Session I
June 27, 2005

I. Childhood; Marquette University; Serving in the Medical Services

AM: It is June 27th, 2005. I'm Andrea Maestrejuan, and I'm with Professor William Schull at his office at the University of Texas, School of Public Health, in Houston for his oral history interview for the UCLA Human Genetics Oral History Project. We'll start at the very beginning and I'll ask you when and where you were born.

WS: I was born on March 17th, 1922, in Louisiana, Missouri. Louisiana is a small river town. It had probably about five thousand inhabitants when I was born. It is roughly seventy miles north of St. Louis. It was then the largest town in Pike County.

My mother's family had settled in that region of Missouri in the 1820s, 1830s. My father's family came later. They had been in Illinois prior to moving to Missouri. Both families go back to pre-revolutionary times in the United States, so despite the spelling of my name now, which I should point out isn't proper. My name actually should be spelled S-h-u-l-l, but I spent much of my formative years in Milwaukee, which is a predominately German city still, and they couldn't see this S-h-u-l-l, it had to be S-c-h-u-l-l, so by fiat essentially they made my name S-c-h-u-l-l. It's such a contradiction that when the war came along, I had to actually have a notary public attest to the fact that Jack S-c-h-u-l-l was actually Jack S-h-u-l-l. (laughs) After a time we just gave up on any hopes of being able to remedy the changes.

At any rate, I lived in Missouri off and on. My father was a peripatetic person who sort of bounced back and forth between Missouri and Wisconsin. In Wisconsin, the focus was always Milwaukee. He was a tradesman, a shoe cutter, and at that time St. Louis and Milwaukee were both big centers of the shoe industry in the United States.

My earliest school recollections were in a Lutheran school in Milwaukee, it was called Grace Lutheran. It had kindergarten through eighth grade. I remember the names of my teachers, but the most, to me, impressive thing about it was that here in the late 1920s this school was already pioneering in bilingual education. Our classes were all in German one day and all in English the next day. So if you had remained in school there through the eighth grade, you would have been truly bilingual. Unfortunately, my dad got the urge to go back to Missouri (laughs), so I only had two years in the school there. As a consequence, I still retain some German, but it's far from being as fluent as it might otherwise have been.

We moved, as I said, back and forth until 1933. At that point in time, of course, it was the depth of the depression. Jobs were difficult to find and even more difficult to retain because so many places were going out of business, corporations in particular. Dad had been in St. Louis when he decided the grass looked a little bit greener in Milwaukee, and he went back to Milwaukee in, as I said, 1933. I was there then, in a sense, through the war years, although I was actually in the service, until 1947. Then from 1947 on I always lived somewhere else.

Milwaukee and the city had fairly profound influences, I think, partly because of the nature of the school to which I'd gone, partly due to the fact that Milwaukee was, then certainly, a very well run city, with a police force that was exemplary, despite its proximity to Chicago and all the corruption that was there. This never happened in Milwaukee as long as the father and son chiefs of police -- their family name was Laubenheimer -- ran the police department. You were safe in the streets anywhere in the city. There was just nothing that ever perturbed one.

It had such a large foreign-born population, most of whom had settled there in the period from roughly 1848 up until World War I. Of those groups, probably the Germans were the most numerous, but there were a lot of Italians, Eastern Europeans, Poles in particular. They had come, for the most part, to escape the rigidity of life in Europe, and they were all -- in retrospect, you would think of them as socialists. They were very much attuned to the notion that government should provide a safety net. As a consequence, from about the time of World War I until shortly after World War II, Milwaukee always had a socialist mayor. It was the only large city in the United States that could make that assertion. That was reflected in their attitude toward schooling, everything about the organization of the city.

Unfortunately, I don't think Milwaukee is like that anymore. It has fallen prey to the same problems that most cities do -- diminishing tax base and all of that sort of folderol. It was unfortunate that that should occur, but it was certainly fortunate for me to have grown up in a city that had that sort of attitude towards education, towards civic cleanliness, to police security, and all the rest of that. It was really fun.

As you might imagine being born and growing up basically in a small river community in Missouri, golly, it couldn't have been more traditional. I can't remember having met anyone who spoke any language except English. There were relatively few people in the community that even had foreign-born parents. Most of them had settled in Missouri shortly after it was purchased from the French.

It had been a state with sort of a mixed attitude towards slavery. The area in which I grew up was part of what was called Little Dixie, so it gives you some sense -- although slavery in Missouri never approached anything like in the Deep South because most individuals that held slaves had maybe two or three, something like that. It wasn't the kind of agricultural way of life that demanded large numbers. You didn't have tobacco or cotton or things like that, orchards, regular grain growing, things like that. It was rather small, although there were a fair number of families that did own slaves.

All of that, of course, led to, on the one hand, kind of a happy childhood. I don't mean the slavery aspect, but here you were also in a very small community. Everyone knew you, which had its plusses and its minuses, obviously. By the same token, they were always watching out for you. You could see that as intrusion if you wanted to, but really, I don't think that was their intent. It was the sort of business that the whole community was, in effect, an extended family. As a member of that family, you would be remiss if somebody got into problems and you had had an opportunity to see that that didn't occur. So you called -- I was always confused as a child, not knowing when someone that I was told was uncle or aunt was actually uncle or aunt, or these were honorifics because of long-standing associations with the family.

As a child I guess I was most fascinated by those members of the community that were strange, that marched to a different drummer or in some instances obviously were mentally incompetent. For the most part, it was the blithe spirits that sort of attracted me. I couldn't have spelled blithe then, I'm sure, and didn't really know what it meant, but it was the ones who were marching to their own drummers that were always the fascinating ones.

AM: Was there anybody in particular that you can recall?

WS: Yes. In fact, my -- although I didn't know him, my great-grandfather was one of these sorts of persons. He had gone to William Jewell College in 1854, which was really very unusual for someone to go to college at that time. He was a Virginian by birth. His family had moved to Missouri.

AM: Is this your father's or mother's grandfather?

WS: Mother's. Had moved to Missouri in I suppose the 1840s. As a young man, he committed himself to the clergy. He became a Baptist minister and was persuaded that he should go to William Jewel, which was a Baptist affiliated school, to learn enough Latin and Greek that he could read the original texts, and things like that. He was apparently a bright man, but definitely out of step with the times. When I learned of the courses that they took, my God, they were formidable as all get-out. They read Juvenal and Latin, and the Greek was equally formidable. Here was this -- you would have thought it would have been much more simple.

At any rate, he was one of those persons, and he sort of always was looked upon in the family as our eccentric. He died in an institution. Probably didn't really deserve to be there, but at that point in time society didn't cope easily with people who were too far from the median, and he was one of those. He wrote poetry, he taught school, he was vehemently anti-slavery and managed to get himself into some difficulties, both with his students and his board of education because of his stance. Because this was a place that was sufficiently ambivalent about the enterprise that they preferred not to talk about it. It was just ignore it. We all recognize it's here, we don't believe it's right, but let's not create waves. So he was one of those.

Then I had one that I do remember well. He was a second or third cousin, although he was an adult. His name was [Jerry] Douglass, spelled with two S's, a family name. He and his sister had a big farm near Frankfort, Missouri, which is about fifteen or twenty miles to the west and a little bit to the north of Louisiana. His sister was a very domineering person. (chuckles) Jerry was to be seen and not heard.

He loved children. They were, at the time, wealthy farmers. They had a car, and everything else, when most people even living in the town didn't have one. So whenever they would stop to visit at my grandparents' place, he would always sort of sneak out and take us kids for a ride in the car. Oh boy, was that ever thrilling. A Model T, old early sort of vintage, and most of the roads were unpaved. But it was really great sport, and the ending of the drive always came too soon, because he had to get back before his sister missed him. (laughs) She continued to run the farm until she was in her early nineties.

If nothing else, the one thing that I hope I have inherited is longevity, because it certainly has been the hallmark of many members of my family. A lot of them made it into their nineties. I don't know of anyone that made it to a hundred, but they got awfully darn close.

AM: There must be something with that part of the world. My family's from that part of the world.

WS: Oh, is that so. Whereabouts?

AM: Well, southern Illinois, but I know St. Louis very well because that's where we would fly into and then visit our relatives in southern Illinois. Well, to get a little bit back to your mother's side of the family, what did her parents do? Were they farmers?

WS: Yes. My mother's parents -- my grandfather both farmed and he was also a tradesperson. He was a barber, and when he wasn't barbering, he was running a truck farm. Through most of the years when I knew him, he wasn't barbering, he was truck farming outside of Louisiana. That used to be, again, a great source of enjoyment. We'd spend our summers there, even though this was like going two miles outside of town. (laughs) But it was so different than living in the town, which was pleasant but quite different from being on the farm and being able to run around and do whatever you wanted.

I would ride in with him most mornings during the summer when he took his produce into the stores to be sold. We'd ride in behind this horse on a wagon that was -- I don't know what kind of a load it would carry, perhaps a dozen or so bushel baskets. Then he would stop at all of the stores. Usually, I'd get a piece of gum or something like that as a gift. It was a great kind of learning exercise, and I could feel more grownup than I actually was because he'd give me the reins. The horse knew where to do and, I'm sure, totally ignored what I was doing. (laughs) The horse had made the trip so often that he knew where to stop and all the rest. But he tolerated me.

It was part of a childhood which, unfortunately, I don't think enough people have the opportunity to have anymore. Small towns had resonances of their own, so totally different from large cities. You had the opportunity to hunt or to fish, to swim in the creeks, to do all that sort of thing, to learn a measure of self-sufficiency that didn't come as easily in the city, where you were always more constrained by what you could do, where you could play. Your opportunities to interact with adults were more limited than they were in a small town. So I feel that I was fortunate in having that upbringing. I'm sure it's influenced me in far more ways that I actually realize.

I still go back occasionally, although I have no close living relatives in that part of Missouri any longer, but I still go back. I have so many who are buried there that I sort of feel an obligation to at least show up once in a while to maintain continuity.

It had been a fairly prosperous little town at one time. This region of Missouri, there was a lot of orchards. They did raise tobacco, but it was big league tobacco, most of which would end up in chewing or cigars. There were several cigar factories there at one time. All of these were ultimately bought out by American Tobacco [Company] and closed, like the little competitors who were there, so the tobacco industry sort of -- I don't know. By the turn of the last century it was pretty much dead in the town, but it had been an important contributor in the nineteenth century if not in the twentieth.

And of course, there was a lot steamboating up and down the river then. It was a major artery, and it wasn't dammed to the point that it is now, where you, I suppose, spend half your time waiting at a lock to go on up the river, or down as the case may be.

It was the sort of view of the United States that I think generated what Tom [Thomas J.] Brokaw has called the "greatest generation." Most of those people, even though they may have been reared in cities, a very large percentage of them had small town roots. They also had roots that came out of farming and agriculture. We don't realize, I think, the extent to which, in the last hundred years, we have a nation that's gone from probably ninety-eight percent of the individuals making their livelihood on the farm to less than three percent now. Agro-business was something that no one even knew. Most farms were a hundred acres, give or take, and they were intensively farmed manually. Few farmers could afford a tractor, even when Henry Ford came out with his cheapie after the First World War.

There was a lot of communal sharing. Threshing would be something that would be done by *all* of the farmers on a cooperative basis. Either one would rent, or in some way or another, get a thresher, and then they would all try to cover as much of the threshing as they could in a day or two, and then they'd move on to another farm and you were just sort of part of that circle of labor. In my grandfather's case, he always participated, but he never raised grains, he was always vegetables of one sort or another. Yet, you could not not participate. It was just part of the neighborly obligations that came to you.

I think that aspect of childhood spills over into the way that you deal with people. Early on, you are brought to the recognition that you don't do anything alone. There are always people back there who made it possible for you to do what you think you did by yourself. Or they have guided you in ways that are often imperceptible, so a thought that you think was yours originally actually stemmed from something that has come from someplace else. I don't see that as being bad or in any way diminishing the originality of individual minds. I think it's just a recognition of the fact that we are the sum and substance of the cultures that we've inherited, the environments in which were fortunate, or unfortunate enough to have grown up in, and that is what we are. To deny any aspect of that is really to be unrealistic. I can certainly -- and you, too, I'm certain have run into many people who believe otherwise, but it's not really an honest -- (laughs). But it hasn't disturbed me in any sort of way.

When we moved north in 1933, I was involved in a large city. Of course, we lived in St. Louis for only a year. We were in St. Louis from 1932 to 1933. I never really got to know St. Louis very well. Famous-Barr [Company] and Stix, Baer, and Fuller [Dry Goods Company] and all of the large department stores that were there. Not that I did that much shopping, but I did do a lot of window looking. (laughs) This was in a time when you were fortunate to have a full meal to sit down to, doing the other sorts of things. And of course, impressed by -- the Lindbergh Memorial already existed, Forest Park was a great place to go for entertainment. You had the Muni [Municipal] Opera, which was always free then.

St. Louis was a grand place, too, and in some ways it shared a lot of similarities with Milwaukee that maybe we don't think about. The South Side of St. Louis still in those years was predominately German, to the extent that during the Civil War, full regiments of Germans were drafted out of the south of St. Louis. Their officers had to speak German. They were 1848 migrants, and they hadn't really learned the language themselves, so you have all these German names still. I went to a school the name of which was Gundlach [Elementary School]. Where would you expect that? You certainly wouldn't expect it in Louisiana. (laughs)

AM: And why did your father move to Milwaukee?

WS: Job opportunity. From that time on, then, as I said, until really 1947, I was in Milwaukee. I graduated from Lincoln High School in 1938, in June. Immediately enrolled in Marquette University, which was the sort of situation that most of us whose families didn't have much greater means went to urban schools. You could live at home, tuition wasn't enormous as it is now, and there were part-time jobs that could be easily found, and whatnot. So I enrolled in Marquette as a premedical student. That's really sort of the direction that I thought I was interested in.

It was kind of a second course of action. My father, for reasons in retrospect I don't really remember, desperately wanted me to go to [United States Military Academy at] West Point. He had made the arrangements so that I could get the appropriate appointment, and all the rest of it. Then, in the ninth grade, I turned up horribly nearsighted, like twenty/two-hundred. In those years, you had to have twenty/twenty vision to even contemplate going there, so at that point I sort of dealt my father an unexpected blow, through no efforts on my own part.

Then I went to Marquette. As I say, I started out as a premedical student. In those years, Marquette still had a medical school. I don't remember exactly when they severed their relationships with the medical school. Probably in the 1960s, maybe 1970s.

AM: Although the medical school is still there.

WS: Yes. It's called the Wisconsin College of Medicine [Medical College of Wisconsin], but it’s physically no longer part of the campus. It's in quite a different portion of the city. It had been a good, although not especially illustrious medical school. I don't think that in the main in the 1930s there was probably that great a difference between most medical schools. Sure, Harvard [Medical School] and Yale [University School of Medicine] would point to their antiquity and to the belief that they were more outstanding than perhaps they actually were.

But all of the schools were primarily involved in training practitioners, so this, it seems to me, emphasized the need for instructors who themselves had had substantial clinical experience. It created a problem because many medical schools, perhaps only half their faculties were full time, and the others were in a situation in which it was probably a useful relationship. They could point to the fact that they were members of the medical school faculty, which was to their advantage from the standpoint of patients. But the medical school could also point to the fact that these were individuals who were, in fact, practicing medicine. It wasn't an exercise in theory, it was an exercise in what really was transpiring in medicine in those years.

I think I skipped one thing that probably was instrumental too I should mention. The thirties, of course, were still years in which childhood diseases -- diphtheria, scarlet fever, whooping cough, all of these things -- were commonplace. Shortly after we had moved to Milwaukee, I developed scarlet fever. In those days, almost all of those childhood diseases resulted in quarantining of the family. The city's public health department comes around and tacks this plaque on your door so that you couldn't open the door without breaking the plaque. (laughs) The plague is here.

They did have a children's hospital for infectious disease, a big one, in South Side Milwaukee, but in 1934 when I got scarlet fever, it was epidemic in the city, so I had to wait almost ten days at home, with my family being quarantined, until they could find a bed for me at the hospital and the family was free to move about again.

It was a funny sort of thing because I guess it attests to the fact that medicine, however more sophisticated it appears to be, has its shibboleths too. At that time, a lot of them seemed to deal with nutrition. What the arguments were have eluded me. You weren't allowed to have meat for the first fourteen days after the recognition of scarlet fever. I had gone through about ten days at home, and I was looking forward to the fact that I was going to get meat in the next three days. (laughs) Well, I got to the hospital and they rapidly diffused me of that notion, they started counting all over again. So I'd practically done my total thirty days of incarceration before I got around to meat again.

But it was another experience, a changing way of life. It was probably, in a sense, the first real time that I was cut off from my family, because your parents weren't allowed to visit you at that time. At the end of about seven days, or something like that, they could, but for the first week you were still viewed as potentially contagious, and they didn't want the parents spreading this around. So all my mother and father could do was to come to the sidewalk beneath the window of the ward in which I was and shout my name and kind of wave and offer encouragement. And that was it.

The only good thing about that period in the hospital was, we got unlimited amounts of ice cream. The other unlimited thing was spaghetti, without meatballs. (laughs) But those were happenings that were very deeply ingrained in the memory. I don't know why those things should stand out to the extent that they do, but maybe they were more traumatic than one really recognized at the time. I don't think as a child you realized whatever emotional strain you might have been going through. Well, heck, it was happening to everybody.

Of course, at that time, through most of the summers there would be problems with infantile paralysis. The swimming pools would all close, and so on. The only place that you could -- Milwaukee was still fortunate in the fact that you could go down to the lake and swim, and there were a lot of beaches there. Obviously, they couldn't be closed, but the pools and auditoria and so on that the city supported all shut their doors, and that was it. I don't know that it actually diminished the frequency of the disease all that much, but the horrors that were associated with infantile paralysis were such that parents were, I suppose, willing to accept almost anything that they were led to believe might diminish the prospects of their children encountering the disorder.

That was certainly a standard feature. I know I had whooping cough and I had scarlet fever, as I said, and I had measles. I went through the gamut of these things. Sometimes it was fun and sometimes it wasn't. Sometimes it was fun because you didn't have to go to school. But when it was whooping cough it wasn't much fun because your chest was always so darn sore from uncontrolled coughing that it wasn't much of a pleasure.

Well, to pick up the argument again. When I went to Marquette, as you might imagine, as a Catholic university, premedical students were obliged to minor in philosophy. Golly, while there, I had the full gamut, from logic through natural theology, with metaphysics and all the rest of them in between. Some of my instructors were really exceptionally well regarded at the time. One was -- well, right after the war it was Yves [R.] Simon, and I heard Jacques Maritain speak. Because the Jesuits -- and Marquette is a Jesuit university -- have the standing that they do in philosophy and matters religious, they could attract people of real standing. I know that our teacher of metaphysics and natural theology was a Jesuit by the name of Father Gerald Smith. If one's interested, his name is a distinguished one in the whole event. So we took that.

In addition, you had all of the usual sciences -- introductory biology and embryology and comparative anatomy and whatnot. In the process of that, when I had an opportunity to take an elective, I decided to take one in genetics, and that changed the whole future.

AM: So they did offer a course in genetics.

WS: They did, mm-hmm.

AM: Did they only have like a School of Sciences, or did they offer it through zoology or botany or --

WS: This was in the Arts and Science College. The instructor was actually a mycologist by training. His name was Eugene [S.] McDonough. He had gotten his Ph.D. at Iowa State [University]. He had worked with -- I can't remember his first name -- [Ernest W.] Lindstrom, who was one of the great names in sort of tomato genetics. Iowa State, at that time, was very heavily oriented towards plant husbandry of one sort or another, corn and everything else _____.

Dr. McDonough was a marvelous teacher, one of those sorts of persons whom you consciously try to emulate when you reach the point where you're teaching too. Because his lectures were spiced with a bit of humor, an awful lot of information, but presented in a way that wasn't conspicuously didactic nor sort of cold and impersonal feeling. He really interacted with his students. And he put most of us on to problems as early as he could in the course. I got a problem in Drosophila, and without really realizing it, in my third year after I'd had genetics, I began to feel I really wasn't sure that medicine was my bag and that genetics was terribly interesting. There was a quantity of nature to it; there was the prospect of being able to define an experiment which would lead to unequivocal results, instead of one of these damn things that you take on and you're never sure what you've got. At the end you have an enormous mass of data, but you're not really certain that you've answered the questions that you set about to answer, because of a variety of circumstances.

That was actually a far more important event than I realized in the time, in several respects. First of all, it meant that when I was admitted to medical school, I decided to turn the admission down because I didn't really think that that's what I wanted to do. Well, this is 1942. At that point in time, I'd been in what was called the Enlisted Reserve Corps, and as long as I would have stayed in medical school I would have been exempted. Once I turned it down, I was in service quicker than I knew what was going on. Ironically, because of the nature of the training that I had received, I spent the war years with what was known as the medical department. This is the enlisted arm of the medical services, and I spent the better part of three years with the Thirty-seventh Infantry Division in the Pacific.

It was -- I don't want to say a rewarding experience, but there was no question but what it was a momentous one. It's a life-changing sort of experience. You could no longer sort of drift. You had to define where you wanted to go, what you wanted to be. And you also realized that the world was never going to be the same as it had been before. The happy days of going to school and doing odd things, and so on, were of the past, and they weren't going to recur.

So I ended up being what was at that time called a T3. This is a technician third grade, which roughly was the equivalent of a staff sergeant. I was the head of the enlisted portion of a surgical team. I managed a table, did most of the -- actually, acted as the surgeon's assistant most of the time. Had this been in the nineteenth instead of the twentieth century, I could have hung out my shingle. (chuckles) You did everything from relatively minor debridements to amputations to the whole cotton picking business you were involved in.

I worked with some very fine surgeons. I was part of what was then called a clearing company, a bit of organization. An infantry division had a medical battalion, and the medical battalion consisted of four companies. By the time of World War II, infantry divisions had been triangularized. As I said, there were three combat teams. This would consist of a regiment of infantry, associated field artillery and all the rest of it. Each one of those combat teams had a collecting company. The collecting company was one part of the medical battalion. A collecting company basically were ambulance drivers, and their job was to pick up the wounded at regimental medical centers and bring them to the clearing company, of which I was a part.

The clearing company -- I think the best analogy is it was like M*A*S*H. We were the most forward unit capable of doing major surgery. In places like the Philippines, we always operated as two platoons -- the company was split in half -- because we always had to be close enough to the front that evacuation could occur and surgery be initiated before the individual died of lack of attention. So we'd hopscotch over one another. We'd be moving about every third day. When we'd first go move forward, we were always within _____ and mortar fire and whatnot, but that would pass us, the front would move on. Then after we'd moved far enough, they would jump ahead of us.

There are a lot of recollections of it that I think are so deeply engraved, I'll never forget them. Some are humorous. Most of them aren't. I certainly would not want to experience that again, but in a way, it taught me a lot that -- God, there has to be a better way to learn it than that way. You entered into another class of relationships with your fellow soldiers in this instance, but your fellow human beings. Up until the time that we got to the Philippines -- I joined the division right at the end of the New Georgia campaign, and then through Bougainville campaign and to the Philippines. We were one of the assault divisions that landed in January 1945.

Up till that point in time, I had -- well, entering the army at the time I did I wasn't already a part of a defined unit, I was a replacement, so as a replacement eventually ended up, as I say, with the Thirty-seventh Infantry Division. It was a division that saw a lot of combat at the time. I don't know whether you've ever heard -- there was a song that was very popular during the war about Rodger Young. Rodger Young was a conscientious objector, an aid man, with one of our infantry battalions, and he was killed on New Georgia and won the Congressional Medal of Honor for it. I don't know who it was that wrote the song, but it was a popular encouragement not to enlist, and whatnot. In the course of the war, I think we had eight Congressional Medals awarded, about half of them posthumous. I knew several of them, in the sense of in some instances I treated them. I was involved in helping them put their parts together again.

It was my first experience with another culture too, really. Not with one, but for practical purposes. When we were in the Solomon Islands and so on, we had very little interaction with what natives were there. They didn't want to be in the way of harm and trucked themselves off into the boonies as far as they possibly could so that they weren't caught between our fire and the Japanese fire. So it wasn't until we got to the Philippines that we really, first of all, saw urban warfare, because Manila was, for practical purposes, the only real urban warfare in the whole Pacific.

Then a culture, predominately Catholic, that spoke Spanish and dozens of local languages. Often you moved ten miles and you were out of one local language into another. The Lingua Franca was either English or Spanish because most of the Filipinos could speak English. Schooling had been obligatory, at least up through probably about the sixth grade, so the bulk of them were exposed to enough English to be able to communicate with us in that sort of way, and for us to communicate with them too.

The result was, for the first time you really saw civilian casualties, and in many ways those are the most troublesome, because it seemed inappropriate. We were -- well, we didn't choose necessarily to be where we were, that was our job. We were there to arrest this land back. In the islands, you never really thought about civilian casualties. They just didn't occur, or they were so rare. But in the Philippines, that ceased to be true. Sometimes they were children, and that was very hard, very hard.

AM: Were you treating them, as well as G.I.s?

WS: Yeah. On the basis of triaging, they were pretty much folded into the program depending upon the seriousness of their wounds.

But the war ended. I must say, even though I've spent a large portion of my life subsequently trying to study the consequences of exposure, I didn't weep when I learned that the atomic bomb had been dropped. We had already been -- the fighting in Luzon had essentially ended about the fourth of July 1945. So we were pulled back, our division, to Cabanatuan, which was where the infamous Japanese prisoner of war camp was, to replace our losses, to get ourselves ready again -- losses both in material and in personnel -- to get ourselves ready for the invasion of Honshu.

As we knew it, the scenario was that the first invasion would come in November of '45. That would be in Kyushu, probably on the east coast of Kyushu somewhere around Noboribetsu. Then in January of '46, the invasion in the Tokyo area in the Kanto plain. The invading force would be a full army with another army in reserve. We would go in -- we had been part of the Sixth Army up until that point in time and had been transferred to the Eighth Army. The Sixth Army had been commanded by Walter Krueger, and the Eighth Army was -- I can't remember what [Robert] Eichelberger's first name was. He was in command of the Eighth Army. The First Army, since the war had ended in Europe, was being brought to the Pacific. The First Army -- I guess that was [Omar N.] Bradley's army -- was to be our backup.

The expectations were really horrendous. Fifty percent casualties is what we were being prepared for and what, as a medical unit, we had to be supplied to cope with that. The assault waves were going to be of the order of a half million **_____**. When you talk about fifty percent casualties, that doesn't mean they're all going to be killed, but that's a slaughter of a sort one just doesn't like to see. We knew it wasn't going to be easy. We'd already learned from Iwo Jima and Okinawa that the Japanese would fight to the bitter end and that there would be a lot of losses, both on our part and on their part. And to the extent to the atomic bombings led to Japan's capitulation earlier, it undoubtedly saved more lives than it cost. But those who have, I think, debated its ethical justification are those who didn't have anything on the line at the time. Those of us who were staring at these statistics were just all too pleased to see the war end, whatever brought that end about.

AM: And you were in the Philippines when the bombs were dropped.

WS: Right.

AM: Did you have any indication that there was going to be some kind of new weaponry introduced?

WS: No, we didn't. When we heard about the detonation of these devices in early August, we had had no -- nothing had trickled down to us, at any rate, that foreshadowed a totally new weapon. I'm sure that, obviously, it wasn't unknown some places, clearly to the crews in Tinian and Guam. But to us, it was. Then when in the middle of August the Japanese capitulated, we began to get ourselves ready to come home. But because there were so many to be brought home, we were brought home on the basis of the number of points that we had accumulated. The division -- what was left of the division -- left the Philippines in -- it must have been the end of November, first part of December 1945, because I got home on Christmas Eve. Really a fortunate turn of events to get home at all at that time.

My brother **[John Schull]** had been in the Atlantic Theater with the British Eighth Army. He was already home. And my sister **and** her husband and family had come from Philadelphia where they were living. That was the first time in, oh, four years that the whole family was able to get together. We were fortunate in that all of us *were* able to get together. Of course, as soon as possible I had to --

AM: You went home to Milwaukee.

II. Graduate Studies; Research on Radiation in Japan

WS: Yeah. I had to start thinking about -- well, I had left Marquette sort of midway in my fourth year. Had the army delayed calling me up for another month, I would have automatically have gotten my degree. But I didn't quite make it to that point, so I was going to have to repeat the last semester. That I did, and I stayed on at Marquette to get a master's degree and in the meantime began to think about where was it that I wanted to go for graduate studies.

At that time, all my work up to that point had been with Drosophila, so I was in effect trained as a Drosophila geneticist. But I'd gotten interested in human genetics, too, and I wasn't really sure which one of those two paths I wanted to follow. When I started shopping around for a school willing to accept me, the two that I chose were Columbia [University], because of [Theodosius] Dobzhansky, and Ohio State [University], because at that time Laurence [H.] Snyder was still there, as was Madge [T.] Macklin and David [C.] Rife. They had more of a group of people interested in human genetics than any other school in the country did then.

I wrote to Snyder. I wish I could have said that my decision was made for me because only one accepted me, but in fact I was accepted at both places. (chuckles) I was married by that time, and the whole business of accommodations and whatnot loomed. Also, the matter of how far in the two different environments would the ninety dollars a month you get take you. So there was obviously important economic considerations.

In the final analysis, I think I could have made it in either place, but human genetics loomed as more interesting to me and I thought held greater promise than did Drosophila genetics at the time. Drosophila genetics was then still -- this is the exciting period of the early thirties when -- well, let's see. The exciting period from the end of World War I until the onset of World War II had seen the definition of sex linkage, linkage ______ as such, the recognition of the giant chromosomes of the salivary gland and the physical mapping of those chromosomes.

All of this had come about, and much of genetics then -- Drosophila genetics was really focused upon -- well, the interesting parts were focused on genetic diversity. Of course, Dobzhansky was a major figure in that. Otherwise, it seemed to me that Drosophila had sort of settled into much of what's going on in human genetics now, just adding more and more genes to the chromosomes that were already known to exist. There wasn't anything that represented some sort of new deep intellectual breakthrough. This was -- I hate to see this word perfunctory, but it was very perfunctory kind of science.

At any rate, I decided to go to Ohio State.

AM: What did you see yourself doing in human genetics, and this whole idea that we could even study human genes at this point?

WS: Well, I suppose in part the very fact that the opportunities were unknown made it more of a challenge. It was clear that it was beginning to gain some momentum. It obviously didn't have the cache that it's got now. I firmly expected I'd probably end up teaching introductory genetics and maybe have the opportunity to offer *a* course in human genetics somewhere along the line, but that I hoped the research that I'd be involved in would be efforts to understand the transmission of genes in human populations.

As a consequence of sort of the nature of the times, my training at Ohio State was heavily oriented toward statistics, even though I obviously had all of the genetics I could get. And it's fortunate that Madge Macklin was there. Though she didn't actually offer courses, she had her ensemble in the same large room where us graduate students were housed, and we could talk to her and she would share with us things that were happening in her research and so on. It was kind of an indirect apprenticeship.

Ironically, by the time I got there, Snyder had gone. (chuckles) Larry was always an administratively ambitious man. I don't mean that unkindly. He had been chairman of what was the Department of Zoology and Entomology, which is where all the geneticists were housed in Ohio State, and from that went to University of Oklahoma as dean of their graduate school, and from there to president of the University of Hawaii. So though I got to know him well subsequently, it wasn't as a student I got to know him really.

So with his departure -- formerly, my professor was David [C.] Rife, who most people in human genetics wouldn't even know of today. Human genetics was more sort of a sideline of his. He had been trained as an animal husbandryman and had done a lot of genetics on cattle, but was always interested in human genetics. He had written a little popular book called *The Dice of Destiny*. I don't know when this would have been published, certainly probably very late thirties or forties right after the war. It was an effort to sort of spread the gospel, as it were. I think it must have sold reasonably well at the time. I don't think I even have a copy of it anymore. I must have had at one time.

That, in large measure, stated what was really going on. The most exciting areas, obviously, were in serological genetics because more and more surface antigens were being discovered. This continued throughout my graduate training. There really had been very little of a biochemical nature short of what [Archibald E.] Garrod had published in his *Inborn Errors of Metabolism*, but nothing like the isozymes [isoenzymes] that were to emerge later, largely beginning with haptoglobin. So you felt like this was a big void in which it would be almost impossible not to write your name in some sort of way. Maybe not in a way that would be as lasting as you would hope, but there were opportunities here.

Everybody was understanding. I think my favorite story to emerge from that, when I graduated in 1949, human genetics was perhaps taught at not more than one, possibly two medical schools in the United States. Duke [University] largely because Larry Snyder had been there before he went to Ohio State, and he had started this program, but it was all fairly low keyed. It wasn't an obligatory course, it was an elective, and of course most of the students elected not to take the elective. (laughs) So there were very few in the class and you obviously weren't having very much of an impact.

At any rate, the lack of a cache meant -- as I said, I expected to be teaching general genetics, possibly even some courses in zoology since I had all that background too. I accepted the fact that *where* I would like was uncertain, so in the spring of 1949, two events occurred. First, I received a letter from [J. W.] Boyes, who was then chairman of the Department of Genetics at McGill and who David Rife knew. I presumed it was through Rife's acquaintanceship with him that Boyes had learned that I was about to graduate, and they were in need of someone. I received a lengthy letter from him offering me a position as an instructor, a rank that's virtually disappeared anymore, at McGill at a salary of one thousand two hundred and fifty dollars Canadian and two hundred and fifty dollars moving expenses.

Well, fortunately, Madge Macklin was there, and I asked Dr. Macklin could we live, my wife and I, in Montreal on that sort of money? She says, "Jack, you can. It'll be bigger bones, but you could live at that expense. But it would be better if you got somewhat more." (chuckles) Actually, on the basis of her evaluation, I wrote back to Boyes -- I think his initials were J. W. Boyes -- thanking him very much for the offer, expressing my appreciation for it, but that I really wasn't interested. I think he interpreted that to mean I was negotiating, so within a day after he would have received my letter, I get this telegram from McGill, from Boyes, offering me a position as an assistant professor at fifteen hundred dollars a year, but no moving expenses. (laughs) All I was actually doing was upping the title that I would start at.

In the meantime, however, Jim [James V.] Neel had begun to recruit people to work on the radiation studies in Japan. I had gone to Ann Arbor [University of Michigan] to talk to Jim. I had met him briefly earlier, but really this was the first chance to get to know Jim. I was intrigued by the offer for a variety of reasons. First of all, it was financially better, which wasn't a non-trivial consideration. Secondly, I was very much impressed by Jim. And thirdly, it involved a study which intrigued me enormously because, of course, by that time I knew much more about the effects of ionizing radiation, the genetic effects. Muller had received a Nobel Prize for it, and there was more of a greater consciousness of what was involved.

Then I guess I'd always felt a little bit shortchanged by the fact that having spent so much time in the Pacific during the war, I never got to Japan. So here was an opportunity to go -- it was on a two-year contractual basis -- to Japan, and it would solve a lot of these things. I'm sure I left with the hope that if I did well, Jim would help me find a suitable position on completion of my term in Japan.

This was another one of those milestone periods because it brought about a change which certainly at the time of graduation I never would have envisaged, because after the end of the first year in Japan, Jim asked whether I would consider coming and joining him at Michigan when the second year was up. Well, I mean, this was just a phenomenal offer because, as I say, by that time I'd gotten to know Jim well. He was one of the best researchers I've ever known in many, many ways. Very demanding. Very sensitive, though. He wasn't a Simon Legree sort of person at all. Jim just believed very strongly that you always did the best you could possibly do, even on the most trivial problem, and that you shouldn't shirk. He also had an uncanny capacity to judge people and to anticipate the direction of science.

So the thought of going back to Michigan when he offered me was phenomenal. This was going to solve my uncertainties, because one of the things that had been negative about going to Japan at the onset was the recognition that this study was at such an early stage in its development that there would be nothing that I could publish in the period of time I would be there, and I'd be stuck out here six thousand miles from what was going on in genetics in the United States, and without any of the credentials that would usually be looked for in recruitment.

AM: What was your sense of the opportunities, though, that this new kind of data set, so to speak, would --

WS: You see, the thing was at that point, I guess, these other factors kind of overweighed perhaps deeper thought than I had actually given it. I assumed that in some sort of way something would come out of it to which I could point. I knew full well that the study, when it really began in the spring of 1947, hadn't yet actually examined enough infants to have any kind of a conclusion possible, and that at the rate we were examining infants, even two more years of work wouldn't bring us to that point. But there was always the hope that, well, odd things turn up in a large survey of that kind that you can utilize to achieve some recognition.

But I'd become interested, I think, largely because of Jim's concerns, with the whole issue of mutation, and if there was a single sort of solitary area that I think one would identify with Jim Neel, it would be the whole business of human mutagenesis. Not just radiation-related, although that was part of it, but spontaneous rates of mutation, all the rest of it. What he was, in a sense, offering me, largely I guess on the basis of what he saw as a creditable job in Japan, was the opportunity to participate in some of the spontaneous mutation rate studies. These were what I was involved in.

Also, given the background that I had then, I was _____ to share with Jim some of the burden of running that study. Jim spent about a month each year in Japan from 1948 through to 1953, and then I went out in '54 to close it. He very much was involved in a hands-on relationship with the staff and with the study itself. So all of us who were involved learned from it, including Jim. Things would arise, as you might anticipate, both of a scientific nature and of a political nature as well. Running a large study of this kind in a defeated nation with the enormous economic stringencies that obtained in Japan was no simple task. One had to be flexible, recognize that certain adjustments were inevitable, and you tried to make those adjustments kinds that wouldn't in any way impinge upon the basic study itself. That wasn't always easy to do because you were trying to look at a pretty foggy glass bowl as to where things were going to go.

As I said, it led to a commitment to the studies in Japan that continued long after -- I don't want to say I ceased to be interested in the genetic aspects, but it encouraged the involvement in other aspects of the studies in Japan, particularly carcinogenesis, brain development and the like have been things that have kept me going long after I left Michigan and became involved in the studies in a quite a different -- obviously, I'm more administratively oriented in a way, but fortunately, through most of the time I've been there, my administrative responsibilities still allowed me time to actually be involved in the science, which was great.

That's sort of an overly long recounting of how I ended up getting to Michigan in 1951. (laughs)

AM: Okay. Well, I think we've covered a lot of ground today, so I think we're at a good place to stop.

WS: Oh, yeah.

AM: And we'll pick up tomorrow.

WS: Okay.

AM: Thank you.

[end session]

III. Family Background; Thoughts on Education, Science, and Religion

Session II
June 28, 2005

AM: It is the 28th of June 2005. I'm Andrea Maestrejuan with William Schull at his office at the University of Texas School of Public Health to continue his interview for the UCLA Human Genetics Oral History Project. I just wanted to start basically with some questions from yesterday. I'm going to take us all the way back to the beginning, and I just wanted to talk a little bit more about your upbringing in a couple of areas. I wanted to ask a little bit more about your father's family background. You had mentioned that he worked in the kind of shoe industry. Was he from a family that had craft skills? What was his background?

WS: That's hard to answer. Actually, I think the family background was probably in agriculture. My dad's family, his immediate family, his father, were raised in Illinois, around Vandalia, and they were farmers. So my grandfather Shull was a bit of everything in the course of his life. He had farmed as a young man, then he was a pilot on boats on the Mississippi, and then he was what was called a stationary engineer. He ran the town water system, the sanitation system and so on, for Louisiana later in life. So he was a bit of everything.

It was the age in which jobs were open to anyone who was prepared to do what was involved. You didn't have to have certificates, accreditation, and all that sort of stuff to even be able to submit a resume. You just needed a job and you were willing to do what was necessary. So that was kind of the tradition in which he had grown up.

I trust this isn't too peripheral. The Scholls -- and the original spelling of the name was S-c-h-o-l-l -- came to the United States around 1740. They were from the Rheinpfalz. They settled in Pennsylvania, not far from Harrisburg. Over time, that name got permuted in a number of ways. It became S-h-u-l-l, it became S-o-l-l, it became S-c-h-u-l-l, and of course it retained its original spelling. Interestingly enough, although they were remotely related to me, George Harrison Shull, who is the father of heterosis, is a distant relative, as is Tibby Russell [Elizabeth S.], who was a Shull originally. Her father was A. Franklin Shull, who taught genetics at the University of Michigan prior to my arrival on the scene.

So the Schulls are a fairly big clan. The confusion of all of this was first pointed out to me when I was at the University of Michigan because there was a member of the German faculty there whose name was William Scholl, spelled S-c-h-o-l-l, and upon his death, the obituary indicated that his brother was A. Franklin Shull, spelled S-h-u-l-l. Then it turned out that William was interested in genealogy, and he published before he died a huge tome that deals with the Schulls from as far back in Germany as he can pick them up. Since he was German, obviously -- he was German speaking at any rate -- he had no difficulty with the language, and then carried it on through with all of the permutations, and so on, of the family.

We know that they went from Pennsylvania, some into Virginia, which is where my branch went, Pennsylvania to Virginia. Then across to Ohio, to Indiana, and then into Missouri. In Tibby Russell's case, they went from Ohio to Michigan. (chuckles) So they pretty much mid-U.S. sort of thing. That's the story in a nutshell of the spelling and the fact that we _____.

AM: That's interesting that -- you had mentioned yesterday that your name had been changed when you got to Milwaukee just because everybody assumed, so it actually was mutating back toward what it was.

WS: It was getting closer to what it originally was.

AM: Okay. Well, you had mentioned yesterday that your great-grandfather on your mother's side had received a college education. Did anybody else in his family pick up that what was seen as an idiosyncratic pursuit?

WS: No. I think probably the only reason he went to school -- he didn't complete the degree program, because after a year, year and a half, he dropped out figuring that he had learned all that he was going to learn that was relevant to his interest in becoming a minister.

His father, I suspect, was illiterate, because at least the documents that I've ever been able to see usually have an X made. Whether that means that he wasn't that literate or these were at ages in which he really could no longer sign his name, I don't really know. These were documents late in his life. They came out of Spotsylvania County, Virginia. That's where that branch of the family came from. The Davenports had been -- his name was Davenport, a family name -- had been in Virginia for a long, long time. There were Davenports even in Williamsburg. I don't know if they were all related, but at any rate, the Davenport name had a certain measure of conspicuousness in Virginia at that time. I'm certain that his father didn't go to college.

I'm not aware really offhand of anyone doing this again until my grandfather's brother's two children both went to college. They are my cousins, but actually of my mother's timeframe. One was a professor at Northwestern Medical School. This is Harold Schull. And his sister, Delphine [Schull], was the product of Washington University in St. Louis. She had gone on into education and spent much of her life as a teacher.

Interestingly, there's an anecdote that's told. I have no way of verifying this, but it's such a story that it doesn't seem likely to have been fabricated. She was in school in St. Louis at the end of World War I. Henri Poincare [Raymond Poincare] made a trip to the United States, and she was delegated to show him about the campus. (chuckles) So when she was about nineteen or something like that, here she is with this astute famous Frenchman. She loved to tell that story. I'm sure it's real, but probably not as noteworthy as I just made it. (laughs)

AM: Well, did your mother work outside the home?

WS: No.

AM: What kind of expectations did your parents have for their children in terms of what they should be doing with their lives?

WS: Certainly, both my parents felt it was important that we be educated beyond the levels to which they had gone. My father's formal education ended in the third grade. He was of that time in which when a boy got big enough to help the family's finances, most of them did. So they would drop out of school. He essentially made it up later with sort of what would be the equivalent of the GED [general education development] and was always enrolled in some kind of -- generally a correspondence school, LaSalle [University] and all the rest of the -- but he didn't want us to make that sort of _____ called in retrospect was a mistake, I thought. At the time he was one of six children, and living was far more difficult then than now. He was a person who would pick up that sort of sense of responsibility for his family more deeply than maybe a lot of children.

So he wanted us to go on. That's why, as I mentioned, he planned on me going to West Point until heredity caught up. (laughs) My brother is a microwave engineer. He was with the National Security Administration [Armed Forces Security Agency] in Germany tracking Russian rockets.

My sister went to art school. She was the one who was the artistic one, involved in the theater and was quite adept with painting and things of that nature. But she didn't have a formal degree of any sort. She had studied at the -- I think it's called the Art Institute of Milwaukee, after she got out of school. And she was very much involved in the theater. She was part of what was known as the Wisconsin players. She always was a source of fascination to me and my brother. My sister had -- well, I don't know how to put it. She always was dramatic. (chuckles) She'd go around practicing these gestures in the house, and so on, and my brother and I would think that she was losing her marbles. But this and all the formality of theater.

I think the way live theater operated then was so much different than what people do today. They really had to learn to project their voices to an extent that really isn't necessary now with all of the ____ that allow a weak voice to sound like a cannon. And you learn gesturing and all the rest of that, and you had to school yourself, you know, the gestures had to be above the waist and all this kind of stuff. She was very dedicated to all those things and of course would always -- when trying out for some role, invariably they would be asked to do something to demonstrate their skills. My sister was the kind who would always have been doing something like Lady Macbeth's soliloquy. Either that or she did an equally famous one that was done by Eva _____ in *Le* _____ when the young prince is searching the battlefield. I don't know why she always picked those, but it was a sense of -- my sister was made for the theater. (laughs) She loved it and never throughout her life ever really got away from it, although she had four children and was a housewife most of her life. But the theater was part of her.

So that's kind of the history of the three kids.

AM: Were you the oldest?

WS: I'm the middle one. My sister was the oldest, I was the middle, and my brother was younger. Interestingly enough, my sister was four years and two months older than me, I was four years and two months older than my brother.

AM: That was pretty good family planning.

WS: Yes, that's right. (laughs) I'm not so sure that that was -- but that's what happened, at any rate.

AM: When your eyesight kind of prevented you from going to West Point, did your brother get the pressure to go to West Point?

WS: No. I think by that time Dad realized that you couldn't engineer those things (laughs), with confidence at any rate. You could try, but -- see, the war came along while John was still in high school, and he went off to the Marine Corps without my parents' permission. Then was injured and was invalided out. But he wouldn't set still, so he learned of the American Field Service and signed up to drive ambulances for the army, and he drove ambulances in support of the Gurkha . Then when he came back, when the war was over in '45, he was then nineteen and finished his high schooling and went to engineering school. So that was sort of *his* story. Then with the convoluted set of events which ultimately led to him being in Germany and involved -- he was an employee of Philco [Corporation], who had big government contracts that had to do with tracking apparatuses and things like that. So that was the story.

Other than that my father wanted John to go on to school, he hadn't chosen his career. I guess all of us thought that -- my brother was always very handy with his hands, much more so than myself -- that engineering was a logical thing for him because he could fix anything, it seemed. And he was very good at the kind of visualization that engineers have to have, circuitry and all that.

AM: So what were your skills growing up?

WS: I don't know, I suppose just being affable. (laughs)

AM: Well, it sounds like your education was a little bit -- you started going to school in Milwaukee at the Lutheran school, and then you moved back to Louisiana, Missouri. How would you describe your education? It seems to me like it may have been disrupted many times and had varying degrees of quality.

WS: Actually, I don't know that there were such striking differences between schools in the large cities and in the small towns at that time. We all learned our reading, writing, and arithmetic in much the same way. Much of it was rote. Much of it was sort of the traditional way in which students were both disciplined and taught. I suppose the primary difference was that, even though you would have substitutes and changes more often in large cities than you had in a small town, many of the primary women who did the teaching were locals in the sense that that town was their home, so it didn't change the way it might be if you were in Milwaukee and they'd be teaching in one school one year and another school another year. And you might bounce back and forth between the parochial school systems because Milwaukee had two big parochial school systems. There was the Lutheran system and the Catholic system. Then there was, of course, public education. So a qualified teacher could easily move amongst those and there didn't seem to be any big problem.

I think in some instances perhaps there was a period of time, particularly in the thirties after the Depression, where there was maybe an attractiveness to being in the parochial school system, primarily because the funding situations in most large cities would be *very* bad. Gosh, I know teachers in Milwaukee were being paid with what were called baby bonds. These were like Confederate currency. You didn't know whether a bank was going to honor it or not. Whereas, if you were teaching in a parochial system, and if you got paid at all, it's going to be in cash. (chuckles)

I wouldn't say that I thought the quality was that great. Perhaps it would have been more noticeable not at the elementary level but at the high school. Of course, most of our back and forthing went on between the time that I started school and seventh grade. I was in junior high school by the time we moved back to Milwaukee, and from then on I was in a large high school. Lincoln had about two thousand students, so it was almost half the size of the town that I'd come from. And scads of teachers. Whereas, the high school in Louisiana then probably didn't have more than ten teachers, if that many. So you'd have one teacher teaching several subjects, not each one a specialist, in a sense. So that would have been different, but I think in the elementary level, I don't really believe -- I certainly didn't notice anything when I moved from St. Louis, where I'd been, as I mentioned, at Gundlach, to Milwaukee. I didn't notice any shift there. As I say, I think it's that you wouldn't really notice it at the elementary school level then. There wasn't as much experimental teaching or anything like that going on that might have struck differences.

AM: What about just getting to know new people and making new friends?

WS: That probably was the most novel part of that growing up period, because of course when we were living in Missouri, and in Louisiana in particular, most everybody in the town had family histories not so terribly different from our own. The biggest employer in town was Stark [Brothers] Nursery. The original John Stark, was a sort of Johnny Appleseed kind of guy who had settled in that region of Missouri in 1820 and brought the science of apples from Ohio, from whence he had come, and established this big nursery.

Almost every other family was very much like that. They were people who had been in the United States for a very long time. Two of the biggest organizations in town were the DAR [Daughters of the American Revolution] and the SAR [Sons of the American Revolution], because virtually everybody qualified. (chuckles) So that was different.

Then to go to Milwaukee and grow up in an area which -- we lived on the east side of Milwaukee, which at that time was sandwiched between two primarily Italian areas of the city. Milwaukee was then -- it's not so much -- it didn't become so much so after the war when the movement to the suburbs began in earnest, but up until that time it had been a structured city like almost every large one in the United States. The Germans lived in one area, the Poles lived in another area. The other Slavs lived in another area because the Slovenians and the Czechs and the Slovaks and so on weren't that cozy with the Poles. So you had these sections.

I happened to live in an area which was between two Italian groups, so even though our principal used to like to brag about the fact that there were something like twenty-five different ethnic groups in our high school, we were primarily Italian. Most of my friends I grew up with were Italians, some from Italy proper, others from Sicily. There were a lot of Sicilians. You learned to cuss in several languages. (laughs)

AM: (laughs) That’s always a handy skill to have.

WS: And you adapted to the cuisine. As kids, we'd be invited back and forth to the home. Or if you were with somebody else's son and he had to go home for something, you were likely to be trailing along, and if he was to get a sandwich, you got a sandwich. That was just the way things went. You were much more involved, I think, in a way. Of course, we all had a jargon which kind of united us. Even though today it would probably be seen as -- well, certainly not viewed as correct. You had Abyssinians and Dagos and Poles and all the rest of it, Huns. But it wasn't so much the word that you used, it's how you used it which determined the thing. You'd call your best friends by one of these names, and no umbrage was taken, but a different inflection of the voice, and you better have your fists up because it was fighting language then. But it wasn't the sort of thing that people felt intimidated about using, words that had been part of your vocabulary from -- just been absorbed.

I don't think that would have happened if we'd stayed in Louisiana. But in Milwaukee you certainly had that because of the variety of origins of people whose children were going to that school. That was an enriching thing. I don't think I really realized how much it was until later in life, but certainly in retrospect I'm very pleased to have experienced that. At the time I think there was fewer strictures imposed by political correctness. We weren't always cordial, but there was more naturalness, rather than you didn't worry first, before you said anything, for fear that it might be taken in the wrong context. Whereas, then you just spoke your piece. If somebody didn't like, they reacted accordingly. If they did, it didn't matter what you'd said so much as the fact that you were clearly friends and nothing was at stake. That part was interesting.

As I say, they were competitive, because most of them were coming from families with backgrounds not so terribly different economically from my own, and they all saw schooling as the ladder to economic success. So the schools were competitive, more so I think than they might have been in a small town, not only because there were just more students and, therefore, a higher probability that you were going to have some really bright ones about you, but the driving force. In a small town, you sort of had a great deal of cultural baggage that went with you, with the origins of your family and all that sort of stuff that you couldn't really escape.

AM: Well, what kind of student were you?

WS: I was a National Honor Student. Probably didn't do as well as I could have done.

AM: So that you did you not have to try very hard?

WS: That was it. I didn't. I certainly was blessed -- and to a large extent I think that's still true, although I notice it failing -- with a very good memory. If the examinations were not too different from the lectures, I could get by on listening to the lectures without ever cracking the book.

AM: That's just not fair.

WS: (laughs) Now, if the exam was something else that you could only get by reading, then I might not do so well. Although I read voraciously, but not necessarily the things that the school wanted me to read. I'm talking really about textbooks. I didn't enjoy that. But I did enjoy reading, so I read a lot -- history and _____.

AM: Were there any teachers or particular subjects that were intriguing you in high school?

WS: Yes. There were things about it that opened my eyes to certain characteristics of good teaching and what literature can actually mean. I remember one in particular. We had a substitute teacher, and we were studying the poetry of Edgar Allan Poe. You'd both go through the formal structure of the poem and all the rest of that, and usually, one of the students would be asked to read. I remember in this particular case I think it was a young woman who was asked to read, and she read words. The teacher stopped her and said, "No, no, no." Then, [dramatically] "Ring out wild bells." The conviction and, for the first time, the whole sense of onomatopoeia came through because she really *read* that, imitating the sounds that obviously were in Poe's mind. Whereas, most of us saw words and we read words. There was no spirit infused into it. Whereas, she did.

That occurred in lectures with other teachers, but that one, largely because I guess it was the first time, poetry became interesting to me. Prior to that, so it rhymed or it didn't rhyme, that wasn't all that exciting. But when you could infuse those words with the imagery that she could provoke. She was very good at it. I don't know that most teachers could do it that well. I think that was partly because she had a resonant voice, with which you are usually blessed. Also, she was very deep into literature, to the point that it was almost a passion. So it was easy. We were always delighted when she had to show up for our regular teacher, not that the regular teacher wasn't good, she was very good, too, but not as good as this gal was, in the poetry particularly.

I remember my speech teacher was a -- oh, I suppose she was maybe a fifty-ish woman then. Her name was Vilma Boyle. She too had one of these marvelously tempered voices, and she would do for us things from various plays. Invariably, she'd sort of stand up, like she was gathering her emotions, then she'd look up and she'd close her eyes and out would come this dialogue which was just absolutely phenomenal. She wouldn't tolerate sloppy speech. She'd been my sister's speech teacher, too. We had broad Missouri accents, and that wasn't going to fly. When we'd come up with -- instead of saying cow, we'd say caow, boy, she'd stop us. (laughs) "No, Jackie, don't say it that way. It isn't spelled c-a-o-w." That sort of set a standard that you could emulate.

Like my sister, I was involved in theater but not in the acting part. I got a lot of kick out of being part of the stage management crew so knew most of the people that my sister had known too, largely as a consequence of her.

But those sorts of people stand out still quite clearly in my mind. I guess, in a sense, are really career forming. It wasn't until I got to college that that took place. But these other people were doing something which I hope in lecturing I had some of that drilled into me to the point where I could be spontaneous enough that -- and persuaded enough of what I was saying that it was said with conviction. I think that's teaching at its best, when you can bring to the student the same sense of excitement that you yourself are experiencing. If you can't do that, I don't think you're teaching, really. You're reciting. And there's a big difference.

It is hard, though, to teach in that kind of a context, day after day. I often wonder about teachers who are charged with the same essential obligation of teaching two-hundred-odd days of the year, year after year. How do you retain that spontaneity that you really need and not become jaded? Because, to some extent, in the first few years you can always get by because you can say the same thing but change the words. But there's a finite number of ways that you can do that before you start changing the meaning, which is not what you're intending to do. So you find that you tend to, I think, dig yourself into a rut to which you can succumb, and many do. I think when that happens the fun of teaching is both gone for you, and it's certainly done from the standpoint of the students. It's something that you really have to sort of, I think, fight at. The subject matter, fortunately, in science changes, and it's been changing rapidly, so that that helps, too. I mean, after all, how many ways can you teach Euclidian geometry or something like that? You're still teaching the same principles that you could relate down through two millennia ago, three and a half millennia ago. So that's different.

And it's fun. I think teaching can be one of the most pleasurable of all professions because you really are doing something that gives you pleasure and can also provide important sources of information or attitudes, or whatever, to your students. It really can be great, but it can also be awfully damn tiring.

AM: Right. Well, that would be -- and this is kind of to make a huge transition here. Interesting that you say that, because I've interviewed many younger molecular biologists who either don't have any teaching responsibilities or see teaching responsibilities as a burden, an obligation that they must do, and it just takes time away from the bench.

WS: I think that's a selfish attitude, frankly. I think the world might be better off if a fair number of them were taken away from the bench. (laughs) I think one of the things about teaching is, if you can't teach your subject, I would argue that you don't know it. So teaching can kind of be a way of measuring your own comprehension of the principles, the practices, and so on of the subject matter that you're trying to teach. Maybe not everyone will subscribe to that, but certainly that's part of my conviction.

AM: Well, now I'm going to shift back a little bit so we can move forward then. One last area I wanted to ask about, because you talk about going to Lutheran school, you go to a Catholic university, what was your religious upbringing?

WS: Well, I think until the war it might be said that my parents were religious. They weren't routinely church-going. They were not doctrinaire in any sort of sense. I think they felt that it was important for us to have faith, but we were just as likely to go to whatever church was closest, as to be anything else. My grandmother was a Presbyterian. I'd gone to Lutheran schools, but I was not really a Lutheran. The same sort of thing that carried on throughout our education. It wasn't until the war that I really settled into a faith and stayed with it. As I said, they weren't doctrinaire, they weren't regularly church-going, but they did believe that faith and the principles that religion tries to teach us were important. So that's kind of the status.

AM: What kind of traditions have you brought forward with you through your adult life?

WS: Well, I'm a Catholic. I think that grows partly out of my regard for the Jesuits, partly out of the sense that I hold to the principles and I hold to the fact that the Catholic Church hasn't always been seen as accommodating. I don't think one has faith if it always has to be structured to your liking. That's not faith, that's just some sort of crutch. The Catholic Church has its principles, and our most recent Pope, John Paul II, was certainly a return to tradition, although he was also a very forward-looking person. I don't think that these notions are incompatible.

The rules of the game are such that if you've been baptized, and I'd been baptized as a child, a baptism is valid, whatever faith it may be. So when I in a sense joined the Catholic Church, I was at an Augustinian girls school in Manila. That was in April of 1945. I'd sort of been incubating it all along until that point, and that's when I made the leap. And that's been my faith since.

AM: Okay. Well, when did you make a decision that you wanted to go to college? Or was that as easy as that?

WS: I don't really know. I think it just sort of evolved from what my father said, from everything else around, that I was going to go on to school. It was, in a sense, strongly urged. Didn't view it, I'm sure, as compulsory, but he would have been very disappointed had I not. Then many of my schoolmates were going on, too. There was a certain aspect of that. The difference then, of course, was that when you went on to college, the prospects of financial support were pretty slim. There were no federal loans, low interest, such as there are now. And the banks certainly didn't, in the main, except perhaps for medicine, look upon a college education as a guarantor of getting paid back. The kinds of scholarships that you would get would be a hundred dollars or two hundred dollars, or something like that, or it might be just waving the tuition. You still had to live. I think these are major reasons that in large cities, the length that most of us went to to schools were in the communities where we lived.

That was certainly my case in going to Marquette. I could have gone to -- there's a structure that was true more than a half a century ago, there was what was called the extension in Milwaukee, which was the first two years at the University of Wisconsin, but then you had to go to Madison after that because they only go to -- the other school was [Milwaukee-]Downer [College], and that was a girls school. (chuckles) You could have gone to Concordia [University Wisconsin], which was a losing school. (laughs)

AM: But Marquette was taking --

WS: It was a full college, all the professional schools, law, dentistry, medicine, and so on, and I could get there on public transportation.

AM: Why did you choose premedical studies? It sounded like you were attracted to literature as well.

WS: Well, I guess -- there's always been a pleasure and a beauty associated with science that led me that way. Medicine was a form of science which also brought better economic rewards than just teaching science in a college did in those days, and still, for that matter. So that seemed like a starting point. The curriculum was fairly well defined. I mentioned that you had to minor in philosophy. Then, so far as the sciences were concerned, you knew what you were going to have to have -- physics, all the chemistries, inorganic and organic and whatnot. Then you had to have English literature, and you had to have one foreign language. I had taken French. There wasn't a great deal of latitude in the program that you had -- perhaps from say the second year, from the sophomore year on, you had maybe one elective a semester. Marquette ran on a semester program. In my case, I took modern French literature and other things like that, whatever kind of struck my fancy. (chuckles)

As I said, then I met Dr. McDonough and I became more familiar with genetics than I'd certainly been in high school. I found that fascinating to combine my interests in an almost mathematical-like science, that there are rules that had evolved from observation, and those rules provide predictions that are testable, and whatnot. So that there was a feature to genetics then, let's say, that was both exciting, new, but had not the rigidity of chemistry, or physics for that matter, but also had some of the structure that they had, too, in the sense that you couldn't just fly off in every direction. There had to be some kind of an organized body of information.

AM: So genetics was basically Mendelian genetics?

WS: Yes. At that time, of course, most of it really revolved around either plants -- and those would have been at the agricultural schools primarily -- or animal breeding. But sort of, quote, theoretical genetics, at that point experimental genetics, was Drosophila. There were a few other things, animals, but never really competed. There's a solitary wasp, *Habrobracon*, that was used by Phineas [W.] Whiting and a group of his students at the University of Pennsylvania. It was just interesting because it was -- one of the sexes had only a single _____, so that you could study certain genetic characteristics easily because all of the genes were expressed. There was no opportunity for recessiveness.

That basically did it. Then I went off to war, as I mentioned, and then came back. By that time, I was even more persuaded that I didn't want medicine. I'd seen enough blood and guts, literally, to have removed any sense of excitement that that might have had. And genetics was it.

AM: Before you went off to the Pacific, did you see that there might be connections between pursuing medicine and pursuing genetics at the same time?

WS: No, I can't claim that I did. Perhaps in retrospect, I would like to have thought that I did, but I really didn't, as indicated by the fact that I was still trying to make up my mind whether I wanted to be a drosophilist or a human geneticist.

AM: I think many science majors, and particularly premed majors, would be aghast if they had to take courses in philosophy. I was interested when you said this was part of the requirement at Marquette. What kind of courses did you take in natural philosophy, and how did they make this connection between the roots of science and natural philosophy?

WS: Well, we would have -- and don't ask me to enlarge on all of them now because it's been so long since I've thought about them. We'd start with logic. Your first course was logic, and you learned about the structure of syllogisms and the errors that could be made. So we'd go through that. Then there was a course in psychology, but it wasn't the kind of psychology that most psychologists would think of. This dealt more with sort of the philosophy of mental thought. Then we had metaphysics. As you might imagine, at a Jesuit school that meant in a sense spending most of our time reading Thomas Aquinas. The same thing was pretty much true in natural theology. Much of that was following the arguments of the doctors of the church as to what natural theology represents.

These courses, almost all of them, were taught by the Jesuits, and they were a very demanding bunch. They used, you know, you couldn't charm sort of business. And they weren't open-minded in the sense that they were clearly very driven in their sense of teaching. At that time, I don't know how big the Jesuit colony at Marquette would have been, probably fifty, sixty Jesuits. I don't think it's that large now. And they taught mostly -- there were a few in the sciences, not very many. Actually, there were two in the life sciences. The embryologist anatomist was a Jesuit, and the physiologist was not a Jesuit, he was a Viatorian [Clerics of Saint Viator], which is another college teaching order. They were both very well _____. The Viatorian was a Johns Hopkins [University] product and had studied with, oh gosh, I can't think of his name now, a very distinguished development biologist, who was probably *the* most renowned person in his area during that time.

But most of them were in history or in the philosophies. Some taught languages, certainly Latin to the extent that you would have -- anyone who was studying the classical languages, they were almost always Jesuits who were teaching Latin and Greek. You wouldn't find many, if any, in the medical school, to my knowledge. I don't think there were any in the dental school. The law school, I don't recollect, but I really don't know. Most of them were going to be in the humanities, with a very small number in the sciences.

AM: And what was the relationship between science and -- at least, how the Jesuits were teaching it -- the relationship between science and religion, at least church doctrine?

WS: They weren't trying to proselytize, or if they did so, it was in a much more subtle form than most of us recognized. They did believe strongly that professionals should be educated. That's a lot different from being just familiar with a particular set of facts and observations. That you should be a scholar. I think one of the things that disturbed me in the relatively recent past in our own sciences is that scholarship is gone, what I know. Many of them don't write well. Everything is in a jargon. And then, if you were to ask them a larger question about what's the relevance of these things to human life, they haven't even thought about it, but boy, they know every end of that damn molecule.

That's precisely what the Jesuits were trying to avoid, that you should be -- we weren't all going to be polymaths , but at least we should have enough command of our own language, and familiarity with some other languages, that we could pretend that we were scholars. (chuckles) With some conviction. I don't think that's really true anymore. There are many of us, myself certainly included, who believe there's been sort of a dumbing down of our whole educational system that I don't think augurs well for us in the long run.

AM: Does that include the sciences as well as the humanities and the social sciences?

WS: I think so in the sense that I would suspect -- I have no proof of this -- that a large percentage of those individuals who now identify themselves with genetics don't even know the history of genetics and don't know much about how the notion of the gene, its location, the proof of its location, all the rest of it, even evolved. As I said, they become specialists in one molecule, and that is the be-all to end all, as far as they're concerned. There are larger issues that medicine and science are never going to address, and that we ought to at least acknowledge the existence of those things, and hopefully be well enough informed that we can generate some thoughts of our own with respect to them, be worthy of some of those notions.

I think that was basically -- the Jesuits really were trying to make us educated people. Education, as far as they were concerned, involved a sense of familiarity with how to think, how do you form an argument which is rigorous, and all of those notions were part and parcel of their philosophy of teaching.

IV. Serving in the Army; Ohio State University and Genetics; On Publishing

AM: Okay. You had mentioned yesterday that you were in the Enlisted Reserve as an undergraduate. Was that a voluntary situation, or was that related to premedical studies?

WS: No, that was a voluntary situation. Well, it was to the extent that anyone who was of my age there was anything voluntary about it at the time. I was, like everyone else, called up to be examined for induction, and at that point in time I obviously wasn't 4F, so I could see myself being drafted and decided instead I would be better off going in to one of the reserves on the supposition that I might at least be able to finish my last year of undergraduate studies. So I enlisted in what was called the Enlisted Reserve Corps. That status only lasted for about three months. (laughs) Then off I went.

I suppose if I'd thought about it, the ASTP [Army Special Training] Program was just coming into vogue then. I could have stayed on, if I'd gone to medical school _____ without any cost to myself or to my father through the Army Special Training Program. But I didn't do that. I guess it was wrong-headed perhaps, but I felt that, particularly with my brother already in service, that the nation needed us now, I thought, rather than four years from now. Kind of an old-fashioned set of values led me off to the military.

As I said yesterday, it was an experience I wouldn't want to go through again, but it was also a learning exercise. You found out that you could run the gap, and you don't often know in life whether you ever are going to have the courage to be courageous when they need us there. Those of us who saw combat certainly know that we could do it. Didn't want to do it, but we could. And you didn't come unhinged.

AM: I wanted then to pick up -- this is moving us towards where we were kind of ending yesterday. I thought we could talk a little bit more about the transition, because it seems to me that your experiences in the war were quite searing and life altering, and then you must make this transition back to civilian life and an undergraduate in which your fellow students at Marquette may or may not have had the same experiences, and you pick up where you left off. Could you talk a little bit more about -- is it just so simply you just go back and pick up your life, and I'm going to be a drosopholist or I'm going to finish my degree? What was the impact of your war experiences on these then kind of more civilian professional career-type decisions you were needing to make after you were decommissioned?

WS: I think the transition for us is, perhaps in many respects, easier than say the one after the Vietnamese affair, largely because there had been fifteen million of us in the military. Throughout my graduate education, my associates were mostly dull service people, the army, navy, marines. And when I started my first teaching, which was at Ohio State, my class was made up almost completely of veterans. I was maybe a year or two years older than most of them. That was the most unusual part because you were really teaching your colleagues, in a sense, because some of them had been in the same sort of situation that I had been, that is they had to complete their high school work, they had left in the middle of high school. Or they hadn't really decided whether they were going to try to go on to a higher education, because many of them, I guess, thought that was beyond their financial reach.

Then of course, with the so-called G.I. Bill of Rights, the Servicemen's Readjustment Act I guess is what it actually was called, there were opportunities open to them. Now, if they were married, there had to be a whole new kind of accommodation, so some of them didn't get started right away, I guess is the point I'm making. They weren't starting till 1947, '48, even though they'd gotten out of the army three years earlier, two years _____. The most likely ones to be going on were those who were eighteen to twenty-five, although there clearly were people who saw this as a marvelous opportunity who had abandoned any hope, even before the war, about college education who started and gritted their way through.

In my particular case, since I'd already been there, except for the time I was actually in service, I didn't lose a great deal of time, not three years yet. But I went right back and enrolled immediately in my final semester, and then into graduate school, and parked at Marquette mostly because I was still trying to figure out where I was going to go and what it really was. This kept me occupied, and I was obviously studying things that would be transferable.

But when I began to teach myself, which was really interesting because what, in 1949 I was twenty-seven. I'd started teaching in '47, shortly after I went to Ohio State. So here I was twenty-five and most of my students were twenty to twenty-three, something like that. (chuckles) You didn't win respect because of your age. But the one thing I will say, some of the most exciting and best teaching I feel I've been fortunate enough to participate in came in that first decade following the war. Largely because these students, the veteran ones, had a maturity that you don't see in students normally. They had gone through life-threatening events. They had a strong sense that they had time to make up, and they wouldn't tolerate a poorly prepared lecture. You'd get hooted. I mean, they were respectful, but boy, you don't waste their time. That was sort of the --

AM: Did you get hooted?

WS: No, I didn't. But I got close a couple of times. (both laugh) It was challenging because they really made you perform. I think most of us we would have anyway, but there was this sense that, gee, you were cheating them too. When you had kind of a dilettantish student body, what the heck? They aren't very much committed, either. But these kids, young men, all were. It was fun. Then we'd sit down and have a beer together. (chuckles) Which you wouldn't do otherwise.

AM: I think this may be overlapping a little bit from what you talked about yesterday, but what kind of exposure to human or medical genetics did you have at Marquette?

WS: Interestingly enough, I didn't have a great deal until I was in graduate school, and then I became interested in some work that Leo [C.] Massopust [Sr.] was doing on early recognition of breast cancer. This was the use of infrared photography, which has the capacity to penetrate _____, something like the skin. In the process of that, the work that he was doing -- because, of course, malignancies of the breast -- most malignancies are highly vascular, so you could pick up with the infrared this increased vascularity that was strongly suggesting that you had a tumor.

In the process of his work, it was revealed that there were patterns of superficial venation of the thorax and that these related to from whence the vessels came, basically two broad categories of origin. This suggested that probably something genetic was going on that was determining how the venation was _____ and where it was coming from, the mammaries or [noise on the tape - inaudible]. The interesting thing was, of course, it gave a tool to looking at the genetics of, quote, normal variations. Because then and still we know much more about abnormal variations than we do about, quote, normal variations. At that time, virtually the only things we knew that might be construed as normal variations were associated with the _____. Oh, there was tongue rolling and PTC [phenylthiocarbamide] testing and stuff like that, but even in those instances, the genetic evidence was ambiguous if you were really critical of it.

So here was an opportunity to look at something which might have immediate clinical application, and also was dealing with a series of structures which we presumed were under genetic control but didn't know how and didn't know much about their evolution. When I went off to Ohio State, I decided that I wanted to continue that work with Massopust's stuff, and it was easy to do. All you really needed was a camera, darkness, and an infrared film. (chuckles) The hardest thing was to get people to take their tops off. (chuckles) So that was part of it, getting involved in that.

I was fortunate to have, I think, very good teachers at Ohio State who were very supportive, and also, I think, opened my eyes to ancillary ways that you -- clearly, the primary pathways to human genetics were either clinical variation of one sort, for which one really needs medical qualification to do well in. Or there was sort of the quantitative aspects with statistics. Of course, in between there is the pure laboratory thing, lab chemistry and whatnot. That has never, ever really interested me all that much.

So deciding at that point in time I wasn't going to be a physician, that left me with going into the quantitative things and to focus largely not on populations -- obviously, I was going to say not to focus on families, you can't really focus on populations without focusing on families, too, but it wasn't -- I was interested in the kind of research that would make it possible to describe the events in a population, not events just restricted to what's going on in this particular family and not knowing how common that family is in the larger sphere of the population of which it is a portion.

So I took a lot of statistics and a lot of mathematics while I was there, and was fortunate enough to run across -- or become part of and a long-time friend with Jim/James [N.] Spuhler. Jim was a product of the Harvard [University] Anthropological School and had come to Ohio State and was teaching there. He had been involved in studies in the Southwest, primarily with a group of Navajo known as the Rarnah Navajo. They had a large field program going on, and he asked me to help in the analysis of the data.

Then also, they were in a position to provide some of the information that I wanted because I had familiarized him with what Massopust was doing. So they set up on one of their field trips to New Mexico to photograph mother, father, and children in families so that I had family information to couple with information from twins that were not _____.

Jim was -- kind of harkening back to what I was saying a moment ago -- Jim Spuhler was *a* scholar. He was exceptionally well read, a self-effacing, gentle, big guy. He'd gone to the University of New Mexico initially on a football scholarship. (chuckles) That gives you some idea. He was a linesman. Jim had a very quick mind, very retentive mind. I don't know how many languages he knew, but he was skilled in Chinese and spent the war as a naval intelligence officer in China. So he was interested in the Orient as well, and that kind of added another thought to it. I found him a marvelous example person, both as a teacher and as a person interested in science. After I'd gone off to Japan to work with Jim Neel, Jim Spuhler left Ohio State to go to Michigan, so he was in Michigan for many, many years in the Department of Anthropology and was head of the department for a couple of tours, however many years they were appointed, three or four, something like that, I think was the cycle for the chairman.

It was an opportunity to continue an association that had started there at Ohio State. Jim must have gone to Ohio State about '47, which is probably the year that I went. He might have gone in '46, I'm not really sure, but I know he was a very new assistant professor when I first encountered him.

The role models I've had, I've been very fortunate with, both in terms of the research and also as teachers. I respect them for what they at least tried to do for me.

AM: To go back a bit to Marquette, you were picking up this work in human genetics, but what was the relationship with that literature that you were reading to your thesis work, which I'm going to assume is in Drosophila.

WS: Yes. My thesis was in Drosophila. There is a particular strain of Drosophila known as the attached-X, and that was a strain that had first been identified, if memory serves me correctly, by Lilian [V.] Morgan, who was T.[Thomas] H. Morgan's wife. It had been identified in around 1921, something like that, and it has a peculiar characteristic in gender determination because of the attached-X structure. Well, the strain that we had at Marquette suddenly starts throwing all these sports, and we didn't know if the attached structure had broken down and whatnot.

The purpose of a master's thesis at that time wasn't necessarily to generate something that was publishable so much as it was to teach you the skills, how you set up an experiment. How you, first of all, define a question, then how you set up an experiment to presumably provide answers to that question, and then to familiarize yourself with the techniques that are needed. Obviously, this was a thing that focused on the chromosome, and therefore I had to learn how to dissect internal organs and to stain and read the chromosome. That was all part of the training. I never did get an answer that was publishable, but at any rate, I developed the skills, which if I'd have stayed with Drosophila would have equipped me well to go on. So that was sort of it.

What had tilted me to what was going on in Leo Massopust's work was one of my fellow graduate students was Leo Massopust's son, Leo Massopust, Jr. All of us who were at the same level of graduate education had an office on the top floor, just a desk really, basically. We'd _____ about all these sorts of things, and in one of these conversations, Leo told me about what his father was doing and what was going on. Then I went up to talk to his dad and saw some of the photographs and all of the rest of it. He gave me copies of the publications that he had had at that point. That was sort of in the back of my mind. If I decided I was really going to go into human genetics, which it seemed like I was going to do, that this was a potential problem to work on that would be interesting and presumably would meet the needs of a doctoral dissertation.

AM: So you moved to Ohio State to work with Larry [Laurence H.] Snyder, but he left when you got there. But he was also one of the -- I believe at that time had an endowed chair in medical genetics, one of the first positions in medical genetics.

WS: Yes, correct.

AM: Tell me a little bit when you got there. Where was human genetics in relationship to what we now call classical genetics and medical genetics?

WS: Well, since Snyder had left, within the Arts and Science College the most widely recognized figure had gone. His ties into the medical school had been largely on a personal sort of relationship. He and Charles [A.] Doan, who was then the dean of the medical school, were close friends, and they had worked together on a few disorders that Doan had seen in the course of his practice. He was, I believe, a hematologist, if I remember correctly. So the medical school had an awareness and some interest but no structure, really, that dealt with human genetics. When Snyder left, the people who were still there in human genetics were essentially day drive and only part time in a way. Madge [T.] Macklin, who was not on the faculty, and a young man whose name was Larry Eisenberg , who really was basically there to help teach the very large numbers of introductory students they had.

So a tie into the medical school didn't really exist any longer, and this meant then -- well, since I was talking about a program that dealt with normal variation, I didn't necessarily have to have a tie into the medical school. If I'd wanted to go on to other things, yes, that would have had to have been developed. I think it could have been.

There was a pathologist there who was very much interested in tumor development. His experimental tool, interestingly enough, was Drosophila -- he was studying a tumor-head, as it's called, it's a genetic strain, Drosophila -- and was interested in the embryology and the development of this tumor in an animal that he could control, and the supposition that there were, presumably, general principles related to a formation. To my recollection -- and I can't think of his name now -- he and Doan were probably the only ones who had any publications that even vaguely dealt with things that were human genetic.

Most of what I did, therefore, was related to Madge Macklin. Then since I was very heavily committed to taking statistics and mathematics, I was involved in courses with Henry [B.] Mann and [D.] Ransom Whitney and all the rest of those. Primarily learning the skills that would be needed in the analysis I would have to do, which was fortunate for me because I think that was what made, in a sense, me attractive when Jim recruited me. He felt that was what they wanted in Japan, somebody who could not only collect information but ride herd on it.

So that all worked out. I'm not the most gifted or endowed with concerns about the future. I don't lay out a program so much as I respond to events. (laughs)

AM: How easy was it to make the transition from being trained in Drosophila genetics and more experimental approaches, bench approaches to problems, to human genetics, which sounds like you were gravitating towards population genetics?

WS: Not so difficult. In a sense, Curt Stern's book taught human genetics from Drosophila. All of the examples, virtually, are examples drawn from Drosophila. Human genetics was more of an avocation with him. Really, his own research was largely in Drosophila genetics. If you understood the principles, the principles were sort of animal or plant independent. These were all-embracing notions, which whether you were applying it to Drosophila or to the mouse or to the human made little difference provided that it satisfied the criteria that was necessary for recognition of particular forms of transmission.

Yes, it meant that you had to learn some jargon that you might not have known otherwise, but I was, in that case, fortunate because of spending all the war years as a surgical technician. I had had more exposure to anatomy probably than most medical students got. (chuckles) And being around people. And our work wasn't solely surgical. Malaria was a common problem, and dengue [fever] was another one, and all the diarrheas that you could think of, bacterial and viral, were all part and parcel of what went on around us. So you had kind of -- as I think I said yesterday, if this had been the early nineteenth century, I could have hung out my shingle because that was basically the way that you learned medicine then, by an apprenticeship to somebody who presumably was familiar enough to be able to teach you.

So I didn't find the transition difficult. I did kind of continue to go through the business of questioning whether I should drop out and go on -- and this is after I got to Michigan -- and get a medical degree, or not. It obviously would have merit. Then I thought, well gee whiz, if I'm going to continue to do essentially what I'm doing, how am I going to profit, other than the fact that it offers another money-making opportunity if one wants to. If I'm going to be a counter, in effect, the only thing that medicine offered to me at that point was to be able to avoid heterogeneity in what I was counting, so I really would know enough to know that this was this particular disorder and not some other one.

Marge [Margery W.] Shaw, that's basically what she did. Marge was in corn genetics and then decided that she really wanted to equip herself to do clinical genetics. Then of course, got off into chromosomes, and from that into the law.

AM: So clinical genetics at that point was not really an interest.

WS: No. Well, it had its own --

AM: As a means to an end anyway.

WS: Right. It was in the eyes many sort of stamp collecting, if you will, odd pedigrees, without being able to put them together in some sort of a coherent whole. Obviously, that's not true now, and when enough markers came along that you could start thinking in terms of linkage studies, _____ were being changed. At that time, that wasn't so. You didn't know any linkages, and it was some years before the first one came along and it was darn near as many years before the second one after that came along. It was difficult to collect enough information. It was a challenge with the computing skills that were available at the time.

Gosh, we took, in the book that Jim and I wrote in '54, a fairly pessimistic look at linkage at that time. We didn't think that was going to be the wave of the immediate future. And, in fact, it wasn't. It wasn't until the sixties that things began to change. Well, maybe a little earlier than that. I guess you could starting dating it, in a way, with Oliver Smithies' work, because then that led to a lot of biochemical markers, and that, coupled with the serological ones, began to give you an opportunity to look more sharply at relationships.

AM: And how did you come to work with David Rife?

WS: Oh, by default. (chuckles) Since Snyder had already left and Rife was the one closest because Macklin was not formally on the faculty, and I needed a mentor with the standing within the university family to be able to -- it could have been a much more difficult show than it actually turned out to be, largely because of Macklin, Spuhler. Dave Rife, too, I don't want to diminish in any sort of way. He was a fine man, very supportive in getting things done.

His own work in human genetics, to the extent that he had done very much, had been with twins. So when I started looking at the patterns of enation, since he had ties into the twins community in Columbus and its environs, he was able to line up a roster of twins that were willing to come in and be photographed. That was a very direct contribution to what I was doing.

I think in many instances it was the supportiveness. Ohio State had a good group of geneticists there at that time. Earl Green, who went on to become head of the [Roscoe B.] Jackson Memorial Laboratory [The Jackson Laboratory], and was a -- he worked with rabbits and mice. And Allen [S.] Fox was there, Elton [F.] Paddock, and -- I can't think of his first name. [L. C.] Ferguson, who was one of the cattle blood groupers.

Ohio State had a large College of Agriculture, and there were people there in almost all aspects of applied genetics, animal breeding. Ralph [George] Japp, for example, was there and he was kind of the guru of chickens. He had people who were involved in cattle and in corn and -- mostly things which had relevance to agriculture in Ohio. Ohio is not a big wheat state, although it did produce wheat. They produced the wheat that's used in cakes. I don't remember, there was einkorn and emmer and whatnot. It's been too long since I've had to think about those to get them lined up.

There was a sizable community of geneticists on the campus. When we'd have the weekly genetics seminar, you'd have fifteen, twenty faculty people and at least that many students who were there. So there was interest. I think human genetics was of interest to virtually everybody there, but it was more a matter of curiosity than it was a way of life for them.

You couldn't really be a geneticist -- I don't think you could be a human geneticist without being interested in Drosophila. I mean, there's still things that can be done with Drosophila that we can't do in our own species. I think that by knowing something about these other organisms, you may be better equipped to know when transfer methodology will work and when it won't work, and when there are methodologies that have been developed in other organisms that merit efforts to transfer it. There's a lot of work done, certainly in brain development, in Drosophila that we can't even begin to approach in the human yet. But I think the time will come. It's just a question of if you know enough about the two species, then you have some better insight into when that transfer might be practical.

AM: Madge Macklin is an interesting character. There isn't a lot of history written about her, but she does emerge in these interviews that I've been doing as both kind of a maverick of her time, certainly her notoriety as being kind of an outspoken eugenicist. But also in her kind of pioneering approaches to genetic counseling. I was wondering if you would talk a little bit more about your relationship with her as more of an unofficial advisor, because she wasn't a member of the faculty and she always had problems trying to get a permanent position somewhere.

WS: It's kind of a mixed story. Our relationship ended on a kind of unhappy note, and it's always bothered me and I tried to set what I thought was Dr. Macklin straight. During the years that I was at Ohio State -- this trouble I'm going to come to in a moment is almost a decade later -- she was very helpful. She knew the clinical literature that had relevance. She set demanding standards for case ascertainment, for diagnostic accuracy, and whatnot. All of these things were really good. She was always willing to listen, too, and she was a good critic in the sense that she often could see more quickly than one did oneself a weak spot in an argument, and would say, "Well, Jack, that's not going to float. That isn't right there."

That relationship was really great. She was a warm, motherly kind of lady, overweight all of her life, I expect. I know that she used to complain -- we'd sometimes go to lunch, because the place where she normally went to lunch was right across the street from the building in which we were housed. She would go on these thousand calorie diets with a view towards losing weight and hardly lose a pound, and would get weak. She was as close to starvation as she felt she could tolerate medically, and yet she wasn't losing very much weight. She wore rimless glasses, always had a smile. I still have great fondness for her.

The problem that arose later on was right here, unfortunately. In the late 1950s, and still for that matter, the Anderson [University of Texas M.D. Anderson Hospital and Tumor Institute] had an annual cancer conference, and at least two of these were on genetics. One was on -- well, had genetic components. One is actually held now on cancer genetics, and the other was on issues related to the health effects of ionizing radiation.

At the one on cancer genetics, Macklin had just recently completed her large study on breast cancer. I was asked to speak after she had spoken, and either I wasn't properly sensitive or she misinterpreted the view I had. She felt that I was diminishing her work. And that wasn't my intention. My intention was essentially to say that using the paradigm that Madge Macklin has used, you can't get any better, but that paradigm is not leading us anyplace. It was kind of an empirical approach to cancer in families, rather than one that had some sort of theoretical basis that could be tested.

She took my remarks as being critical of what she had done. It wasn't that. In fact, I was trying to say the other thing. I didn't see how anybody could do a study of the kind that she was doing better, but that we needed a new paradigm. By that time, that same sort of model had been used by others in breast cancer. Pete [Clarence P.] Oliver and Charles Wolf. Their studies weren't as rigorously done as Madge's were, yet the results were pretty much the same in the sense that, yes, there was an increased risk of second cancers of the same kind in families ascertained to have cancer. But it didn't follow any mode of inheritance we could identify. This had been applied to other things besides breast cancer with pretty much the same result, and I was simply deploring the fact that we needed another model. As I say, she took it to mean that I had been critical of her. We had more or less patched things up before she died, but it was really kind of a sad thing for me because it was the last thing I would have done. I had too much regard for her.

If you go to those books -- I used to have copies of them, I don't know, I probably still do someplace -- it would have been about 1958, '59, something like that. Let's see if I -- no, I don't. They were part of this Anderson series. You'll see Madge's reaction. She was really calling me Dr. Schull. Everything had become so very formal, which it had never been with her before. It was troubling. But I guess if you speak your mind, sometimes people can misinterpret what you're intending to say, or you're simply not saying it well enough, which is probably what I was doing. I just didn't make the distinction I was trying to make clear enough or forceful enough. I was really trying to pay her a compliment. I didn't see how anybody could do it better. But that doesn't mean that the work needs to continue in exactly that same format.

Madge had worked with -- I guess depending upon how you want to interpret it, she'd either missed or near missed the whole business of erythroblastosis [fetalis]. She had a publication in the 1930s, but of course this was before it was known to have a serological basis. It didn't really segregate in families, again conspicuously, although she was persuaded, and as we now know, it is an inherited outcome of incompatibility.

So she was attuned. As I said, her publications go back to -- I know there are publications before 1920. I don't remember the exact date -- 1918, 1919, something like that. She was an advocate for more genetics in the medical curriculum, much as Larry Snyder was. In fact, I think you could say that the three principal advocates were William Allan, Madge Macklin, and Larry Snyder, at a time when nobody else was really beating the drum. The interesting thing was that two of the three were physicians. So they saw the need but just couldn't seem to persuade their colleagues to make the effort _____.

AM: Just an aside here, it's ten o'clock and we can keep going but I don't know what your appointment schedule is.

WS: I have nothing this morning.

AM: Okay. Could we go on for another thirty minutes?

WS: Oh, sure. Because, unfortunately, you ask a question and you get a lecture. (chuckles)

AM: No, this is fine, this is fine. I just want to make sure I'm not taking more time than I deserve.

WS: No, not at all.

AM: Okay. Well, she also was quite vocal in print about her eugenicist views as a mentor to graduate students in genetics at Ohio State. How aware were you of a kind of a goal to the kind of genetic research that she was doing?

WS: I haven't thought about this in a long time, but I don't remember any overt effort at indoctrination. Those were, obviously, her views, much as they were the views of Lee [R.] Dice at Michigan. Yet, he too was not someone who was trying to persuade people. He wasn't encouraging all his students to have like minds. He simply felt that there were difficulties here and that, with the state of medicine and society and so on, something had to be done.

The immediacy of the problem was much like the same difficulty that we have with our own numbers. Some have seen [Thomas] Malthus as just around the corner, and others are prepared to put him off for a couple more centuries. It's a question of how deeply one is committed to a particular perspective and the sense of the immediacy of the problem that you have. You can be deeply committed but yet not feel it's going to happen tomorrow.

When Madge lectured, it was always on her research with breast cancer, so I don't recall any instance in which she was sort of dwelling on her philosophy of what needs to be done, what she thought needed to be done in the cases of individuals with potential for reproductive failures.

As I say, the same thing was true of Lee Dice. He was very active in the American Eugenics Society. He was more of the late Frederick Osborn, positive eugenics. You don't discourage the one, the encourage the other, that sort of an attitude. I certainly, in my own mind, don't think of Madge Macklin as being a rabid eugenicist.

AM: From your own work in genetics in the postwar era where genetics research went amok in the Third Reich, how aware were you as students in genetics of this entire history that predated what happened before World War II in the United States and then under the Third Reich? How much did it enter into a way of thinking about your work?

WS: Well, in my own case, not conspicuously so. Obviously, when you had a question of human genetics, no one could not dwell on this to some extent. But generally the amount of time devoted to that topic was small relative to efforts to understand methods of linkage analysis and stuff like that. There'd be one, perhaps two, lectures on applications of genetic information, and eugenics would emerge as one such application. I think in most instances by the time that I was a student, people were regretting the excesses that had occurred in the name of eugenics. We're not persuaded that there wasn't a problem here, but that there had to be other ways of addressing it than that. I think the ways that have evolved since are maybe not a whale of a lot better than the ones that existed then. It's just part of the whole business of genetic variability.

AM: Well, did you see yourself as doing something different than say what _____ may have been doing?

WS: No. For example, even in genetic counseling, our attitude is always that we provide the best information that we can, and we respond to the concerns of the family as objectively as we can, but the decision making is theirs and we shouldn't consciously try to direct that decision. There are many factors that they should incorporate in the decision making that had absolutely nothing to do with the rules of genetics. What were the economic implications of the decision? What were the psychosocial implications of the decision? We were not authorities on that. I think to the extent that we touched on those issues was simply to make certain that the family, in trying to arrive at a decision with which they were comfortable at that time, remembered that these were all parts and parcel of that decision making.

Obviously, the trauma of having recently had an abnormal child, I think you could build the case that it doesn't provide an environment in which people are necessarily led to the best decisions that they can make. There's too much immediacy to the unfortunate and that they need time to gather together their thoughts in a way that they can make a decision that's more objective. But in that process, they had to look at these other things. Gosh, I can remember having a family come in that had two successive children with biliary atresia, and both children died, but not until after extensive surgery, very costly to the family. They lost their home. There weren't means to cover it as there now are. It was really a very big issue, and the genetics is reasonably clear in the particular case that they had.

In most of those instances, you're dealing with a recessive disorder. There's one chance in four of another child. Some people find that inordinately large, and others don't think -- they take the positive. Gosh, there's three chances out of four it's going to be normal. But when you have to then imagine that suppose that one in four does occur? What are all the implications that go with it?

I don't know that we do any better, in a way, counseling today. With teen counseling that is so popular, maybe it projects a sense of expertise. But I think the more important thing may be to project to the family a sense of compassion, concern, and to not color, intentionally or unintentionally -- I think often it's intentionally -- the information that's provided them. It's not an easy thing to do. I think that what one does see now is that -- a half century ago, most of us who did counseling had no particular training in it. We had training in genetics, and what we learned about the human condition and how you responded to it under this kind of threat, we learned from experience.

Now there are formal courses and certification and whatnot, which should certainly make counselors today better prepared, but it's still a very difficult thing to do. It's harder, I think, in many ways than say the counseling that a physician has to make when a patient is deciding whether they will or not have surgery. It's something that affects them directly. This is affecting another person, a person for whom they obviously just, by virtue of their relationship, have deep concern. It isn't the same kind of issue.

AM: Now, were you exposed to genetic counseling in situations where you did get referred to for genetic counseling at Ohio State?

WS: Not at Ohio State. I started doing counseling first when I was part of the Heredity Clinic at Michigan.

AM: Okay. Well, just to kind of wrap it up and move you towards Japan and Michigan, I don't have much sense of what your publication record was as a graduate student or if your dissertation, there was any publication. Were you able to publish some papers?

WS: Not really. In fact, I never actually published my dissertation. It appears in the dissertation prospectus, but -- Jim Spuhler had presented the results that I had found in a publication that appeared in the Cold Spring Harbor [Laboratory] symposia of 1950. I think the major reason was, and this is something that I've tried to counsel students subsequently, at that time I don't think most of us realized the enormous task that was involved in going from a formal classical dissertation to a published article. You don't do this on a weekend. There's a lot of rewriting that goes on, and that you have to make allowances for the time to do that. Well, having left Ohio State in June and being in Japan in July, there really wasn't the time or opportunity to do that. So the deeper I got involved in the work in Japan, the harder and harder it was to even turn back to the idea of trying to put this together in a publication.

I think now what we tend to do is, while satisfying the usual strictures about the traditional dissertation, we encourage the students as they're actually writing their dissertation to be preparing for publications at one or several _____, that there isn't this lost time. If they take an academic position, the odds are that they're going to suddenly find themselves teaching, and the first time through in preparing a course is mighty damn time consuming, because you haven't really sat down and thought about all these things yourself and how *you* want to put your imprint on the remarks that you propose to make to the students. And mustering all of the material, which maybe you were exposed to in your own education, but not to the depth that you may feel is necessary in the teaching. So if you didn't have a sort of _____ to go to a journal the day you picked up your diploma, the odds are very good that it wasn't going to happen, because you get absorbed in other things and more of the information eventually becomes out of date.

AM: In today's publish or perish climate, graduate students are obsessed with having not only publications but publications in the right journals because that's going to lead to a better postdoc, and if they don't get a good postdoc, they won't get a faculty, tenure track position. What was the role then of publications in trying to get a faculty position?

WS: I can't remember a time in my education too in which that old adage about publish or perish didn't have a lot of life to it. There was a difference in what was expected in terms of a publication. You didn't expect to see a first-year postdoc with fifteen publications to his credit. You did expect that there would probably be something from his dissertation that would be out.

Much of what one's expectations were depended upon the area in science in which we're dealing. Just like now with the molecular biology, then, in the sense of Drosophila, anyone who came up with a new mutant, that was a publication. Same thing is true now in a lot of molecular biology. You can do it on a weekend. That wasn't really so before. There were far fewer journals, for one thing. Now, if you're persistent, some place will publish anything. (chuckles) You can find a journal that's only too happy to put what you've written into print.

Then the numbers were much smaller, and I think the journals were more demanding. The journals were smaller, too. When it started, the *American Journal of Human Genetics* was a quarterly, so you had four issues a year and the average issue was maybe two hundred pages, and it was a full page format.

AM: And that probably wasn't even established yet. That was '48.

WS: That's right. The journal began in '49, that was the first year of publication. I guess the one then that probably had more of human genetics in it was the *Journal of Heredity*. It was monthly, if I remember correctly, but it was fifty to a hundred pages and it wasn't a big thick thing. Now the *American Journal of Human Genetics* generates fifteen hundred, sixteen hundred pages a year, it's monthly, double column format.

So there's a lot more kind of going on than was true then. I think, too, of course, today people, as you were mentioning, are concerned about teaching loads. We just took it for granted that we were going to have teaching, and we sort of expected that in most instances during the normal school year we wouldn't have very much time for research. It was the summers in which you got your research done, and then you tried to write during the school year when you were also preparing your lectures.

The whole kind of style of living that was associated with academic science was quite different then than now. You didn't have the ancillary sources of funding or anything else. There weren't that many grants. Certainly, when I graduated, the word postdoc was hardly known. Now it's an almost essential part of the education of a person. You have to have a postdoc someplace or other. The more prestigious the person under whom you had your postdoctoral fellowship, the better for you. But I think it often ends up creating -- or *can* create a second copy of the professor, and probably not as an original _____.

And the demands. Now molecular biology is expensive on account of all the apparatus. Nobody does anything by hand anymore, as opposed to in my day virtually everything was done by hand. That even included our calculations. So it was very time consuming, particularly if you were dealing with populations.

Well, let me give you an illustration. Our big study of the inbreeding in Japan, the actual data collection took two years, from 1950 to 1960, and immediately upon our return to the United States, we started the analysis. The book was published in 1965. So that's a seven year cycle.

Now, not every study did that, but gee whiz, the study of mutagenic effects of ionized radiation, data collection began in '47 and terminated in the winter of '53-'54, publication in '56, so that too is almost ten years. It was a much slower sort of thing.

Part of that was, I guess, the way we elected to publish our results. In both of those instances, we had support that would allow us to publish it as a monograph, and we felt that there was merit to doing so because you could set out the data and the analysis in a much more comprehensive and coherent way than a lot of little snippets. Now, today it would be the latter approach, because that one book, four hundred pages, is still just one line in the C.V., as is one of these snippets that's two pages long. It's length often is, unfortunately, more important than the depth or the uniqueness of what's being done.

I don't see any reversal of that, and I don't want to imply I'm disappointed by it because I think it kind of diminishes what seems to me to be the best of science. But that's the way the cookie crumbles. I'm sure that Leonardo da Vinci probably thought the same thing. (chuckles)

AM: Well, would you say that this trend away from monographs, or at least more thorough treatment of a particular problem, is -- what's the impact that that's now really not an approach that a scientist would take to their work. What are the implications of that, this isn't the way things are done? And how unusual was it then?

WS: I think one sort of made one's major statements either through books, whether they be monographic or whether they be textbooks. At that time that's what one sort of addressed. Or as was common then too, conferences in which all of the proceedings were going to be published, like the _____ conference, this again was a book-oriented sort of thing. The merits of that is that one can sit down with one publication and you have a full picture before you.

Today, with much of what gets published, if you really want to understand it, either it has to be in your area so that you know all of the technology that's implicit, or you have to go back and start with the number one paper. I'm not saying you have to go back and read the first paper on PCR [polymerase chain reaction], but to really say that now I grasp this problem, it's much harder. You just have to collect all these things. The total amount of reading may not be any greater. In fact, it might in some instances even be a little bit less because there will be a certain redundancy of description of methods and things like that that if you've already read it in paper one, you can skip it in paper two.

Of the many things that profited me from my association with Jim Neel was the study of science as he projected it. I had and still have great admiration for that. Jim wrote well. I mean, it's not often -- realize that very early in his career Jim thought of being a journalist. He wrote well, he could write quickly, he always wrote in longhand. I don't know whether he could use a typewriter or not. I presume he could. And he never got really accustomed to computers. But he wrote well and he always wrote from an outline. He had this stuff well organized in his mind. Rarely did he make changes when he had written it down.

When we wrote the textbook that was published in 1954, maybe the most lasting consequence of that was the tutelage that I unconsciously got from Jim. First of all, we were anxious not to have a book that looked like it was written by two authors, with two different styles. He went through very carefully everything that I wrote, making suggestions and corrections. I went through everything that he wrote but didn't have any suggestions usually. (chuckles) Mostly it was a matter of just trying to see how he elected to develop a topic. That was great. It was a phenomenal education.

I think that working with a book provides you with that opportunity. It's less likely to be so when you're writing a short article for *Nature* or the BMJ [*British Medical Journal*] or something like that. You just don't have the time or the luxury of a measured development. The whole thing is X pages, and boy, if it's longer than that, you won't stand a chance of getting into that journal. But writing a textbook or a monograph, that's really great. It gave you the space and the time and the carryover, because you could refer to things which were immediately accessible to the readers with just the previous paragraph, or the previous chapter as the case might be. Now you're referring to an article that might have been published three years earlier, and it's unlikely that they've got it beside them at their desk.

I like the sense of the wholeness that comes from a textbook or a monograph. The monograph, obviously, has perhaps more immediate relevance in most instances to the science because it's dealing with a problem less of education, although obviously it's implicit in that, but more a matter of dealing with one particular topic and going all the way through.

Whereas, when you're writing a textbook -- and this is particularly true with the textbook that we wrote. The overall editor was Ralph Buchsbaum. Buchsbaum had been at the University of Chicago and was at that time at the University of Pittsburgh. He was challenged with putting together a series of books aimed at one-semester courses, and by a mistake that I don't really know that I could recover, the decision was this could be about three hundred pages, that you could adequately cover in three lectures a week, sixteen weeks.

So when we set out to write our textbook, we had this target in mind. It had to be a multiple of whatever the unit was, that was in the neighborhood of three hundred to three hundred and fifteen pages. So that meant a certain parsimony if you were to adhere to that. It also meant that it had to sort of fit into the overall rubric of what they were trying to develop. C.C. [Ching Chun] Li was writing the book on population genetics. It was in that series, too. And he had the same sorts of problems that we had. Li's book came out later than ours, so it was the sort of thing that C.C. had read ours in draft and had responded to a number of the parts that I had written, in particular the mathematical ones. And very constructively and helpfully. Then when he came along with his own book, there was a -- if not a conscious citation, there was the recognition that this might fit into the larger scheme of things. So that was interesting.

We were asked to revise it, and we never did. It was just about the time that Curt Stern's second edition came out, and we didn't see ourselves as wanting to compete with Curt. After all, he was Jim's mentor. We thought the book was well done. I think it shows the fact that it was not written by a clinician. The most flattering things that I've ever heard said about our book was that this was really the advent of modern human genetics. I don't know whether that -- I find that very satisfying but I'm not sure it's true. (chuckles) At any rate, it was the first textbook in human genetics that tried to teach human genetics with the human as the object, rather than principles which were applicable to the human but were the principles experimental evidence had drawn from other organisms.

AM: And where would you put the [James S.] Thompson and]Margaret W.] Thompson [*Genetics in Medicine*]?

WS: Oh, that's later. I'm talking about 1954. Gosh, then the others, Thompson and Thompson [H.] Eldon Sutton's book [*An Introduction to Human Genetics*], the Mange's [Elaine Johansen Mange and Arthur P. Mange, *Basic Human Genetics*], they are a lot of them. But these things didn't come out for another almost a decade after we had written ours.

We had gone so far as to start, but then, as I say, when push came to shove and we had these monographs we were writing -- it was interesting, in the first ten years, part and parcel of five books. The textbook, the monograph on neurofibromatosis, the one on radiation, the one on inbreeding. Four, I guess. All right. (laughs)

V. Working on the Ionizing Radiation Studies in Japan

AM: Yeah, and very unusual because I'm not used to having to look up as many monographs. Clearly, textbooks, but not all yours are textbooks. I think that you yourself are pretty good at writing. Which brings me back to a point you briefly mentioned, and this is to return us to the chronology so we can move forward again. That is, when you came back from the war and finished up your degree, your bachelors degree, and got your masters degree, it seems to me it was this idea that you would be kind of a teacher of biology possibly, maybe in a zoology department, but certainly that genetics would be just a part of a teaching curriculum.

WS: Right.

AM: When you were at Ohio you get the job at McGill [University], which is basically what this would mean for your career, but you turn it down, not just because the pay wasn't good, there were other reasons. So when did you get the conception that, well, maybe this isn't the only road for geneticists, that they go and teach in a -- or was it a matter of other opportunities?

WS: Well, I suppose that, unwittingly, a large factor was the interview with Jim in the spring of 1949, when he learned that I would be graduating and would be in need of a job and asked me to come to Ann Arbor to be interviewed. I think for the first time that gave me a totally different picture of what human genetics could be. I realized that it would involve teaching, but not as much as would be so if I were in the Department of Zoology, or Biology for that matter, where I would be likely to be teaching things other than just genetics.

At Michigan my teaching would be human genetics. My primary function would be that teaching, to be involved in research, and would have some role to play in counseling. In a sense, over fifty percent of the time really was for research. That was a far more attractive perspective than going to McGill, leaving the difference in money aside. That, obviously, was the clincher. But it gave me a view of human genetics that I wouldn't have gotten and that I found very attractive, and it seemed to me was something that I could find a place in.

Jim really -- his vision of what genetics was and could be, human genetics, was all encompassing. That leaves a lot of room to insert oneself. (chuckles) So that was it. Then off to Japan. Then when Jim asked me to come back to Michigan with him, that was definitely it.

AM: Was it a position originally? Did you have the idea that if you went to Japan there would be a position waiting for you?

WS: No. When I went to Japan I had a two-year contract. That was the contract that was being offered. And there was the understanding that, if mutually pleasing, there could be a renewal of that contract. There was no commitment to any sort of a job, other than the possibility of renewal. And I never took the renewal because Jim, at the end of the first year, had asked me to come to Michigan, so at that point in time I didn't see anything to be gained by spending two more years in Japan at that particular time. So that led me back to Michigan.

I'm sure that both my wife **[Vicky Schull]** and I thought when we left Japan in '51 that that was probably the end of our Japanese experience, but God, it was just the beginning, in a sense. I began to then share the responsibility with Jim, closed the program. Then we had these add-ons with the inbreeding studies that we ran, and other things that were related to it, so it kept us continuously involved. And as I said, my interest in the opportunities in Hiroshima and Nagasaki moved on from not just genetics along to development biology and to cancer. So that kept all of it going. It was a very fortunate thing, in retrospect, but none of it was really foreseen by any means. (laughs) I wish I could have claimed that, but I didn't have that clarity of vision, that's for sure.

AM: Okay. And had you heard or known of Jim Neel before you went up to Ann Arbor, and were you aware of what he was trying to create?

WS: I had met Jim briefly in 1947 at the time of the rump session [of the American Association for the Advancement of Science in 1947] in Chicago, because he was there. He hadn't really published very much in human genetics by that time. The work that he had published and that I was familiar with was the work with thalassemia and Bill [William N.] Valentine. I liked the clarity of his writing and all of that. I certainly didn't know that he would develop into the person that he developed into at that point in time, but what I did see was satisfying. Personality-wise, there wasn't any clash or anything like that. I don't think the sort of situation would be the same say as some of the people who in the 1950s were chasing Josh [Joshua] Lederberg.

Maybe that's the difference between the kind of science that we were involved in at Michigan and bench science, because what I was learning wasn't a series of techniques. I guess in the broader sense it was, but I wasn't learning whether you use your pipette in your right hand or your left hand sort of business, it was more a matter of how to analyze problems, problems in which the data collection was going to be time consuming, and most likely, costly. These are not things that could be done on a shoestring. They were also things which, through much of one's career, probably would have to be done in concert with others.

It was just too costly to do the things. You couldn't have a lot of different studies, you know, a separate study going in Hiroshima and a another one in Nagasaki. You had to be parsimonious, even though that notwithstanding, it was expensive. The budget of the Radiation Effects Research Foundation runs on the order of forty-five million a year. That's more than some universities have for their entire budget.

But it's not small potatoes, and perhaps it encourages a certain conservatism, which might be good and it might not be good. You give a lot of thought to what you're going to do before you ever do any of it. It isn't something that you can sort of throw five different techniques at because you can do one in a day or two days and there isn't much lost. But in this, by the time you've recruited your staff, trained your staff, got the infrastructure in place that you need, you've invested not only a lot of money but a lot of time. You want to be damn certain that you're not off on some will-o'-the-wisp, that you may not come up with the answers that you had hoped you would get, but you have to come up with something which justifies the expenditure.

The neat thing about the studies in Hiroshima and Nagasaki were, in a sense, although they were going to be costly and they were going to be very time consuming, they would be, and still are, the most complete studies of the biology of a human population than have been done anywhere. The only thing that even comes close is Framingham [Heart Study], and Framingham isn't like Japan, because theirs was a series of interpenetrating samples so that you didn't have, as we have, twenty-odd thousand individuals who have been examined every other year since 1958. For almost a half century they've been coming in.

It's the sort of thing that you can do regressions on an individual person basis. Most of the time you've got one or two points. What can you do with that? Here, this is I think now the twenty-fifth cycle, and about six thousand people have been through every one of those cycles. The others have died or have been lost in follow up or something like that. But a very large proportion. Here, you've got twenty-five observations _____ cholesterol every other year on this person. What the hell has happened to their cholesterol as they've aged, and what can you associate that? It's a no-lose situation. It tells us more about us as members of a population, how much variability is there actually within people and between people. It's rare that we have that opportunity.

The few instances in which it's been attempted, other than Framingham, have almost always run out of money. The Tecumseh Study, the Alameda County Study, these were things that went for a few years, driven largely by the force of the character of one or two people, but someplace along the line, the design or what they were intending to do, or so on, was not so unusual as to provide the rationale for continuation.

I think one of the most amazing things about the study in Japan is that the study was so designed that it's been able to incorporate changes that have occurred in our understanding of human biology over fifty years, and it's still generating important information. Oftentimes, much as you would like to believe that you are looking down the road far enough, you're damn lucky if it's a decade in which what you are doing is really current and has been so constructed so that you can change without losing all of the past. Obviously, each one of these examinations is totally different than every previous examination. I mean, there's no continuity. Yes, we've seen these people twenty-five times, but there's no way to put that together in a meaningful way.

Even where there have been changes in technology, it's been possible to do, in sort of an ancillary way, the experiment necessary to be able to adjust the previous measurements to the current method of making that measurement. Like in cholesterol. The way cholesterol's been measured in the years that the adult health study has been underway have changed enormously. Yet, we actually have these observations and we can benchmark them all the way through. It's phenomenal. Now, with frozen specimens on so many people, they have, in a sense, been immortalized, maybe not in the way that they thought of (laughs) but they have been.

AM: In '47-'48, being this kind of young member in this kind of expanding field of human genetics, was this potential somewhere in the back of your mind of what this initial experience in Japan might offer?

WS: I don't really think so, with any clarity at any rate. There was, of course, the challenging issue. We knew that ionizing radiation produced mutations in Drosophila. There was some limited evidence that it would do the same as in 1947 in the mouse, because much of Bill [William N.] Russell's work was still underway at that point in time, so we didn't have the persuasive evidence that we've now got. But we presumed that, while the relationships might be different quantitatively, it would do the same thing in humans.

The question was one of, could we detect that, given that the doses which were used in experimental animals, both the mouse and Drosophila, were very much higher than what human beings can sustain with a whole body dose, which is what everybody got. At that time we weren't really sure what the LD50 [lethal dose] was. Now it looks like it around three hundred roentgens. Well heck, the Drosophila experiments were run at ten thousand. The LD50 in Drosophila, we don't really know, but it looks like it's in the neighborhood of forty to forty-five thousand roentgens. You practically can't kill a damn fly with radiation.

It was easy to generate the numbers of events necessary to demonstrate a dose-response relationship in the human with say a cap on the amount of radiation that a person could absorb and go on to reproduce being so low, relatively. You didn't know whether we would ever see another baby, whether there would be enough children produced for that. We've examined seventy-odd thousand. That's a whale of a big pregnancy study. Yet, that really wasn't sufficiently large to exclude changes of less than a doubling.

In a way, the pessimistic view was that this was going to be a lot of work and we're not going to get an unequivocal answer. Yet, that pessimistic evaluation was predicated on the supposition that the mutation rate in humans would be very similar to the one in Drosophila. We didn't know whether that was going to be true or not, and yet the issue was of such moment that we had to find out. So the justification for the study was that you can't walk away from this one, even though our best instincts tells us we're never going to get to an unequivocal position. But people weren't satisfied with that kind of evaluation. They wanted hard numbers.

You can still question how good a study of mutation per se _____, but it has answered a troubling concern of other people, whatever the origin, that there clearly was no epidemic of congenital defect. Even if the congenital defects were increased, it was small relative to the normal risk. Or as you were getting all this publicity, every other child would be a two-headed monster or a cyclops or something else. And that isn't so, patently. Now, what the human mutation rate is is still not that clear, but it's certainly going to be say within an order of magnitude of Drosophila probably.

AM: The design protocols of this particular study and the surveys that you were developing for the follow up and the idea that maybe it would be more important to follow up on the offspring rather than the survivors, in the back of your mind how far did you see this projecting?

WS: Oh, I think most of us thought maybe a decade. I don't believe anyone could honestly say, who was there in the forties, that they would expect us to be still going a half century later. (chuckles) That was beyond our fuzziest notion. We thought that ten years would probably give us enough information. The difficulty was that -- of course, we could make projections, which we did, as to the number of children likely to be born of exposed parents after 1953, which is when we decided to cut the program off. That number didn't seem to be large enough to justify the cost of continuing to collect that information. We could collect other information that seemed to be relevant, and it was much cheaper. Because the clinical program was expensive to keep twelve teams, six in each city in the field, seven days a week, nurse, physician, Jeep, driver. Even though things were very cheap in Japan then, it was still a large undertaking. Not the smallest part of it was making certain that things that were being done in Hiroshima were being done in Nagasaki and in the same way. We were counting on the capacity to pool the information.

The program came very close to closing long before that time was up, but by the time that the genetics program did close, leukemia had surfaced, and there were other twinges that were going on that suggested that other non-genetic effects -- carcinomatosis, carcinogenesis -- that needed to be followed. In the long run now, probably the richest body of information from that which relates to cancer is what ionizing radiation does.

Of course, now we have a program under way in Japan that is actually looking at diseases of later life of the children who were seen in the pregnancy determination study. I think most of that, that has a large political impetus, I mean, the children were concerned. And it was true that the mere fact that we didn't see anything in the first year of life didn't necessarily mean that something couldn't have occurred ten years down the road or fifteen years down the road. It appeared possible to remedy that, and the Japanese government was prepared to pay the expenses. So a study was set up which is now underway that's looking at that issue.

I will be surprised if it shows anything. But again, I think this is one context in which negative information can be as important as positive effects are. Because it's reassuring, or should be reassuring. Unfortunately, we have a cadre of scaremongers who no amount of information will ever change their minds. Either the study wasn't properly done, or the results are actually being subverted, which has not been true of the -- neither the Atomic Energy Commission or ERTA [Energy Research and Technology Administration] or DOE [Department of Energy] has at any time told us what we had to publish, or what we *could* publish, for that matter. So if it isn't there, it isn't there. That doesn't mean there isn't *something* there, but it's below our level of recognition, and that's pretty good.

AM: One last area I just want to talk about before we wind up for today and then move on and start up tomorrow is -- you briefly mentioned that -- and I know in your book on your experiences in Japan that you've married, and we haven't really talked about how that happened and when that happened and the timing of all that. Why don't we kind of just briefly discuss the other side of a scientist's life.

WS: I met my wife before I went off to war. We had no understanding at the time, or anything like that. We corresponded throughout the war years, and when I returned we reestablished meeting one another. My wife, she has two sisters. The middle sister, who was a year younger than my wife, was getting married. By that time, I was pretty certain that this was the person I wanted to share the years with, so I guess maybe the euphoria of being present at one wedding led me to propose too. So we were married in September of 1946, and we've been married now it will be fifty-nine years in this coming September.

Within a year of our marriage, my wife, who for practical purposes had never been out of Milwaukee except down to Chicago, or a little traveling in the states, we're off to Columbus for schooling. Then I assume that her family and she probably thought we'd find a job somewhere in the United States, hopefully in the Middle West, maybe in Wisconsin. (chuckles) And soon off we go to Japan. Vicky likes to tease me and say it's been all downhill since, because when I was in Japan -- meaning her -- I had three servants. We had a cook, a maid, and a gardener. (chuckles) And we've never had another one since. It was a rewarding experience for both of us in that sense.

She's been a helpmate that I couldn't ask more of. Very tolerant of my flights of fancy and whatnot. It's been one of those sorts of things when you don't really worry about the home front. I don't want that to sound negative in any sort of way. It's not that you don't go home of an evening wondering, my God, what the hell am I going to run into now? My wife's unhappy, a set of events that just don't seem to have a solution. That's never been the case. It's, with singular exceptions, calm and peace. (chuckles)

AM: Was she at Marquette?

WS: No. Vicky was working. She had a high school education at that time. She started school when we were living in Ann Arbor and went on -- she never actually formally applied for it, but she has roughly the equivalent of a bachelors degree, and I think they were calling it general studies. It was the thing that came in in the sixties in which all you had to do was take a certain number of hours. You didn't have to have a major or anything else. So she had taken German and French and Japanese. She enjoyed languages, and what else it took to prepare herself for that. So she had enough hours, and I'm delighted to say that she was a straight A student.

Of course, those were sometimes disrupting things at home. She was preparing for an examination. I'd come home to tell her what was going to happen in my class the next day, and I'd get this sort of far away look. (laughs) But that was great.

That's basically the situation. We've traveled extensively and lived in quite a number of places -- Germany, Australia, Japan. But we've not been very peripatetic as far as the United States is concerned. I don't suppose there are a whale of a lot of people in academic life who can say they've only to two universities. Twenty-one years in Ann Arbor, and I've been here thirty-three years. There hasn't been the temptation to move about. Of course, in that same period of time we spent ten years in Japan, and we spent six months in South America, in Chile, same length of time in Australia, Germany. I taught at Heidelberg for a while and at The Australian National University.

So there have been breaks when we've gone abroad, and found that rewarding and entertaining. We both like to travel, so that's a fortunate thing, because it does require some adjustments, and I think oftentimes, more probably for the wife than for the husband because I go and I'm inserted immediately into things that I know and that, in fact, are the basis for bringing me there to begin with. Whereas, she's suddenly dropped into a new culture, and it may be one in which she doesn't speak the language. You don't have any friends. People are usually considerate and try to be helpful, but they can't babysit you all day. So you're suddenly off on your own. If you're not resilient and not sufficiently curious, I'm sure it can be boring as all hell.

Probably that's one of the reasons why, in some instances, divorces are the consequence, because the husband has a travel bug that is not going to be sublimated and the wife doesn't like it. It's unfortunate that one has to discover that, and particularly for the children, but it does happen. We don't have any children, so travel has not been a -- it's literally lock the door and go.

AM: Okay. I think that's a great place to stop and we'll pick up again tomorrow.

WS: Okay.

AM: Thank you.

WS: Right.

[end session]

VI. American Society of Human Genetics; Racial Discrimination and Living in Japan after World War II

Session III

June 29, 2005

AM: It is the 29th of June 2005 and I'm Andrea Maestrejuan with Professor Schull in his office at the University of Texas School of Public Health for his final session for the UCLA Human Genetics Oral History Project. We're going to be probably jumping around a bit today just to get some things that I want to cover, but I think what we'll start off with is something you briefly mentioned yesterday as we were ending, and that was the AAAS [American Association for the Advancement of Science] in '47, where basically the Society for Human Genetics [American Society of Human Genetics] was born, so to speak. Tell me a little bit about that meeting and the events that led to that and the relationship then -- there's a brief mention of a Human Genetics Society of America, so maybe you can talk about the relationship with the ASHG to the GSA at that point.

WS: Well, prior to that session in Chicago, to the extent that human geneticists were represented in *any* genetic organization, there was like a small arm of the Genetics Society of America. There was no specific organizational structure in the United States, that I'm aware of, that catered to their interests.

There had been a slow but steady groundswell, I think, of feelings that human genetics deserved a professional society of its own. The people who were fostering that notion sought to find out whether there was actually enough sentiment to sustain an organization of that particular kind. As a consequence, they met at the time of the 1947 meeting of the American Association for the Advancement of Science. That meeting occurred, interestingly enough, it was the week between Christmas and New Year's, a period that today wouldn't float at all, but at that time I suppose that most of the societies that met in concert with the AAAS had their _____ academic communities of professionals. Of course, this was the time of the year when everybody was off, so it was easy for them to get together. Chicago was centrally located and had lots of meetings of that kind, and the facilities to do so.

I was already enrolled at Ohio State and had gone home, that is, back to Milwaukee, to be with family for the Christmas holidays, and since the meetings were to be in Chicago, which was then an hour ride, roughly an hour and a half's ride on the interurban, I went to the meetings. The Genetics Society of America was meeting with them in concert, so I could both go to the GSA meetings, as well as this other thing, that I didn't even know was going to occur but, once there, learned from Dr. Rife that this was so.

So I went to the meeting, kind of an informal one, which looked at the issue, and among the participants that I remember -- my guess is that there were probably between fifty and a hundred persons attended. I don't remember the exact number, and I obviously don't remember the names of everyone, but I know that [Herman J.] Muller was there, Charles [W.] Cotterman was there, Curt Stern was there, if I remember correctly, Herluf [H.] Strandsckov, who was at the University of Chicago, was there, of course, and Jim Neel was there, David Rife was there, and I was there, not that that counts in that category, but I was there, as was a colleague of mine who was another graduate student at Ohio State University at that time, an Indian by the name of Redid**.** We mostly were just present here.

The portion of the meeting, aside from the debate and the suggestions that were emanating from the floor as to the possible structure of such a society, the thing that I remember most clearly was Charlie Cotterman's presentation, with slides, having to do with whether or not, if a society was formed and if it sought to have a journal of its own, would there be enough interest to make the journal economically viable. At that time, to the extent that there was anything that published articles on human genetics specifically, it was probably the *Journal of Heredity*, and most of these were short and tended to present peculiar families of one sort or another. They weren't systematic. Obviously, if there was a journal, then there had to be an editor. The outgrowth of it was that Charlie was at least sufficiently persuasive that the bulk of the persons who were there felt that a journal *could* sustain itself. It would probably be tough going for the first few years. A quarterly would be the most likely structure.

The sentiment for that was strong enough that the decision was to go ahead, and Charlie Cotterman was sought as the editor. He was the first editor. I don't remember how long Charlie actually served as editor, probably three years or so. I know he was exceptionally conscientious, and as a consequence, the journal got off to a very good start, in the sense that the quality of the science it was presenting was really very good, much better than you might expect with a start-up journal. That was in large measure because of Charles' own heavy commitment to the thing. He published a couple of articles of his own in it.

I remember, in one instance, that was where a well-known syndrome in human genetics was first published. It's called Waardenburg's syndrome. P. [Petrus] J. Waardenburg was Dutch, and the paper was submitted to Charlie in German. He translated the whole thing, and even Waardenburg, subsequently, who could speak English, felt that Cotterman's manuscript was better than the one he sent to him. (laughs) In his clarity and in the things that he insisted that Waardenburg address in order to make it thorough. But that was the kind of dedication that he had. He read every manuscript, word by word, carefully.

The net result was that, not only could *he* be proud of what the journal was representing, I think those who were associated with the society could too. Of course, Jim Neel's paper on the carriers of genetic disease, one of those papers appeared there. This was where H. J. Muller's presidential address called Our Load of Mutations appeared. So these were papers which had a significant impact on the science at the time and certainly provoked a lot of discussions and thought in experimentation. So the journal got off to a very good start, and it really grew out of this rump session, in which there were enough people present -- although most of them were Drosophila geneticists, really -- to persuade those who were primarily interested in human genetics that there was the wherewithal to establish a society.

Through the first, I don't know, perhaps twenty years, maybe even longer, we were not a sufficiently large society that we ever met alone, as is now the case. The first year or two, we met in concert with the AAAS, and then they began to meet with the American Institute of Biological Sciences, the AIBS. I don't know exactly when that union began, I don't remember. I should know, but I don't really remember. My supposition is that it was around 1951 or '52, something like that, when they began to meet annually with the AIBS. This had a variety of benefits, because the GSA was beginning to meet with AIBS also. They met on college campuses during the summertime, so there was inexpensive dormitory space to live in, and there were, obviously, adequate auditorium facilities and the like. It was a convenient relationship, an inexpensive one, at a time when that was a consideration. When you were making only a few thousand dollars a year, you didn't want to invest it in getting to your annual meeting. That's my recollection of it.

I don't remember enough of the gist of the discussion to even enlarge on it to any extent. The thing that impressed me was Charlie Cotterman's presentation. I had known of Charlie Cotterman. You couldn't go to Ohio State and not. But I had never met him before. It formed a relationship that continued until Charlie's death. I was incredibly impressed by the originality of his mind. He was incredibly analytic in the sense that he enjoyed sort of the axiomatic approach to things. It ought to be like geometry, clear and neat. You start with these premises, and from those premises the rest follows. In his case, of course, the premises might very well be the experimental results or the clinical results that are presented to him. So his papers all are things that are more than important in their own right, but they're worthy objects of study for people who really want to write with exceptional clarity, because Charlie Cotterman could do that.

AM: Okay. And what do you think was the impact to kind of be distinct, then, from the Genetics Society of America by forming a separate society just for human genetics?

WS: Well, for the first two years, until we really got to be large enough to be unquestionably self-sustaining, since we met at the same time the GSA did, people could attend whichever session they really felt like. Of course, through the early years, most of the society's officers were actually people more recognized for their work in Drosophila or other organisms than they were for their work in humans, even though -- obviously, Muller always had an interest, so did Curt Stern, but their reputations as scientists rested primarily in what they had done in Drosophila genetics, instance of radiation or early linkage studies. Or in Curt Stern's work on the demonstration that the chromosomes are indeed the vehicles that the genetic information --

AM: And because you had trained early on in Drosophila genetics and then moved into human genetics, how were you describing yourself?

WS: Well, I just thought of myself as a geneticist. I didn't, at that point as yet, really clearly identify with any specific organism. I wasn't obliged to in a sense, because I was still learning the tools of my trade. (chuckles)

AM: Okay. Why don't we -- we need to move across a large gap, and the history of your work with the ABCC [Atomic Bomb Casualty] Commission has been written about by yourself and by historians who know the documentary history much better than I do. I just wanted to ask a couple of questions surrounding you and your motivations a little bit, and then kind of move on. I did want to talk about it in a couple of ways, and one was, when you went in the late forties on this two-year kind of stint -- the first time you had been in that area was as a potential invader and part of a military force. Then in the intervening years you'd gotten a Ph.D. and had another kind of professional identity. So when you went to Japan, what were you thinking about in terms of what your role would be and your attitude toward these people that you were going to be -- not treating, but collecting data on?

WS: I guess, in a sense, that first tour had so many different facets to it that it's hard in retrospect to really tweeze out some of these issues. First of all, when I landed in Tokyo in July of 1949, the country was occupied, the airport was managed by the military, you had limited access to the facilities. Commercial flights were coming and going. There weren't all that many. But it was primarily supporting the occupation, so you cleared through military personnel. When you left the airport, you checked through a military checkpoint. Housing for you, or places to stay downtown, were assigned on the basis of your rank, so you had to go to the military housing. The first few days, at any rate, and maybe the first few weeks that I was there, virtually all of my interaction was with non-Japanese. You traveled on a military train, you did all of those sorts of things. The Japanese were just people who were in the periphery of your vision as it were.

Then finally when I got to Hiroshima, I had been held in Tokyo for several days, which wasn't the usual fashion. Generally, at that time, when ABCC was recruiting, people would come in and they'd have to clear through and register with the occupation authorities, and they'd be sent on to Hiroshima as rapidly as possible. Well, this time, I was kept there for about a week, largely because Jim Neel was in Japan at the time and was bringing a _____ to help indoctrinate me and get me aimed in the right direction. Moreover, he made an annual report to what was called the Committee on Atomic Casualties, which was the oversight scientific body with the National Academy of Sciences [of the United States of America] for the studies in Japan. That report usually entailed quantitating what had transpired in the previous year, so I was to help him put those tables together and do whatever little preliminary analysis was necessary.

Then I went on to Hiroshima, actually to Kure. Of course, much the same thing happened there. You get off the train not where the Japanese would have gotten off, not that they were on a military train anyway, but you went through what was called the RT or the Rail Transport process, which governed all of the military transport. Though we were not really military, we were there at the sufferance of the occupation. So we were bound by all of their rules. Well, I didn't really know enough about the details in advance to realize that the area of Japan in which Hiroshima rests was at that time occupied by the British Commonwealth [Occupation] Force and not by American forces.

So again we sort of moved out of American control into Australian control because by that time most of the British troops had been withdrawn and they had been replaced by Australians. To the extent that one can characterize people, they're a gregarious, affable group, and they were easy to be around. They weren't anti-Yank or anything like that. We were put up in Australian housing, housing that would have been built for their military.

Then, really, I didn't sort of get my feet in the water, as it were, until I went a day later on to Hiroshima and was introduced to Carl [F.] Tessmer, who was then the director of the studies, and the other principal officers, and told that my office would be in the Hiroshima Red Cross Hospital and that the only unit over there was the genetics unit and that I would be the only one there. So I went to meet my colleagues and associates. Fortunately, two of them were Nisei. The sort of lead position, Koji Takeshima, was born and raised in Hawaii, and my number one secretary, Alice [Reiko] Iwamoto , had been born and raised in California. In her case, she'd been sent back to Japan just before the war for education, which was common with a lot of the families. Takeshima's father was a Buddhist priest and had had a temple in Saijo, which is now incorporated into Hiroshima but wasn't at that time. So when Koji's father returned to Japan, obviously he returned too, and he went to medical school in Japan, and, of course, was in the Japanese army, not electively so but he really wouldn't have had any other options. Fortunately, he didn't serve outside of Japan. He's now ninety, I still am in touch with him.

It was a very rewarding experience for the simple reason that if I could have handpicked the people with whom I worked, I couldn't have done a better job. They were marvelous, and there was enough command of English for important things between Alice and Koji that I was never at a loss there. And most of my Japanese colleagues, to the extent that they knew any additional language, it was generally German because the clinical language in Japan had been German, not English. But it was shifting under the occupation. They loved to try their English on me, sort of thing, and vice versa. We had many good chuckles together about Mongolian always being Mongorian, the *L-R* contradiction. (chuckles)

It was very rewarding, and I never felt then, or at any time subsequently, any deep animosity. There must certainly be some people who would just as soon cross the street as to walk past one of them, but the only sense of any of that that I ever encountered -- and this was always totally understandable to me -- Japan's economic situation was so desperate that the returning veterans were just cast loose on their own, and those who had lost an arm or a limb couldn't find jobs. I mean, whole bodied men were having difficulty, and so often they were reduced to begging and they'd be on the streets. Occasionally, if I were walking past, they'd turn their back toward me, or something like that. But no words, no nothing else. I often tell people I'm not sure I could have done any different than what they did. There was no sense of fear, no physical sense of fear at all.

You were naturally disturbed by the lot in which these people found themselves, because to the extent that they had any resources at all, they've always been hospitable. I mean, it's a hallmark of the Japanese, their hospitality, as far as I'm concerned. It may be structured, it may not be spontaneous, but it is the tradition of the culture and they all honor it. It was far different from say being, I'm sure, in China at the time, or in the Philippines, for example. The Japanese quickly started putting their house back together.

AM: And how well were you able to switch from a military mentality that said these people are my enemy and they caused me a lot of life altering experiences, and I saw the results by treating soldiers, American and allied soldiers, in the field, to a kind of more clinical, objective, scientific attitude?

WS: I think the transition had begun before I even returned to the United States. When hostilities in Luzon ceased, which was July of 1945, our division was in the far north and the company of which I was a part was outside a little town called Alcala. It was near there that our division and the Eleventh Airborne had met, and that sealed the valley, and for all practical purposes, the war was over. We began to get prisoners in some numbers, which we had never had before. The only ones you ever got were those who were so incapacitated that they couldn't destroy themselves. We began to get whole bodied people. Of course, once the word was about that the end had come in the Philippines, we had so many prisoners coming in with wounds, with cellulitis, with malaria, and everything else that, heck, we had no option but to set up tents to take care of them, too.

I spent much of my time then -- because we weren't doing any surgery, we weren't having any casualties -- actually working on Japanese prisoners. So this was kind of a transition. You began to realize that they were there with the same motivations that we were there. They were certain that they were right, they were doing what they had been chosen to do, if they volunteered, you know, what their country asked of them. And they were a formidable enemy. There's no question about it. Once this was over, I never had any problems with any of them. I would walk through the wards with a pistol at my side, which really, in retrospect, was a pretty stupid thing to do because they could easily overpower me. There was enough of them. But there was no effort whatsoever.

Early on, I ended up with a shadow. I'm sure he was probably a Japanese aide man, who followed me around and watched what I was doing, and the next day he would be helping me, he'd know what I needed and stuff like that. There, supply circumstances were so desperate that they didn't have much more than a Ringer's solution and a few things like that to treat any of their injured or ill. No anti-malarial, so they just shook until the chill passed. But you got to realize that they were human beings, and even if you couldn't communicate with them, they deserved the same consideration as every other human being.

That had started, so when I got to Japan I wasn't fearful in any sort of way. I didn't know what my interactions would actually be, and I suppose I was very fortunate there, as I said, in the sense that I had these two Nisei colleagues who could be the bridge, because language, obviously, was going to be the barrier, but they could explain things to me. And when I took an interest in learning to write, everybody was more than happy to show me how. Any evidence of interest in the culture and the language was more than met with enthusiasm and offers of help and whatnot. I really should have stuck with it, which I didn't, unfortunately, because I've regretted that ever since. Age and commitment would probably would have made it easier for me to learn then than later on.

I've never had any reason subsequently, at different times, even when you have strident anti-nuke groups almost destroying the whole purpose of the annual August memorials. I was never threatened or anything like that. I didn't feel apprehensive about walking downtown Hiroshima. That's just sort of outside the realm of what you would expect the Japanese to do, and they more than won my admiration for that.

AM: Well, in James [N.] Yamazaki's memoir, he mentioned that, particularly when he got to Japan and before he went to Nagasaki, that the British occupying forces, including the Australians, were particularly difficult to deal with and even he as a Nisei experienced a lot of discrimination. Were you witness to any of that?

WS: Well, yes. I've known Jim for a half century or more and he's a good close friend. The situation was, of course, the Australian policy was white and it wasn't just Japanese, it was Chinese, Filipinos, it was a very rigid white Australian policy, which meant, therefore, that housing that the Australian's controlled was not open to anyone except Americans or Australians, British, any other white person who might have a claim on such space.

We had other Nisei besides Jim Yamazaki there. Clinicians. Mack/Max [Masao] Tzuzuki **,** who was an obstetrician, Watsutu, who was here for years, another pediatrician. Mack wasn't married, so that was no problem. Watsutu was. But you see, at the time, you really had sort of two different aspects to the occupation. There was on one hand the forces there to subdue any uprisings that might occur. This was the military. In most of Japan, except for the area known as Chugoku and the island of Shikoku, the military forces were all American. That island and that region on Anju **was** the occupying zone for the British, so that the troops that were there to police, to do things like that, were, by that time, Australian. They had been originally British Commonwealth forces.

But adjacent to that throughout Japan was military government, and military government was all U.S. It never incorporated anybody else. So in places were military government would be set up -- and in Chugoku the military government headquarters were in Kure, so military government would have, of necessity, a fairly substantial number of Nisei because they were the ones who knew the language and whatnot. There was a housing area in the little community of Hiro, known as North Camp, which was open to American military personnel or persons assigned to military government because they were sort of like Department of Army civilians. But that was small. And there's always problems with housing there because there just wasn't enough to go around.

So there wasn't housing for Jim and Aki Yamazaki when they arrived there, and the decision was Nagasaki needed American personnel. Nagasaki was in the American zone of occupation, so there wouldn't be any problems of that nature at all. I think in some ways it would have been a very difficult situation for anyone. I think it was particularly difficult for Jim, partly because he'd seen service. He was a prisoner of war. He'd been captured by the Germans and was liberated with the Battle of the Bulge, in the subsequent follow up. So he felt, and properly so, that by God he'd done his part for this country, and yet he was being, as he saw it, discriminated against.

Well, the relationship between the powers that be were such that they were oblivious to such kinds of understanding, so Jim was sent to Nagasaki, as were the [Stanley and Phyllis] Wrights when they came. Of course, they were *the* American physicians who were there for a number of years. I think that's where Jim's sense of discrimination occurred. And it was. I mean, it was certainly housing discrimination at the very least.

I don't think, as individuals, the Australians were like that. A lot of the Australians already had Japanese wives. They couldn't acknowledge them officially and weren't able to until I suppose the mid-fifties, '56, '57 or something like that when the Australian policy was changed, and the ones who had married Japanese women were now able to officially have that marriage acknowledged in Australia and their wives be made citizens. But up until that point in time, it was kind of a dicey situation even for the Australians. As I say, they never believed in anti-fraternization. (chuckles) They fraternized from the outset.

AM: Okay. I know after your two years in Japan, Jim Neel offered you a position at Michigan. Did you have the option to stay in Japan?

WS: If I had wanted to, yes.

AM: And why did you choose to not stay?

WS: Well, I felt that, first of all, it would get me back into the stream of academic life that really had attracted me initially. Secondly, I had the feeling that the longer I stayed in Japan the more divorced from that dream I would be, and in a sense, more the opportunities to do the sorts of things that I wanted to do would be compromised. In fact, when I was leaving Japan in 1951, by that time Carl Tessmer had returned to the United States, and the director of ABCC was [H.] Grant Taylor. Grant Taylor was looking for a head of statistics and specifically asked me if I wouldn't stay to take on that task. Much as I liked Grant Taylor, I felt that that wouldn't solve my problems because it was the same sort of thing, but I did agree to help search for a replacement for statistics, the job that was open there, and I did. There were, in effect, two options to stay on, either in genetics or in statistics itself. My replacement in genetics was Duncan [J.] McDonald.

Given the two sides of the scale, it seemed to me that my future really rested with Michigan and I couldn't imagine a better opportunity opening up even if I stayed in Japan for another couple of years. After all, this is a major school, well regarded. By that time, I knew Jim much better and I had developed enormous respect for his ability. The things that he was interested in doing were of interest to me too, because this was a continuation of the whole issue of the study of mutation. Particularly, my commitment was to the estimation of spontaneous rates of mutation. So it wasn't a difficult decision as far as I was concerned, it was just the most logical one and the one that was in my best interest, at least as I saw it.

VII. Study on Inbreeding in Japan; Commentary on Genetics, Radiation, and their Relationship

AM: In [M. Susan] Lindee's book, she portrays kind of a schism between McDonald and Newton [E.] Morton in Japan and you and Jim Neel, and to a certain extent, at least how I read it, that it was McDonald who felt like the results needed to be calmed down, toned down, rather than some basic differences in approaches to the results and scientific interpretation of the results. I just wanted to ask, how would you characterize that? Your relationship with McDonald and Morton in Japan?

WS: I didn't really know Newton very well at that time. I had met him at the Genetics Society meetings in Minneapolis when I had returned in '51, and he was on his way out to Japan. Duncan McDonald had come a couple of months before I left, so we had a lot of interaction. Duncan is one of those people that needs to be characterized as an unforgettable character.

I think, to me, the basic difference was one of philosophy. I certainly never sensed any personal animus here at all. We had, largely me, elected to look at the data using statistical methods that are called nonparametric, so there are no underlying assumptions about how things are distributed. This, obviously, precludes then things like regression analysis because that is a parametric procedure. Whereas, Newton, I think more than Duncan, had campaigned for a parametric approach.

My concern rested largely in two things. First of all, in regression analysis one makes the tacit assumption that the independent variable is measured without error. The independent variable would be dose. There were no dose estimates then. The only thing that we had to guide any sort of hazarding as to what they might have been exposed was the publication of what was called a nominal atomic weapon, and nobody knew whether that really was characteristic of Nagasaki or Hiroshima or what. So if you were going to adopt the parametric approach, you had to assign to either individuals or to groups of individuals an estimated dose.

Well, there's no possible way in my mind, then, that you could do that with any measure of reliability whatsoever. I felt that we needed a method which allowed you to look at grouped but ordered exposures. We knew that people who had symptoms of radiation illness had to receive more exposure than people who didn't have those symptoms. And we knew to a rough order of magnitude that a dose had to be proportional to the distance from the hypocenter. So we could categorize people who were close to the hypocenter with and without symptoms, a little farther, and so on, so we could kind of rank order doses but we couldn't put a number on those doses.

Then we could ask questions, such as, is there any evidence that these categories differ significantly one from another? And if so, do they appear to differ in a fashion which is consistent with the notion that doses are increasing on average as you go from distant from the hypocenter closer to it.

So it was more a philosophic one than anything else. If you know Newton, Newton will hold his opinions very strongly, as does Jim Neel. So I sort of found myself in between the two camps. There was never any disagreement about what the data were telling us. It was just more a matter of how should we present it, what form should the analysis take? Basically, we ended up, in the monograph at any rate, citing the method that I had advocated, that is, nonparametric analysis of data, which were grouped by distance and symptomology.

AM: Okay. I'm going to run a bit ahead here to just talk a little bit more about the kind of scientific aspects and implications of your research. In the Effects of Inbreeding [William J. Schull and James V. Neel, *The Effects of Inbreeding on Japanese Children* (New York: Harper & Row, 1965).], the monograph that you wrote with Neel, you take on the Morton, Crow, Muller hypothesis on genetic load [Newton E. Morton, James F. Crow, and Hermann J. Muller. 1956 An Estimate of the Mutational Damage in Man from Data on Consanguineous Marriages. *Proceedings of the National Academy of Sciences of the United States of America* 42:855-863.] Why don't you talk a little bit about that particular -- I don't know if you want to call it a controversy. That may be a little bit too loaded of a term. But just these competing explanations for persistence in genetic variance in humans.

WS: Well, these two schools of thought had existed long before we got involved in inbreeding. They were, in effect, championed on the one hand by Muller, on the other by [Theodosius] Dobzhansky, Muller being the one who believed that variability is maintained by a balance of mutation and selection. Dobzhansky, on the other hand, maintained that the variability was sustained by a balance of opposing selective forces.

Each could cite their own sort of experimental data in support of what they were conjecturing. There wasn't really anything in the human that told us one way or the other which of these should be possible. In fact, the argument had reached the point where I guess it was primarily semantic and we weren't going anywhere.

Then in 1956 in what certainly was a seminal paper, Martin, Crow, and Muller had come up with this notion that maybe you could tell which of these hypotheses was the prevailing one by looking at the effects of inbreeding. The contention was that if the predominant force is balanced selection, then the ratio of the rate of increase in disability with inbreeding would be a small number, a number determined by the number of alleles on average at a particular locus. So you were looking for something that might be -- we didn't know how many alleles are present at the average locus, but a small number, let's say under ten at any rate.

The other hypothesis said, in effect, that it's going to be the ratio of one to the amount of selection which is actually occurring in the heterozygote. That should be a large number. So if you looked at this ratio of the intercept to the rate of increase with inbreeding, that ratio was a measure -- or might provide a measure between these two notions.

Well, it prompted an interest in inbreeding, not only in Japan but elsewhere, and by that time we already were interested in inbreeding just for its own sake without initially realizing that it might provide insight into these competing hypotheses. I don't think any of us who went to Japan in the early years anticipated seeing the rate of inbreeding which was actually occurring in the Japanese at that time, not now. And when we did, this gave you a lot of opportunity to do things, so we'd already set into motion studies that were going to try to document better what actually was occurring. The last real documentation went back to the nineteenth century, and the way in which that sample had been put together was open to a lot of questions. It was opportunistic, to say the least, and we didn't know how generalizable the results were.

So the Japanese situation was much better. We had a very large number of children born to parents who were related one to the other, who had not been exposed to ionizing radiation, so you weren't inadvertently confounding these things.

The first big clinical study -- we'd started planning this thing in 1957. We'd been talking about it for a couple of years prior to that, and the actual observations began in 1958. Between 1958 and 1960, when the data collection aspect or phase was completed, we saw something in excess of six thousand infants. Well, they weren't infants any longer. They were between five and eleven years old approximately.

Then when the analysis of the data was completed, several things occurred. First of all, the ratio ended up in the Netherlands. (laughs) By that time, there had been a lot of thought given to the notion of loads and to the various kinds of loads. There wasn't just the simple mutational one as opposed to a sense of non-mutational. There were all sorts of things that contributed. No one was really sure what these other aspects or components of the load actually were doing to that ratio.

I think the fairest and least objectionable interpretation might be that, after an enormous amount of work, and work which provided far more empirical information on the consequences of inbreeding, which are not great, it didn't provide the answer that we hoped it was going to provide.

In time, then, of course, those two hypotheses were largely offset by the notion of neutral mutations, and so on. The theory moved on beyond that point. But as I said, since almost every culture has -- Christian cultures, at any rate, including Judaism -- has had some sort of prohibition on the marriage of close relatives, there had always been the question, was this something that stemmed from the recognition that there were deleterious physical consequences as a result of those marriages, or whether this was simply a social phenomenon to maintain harmony in families when families were extended and you lived with a lot of your relatives?

Well, I think the answer is now primarily in the social aspect, that while there is, in my mind, no question but what marrying of cousins increases the prospect of early death of the child of congenital defect, these are not overwhelming increases so that you would just on the very face of it presume it was stupid to do that, that there are other considerations, the family and all the ramifications of what family means, that could offset those limitations if people chose to see them.

So we ended up, unfortunately in that particular instance, without an unequivocal answer. I think, in a way, most of us in human genetics expected that when you start talking about human populations, you're talking about such a complicated mix of biological events and non-biological events -- social, cultural, and all the rest of them -- that you can't always pull these things out nice and neatly. We had thought that Japan would be useful because there would be a large measure of cultural homogeneity and all of that, but it really wasn't so.

AM: Okay. Well, in '53 you published a paper in *American Anthropologist* [William J. Schull. 1953. The Effect of Christianity on Consanguinity in Nagasaki. *American Anthropologist* 55(1): 74-88] on the different religious cultures and _____. Two questions. When did this idea to study the inbreeding occur? Because this is a little bit earlier than what you said before. And secondly, in the era of molecular genetics and what [Jonathan] Beckwith and [Ruth] Hubbard and others have called the hegemony of the gene, most geneticists would probably raise an eyebrow as to looking at a cultural factor such as religion in the context of studying genetics, and I just wanted to put that out to you.

WS: I think motivated by several things. First of all, through happenstance in Nagasaki but not in Hiroshima -- Hiroshima didn't have a substantial Christian population then, and it doesn't have one now. That was not true in Nagasaki. Nagasaki had a -- depending upon when you elect to measure, thirty or forty percent could be Christian.

Now, the irony of it was that the frequency of Japanese who were Christian was inversely proportional to dose, because the bomb happened to be detonated in the most predominately Catholic area of Nagasaki, so that the people with the highest dose were those with prohibitions on consanguinity, and while it didn't follow a strict linear relationship, nonetheless, as you move farther and farther away from the hypocenter, the proportion of individuals who were Christian declined.

The logical question was, if you have these two different cultural faiths which are correlated to some degree with exposure, what is that going to do --

[pause]

AM: Okay. So we were talking about the initial idea to look at consanguinity and --

WS: Right. It stemmed in part from the fact that I hadn't, certainly when I first went to Japan, hadn't appreciated -- I didn't know that much about Japanese history, of the importance of Nagasaki in the introduction of Christian notions into Japan. I certainly wasn't aware of the enormous amount of work that had been done by [Charles R.] Boxer [*The Christian Century in Japan 1549-1650* (Berkeley: University of California Press, 1951)[and others and hadn't appreciated the extent to which Nagasaki and Nagasaki prefecture were sort of the focal points of that, or the important role that Catholic missionaries had played in Japan, mostly in the sixteenth century.

So when I got there, I began to find out more. It actually started -- we were interested in just the business of -- I soon learned that the Bishop of Nagasaki at that time, his name was [The Most Reverend Paul Aijiro] Yamaguchi, had a rather strict attitude towards dispensation for consanguineous marriages. I did know that you could easily determine among Catholic marriages what the frequency was because there had to be a dispensation. Secondly, marriages had to be enrolled in the Liber Matrimonium so that you had both the denominator and the numerator, and you could do this all as sort of an exercise in record examination.

When we did that, I was working with a young Japanese by the name of Dr. [M.] Furuta. We got particularly interested in those situations in which the Bishop had given dispensation because he was generally adamant about not doing that. Almost without exception, there were really extenuating circumstances. In one instance, it was a leper whose cousin was prepared to take care of him, so rather than just having them cohabit, the Bishop thought it was _____ (laughs). Things of that nature. Then, of course, that got us interested in the other aspect of how dissimilar the rates were between Japanese Christians, as exemplified in that instance by the Catholics in Nagasaki, and non-Japanese.

Well, this had obvious biologic implications as well, so the thought that maybe it was worth looking more closely at the outcome of consanguineous marriages, and then how could we identify a studiable group, what would be the limitations that we might recognize in any such group. For example, utilizing the pregnancy termination study that was underway in Japan to identify consanguineous marriages meant that we would never recognize, or we wouldn't ascertain those consanguineous marriages that were infertile because they wouldn't have a child. So we'd start with marriages which were of necessity fertile. There were reasons for believing that the rates of infertility between consanguineous and non-consanguineous marriages might be quite different, so that led to a second study in time.

At any rate, we began to sort of incubate this, and this included talking with Japanese colleagues about what would be the attitude. There was never any real problem. The Japanese accepted this practice. So we had three young Japanese physicians come to Ann Arbor in late 1956 and spent most of 1957 there to start with the planning notion. Then, obviously it takes time to get financial support, because this -- though consistent with the commission's interests because it did deal with the interpretation of the impact of mutational events, it was not strictly related in the most direct way to exposure to ionizing radiation, so it meant that one had to find other sources of money, and that takes time.

Of course, those sources of money were forthcoming, but not instantly. We also had to get approval of the Committee on Atomic Casualties to use ABCC's good offices and space, and personnel in some instances, to support these. With a study of this size, there are eighteen months almost of preparatory work that goes into launching it.

Then once it even got to Japan, there was a long period of negotiation because these were going to be children of school age, so we had to negotiate with the school authorities and the PTAs to make sure that they were sufficiently supportive, that they would see fit to allow the child to be absent for a half day, have excusable absences. At that time, and still, education is a driving feature of the Japanese culture. It is the avenue to success. I mean, it's the only place in the world I know of where you can be a failure because you didn't get into the right kindergarten. (chuckles) The juku has become a standard feature of Japan. It's not content for kids to go to school five and a half days a week, but then on Saturday afternoons and evenings they go to a juku, which is cramming them with information for these examinations.

So, obviously, to get permission to leave school was a consideration. The school was sympathetic to it, so the schools were. We didn't have any problem with the system. It was a matter that you had to talk to all of them, and the net result was that we must have drunk enough green tea to float a small battleship because that was always served, and the negotiations took on sort of a pro forma aspect, question and answer and whatnot. It was entertaining.

They were extremely supportive, and it was a difficult situation, you see, because many of these children, the mothers, understandably, wanted to be present with the child when the child was being examined. This meant, given the flammability of Japanese houses, that somebody had to babysit the house while Mother was gone, so this would mean negotiating with a neighbor to do it, or somebody else. So even from the standpoint of the individual family, they were very sympathetic to what it was we were wanting to do, and anxious to participate, but it required a fair bit of adjustment.

I've never been involved in another study in which the level of participation was so high. Our refusal rate in Nagasaki was a half of one percent. In Hiroshima it was one and a half percent. Most group studies in the United States, when you're looking at individuals who are not conspicuously ill, think seventy to eighty is fantastic figures. That wasn't acceptable at all there. It was incredible.

AM: To bring this forward a little bit -- well, a lot. Many critics of kind of the genomic era have been critical of molecular genetics because they would argue that -- or at least my interpretation of it is that they'd argue that the molecular aspects of genetics have been privileged over the environmental influences on our understanding of genetics and genetic interaction. In this context, this work, at least in this one paper, of the more cultural environmental impact of some social norms or standards seems kind of odd, I guess, in an era where you really look at molecular genetics. Where do you fit in in your opinion on has genetics become too molecularized and that the more environmental interactions, our understanding of environmental actions and its impact on the genome has been minimized?

WS: I guess I would come down on the side that too many molecular biologists see the world in a way far more simplistic than it is, and that if you think of those diseases where straightforward molecular biology has solved a problem, they are virtually zero. The things which killed most of us are cancers, of which the vast majority of them haven't been shown to be single gene defects. Cardiovascular disease and the like. And these are things in which there's no question but what there is a genetic contribution to one's proclivity to develop the disease, and that's not the sole determining feature. That is, you don't get that disease regardless of what environment that you're in.

I think that what is happening now is a slow but steady shift towards the recognition that these disorders, those sicknesses which account for the bulk of human distress, are not simply inherited, and that we need to understand how environmental and genetic factors interact, and we need to understand how one gene interacts with another gene, in a far better way than we do now. I am apprehensive that there's been an overzealous statement of the potential benefits, when I don't think those benefits have been demonstrated at all. The few efforts that have been made, such as the case in Pennsylvania, have not proved very effective, and in fact have been downright detrimental. So I think that the neatness of molecular biology has perhaps led to a simple interpretation of the world which isn't consonant with what most of us see about us.

Not to mention there's big money. Beginning with the human genome project and now the efforts to do the same sort of thing in cancer and the like, these are scientific factories that got constructed and it's going to be hard to disassemble them, if they are ever to be disassembled. I think specifically, for example, it's now thirty-five years since the lipid research clinics were established to solve a problem which was going to be solved within a decade. Those clinics still all exist. They have a different name now, but they've never disappeared. And it's easy to understand why. After all, these become the major source of financial support to a great number of scientists, not to mention technicians, secretaries, everybody else, because of the nature of federal funding. It'll cover almost any expense.

The universities clamor for it because we wouldn't have the huge administrative bureaucracies that we do if the states were paying for that. It comes out of these overheads which can -- in some university situations, the overhead is as large as the direct cost. It's a hundred percent.

The whole thing has gotten kind of -- like health insurance. For a long period of time, people argued, and rightly so, why do we invest so much money in the benefits associated with pregnancy, when if there was ever -- if you can call it an illness at all -- if there was ever a plannable one, that's one that you know. First dollars were going there -- and so many were going there, certainly, in the fifties and sixties -- that far more catastrophic illnesses were not being covered. You were being cut off because your expenditures lapsed. It was like automobile insurance. Do you cover the first bit of damage or do you set aside say two hundred and fifty dollars deductible and you kick in then, so that you don't get all these trivial kinds of costs. Not to imply that any healthcare cost is trivial, but it has led to people over-abusing emergency services, and things like that.

So we need a better perspective. There's no question about it. And it's going to be hard to get because it's become so convoluted, so mixed with so many other features of our society.

AM: I wanted to just touch on that by going back. What do you think the impact has been of the ABCC and now the Radiation Effects Research Foundation on the field of genetics? And looking at it from the standpoint of funding of genetics and on the other aspect, is it a public perception of the role of genetics in society or a public understanding of the importance of genetics?

WS: Well, I think the studies in Japan, as well as others that have been conducted elsewhere, have surely demonstrated that as a mutagen, ionizing radiation is not a particularly potent one, that there are all sorts of chemicals that are far more potent. So that the fear associated with exposure to ionizing radiation has to stem not so much from the health consequences as other features associated with ionizing radiation. You can't smell it, you can't taste it, you don't know when it's happening, all of these uncertainties. And the fact that many people see much of the exposure to ionizing radiation as an involitional thing. You didn't choose to be exposed. Somebody else exposed you.

I think the studies in Japan have certainly provided the solidest footing that we have, both with respect to the mutational effects of ionizing radiation and the somatic changes that are associated with it, that we're going to get. Sure, the dose has been revised and revised several times, but at a point, those subsequent changes that have occurred are not going to change the basic notion of what the risks are. It may change the second decimal or something like that, but I don't think most of the people let their lives be controlled by the first decimal, much less the second decimal. (chuckles) We look at, is it less than ten percent, or is it ninety percent, or what?

I think judgments are made on the larger perspective of risk, and even though society seems to think that you can move towards a riskless state of life, it obviously isn't true, and sooner or later, we're going to have to apportion, I think, our resources in a more realistic way, more to what the risks actually are. We've seen so many, I think, miscarriages of do-goodism. Asbestos for example. If the money that had been spent in taking asbestos out of schools had been spent on teachers instead, we would probably be much further ahead educationally than we are.

On the one hand I'm deploring it, and on the other hand I'm certainly not in any way saying that society doesn't have a right to assign its own priorities. I just would hope that in the assignment of those priorities it was based upon solid information and measured judgment as to the impact. I don't think that's true. I think the way we get our information now is so colored. It passes through so many advocates of one philosophy or another that the person who really is seeking to understand may not have an opportunity to do so.

AM: Okay. Again I'm going to be jumping around, but we will get to Michigan and your move here, but while we're on these themes, I want to talk a little bit about some editorials in letters to editors that you've written because of your role as being on the Committee on Human Genome Diversity [in the Board on Biology of the National Research Council's Commission on Life Sciences] to write a report on the human genetic diversity project, which was confronted with a bunch of protests by different -- particularly especially, I guess, ethnic groups. How much do you think this kind of public perception and understanding of genetics has played a role in trying to move this particular project along, the human diversity project?

WS: That's a very hard one, in part because rarely -- even in our situation then, for example, what we heard were people who purported to be speaking on behalf of a constituency, and we don't know whether they had a constituency to begin with, or if they did from the point of view that they were projecting on us was actually the point that the bulk of those people held. Short of actually going out and doing opinion surveys in every conceivable culture, I don't see how we'll ever know.

You can carry this to, I suppose, illogical limits. Some were even arguing that, golly, if you're going to ask a town in Italy whether a study should be done in Italy about the genetic diversity among Italians there, should you be asking Italians who live in the United States, France, every other place? I mean, are you invariably a member of your ethnic community wherever you might be? If you take that perspective, my gosh, we could never have undertaken the studies of sickle cell anemia, for example, without having first canvassed all of Africa. That doesn't seem to me to make a great deal of sense. You deal with the here and now, and the now is what's around you.

I think, to some extent, it probably reflects an overreaction of an authoritarian insensitive past -- often insensitive past. I don't want to paint those individuals who are responsible for the decisions which are now deplored as being insensitive necessarily. I think they may have been less attuned to the prospects of differing opinions than they perhaps should have been. But golly, for most of us -- and I think the average individual today probably interacts more with his physicians than was ever true fifty years ago, for example. Your physician said something, you did that. Now, people are beginning to question whether or not that's really the best solution, for them, with their illness. I think that's good.

It can be carried to a limit, though, in which abysmal ignorance is given the same weight as an Einstein (chuckles), on matters of physics, for example, which doesn't really follow, it seems to me, or shouldn't follow. I don't know. I'd as soon not cope with those problems. (chuckles)

AM: Okay. Some of these concerns have to do with the money that will be made. How much is this a responsibility of the field of geneticists, scientists in general, who now live in a situation where cell lines can be patented, DNA sequences can be patented? Henrietta Lacks certainly never got any reward for what she has done for molecular biology. And that everybody wants a piece of the pie, because the pie has suddenly become very, very lucrative.

WS: Right. I know. It is going to take some courageous leadership to get us out of this mess. (chuckles) I don't see that kind of courage anywhere, at least not at the moment.

AM: Well, to get back to your history again and to follow up on just what you were saying about the effects of radiation on mutation rates. Muller has come -- his _____ is a surprising, at least surprisingly to me, character who has come out in many of these interviews. Clearly, his scientific work stands alone. He was a Nobel Prize winner, so obviously he should come up in these. But he's also come up in other ways in his promotion and his activities on the AEC [Atomic Energy] Commission, discussing what are the permissible levels of radiation for humans. You've written a little bit about this in I think the *Journal of Radiology*. You also met him. I know you met him in Japan, and I'm sure you've met him subsequently as well.

WS: Yes.

AM: What was your relationship with Muller, and how did you feel about his particular stance on what the permissible levels of radiation in humans should be?

WS: Herman Muller was a very quick mind, very good experimentalist, and I underline very good. The contributions that he made to genetics above and beyond just measuring radiation, because some of those were in an effort to *be able* to measure radiation, have been incredible in Drosophila. I don't know that he and I would have differed in any sort of sense as to permissible levels. In the best of all possible worlds, we would not be exposed to ionizing radiation, presumably, yet it still remains one of our most effective tools for the treatment of cancer. It's all about us, whether we want to acknowledge that fact or not. The earth's crust is endowed with a lot of radioactive materials. We invariably are concentrating these things in factions which most people don't realize, but they're just part of our natural biology.

Let me give you an illustration. At the time of the [Castle] Bravo test incident, the Japanese got worked up over the prospect of contaminated tuna coming onto the Japanese market. There was an article that appeared in one of the Osaka newspapers, the headline of which said, "Fish eaters," -- and these are people who presumably got some contaminated fish -- "you're radioactive." Now, the thing that that title didn't say but the article did was that everyone's urine is radioactive because you concentrate potassium, among other things, and there are naturally occurring radioactive elements, there's an isotope of potassium, so that you don't have to have the contaminated fish to have urine which is radioactive. They hadn't compared it against anything else. It was just that, yes, if you held a Geiger counter up, you could measure it. The supposition being, which wasn't true, that if you took somebody else that didn't eat the fish, you wouldn't hear anything.

There have been these kinds of misguided, I think, instances which have led to a fear, an almost phobia concern with radiation that isn't anywhere nearly consonant with what it does in fact. You get it -- whenever there's an exposure, there's going to be this epidemic of monstrous children with Cyclops or two heads, or so on. It hasn't occurred, despite all the newspaper comments to the contrary. They pick up *a* single child and -- for example, it was recently called to my attention, it had to do with depleted uranium, and a child living in a home near a military base had hair lip, cleft palate, obviously due to the fact that depleted uranium bullets were being used in exercises on the base. They don't stop to tell you that hair lip and cleft palate is one of the most common of congenital defects. It occurs in approximately one in every five hundred or six hundred births, and in the absence of any exposure whatsoever that anyone knows about. Moreover, it's hard to argue that, in my view, that enriched uranium is deleterious and depleted uranium is simultaneously deleterious. So how can you come to these things?

These are advocacy positions often that I think stem from a distrust that lies deeper than the science itself. Either these are people who insist upon having a bigger voice in the affairs that affect them or the culture of which they're a part, or something. It's beyond, in my view, a rational objective look at what is happening. You can always find a comparison in which the risk is much greater, and we entertain it daily, almost like walking across the street. There are areas, I'm sure, in Houston in which the risk of an accident is at least as big as one in six hundred. (laughs)

AM: Right. And what's the responsibility of the scientist and the geneticist in this? Particularly when the state mediates many of these.

WS: That's a very difficult thing. I think in the early years we were remiss in that we assumed we had discharged our responsibility to society when we did the best study that we could and we spread the results in the scientific literature. We didn't ever really try to write to the public, telling them what does that actually mean for John Doe and Mary Smith sort of thing. We assumed that that was the task of someone else. Well, I think that by doing that, we opened the door to special interests, who are not above, if not manipulating the data, at least selecting it in such a fashion that it satisfied their special interest.

I think it's unfortunate that that occurred. What disturbs me is I don't know how we can redress it now. I mean, people who are really interested in saying ionizing radiation -- the League of Women Voters has a couple of little inexpensive publications -- they're pennies, literally, maybe seventy-five cents or something like that -- with a very straightforward explanation of what ionizing radiation is, what it does, and so on. And one can't -- I don't think you can consider the League of Women Voters as a conspicuously partisan group, except maybe insofar as getting women to vote, but definitely not in their attitude.

The National Radiological Protection Board of [Great] Britain puts out a very good simple thing, which you can write to them and they'll send it to you. It's not an advocacy publication. Theirs you might worry about because it radiation is in their title, but certainly not in the League of Women Voters. And they have those things not only with respect to exposure directly to ionizing radiation, but they have commented things on like proximity to a weapons testing facility, or something of that nature. They have done, I think, a very good job in the main, and there are those sources available. But it's generally not known that they are available, so people tend to get their information from these second-long sound bytes, which are often colored by self interest.

How you now are going to do it -- I mean, inadvertently, there has been created in the public mind an attitude that isn't going to be easily erased or isn't going to be easily redirected. It's like trying to get a resolution of interest between the Democrats and the Republicans. They enjoy the wider the divergence.

Organizations like the National Radiological Protection Board in Britain, the National Council on Radiation Protection and Measurements in the United States, UNSCEAR [United National Scientific Committee on the Effects of Atomic Radiation], and so on, produce documents which vary in their capacity to speak to the average individual, that is, to the non-professional. They could undoubtedly do more than they do. UNSCEAR certainly addresses its findings to the United Nations Assembly, and they offer their summations as much for guidance of radiation protection in nations where they do not have the resources to have a radiological protection board of their own, so this means actually the bulk of the countries of the world.

There's only perhaps ten or so that have the wherewithal to actually make independent judgments -- Britain, France, Germany, Russia, the United States, Canada, Japan, presumably China, too. But the vast majority of places don't. I wouldn't know of a single country say in Central America that would have the wherewithal, and that probably includes Mexico. They don't have the number of authorities. They are dependent upon bodies like the United Nations Scientific Committee on the Effects of Atomic Radiation to find the guidance that they need, the recommendations that that body is making to the United Nations as to what should be done, what is the permissible level.

Even in the language that we use -- permissible level, permissible -- tolerable? I don't know. I'm not sure what you really say because in the outset you would hope that people were not exposed to radiation above and beyond that which is present in the earth's crust and about which we can't do anything, or that originates in space. Again, we're not going to be able to do anything about that.

There's so many things that we do and use that we need radiation for, everything from fractures in metal castings which are picked up by radiographers to -- you could make a very good case, I believe, that the current status of all of human biology would be light years behind what it is without radiation, because it was the use of radioactive traces, often artificial elements, in fact, that have made it possible to study biology with a precision that just didn't exist before. What is the most common way now of ablating the thyroid? Iodine-131. So we've learned so much from that and are still so dependent upon these materials that we can't go back to Adam and Eve's world. (chuckles)

AM: Well, your career has been an interesting period of time in which the United States government, at least through the arm of the AEC and the ABCC, allows geneticists to exercise some technology. I'm putting it very simplistically, but allowed them to do research regardless of what the public wanted. You said that maybe scientists dropped the ball a little bit there. But now we live in an age in which the government stands opposed to certain kinds of development of new technologies, and I'm thinking of stem cell research, in which then it seems the scientific community and the public community seem to stand more in line together against the government, so what's the responsibility then of the scientists to become activists? It's the government who seems to be promoting fears of what this kind of new technology can do, even though many scientists say we just don't even know enough to know what this technology is going to allow us to do. So it seems like the role of the government, the state here, is reversed in your two different points in your career. Although I know that you don't participate in stem cell research, but certainly, it's part of the human genome research.

WS: Yes. I think the dilemma that you're posing -- the problem is that these things often get resolved through some kind of a balance of tensions between those who advocate it candidly often for self interest against those who do not advocate it, in fact take the opposite point of view, and that's often for self interest, too, maybe religious, cultural, or whatever. It's unfortunate we don't have some mechanism by which we can extract individuals who have the intellectual backgrounds to understand the full dimensions of a case and aren't committed to one or the other, who would then be our Solomon who defines the course the course of action that we should take.

I often thought -- and this may be a misrepresentation -- but I've been intrigued by what was alleged to be the governance of [the Republic of] Ragusa, or Dubrovnik as we know it now, during the Middle Ages when the governor was literally isolated. He didn't have any interaction with the community in any way, shape, nor form. He was brought issues, and like the Delphic Oracle, he was to study these things divorced from the self interest that would otherwise be addressed to him. He presumably had the clear mind, unfettered by claims of rightness or wrongness, as the case may be, and would arrive at a decision, and that decision then became, in effect, the law of the city. I doubt that he was probably ever quite as isolated as the implications were, but nonetheless, you'd like to think that there would be people who could be cast in such a role.

Of course, one of the things was that he could not be reappointed. So what we'd have to start with Congress is you can't be reelected, because otherwise everything is governed by their necessity to get themselves reelected, or that seems to be the view that we have. I don't know how we do that, unless we put them all in a monastery someplace, (chuckles) or the equivalent of a monastery, and just feed them the scientific publications as they appear and not be fettered by all these other notions that obviously end up determining positions.

The debate that we see now is not -- it seems to often end up with who can muster the energy to sustain it the longest. You win by not the intellectual force of your argument so much as your capacity to just finally wear out your opponent. (chuckles) I think we've been awfully lucky that it hasn't been more detrimental to society than it presumably has been, if it's been detrimental at all, I don't know.

AM: Okay. We've got still a bit to go, and I guess as a way to move us on, we've really talked a lot about your scientific research that was based on your work in Japan, but that isn't the only work that you've done. You've worked in other populations and in other areas of genetics, so maybe to help us move forward a bit, how do you view the preponderance of attention that's been paid to your work with the Japanese population, and is this your Magnum Opus? You've written several monographs. Is this how you define yourself? Or where does your other research work fit into your identity?

WS: I suppose certainly if you look at the totality of what I've written, I've written, I suppose, more which deals with ionizing radiation, or with studies which were prompted as a consequence of that involvement, like the inbreeding work. But I actually primarily see myself, I guess, as a population geneticist who is interested in the maintenance of genetic diversity, and particularly interested in those features of the environment which may figure prominently into that maintenance, whatever that process is. Radiation in the case of Japan. The work in South America had to do with hypoxia and to what extent did that determine what we were seeing, the way people were reacting to hypoxia? How much of it was actually genetic in control?

I think, unfortunately, looking back at those studies, the design was right, I think the notions were sound, but I think we were twenty years ahead of our time, that with the technology that exists today, if we could have applied that then, we would have been much further ahead. The means didn't exist in the early seventies when we were working to actually characterize biochemically individual genes. We were looking at gene products, and that isn't nearly as good as the other mechanisms. I don't know that the results necessarily would have been interpreted in any different way than they were, but I think we might have had more confidence in our interpretation had we had that kind of --

As so often happens, for example -- certainly this has been true in the inbreeding situation, I think it's true to now to what we were doing in the mountains in northern Chile. Time has moved on to a point in which, in Japan the frequency of consanguineous marriage today is no higher than it is in the United States. You can no longer do the kind of study we did. The opportunities are gone. I think you could build a real case that that would be true in the area of northern Chile, too, that the populations has become much more mobile. Urbanization is occurring in every country, so the people who previously lived their lifetimes at these altitudes now move into the cities and may visit back and forth to their elders, but it's not the same.

And expectations have changed, so it's just unfortunate that the appropriate technology doesn't always exist at the time when the maximum opportunity for resolving a major problem also exists. I think it changes. I don't mean to be pessimistic and say that you'll never get the answer. I don't think that's true. I just think that you have to change what you do and how you do it to accord with the fact that the world is not static.

Looking back on some of the personal problems that we had at altitude, in terms of the fact that here we were coming from -- Houston's about thirty feet above sea level, and I ended up working in some of those villages that were fifteen, sixteen thousand feet. I'll tell you, your heart races, you have trouble sleeping. There are real physical and physiological problems that arise. I think the thing that most of us didn't appreciate the first year we were there is that it can make people who are normally surprisingly even-tempered irascible as all get-out. It's not commonly defined as one of the effects of hypoxia, where our experience was that, yeah, until they adapted, which was usually a couple of weeks problem, people could be irascible as all get-out. They'd go into a tizzy at the drop of a hat. I presume that was partly because your brain was being undernourished. (laughs)

AM: It sounds like there might be another monograph in there on those studies.

VIII. Peer Review; Developing Genetics at the University of Michigan and the University of Texas

WS: Well, actually, I started, and I don't know whether I'll ever finish or not, to write kind of an autobiography. Maybe if Jim hadn't written his, I wouldn't have been tempted, or maybe the reason I've been so slow is because he wrote his. There would necessarily be some overlap between the two of us, but our careers were enough different that I think my perspective would be different than Jim's.

The thing that prompts me, in part, is I think I've been privileged to watch -- as I believe I mentioned earlier -- the development of two of the most vital sciences of our time, namely genetics and computing. And I've participated in them almost from year one. Not that I knew Mendel, but the explosion, at any rate, which has occurred both in genetics and computing has occurred in my lifetime. I was privileged to be part of that, and I was sometimes involved in the administrative aspects that either furthered -- well, I hope furthered, but might have been less a furtherance than I would like to think.

The ways in which the scientific community has evolved peer review, for example, and what peer review meant in the 1950s, 1960s, and 1970s as opposed to what peer review means today, which many would say is no longer peer review, that times have changed attitudes as to what sorts of representation should be present. I think oftentimes they don't really understand -- many people, at any rate, do not in part and parcel of those processes understand how the process occurred, that as it was originally construed and constructed in many instances, I think it would be hard to imagine a fairer way of deciding what notions are worth supporting, what level of support do they merit, and whatnot. Because you were, in effect, distending, albeit it in writing -- you were arguing the case in writing, and you had your advocates and you had the detractors. They sort of simultaneously debated the merits of this particular proposal and then came to a decision, the decision being not that this one does get supported and that one doesn't.

What you ended up with was a committee's evaluation in terms of a numeric value, and then whether it was supported or not depended upon the amount of money that the individual institute had. I think that was good in the sense that we couldn't actually say, well, we know this is going to be supported, in part because it -- I think it kept the process more impartial. Everyone had to advocate certain lines of research, intentionally or unintentionally. This way, if you were simply judging them -- you were ranking this one relative to the others that you were seeing before you at that particular meeting, or had seen at previous meetings. How does this one stack up against others? Does it warrant a hundred? Does it warrant a hundred and fifty? Two hundred? Whatever the case might be. You weren't supposed to trouble yourself with the idea of will there be money to support that. Those were decisions that it's about the amounts of money which presumably went to the bureaucrats and the administrators at the national institute itself.

I think that was as it should be. That is, even though they may be scientists, too, and they have to respond to a different kind of constituency, our job as say a member of the study section was to really evaluate as impartially and as reliably as we could the quality of the science that was being _____. And the reality of getting an answer. Is this a good question? That's item number one. Is what is being proposed likely to provide an answer, or even a partial answer to the question that it addressed?

These are committees generally of twelve or fifteen people representing all of genetics, all levels of genetics. And it was interesting to see the dynamics that would go on. I found one of the most fascinating things about serving on those committees, indeed, serving on most committees, is the dynamics that the committee develops. Not that people set about to develop a particular image for their committee, but you see how it can reflect to better or to worse the personalities of the individuals who comprise that committee at a particular point in time, and how a knowledgeable chairman can manipulate that.

I'm both for good or for bad. When I was chairman of the genetics studies section, for example, I always felt that one of the reviewers who was charged with specifically reviewing these proposals and who then presented to the study section in total their reaction, that it was most favorable to the investigator if the most positive response to his proposal was given to the committee first. I felt confident that if the other reviewers or members of the committee who specifically weren't so-called primary reviewers saw shortcomings that had not been seen by the person who evaluated it very highly, that would come out. But it was much harder to take a negative and turn it into a positive than it was to downgrade a positive.

So I just felt it was better to put the best shoe forward first and then let the chips fall where they may, because people could differ -- some of them were very good advocates for what they were being proposed. Other people were no less bright or gifted or anything like that, but they just aren't very vocal advocates. That was manipulation I saw that was good.

Now, you could also -- if I had a particular point of view in mind, I could have started the other way and take the most pessimistic one, say let them -- because the committee can't help but be influenced, because you know your fellow committee members, and you have, from previous meetings, formed a notion about the person. Are they really fair? Do you respect them for your knowledge? Are they _____ than average? All these little imponderables, subjective elements, come into your impressions, and those are colored ultimately to the vote that you assign to that proposal.

The issue of the dynamic was really great. I think I mentioned it in a somewhat different way with respect to the Macy Foundation and Frank Fremont Smith. He looked at communication more as a method of communicating, but I think what I've been speaking to is part and parcel of that same thing, that there is a dynamic that develops among individuals who, in a decision-making context, that -- let's study it in and of its own self. I don't know whether you can arrive at any generalities or not because it's going to depend so very much upon the persons. It is idiosyncratic in that respect. I always found that fascinating.

AM: I've had the experience of interviewing a lot of young biomedical researchers, so they were coming up for their first renewals of their RO1s [Research Project Grant]. Some were indifferent to the peer review process, and others just thought that this had become too unfair, too biased toward some labs, and it seems like you're saying that there are these kind of institutional biases that arise just because of when you get a group of people together, biases are going to emerge. So how do you select for fairness in an NIH review committee? Maybe it's more, how do you select a good chair?

WS: I think that's a very critical thing, and Katherine [S.] Wilson was adept at that, not just because I was the chair once, but she had people like Ed [Edward L.] Tatum, Ray [D.] Owen, I think Jim Crow. If you think of the persons that she selected for chairpersons, they were all both scientifically above Cavill, but they were also very fair intuitively.

I think one of the problems that has happened is the whole process of peer review, the efforts to democratize it have been misled or have diminished peer review. It seems to me to be, inherently, the whole process is elitist, and you have to accept that, it seems to me. The moment that study sections had to have gender, color, age, geography determined who was to sit on that committee, you lost your peers. And this was long before I -- I don't do that much research anymore, hardly any, in fact. Some would say none. But let's say even in the seventies, in the years after I had served, I'd look at some of these study sections and I didn't know a single person on them. And these were supposed to be my peers. That didn't breed, obviously, trust in the decision-making of the committee, and that, I think, hurt peer review.

I can understand the motivation to do this, but it was the failure to recognize that this is not a democratic process to begin with. (chuckles) To try to make it -- even the Greeks got wise to the fact that you can't run a peer democracy, that it isn't the way it goes. I think you have to find people who have the knowledge, but the open-mindedness, the fairness, those are really the qualities.

I think the difficulty that you had when you started democratizing this was everybody always sort of felt obligated to demonstrate the rightness of their appointment to the committee. Those who weren't really very deep would latch on to a lot of trivia and parade it as if it were the be-all to end-all about the review of this process. That was their way of showing how carefully they had read it, or something. But in actually looking at the idea in its larger context, they weren't skillful at that. I think that really hurt the process in the long run. I understand human motivation. I don't endorse it, but I understand it. Certainly, it was not like -- if you look at that composition there, my God, every one of those people have been members of the academy. Those were your peers, no question about it. You weren't disturbed by your score, of feeling that you hadn't really been judged by persons who knew what it was that you were attempting to do. There's too much of that now.

I also agree with them that there is a feeling that too much goes to a small number of institutions, and that probably is true, too, and most likely comes out of the changes that have been made. One of the things that I think is really unfortunate, so much of grant evaluation today lies outside the traditional mold of standing study sections. They're always ad hoc, study sections, and they can be stacked, there's no question, and they often are.

Too much of the money that goes into science -- we hear that the money increases every year, the number of RO1s is increasing, and whatnot. But neither one of those reflects the fact that there's a very large chunk of money which is earmarked. Alzheimer's disease or HIV, or whatever the case may be. Yes, more money is going into science, because they get counted there. They are responding to RO1s. Yes, they get counted there. But it isn't telling you that you've structured your money in such a way that large areas of the basic sciences have actually been suffering and suffering badly because they're not fashionable or they don't have eloquent enough advocates or _____.

At any rate, that part was fun. You never like to see a grant get turned down where you thought it really had merit. But at any rate, I enjoyed the -- and it was a learning exercise. My God, I was standing there and people were talking about these proposals that had forgotten more than I knew about the whole area. I wasn't there for that purpose, that particular area. Some of them just instinctively gave a lecture. That was Charlie Thomas. (chuckles) He gave this whole lecture on biochemistry. But it was fascinating. It was like you're getting not just your peer but someone who is a major worker in the area. That made it entertaining to us, I think, those of us who participated, because -- certainly, you probably lost money in the whole enterprise. Theoretically, you received a small stipend, your expenses were paid, but you incurred expenses that you wouldn't have incurred otherwise if you hadn't gone to that meeting. You eat a more elaborate meal, and you can only deduct so much for food costs for the day.

It wasn't the dollars or cents that kept you in. It was, first of all, I think, a feeling that if you were a member of the scientific community and this was a need of the community, you should serve, if given the opportunity to serve. Then secondly, when you did and you were with a good committee, it was a phenomenal learning experience.

AM: I do want to get to your experiences at Michigan. Let me just pause the tape a second.

[pause]

AM: Okay. So when you came back from Japan, you knew you were going to go to Michigan. You weren't going to return to Ohio State. What did you expect to be doing with your work? Basically, what was your work going to encompass, and what were going to be your responsibilities? And was your appointment in the Department of Human Genetics?

WS: At that time, there was no Department of Human Genetics. The origin of that department is kind of complicated. Interest in human genetics had begun largely through the stimulation of Dr. Lee [R.] Dice in the 1930s, late thirties. He had gotten funds to support efforts to sort of further the development of human genetics. He had gone to the medical school to encourage them to be the force behind this, and at that point in time they simply weren't interested, like most other medical schools.

The net result was, Dr. Dice, who was -- his own position at the time was Director of the Laboratory of Vertebrate Biology. That laboratory had an interesting history. It had come into existence -- initially, it was called the President's Laboratory, because in the 1920s Michigan had recruited C.C. [Clarence C.] Little to be president of the university. He was, even then, an active cancer researcher using the mouse as a paradigm, and one of his conditions for accepting the position was that they would establish a laboratory for the president to maintain his involvement, and that was the President's Laboratory. I don't know the full details. Little did not spend very many years at the University of Michigan, one or two is about all, I think, and then chose to go elsewhere. It may have been volitional or it may have been involitional, I don't really know. At any rate, the President's Laboratory was there.

Little's successor was Alexander [G.] Ruthven. Ruthven was a vertebrate biologist, and Lee Dice was a close friend of his. They were both in the vertebrate museum. I guess it's called the Museum of Vertebrate Biology, or something like that. At any rate, Dice was put in charge of this laboratory that had been Little's laboratory. It was a two-story brick structure, fairly solid. Little, of course, had worked with the domesticated mouse, *Mus musculus*.

Dr. Dice was interested in problems with speciation, so he continued to work with rodents, but he decided that it wasn't the house mouse. He was working with the genus *Peromyscus*. So the whole laboratory shifted from the house mouse to these wild mice, *Peromyscus*, which is found everywhere, particularly in _____ north. So with this interest in human genetics, when the medical school did not see fit to foster his notions about research in human genetics, Dr. Dice, with his close connections with the president, got a branch established to the Laboratory of Vertebrate Biology, which was called the Heredity Clinic. So the first sort of formal structure at Michigan was part of a mouse laboratory. (chuckles)

AM: That's the origin of the Heredity Clinic.

WS: Right.

AM: So Neel didn't start that, it was already there.

WS: It was started, and among the first people Dr. Dice recruited was Harold [F.] Falls and Charles [W.] Cotterman. Then, of course, the war came along. Not much had been done before the war started. After the war, then Dr. Dice recruited Jim [James Neel] to come. The Heredity Clinic, when I first went there, was still part of the Laboratory of Vertebrate Biology.

AM: And how is that connection made? What was the reason to create the Heredity Clinic and its relationship to vertebrate biology, since I'm assuming it had a clinical --

WS: Yeah, it did. I think part of it was just to emphasize the human aspect of the thing, rather than just the vertebrate aspect, because certainly it qualified. I never really spoke to Dr. Dice about that, but at any rate, that was the status when I first went there in '49 to be interviewed by Jim. By the time I got to Michigan in 1951, in that interim there had been something established known as the Institute of Human Biology, of which Dr. Dice was the director. The Institute of Human Biology had, as I recollect, five or six components. The Laboratory of Vertebrate Biology was one of those components. The Heredity Clinic was another one, an independent component within the institute. Then there was a section -- I forgot exactly what it was called -- Community Dynamics, or something like that. It was really a group of ecologists who were studying wild communities, or native communities, international communities, whatever is the appropriate word. Then there was a group in herpetology. Dr. Dice had kind of an affinity towards orphans.

Then, while the institute was still in its early and more or less formative period, there were two large human studies that got underway. One was called the Hereditary Abilities study. That involved [H.] Eldon Sutton and Steven [G.] Vandenberg. Then there was a study called the Assortative Mating study, which was guided by Jim [James N.] Spuhler. Each of these was addressing specific issues about which we knew relatively little information. Virtually then, and even now, most population genetics begins with the assumption of random mating. Well, in the human, you knew that wasn't true, but you didn't know how non-random it was, so the Assortative Mating study was an effort to look quantitatively at how spouse selection is correlated with stature, with actual age, too, with economic standing, all of the factors which influence mate selection, to do this in kind of a quantitative and as objective a way as possible. The study source was Ann Arbor. That was a typical community in some respects, but if you took the university part out, it was certainly typical.

The Hereditary Abilities was focused on twins and was looking at, simultaneously, biochemical and psychosocial similarities between twins. Each of these were, in a sense, more or less co-equal units, although these two special studies, obviously, deferred to Jim as head of the Heredity Clinic.

This situation continued until 1956 when Dr. Dice retired. It was customary at that time at Michigan, as well as at a lot of other universities, that the president was to appoint a committee. The purpose of that committee was to evaluate the achievements of the institute, to determine whether or not it should continue as a free-standing institution, or whether it had served its functions and now needed to be -- its personnel needed to be assigned to more traditional units within the university. Or to graduate into the level of a department.

Well, when you saw who the composition of the committee was, you knew what the outcome was going to be because one of the committee members was Dean [Albert C.] Furstenberg, who was dean of the medical school at the time. By that time, of course, Jim had very solid ties into the medical school, and human genetics was -- even Furstenberg saw that this was a coming area and, therefore, was more attractive to the medical school. Also, the chairman of the Department of Zoology was on the committee, so these ecologists and herpetologists all seemed logically in the Department of Zoology. The net result was that all of the units that actually were working with human beings formed the basis for the Department of Human Genetics, which came into formal existence on July one, which is the beginning of the academic year at Michigan, 1956. Jim was the founding chairman and was the chairman up until his retirement in -- gosh, I forgot what year it was. Probably about -- must have been the 1980s.

The others all went into the Department of Zoology, as did the laboratory itself. It ceased to have -- it was still maintained as a structure. The head of that was a Morris Foster, who was a mouse geneticist. Although he continued to do work with *Peromyscus*, he was switching back to the house mouse too, so it kind of was going back to what it had been under Little. So that was the situation prevailing in Michigan.

AM: So you arrived at Michigan in '51?

WS: It was the Institute of Human Biology. I was with the Heredity Clinic, which was a unit within the Institute of Human Biology.

AM: And you didn't have any other department affiliation?

WS: I had a non-paying appointment as -- see, when I first went there, the titles in the institute were things like junior geneticist, associate geneticist, and so on. Well, I started as a junior geneticist, which was essentially like an instructor. During the first year, that was my sole appointment, if I'm remembering correctly, but the next year I was made the equivalent of assistant professor. This would have been in '53. At the same time, I was appointed assistant professor, non-paid, in the Department of Zoology, which -- because the courses at that time, the institute itself couldn't offer courses independent of one of the colleges, so human genetics was actually listed as a course in the Department of Zoology. That's where I taught, and therefore I had to have an appointment in that, so I had a non-paid appointment as an assistant professor of genetics.

AM: And what was the relationship at that point to the medical school and the training of --

WS: At that point, we did a lot of work with the medical school. The Heredity Clinic was functioning in a complicated capacity for genetic counseling through the Department of Pediatrics. Of course, Harold Falls was in the Department of Ophthalmology. Jim had a non-paid appointment in the Department of Internal Medicine, and he routinely took a ward every year for a month when he was the -- whatever you call the senior position on the ward. So there were already strong ties beginning as soon as Jim was there.

Of course, as soon as Jim arrived, not only did the study of spontaneous mutations begin, but this was when Jim moved from thalassemia, which he did *some* while he was still at Michigan, but more to the sickle cell phenomenon. We had work going on in both the distribution of the sickling phenomenon in the state of Michigan, but some of the new hemoglobins that were discovered later on. So that was there. Obviously, the guidance for that came through the medical school. Our studies with spontaneous mutations invariably began with groups of patients selected from the medical school, from actually the university hospital.

So there were strong ties. Although we were physically there, we were not part of the medical school budget or anything like that. Except in Jim's case, we had sort of cordial relationships but no structured ones that would show up in a catalog until 1956. Then, as soon as that happened -- well, there were a number of things that were going on simultaneously. More funding was becoming available because of Jim's early work, supported through the Atomic Energy Commission, the work in Japan, and that continually also under underwrote the contract that was doing the spontaneous mutation rates. That was supported by the AEC.

Then by '56, more funds were becoming available. Jim's interest in the hemoglobin had prompted more money. There were other things that -- biochemistry was coming into play. It was 1955 that Oliver Smithies came along with his haptoglobin work. So there were chances to do things that didn't exist before and sources of money to support that.

Jim's view of the department was one which would, in effect, take us from the biology of the gene to the biology of populations. He felt that the Department of Human Genetics needed all of those, so we had people who worked with DNA -- Charlie Ratting, for example. Mike [Myron] Levine was doing work with viruses. Every one of these phases was there, and we already had by that time a laboratory that was interested in serology. Jim's feeling was that one of these groups reinforced every other, and, in a sense, fed off problems one to another. So it was -- not that this is a good analogy -- from womb to tomb, as it were. This is in the biology of the gene and its dissemination of populations.

In 1956, when the department came into being, the department essentially consisted of Eldon Sutton, Jim Neel, and myself. Ed [T. Edward] Reed. We couldn't amongst us cover all the gamut that was Jim's vision of where the department should be. Recruitment started, and this led to the addition over the next half dozen years or so of Charlie Ratting and Arthur [D.] Bloom. Well, Margery [W.] Shaw first in chromosomes. Arthur Bloom came along, and Mike Levine, Dick [Richard E.] Tashian, pretty much the department as you would have seen it up until probably 1970, certainly. Because up until that point in time, we hadn't lost anyone. Well, that's not quite true. Eldon Sutton had left around 1960, I guess, give or take.

Then there weren't any losses until Bob [Robert S.] Krooth went, and so on. This was later when the members of the department had earned levels of notoriety that they were being recruited elsewhere. Jim always made every effort to hold it together as he could. It often comes to a point in which it isn't just money or space. There are other more intangible aspects that lead to people leaving. So the department had a surprising degree of stability for a fairly long period of time, and it functioned quite well.

We were fortunate, too, that the Buhl family provided money to build a laboratory, not a huge one but it helped substantially in providing laboratory space. Originally, the Heredity Clinic was in a two and a half story old clapboard house, and there were virtually no laboratory facilities in it at all. All of the hemoglobin work was done on a Klett [-Summerson colorimeter] that was over in the Department of Physics. It was bare bones. Had some microscopes and a few things like that, because Jim could look at the slides, and whatnot. I know my office had a piano in it, and I never quite knew why there was a piano there until someone told me that when Charlie Cotterman had been at Michigan and was in the office that I had, he was interested, along with Dr. Dice, in absolute pitch, so Charlie needed a piano. (laughs)

The thing grew substantially. If you were to look at pictures taken annually of the staff, it started out with five or six of us to where we were down to three rows about the time that I left. This would include postdocs, too, because in the very early years we didn't have any postdocs. Then things began to develop, and to support part of those activities, through the National Institute of -- well, when the National Institute of General Medical Sciences, NIGMS, was created, then they became interested in training grants in a variety of areas, of which genetics was one. Jim was the initial chairman of the Training Grant Committee, and we were also one of the institutions with one of the first training grants. I don't remember how many students this allowed, but it eventually got up to the point where we were supporting probably about twenty-five students, either at the postdoctoral or the predoctoral level in their training in genetics. If they were predocs, it was full support; if it was a postdoc, that usually was coupled with something else because there wasn't that much money. All of this contributed, obviously, to a growth in number and space and whatnot.

It was in the middle sixties, '64-'65, when Jim began to get interested in primitive populations. Part of that interest, I think, stemmed from opportunities that he saw, but also from the fact that we had had -- Francisco [M.] Salzano was one of our postdocs, so there were ties into Brazil. Another one of our early postdocs was from Venezuela. So we were having South Americans who had the ties, and Jim saw that as an opportunity to do some of the things that he was interested in, which were, in a sense, a continuation of the whole idea of what maintains genetic variability. To do that, one would like to know how genetic variability was seen not only in current populations but in those which were as presumably representative of the earlier stages of man's communal evolution. So the populations in the interior of Brazil and Venezuela seemed to offer such opportunities. They hadn't had more than a very light brush with civilization, if that's the appropriate expression. So this presumably would give some insight into how these communities functioned and sustained themselves, and the implications of that for genetics.

Jim had gotten started in that. He offered me the opportunity to be involved, but I'd had enough time in jungles in the war, and I didn't see all that much interest in going back into a jungle. Moreover, there were other things that were attracting me at the time, so I was supportive, but I didn't actually ever get directly involved in the studies.

When I left Michigan, the factors that led to my decision to do so were complicated. I didn't leave out of a sense of dissatisfaction, and I certainly didn't leave out of a sense of being unfairly treated. The others will tell you I always had privileged positions in the department because Jim and I had been -- we *were* the department at one time, so it went back a lot longer. And it didn't rest solely on the fact that I wasn't particularly interested in the kind of research that Jim was doing at the time because there were other things that I was doing that were equally attractive to me.

Probably one of the things that loomed -- two things loomed large in my mind. Like everybody, there comes a point in time in which you would like to know, well, can I actually build a department from scratch. And if so, how would I structure it? That's one thing. It comes from administrative endings get twitched, I guess. The other thing was, I was growing increasingly concerned because it was clear -- see, Jim was seven years older than I am, and when I left Michigan I was fifty, so Jim was fifty-seven. At that time, Michigan's retirement age was sixty-five for chairmen of departments. You retained your professorship till seventy, but you had to step down -- now, there was some waffling in the sense that as a founding professor, you weren't held to that guideline quite as strictly. So Jim could possibly have stayed on.

It was perfectly clear to me -- not that I was encouraging it -- but that Jim wasn't going to step down until he had to. He hadn't as yet, I think, achieved the vision that he had. He was very effective at what he did. There was no reason _____ that _____ to encourage him to step down. But I got to thinking, gee whiz, when Jim steps down at -- if he goes to sixty-five, I'm fifty-eight. The odds are at that point in time the powers that be are going to deem me probably too old to be the chairman, and I could easily see myself facing a future in which I was, in effect, in the department at the sufferance of someone who hadn't been present in the building and had gone through the periods of feast *and* famine.

So the opportunity to start something of my own loomed progressively larger. I came here initially, actually, at the urging of Reuel [A.] Stallones and Al [Alfred G.] Knudsen [Jr.] because they were planning some activities and ostensibly I would share as a consultant. Well, at the time I left, I had two job offers, in the School of Public Health or in the Graduate School _____. They were very patient in letting me mull, because it was hard as hell to talk to Jim about leaving, for one thing. As I said, I wasn't at all unhappy, and I didn't have a sense of dissatisfaction. The only thing was this nibbling business of maybe looking inaccurately down the road. Perhaps I should have.

At any rate, eventually I decided that I'd come here. I was given what was to be a center, because the graduate school didn't have departments as such. They were in -- the

School of Public Health was in the medical school. And that center would have five academic positions, state funded, and adequate space, and so on. So the financial situation was good, the opportunity to develop what I wanted was very attractive, and at that time here, the Health Science Center as such didn't exist. It was a series of independent units that governed themselves kind of as a collective, with [R.] Lee Clark, who was head of _____, the chairman of this group of _____. That group consisted of the deans of the school. So there was Al Knudsen as dean of the graduate school, Stoney as dean of the School of Public Health, Cheves [McC.] Smythe as dean of the medical school, and [John] Victor Olson as dean of the dental school. I don't know whether nursing was represented or not, but anyway, this was the group that met weekly and made decisions.

Each dean reported directly to the state legislature, or to the Bureau of the Budget, or whatever they call it, in working out their budget. They obviously needed some kind of a mechanism for interacting, but the schools were all independent, self-contained units.

Both Stoney and Al -- Al in particular, I think -- felt that the graduate school could be a link to the other schools and that if that link was to be established, the logical discipline to do it was genetics, that a medical genetics center, which was what Marge [Margery Shaw] had headed, would be sort of the bridge between the graduate school and the medical school, and the center that I was establishing in population genetics would be the bridge between the graduate school and the School of Public Health, whose focus is also on population. So they saw genetics as being this bridging discipline. I found that a very attractive idea.

Another thing which was obviously instrumental in my thinking was I had enormous respect for Al Knudsen and Reuel Stallones and Cheves Smythe. They're all very able people. They also thought outside the box. The view that Stallones, Knudsen, and Smythe had of human biology -- I like to use that expression rather than just medicine because they thought beyond just sickness and health -- was one that I really found very attractive. Of course, the experiment that Stoney was trying here with the matrix approach to education in public health was also a novel notion. Cheves I hadn't known before I came here, but I had known Al from the time he was at Duarte [,California] at the City of Hope, and I knew Stoney when he was still at the School of Public Health at [University of] California [, Berkeley]. So all of that was attractive.

After considerable cogitation, consultation, I talked to Jim. He immediately made the offer of financial equivalents and all the rest of that. But it wasn't those things that were really determining it, so I came here in May of '52.

Interestingly enough, by the time I got here, I didn't realize that the Board of Regents had decided that _____ structure in Houston could no longer be tolerated, so in September of 1972 there came into existence the [University of Texas] Health Science Center [at Houston], which made all of the schools a part of a unit that was actually here and separated our interaction from the [The University of Texas M.D.] Anderson [Cancer Center]. The Anderson was another unit that became the University of Texas Cancer Center, and everything else -- the School of Public Health, the graduate school, and so on, were part of the University of Texas Health Science Center in Houston. So some of the freedom that I had really enjoyed, or was anticipating enjoying, was already gone by the time I got here.

The one thing I will say, at that point in time, Smythe, Stoney, Al, they were not only thinking outside the box, this school was new enough and the horizons looked broad enough that there was no territoriality. That's really hard to say, because if you've been at any university for any length of time, you know everybody has already identified their trees by the usual scenting technique. (chuckles) That hadn't occurred here. Everyone felt that, my God, the horizons are so big we don't have to stake out things which we want for the future but can't start up now, just because we don't want somebody else starting them. There wasn't any of that sense, and that openness, and so on, was really quite attractive.

Then, of course, one of the decisions to come -- by that time, I'd thought out what it was I really wanted to do, but I knew that if I didn't get a good theoretical population geneticist, I wasn't going to move. The person I had in mind was Masatoshi [Nei]. I had known Masatoshi in Japan, and I knew via the grapevine that he was dissatisfied with the amount of teaching that he had to do at Brown [University] and that he was probably movable. So I offered Masatoshi a position as full professor. He was then associate professor, I think, at Brown. He checked out the opportunities by talking to Jim Crow too, so Jim Crow was also instrumental in getting Masatoshi here.

That was just a great selection. Masatoshi, in sort of a small group, in guiding graduate students, is phenomenal. I mean, he is demanding as all hell, but he's very fair. The students -- well, you can tell by how well they have done that he is a very effective person. He doesn't like formal lecturing, really, and I don't think, as a consequence, he's as good at that as he is at sitting down with a problem and a student and work this thing through. As I say, I don't know anyone who is better than he is in that kind of a context. Well, that was great for what I envisaged, too.

Then the one other sort of thing that was kind of compromising in a way, I had been under the supposition -- well, I'd certainly seen that we were going to need a laboratory to support our activities, not that I was going to do laboratory work or necessarily that our people were, but there had to be a laboratory that could do the markers that were going to be needed for population surveys of various kinds.

That was to be satisfied by the laboratories that already existed in the Department of Biology at the Anderson. Well, by the time I got here, that came unhinged, too. Eventually, I found myself obliged to establish a laboratory, not that I did anything more than the paperwork that went with it. The first occupant of that position was Bob [Robert E.] Ferrell. It was probably about 1974 when Bob came. It was a year or two before -- first of all, I'd already committed the five positions, so I had to wait till a position opened that I could recruit somebody, to identify space in setting up the laboratory.

Fortunately, Bob [Robert L.] Kirk, who was professor of human genetics at The John Curtin School of Medical Research in Canberra [The Australian National University], had a sabbatical coming up. I knew Bob both from his time at WHO [World Health Organization], and then I had a six month sabbatical with him in Canberra. He was willing to come, and he really got the laboratory underway, got our technicians trained, and whatnot. Then it kind of ran on a modest basis of its own without an appointed head until we could get Bob here when the position became available.

So it worked out through hook or crook in ways that I had not anticipated. I again was exceptionally fortunate with the people I had. I've lost every one of them, but I'm pleased to say that Masatoshi went on to an institute that was formed for him. Wen-

Hsiung Li went to the University of Chicago. Bob Ferrell was chairman of the Department of Genetics at the Graduate School of Public Health at [University of] Pittsburgh. Ken [Kenneth M.] Weiss left to become head of Anthropology at PSU [The Pennsylvania State University]. So they've all done very well. Everyone left with unhappiness on my part, but with best wishes, because they were really good. It was fun to have that kind of a group together. The intellectual stimulation was fantastic.

[end session]

IX. Commentary on Peer Review and Funding; Comparing American and Japanese Science and Education; Effects of Radiation Research

Session IV

June 30, 2005

AM: It is June 30th, 2005, and I'm Andrea Maestrejuan with Professor William Schull at his office at the University of Texas School of Public Health here in Houston, which will be this time the actual last session of his interview for the UCLA Human Genetics Oral History Project. I wanted to start off with something we were talking about off tape at the beginning today, and that was you had some more comments on the peer review system, some remarks to follow up on what you had said yesterday about the fairness and levels of funding.

WS: One of the potential charges against peer reviewing has always been the fact that those persons on the committee at any given time most qualified to review a specific grant application, qualified in the sense that they were most knowledgeable in that area, were apt to be a competitor of the individual who actually submitted the grant. There have been allegations over time that people deliberately voted low to slow the pace of a competitor and that even notions had been borrowed, let us say, from a grant application that a particular individual might have seen.

Personally, I know of no instances of such allegations that may have been made, but I don't know anything to support it. In fact, I would say that in the years in which I was more heavily involved in the activities of the genetics study section, I don't think it actually occurred, and I think at least the one reason was that grant applications, the minimum score that could be funded then was somewhere in the neighborhood of two hundred and fifty, so that any application that received a score from a hundred to two hundred and fifty was almost certain to be supported. Therefore, it would have been hard for somebody to consciously try to manipulate the scoring, the total score assigned to a grant application, simply because they saw this grant as potentially competitive. It may be, in retrospect, that this is not testimony to the fairness of individuals, naturally, but the system had enough money that it could tolerate a certain measure of slippage, if that's a fair description.

I was impressed then, and I've generally been impressed in terms of the activities of the study sections, that most members of the committee have seen their involvement as, both on the one hand an important thing in their own careers, but in addition, they took the charge that was laid before them quite seriously, and they did the best that they possibly could to be objective in the evaluation of any particular proposal that they were assigned as primary reviewers. So I was overall very impressed by the general quality of the reviews, let's say that were obtained in the years when I was most active, which was the sixties and early seventies.

I think, subsequently, both the change in the nature of federal funding, not only the total amount but the increased amount of the total funds appropriated to NIH that go for earmarked activities, have introduced another conflicting element into the peer review that I really don't feel personally qualified to evaluate because I don't think I've had enough experience under those circumstances. At its best, I find it extremely difficult to identify a method of evaluation that was fairer than the one that the National Institutes of Health, through their study sections, had selected.

There were at least two other competing sorts of systems. The National Science Foundation didn't use peer review in the same sense that I'm describing it for the Division of Research Grants, or what was then called the Division of Research Grants. They didn't have assignment of grants to specific institutions and, therefore, funding being dependent upon the budget of that institution. The Atomic Energy Commission, which at that same time was a big funder of many grants, primarily used in-house evaluations, at least for people who were employees of the Atomic Energy Commission who made decisions about which grants were to be supported and not supported. So you had really basically these three competing different attitudes towards how you should support science.

My personal opinion -- I was involved in all three of them at one time or another -- was that the fairest one was the one that NIH chose. I just wanted to say that, at its best, I find it very difficult to think of a way that can be more objective, can be fairer, and can achieve both the ends of the institutes with their missions, as well as provide the individual investigator who submits what is now called an RO1 application to a study section.

AM: Well, the funding of science has certainly changed in the last half century, or fifty, sixty years, so although there's a lot more funding available, whether it's government-sponsored or private money also has become very important, the total number of labs competing for these, is there some way to structure it, for instance, rather than focus on the RO1s and making that the subject of so many other things in a young scientist's career. Like tenure and everything gets attached to the ability for independent grants. And I'm just throwing this out as a way of thinking about alternatives and focusing more on program-project grants.

WS: In my mind, one of the big aspects of this shift that I really can't put into proper perspective because it involves knowledge of economic affairs which are beyond my _____. It's the sort of thing -- for example, at a time when I chaired the genetics study section, we had grants that ranged from ten thousand dollars to probably the maximum amount that was asked -- I'm talking about the yearly request -- might have been two hundred thousand. Today, I doubt that any study section gets a ten thousand dollar grant application, and what was then a two hundred dollar application is at least a million now. The thing that I don't know is the inflationary element that's involved in this. Was a two hundred thousand dollar grant in 1970 the equivalent of a two million dollar grant now, per annum? I don't really know what the multiplying factor should be.

I think there's no question but what grant applications forty years ago, or even thirty-five years ago, involved more persons in the sense that a great deal of the work was done by laboratory technicians and the actual expenditures on equipment was relatively small, as opposed to situations now where someone starting up a new laboratory in molecular biology is confronting probably a half million to a million dollars worth of equipment expenditures.

I can't put all of that into perspective, partially because I'm not a bench scientist, and then secondly, I don't know how to factor in the inflationary element that has occurred over that period of time. To just use the common metric of three percent per year would not be fair because different kinds of expenditures have inflated at totally different rates. Three percent may be a good annual rate viewed across all levels of expenditure, but we know full well that medical costs have not been inflating at three percent per year. It's been a lot higher than that. Where, for a very long period of time, until the recent crunches, gosh, gasoline was going up at quite a modest amount. So I can't put those things -- I don't have actually the data at my disposal to be able to, in my own mind, factor in these elements with the weight appropriate to what they actually represent in terms of the support of investigative science.

AM: Okay. Well, then, to just cover one area that I want to go back to that we didn't talk about in your experience in Japan, and I just wanted to throw this out. In the histories and reminisces and autobiographies about the ABCC in Japan in the late forties and fifties, there was a lot mentioned about kind of the state of Japanese science, both basic science and clinical science, when Americans arrived. I wanted to ask, because they certainly today are seen as quite scientifically advanced, even if it is more -- they're limited in terms of their model organisms. They're better at Drosophila than other -- what was your impression about the state of basic scientific research with the Japanese scientists that you worked with, as well as the clinicians?

WS: I think there were areas of science in Japan circa 1950 that were exceptionally good. Then there were other areas of science and/or medicine that were far poorer. Genetics was the fortunate one. Japanese genetics has been a very strong science for a long period of time. While they were not particularly strong in human genetics, in cytogenetics they had done a lot of important work. There was a great deal of work that had been done in other areas. Sericulture, for example. The Japanese knew more about the genetics of the silkworm than anyone, including the Chinese, who were their major competitors then.

AM: And yeast. They knew a lot about yeast then.

WS: Right, and they knew a lot about yeast. So in terms of sort of the basic areas of genetics, they were very strong, and they had outstanding people. Taku Komai, Yoshimasa Tanaka, Hitoshi Kihara, Kan Oguma, these were people with international reputations. They used to like to analogize. They had a woman whose name was [Kono] Yasui. Her first name doesn't come to me at the moment. She was always likened to

Barbara McClintock in her sort of role. Kihara was one of the authorities on the evolutionary origin of wheat, for example, and recognized internationally. So these were first-rate.

Physics was good, but it had been very badly dampened towards the end of the war. But they had people in theoretical physics that were very strong. I think the sciences that we ran into, where you might have expected more and I don't think it was particularly strong at the time, medicine was one. Medicine was much more didactic in Japan. There was relatively little that was taught from the bedside. There was no obligatory internship. The whole structure of the medical education was quite different, and I don't think it produced physicians generally who were as well trained say in 1950 as a graduate of a medical school with ostensibly the same level of recognition. Let me couch it this way. If you took a student who graduated from the medical school of the University of Tokyo, which was Japan's premier school, and a graduate from the medical school at Harvard, our premier school, the Harvard student would have known much more medicine than the one from Tokyo, largely because so much of the Tokyo education would have been pro forma lectures, lectures, lectures, lectures. Little opportunity to put into practice what they were learning from these lectures by observing skillful practitioners.

So I would say that at least half a dozen of the areas were very strong. Two areas that were weak then, and they're still weak in Japan -- epidemiology and biostatistics. Good mathematicians, and there's no reason in the world why they couldn't have had good biostatistics. They have had some very good mathematical statisticians. Then, and I think to a large extent now, a person who leans towards statistics -- and I'm really sort of thinking of biostatistics -- is generally going to be embedded in the Department of Mathematics, and his recognition and promotion comes in competition with full-fledged, full-time mathematicians. So if you are, let's say, someone who is interested in multivariate analysis, your prospects of advancing would be in the mathematics of multivariate analysis, not in its application. That doesn't seem to win any merit points. And it still doesn't, unfortunately, because these are areas where they *could* be strong, but they're not, and I really don't know why.

Fifty years ago, I guess one could have seen an explanation. We weren't all that strong. These weren't well-recognized disciplines then, and they weren't -- a compartmentalization has since occurred. It obviously hadn't at that point in time. So you might have made an argument for the absence of biostatistics in 1950, or even the fact that you didn't have schools of public health in Japan to the extent that we have them. So you wouldn't have had the supplemental support in epidemiology and things like that.

But there's no reason why that shouldn't be so now, save for the fact that I guess there's a large moment of inertia built into the system, and there still isn't recognition there. I've had friends who were in Japan with very strong credentials, who were really very much interested in the application of mathematical statistics to problems in biology and medicine, who never really got an opportunity to show what they could have done in this area because they were either writing a probability theory or vector analysis or something. They had to earn their place in their own department in the context of the traditional areas of mathematics. Most of the statisticians, for example, would probably have ended up being probabilists because the was an acceptable area of mathematics, probability theory.

I certainly wouldn't want to, by any matter or means, pass a wand over the entire group. There were areas that were very good. Certainly, the people that I got to know in genetics in 1949-'51, my first tour, were all outstanding people, both as persons and in their own areas. Taku Komai, for example, was probably *the* authority on the evolutionary biology of the ladybug. Kihara, I've already mentioned, was not only probably the most knowledgeable person in the evolution of wheat at that point in time, he was the developer of the first seedless watermelon, for example. These people combined applied and theoretic areas as well. And though Kan Oguma got the number of chromosomes wrong, he was writing at the time of World War I.

AM: Right. And he wasn't the only one getting them wrong.

WS: That's right. As did we in our first book. (laughs) At any rate, genetics was a strong science, I guess is the thing I wanted to say. Not human genetics. Although Komai had published a number of little monographs on inherited abnormalities among the Japanese, but as basically a biologist and not a human biologist. These were collections that were in the literature, and his publications were in English so that there would be some recognition outside of Japan for what was going on. But there really weren't any schools developing in which there were strong programs in human genetics.

A number of people were working in serology. I was trying to remember. I know his first name is Tanemoto, which means the origin of things. Furuhata. Furuhata had been involved in the ABO blood groups almost from year one. Among the first publications that dealt with the secreter phenomenon, that is, the presence of water soluble forms of the AB antigens in the serum, the work was [Fritz] Schiff and [H.] Sasaki. Sasaki happened to be a Japanese student in Germany at the time.

They had been doing good, maybe not equivalent work in terms of proportional representation, but they had some very good people, and they were interested and they were easy to work with. I found them a great bunch. Most of the men that I've been describing were already in their sixties when I went to Japan and couldn't have been nicer to a twenty-seven-year-old recent graduate than they were to me.

They were never condescending or anything else. Most of them had reasonable command of English, too. Komai, for example, was a student of T[homas] H. Morgan, and he and his wife both spoke excellent English. Usually, at a meeting of the Genetics Society of Japan, he'd end up sitting beside me and translating the tables and titles and things like that. He certainly didn't have to do it, and it was just a measure of both the personality of Taku Komai, the obligations of hospitality in Japan, and just being very fine people. They're great.

I had a marvelous time, and it was very instructive. I was fortunate to see a side of it which might not otherwise have occurred. [Hermann J.] Muller, after T.H. Morgan, was the next geneticist to get the Nobel Prize. He got it in -- I think it was '46. It could have been '47, '46. Then he became very active in this thing known as -- I think it was called the Congress for Cultural Freedom, something like that. And in -- it would have been 1950, I believe, or it could have been '51 -- there was a big meeting in India to which Muller had been invited to speak, and, of course, he spoke about his experiences in Russia, where he had spent a number of years with [Nikolay V. Timofeev-] Ressovsky, and his other concerns about the direction of governmental interference in science.

He was invited to stop in Japan on his way back, as a Nobel Laureate. I wasn't -- and Masuo Kodani -- we weren't asked to come to Tokyo. We were summoned, in effect, by SCAP [Supreme Command for the Allied Powers]. (chuckles) Because we were the only two geneticists that they knew of in the country at the time, other than the Japanese ones. So we went to help tour Muller. Among the things that occurred then was the National Institute of Genetics, which is really an extremely strong and able group, had come into being I think in 1948. So Muller was invited there, and, obviously, since I was his guide, I went too. There's an interesting photograph that shows us at a meeting after the formal meeting at the institute at -- oh gosh, the name is eluding me. This is a very famous watering spot, very close to Mishima, and here is a picture of Muller and myself, [Yoshito] Shinoto, Komai, Kihara, this whole panoply of names in Japan, and here in the back of this (chuckles) is me. I gave at least thirty years to the next youngest person. (laughs)

It was fun, and it was an instructive thing because it gave insight into Muller's own standing, the quality of the science in Japan, the alertness. When the National Institute of Genetics was established, Richard [B.] Goldschmidt was professor and about to retire at the University of California, and he donated his entire reprint collection to the National Institute. Since they hadn't been in a position to buy a book in a decade, this was a formidable thing. Everybody who went there got to see this row after row of reprints, not only of Goldschmidt's own work, but of all the reprints that he had collected, or had been sent to him. It was an important gift at the time and they wanted us to see that.

Also, Muller was already familiar with the work that Joshua Lederberg was doing on -- I'll call it sexuality in bacteria, and he spoke about that. Basically, his presentation at the National Institute was sort of the current status of genetics in the United States, and among these things was some of the work on radiation that was going on, work particularly in bacterial genetics and in Drosophila and the like, so it was kind of a skimming across many areas.

Most of them were well enough trained, and certainly equipped enough, and the major figures, almost without exception, understood a substantial amount of English. Asked some very penetrating questions afterwards, which was really a marvelous kind of situation. It wasn't just a showing of the flag, as it were. There was some nice interaction.

AM: Okay. The next question I wanted to ask you was, this was one of the kind of large -- I don't know if I want to say the first, but a very large-scale international collaboration of scientists that -- it had a long duration anyway. What would you say was the impact on both Japanese science and American science as a result of the effects of atomic radiation, and whether it's the early ABCC stuff or the inbreeding stuff that you continued later?

WS: Well, I think there was both a direct effect and sort of an indirect one. The direct effect, I guess I would say, relates to what was an increase in interest in the health consequences of exposure to ionizing radiation, so that an area of human biology that had not seen much support before, in our own country as well, suddenly became more interesting to the Japanese. This led, in time, to the foundation of the National Institute of Radiological Sciences and to a program in radiation genetics at the National Institute of Genetics, to the increasing use of radiation as a tool to understand genetic processes and also as a means of contributing to clinical medicine. Those were all direct consequences.

I think an indirect consequence was that as they saw more and more of us -- and one thing I think I can say about all of the colleagues that I've had in Japan at various times, and I'm speaking about the American colleagues, everyone was more than willing to be of help to their Japanese counterparts in the sense that if I read and edited one paper, I have read and edited hundreds of papers in Japan over the years. Sometimes, it would have been far easier for me to have written the paper just taking their tables than to go through, but there's no learning process in that, or at best a very weak one.

More, it was a matter of sort of sitting down with them and explaining why I thought they needed to do this that they hadn't done, or that they could couch their results in different sorts of ways, and there were the implications of those different ways of couching. Those were all indirect contributions, just our sheer presence, the recognition that in many areas the level of science that was being practiced at the then Atomic Bomb Casualty Commission was way ahead of its competitors in equivalent areas in Japan.

This led eventually to the establishment of two institutes, one at Hiroshima University, one at Nagasaki University, that tried to develop competing programs. I don't use that in a pejorative sense, either, but just they thought these were important enough that the Japanese *ought* to be able to do work in these areas as well, and those events came into being about the same time as the National Institute of Radiological Sciences was established, the very late fifties or very early sixties. I've forgotten the exact date. So there was an important thing.

One of the other contributions you see, throughout the history of the institution, there were actually more Japanese professionals employed than there were American professionals employed. Even in the days when it was called the Atomic Bomb Casualty Commission that was true. And it has always been so. They were privy to what was going on in a firsthand way. They were encouraged to present both internationally and to Japanese society meetings, but their presentations had to satisfy qualities that we set, sort of the standard that we set. Whether they presented it in Japanese -- usually, we would have to evaluate what they were proposing to present in English, and then what would eventually evolve would be a Japanese translation of the English.

So you were encouraging them and establishing a set of standards for presentation which were different. I don't mean to imply that it was necessarily a better science, but it was a different way of presenting science, less dogmatic in some instances, because the Japanese model had been the German one. Certainly there are enough jokes about the Geheimrat that I don't need to enlarge on those. I think those things helped Japanese science.

In those years, it was not possible, except if it was very extenuating circumstances, for an American to be a formal lecturer at a Japanese university. That's all changed now. You can even be an American employed by a Japanese university. But at that time, that wasn't really so. Therefore, if we were to speak at universities, we did so on the invitation, and there was no formal relationship. I used to give lectures in Tokyo in radiation genetics, but these were invitational things. The head of the department was Shigefumi Okada, who had spent a large number of years in the United States at the

University of Rochester and had the strongest department of radiation biology in the country when he returned. He was one of those who was literally bought back to the old country because he was so good, and he stepped in directly as a professor and head of the department. They're almost synonymous the way the Japanese educational system operates in Japan. But there have been others where that's true, too.

They have brought U.S. standards back, and they have the added advantage that it would be possible for a Japanese to see us as speaking down to them. That really wasn't the aim ever, at least among the people I know well. But if it were someone of their own backgrounds, even though they'd spent a lot of their time in the United States, that wasn't seen in the same sort of light. It was welcome, because here was someone who was Japanese would understand Japanese problems but had American credentials, too. That worked very well.

AM: What do you think the broader impact on American science was because of this project?

WS: That's kind of hard for me to answer in the sense that -- the first thing that comes to mind is, internationally, the worst of that study has been what it's told us about the biologic consequences of exposure to ionizing radiation, both in terms of kinds of events as well as the frequencies of those events.

Now, it has in some instances in the United States, for example, given substance to areas of laboratory research because of the application of that information to what was going on in Japan, or the fact that the importance of that research could be attested to by virtue of similar findings in Japan, because, obviously, there are areas in which you can do things experimentally that you can't possibly do in a human population, or in which -- even though to a very large extent what we do in Japan represents a natural experiment, maybe quotations around the natural, what one sees here provides avenues both to provide us a biological mechanism for what we're seeing in Japan, where it would be harder to discern that mechanism purely on the basis of the Japanese studies alone.

Because oftentimes you can learn more certainly about these events. Mutation would be an example, because the Drosophila will tolerate such large doses of ionizing radiation without killing a lot of them that you can increase the probability of a mutation occurring to a level at which you're not dealing with a really rare event. Therefore, you can amass numbers that give you dose response relationships that would not be so easily obtained in a human. It would give you some sense of whether or not different loci mutate at different rates, and which loci would be the more mutable if the former statement was true.

So it could be a nice wedding between -- particularly the things that were going on at the national laboratories, like Oak Ridge [National Laboratory], Lawrence Livermore [National Laboratory], probably, to a much lesser extent, Los Alamos [National Laboratory] because Los Alamos didn't have that large a program in biology. They were much more kind of _____ oriented. The Livermore _____, too, but they had a very strong program in biology.

Of all those laboratories that I can think of, probably the three with the strongest biology programs were -- Oak Ridge I would put at number one, in large measure because of Alexander Hollaender, who ran a phenomenal show there. Number two would probably be almost a tie between Argonne [National Laboratory] in Chicago and Long Island [Brookhaven National Laboratory]. That one had a larger program in medicine, not so much in some of the other areas of biology, although those four institutions probably had the strongest programs in biology, and we did feed off of them, and vice versa.

AM: What about the impact on more non-scientific research-type areas? For example, the role of the government in determining research programs and setting levels of funding, projects that would have an international scope, or even just how science could be organized to cover long-term research projects.

WS: That again is one to which my response will be very subjective in the sense that I had impressions _____. For example, in the Japanese situation, the existence of grant applications in the United States has led to some mimicking of the system that we have in Japan, but as is usually the case, the Japanese have put their own imprint upon it. They have moved away from a kind of grant support mechanism that had been common in the fifties, for example, to one that is more akin to ours.

What used to be so -- and obviously it varies from institution to institution, but this was the general consensus -- was that the monies would go to a university to support research, and that money was not distributed amongst the competing research departments in any way which was proportional to the quality of the department or their achievements. It was pretty much, if there were three departments of medicine, let's say one of which might be very good and two of which weren't _____, each got the same amount of money. There was no effort to judge quality as such. It was a recognition of the importance of research that was going on, but it wasn't reflected in the levels at which the _____ was occurring.

It also was true that a lot of that research was centered on specific individuals, many of whom were past their research prime, and where the money then would sort of support their special interests and/or would be used to the furtherance of their chosen heir apparents. So it really wasn't a system which reflected -- it was not a meritocratic system at all. It was pretty much the worst of the old boy system. It was who you knew and who your mentor knew that determined a lot of the funding.

Fortunately, in the beginning, I would say, since probably about 1980, enormous changes have occurred in Japan with respect to those things, and I think very much for the better, both with respect to the openness of appointments to university, the nature of the funding that is available to investigators, there have been enormous strides. I wouldn't say it was yet equal to the way we distribute money, but it's a much more equitable system than it was.

AM: And in the United States, do you think the way that the ABCC program was organized and designed and carried out had any long-lasting effects on the broader organization of American science?

WS: I don't know how to answer that, in large measure because there really hasn't been -- certainly, there's been no emulation of what's going on in Japan. You can't point to another program that's been underway for, now, fifty-eight years. The nearest would be the Framingham study, but Framingham was a different kettle of fish, too, in the sense that Framingham was a branch of the National Institutes of Health, the initial studies. Whereas, that was never true even of ABCC. It was funded by the Atomic Energy Commission, but the organization responsible for the science was the National Academy of Science.

It's not easy to see how you would compare it, and in fact, although I think that the studies in Japan have more than compensated us for the monies that were spent, if you were to add up what it has cost to maintain that program over fifty-eight years, we have to be talking about a couple of billion dollars, with the funding now running on the order of forty-five million a year. Now, in the early years, it was one or two million, but if you took into account the inflationary factor, you would probably be at more than two billion. Aside from the human genome project, I really can't think of any others that have had that kind of support.

I think it has pointed up to us the importance of ABCC, RERF [Radiation Effects Research Foundation], life studies in particular realms of science, that if you really want to see human biology in its finest structure, then you do that only by looking simultaneously at populations in depth, and people within that population in depth. And you can accumulate those only by long-term observations, to my knowledge. Maybe in some respects you can emulate some of this by cellular studies, but in the main, not. I mean, they're never going to satisfy people, nor should they, that what happens to a cell grown in a culture over the course of its lifetime, or even over the course of many lifetimes of cells descended from those cells, is the same as what goes on in the human body.

I think, to me, the biggest thing that's been -- because of the cost, those studies have to -- where they are to be implemented has to be chosen well, and you need a continuing level of commitment. I really want to emphasize that. You can get the scientist to do the work, and so on, but you need a long-term commitment from the funding agencies who are ultimately going to be responsible for source where this money is going to be spent.

I think one of the biggest problems here is that bureaucrats don't really establish reputations for themselves by simply managing programs that others have initiated and they are basically endorsing. It's the new things that they develop. Now, in a constrained resource environment, that requires new money. And if new money isn't coming from Congress, you get it only by cutting the old programs. So I think there's a conflict of interest there from the very outset. I think RERF has experienced that. I think other studies have, too.

AM: How about the Human Genetic Diversity Project [Human Genome Diversity Project]? Has it been plagued with these same issues, or is it something different?

WS: I think to some extent it has. I think one of the considerations is the potential cost, and then from whence are the monies to come? I think there's still a sizable portion of the scientific community that believes that we would have learned as much about the human genome without the genome project as such as we have learned from it. It would have been learned at a different rate is all. What was learned would probably have been learned in the context of more applicability than goes on here, where the _____ came to be one of knowing every codon in the entire genome, and the subsequent recognition that a very substantial amount of that is redundant and doesn't appear to be doing a damn thing.

Whereas, had it been approached from the standpoint of people who are looking at inherited diseases and then trying to determine the sequence of the genes associated with those diseases, you wouldn't have been doing a lot of work on redundant DNA, as has occurred.

Only time is going to tell. I think we would ultimately get to the same point by either one of these methods. It's just when we would have reached it and how much would we have gained in one instance or lost in another to achieve that timing? I personally probably would have tilted more towards the argument, let's do the sequencing when we know what the heck the genes are doing, when we can identify a gene that has some conspicuous relationship to a disease. But there are others who built strong arguments and I --

AM: Okay. One last broad question, and then we'll get back to some more just basic direct questions on your experiences at Michigan. That is, you had mentioned that, clearly, human genetics in Japan was lagging behind American human genetics, but one could certainly make the argument that American human genetics lagged behind English human genetics, at least earlier, with [Lionel S.] Penrose.

WS: Oh, sure. I would say that was probably true in the thirties.

AM: And what do you think accounts for these lags in human genetics? And again, I realize that this is more asking about your impressions rather than direct experience with working in a lab in England.

WS: You've been asking some toughies. I'm really not sure in this instance. I think, to some extent, there has been a clear hierarchy over time. I would have been inclined to say probably if you were going to evaluate the level of human genetics, exclusive of Hitler's appearance on the scene -- the Germans were far ahead of the English -- the methods of analysis that we were using. [Wilhelm] Weinberg anticipated so much that came along, or even [Ronald A.] Fischer was writing about thirty years later.

But then this was all set topsy-turvy by the political situation. I think that, in some instances, what it amounts to is that genetics must reflect the general quality of science in all areas of science at a particular point in time. You certainly can see it, let's say, in terms of the proportion of important levels of recognition, Nobel Prizes, in the United States prior to say the advent of World War II. We didn't have all that many. We had some in physics, probably more in physics than almost anyplace else. But unlike say what's transpired since 1945 to the present -- I mean, in most of the sciences I would say three-quarters, seventy-five percent, probably, of all of the prizes end up going to a scientist in the United States. Whereas, prior to World War II, that proportion would have been very heavily German and then subsequently English.

What I'm saying is, the science of genetics reflects what was going on in science generally, and we were not a strong scientific country prior to 1940. We had a lot of first-rate scientists, but we profited, I think, by the impetus of émigrés from Europe, both by virtue of their ethnic backgrounds and by virtue of their political standing. We had some who came from Germany and elsewhere who were not Jewish, who just found the whole political scene as it was unraveling in Germany deplorable and didn't want to be any part of it.

We profited from that because that helped to bring another standard. Also, in a way, it made us live up to a potential that we always had, but we were so busy doing other things, like automobiles and whatnot that basic science didn't loom quite as large.

X. Environment at Michigan; Genetic Counseling; Organizing Genetic Study at the University of Texas; Miscellaneous

AM: I have about five questions I have left, and I'll just throw them out there and let you take them where you will, because I know we're running out of time. When you arrived at Michigan, clearly you had enough data to keep you busy, with the Japanese data, but what were your own interests and expectations to do work besides the ionizing radiation research, to pursue something independent of that, and of Neel's work?

WS: Well, the work in Japan had opened a number of issues dealing -- we talked about the most prominent one, biology and radiation genetics. But then there was the whole business about inbreeding and its implications, which could be studied, certainly at that time, much more easily in Japan than in the United States. All of the things that I'd been doing, although in time they would spill off in a fair number of publications, were not enterprises that you initiated, collected, and completed in a year, so that these things were running on simultaneously.

We were beginning to incubate the work in consanguineous, as I said, even at a time when we were finishing the monograph on radiation genetics. That was going on at the same time that we were trying to complete our textbook, which appeared in '54. The monograph on radiation was in '56. That same year was the monograph on neurofibromatosis.

So there were a whole host of things going to which you sort of devoted time and attention. The actual dates of publication maybe didn't reflect the time commitments in quite the same way as actually occurred, because it obviously was extremely important -- well, we could do the neurofibromatosis thing entirely on our own. That was a project initiated at Michigan, totally self-contained at Michigan, and so on. When we talked about the study of the pregnancy terminations in Hiroshima and Nagasaki, it included a much larger number of participants, and if the reports of that study were to be equitable, then everybody had to have an opportunity to voice their concerns or their additions or suggestions, or whatever. So that's a more time-consuming proposition than basically just Jim Neel, Frank [W.] Crowe, and myself sitting down and writing a monograph on neurofibromatosis.

And by the time those things were out, well, gosh, we were already doing the same sort of business on inbreeding. The bulk of the second study of inbreeding wasn't completed until 1970, so that virtually encompasses my entire career at Michigan. Then there were odd things that came along along the way that I was interested in and did things on, but those are sort of --

AM: Did you run towards them or did they run into you?

WS: Well, it sort of was both. Sometimes these were things that just your involvement was serendipitous. You were not planning it, but suddenly along came an opportunity and you saw it and you seized it. It was that. In many instances, it involved a short-term time commitment at any rate, months or perhaps sometimes even just weeks, and it was quite different from the other.

AM: You had mentioned that Neel was getting his funding from the AEC. Where were you getting your funding?

WS: Well, I was actually involved in most of those things. Jim was the principal investigator, but I was a co-investigator, or supported -- although, throughout the time I was at Michigan, my own salary didn't come off of grants, it was from the university.

AM: And did you write aspects -- how much grant writing on your own work did you do, or was that all part of --

WS: No. Invariably, those were joint things that Jim and I would do together. The parts that I was going to be most heavily involved in, I would write, and then we'd work through the things together and make sure that there was a coherence to it and so it didn't look as though you'd assigned A, B, and C to three different writers who weren't aware of what the others were doing.

AM: Did that include just your own work, or did other faculty members also participate in these larger grants?

WS: So far as the things that were concerned with Japan, for example, it was just Jim and I. If it were things that dealt with the department overall, like the program projects that the department had that came from NIH, then everybody was involved. They were in a sense responsible for developing, defending, setting forth their own individual contributions.

There was much to be gained, was the general feeling at the time, through program projects, rather than having a series of individual investigator-initiated and totally defined kinds of projects, because we thought there was a collective strength, that this was one of those situations in which the whole is greater than some of the parts. A program project gives you an opportunity to reflect that.

AM: Were all the faculty members, members of the department, agreeable to that, as far as you knew?

WS: I think that was generally so. There were some who their commitment to the program project might not be there full time, that they would have other commitments. This would be true particularly of those who, let's say, were interested in some area of clinical medicine different from the area that you might be interested in. George [W.] Brewer, for example, was very much concerned with red cell metabolism, so he would have grant applications that were separately funded that went to support those kinds of activities.

AM: Okay. Well, in Neel's autobiography, he talks about he was trained in Drosophila but then he developed an interest in human genetics and really felt he needed his medical degree, so he went and he got it at Rochester. And you come to Michigan with a Ph.D. in a basic science. What was the relationship in the Department of Human Genetics between those trained in basic science and those with more clinical training, or clinical science?

WS: Well, I think there was some mutuality about it all. I don't think that the basic scientists, who might have seen themselves as better scientists, in one sense, viewed the clinically oriented as less -- or _____.

I think the thing that was important was, the group was so structured and the personality composition was such that if two or more people saw a problem they thought was really interesting, where they could collaborate to the profit of both, or all, then they did so. There were not a lot of little individual fiefdoms, and people didn't sort of approach it in that way. I think we all appreciated the extent to which the department had great strengths across the board, and those strengths worked to the benefit of every one of us. All we had to do was to try to figure out ways in which we could both contribute to that overall strength and at the same time utilize portions of it.

AM: What do you think accounts for the synergy that was present at Michigan at this time?

WS: I'm sure part of it was due to the nature of Jim's personality, Jim's vision of genetics, the fact that he was also a consummate scientist himself. But also I think there is the issue of time. Some of the success of individuals or institutions, I believe, reflects an element of fortuity. It all came together at a time which was opportune. It was a new field. We happened to have critical mass *very* early. And all of this contributed to a time at which say, at most institutions in the United States, there were conflicts going on about whether human genetics should be in the medical school, can it be in the Department of Zoology, where should it be? I think more energy was expended on those jurisdictional battles than any one particular way of coping justified.

And we didn't have that. We were in the medical school already. We had very strong ties from the years in which the Institute of Biology existed, and so on, into the Arts and Sciences College. And we provided sort of a nidus of strength for all of the geneticists on the campus. When genetic training grants came into existence, that grant was given to the department, but a strong component of it was to support students in what was called the genetics program, which was more embracing than just human genetics, so that students in genetics who were in the zoology department could qualify, or students of Cy [rus] Levanthal, for example, could, and Cyrus was in physics.

So the training grant, too, contributed in part because it both provided a source of funding for students who wanted to study with one of us who were actually in the Department of Human Genetics, but it also was a source of funding for students who were reaching out into genetics in other areas.

At that time, Michigan had a very distinguished group of geneticists. Allan [M.] Campbell was there in bacterial genetics. Cy Levanthal, viral and sort of the more physical-oriented things. Dave [David L.] Nanney was there. Erich Steiner, there's a whole group in human genetics, in our department. There were several dozen geneticists, most of whom have gone on to establish very distinguished reputations. Charlie [Charles A.] Thomas [, Jr.] was one of them, for example, that had been at Michigan. [H. Orin] Halvorson at the University of Wisconsin was there. So we were -- golly, in the late forties and a few months of the fifties -- spinning off geneticists like you just wouldn't imagine, both in the department and from the others which were attached. One of the really outstanding ones, whose name is eluding me, was chairman of the department at Yale, died a few year back, had been involved in the fracas about communism in faculty at Michigan. He was there. So we really had a great group.

To some extent, even before the department came into being, most of these people had appointments in the Institute of Human Biology, although these were sort of honorary appointments in the sense that they weren't deriving any payment from the institute. Clem [Clement L.] Markert was one of those sorts of persons. He had an honorary position in the Institute of Human Biology.

The department ended up being kind of the nidus around all the rest that was precipitated. That helped us, too, because there were things where we could reach outside the department, down to the mouse house, which was still very much alive and going.

AM: Well, you've written a little bit about genetic counseling, an editorial and another little piece that I know of, so how did one become a genetic counselor in an era when there weren't training programs in genetic counseling? I guess there were, but they probably weren't -- they were on the fly.

WS: Yeah. I think most of it was trial and error. People sought information. Much of it, of course -- I think a fairly large proportion of it had to do with problems arising among children. It was parents or potential parents who were concerned about the repetition of a particular genetic event. The pediatricians were usually reluctant because they weren't familiar with the literature, so they'd pass these problems on to us, and our response would either stem from a knowledge of the inheritance of the disorder, if the disorder *was* known to be inherited and known to be simply so, or from empiric estimates. There was a lot of work that had been done in Denmark, and in the United States, too, relating to congenital defects, and the study of large numbers of families to see the empirical estimates of the probability of repetition.

So those were the sources. It was basically a matter of knowing the literature, and then the first couple of times you groped along, and I guess you became more skillful with sort of the sensitivity issues over time as you saw more and more cases. At that time, the faction was still that whoever was doing the counseling usually did it on a one-on-one basis. You didn't have other people about. Even the physician who was responsible for the case would absent himself, or herself. So you didn't have the opportunity for sort of an apprenticeship in which you saw someone else coping with a problem and how they handled it. You might have one, or something like that. Just before you were dropped into the pool by yourself, Jim would sit down and would go through one of them together, or something like that.

But as a general rule, there was no formal structure. In fact, I think for the most part, although all of us saw it was an obligation, and not an unduly onerous one, it was not one that we looked forward to with a great deal of excitement about doing. It was something that was important, needed to be done, you did it to the best of your ability, but also at the same time there was kind of this nagging notion that it's taking you away from things which I can do better. (chuckles)

AM: You had written in the late fifties, commenting at a conference, that you thought that counseling was statistically naive, empirically naive, at that point and then there was a rush to open counseling centers before there was a really good way to train genetic counselors.

WS: Oh, I think that was true, and I think even when training finally began, I'm not sure that the methods that were introduced in the sixties, or maybe it might be even as late as the seventies before you had institutions that were focusing specifically on training counselors, and this was before certification and all the rest of this came about. These were often not places that were especially distinguished for their genetic research. They were places that maybe had very good clinical departments, and you had persons who were sensitive to the problems and felt, and properly so, that genetics as a discipline really wasn't addressing these things with the commitment that was needed, and they saw an opportunity.

Quite a bit of this started in New York City, and I think that what was done there -- I'm sure they had to go through the whole empirical route. People had notions about how you might do this, but whether it would work well or not was anybody's guess. The whole notion of group counseling that has evolved since -- my God, sometimes these groups can be eight, ten, twelve people, and I would think, to my mind at any rate, those groups run the risk of being more intimidating than they are actually constructive. You have all these people in white coats sitting around, and here you are without a white coat feeling somewhat lost.

There's no question that the group has this strength: If an individual has a question that may arise in the course of cogitating over their own problem or hearing what others say about it, there's going to be an expert there who can answer. I'm not sure that is a sufficient trade-off for the fact that maybe it's good to have that person there *if* someone has the courage and is motivated to ask questions, but I think there are a lot of people who, under those circumstances, will just shut up and they won't ask questions. Then the presence of all of these authorities doesn't help matters. In fact, it works exactly to the contrary.

I would think that probably the best way is to make counseling -- and I know this happens at some institutions -- not a one contact event, but the sort of situation in which the person who has the problem sits down with a skilled counselor, and they sort of, together, mull over the problem and then decide -- well now, maybe we really ought to talk about this again, and how would you like to have an expert in this particular disease present? I think most people would say yes, I think that would be very good. So they have a feeling that this is an evolving process in which someone with considerable sensitivity at the outset, the primary counselor, with whom the patient or the patient's family identify could then expand it.

But to just be sort of plunked down into a large-ish gathering, I'm not so sure that that's any better. In fact, I'd be inclined to think it wasn't. This kind of evolutionary approach is going from establishing trust between *a* person and the individuals being counseled, and then to work out what is really needed in the counselor's view but under a program that gives the person who is being counseled a sense of involvement, a sense of familiarity, and that they have had a chance to mull over some of the things that trouble them.

AM: And how do you assess the impact of making genetic counseling its own -- it has its own medical board, and it's further separated from both basic scientists and clinical geneticists and have their own professional identity now, separate from the main professional organization of human genetics?

WS: I really can't pass judgment because I haven't had enough, myself, involvement in that sort of structure. We have such a group here, run by Jacqueline Hecht, and I think it's very good at what it does. But it's more word of mouth than it is through actual having had a lot of time of contact and seeing how it goes on in practical application. I haven't been involved in counseling as such in probably thirty years.

AM: Also at the time that you were writing about genetic counseling, new technologies were being introduced that allowed for prenatal diagnostics, and also simultaneously was the legalization of abortion, or at least liberalizing abortion laws. How did you resolve issues between your religious beliefs, particularly your Catholic beliefs that oppose abortion for many reasons, and counseling patients that prenatal testing would allow to reveal some genetic orders and this is something that they could actually make some kind of decision about?

WS: Well, you see, during the years in which I had much involvement in counseling, abortion wasn't legal, so I didn't have the conflict of interest that might arise now. At the time when I *was* counseling, the number of options that we had were very limited. In the main, what we did was to provide the families with information and then try to explore with them the consequences of following the various alternatives that we saw. Then it was their decision what they wanted to do. Amniocentesis wasn't about. Chromosomes were just sort of coming into play. We didn't have the information that you would have now through cellular studies to determine the likelihood of a particular outcome long before that would have occurred in the past. It was future reproduction primarily that you were counseling them over. There wasn't much that they could do about commitments that were already made, at least not legally.

AM: Okay. I'm just going to be jumping around. I want to go back to what you were saying yesterday about coming here to the University of Texas. You came not to get away from Michigan, but you saw this as a new kind of really experimental and good way to organize genetics at this new center that was being created here in Houston. I guess two separate questions: Shortly after you came here, the bureaucratic forces that be kind of changed that dream of people here, and -- so how would you now assess how well, or the advantages or disadvantages of the way that the programs in genetics were set up here? The second question is, what was the impact in 1994, these two centers for human medical genetics and then population genetics were fused into one and moved to the School of Public Health?

WS: It's easiest to start with the last. In fact, for all practical purposes, the fusion had occurred before that. It occurred when Margery Shaw decided that she no longer wanted to run the Medical Genetics Center. The dean at the time was [R.] William "Bill" Butcher, and he wanted me to take over that center too. So from the time that that happened -- I'd have to look back to determine what the year actually was, it would probably have been very early eighties -- it offered an opportunity to begin to fuse the two groups.

Margery and I had somewhat different philosophies about what we saw our function as being, and I think I had done more towards creating a unit to stand totally independently. Because Marge's people -- to some extent, I think she was trying to work within the genetics that actually existed in the medical school at the time and to use the Medical Genetics Center as a means of complementing or supplementing what was going on in the medical school. This meant, in effect -- because the bulk of the pediatrics genetics is already going on in the medical school with Rod [R. Rodney] Howell, Bill Hubbard, and others -- that the Medical Genetics Center had sacrificed certain areas to play for this larger role. I think that was a wholly understandable way of handling it, but it seemed to me it also meant then that you didn't have the capacity to be a free-standing institution.

So when Marge became progressively more and more involved in genetics and the law and decided she no longer wanted to run the Medical Genetics Center, and I took it over, my intent was to sort of fuse the two. The purpose of that fusion was severalfold, but it was driven by a major consideration, and that was this. We had had five academic appointments. Margery had five academic appointments, too. What had concerned me a lot was the fact that many of our programmatic activities were built around one person and his or her students. The consequences of that was, if that person was recruited elsewhere, we lost a whole area. I thought, well, if we take the two centers and retain a somewhat more restricted focus, we could have redundancy, and in the long run, if we chose our areas well, that redundancy would pay off because we'd never suddenly find ourselves with a whole program gone. If Masatoshi [Nei] had picked up and left in the middle seventies, there would have gone theoretical population genetics because he was it. But Wen-Hsiung [Li] provided the backup there and stayed on for almost a decade after Masatoshi left.

So the whole thing became one of, well, let's have fewer directions. Let's not worry about what's going on in the medical school or elsewhere. Let's try to get a combined center, although both names still continued, largely because we had two budgets. Maybe it was just lack of courage on my part. I was afraid to challenge -- I was reluctant, let's put it that way. I was reluctant to challenge the administrative authorities and say we want one unit, with the supposition that I'd get both budgets. It might be easier to just leave the paper situation as it was with a line item for the Medical Genetics

Center and another one for the Center _____ and Population Genetics, but in fact, utilize the two as if they were one. That was what I was basically doing.

By the time we finally came into the School of Public Health, that no longer was a matter of moment, so clearly we were going to keep our positions and we would have a budget that reflected both of the original center budgets.

The decision to move us to the School of Public Health, perhaps by that time we *were* identified more with population genetics and we had been set up to be the bridge to the School of Public Health. That was understandable. I didn't feel strongly one way or the other about it. I think the one thing that we were concerned about was what this would mean from a spatial -- a growth proposition, and so on.

It was clear at the time that we moved into the School of Public Health that the powers that be had a different view about where graduate education should stand. This graduate school was really quite unique for a number of years in the sense that it had two faculties, for practical purposes. It had the faculty that it paid and the faculty that it didn't pay. As you know, at most universities, the graduate school has no paid faculty of its own. It's got a group of paid administrators, but the faculties are paid through the schools of which they are actually a part, so their primary allegiance goes to that school. Well, here, you had people in the medical school who taught, you had the people in the School of Public Health, not so many, who taught in the graduate school, and you had a lot of people from the Anderson who taught.

Yet, the graduate school also had a budget that supported three centers -- the Neurosensory Sciences Center, the Medical Genetics Center, and our center. There were administrators at the presidential level who were uncomfortable with that structure. They had grown up in institutions where that had never occurred, so they thought that to continue to support these separate centers conflicted with their notions of how to organize medical education.

So the Neurosensory Sciences Center essentially ceased to exist. Its members got pushed into the Department of Ophthalmology, or to one of the other departments which really reflected their training and their research interests. But it doesn't exist now as a unit as such; whereas, our two centers are basically put together as the Center for Human Genetics and moved in toto into the School of Public Health.

Physically, we -- well, we didn't even change physically for quite a number of years because we continued on in the graduate school, which no longer exists, up until -- oh, gee whiz, when was it? When I retired, we were still in the graduate school, when I formally retired. And that was in February '98, I think was the date. So we remained in the graduate school building up until probably about 2000. I don't remember exactly the year we moved in here, but roughly that. So in those earlier periods, there was no change of our physical location. I always liked it over there because I always felt that it was probably an economically inviable solution.

It was a delightful little school -- two buildings connected by a big atrium, each two stories high. This meant four floors. We had three of the four floors. The dean and his administration occupied the fourth level. So it was great. We were physically close together. Basically, we were in a unit all of our own. We weren't displacing anybody else because the School of Public Health already existed. When they decided to knock that building down and we were to come into here, it meant the displacement of somebody, or somebodies.

That didn't look like necessarily a healthy solution, that we'd come in -- not that the solution was an irrational one, but that we might have inherited some antipathy as a consequence of displacing people, because basically they had to cut the fourth and fifth floors out, both sides, for genetics, and that wasn't true before. There were people here and they had lesser space is what it all boils down to. I don't know that there was any change in funding because we basically brought our funds with us from the graduate school.

I suppose, in the light of how education is structured at most institutions, the present arrangement seems more rational. I kind of liked the other one. Maybe it's just because it had been attractive when I came. It gave us a measure of independence, and in very large measure, one less bureaucratic level to which to report.

XI. The Genome and the Environment; On Computers in Research; Future of Genetics

AM: Okay. Well, in terms of your own work, I seemed to notice -- and this may or may not be true -- that come about the eighties, your audience shifts, at least in terms of looking at your publications, from primarily genetics journals to more -- like the *Journal of Epidemiology,* epidemiological journals, public health journals, physical anthropology journals. I wanted to ask you what accounts for that. Was there a shift in who was interested in your kind of work? Was your research working? Or was the field of genetics changing?

WS: I think there was a little bit of all of it. I think you're right in sort of the timing of the event, but it seems to me the pivotal element was the decision to go back to Japan for two years in 1978 to 1980. I went back as one of the permanent directors. See, at that time, the way that the institution was structured, there were ten directors, four of whom were called permanent directors because they were in full-time residence either in Nagasaki or in Hiroshima. And there were six non-permanent, meaning in location, directors, who only met at the time when the board would meet, which was once, or at most twice a year.

The structure was such that there was a chairman, who was one of the Japanese two full-time directors. There was a vice-chairman, who was one of the American two full-time directors. There was a chief of research. Then there was a director who sort of covered the administrative side on the Japanese. The number two director on the U.S. side when I was out in '78 was also to be head of the Department of Epidemiology and Statistics. That was the biggest department in the institution. We had about a hundred and fifty employees. So I was one of the four -- the directors actually represented the Executive Committee, who was charged with the responsibility of the day-to-day management of things. But in addition, I actually ran this big joint department.

That wasn't where genetics was. Genetics was in the Department of Genetics, which was a separate one and not covered. Moreover, by that time, the genetics department was almost fully committed to biochemical genetics, to some lesser extent, to the effects of radiation on chromosomal abnormality.

Given the position that I had accepted, my sort of biological commitment was to cancer and to those other health effects that are associated with exposure to ionizing radiation, issues such as we didn't really know what the so-called LD60 is. I worked with my Japanese colleagues on that. Another part, too, was the whole element of changes in brain development as a consequence of exposure to ionizing radiation in utero. And, as I said, the whole added element of cancer, and by 1978 we added a growing list of the cancers that were responsible.

So much of what I was involved in in those two years had nothing to do with genetics. It really involved my background in statistics. The genetics was important to me in the sense that what we had done in the genetics program was epidemiology, whether you wanted to call it that or not. I mean, it really is. If epidemiology is the study of epidemics in populations, and an epidemic is loosely defined, then hell, that's what we had been doing from year one, and with considerable success, so I knew how to set up big studies and to do things like that.

Of course, it's always been one of the sorts of situations that if you've been committed while you were in Japan and choose to do so, those commitments carry on after you return. So I was still writing papers from here from work that was being done during those two years, and then in '86 I'm back in Japan again. From '78 on, I was there for a year or longer once every five years, then often for short periods of time, for a month, two months, as the case might be, with respect to specific issues.

More and more I saw myself being either asked or drafted into writing situations which reflected that shift, and in a sense, even extended the shift still further. When I might have gone back to writing something about genetics, I was writing for UNSCEAR, along with Ken [Kenneth M.] Weiss and [Francis] Fagnani on the risks associated with malignancy. Golly, the way the UNSCEAR operates, the documents that the committee issues under its name are actually written by consultants, with the guidance of the committee. So you write sort of, in a sense, at the sufferance of the committee. The committee meets once a year normally, and those meetings last for about a week.

In between those weeks, instructions are given to the consultants who are writing. In this case, Francis Fagnani and Ken and myself were doing this thing on the risk of malignancy following exposure, and we'd meet and defend what we'd written before the committee, and then they would assign new charges for us to do. Of course, part of this defense was a line-by-line reading, which was the most tedious part of the whole damn thing because, for half the committee, English is not their native language, and you've got these people who are complaining about your English construction when they don't know the language very well. (laughs) That's tedious.

But in the long run, it still was a rewarding thing, because everyone was really interested in the same objective, a good report. Now, how we were to get there differed a bit one to another. But the fact that you were committed to that, you see, given the way it operates, it takes UNSCEAR roughly four to five years cycle to complete its reports. It's true, only one week out of fifty-two each year are you actually there while the committee's going on, but you get a charge to write. By the time we finished with the one on malignancy, it's kind of misleading because of the format that UNSCEAR uses of small type, double column. If you had done it in a classical eight by eleven and a half, double spaced, you're talking about four to five hundred pages of writing. You're sort of moonlighting that with your other commitments.

So in a way, the genetics got -- perhaps I drifted more and more away from the kinds of genetics that I'd practiced up until 1978, in effect. I was still involved because

I'd started this program on diabetes in the valley, and whatnot, but at the earliest opportunity I turned those things over to colleagues here who are doing a fine job, and in some instances, a better job, I'm sure, than I could have done. Ken Weiss was involved in the cancer studies in the valley that we were doing. I had this one commitment, but I also had these colleagues with whom I was interacting. Though I had started the programs, when finally we got to the point where they were real publications, they were the ones who were analyzing and doing the bulk of the writing.

Maybe I might have been more reluctant to have accepted the trip back to Japan in '78 had I realized what the consequences were going to be, but I'm not sure that's true. Certainly, I didn't foresee at that time that going back in the context in which I elected to go back to Japan in '78 that it would have so profound an effect on the subsequent almost thirty years of my career.

AM: Okay. And then one last question in this area, and I'll just throw it out there. I don't know from what you just said how relevant it may be, but it's been a long time since the one gene, one disease kind of hypothesis has -- the paradigm pretty much has been eliminated. But it does seem that your obesity and diabetes studies, from the epidemiological point of view, really show this multifactorial, very complex genetic component to the disease. Yet, molecular genetics was -- at least as it was being publicly perceived -- was we're going to find the obesity gene, we're going to find the diabetic gene, and once we do, then we'll have a cure. So how far apart have the boundaries become, or what are the boundaries between these conceptions of what really genetic components of diseases -- how has that evolved?

WS: Well, I think increasingly, as we were discussing yesterday, I think, the diseases of maximum moment insofar as human health is concerned -- cancer, cardiovascular disease, diabetes, obesity, if you want to call it a disease, and increasingly I think the evidence is that it should be so classified -- are consequences of many genes with dissimilar contributions, and all of their contributions modulated to a greater or lesser extent by the kinds of environments in which they come to manifest themselves. So the notion that finding *a* gene for diabetes is going to rid the world of diabetes is false, and I think those who perpetuate that notion are being dishonest. But I know it happens, and I know it happens with people who wouldn't like that characterization that I've just made and who don't believe that they're doing that, in a sense.

Their argument, I presume, would be that if we could identify such a gene and could manage that gene, we would diminish the risk. We might not remove all probability of it occurring because there are these other factors, but indeed, we've come to realize that those diseases in particular are part of a very complex set of events. It's misleading of these fifty, sixty, seventy events that may be necessary to single out one as the causal one, it just isn't true. We need to understand how they interact if we're going to achieve any success, but that success is not going to be by suddenly doctoring a single gene. It's going to be by major lifestyle changes.

I would suspect that maybe -- it would be hard to identify which disorder and to whom credit should go for recognizing this, but I would think an important shift in paradigm would be the recognition twenty years or so ago that the origins of cardiovascular disease lie in childhood. I mean, that's when these events begin. To suddenly take a sixty-year-old and think you're going to get rid of all his or her problems is nonsense. They have been the accumulation of forty, fifty years of living, and living a particular way. So if we're to have major impact on these, we have to make far more fundamental changes, I think, in the way we live and most people have so far been prepared to make, or are just not yet properly attuned to the need to make those kinds of changes.

It's sort of interesting, you see, when we began our work in the valley on diabetes, one of the things we were trying to impress on the youngsters, to tell them what their risks were of diabetes, meant absolutely nothing. They weren't motivated by it one twit at all, because this was something that if it occurred to them at all, it wasn't going to occur for thirty or forty years, and that was so far down the line they weren't even thinking about it.

Our pitch was to -- *you* may not see the immediacy of this, but your parents and your uncles and aunts are in the ages where risk is significant. Yet, people have a natural inclination in some of these instances to try to deny the presence of disease, and *you* have to be -- meaning the child -- *you* have to be your family's monitor. You have to watch. If there's rapid weight gain, or there's a sore on the leg, or something like that, that doesn't heal properly, you have to be the one who encourages, demands, whatever the appropriate expression is, that your parents get attention. *You* are going to be the health witness, as it were, or spy, or whatever. That was something they could understand, and they would become -- I don't know how effectively they did it because that's very hard to measure as yet, but they did sense that there was a need for them to be attuned to what these events were, that it could be of moment to people that they loved.

Now, the interesting thing that we -- we go through all this business of slides because we were asked -- we gave this course every year at Rio Grande City and the other schools. The thing that attracted the students most was the table that gave weight per stature. (chuckles) And what the variants of this were. They were always interested in knowing whether -- they'd look down, and there'd be all kinds of expressions that would arise from the class when they find the line and the row and the column in which -- (laughs)

It's a very interesting issue. Just as addressing these problems is not only a matter of a lifetime of experience, it's also an experience which occurs in a community, that some of the things that need to be done can only be done by the communities. They can't be done by individuals. And these can almost seem to be trivial events. The woman who was the dietician for what was called the Rio Grande [Consolidated] Independent School District -- Texas has school districts, each one of which is ostensibly independent. It's _____ by its own internal rules and regulations, and the Rio Grande Independent School District governed the schools that served the community of Rio Grande City.

The woman who was the nutritionist for that school was herself a diabetic. She was one of those kinds of diabetics, by golly, she's going to cram this information down your throat one way or the other. I don't mean that she was arrogant or uncharitable or anything else. She just thought that somebody should have told *her* at a critical time. So she did two little things.

The Rio Grande Independent School District was located on what was called Fort Ringgold. See, after the war of 1848, the Mexican-American War, forts were built at hundred mile intervals from the Gulf all the way to the Pacific Ocean. Brown's Hill was

Fort Brown and the next one up the river was Ringgold, and then there was the one that got its name -- that is at Laredo, and all the way up. Well, after World War II, these forts were given to the cities, or communities in which they had occurred or had grown up around them, for a dollar. Of course, they got everything, all the buildings and all the rest, so this was a great place for Rio Grande City, which is where Fort Ringgold is, to suddenly have school facilities like they've never really had before. The whole thing was paid for.

I can't think of the woman's name right now, but she immediately instituted two things. One, sugar was taken off the tables in the cafeteria. There was no sugar on any table. If you wanted sugar, you had to ask for it and you would get that look of askance.

The second thing was that right across to the direct east from where the fort had been -- the fort was surrounded by _____, which hadn't been particularly maintained over the years. The school didn't see a lot of reason to do so, and probably didn't have the money. But immediately to the east of where the fort stood was a big shopping center, and the kids would go over there to get Coca Cola and things like that every time there would be a break or a recess or something like that. Well, there was a hole in the fence that facilitated that, so she immediately got that hole repaired. (laughs) So if they were to go over there to get a Coke, they had to go out the front gate, around that way, and all the way, and that deterred them.

Now, these were very simple solutions, but they must have had some kind of impact. How you'd measure it, I don't know. But her husband made other suggestions, which were equally simple but equally effective, I thought, or could be. He was a grocer, and he pointed out that certainly in the valley, and I suspect in most places, if you look at specific categories of food, the biggest profits are in produce, and that stores have, in the past, certainly not recognized that because usually the produce has been set at the very back end of the store. Well, by the time people got there, they've already spent most of their money, so they can't buy the thing which tends to be cheapest but in which the profit is larger. He said to move all that up to the front so they pick that up while they still think they've got X amount of money to spend. And the grocer's going to be better off because he makes more on that.

The second thing he said, which was a simple little business, he said, one day a week, in effect, have a lottery. Every cash register -- there's some kind of identification number on the receipt. Assign random numbers, and whoever happens to get that, their whole expense is waived, gratis of the store. He said it isn't going to amount to much money, a few hundred dollars at most, and it's going to bring people in, and they will most likely buy more of the things that were right up in front where they make the most profit.

Here are little things which you, as a diabetic, can't do anything about, but if the community is sensitive to this and really believes that the issue is an important one, these are small changes that don't dislocate anyone to any great extent, if at all, and yet represent a commitment to an issue where we need that kind of commitment, as far as I'm concerned. If we're talking about these diseases, it's not sufficient to just talk to the physician, nor the person with the disease. You really have to ask the community, how can you members participate?

The impact that we had was, we said we can show you that by the age of thirty-five, in that community -- or let me put it this way. Every individual who reaches the age of thirty-five in that community either is diabetic or has one chance in two of being the major support of a person with diabetes. We're not talking about something that represents one in a hundred people. This is real clout, because you're ripe at thirty-five, you're probably in the middle of your most productive years, and certainly when you as a person are most pivotal of your family needs, because the family's all young, not capable really of managing events on its own. So that's when you can least afford the loss of a person, either through themselves being too ill with diabetes to do anything about it, or having to devote a certain fraction of their resources to the support of someone who is ill. It really requires a totally different kind of approach to disease, in my view, and it --

AM: Than genetics can provide?

WS: Than genetics alone can provide, yes. I think we just have to be one of the players in the game, but we're not *the* player.

AM: Okay. One brief contextual question and then my last question, which will be a little bit broader in nature. You a couple of times mentioned one of the greatest impacts in the field has been the use of computers, and you've written on the use of computers. I just wanted to ask what was the context of this? To me, it seems like to population geneticists, it would be the greatest thing to be able to mechanize a lot of this. But was there some kind of reluctance within the field of human genetics to incorporate computers?

WS: I don't think so. I think it was just that the computers that were available -- when I started the game, the computer consisted of a mechanical desktop calculator, and that continued to be the major force for most of us for a long period of time. I don't have the statistics at my fingertips, but I think they are in that issue of the *American Journal of Human Genetics* that Charlie [Charles F.] Sing and I edited. You went from two machines, basically, Mark [I] at Harvard [University] and ENIAC [Electronic Numerical Integrator And Computer] at [University of] Pennsylvania in 1946 or '47 to by, let's say, 1955 those two had become maybe fifteen or twenty. That still isn't very many, when you thought of the enormous number of universities and places where computing was important.

They were extremely costly, because I know the first one of these machines with which I worked was MIDAC [Michigan Digital Automatic Computer] at [University of] Michigan. MIDAC literally had to be babysat twenty-four hours a day by an engineer. You really almost dared not turn the machine off because if you turned the machine off -- these were all vacuum tube-oriented machines -- and you turned it back on, you were going to lose a lot of those tubes. They'd just go. So as long as they were continued to be warranted, it was all right. (chuckles)

What constituted a chip then, or the equivalent of a chip, the engineer went in and he pulled out this relay. It took two hands to pull this damn thing out. And it would be maybe six inches wide and twenty-four inches deep. Probably weighed thirty pounds, with all the paraphernalia that was in it, and so on. And MIDAC at Michigan took up a whole building, just like ENIAC did. More of it was out at Willow Run because there wasn't that much space at the university for the darn thing that could be set aside separately.

You really didn't begin to move towards the kind of computing that we think of now, it seems to me -- the first one began probably with the card program electronic computer that IBM [International Business Machines] -- didn't even have a number, but then it became the 800 machine. Those were still not cheap. Our first dedicated machine, in the sense that the machine that we had in our department was an IBM 1120. That machine we got, I think it was 1964, it cost about a hundred and twenty thousand dollars, and a hundred and twenty thousand dollars then was a lot of money. I was probably getting, I don't know, fifteen or eighteen thousand dollars a year was my salary. That machine was on the order of eight years of my salary. And it had 8K of memory. Everything was programmed through punched cards.

Yet, my God, that was liberating. The whole analysis for the first big book on inbreeding was done on that machine. We could do things that we'd never been able to do before, with ease. You were no longer constrained, let's say, in the complexity of a regression where you had to refer to matrix. My old rule of thumb used to be, it would take the cube of the rank of the matrix, so if you were talking about a five-by-five, this was a hundred and twenty-five minutes of computing, two hours of solid work, based on the supposition that you made no errors, because of you made an error, it was all upset.

Now, the problem is that this goes from a hundred and twenty-five minutes, if you go to a six-by-six, now you're at two hundred and seventy-six minutes. (chuckles) Now people think nothing of inverting a forty-by-forty. And that has both its benefits and its disadvantages. I think the biggest disadvantage is it gives you opportunity to do a lot of shopping, now called data mining, without really knowing why you're doing what you're doing, other than that you have the capacity to do it for practically no expenditure, very little cost. Before, the expenditures were such that you thought long and hard about why you were going to do this and what you actually anticipated out of it.

I think it forced more thinking about the issue than is apt to occur now, and I think one of the consequences of what happens now is we get a lot of these false leads. The rough rule of thumb goes, if you do a hundred tests, five of them are going to be significant even in the absence of any reason for them. So we get all these things that, quotes, "*the* breakthrough," and then it takes another year to show that that was just a random event. There wasn't a damn thing there. But in the meantime, all hell has broken loose. Congress has probably written another law, which never gets corrected. So it isn't like it was then. But you trade off the one against the other. I guess that's just the nature of living.

Now, when I think of something like this, my gosh, this is a pretty potent machine. It isn't actually as good as the one I've got at home, but the one I've got at home, a sixty gigabyte memory, a gigabyte of -- no, sorry. One megabyte of core. It was like about two thousand dollars, and in time and computing capability, it makes that 1120 look even more primitive than the old Marchant [calculator] used to do. I know when I got a Frieden square root machine, which must have been about 1960, on a grant, and that darn thing cost thirteen hundred dollars, I thought I was in seventh heaven, because I no longer had to go through the old routine of the subtraction-addition method of determining a square root. You just put the number in and press the one key. It ground away for a while, and then -- but it was a big, cumbersome damn thing. It must have weighed about forty pounds.

AM: Okay. Well, my final question then is, in the very first session you had mentioned what encouraged you away from Drosophila genetics and helped you make the decision to pursue graduate studies in human genetics is you kind of felt like the genetics of Drosophila had become perfunctory, and you said then as much the way that human genetics is now. I just wanted to ask in a very kind of broad way how you feel -- you've been part of this progression in the field of human genetics, and how do you feel the field has developed?

WS: The way you couched it perhaps seems to overstate my position. I guess what I'm saying is that real events that change the direction, the scope of a whole science, come down the pipe relatively infrequently. Perhaps that's just as well in one sense because one such event, it may take a decade, two decades to mine, if it is to be done well. I think this, in a sense, occurred, for example, once [Joe Hin] Tijo and [Albert] Levan had established that there are only forty-six chromosomes in the human. And then when the recognition of the sex chromosomal abnormalities occurred, and [Charles R.] Ford and [Jerome] Lejeune showed that Down syndrome was another chromosomal defect, for almost a decade a lot of the excitement had to do with mining that technology, to looking at larger and larger numbers to determine how frequently these events occurred, were there still others that we hadn't previously recognized, which in fact was true.

But today, I think you would have to say that human cytogenetics is in a kind of quiescent stage in the sense that, yes, it's an important diagnostic tool, but in the sense of having the excitement that existed in 1960, it doesn't have that. To some extent, too, you could say the excitement of looking at isozymes, which roughly at the same time but on the biochemical sphere, was *the* excitement. That's not much now. What the heck? If you could sequence a gene, why should you be paying that much attention to its product, unless there was some other justification for it?

I guess what I'm saying is that, periodically, every science needs a new direction. In that period of time immediately before the new direction arises, things are pretty perfunctory. You've probably exploited most of what's going to be easily exploited, maybe most of what can be exploited at all. You're now just down searching the dregs, and you're using a technique that has already established its worth, but really it's not that novel any longer. Then bang, along comes something, like PCR [polymerase chain reaction]. My golly, that changed the whole business.

AM: Where's the next thing in human genetics going to come from?

WS: Golly, I don't really know. I expect it's going to come out of some laboratory enterprise, but not being that closely related to the laboratories any longer, I would hesitate to even guess. I suspect that's where it's going to come. Someone will find a new way to do something that opens opportunities that didn't exist before.

One of the problems, obviously, with linkage studies -- linkage studies really didn't begin to blossom until new technologies came along, so that you could get large numbers of events relatively inexpensively. When you were looking at one individual with a particular disease, and one blood group, or a dozen blood groups, the chances of finding linkage wasn't very good. Just assuming these are random events, the priori probability would be small, being able to select two genes and find that they were linked. Now, gosh, you're doing forty, fifty linkage, or even more, at a single crack. Now we've done a lot towards defining physical locations in the genome that wouldn't have existed before.

I think probably what's going to happen is something akin to that again, but where it will be and precisely what it will be, I can't even hazard a guess. (chuckles)

AM: Well, that's the end of my questions, and I just want to, as we do at the end of every interview, hand it over to you and ask you is there something that you'd like to talk about that we haven't mentioned.

WS: Golly, no. I've probably antagonized enough people already with my remarks that I would just as leave keep the few friends I still have. (laughs) Thanks very much. I've enjoyed it. I do look forward to the finished product.

AM: Okay. Well, thank you for letting us take your time.

[end of session]

[end of interview]