograph.[126] Pauling was very, very bright. He would have probably thought of the proper structure. So I ended my little essay by saying that it's just possible that Watson and Crick owe their success to Joe [Joseph] McCarthy,[127] which is as a thought too horrible to contemplate, really.

AM: Did Jim Watson ever get back to you on that?

JC: He did. He liked it. (laughed) I was afraid he might not, but he did.

AM: Well, I think that's all I have to ask.

JC: Let me see if I have anything else I wanted badly to say. We talked about it a little before, I have spent a little time recently chairing a couple of committees on the use of DNA for forensic purposes. There's not an awful lot to say about that except that it's something I'm very happy to have done. I think it changed – it was very controversial, and what our committee did was bring into the routine practice the use of good statistical methodology that is now part of the business. I'd say the standards of DNA, except for human mistakes, is really very, very good.

One of the consequences that I hope will happen, it hasn't yet, but I think we should have the same kind of statistical examination of fingerprinting and ballistics and other things that are used in court, which are on much less firm ground than DNA chemistry is. I'm hoping that, as a consequence of our report, that there will be a good statistical look at other crime investigatory methods. I guess that's all I can think of. We've covered my whole life, so far. (laughs)

AM: Well, this has been truly a pleasure, and thank you for giving us your time to do this interview.

JC: I enjoyed it.

END INTERVIEW

---

[1]Cherokee Strip: Area in Oklahoma originally assigned to the Cherokee Nation and later bought back by the Federal government; opened to settlement in the famous “land run” of 1893.
[2] Friends University: A private non-denominational Christian university in Wichita, Kansas, founded in 1898, which has a past association with the Quaker faith, but is now Christian nondenominational.
[3] Haverford College: A private liberal arts college in Haverford, Pennsylvania and established in 1833 by the Quakers.

[4] Clarence E. McClung, (1870 - 1946), American geneticist. From his study on a species of grasshoppers, McClung was one of the first to argue that chromosomes determine the offspring's sex.
[5] David H. Wenrich, (1885 – 1968), American scientist who studied sex in microorganisms.
[6] Eleanor E. Carothers, (1883 – 1957), American cytologist who first showed evidence of independent assortment in the first phase of meiosis.
[7] Ursinus College: A private liberal arts college established in 1869 in Collegeville, Pennsylvania.
[8] Tuberculosis: A highly contagious and often lethal bacterial infection which can cause a number of symptoms including lung infection. Although treatable with modern antibiotics, and controllable with modern methods, it remains a serious health problem in many countries.
[9] Quakerism: also known as the Religious Society of Friends, a religion based on the idea that anyone can communicate with the divine without the need for a spiritual leader. George Fox (1624-1691) is considered one of Quakerism's founders. See the World Health Organization fact sheet at: http://www.who.int/mediacentre/factsheets/fs104/en/index.html
[10] Unitarian/Universalist: A religious belief rooted in the Christian faith and characterized by a search for spiritual truth, but without adherence to specific doctrines.
[11] Bertrand Russell, (1872 – 1970), prominent English philosopher, logician, and social critic of the 20th century who contributed to many scholarly fields. See http://plato.stanford.edu/entries/russell/.
[12] Victrola: Popular brand of home phonographs in the early 1900s.
[13] *Journal of Heredity*; A scientific journal founded in 1903, soon after the rediscovery of Mendel's work, to publish research in genetics and heredity. See http://www.oxfordjournals.org/our_journals/jhered/about.html.
[14] Norman Thomas, (1884 – 1968), Socialist Party of American leader as well as a civil rights and anti-war activist who ran for President multiple times without success.
[15] Franklin D. Roosevelt, (1933 – 1945), 32nd President of the United States during the Great Depression and the Second World War.
[16] Earl R. Browder, (1891 – 1973), General Secretary of the US Communist Party 1932-46. He was expelled from the Party in 1946 after he attempted to distance it from the Soviet Union.
[17] Emergency Peace Campaign; Started by the American Friends Service Committee in the late 1930s, this group campaigned against US entry into WWII and for peace, encouraging other peace, religious, and ethnic groups to rally as well.
[18] Freedom of Information Act; passed in 1966, the Act ensures that the US government allow the American citizenry access to unreleased government records. It has been amended several times to insert and remove exemptions.
[19] Edmund C. Sinnott and Leslie C. Dunn, along with Theodosius Dobzhansky, published *Principles of Genetics: An Elementary Text with problems,* in 1925. The book went through three subsequent editions.
[20] Krebs Cycle: An intricate physiological process with multiple steps through which organisms use oxygen to produce energy and CO_2. Also known as the Citric Acid Cycle and the Szent-Gyorgyi-Krebs cycle, it was elucidated in the 1930s by Nobel laureates Albert Szent-Gyorgyi (1893-1986) and Hans Adolf Krebs (1900-1981).
[21] James D. Watson (1928 -) and Francis Crick (1916-2004) were the first to propose the double helix structure of DNA in 1953 and are renowned for having done so. Along with Maurice Wilkins, Watson and Crick received the 1962 Nobel Prize for their work. See http://www.time.com/time/time100/scientist/profile/watsoncrick.html
[22] Rhodes Scholarship: A prestigious scholarship awarded annually for international students to study at the University of Oxford.
[23] Thomas H. Morgan, (1866 – 1945), A leading American geneticist awarded the Nobel Prize in Physiology or Medicine for his work explaining the locations of genes on chromosomes and the mechanism by which they transmit hereditary traits in his work with *Drosophila* at Columbia University. See http://nobelprize.org/nobel_prizes/medicine/laureates/1933/morgan-bio.html. Morgan's group included his students Alfred H. Sturtevant and Calvin Bridges, who arguably contributed at least as much to the work as Morgan himself. With Hermann Muller, the three authored *The Mechanism of Mendelian Heredity* in 1915.
[24] Hermann J. Muller: (1890 – 1967), A leading American geneticist and former student of Morgan's, who was awarded the Nobel Prize in Physiology or Medicine in 1946 for demonstrating that artificial mutations can occur as a result of X-ray manipulation. See http://nobelprize.org/nobel_prizes/medicine/laureates/1946/muller-bio.html.

[25] John T. Patterson, (1878-1960), distinguished American researcher in *Drosophila* (fruit fly) genetics and embryology. He worked with Hermann Muller to show that mutations were reversible.
[26] Theophilus S. Painter, (1889 – 1969) American zoologist who worked with Muller to identify *Drosophila* genes and was the first to come close to a correct approximation of the number of human chromosomes.
[27] Wilson S. Stone, (1907 – 1968), American geneticist who worked with Patterson, Muller, and Painter to examine the effects of ultraviolet radiation on mutation.
[28] Hiram Bentley Glass, (1906 – 2005), leading American biologist and pioneering geneticist who was noted for his writing on the dangers of eugenics and the nature of science in the future. Served as President of the American Society for Human Genetics in 1967. See http://www.nytimes.com/2005/01/20/science/20glass.html. His papers are at the American Philosophical Society; See: http://www.amphilsoc.org/mole/view?docId=ead/Mss.Ms.Coll.105-ead.xml;query=Bentley Glass .
[29] *Drosophila*: A genus of fruit fly, most commonly *Drosophila melanogaster*, that has become a model organism used in genetics experiments since it is relatively inexpensive, easy to care for, has a high fecundity and a short life span, and exhibits a readily discernable phenotype and genotype. See the NCBI site at: http://www.ncbi.nlm.nih.gov/mapview/map_search.cgi?taxid=7227 .
[30] Cytogenetics: The genetic subfield that focuses on the structure and functions of the cell and its chromosomes.
[31] Speciation: The evolutionary process by which new biological species develop.
[32] Meiosis: Cell division producing gametes or spores, leading to sexual reproduction.
[33] Sewall G. Wright, (1889 – 1988), leading American geneticist who helped to develop theoretical population genetics. He computed the amount of inbreeding of members of populations as a result of random genetic drift and, with R. A. Fisher, pioneered methods for computing the distribution of gene frequencies among populations. His papers are at the American Philosophical Society: See http://www.amphilsoc.org/mole/view?docId=ead/Mss.Ms.Coll.60-ead.xml.
[34] *Mulleri* Group: A subspecies of *Drosophila*.
[35] This noted textbook has gone through multiple editions under three names and at least four different authors . The first was published by Getman alone in 1913 as *Outlines of Theoretical Chemistry;* Daniels joined him on the 5th edition (*Outlines of Physical Chemistry)* in 1931 and continued publication after Getman's death in 1941. Crow is presumably referring to the 6th edition (1937).
[36] Joshua Lederberg, (1925 – 2008), American molecular biologist, who demonstrated that bacteria can exchange and transmit genetic information in reproduction. He received the Nobel Prize in Physiology or Medicine in 1958. One of the deans of American science, he served on numerous committees and also worked with artificial intelligence and space biology programs. See http://www.nobelprize.org/nobel_prizes/medicine/laureates/1958/lederberg-bio.html .
[37] Norton D. Zinder: (b. 1928), American biologist who discovered in 1952 the process of genetic transduction, the use of bacteriophage (a virus) to exchange genetic information between organisms in *Salmonella* bacteria.
[38]Salivary Glands: ducts in mammals that secrete salivary amylase to break down food in the mouth. Geneticists study salivary gland chromosomes in *Drosophila* because they are relatively large and easy to see.
[39] Hybridized, that is, cross-bred across species or subspecies.
[40] Reinforcement: also known as the Wallace effect, after evolutionary pioneer Alfred Russel Wallace (1823-1913), is the process by which natural selection increases reproductive isolation through the increased incompatibility of species and subspecies until they can no longer cross-breed or produce only sterile or unfit hybrids.
[41] Ronald Aylmer Fisher, (1890 – 1962), great English statistician and geneticist who created many of the tools and theories of modern biostatistics.
[42] James V. Neel, (1915 – 2000), American geneticist who was one of the first to define genetics as its own field of study. He researched genetic epidemiology, the genetics of sickle cell anemia and the effects of radiation from atomic bombings. In 1956, Neel was the founder of the first US Department of Human Genetics based in a medical school at the University of Michigan. See http://www.aps-pub.com/proceedings/1461/109.pdf.
[43] Harrison D. Stalker, (1915-1982), American evolutionary biologist and late professor at Washington University in St. Louis.

[44] Translocations: the movement or rearrangement of sections of chromosomes, leading to gene fusions and other types of abnormality. Translocations can be detected through cytogenetic studies (karyotypes).
[45] Genetic mapping: Refers to a type of gene mapping using linkage analysis to determine the relative positions of different genes on a chromosome, based on the frequency of their recombination.
[46] These Russian geneticists and students of Muller's were executed by the Soviet government in the late 1930s.
[47] Theodosius G. Dobzhansky, (1900 – 1975), a noted Ukrainian geneticist who worked with Wright on *Drosophila* and contributed to the synthesis of genetics and evolutionary theory.
[48] Chi Square Distribution: A classic statistical distribution which can be used to test goodness of fit, independence of data, and the confidence interval for the standard deviation.
[49] V-12 Navy College Training Program: During World War II, male college students participated in officer training regimens along with their classes.
[50] Parasitology: The scientific study of parasites and parasite-host relationships.
[51] John and Mary R. Markle Foundation: A private non-profit agency established in 1927 with the goal of advancing health care and social welfare. See http://www.markle.org/our-story/history.
[52] *Othello* is the classic Shakespeare tragedy about misguided jealousy. Paul Robeson (1898-1976) was one of the finest singers of his era and a leading civil rights advocate. His career was permanently tarnished when he faced unjust scrutiny during the McCarthy era. Jose Ferrer (1912-1992) was a Puerto Rican actor and director, the first Hispanic actor to win an Oscar in 1950, for *Cyrano de Bergerac.* Uta Hagen (1919-2004) was a German-American actress who appeared mostly on Broadway, where she won several Tony awards.
[53] Neuropathy refers to any disorder of the peripheral nervous system. Alcoholics who consume little food often have vitamin deficiencies. Vitamin B1, or thiamine, aids in the breakdown of carbohydrates and in the biosynthesis of the neurotransmitter acetylcholine, which is important to the motor neurons.
[54] Dorothy M. Wrinch, (1894 -1976), an English mathematician and biological theorist, used mathematical principles to deduce and elucidate the structure of proteins and amino acids. She was nominated for the Nobel but never won.
[55] Andre Dreyfus is considered a pioneer of Mendelian genetics in Brazil. He transformed the University of Sao Paulo into the country's leading genetic research institution and invited Dobzhansky there in 1943 to train *Drosophila* researchers.
[56] Edmund Spenser, (1552 – 1599), was an English poet of the Elizabethan era. His most famous work is *The Faerie Queene,* a epic allegorical poem that spans six books.
[57] Rh or Rhesus Factor: The immunogenic D antigen, part of the Rh group, which can cause hemolytic disease in a newborn, if its mother lacks the antigen. Three-locus model refers to the fact that the Rh factor involves three different genes in three different locations on a chromosome pair.
[58] J.B.S. Haldane, (1892 – 1964), British geneticist and evolutionary biologist, one of the founders of population genetics. He was the first, in 1937, to calculate the mutational load caused by recurring mutations of a specific gene, the work Crow refers to here. The idea of genetic load was applied to humans by Muller in 1950. See: Muller, Hermann J. "Our Load of Mutations." *American Journal of Human Genetics* 1950; 2 (2): 111-176.
[59] Genetics Society of America was founded in 1931 to promote genetics education and research, through regular conferences and its journal, *Genetics* (first published in 1916). Crow would later serve as GSA president in 1960. For a history of the organization, see: http://www.genetics-gsa.org/pdf/GSAhistoryscrapbook.pdf .
[60] Overdominance hypothesis: A proposed explanation for hybrid vigor (heterosis), suggesting that certain combinations of alleles obtained by crossing two inbred strains prove to be advantageous in the heterozygote. Opposed to the dominance hypothesis, which suggests that undesirable recessive alleles from one parent are suppressed in hybrids by dominant alleles from the other. These two theories were first presented in 1908, and are still debated, although dominance is perhaps the currently (2011) favored view. See Crow, James. (1948). "Alternative Hypotheses of Hybrid Vigor". *Genetics* 33 (5): 477–487; (1998). "90 Years Ago: The Beginning of Hybrid Maize". *Genetics* 148 (3): 923–928.
[61] Susceptibility or resistance to the anesthetic drug ether is a genetic trait much studied in *Drosophila.*
[62] American Philosophical Society is the US' first learned society, founded in Philadelphia in 1743 by Benjamin Franklin and John Bartram to promote useful knowledge in the sciences and humanities. The

Society Library includes the largest collection of manuscripts in the history of genetics and eugenics in the country. For more information, see: www.amphilsoc.org .

[63] Mr. Chips (Chipping) is the title character in the 1934 novel, *Good-bye, Mr. Chips,* by James Hilton, and subsequent film adaptations, notably the 1939 movie starring Robert Donat. Mr. Chips devotes his life to teaching and inspiring boys at an English public school.

[64] Tracy Morton Sonneborn, (1905 – 1981), American biologist who studied the unicellular protozoan group *Paramecium*.

[65] *E. coli:* An model microorganism that occurs naturally in the human intestine and was the subject of Lederberg's Nobel research. Well-known for causing intestinal symptoms, but most strains of the bacterium are harmless.

[66] The International Biometric Society, headquartered in Washington, DC, is an international society promoting the development and application of statistical and mathematical theory and methods in the biosciences. It publishes *Biometrics,* a quarterly journal.

[67] *Neurospora*: a genus of fungi. The bread mold species *Neurospora crassa* is often used as a model organism in genetics; it reproduces rapidly and has minimal nutritional needs. Specifically, *Neurospora* was used by George Wells Beadle and Edward Lawrie Tatum to develop mutations differing in specific nutritional requirements; this work led to their Nobel-winning (1958) one gene-one enzyme hypothesis.

[68] Archibald E. Garrod, (1857 – 1936), British physician who was the first to claim that genes . He worked extensively on hereditary metabolic disorders, for which he developed the term, "inborn errors of metabolism," and postulated that these resulted from genetic mutations resulting in the deficit of a single enzyme. His landmark work, *Inborn Errors of Metabolism*, was first published in 1909.

[69] Endocrinology: The study of the endocrine system, the system of glands and tissues secreting endogenous hormones that regulate the body's growth and metabolism.

[70] Dr. Royal Alexander Brink, (1897-1984), plant geneticist who was the head of the Department of Genetics at the University of Wisconsin-Madison from 1939 to 1951.

[71] DDT (dichlorodiphenyltrichloroethane) is a controversial pesticide first synthesized in 1874. It was used very effectively to control malaria and typhus in World War II and subsequently much used in agriculture and as a disease prevention measure in the US and in the 1950s. The 1962 book, *Silent Spring,* by the biologist Rachel Carson, brought attention to its lethal effects on bird populations. DDT's routine use in agriculture was banned in the US in 1972, and worldwide by the Stockholm Convention of 2001, but it is still in limited local use for disease control.

[72] Second-generation Japanese-American.

[73] Now Professor Emeritus of Zoology at the University of Oklahoma.

[74] Newton Morton is currently Senior Professorial Fellow in Human Genetics at the Human Genetics Division of the of the University of Southampton School of Medicine. His major contributions have been in the fields of human genetic linkage analysis and genetic epidemiology.

[75] Motoo Kimura, (1924 – 1994), A very prominent theoretical population and evolutionary geneticist who proposed in 1968 that most genetic change at the molecular level is neutral with respect to natural selection – that is, genetic drift is a primary evolutionary factor. Crow has written memoirs of appreciation for Kimura in different scientific journals. For one such article, see Crow, JF, "Memories of Motoo." *Theor Popul Biol* 49 (1996): 122-127.

[76] Logarithm of the Odds Score, a measurement of the probability that a set of genes are linked, developed by Newton Morton.

[77] Jay Lawrence Lush, (1896 – 1982), American animal geneticist based at Iowa State University who advocated selective livestock breeding based on genetic and quantitative information rather than physical appearance and subjective perceptions. Lush is sometimes known as the father of modern animal breeding.

[78] Consanguinity: Kinship based on sharing common ancestors.

[79] Biological Effects of Atomic Radiation Committees (BEAR) were established by the US National Academy of Science in 1954 to compile and make sense of available knowledge and research about the effects of atomic radiation on living organisms. There were six BEAR subcommittees, each studying a particular area of life, and a number of subcommittees, including one on Genetics (1962-64). The committees published a series of reports before BEAR was phased out in 1964.

[80] Morton NE, Crow JF, and Muller HJ. "An estimate of the mutational damage in man from data on consanguineous marriages." *Proceedings of the National Academy of Sciences of the U.S.A.* 1956; 42: 855-863.

[81] Rayla Greenberg Temin: American population and *Drosophila* geneticist perhaps best known for this 1960 paper with Crow on recurrent mutation and inbreeding. See Greenberg R and Crow JR, "A Comparison of the Effects of Lethal and Detrimental Chromosomes from *Drosophila* Populations." *Genetics* 1960; 45: 1153-1168. She was married to Howard M. Temin (1934-1994), co-recipient of the 1975 Nobel Prize in Physiology and Medicine.
[82] John Z. Bowers, (1913 – 1993), American physician who served on the 1949 commission to study the long-term biological effects of atomic radiation in Hiroshima and Nagasaki and pursued research in this field throughout his long career. He served as Dean of the University of Wisconsin School of Medicine 1955-64.
[83] Klaus Patau, (1908 – 1975), German-American geneticist who worked at the University of Wisconsin Madison from the 1950s, and in 1960, found the extra chromosome in trisomy 13 responsible Patau syndrome, a developmental disorder involving multiple defects, including heart and kidney abnormalities.
[84] Oliver Smithies, (b. 1925), British-American scientist who invented gel electrophoresis in 1955 and the same year developed the technique of homologous DNA recombination to alter animal genomes, making possible the creation of "knockout mice" and targeted gene mutations. For the latter accomplishment, Smithies shared the Nobel Prize in Physiology and Medicine in 2007.
[85] John M, Opitz, (b. 1935), German-American medical geneticist and Professor of Pediatrics and Human Genetics at the University of Utah. Opitz was one of the first to identify specific groups of pediatric anomalies as genetic syndromes, several of which now bear his name. He was the founder (1976) and long-time Editor-in Chief of the American Journal of Medical Genetics, and a founding member of the American Board of Medical Genetics.
[86] Curt Stern: (1902 – 1981), pioneering German-American geneticist who taught at the University of Rochester 1933-47 and at UC Berkeley from 1947 until he retired in 1970. He was the first to demonstrate the crossover of homologous chromosomes in *Drosophila,* and made many contributions in the areas of genetic recombination, radiation risk, gene regulation, and the separation of human genetics from race-based eugenics. His landmark textbook, *The Principles of Human Genetics,* was first published in 1949.
[87] Eugenics refers to the management of breeding practices, specifically of human beings, to improve the genome. Highly popular and advocated by many distinguished scientists in the early 20th century, eugenics became associated with forced sterilization, racial discrimination, euthanasia, and Nazi extermination programs in the 1930s and 1940s and geneticists made a conscious effort to distance themselves. Recent developments in prenatal diagnosis and in genetic engineering technology have brought the ethical and social questions involved in genetics to the fore again.
[88] The Atomic Energy *Commission* was created by the Atomic Energy Act of 1946 to regulate nuclear energy and to develop peacetime uses.
[89] The US National Academy of Sciences, of which Crow is a member, was incorporated in 1863 to bring together the country's most distinguished scientists to advice the government on science, engineering, and medicine. The NAS meets annually and publishes its *Proceedings,* as well as additional reports, books, and articles. Election is one of the highest honors awarded to an American scientist.
[90] Warren Weaver, (1894 – 1978), an American mathematician and science administrator, was Director of the Division of Natural Sciences at the Rockefeller Foundation from 1932 to 1955 and subsequently a consultant and officer at Memorial-Sloan-Kettering Cancer Center in New York.
[91] William L. Russell, (1910 – 2003), a prominent English researcher who pioneered the study of mutagenesis in mice and whose research helped to establish acceptable levels of radiation exposure for humans.
[92] Genetic load: see note 58 on page 28.
[93] Alfred H. Sturtevant, (1891 – 1970), leading American geneticist, who created the first genetic map while working with Thomas Hunt Morgan.
[94] Alan P. Poland, American scientist who discovered and characterized the TCDD, or dioxin, receptor in cells in 1975, helping to explain these and other environmental contaminants contribute to carcinogenesis. Mutagenesis is the alteration of DNA. Dioxins are a group of highly toxic chemicals formed from the combustion of organic chemicals and plastics with chlorine.
[95] Yuichiro Hiraizumi, (1927 – 2003), a Japanese scientist who, while working in Crow's laboratory with Larry Sandler, discovered segregation distortion, an apparent refutation of Mendelian rules. In normal meiosis, every allele has the same chance of transmission to a functional gamete and thus to the next generation as every other allele. However, segregation distorter genes circumvent this rule and are present in *a majority*

of functional gametes. See: Sandler L, Hiraizumi Y and Sandler I. Meiotic drive in natural populations of *Drosophila melanogaster.* I. The cytogenetic basis of Segregation-Distortion. *Genetics* 1959; 44: 233-250.

96 American Society of Human Genetics was founded in 1948 to provide leadership in research, education, and service in human genetics. Its journal is the *American Journal of Human Genetics.* Crow served as President in 1963. See the ASHG website at: www.ashg.org .

97 The American Genetic Association was founded in 1903 to promote research and education in all areas of genetics, including plant and animal genetics. See the AGA website at: www.theaga.org.

98 The Society for the Study of Evolution, an interdisciplinary group, was in fact first organized as a committee of the National Research Council in 1943 and then as a formal organization in 1946, with the goals of promoting the study of evolution and integrating work from different fields. See: www.evolutionsociety.org.

99 See note 58 on p. 28.

100 See note 80 on p. 36.

101 Arthur P. Mange (b. 1931), American geneticist who, as noted, studied the genetically isolated Hutterite population while working with Crow. He also worked on *Drosophila* and on early computer models of genetics. See Crow JF and Mange AP, "Measurement of inbreeding from the frequency of marriages between persons of the same surname." *Eugen Q* 1965; 12: 199-203; *Soc Biol* 1982; 29: 101-105.

102 Hutterites: A communal and pacifist branch of the Anabaptists, founded by Jakob Hutter (d. 1536) in Austria. They were forced by persecution to migrate throughout Europe, finally moving to the Dakotas in 1873 and later to Western Canada. Although persecuted for their pacifism in North America, they have flourished there in the 20th century.

103 Dow Chemical, one of the world's largest chemical companies, was the target of student protests in the 1960s and 1970s for its manufacture of napalm and Agent Orange.

104 The Clinical Genetics Society was founded in 1970 to promote information-sharing among doctors and other professionals involved in the clinical care of individuals and families with genetic disorders.

105 Lionel S. Penrose, (1898-1972), leading British geneticist, psychiatrist, and mathematician who identified many of the genetic causes of mental retardation.

106 Hans Spemann (1869-1941), was a German embryologist who discovered embryonic induction, the directed development of groups of embryonic cells into particular tissues and organs; he won the Nobel Prize for this work in 1935. Richard Goldschmidt (1878-1958) pioneering German-American geneticist who was one of the first to integrate genetics, development, and evolution.

107 See note 21 on page 11.

108 An operon is a cluster of genes that are co-regulated by another gene, or genomic segment, known as the promoter, and co-expressed. The operon model was first described in 1961 by French biologists Francois Jacob and Jacques Monod, who won the Nobel Prize in 1965 for their work.

109 See note 75 on page 35.

110 Sewall Wright's shifting balance theory, proposed in the 1930s, states that each of the evolutionary forces – natural selection, mutation, genetic drift, and gene flow – plays an important role in adaptive evolution.

111 Intelligent design is one of the counter-arguments to natural selection, which contends that the evidence shows that the universe was created and is still driven by an intelligent force (God), rather than by the undirected mechanism of natural selection.

112 The argument made by proponents of intelligent design that many forms of life are too complex to have evolved from simpler forms.

113 Scopes Trial: In 1925, biology teacher John Thomas Scopes (1900-1970) was put on trial in Dayton, Tennessee, for teaching evolution to his students. The case was a test case of Tennessee's Butler Act instigated by the ACLU (Scopes may never actually have violated the Act) and one of the major new stories of the era. Clarence Darrow led the defense team and William Jennings Bryan was one of the prosecutors. Scopes was found guilty, but his conviction was reversed on a technicality; although the prosecution was ridiculed by many commentators of the day, the trial probably inhibited the teaching of evolutionary theory in schools for a generation.

114 Jonathan Beckwith (b. 1935), American Cancer Society Professor of Microbiology and Molecular Genetics at Harvard University, is noted for isolating the first gene from a bacterial chromosome in 1969 and has made many contributions to bacterial genetics. He is an advocate for the social responsibility of scientists and has written extensively on ethical and social issues. Richard Lewontin (b. 1929), Alexander Agassiz Professor of Zoology and Biology at Harvard, is an evolutionary biologist and population geneticist who

pioneered the application of techniques from molecular biology to solve evolutionary and genetic variant problems. Like Beckwith, Lewontin is a scientific activist; in particular, he has been an opponent of genetic determinism.

[115] Arthur R. Jensen (b. 1923), Professor Emeritus at UC Berkeley, is a major proponent of the idea that genetics play a significant role in human personality, behavior, and intelligence. In particular, he has argued for race-based differences in intelligence, particularly between black and while individuals, a highly controversial and disputed position.

[116] Harold J. Laski (1893-1950), a British economist, political theorist, and Marxist, taught at the London School of Economics from 1936 until his death.

[117] Artificial insemination: the use of donor sperm injected into the reproductive tract to enable a woman to conceive.

[118] Natan Sharansky (b. 1948) is a Jewish Russian-born human rights activist who was accused of espionage and treason and imprisoned in the Soviet Union for 8 years; he was released in 1986 and emigrated to Israel where he has had an active political career.

[119] Fisher's reproductive value: defined in 1930 as the expected reproduction of an individual from their current age onward.

[120] Mendelian population: a group that interbreeds and shares a common gene pool.

[121] Paul A. Samuelson (1915-2009), American economist who won the first Nobel Prize in his field in 1970. Considered by many to be the father of modern economics.

[122] Wilhelm Weinberg, (1862-1937), German-Jewish physician who in 1908 independently developed the concept of genetic equilibrium, now known as the Hardy-Weinberg principle, which states that the frequencies of alleles and genotypes in a given population will remain constant from generation to generation in the absence of disturbances, such as mutations, non-random mating, genetic drift, and others. Weinberg also studied many autosomal diseases.

[123] Achrondroplasia: a genetic disorder characterized by abnormal bone growth; a common cause of dwarfism. Sibship is the cohort of children born to a given set of parents.

[124] Apert's syndrome: a genetic disorder characterized by malformations of the skull, face, hands and feet.

[125] Rosalind Franklin, (1920-1958), British biophysicist and X-ray crystallographer whose X-ray diffraction image was the key piece of evidence that allowed Watson and Crick to formulate the double-helix model of DNA, which Franklin herself determined independently. Most historians feel that Watson and Crick failed to credit her work adequately in their publication of the model in 1953. See: http://www.pbs.org/wgbh/aso/databank/entries/bofran.html .

[126] Linus C. Pauling, (1901-1994), leading American biochemist and peace activist; winner of two Nobel Prizes. His 1949 identification of sickle cell anemia as a molecular disease was the founding event in the field of molecular biology. Pauling was working on the structure of DNA in 1953 and was scheduled to visit the Cavendish Laboratory at Cambridge where Crick and Watson were working, but was denied a visa by the State Department due to his pacifism, which led some critics to accuse him of Communist sympathies.

[127] Joseph McCarthy (1908-1957), Republican Senator from Wisconsin, who played on Cold War tensions in the early 1950s to boost his own career and accused many prominent individuals of Communist ties and sympathies. His tactics prompted a series of interrogations by the House Unamerican Activities Committee and the Senate and later the Army-McCarthy hearings which ultimately led to his downfall and censure by the Senate.