

AN ORAL HISTORY WITH DAVID BLACKWELL

Interviews conducted by Nadine Wilmot in 2002 and 2003
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It is recommended that this oral history be cited as follows:

David Blackwell, "An Oral History with David Blackwell,"
conducted by Nadine Wilmot in 2002 and 2003, Regional Oral
History Office, The Bancroft Library, University of California,
Berkeley, 2003.

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INTRODUCTION

It is a pleasure to be asked to write an introduction to David Blackwell's oral history. I suppose I owe this invitation to the fact that I have known him longer than anyone else in the Berkeley community. During World War II, I worked with Abe Girshick in a military research group, and it was Abe who had interested David in mathematical statistics after his graduate work with J. L. Doob and his early papers in probability. Through Girshick I came to know of David.

When we met in 1946, David was living in Washington D.C., where my parents also lived. My frequent visits to the city often included a meal at the Blackwell home, and I still remember in those early days driving with him from Washington to an Institute of Mathematical Statistics meeting at Cornell in 1946.

In January of 1947 I accepted a position at Stanford, where I started to organize a separate department of statistics. (The mathematics department at Stanford, unlike the one in Berkeley, did not want to include statistics.) Girshick joined the department, and David became a frequent short-term visitor and also spent the 1950-1951 academic year in residence. During the 1950s he spent several summers at the Rand Corporation, and became acquainted with Kenneth Arrow, another early star in statistics at Stanford.

His Berkeley connection was established when, in 1954, he came to Berkeley from Howard University as a visiting professor. The following year he accepted a position as Professor of Statistics. I had become chancellor at Berkeley in 1971, and this was a wonderful opportunity to renew our friendship.

David has been an ideal faculty member. First and foremost, he is an excellent classroom performer. His classes have been both rigorous and popular, and he is also listed as the supervisor of nearly fifty PhD dissertations. He has done his share of administrative and committee work: a term as department chair, Director of California Study Abroad Center, Faculty Research Lecturer and chair of the committee to select others, and member of the committee that recommended my own appointment as chancellor.

David's recognition is not limited to his work as a teacher. He has done fundamental work in game theory, Bayesian inference, and information theory. He was a principal developer of Bellman's dynamic programming ideas. He founded the theory of comparison of experiments, later developed by his colleague Lucien Le Cam to become one of the central tenets of mathematical statistics. With Arrow and Girshick he developed the backward induction method of solving sequential decision problems to solve the problem of determining Bayes solutions to such problems. He is the author of the classic book, *Theory of Games and Statistical Decisions*, along with some eighty papers in professional journals.

Although I do not want to wander too far from his academic and scholarly contributions, I cannot refrain from mentioning at this point that in many ways in his career was deeply influenced by the fact that he is an African American who had to overcome great educational and professional obstacles. He grew up in modest circumstances in Centralia, Illinois, a home town of railroad employees. It is to the credit of his family and his teachers that they recognized his remarkable ability and encouraged him to seek higher education. He was accepted by the University of Illinois, although there was some reluctance to have him pursue advanced study there. After obtaining his doctorate and spending a year at the Institute for Advanced Study, he applied to traditionally black universities, teaching first at Southern University and Clark, before going to Howard and only much later to Berkeley. In fact, the first proposal to hire him at Berkeley was defeated on racist grounds.

Despite the limitations that some tried to place on him, David has constantly forged ahead. I note that we are the same age, and he completed his PhD in 1941, the year that I graduated from college with all the privileges and encouragement that I accepted as a matter of course! Recognition has come to him over the years. He has served as president of the Institute of Mathematical Statistics, the Bernoulli Society, and the International Association for Statistics in the Physical Sciences. He has given a number of prestigious named lectures, and has a dozen honorary degrees. In 1965 he became the first African American to be elected to the National Academy of Sciences and was elected to the Academy of Arts and Sciences in 1968.

In recent years universities have actively sought diversity in faculty ranks, and being an African American is not as much of a disadvantage as it once was. But David's remarkable career was established at a time when prejudice ruled in academia. We salute him for his persistence and human courage as well as for his great scholarly achievements.

Albert H. Bowker
November 2003

INTERVIEW HISTORY

David Blackwell was born in 1919 in Centralia, Illinois. He went on to become a great mathematical thinker and a professor who is known for his dedication to teaching. Blackwell has made fundamental contributions to the areas of probability theory, mathematical statistics, set theory and logic, and of course, game theory, to name a few. His numerous achievements, honors, and affiliations are well documented, so I won't do that here. I would, however, refer the reader to the preface and body of *Statistics, Probability, and Game Theory: Papers in Honor of David Blackwell* (1996, Institute of Mathematical Sciences, Lecture Notes-Monograph Series, Volume 30) created by his friends and colleagues, Thomas S. Ferguson, Lloyd S. Shapley, and James B. McQueen. This volume is excellent for a concise overview of his professional contributions and some insight into his intellectual milieu and continuum for the layperson. There are also two excellent short interviews, one in *Mathematical People Profiles and Interviews* (1985, pages 17-32, eds. Donald J. Albers and G.L. Alexanderson, Contemporary Books, Inc.) and the other in the very first issue of *Statistical Science* (1986, vol. 1, pages 40-53) that were my first points of reference in preparing for this interview.

Who are you calling a first? When Dr. Blackwell came to UC Berkeley in 1954 after a decade at Howard University in Washington D.C., he became, we think, the first African American ladder rank faculty person systemwide. What does it mean to be "first," as David Blackwell was in so many arenas? As Dr. Blackwell himself says in his interview, "You have to be careful when you talk about firsts." This interview sheds some light on this much lauded but often mysterious experience.

It must be said here that this interview is a conversation between a theoretical mathematician and someone with little background or training in math and statistics. When I suggested to Dr. Blackwell at the outset that we bring in a second co-interviewer who was more fluent in game theory and mathematical statistics than I, he declined. This makes for an exchange between us that is sometimes productive and sometimes rudimentary, peppered with my almost comical misunderstandings when it comes to the subjects of game theory and statistics. I think, though, that Dr. Blackwell and I did the best we could given this barrier.

The interview began in spring of 2002 and concluded in January of 2003. In total, there were seven full interview sessions in addition to a series of three smaller recorded follow up conversations. The follow up conversations have been appended as a follow up interview and integrated into the body of the interview as noted. All interviews took place in Dr. Blackwell's office on the third floor of Evans Hall on UC Berkeley's campus. This interview was recorded on minidisc. Dr. Blackwell declined my proposal to video tape the interview. The transcript was reviewed by Dr. Blackwell for accuracy of names and dates.

David Blackwell's interview is the first in the African American Faculty and Senior Staff Oral History Project series, which explores the experiences of African American faculty and senior staff at UC Berkeley as part of the broader history of the University of California and its commitment to access and diversity. This series is grounded in the premise that higher education is one of the primary strategies for gaining social equality access to employment and income for historically disadvantaged communities. Moreover, the university, comprised of its students and faculty and administration, with all of its intellectual and financial resources operates as a critical touchstone in processes of systemic social change. Therefore, the university functions not simply as an educational institution, but also as a significant site of historic and future potential for imagining and crafting opportunity for ethnic and racial groups formerly excluded from higher education. This project recognizes that the University of California, as California's premier public educational institution, plays a significant role in the socio-economic mobility of all of California's residents. The story that we hope will emerge from this project is a story of California its people and one of its most important public institutions.

This interview was conducted under the auspices of the Regional Oral History Office. The Regional Oral History Office was established in 1954 to augment through recorded oral memoirs the library's materials on the history of California and the West. Copies of all interviews are available for research use in the Bancroft Library and in the UCLA Department of Special Collections. The office is under the direction of Richard Candida Smith, Director, and the administrative direction of Charles B. Faulhaber, James D. Hart Director of the Bancroft Library, University of California, Berkeley.

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December 2003

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INTERVIEW 1

APRIL 11, 2002

W: Well, to begin, I was looking at this photo from when you were a very young person \when you were in school.

B: Yes.

W: How old were you there?

B: Maybe eleven.

W: You were in sixth grade.

B: I'm not sure.

W: I'm looking at the expression on your face in that photo; it's just this wonderful expression. So, I guess to begin, I wanted to ask you, where were you born?

B: I was born in Centralia, Illinois. That's a small town in southern Illinois, about 12,000 people.

W: When were you born?

B: I was born in 1919. I grew up in Centralia. All my elementary and high school schooling was in Centralia.

W: What kind of a place was Centralia?

B: Most of the people worked for the Illinois Central Railroad. I believe, in fact, Centralia was named because it's on the Illinois Central Railroad. It's a junction point for the railroad; and my father and the fathers of most of my boyhood friends all worked for the Illinois Central Railroad. It really was a railroad town.

W: Can you tell me your parents' names?

B: My father's name was Grover Blackwell. My mother's name was Mabel Johnson Blackwell.

W: What did your father do for the railroad?

B: He was what's called a hostler. He took the engines from the engineer and put them into a roundhouse. Then he took them out of the roundhouse and delivered them to the engineer. I believe he had that job since he was eighteen years old, all his working life. He loved it.

W: Did you ever go to his place of work? Was it a place where young people would go?

B: Very often!

W: Yes?

B: Yes, he used to take me on rides in the steam locomotive, and even let me pull the throttle once or twice.

W: Wow! That must have been so exciting.

B: Yes.

W: Because that's like a fantasy for a young boy and then suddenly it's reality.

B: I still have a great affection for steam locomotives. So for me, they're just big and beautiful and wonderful. You know they're noisy, and they're smelly, and they shake the ground oh boy, it's an impressive thing to watch a steam locomotive.

W: Yes, I agree. Now can you tell me what your mom did?

B: She didn't have any outside activities, she was a housewife. She raised me and my two brothers and my sister, and looked after the house.

W: Wow, sounds like a big job in and of itself.

B: Well, she had a couple of rental properties that she managed. She was pretty active.

W: Were your parents active in any organizations?

B: My mother was active in a church. My father wasn't active in any organizations.

W: Did he have any professional affiliations as a hostler, or?

B: I don't think so. No, none that I remember. He liked his job so much that when the working day was reduced from seven days a week to six days a week, he used to go down to the shop on his day off just to make sure that his replacement was taking care of those steam locomotives.

W: Yes, making sure they were okay.

B: [laughing] That's right!

W: It sounded like he almost viewed them like they were, like, alive.

B: Oh yeah!

W: He took really good care of them.

B: Well, you know that each steam locomotive sounds different. My father and all the other railroad men could tell you the number of a steam locomotive just by listening to it. Sometimes, they could even tell you the name of the engineer who was driving it, because they would have different whistles and different sounds.

W: So when you say "your father and all the other railroad men," that was your friends' fathers as well?

B: Yeah.

W: Were they also hostlers?

B: No, there were only three hostlers. Each hostler worked an eight-hour shift, because there were only three hostlers. And a fourth man who would replace one of them if he was absent.

W: Sounds like a very finely tuned system.

B: Yes.

W: Did your family then know the other two hostlers?

B: Oh, very well! Yes.

W: Did they have children your age that you remember?

B: No.

W: The house you grew up in, were you in the same house from the time that you were very small until the time that you went away to school?

B: Yes. The house I was born in my parents continued to live in, until they died, always the same house.

W: What was that house like?

B: Oh, just an ordinary two-story house on what seems to me now like a lot of land. Our house extended from one street to the next street. So it was, maybe, on a lot, maybe, two hundred and fifty feet deep by, maybe, a hundred feet wide or something like that. So we had quite a large lot. But the house, as I say, was just an ordinary frame house.

W: Were there lots of trees and gardens on that lot?

B: Oh yeah, a tremendous number of trees. My mother's family owned the entire square block in which I lived. Now, that sounds a lot more impressive than it was because they were not by any means well off. But they just happened to own that whole square block. Next to our house, in a lot, on one side was my grandmother's garden and on the other side of my grandmother's garden was my Uncle Fred's lot. And then, going the other way were a couple of rental houses that my mother owned, and then a lot of vacant space where there was a garden. My uncle grew corn there. Now, as I say, that sounds like a lot. But we were just poor, working people. But my mother had inherited that land.

W: Sounds like you were surrounded by family, which is really nice.

B: Oh yes! And across the street my another uncle and aunt lived, and my grandmother lived upstairs across the street. That whole area was sort of known as "Johnson territory." My mother's maiden name was Johnson. And I remember, once my wife and I were visiting there and she started walking around and she ran into a fellow. And he said, "I know you, you're one of them Johnsons." [laughter]

W: Funny. So your mom's family, the Johnsons, where did they come from?

B: Her father was born in Cleveland, Ohio. No, in Columbus, Ohio. Then he became a school teacher in Weakley County, Tennessee. My grandmother was one of his pupils there, and they got married. Then he moved to Desoto, Missouri, and then he moved to Centralia, Illinois, and founded a grocery store there. It was quite successful, apparently, so he acquired a fair amount of property, I'd say a couple of square blocks all together.

W: And your father's family?

B: I don't know much about my father's family, [pause] They came from Weakley County, Tennessee, but I don't know very much about them at all.

W: Okay, do you know the story of how your parents met and came to be together in Centralia?

B: I know what my mother said. She claimed that my father came up to her one Sunday after church and claimed to have met her before, [laughter] So that's how they met.

W: Sounds like it was effective.

B: Well, yes.

W: A very effective approach.

B: Probably it's been used many, many times. "Haven't I met you somewhere?"

W: Yes, for sure, it's a good one. So was the neighborhood that you lived in, was it primarily African American, or was it mixed?

B: No, it was quite mixed. I would say primarily white. But a fair number of African Americans well, you see from this picture, there aren't many black students in that picture. But the neighborhood was somewhat blacker than this picture would give you the impression of. I'd say 30 percent black.

W: Did you find that mixture was reflected in your playmates and your social life?

B: No, my playmates were mostly black.

W: So, you went to school in Centralia public schools. And that's where, I assume, from what I've read, you really discovered that you love math. So I wanted to talk to you a little bit about what your education was like there, and who you remember there.

B: [pause] Oh, I just had a standard education. I got skipped a couple of times so I was in elementary school only six years instead of the usual eight. But it was in high school that I found something that I really liked. I really liked geometry. I had always been pretty good at math and somewhat interested in it, but geometry really excited me.

But I certainly never thought of making a career in mathematics. In fact, I had decided that I was going to be an elementary school teacher. The reason for that was rather simple. I grew up in the Depression, and getting a job was very important. When I was in high school, one of my father's friends promised me a teaching job if I would finish college. He was on the school board in Pulaski County in southern Illinois. So that just settled it for me right there. By the end of my freshman or sophomore year in high school, I already knew that I was going to go to college, and graduate, and then be an elementary school teacher.

W: Do you feel like the students you went to school with, your classmates, did they have similar aspirations?

B: Yes. In high school, two of the fellows who were in the same mathematics club with me, and with whom I worked quite a bit, went on to college. They both became electrical engineers. One of my black classmates playmates went on to college and he became a college history professor. Yes, there were several of the boys that I associated with had aspirations to go to college. As I've said, there was just never any doubt that I was going to go to college.

W: And that was the attitude of your family as well?

B: Oh yes.

W: From what I've read, it sounds like your family was very committed to your achieving at that level, to going to college.

B: There was just no question about it, that's right. I don't know how that happened. Even though neither of my parents had gone to college, they just assumed that their children were going to go to college.

W: Why do you think that was?

B: I don't know, but my parents friends shared the attitude: they wanted their children to go to college, and many of them did.

W: That was their commitment.

B: Yes.

W: Now, your father, he was educated to what level?

B: Oh, only to about the fourth grade, [pause] He was not an educated man, at all. He read very little, for instance. My mother dropped out of high school after her sophomore year. But she was reasonably well educated for her day. I got many letters from her over the years, she never misspelled a word, for example. She...

W: Well, there are some that say also that education was better. What you got in school was better than what you get now.

B: I think that's right, yes. Yes, it was certainly more rigorous in many ways.

W: So they both were like, you are going to school, definitely, no ifs, ands, or buts.

B: That's right.

W: Can I go back to the experience you said of being skipped several times? Now, what grades did you skip?

B: I think I spent only half a year in each of the first four grades, as I remember it, it was sort of like that.

W: That's just a blur, you moved really quickly.

B: Yes, they were basing it on my reading skills. Somehow, I learned to read very early. And so, if I could read at a fourth grade level, they put me in the fourth grade.

W: So, you were always in school, then, with people that were older than you?

B: That's right. That's right, [pause] Yes, a year or two older, yes.

W: Wow.

B: I'd never thought of that before, but it was true! [laughing]

W: Sometimes that's what this is for. It gives you an opportunity to think about things you never thought about before, it's very funny. So, that must have been a very interesting experience for you. To just, you know, always be with people who were older than you but those were your peers and who you socialized with. Did you socialize with your..

B: Yes.

W: That must have been really interesting, [pause] I was wondering, since you mentioned that geometry actually, I wanted to ask you a question about reading. Do you remember which books if you remember any books that absolutely transported you when you were in that elementary school time. It may be that books that transport you don't come until a little later, but I'm wondering what you remember?

B: I remember the first book I ever read, and that was before I went to school. It was called The Gold Hunters, by James Oliver Curwood. It was a book about the far north, and traveling with sled dogs over the snow. And I remember the second book that I tried to read. It was, I believe, by a woman writer named Gene Stratton Porter. And it was a novel about love and romance. And I didn't understand a bit of it! And I stopped reading it after the first few pages, [laughs] Because I was just not ready for that at all. But I always liked reading a lot. I read a lot of fairy stories when I was a kid. I read a lot of Tarzan books. I don't know that they influenced me especially, I just enjoyed reading them.

W: Yes, I mean, I think it's just when you're young that's an imaginary place that you go.

B: Yes.

W: So you were reading before you went to school, so did you learn at home?

B: I learned to read in my uncle's grocery store by looking at seed packages. There was this big stand that had seed packages on it. There would be a picture of tomatoes and a sign that said tomatoes. Well, I knew what tomatoes were, so that must say tomatoes. So just by looking at pictures like that, I learned to read.

W: You learned what all the letters sounded like, and ?

B: Well, sure, if you get enough experiences like that you just sort of figure it out.

W: I bet seed package designers had no idea what they were doing with these useful educational tools, so that's great. Did you spend a lot of time in your uncle's grocery store?

B: Oh, yes. Yes, as I say, that whole area sort of belonged to our family. So, I spent about as much time across the street as I did at home.

W: So, you mentioned that geometry was the place where it really was exciting for you academically. Do you remember who your geometry teacher was?

B: Oh, certainly. Her name was Luther, Caroline Luther. She also taught German as well as geometry, and she sort of kept up with me after I left high school. She invited me to come and visit her a couple of times after I was in college. And two of the other boys that I worked with in high school were also in that same geometry class, and she paid attention to them, too. There was another high school math teacher that influenced me a lot. His name was Huck, Raymond Huck. There was a mathematics magazine that was partly for high school mathematics students, and it had a problems section. Mr. Huck encouraged us to try to solve the problems there, and sometimes we did. I solved a couple of them, and Mr. Huck wrote up my solution and mailed it into the mathematics magazine. And a couple of times, my name appeared in the magazine and once even my solution really Mr. Huck's write-up of my solution appeared in the magazine. I was very pleased with that.

W: Really exciting, I mean, you were in high school!

B: Yes.

W: Were there any other teachers that you can think of besides Mr. Huck and ?

B: Oh, I can think of several other teachers, yes. There was an English teacher, Grace Seiler, who encouraged me to go to college. In fact, she wanted me to go to her alma mater, which was DePauw University in Greencastle, Indiana. But that was a little far away for me. I got a chance to go to [University of] Illinois, so I chose Illinois. She was very interested in me. I say, "they were interested in me;" they had a general interest in the students, not just me particularly. They kept up with students after they left.

W: In general, did you feel like you really had access to good education and exciting, caring teachers? It sounds like maybe you did.

B: I had an absolutely fabulous high school education. When I went to college, I was a semester up on most of the college students there. There were things that they studied in college algebra I had already studied in high school. Things that they studied in freshman English I had already studied in high school. High school was harder for me than college!

W: Oh, my goodness. Do you remember the process of choosing, or applying, to college and thinking about the different places you could possibly go?

B: There was never any doubt in my mind, I wanted to go to the University of Illinois, and I intended to go there.

W: Did you apply anywhere else?

B: At the urging of this high school teacher, I applied to DePauw University in Greencastle. But, you see, in those days, any graduate of a high school in the state of Illinois was eligible for admission to the University of Illinois. There wasn't the big hassles, all the "Can I get admitted?" that there is now. There was no question about it, if you were a high school graduate, you were in. So, application was just a matter of saying that I'd like to go.

W: Wow, that is different.

B: Of course a fair number of students flunked out, too.

W: Of college or of high school?

B: Of college. In the fraternity that I was in, in college, about a third of the freshmen would flunk out. And that was not atypical.

W: So you applied and were accepted to the University of Champaign-Urbana, Illinois, and that's where you went. Do you remember what it was like arriving on that campus the first day? And did your parents come with you?

B: My parents did not come with me. Certainly, I remember very well what it was like. I came on the train, with suitcase, walked a mile and a half with my suitcase from the train station to the campus. I was heading for some university office where I was going to inquire about housing. A fellow saw me from across the street and said, "Hey,

young fellow, come here, come here!" And I walked across the street, it turned out that he was a member of a black fraternity there and he invited me to come and live in that black fraternity house. And, I did. So that was a big first day for me. That was an important encounter.

W: How far was your university from your home town?

B: A hundred miles.

W: And how long was that on the train?

B: Oh, three hours, something like that. Two or three hours.

W: What was that train ride like?

B: Don't you know what a train ride is like? It was no different from trains now. Well, of course, a steam locomotive is different than a diesel but since my father worked for Illinois Central Railroad, I had a pass. So, I never had to pay anything to ride on the train.

W: So, you met this very important person on your first walk outside the train station with one suitcase in hand?

B: Yes.

W: Wow! And can I ask you, I've been wanting to ask you, this fraternity, which one was it?

B: Alpha Phi Alpha. Ever hear of that?

W: Yes.

B: Good. Yes, I lived in the Alpha house all the time that I was in school at Illinois.

W: So that's...

B: Six years.

W: And when did you pledge?

B: Right away.

W: Were any of your fraternity brothers also involved in math and statistics?

B: No. I was the only math major in my fraternity house.

W: Do you remember what areas your fraternity brothers were involved in academically?

B: Vaguely. Three of them were philosophy majors. You wouldn't find that nowadays. But, in those days, jobs were scarce. So, you didn't especially choose your major with reference to getting a job. You were more inclined to choose as your major something you'd like. Well, these guys chose philosophy, some chose physical education, some chose pre-law. My closest friend chose bacteriology. I don't know what his original plans were in choosing bacteriology, but he actually went on to get a PhD in bacteriology. But when he started, he wasn't thinking of getting a PhD any more than I was thinking of getting a PhD.

W: What was his name?

B: Arthur Webb. He was from Chicago. Practically all the people in my fraternity house were from Chicago.

W: So maybe you were the only one from a small town a hundred miles away?

B: That's right, yes.

W: Can you tell me, as an organization, your fraternity what kinds of things did you do as an organization on campus?

B: Mostly we supported each other. For instance, there was a big file of essays in our fraternity house on various subjects. And each essay had a history. Some of them were "A" essays, and some of them were "B" essays [laughter] That is, essays that had been graded "A," some graded "B," and some graded "C." And students would go and pick one of those essays and copy it if it was appropriate for their assignment. If you'd been doing "C" work, you didn't pick an "A" essay, you picked a "B" essay.

W: Yes, so you supported each other.

B: Yes, and we supported each other with jobs. We, the Alphas, owned a certain sorority, the Pi Beta Phi sorority in this sense; all the waiters in the Pi Beta Phi sorority were Alphas. And when one Alpha would graduate, he would turn over his job as a waiter in the Pi Phi house to another Alpha. And the Kappas, another black fraternity, had a fraternity that they "owned" in that sense. They had all the waiting jobs in that fraternity, and passed them down from one to another. And there were various other jobs that got passed down that way. So, hi that sense, the Alphas helped each other through school.

W: Do you know how now they have work-study jobs, work-study jobs for people who come to college?

B: Yes.

W: At that time, were there also positions available on campus, too?

B: Yes, and that was very important for me and many of the other students. It was called NYA, National Youth Authority. That was the equivalent for college students of the WPA, the Works Progress Administration. These were all Roosevelt's ideas for helping people through the Depression. That made a big difference for me and many other students.

W: Do you remember what kinds of jobs you held during that time?

B: Certainly. My NYA job was working in an entomology laboratory. And the one thing I had to do when there was nothing else to do was go through all the vials of bugs and keep them filled with alcohol. The bugs were preserved in alcohol, but the alcohol would evaporate, so it was my job to go through and fill those vials with alcohol. Some of the NYA students would drink some of the alcohol, [chuckles]

W: That's very high-proof alcohol.

B: I think so, yes, I think it was 180 proof or something like that

W: Yes, that's in almost medicinal levels, that's not for internal consumption.

B: Well.

W: Oh my goodness. Well, do you remember, were other members of your fraternity also employed through the NYA jobs?

B: Oh yes. Yes, NYA helped a lot of us get through college.

W: And socially, what kinds of things did your fraternity do?

B: Oh. Our fraternity sponsored a used to give a dance or a party a couple of times a year, and we would play basketball against the Kappas once a year. But that was about it.

W: Did you continue to act as a welcoming committee for new freshmen?

B: Oh, certainly. Every fall we would go down and meet the train coming from Chicago and try to spot incoming freshmen who weren't sure where they were going to live. And we would try to get them to come and live at the Alpha house and the Kappas would try to come and get them to live at the Kappa house.

W: So what was that like? How did the Kappas and the Alphas try to bring people into the organization?

B: Well, as I say, you would go down and meet the trains. And if a black fellow was getting off and looked bewildered, you'd go up to him. "May I help you?"

W: Yes, so it was a very critical first moment depending if he met a Kappa or an Alpha first.

B: That's right. Of course, most of them were from Chicago and they already knew where they were going.

W: So, did the Kappas and the Alphas work together around different projects?

B: I don't remember anything that we did together.

W: Well, you were there six years and you stayed in the Alpha house for six years. Were you a leader in the fraternity?

B: Not especially. I was in the Alpha house for six years, but it couldn't stay the same house all that time. We were there for three years, and then we were forced out and didn't have a house for one year, and then we got a new house for the last two years or something like that.

W: How did this happen?

B: Our lease expired. We were leasing only the second floor of a two-story building, and the owner found a new tenant for the entire building.

W: Okay. So, you arrive on campus and you find a community and a place to live all at once. And you started going to classes?

B: Well, yeah!

W: And how was that? How were your classes when you first started there?

B: Well, I remember one significant thing that happened. You had to take a foreign language, and I had studied Spanish in high school and I planned to continue that. But one of my fraternity brothers told me this was within two or three days after I got there that I should take German instead of Spanish. Because, he said, "If you're pretty good at mathematics, you may want to go on for a PhD. And Spanish won't help you, but you'll be required to read German, so you should take German." That was extraordinarily good advice from just one of my fraternity brothers who was two or three years ahead of me and could look that far ahead.

W: German is important for math?

B: It was very important in those days for mathematics. You had to pass exams in French and German in the PhD, and many of the books and papers you studied were in German.

W: So he helped you make some good choices?

B: He certainly helped me make that very good choice, yes.

W: So you went to college and you were planning on being an elementary school teacher. How did you choose your course load? Did you choose education classes or ?

B: No, education courses had a bad reputation in those days. They were joke courses and well, you put off taking them as long as you could. So I didn't say, "I'm not going to take them," I just kept postponing it. And I never did get around to it. [chuckles] No, I knew that in order to be an elementary school teacher I would have to take them. And, as I said, I planned to take them, but not right now.

W: And instead, you started getting into mathematics and statistics courses?

B: I took every undergraduate mathematics course that was offered. Just because you had to major in something, mathematics was easy, I liked it so that was that.

W: And did you take any of your literature classes while you were in undergraduate school?

B: Oh, yes, I took whatever you were required to take.

W: Now, you're here in college with college being easier for you than high school. And what was that like, being surrounded by people who were a little bit older than you but who were challenged academically by something that was, for you, very easy?

B: [pause] I don't remember very much about that, especially. Academically, I spent more time with the other math majors than I did with my fraternity brothers.

W: Okay.

B: And, of course, I was challenged just as much as they were.

W: Okay, my question just came from when we were talking earlier about high school and about how you found college to be easier and I was just coming from that perspective. But, okay, that kind of gets into the question of socially, what was college like for you? Like, were you one of the people who was really socializing? Or you were, were you more academically focused?

B: Well, I was almost completely academically focused and economically focused. I had to make a living! I had to after my freshman year, I got practically no support from my parents. I found out that my father had been borrowing money to send me to college. So, I told him that I could manage, and I did. But that meant that supporting myself was an important a big part of my activity.

W: Did you do that primarily through the entomology laboratory job?

B: Primarily through that, and meal jobs washing dishes and waiting tables at the Pi Phi house.

W: Did the institution help you at all, did your university help you at all, financially?

B: Well, of course the entomology lab job was a university job.

W: Were you definitely responsible for paying your full tuition?

B: Oh! I went there on a scholarship. And the scholarship was thirty-five dollars a semester. That was the tuition. It sounds absurdly low, but it was something in those days.

W: How did you win that scholarship?

B: By taking an examination. The state of Illinois, at that time, offered one scholarship in each county. There were 102 counties in Illinois. You took an examination and whoever scored the highest on that examination got the scholarship. So I won the Marion County scholarship that year in high school.

W: Yes, that's an important thing. And that reduced tuition to thirty-five dollars per semester.

B: That paid the tuition which was thirty-five dollars a semester.

W: Okay, but you were responsible for all of your..

B: Living expenses and textbooks and so on, sure.

W: So that was you, okay. When you were working with the entomology department, what kind of job was that? Was that a place you felt like you were learning anything while you were there? Or was it just work?

B: No! Just work, friendly atmosphere, but just work.

W: Also, while you were washing dishes, was that an experience where you worked with other people who you in a friendly atmosphere?

B: I worked in a friendly atmosphere, with my fraternity brothers . And working for the woman who ran the kitchen.

W: So let's move more into the academic area.

B: Okay.

W: If you can tell me what it was like encountering this community of mathematicians. You said that you mostly were, kind of, moving in a math circle at that point, socially. So, if you can talk a little bit about who your peers were.

B: Well, it's not easy to remember. There was one fellow named Abe Charnes that I learned a lot from. We took several classes together. But, somehow, he always knew more than I did. So, I learned a lot from talking to him. It's all sort of vague in my mind. There was a room for undergraduates in the mathematics library. And we used to go into that room and discuss problems with each other. But, as I say, I have the idea that I learned more from them than they did from me. Maybe they'd say that they learned something from me. But I know that I learned a lot from them.

W: You mean from the other undergraduates?

B: The other undergraduates, yes. [pause]

I remember that this is going to be a little technical Abe showed me how to negate a mathematical statement by changing every "for every" sign to a "there exists" sign, and every "there exists" sign to a "for every" sign and then negating the inner proposition. I had been doing that informally, but sometimes, in a complicated thing I would make a mistake. But he showed me a way to do it where you never made any mistakes. This general rule and various things like that I learned from him. I saw him a few years ago, by the way, he is a retired mathematics professor at the University of Texas, I think it is.

W: So he went on to be a professor?

B: Yes. Also he, as an undergraduate student, had started accumulating a mathematics library. And that really impressed me.

W: Did you go and start building your own as well?

B: In a weak sort of way, but I sort of remembered that. As I say, though, at that time, I hadn't really determined that mathematics was my career. It's just that I liked it.

W: If you were to name a turning point, then, where you realized that you were going to get your PhD?

B: Let me tell you sort of what happened. I had a four-year scholarship. I completed my undergraduate work in three years. So I decided to use that fourth year of my scholarship to go on and get a master's degree. Then, while I was working on that master's degree, I was encouraged to apply for a fellowship or a teaching assistantship to go on for a PhD. So my focus was gradually shifting and I did apply. And I got a fellowship. So then, that settled it. Then, I was going to go on for a PhD.

But let me tell you something very funny that happened. See, there were two kinds of awards, fellowships and teaching assistantships. They paid the same amount of money, but for a fellowship you didn't have to do any teaching. So they were the preferred award. And there were maybe three fellowships and twenty teaching assistantships every year. But you submitted a single application. We all submitted applications. One of the other graduate students told me that I was going to get one of the fellowships. I said, "How do you know that?" He said,

"Well, you're good enough to be supported and they're not going to put you in a classroom!" Because I was black, of course. He was right, sure enough I did get one of the three fellowships. And I'm sure that a partial consideration was, "Well, we need to support this fellow, and we can't put him in a classroom, so let's give him a fellowship."

W: Whoa.

B: So it was an actual advantage to be black.

W: Yes, I also wonder, once you had a fellowship, did that change your life on campus in terms of working to support yourself economically or was it still very much an onward struggle?

B: Yes, after I got the fellowship I didn't have to work any more. Yes, that's right.

W: Okay, so that must have just left a lot more room for the intellectual life on campus?

B: Somewhat more, yes. Yes, at that time, of course, all my work was in mathematics.

W: I guess I want to ask you some questions about the institution. Since you were there for six years, I want to know more about the University of Illinois, at Champaign-Urbana.

B: Okay.

W: Was it a large student body and was it diverse?

B: Would you say that again?

W: Sure, I did that thing where I talk too softly, huh?

B: Yes.

W: First I wanted to ask you if this was a large student body?

B: Oh, I believe about 12,000, which is small compared to UC Berkeley.

W: Yes, but maybe it seemed larger, larger to you than where you'd come from.

B: Oh, much larger, yes, certainly.

W: And was this a if you remember was this a diverse student body? Was it primarily white, or were there people from other backgrounds there as well?

B: Well, it was overwhelmingly white. There were about a hundred black students out of 12,000. So, yes. All the black students knew each other; it was that small. And there were rather few foreign students or rather few non-white students. Yes, it was just overwhelmingly white.

W: And as far as your instructors and professors and also the administration, did you have the opportunity to interact with administrative people very frequently in terms of ?

B: Not very much, no.

W: Okay.

B: But the interactions I did have were friendly, [pause] I interacted much more with students than I did with professors or administrators.

W: Well, I think I'd like to spend a lot of time on that the next time we get together.

B: Okay.

W: I think that for today, I would like to end. Is that okay?

B: Yes, sure.

[end of interview]

INTERVIEW 2

APRIL 18, 2002

W: It's the morning of April 18. I'm here with Dr. David Blackwell. Dr. Blackwell, I had a couple of questions to follow up from our last conversation.

B: Okay.

W: Our last conversation, we focused mostly on your early childhood and there were some things that you raised that made me think of more questions. So I wanted to go back to them.

B: Sure.

W: The first one was really about your father again, and his work as a hostler.

B: Yes.

W: Which, by the way, when I found it in your bio-bib, I had to go to the dictionary. And it said two things; one of the things it said was taking care of engines, and the other one it said was an innkeeper. So, it was good when you clarified for me what that was.

B: So he was an innkeeper for engines, for steam locomotives, [humorously]

W: Right, I was wondering, if there are three people who had shifts, which shift did your father work, or did it change a lot?

B: It varied. The three shifts were three to eleven in the afternoon and evening, eleven to seven overnight, and seven a.m. to three p.m. Those were the shifts. And my father had all three of them at one time or another.

W: Okay, and were the other hostlers also African American?

B: Yes, yes, they were all black. I think that the Illinois Central Railroad had very little mixing in this sense: some jobs were black and some jobs were white, and you just didn't go from one to the other.

W: Did your father share with you how he came to have that job, how he came to become a hostler?

B: No, I don't think there was ever any idea that I might become a hostler. There probably was an idea that my younger brother might become one. I think that he probably wanted to do that.

W: My question was more around did your father was he ever anything else besides a hostler? Did he have another career besides that?

B: No, no.

W: And you said your younger brother was perhaps more interested in becoming a hostler?

B: Yes, and he did get a job working for the Illinois Central Railroad when he was about sixteen or seventeen, and worked there all his working life.

W: Can you tell me his name?

B: Yes, everybody called him Skeet. His real name is Johnson Wesley.

W: Can you tell me the names of your other siblings?

B: Yes, my other brother is named Joe. And my sister is named Elizabeth Louella.

W: So it's Joe, Skeet, and Elizabeth Louella.

B: Yes, Joe is a lawyer in Cleveland. And "Betty Lou," as we call her, is an elementary school teacher in Chicago.

W: And, I had a question also, just to follow up on our last conversation, about you'd mentioned that your mother went to church. And I was wondering, first, which denomination? And was your whole family going to church or was it just your mom?

B: Roughly speaking, the women went to church and the men didn't, [laughs] I don't think my father ever went to church. Nor did my uncles. But my mother did, and my aunt did, and my grandmother did.

W: Which denomination was it?

B: Baptist.

W: And I wanted to ask you also your grandparents names.

B: My mother's father's name was David S. Johnson. My mother's mother's name was Elizabeth Beckett. My father's father's name was Dabney Blackwell, and my father's mother's name was Ida White Blackwell.

W: Was your family unique in Centralia as a black family owning land? Were there other families who were owning land?

B: Many black families owned their own homes. I don't remember other black families owning a substantial amount of land other than their homes.

W: It strikes me that in Centralia, you lived independent of white people as well as very closely with them in a highly integrated environment. Does that sound correct? Does that sound true?

B: I'm not sure what you mean by "independent" of white people.

W: It seemed to me that, because you lived in something like a family compound, like you were surrounded by your family. And also because I read in an interview with you that you said your grandfather didn't send your uncle to school because he didn't want him to be...

B: He didn't want him to be discriminated against, that's right. But, of course, that was in the generation before mine. I have no doubt that I was discriminated against in certain ways; but I never felt it.

W: Did you have a sense that your family was aware of different social-political movements like Marcus Garvey's UNIA, or W.E.B. DuBois Niagara movement?

B: No, my family was not very political. I believe that they routinely voted Republican, but not out of particularly strong political conviction. Practically everybody that I grew up with was Republican. In fact, at election time, the Republicans used to come around and pay everybody five dollars to go and vote. Because they knew they would vote Republican if they voted! [laughs]

W: Also during the twenties and thirties when you were coming up, I believe there was a sense of and it was by no means mainstream, but there was a growing sense of Pan-Africanism, meaning that people were thinking in terms of the Caribbean, there was a growing sense of that. I was wondering, was that a sense that was reflected at all in your community when you were growing up?

B: No, I suspect that was more in big cities like Chicago and St. Louis, no, we didn't feel that at all.

W: You've mentioned several times the effects of the Depression in shaping your choices and decisions, and just your outlook. And I was wondering if you could elaborate more on that and also if you can tell me about how your family fared with the Depression.

B: Well, my immediate family was lucky, in that my father held on to his job throughout the Depression. My uncles didn't do so well. They had a grocery store that went bankrupt in the Depression and they had to find other work.

So, we were not directly affected by it. That is, my father and mother and I, and my brothers were not directly affected by it. But many people around us were.

W: Do you remember what kind of work your uncles found?

B: Yes, one of them went to St. Louis and got a job in a packing house, I believe. And, another one went to Chicago and then somewhere in Wisconsin. And I don't remember exactly what kind of job he got, but they had to leave Centralia to find work.

W: And the uncle who ran the grocery store where you used to spend a lot of time, can you tell me his name?

B: Yes, his name was David S. Johnson. He was named for his father.

W: For much of the Depression, because you went to school in 1935, you were in school.

B: Yes.

W: And I was wondering, for your sisters and brothers, were they working during that time? Or were they much younger than you? I guess I should start by asking if you were oldest.

B: Oh yes, yes, they were younger than I. They were in elementary and high school during the Depression.

W: When we spoke last time, you told me there were a hundred black students in a campus of 12,000. And I wanted to ask you, did you have the sense that you were one of the first groups of black students on campus?

B: I'm not sure exactly what the question means, but I'll say this: I had the impression that there had been about a hundred black students on campus for many years. It's just a changing group; but I think it stayed about the same size. My fraternity house had been there for a good many years and so had the Kappa house.

W: As far as being part of a group of a hundred people on a campus of 12,000, what did that mean for you in terms of how you approached your work academically and your campus life?

B: I'm not sure exactly what that means. There we were, mostly living in two black fraternity houses, one black sorority house, and a few other houses, and we all knew each other.

W: You mentioned the story of arriving on the train and not knowing anyone and there was a fraternity brother to greet you. And that was one reason why you became a member of the Alphas. There were probably other reasons that you joined the Alphas and I wanted to ask you about them, because the Alphas have a very specific historical legacy and tradition. Were there other reasons?

B: No, there was no more to it than that. I had never heard of black fraternities until this fellow called me across the street. And I didn't know where I was going to live and he suggested I live there and that was great with me. It was right across the street from the campus.

W: When you think of pledging processes today, do you feel like it was much more rigorous or less rigorous than what people do today?

B: Looking back on it now, the whole process looks a little ridiculous. And, of course, there would be no paddling nowadays. Do you know about paddling? [laughs] That was just routine in those days.

W: I suspect it may still be routine.

B: Well, I'm not proud of my fraternity if it still is routine. I don't remember very much about it.

W: Did you provide social services to the community outside of campus, as an Alpha?

B: No. We were just busy trying to make it and trying to have a good time and I got to know a fair amount, indirectly, about Chicago, because, as I said before, most of my fraternity brothers were from Chicago. One of the

things that surprised me was how corrupt a city Chicago was and how they, essentially, accepted this. For instance, certain jobs had a price. If you wanted to get a job in the Chicago school system, that would cost you a certain amount of money. If you wanted to be a policeman, that would cost you a certain amount of money. They simply accepted this as the way things were.

W: That was very different from where you were from.

B: As far as I know, yes, I certainly never heard of anything like that in Centralia.

W: Did everyone who was in your fraternity live in that house with you? Or were there people who were outside of the house as well?

B: Most of them lived in the house. A few of them lived at home in Champaign-Urbana, but most of them lived in the fraternity house, yes.

W: Do you remember having discussions, together as a group, about the university, and your experience there?

B: We certainly had bull sessions, but I don't remember what they were about. I remember more that they were heated than what they were about, [laughs]

W: What's a "bull session?"

B: Oh, just where a group of fellows get together, maybe in somebody's bedroom, and just start arguing about this and that. You learn a lot that way, though.

W: Did you also talk about politics? Just the government and what was going on in the world?

B: I suspect that some of the sessions were about that, but I was never interested.

W: Can you tell me the name of the black sorority that was on campus as well? You said there was one sorority.

B: Yes, Alpha Kappa Alpha.

W: How often did you get on a train and go visit your family? While you were at graduate school.

B: Just about every week. I think I told you, that was how I got my laundry done a lot of the time. I didn't tell you that? Every Wednesday, I would go down and get on a train and ride to Centralia and deliver my dirty laundry and pick up some clean laundry, and then get on the train and come back. So, I spent virtually every Wednesday night on the train.

W: And did your family come visit you on campus?

B: From time to time, yes. There used to be a state high school track meet and my brother would come up every year for that.

W: While you were at Urbana-Champaign, were you involved with any academic committees or groups, other than your fraternity?

B: Yes, I belonged to the mathematics club there.

W: I understand you were the president.

B: I think so, but I didn't have any duties. I suspect it was really run by the faculty sponsor.

W: Who was that?

B: I think it was Professor Bailey, but I'm not sure.

W: Your peers in the math department, did you have the sense that they were also having multiple jobs in the way that you did?

B: Some, yes, but mostly, no. I have the impression they were mostly supported by their parents.

W: And were they all from the community surrounding the university as well? And had they all got a similar education as you?

B: Well, let's see. One was from Chicago, one was from Kansas City, Missouri. And, I don't know where the others were from. No, we were just all undergraduate math majors there together. And then, later, in graduate school, of course, the graduate students in mathematics came from all over.

W: Did you have any guidance in selecting your courses? Other than the guidance that was offered to you by your fraternity brothers?

B: Yeah, let's see. I don't think so. As I told you, as an undergraduate I took every mathematics course that was available to me. And as a graduate student, the program was pretty well set. So, I didn't have many options.

W: So, did you need assistance in navigating the courses and requirements? And if you had that need, was there someone there to help you?

B: I discussed things with the other students, as I've mentioned. I got much more help from students than from faculty in choosing courses and in learning things.

W: Did you have an academic advisor prior to finding Joseph Doob?

B: I don't think so. I'm sure there was someone who signed the programs of the students, but I think that was a proforma thing. I think you just showed up saying what you wanted to take, and he checked if it was reasonable and it allowed you to make reasonable progress towards graduation, he'd sign it.

W: Do you remember when Jerzy Neyman came to address the University of Illinois in 1937?

B: I remember it very well, yes.

W: Can you tell me about that?

B: One of my mathematics teachers at Illinois was Professor Crathorne, C-R-A-T-H-O-R-N-E. He this is not quite on the subject, but I'll get there. He was very helpful to me. I took calculus from him, and I took a course in probability from him. And he gave me a couple of jobs. One of the jobs was bookkeeping for him. He was treasurer of some church there. So, it was my job to keep books for him. Another job was going through an algebra book that he was writing and doing all the problems and making sure the answers were correct.

And, of course, I appreciated the jobs and he paid me a certain amount for them. But looking back on it, I realized that that was just his way of helping me. He was treasurer of the church; he didn't need me to keep books. And, well, the other job, checking all the answers, was of some use but what he really wanted to do was to help me.

And it was he who invited Jerzy Neyman to come and speak. So, he told his class, "Be sure and be there." And he told me, in particular to be sure and be there. And, at some point either before or after the lecture, Professor Crathorne introduced me to Neyman. And I hadn't understood much of what Neyman was saying but, in any case, I was honored to be introduced to this very distinguished man. It never occurred to me that I would have any later contact with him, but that incident stuck in my mind.

W: It sounds like Crathorne was a good mentor. It sounds like he was he was thinking about you.

B: Yes he was. I didn't appreciate it at that time. It was only when I look back on it that I realize how he helped me.

W: And Neyman came and spoke.

B: Yes, he spoke about his work and what was going on in statistics. As I say, I understood very, very little of it. But that was, I believe, before Neyman came to Berkeley.

W: How did you come to work with Joseph Doob as your academic advisor?

B: There was a student there named Don Kibbey, and I was talking to him one day. And he asked me who I was going to work with. And I said that I didn't know but it was time I was deciding. And he simply said, "Why don't you work with Joe Doob? I'm working with him. He's a nice guy and he might take you." So, I trusted Don Kibbey, and I just went and asked Joe Doob if I could work with him and he said yes. And that was it. It wasn't complicated at all.

W: Did you think he knew about you as a student? Was it a small enough department that he knew who you were and knew a little bit about your work?

B: He probably knew something about me, yes. It was a small enough department that all the professors knew or knew about all the graduate students, yes.

W: So, as your advisor, what was his job?

B: Why, his job was to give me a thesis topic and guide me in writing it.

W: And is that what your working relationship was like?

B: Yes, the first thing he did was to give me some papers to read. These were papers in measure theory, and most of them were in German. Paul Halmos was there, he had been a student of Joe Doob's, and he had read these same papers a year or two before, and was very anxious to talk about them. And I wanted to learn about them. So, I talked to Paul Halmos and he essentially gave me a course in measure theory. And, later, he wrote a book on measure theory. I think I received the first version of Paul Halmos book on measure theory.

W: I want to back up a little bit and ask you a little more about Joseph Doob's background and special interest area in terms of academics. What was his background and training?

B: He got his degree, his PhD, at Harvard in 1932. But his thesis was in analytic functions that had very little to do had nothing to do with probability theory. He learned probability entirely on his own and really helped to lay the foundations of the subject. I was very lucky to have him as an advisor. The things that he told me to read and the things that he wrote were just fundamental in the future of the subject of probability.

W: When you think of what he conveyed to you about ways to approach the work, can you think of?

B: [pause] I couldn't even begin to describe it. There's just a certain cleanness about his work and he asked the right questions.

W: Do you feel that you took that away with you, in terms of your approach? From your experience with him?

B: Yes. The first few papers that I wrote, the main question I asked myself was, What will Joe Doob think of this? And I tried to say things that he would think were interesting.

W: Did he have other advisees besides yourself?

B: Yes, Paul Halmos whom I mentioned to you. And Warren Ambrose was one of his advisees.

Now, in 1941 Joe Doob was going to go to the Institute for Advanced Study in Princeton in 1941-42. Paul Halmos had finished, and Warren Ambrose had finished, and they were going to go to the institute with him. Now, there I was. So it was important to Joe Doob that I should finish. Because he didn't want to leave me hanging there; at the same time, he was going to go to the institute. So he sort of pushed me to finish my thesis so that he would be free to go to the institute without worrying about me. And I have always thought that he approved my thesis a little sooner than he would have if he'd been staying there.

W: Yes, he was like, hurry up and come on.

B: That's, hurry up and finish your thesis, so I can go to the institute without worrying about you. And so, he not only pushed me to finish my thesis and approved it but and this is something you were going to touch on a little later got a Rosenwald Fellowship for me so that I could go to the institute, also. When I say, "He got it," I'd never heard of such things. He told me to apply for it. He got the necessary information, and he sort of suggested what I should say. So, Joe Doob really just got me this Rosenwald Fellowship. So, I did finish my thesis and we all went to the institute together. So, in 1941-42, I was at the institute, Joe Doob was at the institute, Paul Halmos was at the institute, and Warren Ambrose was at the institute.

W: I have a few more questions for you before we arrive at the institute.

B: Okay.

W: This is just a list that I found on the internet for your other members at the institute these two years. I wanted to ask you about your thesis on Markov chains. I wanted to ask you first, what was the inquiry and conclusions of your thesis?

B: Let's see. Well, one part was can you describe all the Markov chains that are idempotent? Now, I have to tell you what that means. That means that given where you are today, where you expect to be two days from today is exactly the same as where you expect to be one day from today. Your future two days from now looks exactly like your future one day from now. And, it was known for finite state chains what all the idempotent chains are, and Joe Doob suggested it would be interesting to explore that question in general, and see if I could describe them all. And, I more or less did.

Well, that was an interesting, fairly small question. But then there was another big question. Joe had discovered that Martingales going backward converge. And if you apply that to finite state Markov chains, you get an interesting theorem about convergence of products of Markov matrices. And he suggested that I describe what you get there. And, again, I more or less described that, not completely, but I made a fair amount of progress on that.

So that, the first topic, was a sort of small problem that you could complete and then forget about. But the second one was one that had big implications and that was worth studying a lot. I later published a small paper on each one of those topics.

But they were just Joe Doob's ideas, I was just exploring some questions that he had raised.

W: Were there any other topics that you were interested in pursuing as a thesis, or were you quite happy to work with those ideas?

B: Oh, I was quite happy to work with those and they took all my time during my last year at Illinois. And they were a big part of what I worked on during my year at the Institute for Advanced Study.

W: This is a question from the non-statistics background person. What was the potential real-world application of your thesis?

B: [laughs uproariously] None. Mostly, you don't do mathematics because of its application to the real world. You do it because it's mathematically interesting. Of course, you're pleased if someone happens to find a real-world application. But that's not why you do it.

W: As your time at graduate school drew to a close, how did you begin to think about your next steps in terms of career with Joseph Doob off to the institute? What were your concerns and priorities?

B: I was pretty sure I would get a job in one of these 105 black colleges. I was interested to see which one. [chuckles]

During the three or four months, during the summer between the Institute for Advanced Study and my job at Southern, I got a job working for the Office of Price Administration. I bet that's not in my bio-bib, or whatever. Anyway, I'll tell you a story about that.

I had a very good friend at Illinois named Dunbar MacLaurin. He was a black economics student who got a PhD at about the same time I did. And he got a job in Washington, working for the Office of Price Administration. So, he was there while I was at the Institute for Advanced Study. Now, at that time, young people were being drafted to go into the army. MacLaurin didn't want to go into the army; I didn't want to go into the army. He wrote to me and said, "If you want to be protected from the draft, come down here and get a job working for OPA. That will be an essential occupation and you'll be protected from the draft."

So I came down to Washington, D.C. MacLaurin arranged for me to be interviewed by somebody at OPA. And a few weeks later I got a job at the Office of Price Administration. And, of course, I took it because that would protect me from the draft. But, shortly after I arrived at the Office of Price Administration, MacLaurin, who had claimed to have a job that was exempt from the draft, his boss was drafted! [laughter] So that told both MacLaurin and me that our jobs were not exempt from the draft. So, that settled it for me. I had already accepted a job at Southern, but I had planned, I suppose, to try to postpone it so that I could keep this government job. But when I found out that that wasn't going to keep me from the draft, then I just resigned that OPA job and went on to Southern. So, for about three months I worked for the Office of Price Administration.

W: What did you do there?

B: I don't remember much about it.

W: Maybe you had a desk?

B: Oh, I certainly did have a desk. And, I remember a few of my colleagues. But it's not clear what we did. [laughter]

W: Okay, I kind of imagine it was some kind of number-crunching, but I'm not sure. Certainly from the title...

B: Well, now, I'm going to tell you something and it's not going to be right; but it's going to be fairly close to being right. The director of the office of the Office of Price Administration was a man named Leon Henderson. And he needed an economic advisor, somebody just to help him make decisions. There was a Harvard economics professor that he wanted, I think his name was Richard Gilbert. So Leon wanted to bring Richard Gilbert to come to Washington to be his economic advisor. So you had to ask the question, "What would be a suitable salary for Richard Gilbert?" Turned out that in order to pay Richard Gilbert that salary, he would have to be the director of a division. So they created a division for Richard Gilbert called the research division. That's the division I was in. [laughter]

W: I think I understand better why you were not sure what you were doing there.

B: Yeah. I'm not at all sure that I have the details right. But it's a good story, and it's approximately right.

W: Well, that's the best. And, you know, feel at liberty any time you have an approximately right, good story, put it on the table. That's good.

B: As long as I say, make it clear, it's approximately right but I'll get the details wrong. For instance, I'm not sure about the name Leon Henderson or about Richard Gilbert, but something like that happened.

W: That's pretty good, that's pretty good. I wanted to go back a little bit. We're kind of slowly inching up on your time at the Institute for Advanced Study, but I just wanted to talk a little bit more. In 1941, the U.S. mobilized to enter World War II.

B: Yes.

W: And you talked a little bit about how you experienced these events and trying to find a job for shelter from the draft. Because I guess, you know, being a graduate student or an academic or a professor wasn't considered an essential occupation, or was it?

B: I'm not sure, but I know that I had the impression that working for OPA would be more essential, but I was wrong.

W: I actually heard this story from Russ Ellis. He said that you were somehow excluded from the draft.

B: Oh. This is later, in 1943 or early 1944 they started scraping the bottom of the barrel, as they called it, in drafting people. So, they called me to be drafted. And I had to go to Chicago and take some kind of examination. And I did, and among other things you had to go to a psychiatrist. And he asked me a few questions, wrote some things down. But before that happened, I had mentioned to one of my students, well, I had mentioned, maybe in my class, that I was going to be drafted or something, and didn't want to be.

So, a student came into me, and said, "Prof, I understand that you've been called for the draft." I said, "Yeah." "And that you're worried about it." "Yeah, I am worried about it." He said, "You don't need to worry. Because," he said, "I was drafted and I went into the army and examined and I was rejected for psychoneurosis. I am one, I know one when I see one, and you are one." [laughter] "You don't have to worry!" One of my students told me that, and Nadine, he was right! [laughter] I showed up and was examined by the psychiatrist among other people. He didn't make any decision right then, but a few weeks later I was notified by mail that I had been rejected and the reason was what they called "anxiety neurosis." So I was just very pleased with that.

W: I can imagine. I'm not sure what it means, "anxiety neurosis" or "psychoneurosis," I'm not sure what those designations mean. But does that mean high-strung? That could mean anything.

B: Yes, I'm not sure what it meant. But, anyway, the psychiatrist rejected me and he was right to reject me because I would not have been suitable for the army.

W: Right, I understand. Well, I want to ask you then, did either of your two brothers end up being drafted or fighting?

B: No. Skeet was not drafted. I've forgotten how that happened. But Joe was younger, and by the time Joe was ready to go to college, the war was over and the G.I. Bill had been passed. And he volunteered, and spent two or three years in the army. And then used the GI. Bill; and that supported him all through college. Both Joe and my cousin John did the same thing. They graduated from Centralia High School in the same year and volunteered for the army, and then went to college under the GI. Bill.

W: I have a fairly large question, which is and it's very general and maybe it's something that you'll answer in small pieces but how then did you experience World War II? For example, do you remember when the Japanese bombed Pearl Harbor? And how did you experience these events that were happening around you?

B: Yes, I certainly can't answer that question, [pause] I was just not politically aware in those days. I almost never read a newspaper, for example. I was much more interested in mathematics and athletics and girls.

W: What were your expectations when you went there, to the LAS?

B: Oh, just that I would continue studying the topics, my thesis topics, and continue to talk to Joe Doob and Paul Halmos and Warren Ambrose.

W: Was this the first time you had traveled outside of Illinois?

B: Oh no. It was the first time I had lived outside of Illinois, certainly.

W: What did your family think of this opportunity for you?

B: I don't think they had any opinions. They were assured that what I was doing was reasonable and just glad that I came back to Centralia for a visit.

W: I read in one of your interviews that you had noted that your mother had more, kind of, questions about the path you were on than your father.

B: Yes, she wanted me to study business somehow. But she accepted that I was going to make a reasonable living doing what I was doing.

W: You mentioned that during your time at the Institute, you basically kind of continued the work that you had started in your thesis, the two problems that you'd been really working on and focusing on in graduate school under the advisorship of Joseph Doob. Did you start to branch out into other areas as well? Could you describe the other primary math and statistical themes you explored while you were there?

B: Yes, one of the things that I got interested in while I was there was game theory. Von Neumann was there, and he had proved the basic theorem in game theory, and that was sort of in the atmosphere; so I got interested in that.

And also, I got mildly interested in statistics at that time. There was a fellow named Sam Wilks at Princeton University who gave the course in statistics, graduate course in statistics, there. And he was a pretty important man. He was the editor of the leading statistics journal. And I sat in on Sam Wilks's statistics course while I was at the Institute for Advanced Study. And one of the main ideas that I got from that course was that I wished that I could understand statistics and the way statisticians thought, but it was too hard. But I knew there was something interesting there and I wished I could understand it.

W: What do you think appealed to you about it at that time?

B: Well, the questions that statisticians ask are interesting. "Given this data, does that say that this treatment is better than that treatment?" That's clearly an important question. And it was also clear that it was what statisticians were trying to answer. And Sam Wilks proved lots of theorems that, as I say, I couldn't quite adjust to my way of thinking. But, as I say, I knew that I would like to understand it.

W: Which faculty members did you work with, while you were there at the institute? My understanding is that there is a standing group of faculty members who are there.

B: Yes. I didn't work with any of them. I had one or two conversations with Von Neumann. And I had just one conversation of one minute duration with a very distinguished logician named Godel. But except for that, I didn't interact with the resident professors. But I did with other young people there. Again, as in my graduate days, I learned more from my contemporaries than I did from the higher-ups, so to speak.

I see you have a list here of people who were at the Institute at that time.

Yes, Paul Halmos, Shizuo Kakutani. He was a young Japanese mathematician at the institute at that time. I learned a lot from him. He had also worked in Markov chains. But he understood them a lot better than I did, and I really learned a lot from him. He's at Yale, by the way, he retired some years ago. I think he went to Yale right after the war, and stayed there all his career.

Gerhard Kalisch, I worked quite a bit with him and learned quite a bit from him and we were friends. We used to go for walks together. I once admired a tie that he was wearing, a beautiful brown tie. He took it off and gave it to me. [demonstrates] He got a job at UCLA and taught there until he retired. I used to see him occasionally.

George Mackey was there, yes. I learned a fair amount from him at the time. Several years later, he and I discovered the same theorem at about the same time. And it was interesting to me because the way he came at the theorem had absolutely nothing to do with the way I came at the theorem. Somehow we both found the same theorem.

W: Can you describe the theorem and then describe your different approaches to it?

B: I can [pause] I can describe the theorem and I can describe what led me to it, I think, [pause] Well, I'll first I don't know if I can describe it or not in a way that you'll understand, [wondering] It says that two countably generated sigma fields of Borel sets that have the same atoms are identical. Now, wait a minute, and I'll see if I can find the paper that says that, [rifles through papers with no success] I'm sorry, Nadine, my files are not in very good shape. Then Dorothy Maharam.

W: That's the one woman.

B: Yes. Again, I learned a fair amount from Dorothy Maharam. She is also interested in measure theory. And knew a lot more about it than I did. She and her husband, Arthur Stone, were both at the institute that year. I haven't seen

them for three or four years, but they come to Berkeley from time to time, and always come by my office when they come. And again, I've had mathematical contact with Arthur Stone over the years. I asked him a question once; and he remembered the question and asked the question of somebody in England; and that person answered the question. I don't remember exactly what it was, but I remember how sort of indirect this was.

And Jimmie Savage was at the institute that year! Well, he and I have a number of interactions over the years.

W: Didn't you work together at Rand as well?

B: Yes, yes, yes, we did, among other things. Well, those were the people at the institute that I've worked most closely with. But I tell you again, it's sort of my contemporaries that I worked with.

W: You have an interesting way of always framing it as you were learning from them and, I mean, I know you were learning from them. But I'm sure there was a lot of times when they were learning from you, as well.

B: Well, you'd have to ask them about that, [laughs]

W: It seems to me also that, just from your describing where one other person talked to his friend in London and the response came back to you, that you were suddenly in a much larger, more international network of people who were thinking about math and statistics once you were at that institute.

B: Well, mathematics is completely international. Yes, yes. Of the people that I met at the institute, let's see. Halmos is actually originally from Hungary. Kakutani is from Japan. Mackey is from Texas. And Maharam is from the East Coast somewhere. Jimmie Savage is not sure, I think from Michigan. Arthur Stone is from England, so they [pause] Nadine, I'm glad you're interested in all these ramblings, but I still don't know why. [laughs] But that's all right.

W: Okay, well, I'll try and share with you more. I think it's important to have perspective on the larger framework.

B: Okay, sure.

W: I want to ask you, this is kind of a general question, but what was life like as an IAS member? And the reason that I'm asking is because I just kind of imagine this group of young people having this incredible opportunity to share and exchange ideas. Is that basically what it was like there?

B: Yes. You had a small office and a library and rooms where you could gather and talk, go to the blackboards and you just bump into each other, and for instance, I remember one day I was reading something in this book, Paul Levy's book. And I puzzled over it, and puzzled over it, I couldn't understand it. And a fellow name Ralph Phillips was sitting a couple desks away and I just went over and asked Ralph whether he could understand this passage. Let's see, is his name on here? [on the list] Well, Ralph Phillips was there, and I went over and asked him, "Ralph, I can't follow this reasoning, can you explain it?" Now, he, although not a probabilist, started reading it and he explained it to me. Well, that was the sort of casual atmosphere that there was. We were all there trying to learn and if you thought somebody could help you, you did, you asked.

W: This book, let's see if I can pronounce this correctly, *Theorie de L'Addition des Variables Aleatoires*, it's from Paul Levy.

B: Sounds French to me.

W: I don't know what that means in English, though.

B: The Theory of Addition of Random Variables, [pause] And many of these are papers that I grew up on, the papers that are cited in this book, [looks through book]

W: Let me start here, while you were at the institute, the director there, was it [Abraham] Flexner or [Frank] Aydelotte?

B: I believe it was Aydelotte.

W: Okay. In the interview, it kind of mentions that Princeton had had a real problem with your being there, and that the director had to really go to bat for you. And had staunchly gone to bat for you. Can you tell me that story?

B: I can tell you the story as I understand it. I didn't find out about it until years later. There was a custom that all members of the institute would be made honorary faculty members at Princeton. And so when I was invited to become a member of the institute, that meant that I would be appointed an honorary faculty member at Princeton. Well, the president of Princeton did not want any black honorary faculty members at Princeton. And as I understand it, he notified the director of the institute and there was a big fuss over this. And several of the professors in the institute complained about it and threatened to disconnect the institute from Princeton unless I was accepted. And I guess it wasn't a big thing, so the president of Princeton backed down.

I never knew anything about that. Of course, it was all settled before I got there. And I was just welcomed cordially along with everybody else. It was only much later that I found out that there had been all of this to-do.

W: Did you learn who were the faculty who threatened to sever ties with Princeton?

B: One of them, I understand, was Oswald Veblen, who was a professor of mathematics at the institute, a geometry man. When I arrived, he was very cordial to me [laughs], but didn't act as if there was anything special about me, so I didn't know anything about that, then.

W: You didn't know the other names of the people who stood for your ?

B: No, it seems to me that I once did know something. But, no, I've forgotten. I don't know who would know anything about it now.

W: How did you hear about this? You said someone told you years later, who told you? How'd you hear about it?

B: I don't know. I have the impression that someone just mentioned it under the impression that, of course I knew all about it, but I don't know.

W: But you really didn't know all about it.

B: No, I didn't know anything about it.

W: Other than that experience, which kind of happened

B: off-stage!

W: off-stage. Was the institute an environment that felt free from racial prejudice?

B: Yeah, [pause] Yes, I never had any problems.

W: And was it a place where no one questioned your right to be at the table?

B: That's right.

W: Outside of the institute, in academic circles, when you encountered people who questioned your abilities from their own racial prejudice, how did you address that? How did you deal with that?

B: I've never had that kind of incident. I've been denied access to things because I was black, but not among my colleagues or the people I associate with.

W: I want to go back to that question of being denied access, and ask you what that meant?

B: [pause] Well, there have certainly been incidents in my life where I was "denied access." My memory on this is somewhat vague, but there was a meeting of the American Mathematical Society somewhere in Virginia, and I and two white mathematicians drove from Washington, B.C. down to the meeting. And I was not allowed to stay in the dormitory where the other mathematicians were staying, and where the meetings were to be held, I think. So, I

simply turned around and drove back to Washington, D.C., and missed that meeting. That's certainly one case where I was, quote, "denied access," unquote, and there may have been other incidents like that. Maybe two or three. I remember some correspondence I had with the secretary of the American Math Society over such incidents. I don't remember any others, but there may have been others. But again, my mathematical colleagues were quite sympathetic. They gave me the impression, at least, that they wished it had not happened.

W: Is there a way to bring issues of structural racism into probability? Is there a way to theorize these things in math?

B: [laughing] I don't know how. Structural racism [musing] that's a new term to me.

W: Okay. When I say structural racism, I guess what I'm speaking to is practices of discrimination that are embedded in institutional societal practice.

B: I understand. I just gave you an instance of that. I just thought of another one that happened to me and my wife in Washington. I may have told you about this one. There was a play that my wife and I wanted to see. So, we went to the downtown theater in Washington and stood in line for tickets. And when we got to the head of the line, the sales clerk said that she couldn't sell us tickets. So, I stood there for a few minutes sort of arguing with her, and the people behind started a chant, "Sell him the tickets! Sell him the tickets!" But she couldn't. She said she would lose her job if she did, she wasn't supposed to do that. So, we finally left. So, there is, in what you call this institutional racism, something that is stronger than the particular people that are involved in it.

W: Yeah, it's not about hurt feelings. It's about denied access. It's much bigger than that. For example, one time, when you and I were speaking, and you mentioned the death penalty. And I was thinking about you know how there's this whole thing right now where they're reviewing the death penalty, and thinking about how often it's been wrong, basically. Is there like a place where you could take probability into that kind of analysis and theorizing, given the different factors that go into making those kinds of decisions?

B: Well, people do do informal probability calculations. The governor of Illinois probably said that since this one innocent fellow came within two weeks of being executed, probably there are other innocent people that have been executed. That's kind of probabilistic reasoning, which we all do, every day. And of course, probability concepts enter into our criminal justice system, generally. Preponderance of evidence is a probabilistic concept. Beyond a reasonable doubt is a probabilistic concept. So, probabilistic reasoning enters into these things.

W: I have another question about the institute. You were telling me, you said, "Oh, we talked mostly about math. We were talking about math." And if it's possible to say what were the hot issues to talk about in math at that time, like where were yourself and your colleagues focused, around math? Like what issues were the ones that you were talking about all the time? Which were the ones that kept you awake all night?

B: No. Issues is not the appropriate word at all [laughing]. What problems, what concepts, what ideas, [pause] I was still learning. For me, I was mostly learning about probability, real variables, point set topology. Some of my colleagues and friends were learning about Hilbert space, and I learned a little about it just from listening to them, but... oh, nothing big. We were each doing our own research and if you came across a problem that you thought one of your colleagues might know something about, you'd ask him about it. There were a lot of us around there, just beyond the PhD. We were just getting started. That was a big year for me.

W: When you say it was a big year for you, I imagine you in this kind of accelerated learning space, and just really open and really delighted.

B: Yeah, I was with all well, there were essentially two kinds of people there. There were the really great mathematicians and all these fresh new PhDs. So, we didn't have much contact with the really great mathematicians. They gave lectures from time to time, you'd learn something from them. But you had a lot of contact with the other young people. I learned a tremendous amount from them.]

W: I have a question for you about applying for work after your dissertation and after the institute which is that I'm wondering if you'll tell me the story of how you looked for work?

B: Well, I did two things. I wrote those 105 letters. And I went on an automobile tour of about thirty colleges. And just drove up to an institution and looked for the mathematics department and went in and introduced myself to the head of the math department and told him I was looking for a job. Crazy way to do things! But I didn't know any better, [laughs] And, mostly I got cordial receptions but it was made clear to me that well, often, I was referred to the president of the institution because the head of the math department didn't make appointments to the math department; the president of the institution made appointments. So, sometimes I got to see the president. And out of all this activity came three job offers.

W: And all 105 applications you sent out to historically black colleges?

B: Yes.

W: And where did you tour?

B: On the East Coast. I started at Morgan, and went to Howard, and on down the East Coast. I don't remember the names of all the colleges that I visited. But I was having fun, just getting to see what the country was like and what the black colleges were like.

W: When you stopped in to visit at Howard, did you, did you have that was eventually where you ended up for ten years. You met with the math department there?

B: I thought I told you that story, but I'll tell you again.

W: I don't remember you telling me that story.

B: All right, yes, I showed up at Howard and went in to talk to the head of the math department. He was very polite. I believe he was a Jamaican, by the way. I don't remember his name. As I say, he was very polite. Asked me a few questions, but it was clear to me, even at the time, that he wasn't really interested at all.

However, another man who was not head of the department was sitting at his desk over there, listening to me. And after the head of the department had finished talking to me, he asked me one or two questions. And then, that was that. Well, two years later, the head of the Howard math department retired and this other man became head of the math department. He wrote to me and invited me to come to Howard for an interview. He remembered that conversation, and then he offered me a job, which I took.

W: Do you remember his name?

B: Oh, very well. His name was Woodard, Dudley Woodard. Let me tell you a story about Woodard. I've told this story before, but maybe not to you.

W: Not to me.

B: Not to you, okay. For many years, Woodard was dean of the College of Letters and Sciences at Howard. At the age of forty-five, he decided to go and work for a PhD in mathematics. And he did, at the University of Pennsylvania, studied there for three years, and wrote his thesis. And then, he came back to Howard, not as dean and not as head of the math department, but as a professor of mathematics. So that was a two-level demotion. And if you know how authoritarian Howard University was at that time, you will recognize that that was a big decision that he made to give up the deanship to go to work for a PhD in mathematics to come back for a professorship. But he did.

That was the man who was sitting in the other chair. And I was really impressed with Woodard. He was a serious mathematician. He persuaded the dean to set aside a special room for a mathematics library at Howard. And he started a mathematics seminar at Howard. I would give talks and he would give talks and graduate students would give talks. As I say, he was a serious mathematician and a serious man.

W: Well, it sounds like it to make a choice like he did, to make a choice like he did it sounds like he was very serious. He really made, it was definitely a commitment. And it sounds like he was very passionate about teaching and math.

B: That's right.

W: When you went on this tour of the different schools, it sounds like you were kind of investigating and exploring the math departments. What did you want to find? What were you looking for?

B: I wanted to find a job! [laughter] I'm sorry!

W: But, I mean in terms of your criteria when you were kind of exploring what it would be like to be at this institution. What were the criteria? What were you looking for in terms of institutions?

B: I didn't know. I'd go to all these places and find out what it was like and apply for jobs, and then of all the offers I'd got, then it would be time to think about which one I wanted. But I was just trying to find out, that's all. I didn't have criteria in mind.

W: Out of all of the hundred and five applications, when you got back, how many affirmative responses?

B: Eventually, I got three offers. One was from Southern, one was from Clark College, and one was from West Virginia State. But as I remember it, as soon as I got the offer from Southern, I accepted it and didn't worry about maybe getting other offers.

W: Because that was where you wanted to go, or it was the first offer you got?

B: It was just the first offer I got. [laughs]

W: Southern. That's in Louisiana?

B: Yes. State university in Louisiana, and at that time, of course, all black.

W: And was that in Baton Rouge?

B: Yes, the post office was Scotlandville; but it really is in greater Baton Rouge.

W: Now, were these one-term, one-year positions, that were offered to you, or did you make them one-year positions?

B: I think they were one-term positions, presumably renewable. I believe, at that time, in the black colleges there was no such thing as tenure except maybe at Howard. All positions were one-term renewable at the pleasure of the president, but normally renewed. I'm sure I could have stayed at Southern forever on one-year positions.

W: When you arrived there, what did you find in terms of institutional support for the math department?

B: Why, there was a man who was the head of the math department, he simply told me what courses I was to teach. Institutional support is a fancy term, [laughs] As I remember, the textbooks were already chosen. It was a friendly atmosphere.

W: How many other math professors were there?

B: I think nobody except me and the head of the department.

W: Wow, so were you teaching many classes?

B: I believe I taught fifteen hours, yes, I believe it's something like that.

W: And this was your first time in the classroom?

B: Yes.

W: Wow, how was that?

B: It was fun for me, but it was hard for the students. I don't remember much about it, but I'm pretty sure that I made the courses more difficult than they should have been.

W: How?

B: I think that I tried to make arithmetic challenging, instead of just a drill.

W: Do you remember what course you were teaching?

B: No. It seems to me that they were mostly freshmen courses, though. The advanced courses were taught by the head of the department.

W: Were most of the students just sort of checking in at basic math, or did you find students who were really excited about pursuing more advanced ideas?

B: I remember one student who showed real mathematical talent. I think he went on to become business manager at Southern University, something like that eventually happened to him. But he's the only one I remember especially as being talented.

W: This was your first time, also, living in the South.

B: Yes.

W: How was that for you?

B: It was interesting, and I didn't know quite what to make of it.

I did one thing that if I'd been wiser I would not have done. The first time I got on a streetcar in New Orleans there's a little board that you plugged into the top of the seat, and on the front of it said "White" and on the back of it, it said "Colored." The idea of it was that if the board was here and all the Colored seats were taken, and the next row above was vacant, you moved the separation board up one row and then sat there, and vice-versa. I thought that board was rather funny. And when I got off the streetcar or the bus, I took the board with me. [laughs]

W: What did you do with it?

B: I don't know, maybe took it back to my room, posted it for a while. So I, of course, accepted segregation but I didn't take it very seriously. Just another one of those silly customs.

W: Did you have the sense that other people around you were taking it very seriously?

B: I don't remember.

By the way, that reminds me of an interesting experience that I had. I wanted some mathematics books, and of course, they were not at Southern. So I went over to Louisiana State University to use their library. Of course that was a segregated institution. And I went to the mathematics department just to find out how I would go about it. There were two people on the faculty in mathematics at Louisiana State University that had been my fellow students at Illinois. So, I ran into them. One of them sort of avoided me and made it very clear that he didn't want to be associated with me there. The other one was extremely helpful and took me in, introduced me to the librarian, and asked that I be allowed all the facilities and shown the courtesies and so on. And that was that.

But the interesting thing to me was that the man who refused to have anything to do with me had grown up in the North, and the man who was so courteous and helpful was from Mississippi. I suppose the northern man was trying to bend over backwards, making sure he didn't want to be associated with black people. I imagine the Mississippian wasn't worried because he knew how far he could go. I guess that was it, anyway.

W: Things don't always turn out as you expect.

B: Right.

W: For sure, they're definitely not always very clear, "if p then q" kind of lines. It was also, I understand that when you went to go teach at Southern, it was also the first time that you'd been in an educational institution that was all black.

B: That's right, [pause]

Yes, I learned a lot about black people during that first year. For instance, there's what's sometimes called the Negro National Anthem, "Lift Every Voice and Sing." I had never heard that until I went to Southern! Just a lot of things like that, were just completely new to me. It was a learning experience for me. By the way, that Southern choir was one of the most, well, the most beautiful singing group I have ever heard.

W: Many people have described teaching to be this really transformative process, basically because it forces the teacher to learn as much as the students. Did you find that to be true? And not so much in terms of the subject matter, but in terms of how to communicate it to another human being in a way that they can hear it.

B: I have found that I always learn something when I'm teaching. And if you're trying to teach something to someone else, you need to understand it really well because you want to make it as easy as possible. And that means you have to be able to look at it from all directions. Yeah, I've always found that even if I'm teaching a course for the tenth time, I still learn something every time I teach it. And I think I've become a little better teacher because of that.

W: Can you give me an example of this?

B: No, I can't

W: Why did you leave Southern University and go to Clark in Atlanta?

B: It just sounded like a more interesting place to be. There was not only Clark, but there was Morehouse and Atlanta University all in the same region.

W: Was there more than two people in the math department there?

B: No. At Clark I got to teach advanced mathematics courses, or some, because the head of the math department was on leave. But I think if I had stayed, I would have been teaching only elementary courses at Clark, too.

But there was a cooperative atmosphere there. Clark, and Morehouse, and Morris Brown and Atlanta University cooperated and I had students from all four institutions in my classes. And there was joint seminar, that was run by me, during the year I was there and a man from Morehouse and a man from Morris Brown, and a man from Atlanta University. I made the right decision from that point of view. It was a richer experience than Southern.

W: In the seminar, you were teaching advanced math?

B: Yes, each one of the four of us would give a talk, and graduate students from Atlanta University would give talks, yes.

W: That sounds very exciting.

B: One of the fellows, the man from Morehouse, was an extraordinarily able teacher. I learned a lot about teaching from him. By the way, my wife, whom I had not met at the time, had also studied mathematics with him. His name is Dansby, Claude Dansby. And there's a building at Morehouse now that's named after him. I was glad to see that, that he was really recognized as a distinguished teacher.

W: Do you remember the names of the other people who were part of that seminar?

B: Yes, Joe Pierce was from Atlanta University and Herbert Boggs was from Morris Brown.

W: So there was definitely a community of sorts.

B: Yes. Boggs introduced me to my wife, by the way. [laughs] Yes, it was a community, all right.

W: So it was in Atlanta that you met Mrs. Blackwell.

B: Yes.

W: I want to ask you, as far as these one year instructorships at Southern University and Clark College, did they contribute to your knowledge about how to work within an academic institution? You were a fledgling teacher.

B: Oh, I'm sure they did. As I say, I'm sure I was a considerably better teacher after that first year at Southern. You get a feeling for what to expect of your students and how to present things.

W: I have to check in with you about time, because we've been talking for about two hours and I can talk for the whole day. I know that you have commitments, would you like to stop now?

B: Yes.

W: Okay, we're signing off for today.

[end of interview]

INTERVIEW 3

APRIL 25, 2002

W: Thank you. Okay, I think we're all right. So, good morning.

B: Good morning.

W: I had some follow-up questions from our last conversation.

B: All right.

W: And actually, one of them goes back to one of our first conversations. I wanted to ask you and this goes back to Centralia, Illinois when you applied for schools, for college, did you ever think of going to a black college at that time?

B: No. I had always assumed that I would go to the University of Illinois. And the only other college that I thought of was the college in Indiana where Ms. Seiler got me a scholarship. I've forgotten the name of that college now, but I told you last time. [DePauw University]

Where I was growing up, for most people there were only two colleges. The University of Illinois and Southern Illinois University at Carbondale. And if you thought you could make it at Illinois, you went there. And if you weren't sure you could make it at Illinois, you went to Carbondale.

W: So it seemed that there were just two different levels of college education?

B: Just two colleges that most people considered.

W: My next questions are about the Institute for Advanced Study at Princeton, my next follow-up questions. While you were at the institute, World War II was taking place. And my understanding from talking to you is that everyone at the institute was from other places. Not all of them, but many of them were actually from Europe where the war was happening. And I wanted to ask you, what was the atmosphere at the institute at that time around these issues?

B: I don't remember very much about the atmosphere. There was some conversation about the war, but mostly it was about mathematics.

W: The other thing that I learned about the Institute for Advanced Study is that while it was really I think there's actually a quote from Abe Flexner, from Mr. Flexner who was a founder he said that basically it was established with the idea that people could come and push the boundaries of the known.

B: Sure.

W: Just push the boundaries of the known. That was it. It's a really amazing idea to me that it was just a place where people could just think their thoughts and push their ideas. And at the same time, it was a place that was founded during a time when many Jewish intellectuals were fleeing or coming from hostile environments in Europe and finding a place at the institute. And I wonder if you have what your experience was around this.

B: I didn't really. For me, the institute was a place where a lot of young mathematicians got together, and we talked mostly mathematics. It was a very friendly place. But it was pretty isolated, mostly, from what was going on in Europe.

W: Also, when you were at the institute, you told me you had a brief conversation with one of the faculty members there, Godel. My understanding is that he had this argument that was based on math for the existence of God.

B: I didn't know anything about that. I remember my conversation with Godel. I said something that, when I look back on it, was really kind of dumb. I said to him that it seemed to me that there are some propositions which, if they were undecidable, must be true. And he sort of shrugged his shoulders, "Oh, yes, yes, yes, we know that." And, of course, now that I look back on it, of course, every logician knew that very well, but it was just new to me.

W: Well, I think there's that moment oftentimes when you have a conversation with someone and you feel that you've made a great discovery and you share that with someone and you realize that it's been in the world for many years.

B: Yes.

W: I'm just going to take a minute and listen to us talk here on the CD, I'm a little concerned about making sure it's working well. Hello. Dr. Blackwell, could you say some words?

B: Okay, what would you like for me to say? [laughs]

W: Well, I think whatever you think is important to say is the point of our conversation, so... I would actually like to ask you the question about Godel again because I'm worried that it caught everything else we talked about but I think I may have turned it down about at that time. So if I could ask it one more time, I'm sorry. What was the conversation with Dr. Godel like?

B: Well, we were walking along and I said to him that it seemed to me there were some propositions which, if they were undecidable, must be true. He said, "Yes, yes, we know that." [laughs]

W: And you felt like you were telling him something new.

B: Well, I was telling him something that was interesting to me. And as I later found out of course, that was page one in what logicians knew about things.

W: So what do you think of that possibility of using the language of math or logic to theorize the existence of God?

B: [pause] There you've got me. I'm not religious, it wouldn't occur to me even to ask that question. [Nadine hands Godel info] Wow. Nadine, I'm not going to have any comment on this, [laughs]

W: I think I was more trying to verify that I was actually in the same ballpark with the kind of work that you do, if that makes any sense. Just in terms of the tools, that's all. There were some other faculty member at IAS who we were discussing earlier, and in addition to Godel and Von Neumann, was there anyone else who you remember interacting with?

B: No. I had some interaction with Sam Wilks at Princeton, not a lot. But he taught the main statistics course there. I sat in on that course and had a few conversations with him. In fact, Sam Wilks recommended me for some job in Washington, I've forgotten now what it was. There was a war going on, and some of the people at the institute and at Princeton were very active in government work. Von Neumann was one, Sam Wilks was another one. I've forgotten now what it was that Sam Wilks recommended me for.

W: We talked a little bit about that. Did you have any interaction with Albert Einstein while he was there and you were there?

B: No, just a few polite courtesies, that's all. Nothing serious.

W: When we were talking about this last week, there was someone you mentioned who was in the year who didn't appear on my list but was there.

B: It may have been Ralph Phillips.

W: Yes, I think he was in that year preceding you.

B: Yes, Ralph Phillips. Then he must have been just visiting the institute for a day or two or something, because I certainly had that conversation with him.

W: There also may have just been some overlap in the years, in the academic years. Was there anyone else in that previous year that you recalled talking to or working with closely?

B: Well, [Paul] Erdos, of course, was probably also there in 41, 42, but he's not listed there.

W: No, he isn't.

B: He's not listed here, but he was certainly at the institute often during that year. And Paul Halmos, he's probably listed. Yes, he's listed. And I don't see any of the others.

W: Okay, I would like to move on to Howard. You told me this really interesting story about how when you were doing your drive, it was like a drive through the country to propose yourself to the different math departments at the different colleges. And you were at Howard, and you interviewed with the director of the department and there was someone else listening.

B: That's right, I've forgotten the name of the head of the department, but Professor Woodard was sitting at the next desk and listening. And at the end of the interview, he asked me a couple of questions, and I left. And a year or two later, the head of the department retired and Woodard became head of the department. And right away wrote to me and asked me if I was interested in coming to Howard, invited me up for an interview. And subsequently, offered me a job.

W: Who else was in the math department when you arrived there?

B: Woodard was the head of the department. Elbert Cox was also in the math department. There were a couple of young instructors in the math department. Woodard and Cox were the only senior faculty members in the department at that time. By the way, Cox was the first black man to get a PhD in mathematics; and Woodard was the second black man to get a PhD in mathematics. So they were the first two and they were the only senior math members at Howard.

W: When you arrived at Howard, what was the state of the math department? And I should qualify that question by saying in terms of the kinds of resources that you had to operate with, and the kind of intellectual level of the work there.

B: Well, there was a math library. It was [pause] I don't remember going outside of it very often. Mostly, I was interested in mathematical statistics at that time. And, of course, the Howard library carried the leading math stat journal.

But I went outside the university quite a bit for mathematical contacts. My friend, Abe Girshick, for instance, was a statistician at the Department of Agriculture. And, I was a consultant at the Operations Research Office in Washington, and there was some interesting mathematics going on there.

W: Operations Research Office?

B: Yes.

W: I'll have to ask you some more questions about that.

B: Well, I won't be able to tell you very much because I don't remember very much. I was reminded of it by your list, because you had the name Nicholas Smith on there. He was the head of the group at the Operations Research Office that I worked with.

W: Do you remember what kind of work you were doing there?

B: Mostly game theory and optimization theory.

W: And may I ask, what were the potential applications, around what kind of applications?

B: Mostly having to do with wars and fighting. I doubt that we were very helpful, but it was mathematically interesting, anyway. Yeah, one of my good mathematics ideas came out of thinking about wars and how to fight them. Just a minute, [tape interruption]

Anyway, thinking about that kind of conflict led to what I call "approachability." Other people have called it "approachability theory." I just introduced the concept of a set being approachable by one player or excludable by another player. And it's been used by a number of people working in game theory. The point is that it came out of my thinking about wars in connection with my work at the Operations Research Office.

W: So, to what extent did you feel like this, the problem you were working with, could translate into a real, into an application in a war or a battle?

B: I didn't think that it could, and I certainly don't think now that it could, very well. For me, it all went the other way. If you can look at a real situation and translate that into something that's interesting mathematically, that's sort of what I like to do. Not going the other way, taking mathematics and interpreting it to the real world. But taking something from the real world and see if it suggested some interesting mathematics.

Of course if you're going to make it interesting mathematically, you have to change it and simplify it. So probably the changed/simplified situation won't correspond to anything in the real world at all. So, what you're doing is not practically useful. So, I don't think this work on a multi-component attrition game is militarily useful, but it's mathematically interesting.

W: When you were working for the Operations Research Office, did you get the sense that people outside of your immediate statistics unit found your work valuable? Valuable is the wrong word. Useful. Or that they understood it?

B: I don't think very many people outside of the group knew what we were doing at all. [chuckles] For example, I don't think that this particular paper, "On multi-component attraction games" [Naval Research Logistics Quarterly, 1954], has ever been cited by anybody else. As far as I know, it hasn't been. But it led me to a couple of theorems that have been cited.

W: Which were these theorems?

B: Oh, a theorem on approachability, let's see. [shuffles through papers] This is the paper, "An Analog of the Minimax Theorem for Vector Payoff." Let's see, here I introduce the concept of approachability. Yes, "approachable" and "excludable." Now those concepts of approachable sets and excludable sets have been used by several people in game theory.

W: Okay. I think you've really opened up a place where I may need to develop more questions around this. So, I'm going to return to Howard now, because that's where I'd like to talk today. But this is very interesting, this is very interesting. I wonder how you had time to consult while you were also teaching at Howard full-time, that's pretty amazing.

B: Consulting was just a part of my mathematical activity. While you're teaching, you're also learning and you're... My consulting was just talking to other people. I think I helped them and taught them something, but I sure learned a lot from talking with them. The fact that I got paid for it was just something extra; I would have done it for nothing because it was part of my mathematical activity.

W: Were you teaching a full complement of courses at that time?

B: Yes, I think the standard teaching load at Howard was twelve semester hours. And yes, that's what I taught. That's what I taught, I think, most of the time when I was at Howard. Maybe when I was head it was reduced to nine, or something like that. Yes, I had a substantial teaching load.

W: And when you went to Howard, did you find that there were that the math department, and you may think of this because at the time you were coming from Southern, and also from Clarke. Did you feel that the math department had resources?

B: The main mathematical activity at Howard when I went there was the activity sponsored by Woodard, who I mentioned earlier. Woodard had a mathematics seminar at which, well, we met once a week. And he would speak, and I would speak. Sometimes Professor Cox would speak and sometimes, one of the graduate students would speak, mainly about topology, because that was Woodard's main interest.

W: Topology?

B: Yes. But as I said before, I went outside Howard for a lot of mathematical activity.

W: I was trying to speak to financial support of the math department, how well the university kind of gave the math department financial support.

B: Well, I... They paid our salaries! [laughs] Howard had, what I thought, and still think is a very good travel policy. Howard would pay your expenses to attend one professional meeting a year, whether you gave a paper there or not. Just so you could go there and find out what was going on. Except for that, I don't remember much support, but you don't need other support.

W: I understand. Was the math department housed in the College of ?

B: It was in the College of Letters and Science.

W: Did you find students at Howard who were really excited about math? That first year?

B: Now, most of the students that I taught were, of course, not mathematics majors. And I had many very bright students, yes, at Howard. And there were not many graduate students, but among them were some who were just as good as the graduate students I've encountered here at Berkeley.

W: What classes did you teach?

B: I would say that a typical course load for me would be a freshman course... No, maybe two pre-calculus courses, one calculus course, and one more advanced course which might be a senior level course or it might be a graduate level course. That would be a typical teaching load.

W: So that's four classes.

B: Four classes, each one three units, so maybe twelve units all together.

W: Okay, were there courses that you particularly enjoyed teaching?

B: Yes, I think the course that I liked best was an upper-division analysis course. But I liked teaching them all.

W: While you were at Howard, did you remain active in your fraternity, the Alphas?

B: You know, I don't think I was. I had never thought about that, but no, I don't remember being active at all at Howard.

W: I understand that you became chair of the math department within three years after you arrived there at age twenty-eight?

B: Something like that, yes.

W: And that you remained chair until you left Howard in 1954.

B: Yes.

W: And I wanted to ask you, first about tenure at Howard, and then more about the chairship. So those are my questions. But first about tenure, how soon did you get tenure at Howard and what was that process like for you?

B: I think I got tenure after three years. But it was completely painless. I didn't ask for it or have anything to do with it. I was just sure that I would get tenure eventually, and I was surprised by how quickly it happened, but I . It was nothing like the same somewhat painful process that it is here, stress-inducing. No, it just was not like that at all. There were people at Howard who had been there for ten or fifteen years and who did not have tenure. But we were pretty sure that they would remain there as long as their teaching continued to be satisfactory, and it was. So, mostly,

getting tenure was a question of how much research you did. And, I was just pretty sure that I was going to do enough research that I would get tenure, and as I say, I was surprised that it came within two or three years.

W: And they just let you know, "By the way, you have tenure now."

B: Yeah, that's right!

W: Okay, good. So as chair at the I mean, I must say, twenty-eight is a young... I know times are different, but to me twenty-eight is a young age to be chair. My understanding is that you became chair when Dr. Woodard retired?

B: That's right.

W: So he kind of made a decision to step away?

B: Well, he had reached, maybe had reached mandatory retirement age. I think that's probably the way it was.

W: And Cox was still there?

B: Cox? Yes.

W: Okay. So what major projects and initiatives did you undertake as chair?

B: I don't remember. I would have said that I didn't do anything, but one of the later chairs told me that I had done some things when I was chair, but I don't remember. I think my idea was just to continue running the department, continue letting the department run the way Woodard had let it run. I thought it was a pretty satisfactory department.

W: So, the position of chair is more of a custodial kind of position than a "visioning" position?

B: No. My main job as chair was to make sure that all the classes ran. See, this was right after World War II, in 1947, 48, 49. A lot of students were coming in. So, we had more students in the math department than the regular faculty could teach. So, it was my job to find visiting faculty to teach those classes. And that was my main activity as chair was just to make sure there was a teacher in front of every class. And there were certain people out in the community that I called on to come and teach at Howard. And occasionally, there was a vacancy in the department or the dean approved a new position, so it was my job to go out and find someone for that new position. And I did that, I initiated two or three appointments while I was there.

W: Where did you look for the ones who were not full-time instructors? There were two appointments that I have here, but when you were just trying to get people in front of the classes, where did you look?

B: There were people who applied for positions at Howard. One of them was a full-time teacher at another institution.

W: So you can borrow people from other institutions?

B: Yes. See, there was a teacher's college right across the street from Howard, and sometimes we would get people from the teacher's college across the street to come in and teach.

W: As far as curriculum, when you became chair, did you make any changes in math major requirements? Or were there priorities that you felt needed to be more front and center academically for students?

B: I'm pretty sure that I made no changes.

W: Okay. The two people that I understand that you brought, that were brought on while you were chair, were William W.S. Claytor and Jeremiah Certaine.

B: That's right. Yes.

W: Were you instrumental in bringing them to Howard's math department?

B: Yes, indeed. Claytor was an extremely able man. He had been Woodard's student. And he went on to get a PhD under the same man that Woodard had studied under, J.R. Klein. There was some kind of tension between Claytor and Woodard. And Woodard would never appoint Claytor. Woodard appointed me when, looking back on it, I think he probably should have appointed Claytor. But when a vacancy came and I was chairman, I did appoint Claytor, got Claytor appointed.

Claytor had been at the University of Michigan as a post-doctoral fellow for several years. And I believe a serious attempt was made to appoint him there, but at that time, it was very hard for a black man to get an appointment in a white university. So, that did not go through, but a lot of people were in favor of it. Claytor was one of the best lecturers I ever heard in my life very clear and well-organized.

W: What was his area of expertise?

B: Topology. Same as Woodard, yes.

W: Was it very easy to bring him on?

B: Yeah, in the sense that there was no trouble getting him appointed.

W: And in terms of getting funding for his position?

B: Well, the funding came first. You would ask the dean for a new position and if the dean gave it to you, then you would go out and look for somebody.

W: It says here that not counting theses, Dr. Claytor was the first African American to publish mathematics research, and that he was awarded a Rosenwald fellowship. And that was how he did his post-doctoral studies at the University of Michigan.

B: Oh, Claytor got a Rosenwald fellowship! I also got a Rosenwald fellowship.

W: I know.

B: Ahh, yes, I had forgotten this! In 1941, Paul Halmos told me, "There's this black mathematician who is up at Chanutte Field, which is only thirty miles north of the University of Illinois. Let's go up and meet him." So I remember now, Paul Halmos drove me up and I met Claytor there. [Looking at brief bio of Claytor] I hadn't seen this before.

W: Did you find his ideas his mathematical inquiry very engaging or interesting? Did you have the opportunity to engage with him intellectually or were the areas of work so different?

B: Yeah! We had many conversations mathematical. Claytor was a perfectionist in a certain way. He was very interested in music and he let's see, he had a device for reproducing music. He had some fancy record player, and he would adjust it to get the tone just right. And he would finally adjust it to the point where he couldn't make any adjustments that would improve it as far as he could hear. But then he borrowed equipment from the physics department and let the equipment listen to it! [laughter]

And he kept adjusting it until the equipment couldn't hear any changes when he tried to improve it. That was the kind of perfection that he was looking for.

W: I wonder what kind of equipment it is that can do that.

B: I don't know. Also, we used to play golf together. Again, Claytor was a perfectionist. He had all these rules that he tried to follow in adjusting his drive and making sure he did everything just right. He was a pretty good golfer, but . Excuse me for a minute, I'll be right back, [leaves room and returns]

W: You know what, Dr. Blackwell, it's very interesting that they, in this writing it says they think you met or they've written that you'd met Dr. Claytor at an army base at Chanute Field, about twenty miles from Urbana, Illinois.

B: Yeah, that's what I was telling you!

W: So, Jeremiah Certaine, how did you meet Dr. Certaine?

B: I don't know, I'm a little vague about... I think he simply applied for a job at Howard and he came down for an interview, and we appointed him.

What I remember more about him is how he left. There was a man named J. Ernest Wilkins, you probably I don't know, did you ever hear of him? No, well, he got a PhD in mathematics at the age of nineteen, or something like that. Really precocious, very good mathematician, black fellow from Chicago. And Wilkins was working for some company, I believe it was the American Optical Company up in New York. And I had been trying to get him to come to Howard.

Wilkins came down to give a lecture at Howard and I talked to him about coming to Howard and he wasn't particularly interested. And he left, and very soon after that Jerry Certaine resigned and went to work for the American Optical Company. So what Wilkins had done, when he came to Howard, was to steal Jerry Certaine from us!

W: Yes, he recruited him.

B: He recruited him, that's right, [laughter]

W: Did Jerry Certaine work with you?

B: Not especially closely. I don't think Jerry was at Howard more than two or three years. Does it say how long he was there?

W: It says 1947 to 1951.

B: Yes.

W: So again, it seems like that first year that you became chair, you brought him, you brought them both right on to deal with that shortage of instructors.

B: Yes, that GI. influx was a big thing.

W: Did you find that more people were interested in hard sciences and mathematics as a result?

B: I don't know if they were more interested but they certainly worked harder. The students in 1947 and 48 were a few years older than the typical students. They were quite serious and hard-working. It was a pleasure to teach them.

By the way, the building facilities were so over-crowded that we had to extend teaching hours. The first classes started at seven o'clock in the morning, and we had Saturday classes. Then, after a few years, the enrollments dwindled a bit, facilities expanded, so we could give up Saturday classes, and seven o'clock in the morning classes. But I remember one fellow came to me and said, "May I have a seven o'clock class?" Now, why do you think he wanted a seven o'clock class?

W: I really don't know.

B: Parking. Traffic.

W: Oh!

B: Traffic was light driving to work at that hour in the morning, and parking was easy at that hour in the morning.

W: Did you oblige?

B: To some extent. I think the dean didn't want us to have seven o'clock classes.

W: So, I think also because as a chair you had taken on more administrative responsibilities, did you interact with other parts of the College of Letters and Science? In particular, what I'm trying to say, and I'm saying it in my usual roundabout way, is how did you navigate the administrative workings of the university to get what you needed for the department?

B: Well, Howard was a very authoritarian system. You just worked through your dean, you asked your dean for what you thought you needed and he would decide, yes or no. There wasn't a lot of political maneuvering, at least not that I knew about.

W: Who was your dean?

B: J. St. Clair Price was the dean.

W: Okay, did you get along well with him?

B: Yes.

W: Can I ask, what was his academic background or training?

B: I think his PhD was in education, but I'm not sure.

W: Did you have the opportunity to interact with president of the university, Mordecai Johnson?

B: No.

W: Did you interact with Frank Snowden?

B: Just a bit, yes. He was on a floor below me in Douglass Hall. How'd you happen to ask about him?

W: Well, to be honest, I was looking through this book, and I was trying to find deans. I didn't find your dean, the College of Letters and Science, and I was just trying to find any dean who was there at that time and he was the dean of the College of Liberal Arts.

B: Ah, yes. But I think he became dean after I left. Yes, he was just professor of classics when I was there.

W: So, you said your offices and your department were based in Douglass Hall?

B: Yes.

W: And you mentioned that you shared that space with a number of other people, including John Hope Franklin?

B: Yes, and E. Franklin Frazier, and Rayford Logan, and Sterling Brown, and Alain Locke, and there were a number of very distinguished people there.

W: Did you all have lunch sometimes? Or just, did a couple of you get together and have lunch across disciplines?

B: Not very often, no. No, we would just have informal conversations in the hallway, that's all.

W: Were there any of those that you just mentioned who you formed friendships with?

B: No, the main ones that I was friendly with were the other people in the math department.

W: And did you spend time together as individuals, or with their families?

B: Both. Claytor and I and our wives, and another fellow named John Doggett and his wife all used to get together socially. And Claytor and John Doggett and I would go out and play golf together.

W: Who was John Doggett?

B: John Doggett was a young instructor at Howard. He got his master's degree at Howard, and just stayed on as an instructor. But he got interested in computing and learned how to write programs for a computer. At that time, computers were just becoming popular. Lots of people wanted to use them, but very few people knew how to program for them.

So, John Doggett got a job at the National Bureau of Standards as a programmer, paying nearly twice what he was being paid at Howard. So he left and went to the National Bureau of Standards. And he didn't stay there very long because some private institution hired him at a much higher salary than that!

W: Wow. Now, during this time I understand your wife, Mrs. Blackwell was she working while you were teaching and consulting with the Office of Operations?

B: No, she didn't have a job while we were at Howard. But during that period, we had seven children, so it's not quite right to say she didn't have a job! [laughs]

W: Can you tell me when your first child was born?

B: Yes, September, 1945.

W: That was a year after you came to Howard.

B: Yes, we got married right after I came to Howard.

W: And you told me that you met, you actually met through . Oh, wait, I had it written down.

B: Well, we were introduced by...

W: Joe Pierce?

B: No, Herbert Boggs. You want me to tell you the circumstances? Okay. I was to teach an elementary physics course in the summer of 1944. And Ann came to register for the physics course, thinking that an old friend of her family named Eagleson was going to teach the course. Well, Eagleson had taken the summer off, he, sort of, owned the physics course, and asked me if I would teach it and I had agreed to.

So, Ann showed up to register for this physics course under Eagleson. And Boggs and I were registering people, and when she found out that Eagleson wasn't going to teach it, she started to leave. But Boggs persuaded her to stay and introduced me to her and told her that I would be a reasonable teacher. So, she took the course. But if hadn't been for Boggs's intervention, she would have gone elsewhere because she was interested in the course mainly because Eagleson was going to teach it.

W: So she was also a mathematician, you told me?

B: No, she was a chemistry student at that time. In fact, she was teaching chemistry.

W: And was it easy for you to go from math to physics and teach physics?

B: No, it was hard. And I think the way I taught the course, it was mostly mathematics

anyway. As I remember it, the students helped me a lot in doing the experiments. I got a few of the students who were pretty good at setting up the equipment and making the experiments go, so that's the way that all worked out.

W: Okay. So, your first child, can you tell me the name of your first child? I'm jumping a little bit to focus on family history.

B: Yes, that's all right. Her name is Ann Louise, my wife's name was Ann Elizabeth, and my first child was named for her Aunt Louise. I'll just go on down the list. Our second child is a daughter, Julia Madison. Julia was my wife's sister, and Madison was my wife's maiden name. Then, David Harold, named for me. Then, Ruth. I'm not sure where the name Ruth came from. Then, Grover, named for my father. Then Vera. I think we just liked that name, I'm not sure where that came from. Then Hugo. I liked that name. An old friend of my father's was named Hugo, hi fact, the man who had promised me a job teaching in the elementary schools in southern Illinois was named Hugo. And, there was a mathematical friend of mine named Hugo Steinhaus. Anyway, the name Hugo came up. Those seven children were born in about a nine-year period, something like that. Then there was a gap of several years, and my youngest daughter Sara was born then. I'm not sure where the name Sara came from.

W: And she was born here at Berkeley?

B: Yes, yes, she was born here at Berkeley.

W: Thank you for sharing that with me.

B: Oh, you're welcome. Sara is the one who told me [when she learned of this oral history], "Ooh, you get to make things up!" [laughs]

W: Well, you are totally at liberty to make anything up you like. What I really appreciated was the last time we met and you said, "Well, this is approximately so, you understand, it's approximately so!" I thought that was very good. Goodness, so you were really building and raising your family during those Howard years.

B: Yes, those were busy years.

W: And where did you live in D.C., your family, where did you all live?

B: The first year we rented a house. The second... Then for four or five years we lived in an apartment building in northeast Washington. And for the last two to three years we lived in a house that we bought in Washington. Again, in northeast Washington.

W: I'm not so familiar with Washington D.C. s neighborhoods; I'm having trouble placing what kind of neighborhood it was.

B: Well, the house was near the Maryland line. As I remember, about a thirty-minute commute from Howard University.

W: Did you drive in?

B: Oh, yes. [laugh]

W: I have to get my transportation questions in.

B: Yes.

W: Well, I have a question for you which is . And it's one of those questions that you may wish to throw out. But I wanted to ask you what a typical day was like while you were teaching? And I'm thinking when you became chair, I'm thinking around that time.

B: Nadine, I don't remember that, [laughs] It's possible that all my teaching was on Monday, Wednesday, and Friday and on Tuesday and Thursday I went to the Operations Research Office. That's possibly the way it was.

W: And maybe may have taught a 7:00 a.m. class on Saturdays.

B: No. [laughs]

W: I understand that it was while you were at Howard that you met Abe Girshick?

B: Abe Girshick? Yes.

W: I understand that that was also the time when you kind of fully made that turn.

B: Into statistics, that's right.

W: Can you describe that meeting for me?

B: Sure.

[end of interview]

INTERVIEW 4

MAY 2, 2002

W: Good morning, it's May 2 and I'm here with David Blackwell and this is interview number four. Last time when we stopped we were talking about your years at Howard. You were beginning to tell me about Abe Girshick. I wanted to ask you a few more questions about Howard.

B: Sure.

W: Do you remember your first day on Howard's campus when you first arrived there?

B: No, I don't remember when I arrived there to take up the job, but I remember the first day I visited Howard, I told you about that earlier.

W: You did, you did. This is kind of a small question, but what kind of car was that first car you bought for visiting all the colleges on the East Coast?

B: Yes, that was a 1939 Buick Century. It was a fairly big, fancy car. In my economic position, I shouldn't have had a car like that, but I did. I got that car while I was a graduate student at Illinois.

W: I was in touch with the archivist and finally he got back to me from Howard and he gave me this list of instructors and professors who were there while you were. And, you told me about most of the professors who were at Howard's math department the years you were there.

B: Yes. Some of them I don't remember myself anymore. Do you have any questions?

W: Yes, I do have some questions. I was just wondering if there was anyone there who you in particular, Professor Butcher?

B: Yes, George Butcher was a Howard University graduate who went on to get a PhD at the University of Pennsylvania, studying with J.R. Klein. The same one who had been the thesis director for Woodard and Claytor. So Butcher was the third in that series. He came back to Howard, yes, hi the 1950s and stayed there until he retired. He still lives in Washington, D.C.; he's long since retired, though.

W: What was his area of math?

B: Topology.

W: Topology?

B: Yes.

W: They all were topologists.

B: Yes, as I say, they all studied topology under the same man, J.R. Klein at the University of Pennsylvania.

W: And are there any of those instructors that you remember and wanted to say anything about? You talked to me already about Doggett.

B: Yes, Doggett and Claytor and Certaine.

W: Okay. You told me that you found them, many of them you found from the teacher's college across the street?

B: None of these came from the teacher's college across the street. These were people who had regular appointments at Howard, but almost every year, I had to go out and find other people for temporary appointments and they're not listed here.

W: Okay, they were supplementing the regular appointments.

B: Yes.

W: Can you tell me, what about a person made you think they might be a likely instructor? Like, good at math, good at teaching math?

B: Most of them I got because they were already teaching math. One of them I don't remember much about that.

W: Was there a sense that people were happy to have instructor positions in Howard's math department?

B: Oh, I think so, yes. Yes, I think that some of them came to expect that they would be called and they were rather hoping for an overflow of students so that it would happen.

W: Yes, for the job. Also, I think in one of your previous interviews, you described Howard University as every black scholar's dream.

B: Yes.

W: Can you speak to that?

B: Well, it was just . That's where many of the major black scholars were. That's where E. Franklin Frazier was; that's where Alain Locke was; Sterling Brown was there, and... Well, there was a ranking among black colleges that just everybody accepted, and Howard was at the top, and I think rightly so.

W: You've also described Howard as a very authoritarian environment, what does that mean?

B: Why, it means that you did what your boss said. The head of the math department really determined what courses the other people taught and determined what appointments to recommend. The dean of the Letters and Sciences approved, or disapproved, everything that the head of the department said. An instructor wouldn't dream of going over the head of the math department to the dean. The head of the math department wouldn't dream of appealing the decision of the dean to the vice president, just very hierarchical.

W: Okay, I'm going to jump just a bit, and before I go back to Howard, was that different from Berkeley?

B: Well, yes. Professors here are much freer than they were at Howard. For example, at Howard the head of the department determined the textbooks, and everybody if there were several sections, they all had to use the same textbook. But here, if there are two different sections of the same course, each instructor chooses his own textbook.

W: Granted, I realize that you were at Howard in really a different era, almost ten years difference, so I can imagine that there d be different . I mean of course they were very different places.

B: I suspect that it is quite different now, yes.

W: So in that very authoritarian environment, you were the chair. You were the authority, you were the one who was putting down the rules at Howard?

B: Yes.

W: Right. When you thought about basic math standards for undergrads, that was primarily who you were teaching, right? Undergrads?

B: Yes.

W: When you thought about basic math standards for undergrads, what was your frame of reference? What were you thinking about making sure people had before they went out into the world?

B: We had a fairly standard curriculum. Calculus came in the second year and upper-division courses came in the junior and senior year. And that was not very different from the University of Illinois. Nowadays, of course, calculus comes in the freshman year.

W: Were basic math standards higher then than they are now, do you think?

B: Oh no, basic math standards were lower then than they are now. Actually, even then, I would say that Howard University seniors took courses that would be given at the junior level here at Berkeley. So that Howard was about a year behind Berkeley. Level for level, though, the Howard students were just as good as the Berkeley students, it's just they were at a slightly different level.

W: How would you talk about a career in math with students at Howard?

B: I don't think I ever talked about a career in math at Howard. As I remember it, most of the math majors expected to be either high school math teachers or to get a job in the civil service in Washington. I don't remember, but I don't think that any of them had particular problems getting a job.

W: So you wouldn't say, "You, too, can become a professor of mathematics, like myself."

B: No, and I don't think most of them were interested in that.

W: It's a very uncommon occupation, becoming a professor of math, deciding to teach and think about, and be in a world of ideas.

B: Yes, and certainly when I started college it's not what I expected to do, but my focus just gradually shifted.

W: I think you also said you made classes much harder than they had to be. And you talked about this, I think, mostly when you were at Southern. You said that you made classes much harder than they had to be, as a new teacher. And by the time you were at Howard, you were no longer a new teacher, but do you still think you were making classes harder than they had to be?

B: No, I think I learned in one or two years what the appropriate level was.

W: As a result of your time at Howard, do you think that black educational institutions are very important?

B: Yes, I do. I think there were, and probably still are, many students, many black students, who are slightly uncomfortable around white students and they feel more willing to express themselves among other black students.

W: You were at Howard at a very, as far as I understand from looking at your work, at a very formative time in your work. And I know that much of your mathematical activity occurred outside of Howard. But in terms of your own development for yourself and for your family, do you think that that was a very important institution for you to be at? And the fact that it was all black was very important?

B: The fact that it was all black was not very important, but that was certainly a very important time for me, yes. My interests were basically formed at that time, while I was at Howard. It was not a handicap, being at a black school, at all.

W: In terms of the role that historically black colleges and universities currently play within the black community, can you talk a little bit about that?

B: I've been away from that a long time and things have changed so much that I don't really have any useful opinions on that.

W: You can always volunteer opinions, you know. They don't have to be useful.

B: I know, [laughs]

W: I want to ask you, you said that you still go back to Howard on occasion.

B: Yes.

W: Yourself and your family, or maybe just yourself?

B: No one in my family has been back since my daughter Sara moved from Washington. But, I go back to Washington on business occasionally, every few years, and whenever I go I always make a special effort to visit Howard, see my old friend Jim Donaldson, who was head of the math department for many years and is now dean of Letters and Sciences at Howard.

W: Did any of your children go to Howard?

B: No.

W: Did any of your children go to historically black colleges?

B: No, they're all Californians.

W: Did you want them to?

B: No, I left that pretty much up to them. I don't think they would have listened to me, anyway.

W: You've always said that your family raised you with the attitude that definitely you're going to college; college was definite.

B: Yes.

W: And did you raise your children in the same way, with the same attitude?

B: Well, yes, certainly, it was assumed that they would all go to college and they all did. Not all right away.

W: You know how at UC Berkeley there was a loyalty oath controversy?

B: Yes.

W: And I was wondering if there was ever a corresponding atmosphere at Howard University where people were concerned about being accused of anti-American beliefs?

B: There was very little of that at Howard. No, there were a few people on the faculty who were said to be Communists, but no one bothered them and that was just something interesting about a person, if he was a Communist. My dates are a little vague, I'm not sure just when [Joseph] McCarthy flourished. I guess it was around 1950, wasn't it? No, I don't remember that touching Howard, especially.

W: Yes, I think it was 1950 through mid-1950s. And I know that the loyalty oath here was 1949. It's funny because Al Bowker, in his memoir, in his oral history, says Stanford benefited from Berkeley's loyalty issues because key people left and went to Stanford. But we'll talk about that another time. Yes, and I think it was also during that time I think it was also about the time when W.E.B. DuBois was I guess he was targeted as a Communist and he chose to go away. People could basically have their political beliefs and weren't censured in the same way that they may have been had they, if they were at Berkeley?

B: That's right.

W: I wonder. Because my sense is that Howard also received some federal money, and maybe that wasn't the, kind of, crux of the issue as to whether or not you received federal money, but I kind of wonder about that.

B: It seems to me that Howard was just beginning to receive a lot of federal research money at the time I left, around 1953 or 54. Before that, I don't think federal research money was a big part of Howard's budget. Of course, Howard is a kind of federal university, you know. It gets its biggest appropriation from the federal government.

W: Were you instrumental in bringing some of the research money to Howard?

B: Just before I left, I did have a small research contract, yes. But I don't think it amounted to much.

W: Tell me about the research contract.

B: It was for the summer of 1953, to do research in game theory. One of my papers that I like best, "On Optimal Systems," has a footnote saying that it was written under the contract

W: While you were at Howard, do you remember hearing about the loyalty oath at UC Berkeley? From your friends in statistics?

B: Vaguely. Again, I was just not very political at that time.

W: I understand that it was while you were at Howard that you met Abe Girshick and fully made that turn into statistics away from . Not away from math, but it was somewhat of a departure. And I wanted to ask you how you met Abe?

B: How I met Abe? When I was in Washington at Howard, I looked all around Washington, D.C., for mathematical activities. And I went to a lecture, sponsored by the Washington chapter of the American Statistical Association, by Abe Girshick. And he spoke about Wald's work in sequential analysis and mentioned this equation called Wald's Equation. Well, that equation surprised me, it was new to me and I didn't even I didn't think it was true.

So I went home, and thought I had found an example showing that it wasn't true. And I wrote to Girshick, he was at the Department of Agriculture, explaining my counter example. Well, instead of just dismissing it, because my counter-example was wrong by the way, he invited me to have lunch with him.

W: So it was wrong but interesting in the way it was wrong.

B: Well, apparently so. Anyway, he invited me to have lunch with him, and I did, and we discussed it. And he got me interested in sequential analysis, and had various problems that he was working on, and I started working on some problems with him.

W: Can you tell me what Wald's equation is?

B: If you're in a gambling house where every gamble is fair, there are systems that you can use that are certain to make money. For instance, if you and I keep betting on a fair coin. I get a dollar every time it comes down heads, you get a dollar from me every time it comes down tails. If we keep tossing that coin, sometimes I'll be ahead and sometimes you'll be ahead. So eventually, there will come a time when I'm ahead. If I just wait until that time comes and then stop, then I stop while I'm ahead; I've won money from you. So, as I said, there are systems where you are certain to win money.

However, before you are ahead, you may be behind by a certain amount. Wald's equation says, among other things, that you could be a large amount behind before you win. In fact, the amount you're behind before you win has an expectation of "infinity." Or, yes you expect to be an infinite amount behind before you win, that's a consequence of Wald's equation. And, as I say, that seemed quite remarkable to me. Wald didn't put the equation in that way. The way he put it was, if you don't expect to be an infinite amount behind, there is no winning system, the gain is fair. So that's what the equation says, and I eventually proved it under more general circumstances than Wald did. I extended Wald's equation, but I started out by not believing it.

W: So you were saying you'll only be a finite amount behind?

B: That's right, the expected amount [pause] Yes, I first thought there could be winning systems where you'd be only a finite expected amount behind, yes.

W: So, in 1946, when you wrote this paper, "On an Equation of Wald," was that when you expanded Wald's equation?

B: Yes, that's right. And, as I say, my interest... I first heard about Wald's equation in this lecture by Abe Girshick.

W: I have a question about "On an Equation of Wald," but I want to get back to I'm torn, because I want to get back to Abe Girshick but I also want to ask you about this equation of Wald, can I ask you about the equation of Wald?

B: Yes.

W: So, how did you expand his theorem, his equation? What did it how did you expand it? In what ways? What did your paper say about Wald's equation?

B: Wald had talked mainly, maybe only, about systems where you make the same bet over and over. I extended it to the case where you could make many different bets, just as long as they were all fair. You didn't have to stick to the same game. In the case Wald considered, I found a new proof of his theorem. So I added something, all right, to it.

W: Yes. So let me just try and ask one question, which is about... In extending it to other games, were all those games still heads and tails games?

B: No, it could be any game as long as it's fair.

W: And when you say "fair," that word is confusing to me.

B: That's a technical concept, but it means that if you play it many times, your average gain will be near zero.

W: The average score of the game or the average game?

B: Your average winnings will be near zero.

W: You know, it's hard because I think when you translate the mathematical terms into that gambling scenario, which is what happens often. But I think that it's hard, even though in some ways it's often the most suitable kind of metaphor for talking about this, it still doesn't quite match up to what goes on for me.

B: Nadine, just think about Reno or Las Vegas and a casino, and you'll be thinking in the right way. You go in, there are all these games. You can bet on this one. You can bet on that one, and so on. Now it happens that most of them are slightly unfair, they have a slight bias in favor of the house. But now let's imagine that instead, they were all exactly fair. Now you could go in and you could still try out various systems. And the point is, if you have a system where the expected amount that you're behind is finite, then it must be fair. If each game is fair, either the system is fair or you can expect to be an infinite amount behind before you win.

As I say, the only technically difficult thing is the idea of "expected income," and that's... Well, if you study elementary probability, you learn about expected value.

W: Did you ever take these systems to an actual casino?

B: No. I hate to lose money, [chuckles]

W: What was the response in the mathematical world to this paper, "On an Equation of Wald?"

B: You never know that. I think I've done only a couple of things that got any kind of noticeable response, and that was not one of them.

W: No letters from fans of Wald who said, "That's wrong," or "That's right"?

B: No, no. [laughs]

W: Okay, so you met Mr. Girshick and you wrote him. He invited you to have lunch after reviewing, after seeing your contest to Wald's equation?

B: Yes.

W: And what was that like?

B: I don't remember in detail. We had a conversation, he mentioned some problems; I went away and thought about them and we had another conversation. I think he was going to go to Rand. Let's see, what was it? I'm sure it was through Abe Girshick that I got invited to go to Rand as a consultant. Maybe it was at the time he left the U.S. Department of Agriculture to go to Rand. My memory is a little vague, but it was right around that time.

W: I have some questions on Rand I wanted to ask you quickly. Abe Girshick, I know he worked at the Department of Agriculture. What was his professional work then, his work at the Department of Agriculture? What was he using statistics to do there?

B: I don't remember.

W: Did you get the sense that he was seeking out mathematical activity outside of his work place?

B: Oh, he was certainly doing mathematical statistics and welcomed the chance at talking with someone, sure. Just as I did.

W: Right, can you tell me his training? What was his training?

B: Yes, he got his master's degree in statistics at Columbia University, and later his PhD., also from Columbia, working under Abraham Wald, by the way.

W: Oh, very interesting!

B: Well, mathematical statistics was a fairly small group at that time.

W: I guess, for me, it just shows me his investment in trying to work with Wald's equation and engaging you around it. So, it just shows me something.

B: Investment?

W: Yes, when I say that I mean his interest, his avid interest, because he was a student of Wald's.

B: During the war, during World War II, there was a group at Columbia University called the Statistical Research Group. I was not a part of the group. In fact, I didn't know anything about it. But Abe Girshick was, and Wald was, and Jimmie Savage was, and several other people were. So that's how Abe got started, I guess, doing statistical research. And while he was at the Department of Agriculture, he simply continued that.

W: One thing that Al Bowker has said that sometimes, while you weren't part of that group, sometimes members of the group would talk with you about problems they were working on. Do you remember this?

B: Vaguely, yes. I certainly talked not only to Abe Girshick but to Jimmie Savage.

W: I have in that group, a Paulson?

B: Ed Paulson.

W: And Solomon.

B: And Herb Solomon, yes.

W: And Bowker.

B: Yes.

W: Do you remember what kinds of problems they were working on?

B: No, I don't

W: So what did that mean for you to leave what you had known around mathematics and move into statistics? In terms of your work at Howard, did that change anything at Howard?

B: No, it... My boss at Howard, Woodard, certainly encouraged me in this. He was just glad to see me active, doing things.

W: While you were at Howard, were there graduate students who were also very interested in mathematical statistics?

B: No. They were mostly interested in algebra or topology. I think I taught statistics, mathematical statistics, just once while I was at Howard. I was still learning while I was at Howard. I wouldn't have known enough about statistics to teach several courses in it.

W: Can you tell me if this is true? My understanding of statistics is that it's a field that grew out of actuarial and agricultural applications. And, so...

B: Well, those are certainly two of the sources. It also grew out of biological and medical problems. Statistical problems arise in many different fields, in engineering

W: I understand that it came of age I mean, when you became interested in statistics, it was in some ways still a very young discipline. Would you say that's true?

B: No, mathematically, it was a rapidly developing discipline. People were looking for the proper mathematical foundations for statistics. Neyman, and R.A. Fisher, and Wald all made really essential contributions to understanding what the proper mathematical framework for statistics is. So, I was lucky in that I happened to get into statistics at a time when it was very rapidly advancing.

W: I've been learning about systems theory. And a group of people who really focused they focused all of many different disciplines on trying to posit different systems or systems theory for different either naturally occurring phenomenon or social behavior, so many different things. And as I understand, Von Neumann was very active in that group or was one of the key members in that group, in terms of people who were thinking about that. And it was almost like this commitment to truth, and making sense of what seemed very, kind of, illogical and bringing it to a systems theory place and making it truthful and real, using, and sometimes often, mathematical statistical tools.

B: I'm not sure just what you're talking about. Von Neumann had very wide interests. I, myself, have never gone very far outside probability, game theory, and statistics.

W: Were you aware of other people in your circle who were pushing statistics in other directions, that had more to do with ?

B: My own interests have always been to look at a problem, and try to find some interesting mathematics in it. Never mind if it helps in solving the actual problem, but just is there any interesting mathematics there?

W: I understand. So when you in that case, when you thought about applications I understand that you were more focused on the theory. Is that true?

B: Yes.

W: I should ask, did you think about applications of your work?

B: No. I doubt if the things that I've done have any applications. As I say, my own tendency is to look at a real problem and see if it suggests any interesting mathematical problems. Now, usually, to get an interesting mathematical problem you have to change and simplify and distort the real situation a lot. So that the mathematics that you do isn't helpful in the real situation. It's helpful in the very simplified situation that you created.

W: I understand that, but I also see that in that time during the growth of statistics as a discipline, or during that time of a lot of activity, there were a lot of people who actually saw real world applications and funded people to work with the idea that there were real world applications. And I learned about that mostly in your Neyman book. Primarily it was agricultural, but I understand that that's different than what you were doing, but I'm just thinking about...

B: No, it's there, and it's big, but I'm not the person to talk about it.

W: So, for you, when you thought about game theory in particular, it was not a vehicle, in and of itself to take you to an application?

B: That's right.

W: When did you develop the Rao-Blackwell theorem?

B: Well, let me begin by giving you a little history. Up until about 1940, statisticians had studied how to deal with samples. But the samples were always of a fixed size. You'd go out and ask two hundred people what they think about this or you look at fifty specimens of something and take measurements, and then you'd try to decide what to do. Wald, about 1940, started studying a situation where you keep sampling until you decide that you've seen enough and then you stop. Well, it turned out that it wasn't clear how to adapt certain concepts that had been developed for a fixed sample to this kind of sampling, which was called sequential sampling. For instance, if you want to estimate the proportion of people with a certain characteristic in a population, you take a sample and you look at the proportion in your sample with that characteristic, and that estimates the proportion in the population. And it has a property of being unbiased. That means that if you do this many times, the average value of your estimate is near the true value. But it turns out that if you don't decide in advance how big a sample you are going to take, that using the sample average as an estimate of the population average is not unbiased always; it can give you biased results. So, statisticians started looking at how can you get unbiased results with sequential sampling where you don't decide in advance how big a sample you are going to take. Now, the simplest case is where you are trying to estimate a proportion is it 40 percent or is it 90 percent?

Three statisticians Abe Girshick, Fred Mosteller, and Jimmie Savage found a way to get an unbiased estimate from a sequential sample. But their formula was rather mysterious; people didn't really understand it. They could prove that it worked; but it wasn't clear what was going on. So I was one of those who was trying to understand their estimate, trying to understand why it worked. And I was walking along one day and all at once, the idea popped into my mind that, It's a conditional expectation. That's what their estimate is.

W: You were literally just walking along?

B: Yeah, in a casual way. And all at once that idea popped into my mind. Well, that's really all there is to this the theorem says that if given a sample, you calculate the conditional expectation of any unbiased estimate, you get another unbiased estimate which is better than the original one you had. And of course, that told people how to do unbiased estimation in sequential sampling, so people paid attention to it. But that's really all there was to it. I was just trying to understand their estimate and I was able to explain it.

W: Around what year was this?

B: I probably did it in 1946; it was published in 1947. Now, two years before that, though, in 1945, [C.R.] Rao published his thesis. And the same result that I had was one of many results in his thesis.

W: Where was he?

B: He was a student of R.A. Fisher's in Cambridge, England. He, himself, was originally from India. So, because in his thesis, the result was buried among several other results, people hadn't paid much attention to it, and I didn't know anything about it. But, when I rediscovered it, and used the rediscovery to explain the Girshick-Mosteller-Savage estimate, people paid attention to it. [pause] But once I discovered it, anybody could prove it. Any competent statistician could prove that yes, it's correct, if you take the

conditional expectation, you do get another unbiased estimate, and it's better than the one you started with. So, it's not a deep thing mathematically; it was just a new idea.

W: Were you in communication with Rao at all?

B: Not until three or four years after that.

W: After 1947?

B: After 1947, yes.

W: What was that like? What kind of communication did you have with each other?

B: I don't remember what our first meeting was like. I do know, though, that he's not especially happy that my name is attached to the theorem. And he shouldn't be, because he has the priority by two years. It's just that somehow when I did it, it got publicity.

W: So did you get a lot of responses to the paper about this theorem?

B: A fair amount. As I say, there were maybe twenty citations in the first couple of years. And for me that's big! [laughter] Most of the things that I do get zero or one citation.

W: As far as the work that you pursued after that, did it lead into more discoveries, as the result of this? Or was it the building block in other work that you did later?

B: No. No, I don't think I have. I may never have cited that paper after that.

W: It's interesting to me too, when you describe Rao, because Rao was a student of Fisher and you worked closely with Jerzy Neyman, who

B: yeah, Neyman and Fisher were antagonists in a way.

When I first started working in statistics, the mathematical community was pretty small. And we all knew each other or knew of each other. I was lucky to come along and get interested in statistics at the time when a lot of new things were being discovered and evaluated and explored. So twenty years after that, all the things that were easy to discover and understand and explain had been discovered and understood and explained. It's harder to find new, interesting things.

W: So, you said that you also spent some time at Rand, and that Jimmie Savage had, kind of, probably worked on bringing you there? Or no, Abe Girshick had also, had gone to Rand?

B: Yes, Abe Girshick and I worked together quite a bit at Rand, on various problems. And Jimmie Savage was there for at least one summer that I remember. I remember one big influence that Jimmie Savage had on me. He converted me to Bayesianism, so to speak.

W: You became a Bayesian.

B: Yes, shall I tell you about that?

W: I would love to hear about that.

B: Well, a Rand economist came in one day to talk to me while I was visiting there. And he said, "I need a number. I need to know the probability of a major war within the next five years." And he explained to me why he needed to know that number and it made a lot of sense. But, I turned him off. I said, "The concept of probability makes sense only in a long sequence of events under identical conditions." And the occurrence of a war in the next five years is a unique phenomenon and the probability is either zero or one and we won't know for five years. And he looked at me, and he said, "Thank you." He said that he had spoken with several other statisticians and they'd all told him the same thing, and he left.

That conversation bothered me. The man had asked me a serious, reasonable question and I had given him a kind of flip answer, off-hand, "Ohh." And a couple weeks later, Jimmie Savage showed up at Rand. I went in to welcome him to Rand, and I told him about this conversation I'd had with this economist. Then Jimmie started explaining to me about the subjective theory of probability. And how, of course, the economist's question made perfectly good sense and I'd been wrong to turn him off in that frivolous way.

What Jimmie said made a lot of sense to me because I had been thinking about the probability of single events all my life. And to me, that was the natural way to think about probability. I just regretted that it wasn't the correct way. And now Jimmie was explaining to me that it was the correct way. And, for me, that was a very important intellectual shift. The idea that probability does not apply just to events that occur under identical conditions but that the concept of probability applies to single events, unique events. By the way, the statistician who made this approach popular among statisticians was Abraham Wald.

But Wald knew that applying probabilities to single events would not be respectable among statisticians. So he didn't call them probabilities, he called them weight functions. However, in giving a general name to his approach, he did call them Bayes solutions. He called his solutions obtained by putting probabilities on single events, he called them Bayes solutions. And the Bayes approach has become fairly popular among statisticians, but it has gone far beyond statistics. Engineers, economists, and business people all use the Bayes approach very much. And it all grew out of Wald's work in sequential analysis.

W: Why do you think that approach is more popular with businessmen and economists? What about those disciplines make it more interested in subjective, looking at subjective probability? If that makes sense.

B: Well, I think that it's just that they are applied, and real people face single events, that you can't embed in a context of large sequence of events under identical conditions. Every event is unique.

W: So when you're tackling a problem using a Bayesian approach, how do you factor in those unique circumstances that affect the probability of something happening?

B: That's a big question, and a lot of people have studied that. It's no different from how you do it in everyday life. You make judgments all the time. Is it likely to rain? Should I buy this car or that one? You know, all kinds of uncertainties. And you're just asking, how do you decide on which is more likely?

So it kind of makes it a more textured and nuanced inquiry? With more dimensions? We're just talking about uncertainty. You understand uncertainty as well as I do.

W: But in terms of that original problem, what's the likelihood of having another world war in the next five years, approaching it from a "frequentist" approach would mean that you would say the chances are one or zero. Is that correct?

B: The frequentist would say that that concept doesn't really make sense. You'd have to put it in the context of, "If our sequence of identical worlds under the same conditions, and in some of them war would break out and in some of them it wouldn't," and you'd have to count the frequency. Nobody wants to do that. So that's why frequentists would simply say that that question didn't make sense.

W: Whereas Bayesians would say, "Not only did that question make sense "....

B: But it's an important question and you have to take account of everything you know, and how you take account of everything is not clear.

W: That's the part where I'm lost, is like how you take account of everything. That's the part where I'm trying to understand the approach.

B: I don't know that.

W: Okay, [laughter] Can you tell me a little bit about Jimmie Savage, who brought the Bayesian approach. Did he also study with Wald? Was he also a student of Wald's?

B: No. He got interested in statistics seriously, I guess, when he went with the Statistical Research Group. But I first met Jimmie at the Institute for Advanced Study in 1941. He had just finished his PhD, I guess, at Michigan. I think his work was in geometry. But again, he was vaguely interested in statistics, and he and I both sat in on Sam Wilks lectures that year. Then I ran into him a couple of summers later at Brown University. The government was trying to get all mathematicians, and other people, too, interested in war work. And there was a big session at Brown in applied mathematics. I went there for that session, and so did Jimmie Savage. We had a good many contacts during that summer. I remember something that Jimmie said that I thought was funny. He said, "Blackwell's theorem is: every suitcase can be closed." [laughs] You see, I had said that once. We were in a room and somebody was trying to pack a suitcase and put a lot of stuff in it. And I simply said, "Every suitcase can be closed." And Jimmie remembered that and quoted it back to me a month later as a theorem.

W: That's funny, so there wasn't a whole deeper statistical meaning in it.

B: That's right, [laughter]

W: Okay, what does that mean? I think sometimes that's true, actually. Well, in any event...

B: Have you ever had a suitcase that you couldn't close?

W: No. I work on it. I also travel very light, I'm one of the lightest travelers ever.

B: There you go.

W: Do you remember there being, when you attended this conference at Brown University where you saw Jimmie Savage, was there an excitement about the work that the military was proposing? Wartime work for mathematicians and statisticians?

B: I didn't feel that. Again, I was just there to learn mathematics. And one of the people that I had a fair amount of contact with that summer was Will Feller. He was a very good friend of my thesis advisor, Joe Doob. He was at Brown at that time. My memories are pretty vague, but...

W: That's okay. So what kinds of things were you working on while you were at Rand?

B: The most significant thing that I did at Rand well, there were two. One was duels, [laughs] Do you believe that?

W: I do, I've read about your duel theory.

B: Yeah, and that lead to an interesting class of games. You see, I thought of a duel as a game. There are two people approaching each other, and each one has a gun with one bullet in it. And you can fire anytime you please, but to make it interesting, after you fire you can't run away. You have to keep approaching. Let's say you're on a treadmill, you're fixed. The roads are moving this way and it's going to keep moving. And you can decide when to fire. Now, you have to think about two things, you see. The closer the longer you wait, the better chance you will have of hitting him. On the other hand, he might fire first. The question is, when should you fire? Well, that was the first problem that I posed and that has a fairly easy solution. And the other was something called "A Comparison of Experiments." Here I got the idea from some other people at Rand.

And again, it had a kind of a military origin. They called it "Comparison of Reconnaissances." Yes, you're going to play a game, but before you play the game you have a chance to get some information about what the enemy is doing. So you have, maybe, two spying systems. And the question is, which one to use? But then, I made it more interesting. The guns are silent. So there you are, if you know that he had already fired and missed, you would wait until you got right upon him. On the other hand, once you get pretty close, if he hasn't fired you'd better fire. So now the question is, when should you fire? And I solved that problem. And that got expanded into a general theory that other people did called "games of timing." And those games of timing all grew out of this simple duel. So, that was one of the things I worked on at Rand.

Now, it could happen that one system will give you more information than the other under all circumstances, in which case it's clearly a better system. And these fellows, [Henri F.] Bohnenblust, [Seymour] Sherman, and [Lloyd

S.] Shapley, developed a great theory for this in the case where there were only two hypotheses. And then I looked at their work, and thought of another, even stronger way to compare two systems. And I asked the question whether these two methods were equivalent.

In one direction it was clear. It was clear that my method was stronger than theirs. But the question was, was it really stronger? And I didn't think it was really stronger, and I was able to prove that no, that in a case of just two alternatives the two systems the two comparisons are the same. And then other people proved that, in general the two systems, the two methods of comparison are the same. The main interest is not in military things at all, but the problem grew out of thinking about military problems.

W: Did you have a sense, once you solved these problems, that there were people who understood your work and could, maybe, apply it?

B: Oh, there were certainly people who understood it and were interested in it. I don't think there have been I'm not aware of any applications.

W: For the solution to the dueling problem, was that actually a number? A time when should fire? Or was it a calculation?

B: Ah. In the case where the guns are noisy, you calculate an actual time when you should fire, yes. But in the case where the guns are silent, there is no one time you should fire, there are probability distributions over firing times. And so the solution is to find the probability distributions.

W: And for the problem on reconnaissance?

B: Here, it was just a yes/no question. Are these two methods of comparison the same? Are they equivalent? And the answer turned out to be yes, they are.

W: So while you are at Rand, was it a place where you came with your own problems and developed problems? Or was it a place where people say, "Here are some problems, can you work on them?"

B: No, it was closer to "Here are some problems, let's work on those." In a general context of anything that has to do with warfare is interesting. So, you start thinking about conflicts in general. As I say, a duel is clearly a very special kind of fighting, and this comparison of experiments was put in terms of comparisons of reconnaissances in a game theory context. And, of course, games are clearly conflict situations. So, that meant that Rand would be interested in them.

W: I'm trying to think of other kinds of things like warfare or games where you can basically have determined, specific variables and winners and losers that could make it a good arena for this kind of analysis.

B: Well, economists have all kinds of problems like that. There are whole journals devoted to that. This one called Games and Economic Behavior.

W: Yes. I have a copy of this one that's dedicated to you, actually, I think.

B: Oh, that's right, there was one.

Wilrnot: So who was deciding what kinds of problems that the group of people at Rand would work on? I understand that it was everything that was warfare, but maybe there were more specific problems that people brought in and said, "Work on this."

B: Well, Rand had a regular staff of permanent people who had problems they were working on. And you were welcome to help them work on their problems or find your own problems. The atmosphere at Rand was extremely informal. I was working with a man named Olaf Helmer. I worked quite a bit with him, and one day, he told me that we couldn't meet that morning because he had to go to a meeting of group leaders, or something like that it was called.

And I said, "Olaf, I didn't know you were a group leader. Who is in your group?" And he said, "You are, for one!" [laughs] So that's how informally it was organized, see. I had no idea who my boss was, there.

W: Wow, that sounds like a very interesting atmosphere.

B: Oh, it was a great atmosphere.

W: So, it was a place where you were more focused on warfare and not so much thinking about economic industry, engineering . It was mostly just warfare?

B: Yes. But, of course, the military is interested in all kinds of things. I mean, name something that doesn't have an application to warfare.

W: My first thought was agriculture, then I had to withdraw it.

B: Yes.

W: Do you remember who sorry?

B: I was just thinking, one of the things that Olaf Helmer, my boss, was interested in was what he called Delphi. The idea was, how can you take a group of experts, and ask each one what he thinks about something, and then combine all the opinions and come out with an opinion that is more reliable than that of any individual expert. For instance, if you wanted to know how likely a war is in the next five years, get five experts to talk about it and tell you what they think and then try to combine their individual opinions into one opinion. Olaf was interested in finding methods of combining expert opinions.

One example he was interested in looking at was horse races. Every day, in the newspapers, several horse racing experts would write their opinions. You know, who's a good idea for this race, and who's a good longshot for this race, and so on. So, Olaf tried to see if he could find a consensus among the experts that would be more reliable than any one expert opinion. He wasn't able to do very well on that, but I remember that someone wondered why he was subscribing to all these daily racing forms! [laughs]

By the way, one thing that we did on that that I remember was this. We tried to see if we could combine expert mathematical opinions into an overall mathematical opinion that was more reliable than any one expert. So, some of us got together and devised a long mathematical questionnaire. Now, these were questions that could be answered if you had enough time. But, we gave the experts only a very short amount of time on each question. "Ms. Wilmot, just off-hand, what do you think about this, and this, this, and this." And, of course, we, having lots of time, knew what the answers were. So we tried to combine all the individual opinions into a group opinion that was more reliable than any expert. We had various ways of doing it. And the most reliable consensus that we had was able to get the right answer 81 percent of the time! However, it turned out that there was one individual who was right 80 percent of the time. So alas, you didn't really need a consensus, all you needed was to ask Lloyd Shapley because he was almost as reliable as the best consensus we could find, [laughter]

W: When you say "we," who is "we?"

B: Olaf Helmer, and I, and Abe Girshick, and a fellow named Alex Mood, and maybe there were two or three others.

W: So, instead you just found the one expert.

B: Found the one expert, yes.

W: And rendered all the rest of you, kind of .

B: Superfluous!

W: Was that your first time in California?

B: Yes.

W: Did you stay pretty close to Rand, or did you kind of get out a little?

B: I stayed pretty close. I think we went down to Tijuana a few times, but . Sometimes, I came to Rand just for the summer and my family stayed in Washington. But once I was at Rand for six months and my family moved out.

W: Do you remember them liking California?

B: Yes, my children were pretty small, but .

W: I have a couple of more questions on the Bayesian approach.

B: Yes.

W: I read an interview with you where it said that you were surprised that that school, that approach was still flourishing. Because, initially, when you, kind of, first embraced it, it seemed to you that it was dwindling and there weren't very many people who were adherents to that approach.

B: Yes. Now, statisticians seemed to have advanced beyond both the Bayesian and the frequentist approach. Such questions are not much discussed among statisticians anymore. And as I say, engineers, and economists, and biologists have pretty much gone to the Bayes approach. Especially engineers and economists.

W: My understanding, though, is that Berkeley has always been the stronghold for a frequentist approach.

B: That's right, and still is.

W: In fact, when I was trying to research colleagues who I could speak to about you to learn about the Bayesian approach, there were very few people who I could find who were also Bayesian in their approach.

B: I think there's just one. A fellow named Kjell Doksum is a Bayesian, but he and I haven't worked together at all. But, I think he's the only one.

W: And I'm kind of just going to jump ahead a little bit, but I wonder what that was like for you when you came to Berkeley, and everyone was frequentist and you had this Bayesian approach?

B: No problem whatever! [chuckles] When I taught statistics, I taught it in a Bayesian way, and other people taught it in a frequentist way.

W: In particular, I was wondering about when you interacted with Jerzy Neyman. And he was not Bayesian at all. Did that ever come up between you two, did you ever talk about it?

B: No. And he was not completely anti-Bayesian. But, no, he and I never discussed it.

W: I'm going to ask a lot more questions about Neyman later on, but not now. For me, coming from a place where both of these things are fairly new ideas for me, I think the thing I'm trying to understand is when you take a frequentist approach or a Bayesian approach, do you end up at different places as a result of those approaches? Or do you travel different roads and arrive at the same place?

B: Usually, you arrive at the same place. If there's a lot of data, a lot of information, then you will almost certainly arrive at about the same place, yes. But when there's very little information, then... Well, when there's very little information, a Bayesian would say that the frequentists don't arrive anywhere. And the Bayesians have a lot of uncertainty, but...

W: Okay. I wanted to just look at some of these problems. How are we doing for time? Are you okay for time?

B: Yes, I can stay a little longer.

W: Okay, good.

B: Yes.

W: I guess I should start off by asking if there's any on this list here that you want to talk about? Because for me, a lot of these are the things I don't know about, I tend to pick on the basis of language and, like, what has the most beautiful language associated with it.

B: Yes, all right. Well, you have marked them in yellow. That's with Richard Bellman, and "Some Two Person Games Involving Bluffing." Yes, Dick Bellman and I noticed that in a lot of games, card games, for instance, in our theoretical, continuous version, no bluffing was needed. Whereas in actual card games, people do bluff all the time. So, we asked, why, theoretically, did it come out that often bluffing is not needed? And, it turned out that if you have a large number of cards, that you can replace bluffing by something that that you don't need to bluff.

And let me give you an example. Suppose that when you get... Suppose that no matter what card you get, you should take one act with probability one-half and the other act with probability one-half, bluffing, you see. And what you can do is this, you can just say . You can divide the cards mentally into two groups and say, "If I get a card in this group, I'll bluff, if I get a card in the other group, I won't bluff." Now, since you don't know which card I got, to you that looks just like bluffing. That's all. And we showed, mathematically, that you can replace bluffing by this splitting the cards into two groups and deciding to take one act if you see a card in this group, another act if you see a card in the other group.

In other words, you let the cards you're dealt do the bluffing for you. So, formally, it doesn't look like bluffing because bluffing involves you doing one act of probability with a certain probability and another act with a certain probability in the same circumstance. Well, you're not doing that, but it has the same effect.

W: And that was with Richard Bellman?

B: That was with Richard Bellman.

W: Where was Bellman?

B: Bellman was also at Rand.

W: So, when two people are working on something together, do you just kind of trade back and forth the ideas? What is that process of working together on something like?

B: Somebody asks the other person a question, and then you both start thinking about it. And the next day, maybe I talk to you or you talk to me. Whoever has an idea talks about it, and it goes back and forth like that, and sometimes you're in the same room together, talking about it together. But, as often, you're thinking about it separately and then coming in to say, to tell the other person what you have discovered. And finally, you either give it up as hopeless, or you decide, well, "We found something interesting here, let's write it up." You know, this is a paper that I wrote with Dick Bellman, I wrote another paper with Dick Bellman on moment spaces.

Let me tell you something about that paper on moment spaces. Dick and I discovered this method of solving certain problems, and he wrote it up and sent it in to a mathematics journal. And, his writeup was about thirty pages. The paper came back with almost complete rejection. And Dick had written the paper, and he gave me the returned paper with the rejection, "What do you think we should do?" And I said, "Let me rewrite the paper and I'll send it again." So, I did. I believe that my rewrite was nine pages, and I sent it in and it was accepted! And Dick, who is a year younger than I, walked over and patted me on the shoulder. "My boy, you have learned how to write mathematical jargon; you will go far." Because I had compressed it, you see, and condensed it. Dick's writeup was rather discursive, really trying to explain to people what was going on in a way that it would look interesting. I was just trying to make it short and correct. And, it turned out that the short, correct, unreadable version was more acceptable than the longer one.

W: What's a moment space?

B: I don't know, I can look it up.

W: Okay, it's just that to me, that's just poetry. For me, because I don't have the statistical background, there's many things here that just sound very poetic to me, but moment space, I wonder what that is.

B: Did you ever take elementary physics?

W: Yes.

B: Well, they talk about moments in physics, that's the kind of moment this is. [pause] I don't know a simple way of describing what moments are that it's a little complicated, yes.

Let's see, you marked this paper with Nick Smith and Walters, and Brooks, "The Theory of Value and the Science of Decision." At the Operations Research Office, Nick Smith was my boss at the Operations Research Office. I don't remember what's in that paper at all.

With Arrow and Barankin, "Admissible Points of Convex Sets." What was it? You have a set of points, and if x . Let's think of each point as describing how much money you get. Let's just talk about the two of us. If we do something, you'll get a certain amount of money and I'll get a certain amount of money. We plot it as a point. This is how much I get and this is how much you get. There may be something else we can do where I get more money and you get more money. If there is, than this behavior is not admissible, because there's another one that's better for both of us. Now, you look at the set of all points that are admissible. So, if we're at an admissible point, there's nothing we can do that makes both of us better off.

Now, in two dimensions it turns out that if you have a sequence of points, and each one is admissible, and if they converge to a limit, then that limit is also admissible. But in three dimensions, that's no longer true. You have a sequence of good behaviors, but the limiting behavior is not good in that it can be improved on. And that's essentially what is in this paper. It's just an example which surprised me because, since it was true in two dimensions, we thought it would be true in three dimensions, but it's not.

W: It was interesting to me that you used the money to describe the favorable action.

B: Yeah! Well, everybody understands money.

W: Yes, that's true.

B: And, "On Optimal Systems," 1954, you also marked that one. That was one of the best papers I ever wrote. It's a short paper, and I wrote it because I needed the result for another paper I was writing. But, in writing the paper, I found a new approach to putting a bound on certain probabilities. And the approach was later rediscovered by Jimmie Savage and Lester Dubins, and they wrote a book about it on gambling systems. But I already had the idea back here in this "On Optimal Systems" paper.

Roughly speaking, it was like this. You have a particular system let's say a gambling system you want to know how good it is. Instead of studying this particular system, you find a class of systems and you ask, "What's the best system in this class and how good is it?" Well, if you can find the best system, and put an upper bound on how good it is, that answers the original question that you were interested in, and more. And so that was my approach. I was really interested in a particular system, kind of situation, but instead of concentrating on that, I looked at all possible systems and found out and put an upper bound on them.

W: How did you determine the class of systems? The pool, the larger pool? Was it for similarity to the first system?

B: Yes. That's a good question. You try to think of a class that you know how to work with. And, maybe, the first class you try doesn't work, in fact it probably doesn't. So you're interested in this thing, you try to find something bigger that you can understand. You try that bigger thing, it doesn't work. You try this bigger thing, it doesn't work. You try another bigger thing that does work. That's sort of the way it goes.

W: When you look at these, where do you remember to be your very important papers?

B: Papers that I like.

W: Papers you liked, but papers that, like, were important to you for a reason. They marked a place in your development, [tape interruption]

B: No, no, it's fine. What I can tell you best is papers that I've liked, papers that I think are good papers, that's what I'm telling you about.

W: That's good. I guess I have a question on "On Optimal Systems," that last paper you were describing to me. What brought you to that point where you were asking those questions? [tape interruption]

B: If I don't write things down, I forget.

W: I understand. Well, I have about thirty more minutes on this maybe it's not thirty more minutes, maybe twenty-five more minutes on this CD. How we doing for time, are you okay?

B: I would like to go home, [laughs]

W: Okay. I'm going to start next time with this question of asking you "On Optimal Systems," why you were asking those questions about

B: Yes, I understand

[end of interview]

INTERVIEW 5

MAY 9, 2002

W: You were saying you felt that the mathematical mind does operate in a different way?

B: Well, let me give you an example. A man named Merrill Flood was once asked by some tax commissioner in the state of West Virginia, I believe, what to do about evaluating property for tax purposes. People were constantly complaining that their taxes were too high. And Merrill Flood's suggestion was, let each person evaluate his own property but then give the state the option of buying the property at the specified value. Now, that's sort of a self-enforcing plan, you see. You won't evaluate your property too low. Otherwise the state might buy it. And, of course, you won't evaluate it too high, because you would be paying more taxes than you ought to. Now, if I hadn't known who proposed that method, I would say that a mathematician did that. It has a kind of neat, self-enforcing simple-mindedness that would appeal to a mathematician. So, I do think there's something different about the way mathematicians think. But in their behavior otherwise, I don't see that they're different from other people.

W: Yes, I recall this article that was in the Berkeley alumni magazine. It focused on eccentricity and social behavior.

B: Yes, well, of course no one thinks that he's eccentric, [laughs]

W: Well, that's an interesting question; do you think that other people would say that you're eccentric?

B: Oh, I don't think so. [thoughtfully]

W: Okay, good. I just wanted to ask you some general questions about math. Many people talk about, and specifically many mathematicians, talk about their work as creative work. And I just wanted to ask you, has it been a creative process for you?

B: Has it been a creative process? Well, certainly, I try to study and understand mathematics. And sometimes, in trying to study and understand it, you come across a new way of looking at something, a new way of organizing something. So, you could say that that's being creative. You don't start out trying to be creative, though, at least I don't. You just start out trying to understand something.

W: And what does success feel like? What does that look like when you've understood something?

B: Oh, you think, "That's beautiful, let me go and explain it to somebody else!"

W: If we can choose one, maybe, one problem to talk about in trying to think about how you would describe your approach to solving problems. If you think about your approach as its specific to you, how would you describe your approach to solving problems?

B: I don't know. You just look at the problem from all different aspects and I couldn't begin to describe it.

W: I'm going to come back to that question, because I think in other conversations we've had you've kind of talked about . You said, "Nadine, my approach is that I look at real life and I look for interesting math, and then I bring it and bring it into the math." And there's other kinds of things you've described to me, and I think I'm going to ask that question differently, because I feel like we've talked about it some. And I think maybe I framed it too generally.

One of the things that strikes me about your career is the range of your contributions and your participation in different areas ranging from Bayesian statistics, probability theory, game theory, set theory, dynamic programming, information theory, decision theory, approachability theory, merging theory these are all the things that I've learned that you've touched.

B: All those are titles of articles that help people guess what the paper is about. They aren't really that different, for the most part. Practically all my work has involves probability and uncertainty. Much of it is about what to do in the face of uncertainty. A few things that I have done are about just the foundations of probability, but mostly it's studying specific probability problems. And since that was what my PhD thesis was about, I haven't really deviated very far from that. It's just that probability occurs in many different kinds of situations.

W: In some ways that addresses my question, which is about what is the common thread in a lot of work that you've engaged in these different areas.

B: Yes, it is probability, uncertainty.

W: At the same time, while that was kind of the core from which you were operating when you thought of different areas, how did you move from one area to the next? Did something strike you and then you moved to bring your tools to that area? As it interacted with other people?

B: That's a good question. I told you that I got interested in statistics through talking to Abe Girshick. Well, now I'll tell you how I got interested in dynamic programming. I was at Rand with Abe Girshick, and Kenneth Arrow, and some other people. And we were very excited about sequential analysis and sequential decision problems. And Dick Bellman was around there, and noticed all this excitement and energy, and asked me what we were talking about. So, I started explaining sequential analysis to him, how you look at a sample and you decide whether it's time to stop sampling and take some action, or whether you should go out and get more information some of the general theorems about that. And Dick saw that the ideas had applications far beyond just this simple sequential decision theory. So he sort of invented a new field and called it dynamic programming. And I, in essentially developing his work, found several new theorems that were quite general. So, the way I moved there was simply to try to understand what Dick Bellman was talking about.

And I think, generally, I have never just looked around and said, "Now, what's new that I can start working on?" I've been trying to understand what other people are doing. And in trying to understand that, sometimes you see something new that they haven't noticed. So, in general, I have not been "doing research," quote. I've been trying to understand other people's research, and in trying to understand it, I sometimes find something new.

W: That distinction is really important to me when you say that "research versus understanding," because understanding is just much more of an independent endeavor and it's just something that one does, almost, for your own delight, for your own satisfaction.

B: Right.

W: And, it's something that some people do because they're compelled to do it. They have no other choice; they have to find out.

B: [pause] Yeah, right. To some extent, I'm motivated by teaching, or I have been. My job was to teach people and help other people to understand things. Well, if you're going to teach something to somebody else, you have to understand it really well yourself.

W: That's really true. And then you have to kind of field other people's learning styles. And in fact, I had this conversation with Professor [Roger] Purves.

B: Yes.

W: A graduate student of yours who is now a professor here.

B: Yes, yes.

W: And one of the stories that he told about you was about you teaching a class to engineers, and it was a class of graduate students who were both engineers and statistics and math students. And how you were able to really bring the material to reach people wherever they were, and how important that was to him to learn about that.

B: Good. I'm glad you talked to Roger, he's an interesting fellow.

W: Yes, he seems to enjoy conversation. One thing he was telling me about was how Americans aren't as good at conversation as they could be. I was hoping he didn't mean me! [laughter] But the other thing he said about you is that you where other people are really concerned with making sure that, you know just would disdain teaching

people who didn't have certain standards, that you were very committed to reaching people wherever they were. Regardless of if they had gotten certain standards. You were really committed to their understanding.

B: Well, as I said, I've always regarded teaching as my job. And, I've enjoyed it and try to do it well.

W: I just want to go back to this question when I was thinking also about how you've been in many different areas when you, kind of, started off in one area and moved into mathematical statistics.

B: Yes.

W: And I've been framing it in this way of, well, your departure from math and to mathematical statistics. And I finally realized that I should ask you, was that actually a departure when you met Abe Girshick and started?

B: No. All my life I've been proving theorems, all my professional life. And that's just a part of mathematics. Practically all my work has been published in mathematics journals.

W: And is the distinction, for you, between statistician and mathematician, is that a really relevant distinction for your own self?

B: I never try to label myself, no.

W: Do you think it was important in the field? That distinction?

B: Each person is unique. And trying to put people in pigeon holes is, to me, not particularly helpful. Different people have different interests. I learned a lot, for example, from the work of Claude Shannon, who was an electrical engineer. Mathematical ideas occur wherever people are thinking about things. The idea, for instance, that led to sequential analysis, I think came from a man who was not a mathematician or a statistician at all.

Someone had told him to take a sample of a certain size, and if he found more than this many defectives, do one thing; if he found fewer defectives than that, do another thing. Well, this man started taking the sample. You're supposed to take a sample at, say, two hundred. And by the time he had taken a sample of one hundred, he had already found the required number of defectives. So, he reasoned that he didn't need to look at the others, because he already knew what he was going to do. But, the theory had been developed on the assumption that you looked at all two hundred. So, then people recognized that he had done the sensible thing and the question was what was the theory that justified what he had done? And that's how Wald, I think, became interested in sequential analysis. So, ideas can come from anywhere.

W: And what did you remember finding very beautiful about his work, or interesting?

B: Well, of course, he invented the whole subject of information theory. And the one idea. Or one great idea that persists through his work is that what you learn when you observe something is the logarithm of the probability of that event. That's just a simple idea that has all kinds of consequences.

W: Consequences like what?

B: For instance, that the amount that you expect to learn when you observe a random variable X is given by the formula: $H(X) = -\sum p(x) \log(p(x))$, so that, if Y is a function of X , then $H(Y)$ is less than or equal to $H(X)$.

W: I have a broad question, which is what would you say were the very important breakthroughs that occurred in the field of mathematical statistics through the years?

B: Yes, that's a broad question and it's not the kind that I'm any good at answering.

W: Okay. In an interview, and in fact it's interesting, because I took that question wholesale from an interview that was done with you. And your response there was you'd mentioned Charles Stein's discovery that \bar{x} was inadmissible.

B: Oh yes, \bar{x} .

W: The \bar{x} . And then Herb Robbins work on empirical Bayes.

B: Yes.

W: And of course, you mentioned Abraham Wald. You said Abraham Wald's ideas have pretty much dominated this early part of the century.

B: Yes, that's right. That's no longer true, but it was certainly true at the time I said it, I think, [pause] I'm not very good at seeing the big picture, [pause] I know mainly what has interested me over the years.

W: Can you speak to that? Or we've been talking about it all along?

B: We've been talking about it all along, [laughs]

W: The reason why I thought to ask that question again once it's already been asked to you is because I was thinking about how things have changed since you'd answered that question. And, I believe that interview I think it was about, maybe, fifteen to twenty years ago. So, I just wanted to ask you if there are any trends and changes that have occurred in the past twenty years that you're paying attention to, and think are significant, and marvelous, and interesting? Specifically in mathematical statistics.

B: Nadine, the field of statistics has changed a lot. But I haven't changed; my interests

haven't changed. I'm still interested in the same kinds of things that I was interested in twenty-five or thirty years ago. That's why we need new people to come along and pursue new directions. If I wanted to change direction now, it would be very hard. So, I haven't really paid too much attention to these new directions.

W: While I know that you are professor emeritus, you are retired, do you continue to do mathematical work?

B: Every day.

W: Can I ask you what you're working on now?

B: I'm trying to understand the difference between sampling with replacement and sampling without replacement, very old idea. How much of a sample do you have to look at before you can tell the difference between sampling with replacement versus sampling without replacement. I was led to that by a more general question. But, if I can understand that specific, special case, I'll have a better hold on the more general question.

W: What was the more general question that led you to this?

B: Suppose something quite unexpected happens in a Markov process, what can you say about the Markov process, given that. I almost have an answer to that question that I like, but I don't know exactly how to formulate it. And, looking at this special case of sampling with replacement versus sampling without replacement may tell me what the general answer is. I've been working on that for three or four months, now, off and on. [pause] I'm not sure that I'll ever find anything worth publishing, but maybe.

W: Is this something that you were working on with someone else?

B: No, but I'd be glad to talk about it to other people, if I find anybody that's interested.

W: Is there a set time every day when you sit down and work on this problem?

B: No. I'd say I spend an hour or two every day working on it, just whenever it's convenient. Sometimes when I'm just driving home from work I'm thinking about that problem.

W: It's on your mind.

B: Yeah.

W: That's really interesting. Well, I want to ask you a question. In many ways, I see you as someone who has seeded new areas for other people, and when I say "seeded" I mean you've planted seeds in many different areas that people have come and built upon. Does that sound right?

B: People have quoted my work from time to time, but I wouldn't describe it as building on it. Extending it, perhaps.

W: In particular, in the area of economics, people have really taken a lot of the work that you've done around game theory and decision theory and extended it and built upon it.

B: I understand that people have used some of my work, but I don't actually know what they've done.

W: All right, [pause] I see in some of your work you partner with other people. Do you find that to be a very important and rich process?

B: Well, yes. It's important to talk about your ideas to other people, and to try to understand their ideas. No two people look at things exactly the same way. Sometimes just an offhand comment that someone makes can send you in a new direction that you wouldn't have thought of otherwise. And it's not just from talking to people, it's also from reading their work, but people sometimes talk informally in a way that they would not write. For example, if Lester Dubins ever comes into my office I'm going to tell him about this problem I'm working on. Because I think he might be interested.

W: Can you talk a little bit about the work that you and Lester Dubins have done together over the years?

B: Let's see, we've done a number of things. Some of them I no longer remember.

W: The question I'm trying to ask is actually about how has the ways that he thinks differently about things and the way that you think differently about things made for exciting math?

B: Well, there's one technical concept that we simply disagree about the importance of. It's called finitely additive measures. He thinks they exist and I think they don't. And, he and Jimmie Savage wrote a beautiful book about gambling in which finitely additive measures, they think, played an essential role. But I found that it's possible to do the same things they did without using finitely additive measures. It is true that my approach is somewhat more complicated than theirs. But, I thought it was important to do the things that they did without using the concept of finitely additive measures.

W: Why did you think it was important?

B: Because finitely added measures don't exist! [laughs]

One thing that Lester and I did together, and it's been studied and quoted by several people, is what's called "Merging of Opinions." And, the idea there is, if two people start out with different ideas about what's likely to happen, if they observe the same thing happening, then their opinions about the future will come closer together as they have more and more common experience. And ultimately, unless it is clear from the beginning that they may never reach agreement, they will reach agreement. So you have to say, what does it mean that it's clear from the beginning, if they'll never reach agreement?

And what it means is this, if there's some event that you think is impossible but I think is possible, then if that event occurs we'll never reach agreement because once you think something is impossible you must continue to think it's impossible. Whereas, once . . . If something is going to occur, if I ever thought it was possible, I will think it's more and more likely as time goes on. So, we'll never agree. That question has been of interest to many people. Will people observing the same events come to agree more and more about the future? We found an interesting answer to that question.

W: What's that?

B: Yes, if they agree initially on which events are possible.

W: You also worked with David Freedman?

B: Yes.

W: I know that you've worked on a series of problems with David Freedman, but I'm also wondering what is it about the way that he thinks differently and the way that you think differently that is productive?

B: It's hard to say, exactly. Let me remind myself of what we have worked on. I don't remember much about it anymore. What I do remember is that he did things that I could never have done. I don't remember very much.

W: In the very brief conversation that I had with him he said that he came here largely to work with you.

B: Oh! When I was chairman of the statistics department, David Freedman had just completed his undergraduate work at, I believe, McGill, and applied to come here as a graduate student. I tried very hard to get him to come here as a graduate student, but didn't succeed. He went to Princeton instead. But, when he got his PhD, then I, as head of the department, offered him a job here and he came. So, I'm very pleased that I recognized very early that David Freedman was going to amount to something.

W: And that, actually this is just an aside, but that was something that I determined from talking to Professor Bowker, Al Bowker. When we get into your Berkeley time, I was going to spend a lot of time on the chairmanship. And he said, "Oh Nadine, that's not really a big deal. That's just making sure there's quality people here."

B: He's right.

W: Yes, so he saved you from exhaustive questions about being chair.

B: Yes! [laughs]

W: You collaborated with a lot of people over the years. But, someone I'm also thinking of is Leo

B: Leo Breiman? We've worked on information theory together.

W: Yes.

B: Leo has a very original mind. It's different from everybody else's, [pause] As I remember it, in our collaboration he would suggest things that I could never have thought of. But then, I was better at the details of proving them than he was, somehow. His mind would go in wild directions, and come up with all kinds of things.

W: Are you thinking of something in particular?

B: Ways of evaluating channel capacity was one of them. He and I and Aram Thomasian worked on sending messages through channels when you're not really sure of the properties of the channel.

W: That sounds like an amazing idea.

B: Well, no, that's the way real channels are. You're never sure, exactly, what the properties of the channel are. One thing that we discovered that's never been used as far as I know is this; if you want to have reliable transmission for absolutely every channel in a certain class, you need to use random codes. But as far as I know, people don't actually use random codes. I'm not sure, but I think they don't, [pause] But they're useful in proving theorems, [pause] Okay?

W: Some have said that mathematics is a very lonely profession because so many people don't understand it. Many people talk about mathematicians whose wives and children do not understand what they're doing at their work or just, you know, other people in the community and, I wanted to ask how has that been for you?

B: Well, it's important to work with other people. And I've always worked a lot with other people. One of the ways that I've worked is this. I start out with a problem and I work on it for a while. If I can solve it, fine. If not, I go pestering other people. And I keep doing that until somebody solves it. I've done that several times.

W: Sounds like we really need to get Lester Dubins in here to work on that problem you're working on now.

B: That's right!

W: I found this interesting quote from [Herb] Robbins. It was real interesting to me because it said, "The public has a terrible fear of mathematics, I think it's quite real and it's not going to be overcome by restructuring the curriculum or anything else, say, like painless dentistry. The ability and the desire to think abstractly and rigorously is not generally fostered in our society. Most people haven't the faintest idea of what mathematicians do."

B: Wait, who said that?

W: This is Robbins.

B: Oh, Herb Robbins.

W: Herb Robbins. It's right down here.

B: Yes.

W: But, I'm sorry, the reason why I'm going on and on about this. Let me just read the part that he said that I thought was really interesting. "Most people haven't the faintest idea of what mathematicians do, how they think, or what they contribute to society. Mathematicians are regarded with a sort of awe that attaches to any scientist, although we're not really scientists, because we are engaged in a very elusive form of activity." And I just thought that was a very interesting way of talking about the public's attitude towards mathematical activity. Just fearfulness of it, and feeling very far away from it.

B: Well, there certainly is some of that. I expect that a lot of people do think that mathematicians are a bit peculiar. As I've told you, I don't think so. And I don't know that people worry in detail, or think in detail detail about what mathematicians do any more than they think about what dentists do. In any case, it never bothers me to worry about what people think about mathematicians, [laughs]

I've said something about Herb Robbins in an interview, [chuckles] I was asked to name important advances in statistical thinking, and I mentioned Herb Robbins Empirical Bayes as something that made a big difference, but possibly in the wrong direction. And when I saw him after that, he was giving a lecture, he mentioned that quote in his lecture and said a couple of unkind things, but that was his way.

W: Hmm, I didn't realize that because what you said in that interview actually sounded like you appreciated his work, even if you thought it was going in the wrong direction.

B: Oh, it was very new and unexpected. But it was like, after you have an experience, deciding what you thought before you had the experience. And that somehow looks illegitimate to me. Yeah, he invented something called Empirical Bayes, and it's still widely invoked. But I think it's just theoretically completely unsound, [laughs]

W: If someone said, "What's your best thing?" What's the best thing, and not just one thing but when you think about what your contributions are, how do you rank them?

B: I wouldn't say that any of them are important. I can pick out the ones that I found the most interesting, and I can pick out a few that I think other people would find interesting. But I wouldn't use the word "important" at all.

W: "Interesting" is a good word.

B: Okay, well, let's see. Which ones was I the most pleased to discover, that made the biggest difference to me? I've already talked to you about some of them.

W: Through 1954.

B: Through 1954.

W: Through "Optimal Systems ." And it was very interesting, because in our last conversation you said, "I wrote that paper in order to write another paper."

B: Yes. The other paper was in 1956. "An Analog of a Minimax Theorem for Vector Payoffs," now that is a paper that both I found interesting and other people have found interesting. They've quoted this "Analog of a Minimax Theorem," it's called approachability theory, several people used that. I thought it was interesting when I did it.

This next paper, on a "Class of Probability Spaces," I think that's one of my best papers and nobody else has paid any attention to it at all.

W: Why do you think it's one of your best papers?

B: Well, because I think that I found the right model for probability theory. Most people use Kolmogorov's model, and I think his model is just a bit too general, and that mine is just right. But as I say, nobody else has paid any attention to that.

[pause] Now, the next paper that I found really interesting was a very small paper, "Another Countable Markov Process with only Instantaneous States." That word "another" is significant because people have already found examples of this curious phenomenon, it's just that my example was much simpler than any that had previously been discovered. I remember how pleased I was when I found this example. I was just walking down the hallway, and it occurred to me that this would be a good way to do that.

W: It strikes me that that word "only" is also important.

B: Yes, as I said, people had already found examples of countable processes with only instantaneous states but mine was much simpler, more natural.

And the next one that gets my attention is this paper with Lester Dubins, "Merging of Opinions," I already mentioned that to you.

W: Yes.

B: And ["Non-Existence of Everywhere Proper Conditional Distributions"] number 63B, with Ryll-Nardzewski. That's a small paper that I was delighted with because it answered a question that had been nagging me for some time. But it's one of those fine points that most people just don't give a damn about; they're just not interested in that. But, as I say, it pleased me.

W: What was the problem that it answered that was so important to you?

B: You want to define what your opinion will be when you observe a certain phenomenon, and you want to define it in such a way that your opinion is a real opinion in that it's a probability distribution. And that whatever you see, anything that you know is going to happen has probability one. It turns out, remarkably enough, that you can't do that, [laughs] And people had tried to prove that you could, and sometimes even assumed that you could, but it turns out that you can't.

W: It's very interesting to me, this idea of translating opinion. I'm not sure if it's some kind of metaphor that you've said to simplify it for me, but it's just interesting to me, this idea of translating opinions into probabilities.

B: But that's oh. The word "opinion" has two different meanings. In one, you're describing whether you're for something or against it. If people say, "What's your opinion of capital punishment," that means "Are you for it or are you against it?" That's not the sense in which I'm using the word "opinion." I'm using the word "opinion" in the sense of "how likely is something to happen."

W: Oh.

B: So, "opinion" is not whether you like it but whether you think it's likely, [shuffles through papers]

Ah, 67B and 56, "Infinite Games and Analytic Sets." I was just delighted when I found this. This used game theory to prove a theorem in set theory. So it, it related two areas that, up to then, had been fairly distinct. And, not only did I like this but other people liked that. Logicians have quoted that paper.

W: In speaking to Roger Purves, he did describe your contribution to set theory and, in fact, it was descriptive set theory as far as I understand. And there was a book where Moskovokis had cited you as a big influence, and I'm wondering if that relates to this paper?

B: Yes, that's this paper.

Now, the next paper that I want to comment on is with Tom Ferguson, number 59, "The Big Match." Tom and I did this just because well, we were just having fun. We liked it, but I don't think we ever expected the other people to pay much attention to it. But they did, and several other papers grew out of this, that other people did, grew out of this paper "The Big Match." And the name has persisted, "The Big Match," but I'm not sure people know why we called it that. Shall I tell you why we called it that?

W: Please, I was going to ask. I wasn't sure if it was a Stanford-Berkeley.

B: Oh, yes. [laughs] No, it's suppose you and I are matching coins, do you know how to play matching coins?

W: No.

B: I show heads or tails and you, simultaneously, show heads or tails. If we match, I win, if we don't match, you win, see. So, we can continue playing matching coins. Now, but in "The Big Match," we continue to play matching coins as long as I show tails. But, the first time I show heads, if you show heads on that trial, I win every time from then on. But if you show tails on that trial, you win every trial from then on. So, this time that I show heads is the big match, you see, because it determines the whole future.

W: Yes, pivots the game.

B: Yes, that's right. Now, of course, it may happen that I never show heads, and then you have to worry about what to do every time. But, anyway, up to our work, people had not known how to impute a value to this game. We showed that this game had a value and how you could nearly achieve it. Anyway, that was a fun paper.

W: Where was Tom Ferguson?

B: Tom was at UCLA, and still is. But Tom is a Berkeley graduate. He graduated from here, I think, in 1958 or something like that. I knew him when he was a graduate student here. So, he still comes back to Berkeley from time to time. It was while he was just visiting up here when he and I started working on this stuff. Tom has had an interest in game theory for a long time, so he and I like to talk about games.

Well, the next paper that I like, really, is with Jim MacQueen, "Ferguson Distributions by Polya Urn Schemes." That "Ferguson," by the way, is the same Tom Ferguson. Jim and I were explaining some work that Tom Ferguson had done. And he did it in a more analytic way, but Jim and I did it by talking about balls and urns, and sampling with replacement and addition. And that explained sort of gave another way of thinking about what Tom Ferguson did. Again, that paper has gotten a certain amount of attention. We knew it was a good paper and other people thought so, too.

Then, there's the paper number 71 called "Borel Programmable Functions." That's a paper, a short paper, that I liked very much, and I still like it. I thought that this was on the way to distinguishing between functions that were mathematically useful and functions that were not mathematically useful. But, nobody else thought so, so that paper just [pounds table] landed dead, so to speak.

W: Yes, I noticed when you read that paper, you threw down your pen and started laughing. And I was like, "He must have really liked this paper."

B: I did like it, I still think it has some interesting ideas in it. And the next paper, "There are no Borel SPLITS." Again, I like this idea. Notice that that paper took only two pages, by the way.

W: Yes. Does that mean it was expressed in a very neat way?

B: Yes, people were they were trying to see how to go from convergence in probability to almost everywhere convergence. And, roughly speaking, a "SPLIT" is what enables you to do that. The word "SPLIT" stands for "Special Probability Limit Indicator Functions," and I found that, alas, there are no SPLITS. Or, at least I proved that there are no Borel ones; but that means to me that there really aren't any at all. People used this to settle two or three other problems, I've forgotten now what they were. But that was a neat paper.

W: And it got attention where the one before didn't?

B: Yes, that's right.

W: I have a question about getting attention, because I think the last time we talked I said, "What is it like to get feedback from your peers about your work," and you said, "Oh, well." You said, "Well, that's happened a couple times. It's only been a couple times that people were really excited about certain things I was doing." And I just was wondering, like, where did that feedback manifest itself? How does that look?

B: By quoting it!

W: By quoting it.

B: Yes, you appear in the bibliography, or even more, you are invoked in the course of the paper, yeah. So, people have actually used this fact, that there are no Borel SPLIFS, to prove other things, [pause]

And, this paper in 80, or 1974 with Ramamoorthi, "A Bayes but Not Classically Sufficient Statistic" settled a question that had been worrying me a long time. I had tended to worry about details, tidying everything up, making everything neat in a way that doesn't bother most people at all. So there's this concept of "sufficient statistic," which is an extremely important statistical concept. And there's the Bayes approach to statistical ideas and there's the classical frequentist approach to statistical ideas. Well, there's a Bayes concept of sufficiency, and a classical concept of sufficiency. And, it's a general theorem that under a certain hypothesis, they're the same. Now, this theorem covers most cases. But most people don't care about cases not covered by this theorem. But I do, I have that kind of a nit-picking mind about certain things. Are they really the same? And I found that, no, they are not the same in certain cases. Here's an example of a statistic that is Bayesian sufficient but not classically sufficient. So, that paper interested me and Ramamoorthi, but nobody else. It's like that, sometimes.

W: I'm still trying to understand the word "moment" from last time we spoke. I looked it up on the web, I'm still looking for it, but that's just me. I understand it's a physics concept, but I'm still having trouble with it.

B: Well, I don't see anything exciting or even very interesting that I did after that.

W: Hmm. What about . I was wondering about two things. One is the work you did with Ashok Maitra, "Factorization of Probability Measures."

B: Oh yes. Again, that's one of those very technical points that's of interest to people who like to see things tidy and neat. Ashok, who was one of my students, by the way, was interested in this. Again, I doubt that anybody else would pay attention to it.

W: So, when you say it's like that sometimes, sometimes people just aren't you know, you're going to spend days and weeks moving an idea forward and moving your understanding of a theorem forward and then it doesn't . How do you make a space where it's important because it matters to you even if it doesn't matter to other people?

B: You can't decide whether it's important to you by whether it's important to other people. When you decide whether you're going to like a piece of music, when you like the piece of music, you don't ask yourself, "Do other people like it?" Or at least, your opinion of it doesn't depend on what other people think about it. You hope other people will like it, but... And, this is no different from that. You can't help what you like, it's just the way your tastes are.

W: It strikes me that it's just that doing mathematical work, on one hand, it's your career, and it's your professional life. But on the other hand, it's very personal.

B: Yes, it is. Sam Wilks was editor of the Annals of Mathematical Statistics for many years. And his criterion for publishing a paper was this: "Is it new?" and "Is it correct?" He never asked, "Is it interesting?" or "Is it important?" And, I think he was right. So, if I discover something that is new and correct, if I'm interested in it, then I tend to submit it for publication. Of course, you hope that other people will be interested. Sometimes you're pretty sure they will be and sometimes you're pretty sure they won't be. [laughs]

W: It seems to me that having that criteria, is it new and is it correct, is pretty central to maintaining a place of integrity for intellectual growth and development.

B: Absolutely. And it makes a referee's job a lot simpler. It's hard to decide whether something is important. Whether it's interesting is very subjective, but it is possible to make a reasonable decision about whether it's new and whether it's correct.

W: This brings me back to a question that I had. During that time when mathematical statistics was really growing so rapidly and the government was putting a lot of money into basic research, fundamental research, did that create a place for intellectual growth to occur in a place of integrity?

B: Oh, it certainly helped a lot. Well, for example, supporting research in the summer influenced many people to continue working year-round instead of, say, going out into the woods and chopping trees, or something like that. And, supporting travel made it possible to go and talk to your colleagues across the country or around the world. There is no question in my mind that that was tremendously helpful in stimulating research.

W: Did you find it shaping the kind of research that took place, though?

B: You might be surprised at how little effect it had on shaping the kind of research, at least within statistics. People who supported research were quite broad-minded in letting people work on the things that interested them, never mind whether it's of immediate value to the government or the country, or not. They were very smart and far-seeing in that way. I think some of us were afraid that government support would affect the direction of research, but it didn't. It just made more of it.

W: I mean, part of that question comes from reading, and some people saying, "Well, it really affected the quality of their work." It became there was so much money being thrown at it.

B: Yeah. It did that. People published things that they would not otherwise have published, just because to get your grant renewed, it was important to show that you'd been doing something. The easiest way to show that you've been doing something was to show that you've published things, [pause] But that's fairly harmless, because it's easy to overlook junk, when you see it in the paper.

W: That reminds me, in just the way that people were talking about it, it reminds me a little bit of the dot-corn boom, where people were throwing money at different ideas.

B: Yes, I never really understood that, but

W: You didn't have to show profit

B: Yes, that was really strange to me.

W: You know, if you go on-line, there's an oral history from that, the dot-corn boom and bust. I'm going to write down the website for you before I leave today, but it's pretty interesting. A lot of the people who were talking about what it was like being part of that hustle-bustle, having all that money thrown at you, and then having it all go away.

B: Instant millionaires.

W: Can I ask you about the paper Daniel Mauldin, "Redistribution of Energy Problem?"

B: I don't remember much about that. I don't remember how that collaboration happened to take place. He's at the University of Texas, I think.

W: Hmm, okay. It's about eleven o'clock, are you good to talk to me for another thirty minutes, or do you want to? How are you doing for time?

B: I would like to go on home pretty soon.

W: Okay, does that mean, like, ten minutes?

B: Ten minutes, yes.

W: Yes, so let me go into my other questions, then.

B: Okay.

W: What are your thoughts on the way that statistics has been used to develop theories of evolution, eugenics, and social Darwinism?

B: You know I have no thoughts on that, [laughs]

W: Okay. I thought I would just ask that question.

B: Yes, okay.

W: One thing, when you were explaining to me some of your work previously, we were talking and you were saying there is this difficulty when you reduce complex variables into simple ones for math, especially when you're working with problems that you found in life and translate them into math. And I wanted to ask, how do you deal with that? How do you think about it? How does it affect the way you approach a problem?

B: You simplify! For instance, take a very simple example. When Dick Bellman and I studied poker real-life poker has fifty-two cards, we studied poker with three cards.

W: Hmm, that's a different way of playing poker.

B: Sure! And what can you say about it there? And if you understand that, maybe that will help you understand fifty-two-card poker, maybe not. Yes, you take a real life situation, you simplify it and distort it and change it to make it interesting mathematically but at the same time something that you have a chance of solving. And you don't worry about what it tells you about the original situation. That didn't make us better poker players to solve this game, but...

W: In our past conversations I've been asking about applications of your work. I was thinking about you working in the context of wartime, federal funding, and to what your extent your models and the systems you developed were applied. Then I realized, that I was actually, by using those words, that there's actually this whole or there was, at one time this whole debate around applied math versus pure math.

B: Yes.

W: And I don't think I realized that I was stepping into that when I was asking you those questions.

B: Well, you certainly were.

W: Yes, and I didn't realize that. So now that I know that I was asking those questions, I want to ask them more explicitly. Can you talk a little bit about that division between pure math and applied math?

B: Well, take the example of poker that I mentioned before. If you look at real poker, and then change it into three-card poker, and then solve three-card poker, and then recognize that that tells you nothing about fifty-two-card poker practically nothing is that pure math or is that applied math? It came from a kind of applied problem, but it doesn't help you solve the applied problem. That's the kind of work that I've been doing.

You look at real situations, simplify, and abstract, and get something mathematically interesting and try to say something about that mathematically interesting situation. And don't worry about what that tells you about the original situation. Now, I don't know whether to call that pure math or applied math. That's all I can say about it.

W: I know there was a time when people were very much, kind of, they were denigrating applied math basically because they thought like it was sloppy mathematics, as far as I understand. Is that correct?

B: I don't know. I guess I don't know what "applied math" is. To me, applied math is probably something else. If you use mathematics to design an airplane wing and the airplane flies, you can call it flight mathematics, but to me, it's aerodynamics or engineering! As I say, I'm not clear about what "applied math" is.

W: From what I've read, it appears that there's this connection between physics and math, and that's where the applied math debate occurred. It's like fifteen years ago, from that time, and I'm wondering if that division is still relevant or interesting, if people still debate that with any kind of fervor or commitment to it as they used to. They used to really be up in arms and now . I'm wondering if that's still a relevant position.

B: [pause] I'm not the one to ask. All I can say is that a lot of interesting mathematics is done outside mathematics departments. Here at Berkeley, it's done hi the statistics department, in the economics department, and a lot of it in electrical engineering and computer sciences, and other departments, too. For administrative reasons, you have to break things up into departments and try to classify them. But as I've said, individuals defy classification, each one has his own interests and you put them in boxes and departments, but that doesn't really fully describe what they do.

W: Can you tell me a bit about how and when you came to use computers in your work?

B: Oh, yes. I was first impressed with computers probably about 1970, when a computer essentially produced a new theorem; that is, David Freedman and Philip Deuel and I had been doing some calculations on something, and the computer printed out a table, and it turned out that two columns of numbers that I had expected to be different were the same, so that suggested they would always be the same, of course. And then we tried to prove the theorem, that they were the same, and we did, and that was new. It was my first experience with having a computer discover a theorem. Since then, I have used computers a lot, sometimes to discover what's true, and sometimes just to check algebraic calculations. In my work, I've had to do a fair number of algebraic calculations and simplifications, and I'm not very good at it. I make a lot of mistakes. And now I routinely use a computer just to check my work.

For instance, you have a formula involving, say, X , Y , and Z , and you go through and you make a lot of changes and simplifications, and you end up with a new formula that you hope is equivalent to the old one. One way to do it is just to compute the original formula for certain values of X , Y , and Z , and the new formula for those same values of X , Y , and Z . And you can pick any X , Y , and Z you please. X is 5, Z is 12, and Y is 37 and a half. And if you get the same result from the beginning formula as from the end formula, you probably didn't make a mistake. So that's a very valuable way to check whether an algebraic computation is correct or not. And, as I say, since mine are often not correct, that's been very helpful to me.

Another kind of problem where the computer is helpful is: you have a sequence of numbers, and it looks as if it's approaching zero, so you want to try to prove that it is approaching zero, but it's sometimes helpful if you know how fast it's approaching zero. So you just compute the sequence. Maybe you compute several terms and try to guess a formula that approximates the sequence. You compare what the formula predicts with what the calculations give you.

This kind of thing would have been very difficult and time-consuming without computers, but with computers, it's easy and simple. Many times it tells you what's true. It doesn't help you much to prove it always, though sometimes it does, but mostly it just tells you what's true. I've worked on many things that I wouldn't have touched at all if I hadn't had computers to help me.

W: And in which areas did you find the computers to be the most useful? You've talked about, like, what you've used it mostly for, but I'm thinking in terms of which areas of math.

B: [pause] Probability, estimating probabilities of unlikely events, [pause] And the programming language that I like best is True Basic. It's simple, yet it's fairly powerful.

W: You were saying you felt that it was designed for users.

B: Yes. [pause] In many programming languages, you have to say whether a variable is an integer or a short decimal or a long decimal, or I forget. There are other categories, too, that you have to use. But not in True Basic. You just say, "X = 5," and that tells True Basic that X represents a number, and it doesn't distinguish between 5 and 5.0 and 5.000, so you don't have to worry about that.

On the other hand, that makes things a little harder for the people who have to tell the computer how to understand that. It's then their job to explain to the computer how to distinguish between integers and real numbers and so on. I have no idea how to do that at all, but if it's already in the program, that makes it easier for the people who have to translate the program. So if you're writing a language for the other programmers, then you want to make all these distinctions, but, as I say, that makes it harder for the person who's just using the language.

W: Okay. So you said you used to write programs as well?

B: Oh, I have written many programs, yes. [pause] Every time you want to calculate a new function, you have to write a program for it. There are an infinite number of functions, so they can't all be canned for you. You have to write the programs if you want to use them.

W: I think my question is coming from my understanding programming like this person who sits at a desk somewhere in Silicon Valley and creates applications, and I think that's different than what you're talking about.

B: It's different by a factor of maybe a million. You're talking about people that write big programs for big functions. I'm talking about a program that calculates that finds out the value of X that makes a certain function equal to zero. Nobody else is interested in that function, so there won't be any canned program for that particular thing. So I've written many small programs like that.

W: That's neat. Well, that's good.

B: Good.

W: Okay. And also, I wanted to ask you before we close today, this is a question from a couple conversations ago. Hugo Steinhaus

B: Yes.

W: When we started meeting and you told me you'd named your son after Hugo Steinhaus, and I wanted to know who . His name doesn't appear as someone you write with. And I'm sure there are many people like that who ve influenced you who aren't necessarily people you write with. So I just wanted to know more about who he, who he is, or who he was.

B: Steinhaus was a Polish mathematician. I met him at Cornell one summer. Mark Kac, one of his distinguished students, was teaching at Cornell. Steinhaus spent the summer there. And he and I became good friends, I'm not sure just how. But I invited him to come to my house, and he spent a couple of weeks at my house, I remember, in Washington, B.C. He was one of the earliest people to see that game theory was interesting for mathematicians. Games with an infinite number of moves, he was one of the first people to study that. And he was at our house maybe when my wife was pregnant with Hugo. But anyway, that's not basically why we named our son Hugo, but that enforced it.

W: Well, shall we close for today?

B: Sure, okay.

[end of interview]

INTERVIEW 6

MAY 16, 2002

W: Good morning, it's May 16. We're here with interview number six of Professor David Blackwell. Well, when we closed our last conversation you had said you had an interesting story about Julia Robinson at Rand that I wanted to hear about.

B: Oh yes, we were studying three-person games. I wanted to see how people actually behaved in a three-person game. And, the way the game worked was this. If any two of the people cooperated, they could get a dollar from the third person. So, Julia Robinson and Marian Shapley, that's Lloyd Shapley's wife, and I were all at Rand. So we decided to play this game, just to see how it would work.

So we played it, and Julia chose Marian and Marian chose Julia. So it didn't matter who I chose, I didn't have any partner. So I had to pay them each a dollar. And, after that, I offered one of them a bribe to be my partner, but it didn't work. We played it again and again they chose each other and I paid each one of them a dollar. And finally, I offered one of them two dollars just to choose me. But no, no, she wouldn't do it. They stuck to each other. So, they laughed about it and I sort of laughed about it. I finally recognized that once two people form a pair, it was very hard to break up. And I resented that with Julia Robinson and Marian Shapley for about a week, I'd say. It took me a week to get over that.

W: So the rules of the game actually allowed you to bribe?

B: Oh, yes! You were supposed to have what was called "pre-play communication," you see. But once two people form a pair against a third, that poor fellow better back out of that game as soon as he can.

W: Maybe that sounds like a theorem in and of itself: once two people are bonded, they stick together.

B: Right.

W: And you had, you know, you also said you also had a story about Norbert Wiener?

B: Well, this is a side of Norbert Wiener that a lot of people may not appreciate. He spent a summer here. And at that time, I had a Kriegspiel game going in my office. Every day at noon, some people would show up and play Kriegspiel.

W: What is that?

B: That is a variation of chess in which you don't see your opponent's board. You make moves and your opponent makes a move, and there's a referee who sees both boards and makes an announcement. For instance, he tells you that the move you're making is illegal.

Anyway, every day at noon there used to be a Kriegspiel game in my office. And Norbert Wiener showed up. And he started playing the game. Wiener was a miserably poor Kriegspiel player. Everybody could beat him. But somehow, that didn't dim his enthusiasm at all. At two minutes to twelve, he would be outside my office door waiting to come in and play Kriegspiel. I thought that showed a side of Wiener that most people were not aware of, how day after day he could show up with the same sunny disposition that he had before, ready to do it again.

W: He was excited about this game and interacting with other people, maybe.

B: Yes, I think that was a part of it, certainly.

W: Wow, can you spell this game for me?

B: Yes, K-R-I-E-G-S-P-I-E-L.

W: There's a reason I haven't heard of this game before.

B: Well, "Krieg" is "war," and "Spiel" is "play." So it's "war play."

W: And who else used to play this game?

B: I don't remember whether it was, at that time . Dorian Feldman and Martin Fox, and at various times Tom Ferguson. And Jerry Klotz, who was a graduate student here at that time. But, many times Jim McQueen and Tom Ferguson and Lloyd Shapley and I have played Kriegspiel. Sometimes all night.

W: Who was refereeing the game that you were playing?

B: Well, you take turns, you see. They're... You need three people in the room, at least. Two to play and one to be referee. So you take turns refereeing.

W: And when you win, is there something that you get?

B: Just the pleasure of winning.

W: Okay, good, [laughs] I wanted to move on to talk a bit about Stanford. That paper that we were discussing, your "Comparison Of Experiments," it seems that you presented it at the Second Annual Berkeley-Stanford Symposium on Mathematical Statistics and Probability in 1951.

B: I expect so.

W: Yes, and I wanted to ask, was this your first time connecting with that Berkeley-Stanford mathematical statistics community?

B: I don't remember. Stanford and Rand were sort of related in my mind.

W: Yes.

B: And my connection with Stanford, and my connection with Abe Girshick and Al Bowker are all kind of interrelated. And it all started around 1947 or 48 or something like that.

W: Can you tell me about how you came to spend time at Stanford, how that happened?

B: Well, I was there to work with Abe Girshick. Abe was a professor at Stanford. He and I had already started working together before he went to Stanford. So that essentially continued.

W: And that was in the years of 1950 to 1951?

B: Yes. I guess I spent that entire academic year at Stanford.

W: I keep on calling it a community, mostly because I've heard Al Bowker, Professor Al Bowker, talk about it as a community, in his memoir. Very child-friendly, he says.

B: Very what?

W: Child-friendly, family-friendly environment. And I wanted to ask you if you could tell me a little bit about what that environment was like?

B: I don't remember very well except I just have very pleasant memories of it. The people you worked with were the people that you saw in the evenings and on weekends. And it was just an extremely stimulating environment.

W: Yes.

B: By the way, I can connect that up with Kriegspiel. Every day at noon, we used to play Kriegspiel at Stanford. And Abe Girshick hated that. He felt that you shouldn't spend an hour playing Kriegspiel when you could spend it doing statistics instead, [laughs]

W: In terms of the other people that I understand were there around that time... I have, you know, of course Professor Bowker. Erich Lehmann and Paul Garebedian.

B: Paul Garebedian, yes, he was an applied mathematician but he was pretty close to the people.

W: Charles Stein.

B: Oh yes, Charles Stein was there.

W: And Hans Levy, George Polya, Gabor Sego, Kenneth Arrow, Charles Lowner, Steven Bergman? No?

B: Not all these people were in the statistics group.

W: Okay.

B: You got those names from Al Bowker?

W: I got them from his memoir.

B: Yes.

W: I think when he was talking about how he built up the...

B: Oh yes, Al built the math and statistics departments.

W: He also mentioned Steven Bergman and Max Schiffer.

B: Schiffer, yes. I didn't associate with them very much at all. My association was almost completely in the statistics group.

W: And were there people that I mentioned who were in the statistics group that you remember associating with a great deal?

B: Yes, would you read their names again?

W: Sure. Erich Lehmann.

B: Erich Lehmann is at Berkeley, he's still here, by the way. I don't remember him being at Stanford at that time, but maybe he was.

W: My information might also be a little off. And Paul Garabedian?

B: Paul Garebedian was at Stanford, not in the statistics group.

W: Applied mathematician?

B: Yes.

W: Charles Stein?

B: Oh yes, Charles was very much in the statistics group. He was, I would say, the intellectual leader of the statistics group. I got at least I claim that I got Charles Stein to solve the "Comparison of Experiments" problem. I'd been working on the problem for a long time and couldn't solve it. And I had to have it solved. Whenever a problem was too hard for me, I burdened other people with it. And I kept mentioning the comparison of experiments problem to Charles Stein, and he finally solved it.

W: So you just kept bringing it to his attention?

B: Yes.

W: What kind of person was he?

B: I'm no good at describing people, [chuckles]

W: Okay, all right. And then there was Hans Levy and George Polya, but those were people who were not in the statistics department?

B: They were not in the statistics department.

W: And was Kenneth Arrow there at the same time?

B: Yes, Kenneth was there. He and I and Abe Girshick wrote a big paper that made important advances, I think, and got us into trouble. Because Wald and Wolfowitz had started this work and we developed their work and went somewhat beyond what they had done. But we didn't give them proper credit. So, they were upset about it and properly so, I think. Wolfowitz didn't speak to me for about twenty-five years.

W: That sounds hard.

B: Well, he had some justice. As I say, we did not give them proper credit.

W: Well, I'm sure it wasn't intentional, I'm sure it was not intentional. Let's see, and was your family at Stanford with you?

B: Yes, we stayed in Stanford Village, which was a place occupied by young faculty members at that time. We had a very pleasant year there.

W: In Bowker's memoir, he says that you determined that Stanford wasn't a good place for you to raise your family, so you left and went back to Howard. Can you tell me about that?

B: I don't remember much about that, what went into that decision. I don't know that it was negative about Stanford, just positive about Howard and Washington. We enjoyed Stanford a lot.

W: What did you enjoy about it?

B: The colleagues, the atmosphere, the weather.

W: Yes.

B: Both the work and the parties, it just was a great year.

W: One of the things that I get just from talking to you and also from reading about this time in the growth of mathematical statistics and math was just that it was a really exciting time to be there and be connected to young people who were thinking.

B: It was an exciting time in the development of statistics, yes. I feel very lucky that I came along at that time, to be part of a subject that was growing and expanding.

W: I'm wondering also about Abe Girshick, who I understand was he was very central in a lot of the work you did up to that time. And I wanted to ask you here s another one of those questions but I was trying to get a personal sense of who he was, what kind of person he was?

B: I'm not good at describing people.

W: Okay.

B: He had a great sense of humor. But at the same time, he was very intense.

W: You mean intent upon his work?

B: Well, he had strong feelings. He could get really enthusiastic about a theorem. And he was intense in his dislike for communism. See, he came from Russia. I believe he came to this I believe he was about fifteen years old when he came to this country, so he had had fair experience with communism and he didn't like it at all.

W: Was it while you were at Stanford that you and Abe Girshick started developing the book or the work that would become Theory of Games and Statistical Decisions!

B: Yes. I don't remember whether we started it at Stanford. But, we certainly devoted a lot of time that year to working on the book, yes. Abe was really the main author for most of the book. I think I did the part that emphasized game theory and Abe did the part that emphasized statistical decisions.

W: How did you work together? What was working together like? I know that's a big question because you worked together for years and years and years.

B: Well, it was my job to write certain parts and his job to write certain parts. And I would criticize the parts that he wrote and he would criticize the parts that I wrote, [pause] I don't remember that much about it.

W: That memory right there is very good, the one you just told me about. I'm a little interested in how you helped each other grow by criticizing each other. I'm wondering, well did he say, "You need to make this more transparent," and you said, "Well, you're phrasing this in too simplistic ways." Or was it more ?

Okay, so we were talking about the way that you and Abe Girshick worked together. And how you kind of moved each other forward.

B: Yes. As I say, I would write a section and show it to him and he would criticize it and make changes. And he would write a section and I would read it and make changes. I don't remember very much disagreement. If I would make a change he would accept it, and if he would make a change I would accept it.

W: So that was a very big book in the field. It really moved the field forward, is my understanding.

B: Yes, it sort of collected and organized things that had been done up to that time, yes.

W: Yes. I wanted to talk to you a little bit more about Berkeley now, or to begin.

B: Okay, sure.

W: I understand that Jerzy Neyman contacted you for initial conversations about coming to teach at Berkeley as early as 1944 while you were at IAS, the institute?

B: Yes.

W: And I'm wondering if you can tell me the story of how you came to Berkeley from that first time to ?

B: Yes. This is in the year . During the academic year 1941-42, while I was at the Institute for Advanced Study, Neyman came east and interviewed me for a possible position in the Berkeley mathematics department. And the interview took place in New York. I was at Princeton forty miles away, so I went to New York and he interviewed me. I hadn't dreamed of getting a job at Berkeley. My whole horizon was the black colleges. But I was pleased that Neyman interviewed me. And after the interview, I just said, "Well, that's that." A few months later I got a letter from Neyman saying that he was sorry, but because of the draft situation, they had decided to appoint a woman to that position. So that was fine, I hadn't thought there was any possibility I would get the position, anyway.

W: Why not?

B: Well, Berkeley was an absolutely first-class university and I was just a fresh young PhD and so that was that. However, only ten years later did I discover what had actually happened. Neyman had decided to offer me the

position in 1942 and suggested it to the mathematics department and the mathematics department agreed. They had no complaints. But the head of the mathematics department, Mr. [Griffith] Evans, mentioned it to Mrs. Evans. And as I understand it, she objected. See, at that time it was the custom of the Evans to invite the entire mathematics department to their house for dinner. And Mrs. Evans, who was from Texas, said, as I understand it, she "was not going to have that darky in her house."

W: Those words?

Blackwell:[laughing] Yes. Now, this is the way I got the story.

W: Oh boy.

B: Anyway, so Mr. Evans came back to the math department and said, "Well, perhaps we need to rethink this." Ah, and they did, understandably, and so that was that. That's what as I understand it actually happened, but I have no idea if that was what happened. Yes, but Neyman didn't forget me, and ten years later he offered me a position here.

W: That story, do you recall who you heard that from?

B: I don't remember, no.

W: Okay. So then, ten years later, Neyman came and found you again. Can you describe how that initial contact happened again? And your interview and...

B: By ten years later, you see, I had contended I had worked within statistics for almost that entire ten years. So, ten years later, I knew everybody in the Berkeley statistics department. So in fact, as you've mentioned, already by 1950, I attended the second Berkeley symposium.

W: And I think you'd also, by that time, you'd published your book. You'd published a number of other significant papers.

B: Certainly a number of other papers, yes. Anyway, by that time, the mathematical statistics community was pretty small and we all knew each other. And . [pause]

W: Was there an interview?

B: No! [laughs] As I say, we all, we all knew each other very well.

W: And do you remember who you had conversations with about coming to Berkeley? Was it primarily Jerzy Neyman?

B: Yeah!

W: I want to ask you a bit about Jerzy Neyman. And I'm not sure, I'm wondering just what kind of person he was and what kind of relationship you two had?

B: We were very close. In some ways, I may have been his closest friend. See, Neyman was kind of the father of the statistics department. He created this statistics department. And, when I came in, most of the other members were his children, his intellectual children, so to speak. Some of them had written their theses with him and they were all well, as I say his intellectual children. And I was the only one who could talk back to him, so to speak. He and I were just buddies. After we had had a hard day, sometimes at five o'clock, I would go into his office and suggest that he invite me over to his house for a martini. And he would! [laughs]

W: What was a hard day at Berkeley's Department of Statistics?

B: I wish I could remember. Neyman got into arguments with people and had disagreements. I wish I could remember what they were about. I remember there was some man that he wanted ejected from the National Academy of Sciences for intellectual dishonesty of some kind. I don't remember the details at all. And I would try to persuade him that he just forget about it. [laughs] I don't remember the details.

W: Yes, I read about how you'd be the only one who'd say, "Jerry, I'm not going to do that." I read that in his book. And you would laugh and throw up your hands and say "Jerry, I'm not going to do that," and call him Jerry.

B: Yes. He and I got along very well.

W: Did your families spend time together?

B: Did my family what?

W: Did your families, his family, your family, spend time together?

B: Yes. Jerry and Betty Scott would have parties from time to time. And Ann and I would always, always go to them, yes.

W: But you and Jerzy Neyman had kind of different areas of work, is that correct?

B: Yes, he had done important work in theoretical statistics earlier. But by the time I started working in statistics, his interests had shifted to applied statistics. So, yes, he and I didn't work on the same kinds of problems, that's right.

W: I understand that he was pretty instrumental in bringing in government contracts to the statistics department at that time?

B: Oh yes, at that time, I think all government contracts were administered by him. I think that's right, yes.

W: So when you had this interview well, you didn't have an interview. When you came to Berkeley, were you offered a tenured position at Berkeley which was commensurate with the position that you held at Howard?

B: Yes, it was probably a little higher salary but not a lot higher. Yes.

W: And my question now is what was it like coming from Howard to Berkeley? Big question. And I want to ask that in a smaller way. How did your family settle into Berkeley?

B: [pause] The first year we were here, we lived in a small, rather uncomfortable house that we had rented. But then, a year later, we bought a house, a big house, and we're still in that house in south Berkeley, about a mile south of the campus. So my children did essentially all their schooling in the Berkeley schools.

W: In the Berkeley public schools?

B: Yes.

W: And what area was the first house that you rented? Where was it?

B: It was on Parker street just below Sacramento. I don't know if you know Berkeley.

W: A little. My understanding is that during that time period there was a lot of discrimination around housing housing was actually very racialized in terms of racial covenants. I don't know if you had any experience while you were looking for housing around that? Or if it affected the neighborhood where you settled?

B: Why, I would say that the real estate agent was very restricted in the houses he could show to us. There were some areas where black people simply did not live at that time. So even if we could have afforded it I'm not sure whether we could have or not, maybe we could have they would not have shown us a house in the Berkeley hills.

W: Yes. When you moved into your community, did you enjoy meeting your neighbors? Were there people there who you enjoyed meeting and socialized with?

B: Our immediate neighbors on one side were extremely friendly. And on the other side, not particularly friendly but not particularly hostile either.

W: Okay, good. That's good, having neighbors that aren't particularly hostile is always a positive thing. Who lived in that neighborhood where you moved? Was this a black neighborhood?

B: No, let's see. On one side of us, there were white people in the house next to us and the house next to that. A black woman in the house next to that, and then two white people in the other two houses on the corner. On the other side, there's a white person next to us, and there was one black woman who lives four or five houses down the street, and the others were all white.

W: So it was a mixed neighborhood, and more white than black.

B: Yes.

W: Okay. And were there any Asian people in your neighborhood?

B: Asian people?

W: Yes.

B: Why yes, that's right. Down the street, one of my daughter's playmates was a girl named Annie Katsumoto, a Japanese girl. Yes, there were a few Asians.

W: Can you tell me a little bit about the department when you arrived? What was the atmosphere here and who did you work with? I mean, actually, you were a professor so you had graduate students.

B: Yes. [tape interruption]

W: Let's see, where were we? We were talking about how your family settled into Berkeley. And now I wanted to know a little bit more about the department when you came here. The people that were here.

B: Let's see, who was here? Neyman, Elizabeth Scott, Evelyn Fix, Henri Scheffe, Erich Lehmann, Ed Barankin, Joe Hodges, Lucien Le Cam. That sounds like it.

W: Among those, who did you spend time with? Or did you all spend time together?

B: Oh excuse me, Michel LoeVe.

W: Michelle?

B: M-I-C-H-E-L Loeve.

W: Oh, okay.

B: I talked a lot to Erich Lehmann, this is about statistical things.

W: Yes.

B: I talked a lot to Erich Lehmann and actually wrote a joint paper with Ed Barankin, a short one. And a couple of papers with Joe Hodges. And I had many discussions about probability with Loeve.

W: And were there people also who were your colleagues who were also friends with whom you socialized?

B: At least as much friends as colleagues in some cases, yes. For instance, I was many, many times at Betty Scott's house, and at Henri Scheffe's house, and at Erich Lehmann's house, and Ed Barankin's house, and Joe Hodge's house, Lucien Le Cam's house, and Loeve's house, yes.

W: So all of them. It sounds like, again, like it was very much a community.

B: Yes. Neyman was responsible for that. He was a gregarious sort of person, himself. And insisted that we all work together and socialize together. For instance, every Saturday morning we used to go and have lunch at a certain restaurant at a certain table. And Neyman sort of insisted on it, and it worked.

W: Was this a restaurant called George's?

B: Yes! [laughs] Yes. You may have gathered from that book [Neyman from Life by Constance Reid] that Neyman was a fairly forceful man.

W: I did gather that from the book. I really did, definitely. He sounds like he had a...Personable is not the right word, but he had a very strong personality and made a lot of things happen, and it sounds like he interacted with people in ways that were sometimes hard for others.

B: Yes.

W: Where did the power lie in the department?

B: Well, when Neyman was chair the power was with him. But after he gave up the chairmanship . Well, I was the next chairman and I neither had nor wanted power, especially. So the power devolved on the individual faculty members. There just wasn't a power center after Neyman retired.

W: And, again, when it came time to, kind of, as a chair person, were you involved in making sure the department had what it needed to go forward? And who did you go to to get things for the statistics department? To get money and resources?

B: I was not a very active chairman. I just went to the dean of the college. I was somewhat . I was not a very good chairman from the point of view of the dean. I remember that to make a budget you had to go through all kinds of calculations. I didn't bother with any of that. I essentially said, "Next year our department is going to have to teach these courses, probably this many sections, so we're going to need this many teaching assistants. And so-and-so will be ready for a promotion, so we will need this much extra money," and so on. Just in a reasonable way I figured out how much money we would need and sent the request into the dean. That wasn't at all the way the dean wanted it. So, the poor dean had to do the work he had asked me to do. He did the work and made the calculations. And as I remember it, the amount of money that he came up with for us was a little more than I had asked for. So it, it worked out all right. As I say, I didn't make things easy for the dean.

W: What was your dean's name at that time?

B: I think it was Lincoln Constance. Lincoln and I got along very well, he was very tolerant. But, as I say, there was no power center after Neyman retired.

W: You've said hi the past that much of being chair was figuring out what people wanted done and then doing it.

B: Yes, yes.

W: And I wanted to ask you, what do you think your major contribution to the department as chair was?

B: To give people freedom. To arrange it so that each person could choose his own textbooks, so that if you had two sections of the same course they could use different textbooks if the people wanted to. To let people introduce new courses if they wanted to.

W: I understand that you were also responsible for hiring some young professors that you brought on the faculty?

B: I think I hired Lester Dubins and David Freedman. I think so, yes. And if so, I'm very proud of it. For those were two very good appointments.

W: Yes, they've both really done some very important work in the field, is my understanding.

B: Yes.

W: How did you get to be chair so soon after you arrived at Berkeley?

B: Somehow there was no one else that everyone else could agree on. [laughter] I got along with everybody.

W: Oh, okay. Because it strikes me as very interesting that you were here for maybe two years and then you were chair.

B: Actually, three years, yes.

W: The other thing I understand that happened is soon after you arrived at Berkeley, the statistics department split from the math department. Was that a big deal, and how did that happen?

B: No, that was a formal recognition of what had de facto been the case for several years. While we were in the math department, the math department recognized Neyman's group as being separate. And they let Neyman make the decisions for what happened in the statistics group in the math department. So this was simply, basically, the university recognizing what was, in fact, the case. Nobody was unhappy about it.

W: You were doing so much during that time, especially 1960s on as far as I can tell, in terms of your publishing, your teaching, your chairing, and also your administrative work, your presence as a faculty member in this university community. And I wanted to ask you, did you feel that your research, or your mathematical activity, suffered as a result of all that that you had taken on?

B: No. I was busy, but I think that probably by that time I may have started saying the same things over and over, I'm not sure exactly. It is true that I was doing a lot, but not at the same time, somehow. I think I did a lot of mathematical activity during the summers.

W: I'm wondering if you could take a look. This table I made, I showed it to you before, I was wondering if you could take a look at it and tell me . There are so many things you did, you know, refereeing so many committees you were on for the Academic Senate and the department. And I wanted to ask you if you could tell me which of these were actually very important or meaningful to you so that I don't ask questions about .

B: Yes, I understand, I understand. Some of these things I just don't remember at all.

W: Okay.

B: This was a great semester! During that semester I was not here at all.

W: This is 1960?

B: Yes, 120 lectures in thirty colleges.

W: Yes, what was going on?

B: The Mathematical Association of America had a visiting lectures program in which you took a semester off and visited many colleges and gave lectures. And I decided to do that. So the spring semester of 1960 I was not here at all.

I had a schedule to visit thirty colleges in the Middle West. So my family and I moved back to Centralia. So we were settled in my mother's house in Centralia, that was my headquarters that semester. And I traveled by car to about fifteen colleges and gave about thirty lectures. No, more than that. It was the... I had a list of, I believe, six lectures. And each college could choose one or two or three, depending on how long they wanted me to stay. And I would give those lectures. It was strenuous, but it was fun. I really liked that. It was completely different from anything I had done before.

I remember there was one disappointment, though. Any college that wanted to could invite me, and they didn't have to pay any of the expenses. I was hoping that many black colleges would invite me, but only four black colleges did. That was a bit of a disappointment to me, but as I say, I had fun.

W: Were you lecturing on game theory and ?

B: Yes, as I say, there were six topics. I think one of them was game theory, yes.

W: I wonder if this sorry?

B: I was just going to say, I'm trying to remember what the others were, but I don't remember.

W: So this must have been a very special time also for your family. You know, your children were meeting their grandparents and spending time.

B: That's right. Yes, that's right. Yes, looking back on it that was important for them, just to get to know Centralia.

W: I wonder, you told me the story of when your wife was walking around in Centralia and a person had said, "Are you one of those Johnsons?"

B: That's right, only he said "one of them Johnsons," "one of them Johnsons!" [laughs] Yes.

W: Why do you think that more black colleges didn't... Was it because people weren't hearing about this opportunity?

B: I don't know.

W: Because there was another time in your career when you were doing extensive lecturing when you did have the opportunity to lecture at a lot of historically black colleges.

B: Yes. A couple of summers, I had summer programs here and brought students here from black colleges for the summer. To recruit those students, I visited the black colleges and gave a lecture. Seems to me that was somewhat later.

W: Yes, in here it says you were the PI [principal investigator] of the Summer Statistics Program for Disadvantaged Students and...

B: That's right. Yes, right there. Program for Disadvantaged Students.

W: So that was actually starting in 1970?

B: Yes.

W: How did that program come into existence?

B: I don't remember except that it was Neyman's idea and Leon Henkin's idea. I don't remember much about it. As I say, Neyman had done something like this earlier and Leon Henkin had a somewhat similar program. And I just know that they encouraged me a lot to do this and...

W: What I know about this is that there was one time when Neyman took a tour of the South, and this was in the civil rights era. And then when he returned, it seemed from his biography. I learned all of this from the biography it seemed that he was very, kind of, awakened to racial injustice in the South. And I understand that at that time the department became very involved in soliciting money for civil rights groups like CORE and SNCC. What I wrote down is that Neyman created a diversity plan and submitted it to Francis Keppel.

B: Yes, yes, I don't remember that, sorry.

W: So with this Summer Statistics Program for Disadvantaged Students, how were you involved in it? I understand that you were the director of it, or PI.

B: Yes, as I say, two summers maybe fifteen students from black colleges came here for the summer and I taught them, and I don't remember other people taught them as well. And I kept track of a few of them for a little while

after that, by the way. Three of them went on to get PhDs in mathematics, I know that. And one of them, a woman, is now chairman of the mathematics department at Florida A & M.

W: Hmm. Why did you feel that it was important to have a statistics program, a summer program that targeted minority students?

B: I wanted them to get a somewhat broader view of mathematics than they were likely to get in their home colleges. See what it was like to be at a big university, and... And also, partly just to get to know their fellow students from other colleges. That would be a valuable broadening of them.

W: You have a very exciting and broad view of education, and what education means. I've noticed that.

B: Well, I hadn't noticed that, but... [laughs]

W: What I mean when I say that is just knowing, recognizing a large part of growing intellectually is interacting with other people.

B: Oh yes. As I've said, when I was in college and before, too, I learned at least as much from other students as I did from teachers.

W: And from this vantage point, in retrospect, do you feel that this program was successful?

B: Why, it was certainly not a failure. I enjoyed it and the students enjoyed it. And as I say, I know that three of the students went on to get PhDs in mathematics. I don't quite know how to evaluate it.

W: How to measure the success?

B: Yes.

W: Did you design the curriculum for the students for the summer?

B: I don't remember, but I doubt it. If I invited someone else to teach the course, I wouldn't dream of telling him what to teach.

W: But did you choose the courses?

B: No! I would say, "I have these students coming here and they will be in their junior year and they'd probably have these courses. Would you like to teach them something interesting this summer?"

W: And that was an invitation that you extended to your colleagues?

B: I don't know, I'm just saying that is almost certainly the way I would have done it. I don't remember.

W: I understand. The other thing that strikes me is that when you were discussing Abe Girshick's kind of impatience with the game of...

B: Kriegspiel.

W: Kriegspiel, and what strikes me is that a lot of the way . I'm wondering, this is a question before I make a declaration. Is a lot of the way that you teach about conveying delight?

B: About what?

W: Conveying delight in the subject?

B: I don't try to do that. The subject has its own delights. If you convey the subject and you do it well, people will feel delight, yes.

W: Good thing I asked the question and didn't make a declaration.

B: Well! [laughs]

W: So, okay. Should we continue looking at your lists of involvements during that time?

B: Sure. I don't remember any of these especially. But, I do remember serving as assistant dean of Letters and Sciences, and I enjoyed that a lot.

W: What did you enjoy about it?

B: Helping students! My job was mostly to deal with individual students who had petitions, and it was my job to recommend that the petition be granted or denied. And then we would all meet, all the assistant deans, and decide on what to do. And I was usually in favor of the students. The student was asking that he be excused from some regulation or be given some exemption or something of that sort. And I almost always said yes. We would have lively debates in the council of deans, and sometimes I'd win and sometimes I wouldn't. But I enjoyed that. And I think I was helpful to the students.

W: And the dean during that time, was it still ?

B: I've forgotten whether it was Walter Knight or whether it was Bill Fretter. I think it was Bill Fretter.

W: Okay. And as an assistant dean of Letters and Science did you interact... Did you start to interact then with the vice chancellors at that time? Was that..

B: No, this was entirely dealing with students, [pause] All of these sound like a lot more than they were! [laughs]

W: They look like a great deal to me, actually.

B: Where are you?

W: Well, I have a couple questions about some of them. Would that be all right?

B: All right.

W: When you're a member of the senate, what does that involve?

B: Now wait, if you could show me where you're looking. You see, everyone is a member of the Academic Senate.

W: Well, here you were .

B: Ahh! Statewide!

W: Statewide.

B: Yes, each campus sends a certain number of representatives to the statewide Academic Senate. And I've forgotten whether the number from Berkeley was five or seven or something like that. And, as I remember it, the statewide Academic Senate decides on admissions standards for the university. And probably not much else. But . I don't remember much about it except that there were bitter arguments, [laughs] Nevertheless, I certainly enjoyed the meetings.

W: Okay, [laughs]

B: I never missed one while I..

W: So, how did you get chosen, you know, out of a faculty of hundreds? How did you get chosen to be one of the five to seven statewide representatives from Berkeley?

B: I don't know.

W: I have questions specifically about some different things, but in particular the committees on discrimination.

B: Okay.

W: And I'm wondering what that experience was like and what kinds of issues were discussed there, if you're at liberty to say.

B: I'm at liberty to say anything, I think, but I don't remember! [laughs] I don't even remember there being such a committee.

W: Well, let's leave it, then.

B: I see Special Committee on Discrimination, Advisory Committee on Discrimination, Advisory Committee on Operations [trails off]. I'm sorry, Nadine, I don't remember these things.

W: Don't apologize, that's fine. That's just fine, because what I am wondering a little bit about also is that you were involved with this group in bio-statistics? Actually, it seems to me that you maybe only did that once. But I was really interested in the ways that you worked with other people in the campus around moving parts of statistics into their disciplines. And I was wondering do you recall if that was something that you worked on, in particular? Like the bio-statistics department in public health, or?

B: Yes, I've certainly been on the bio-statistics committee, but I am not an expert in bio-statistics at all.

W: I know.

B: And my main job was to let the people who were experts do what they wanted to do. So, I always supported whatever Chin Long Chiang or the other people in the bio-statistics group who were running it wanted to do.

W: Can I ask you about this? Do you recall participating in the Committee on Committees?

B: Yes.

W: I understand that that's an important piece of work there.

B: Yes. Most of the work of the Academic Senate is done through its committees. So if you choose the committees, you are in effect choosing the direction that the Academic Senate will go. So that is an important committee, the Committee on Committees. It's elected, and somehow I was persuaded to run for the Committee on Committees and got elected. In fact, at one time, I was chairman of the Committee on Committees.

W: It appears that you did that for two years. Who would have persuaded you?

B: Yes, that's a good question.

W: Or is that just a figure of speech?

B: No, that's a good question. And I'm trying to remember how it happened. See, people have the idea that committees need to be representative of the faculty. And that means that you have to have some science and some art people, it means you have to have some women, it means you have to have some black people. So, probably, I was persuaded to run and elected because I'm black. Who knows? I don't remember, but I would guess that that's how it happened.

W: Do you recall moving the committees in specific ways at all as chairman?

B: Yes. I instituted the idea that we should ask people to volunteer for committees. And I believe that's still the case. That now, the Committee on Committees asks people to volunteer, that was my idea.

W: There's just one more question, which is hi 1969, it said you were the Afro-American Studies Committee chairman for two weeks. And that's what you wrote into your bio-bib, and I wanted to ask you what that experience was like, and why for two weeks?

B: Well, Walter Knight was the dean of the College of Letters and Sciences at that time. And there was this move to establish an Afro- American studies department or something like that. And the students were demonstrating and making noises, as I remember it. Anyway, Walter Knight asked me to serve as chairman of a committee to discuss the feasibility of this department. So I accepted, and I met with a group of students who were active in promoting this. We had a couple of meetings. And my idea was this: I don't know anything about Afro- American Studies, let's look around and find somebody who does know something about it and try to get him to come in as chairman of the department. And I had a specific suggestion, namely a man named John Hope Franklin, I don't know if you've ever heard of him.

W: Of course.

B: You have heard of him?

W: Yes.

B: Good. Well, that was my specific suggestion. He was at the University of Chicago at that time. I wanted to get him to come as professor of history and as chairman of the new Afro-American studies department. So we discussed this and other things a couple of times, and then one of the students, sort of the leader of the student group, came to me and said that they were not happy with my leadership and they wanted me to resign. So I told him, "Well, I'm not going to resign right away. But I'm not going to do anything, either, for a week. You discuss this with your colleagues and if you still want me to resign, come back a week from now. If you still want me to resign, I will." And he came back a week later saying yes, they still wanted me to resign, so I did. I'm not sure exactly why they wanted me to resign, but it's clear that you can't lead a group that doesn't want you to be in charge.

W: That's interesting. I wonder who they wanted to come and be in the department.

B: There were a couple of names, and I would recognize them if somebody mentioned them, but I don't remember now who they were.

W: Okay. And the focus of this conversation, or this group with the students was really about what would a Department of Afro-American Studies look like and how do we want to make it happen?

B: Yes, as I say, they wanted the department . I saw this more as an opportunity to appoint some good black professors. And I thought of John Hope Franklin. I told him about this later, and he said he would not have accepted, [laughs]

W: Why was that, why wouldn't he have accepted?

B: I don't know, maybe he was just happy at the University of Chicago.

W: Yes. That's very interesting. I guess I'm surprised because he's known for doing so much and doing such amazing work. So that's very interesting to me that wasn't something that the students were on board with.

B: I don't know whether it was that they objected to him or whether they just had other people in mind or I don't know.

W: How did the statistics department experience the Free Speech Movement and the protests, student protests and activity around the Free Speech Movement?

B: I don't remember. What I remember mainly, and I don't remember this very well, is that two or three of my classes were disrupted.

W: When you say "disrupted," what happened?

B: I don't remember. But whatever it was, it stopped in a few minutes and the class continued, [pause] Oh! At the beginning of one class a student came up to me and asked me if she could make a few remarks to my students, and I said yes, and she did, and then left. I don't know, announcing some meeting or something or other.

W: Do you remember the feeling among your friends and colleagues in the statistics department around the Free Speech Movement? If there were any splits in the department or ?

B: Nadine, I don't remember. I don't think it touched us very much. I think that was more on the lower end of the campus.

W: Yes. A lot of what I read in Neyman's biography is that in some ways, even when he stepped back from the department, it seems that he was very politically involved. And so that's part of where I'm asking from.

B: He was politically involved, but not so much on campus, I think. More nationwide.

W: Would you say that the rest of the statistics department followed suit in that respect or were other people?

B: I don't think most of the people in the statistics department were involved one way or another in the Free Speech Movement. My guess is that Betty Scott would have been, and very much on the side of the students. I'm kind of vague on that.

W: And during that time what was your position?

B: My position was that the students were absolutely right in what they wanted, but I didn't like all the noise they were making.

W: Were there things that you did actively to help them forward their cause or support them?

B: I don't think so. I doubt it.

W: I wanted to go back to something. There was an air force contract that you were the co-PI on, and you did that work through 1973. And I wanted to ask you about that, and what was the work that you did for the air force? Or were the principal investigator on, and who was the other principal investigator, and questions like that.

B: Government agencies support a certain amount of research in the statistics department just as they do in many other departments. So, they do it through grants or contracts, and each grant or contract must have a principal investigator. Now, if one person is the principal investigator on a contract, and he has three colleagues on that contract, each person works on what he wants to work on. The principal investigator is, so to speak, the poor bookkeeper. He has to prepare the requests for the money and do the administrative work, and the other people simply do their own research. So the PI has another duty, he has not only to do research, but to prepare the proposals and so on.

W: That doesn't sound fair!

B: Well, it's not. So, I think in our department people sort of took turns being principal investigator to share the work.

W: Okay.

B: The agency sort of decided how much money they were going to give to the statistics department, and the way it worked out was that anybody who wanted summer work could usually get it, could be on some contract. And there was some discussion about who would be on whose contract and so on. And I was occasionally principal investigator, but I usually managed to avoid being principal investigator. This continued until 1973 or 74. At that time, I was not doing as much research as I used to do, and I decided to stop working on government contracts. I continued to do my own research, but I didn't get any money from the government for research after that. So, as I say, I just decided it was time to stop. I didn't like the hassle of preparing a request for a proposal and stating what I was going to work on, because I didn't know what I was going to work on. So I just stopped at that time. So if you check it, you will see that up

to 1974 I was on contracts, after that I was not.

W: Was there specific . Was this a similar situation as to when you worked at Rand where there was kind of a framework for doing which was kind of related to battle but you basically could just advance basic research?

B: Yes, the military people were very broad in their views. They took the view that if it advances mathematics or if it advances statistics, it's good for the military. They didn't try to control who... Well, there were some directions that they felt were important, and they would encourage you to work in those directions. For example, computers. The military people were among the first to recognize that computers were very important. So they were happy when people decided to work in that area, but they would support you no matter what area you worked in.

W: Were there deliverables?

B: Deliverables?

W: Yes.

B: Not in statistics, the deliverables were theorems, methods for doing things, [chuckles]

W: Okay. Do you remember, was there a specific problem or area that you worked on especially under these contracts?

B: Whatever I worked on up until 1974 I worked on under the contracts.

W: Okay, I understand. Let's see, it's a little before eleven. Do you have about twenty more minutes for me or would you like to go?

B: Sure.

W: Okay. Let's see, now that I have this twenty minutes I have to think of some good questions for you. Also during that time, right before you stopped working on those contracts, it was in the early seventies, I understand that there was a great deal of activity on campus and in the world protesting the Vietnam War. And I wanted to ask you about one thing in particular. Which is, I understand that in the department, and Neyman in particular was involved with this, even though so much of statistics work had been grounded hi military streams of money for so long, that there was an attitude of "Don't put mathematics in service to this cruel war." And there was actually a quote that I found, and ads were taken out in the Daily Californian by a group of people here in the statistics department. Were you involved in that?

B: I don't remember, [pause] I was against the war, but I would have taken the position that mathematics doesn't have much to do with it, anyway. I don't know.

W: Did the Vietnam War, in particular, make . Did you see a re-evaluation either personally or within the department of the connection between military money and basic research?

B: I don't know how to answer that. As I've said, in mathematics and statistics I don't think military money influenced the direction of research. It simply made it possible to do more, and made people a little richer while they were doing it. But I don't think it determined the direction by any means. I always admired the people in the military agencies for their far-sightedness in being willing to support basic research just generally.

W: Well, at that tune, and I'm thinking of that time from the 1960s through early seventies, and I shouldn't lump it all together, but in some ways I'm thinking of it as one continuum. And I'm wondering how did that time manifest itself in your family? Did that social change come home to you? I'm wondering specifically if your children were war age?

B: The main thing I remember is that several of my children participated in anti-war demonstrations, [pause] I don't remember.

W: I'm also wondering because you were here in the Bay Area, and during that time... Well, it was late sixties, early seventies, when the Black Panthers and black power really came to the fore of the national consciousness. And I'm wondering also, was that something that your children kind of picked up and were aware of?

B: Let's see. I think all three of my sons wore afros at one time or another, [pause] I don't remember any other manifestations, [chuckles]

W: Manifestations is a funny word, that was my word, sorry.

B: Oh! [laughs]

W: In 1973 and 75 you went abroad. You went abroad to become director of Berkeley Study Abroad programs. Was that in London or ?

B: That was in London, yes.

W: How did that occur?

B: I applied for it. I'd always wanted to visit England for a long time. And actually, I think I applied for it once before and was rejected but encouraged to apply again. And then I applied again and was chosen. So I was the director of the education abroad program in England for two years. My wife and I and our youngest daughter, Sara, moved to England for that two-year period. And as I mentioned earlier, one of the things that happened during that time was that I gave up smoking. And it was a big change in lifestyle. My duties were not onerous, they were big at the beginning and end of the year. We had about a hundred students coming in, and we had to place them in seven or eight British universities. So the students would come in, we'd go through this orientation program, and place them in the universities. And I and my colleague, there were two of us, would go and visit them from time to time to see how they were getting along. But our main job was to take care of them when they got in trouble. Maybe somebody would get really sick or maybe somebody would get unhappy and decide to drop out of the program and... To go and keep them out of trouble, we'd have to bail a couple of them out of jail or something like that.

W: So it wasn't very onerous.

B: It was not onerous at all except, as I say, for the beginning of the year, when you had to orient them, and the end of the year when you had to make sure they all got off.

W: So that meant you had a good space of time to really explore England.

B: I had a good stretch of time to explore England and get to know London a bit. And I visited several British universities and gave lectures there.

W: You said it was such a different environment and that was part of what facilitated your stopping smoking. Not in relation to the smoking, but what was so different about it? What do you recall as being kind of strange and different?

B: One of the things that was different was the politeness. The British people "queue," they stand in line and wouldn't dream of pushing ahead of someone else. And that was just, to me, so different and so relaxing. You just stand in the queue in your line and you don't have to worry about anything. And that same politeness showed itself in another way. If you were walking along the sidewalk and an English person was coming the other way, he would get way over on his... As I say, complete lack of competitiveness in the way that I hadn't realized that it was so in this country except for the contrast over there.

Another way it affected me was through the police. I've never regarded the police as my friends. They were always the enemy. Not that they ever did anything to me, but somehow I never regarded the police as my friends. But in England I did. I'm sure that part of it was that they didn't carry guns, whereas American policemen did. But also there's just their general attitude. Their attitude was to try to be helpful. Not to try to control you, but to try to be helpful. I just noticed that those were just some of the differences that I felt over there.

W: Yes, it's major. It's a very different police presence, it's huge. I want to ask you, did you feel that people's attitudes around race were very different?

B: Did I feel what?

W: That people's attitudes around race were different?

B: [pause] No, I'm not particularly sensitive to people's attitudes about race. You sort of have to hit me over the head before I notice, [laughs] No, I didn't notice that.

W: It's 11:10 and I have one last question and then I want to just close for the day, is that okay? My question is about the Rouse Ball Lecture in 1974.

B: It's about what?

W: Rouse Ball.

B: Oh, yes.

W: Am I saying that correctly?

B: Yes.

W: I want to ask who Rouse Ball was, and what he meant to you, and then about the lecture.

B: About the what?

W: About the lecture, being chosen for that lecture and the honor.

B: Oh, yes. I don't know how I was chosen for that lecture. But Rouse Ball was a nineteenth-century British mathematician. And when I was introduced as a Rouse Ball lecturer in Cambridge, I remember my opening sentence, which was, "I had heard of W.W. Rouse Ball long before I'd heard of Cambridge University." And that was true. In my high school library, there were perhaps five mathematics books. I was already interested in mathematics, and I looked at those five mathematics books. And one of them was authored by W.W. Rouse Ball, it was called Mathematical Recreations and Essays. So, that book by Rouse Ball was one of my first introductions to mathematics. So, that gave me a special pleasure to be invited to give the Rouse Ball lecture.

W: Do you remember Cambridge's campus?

B: Yes, I remember it sort of vaguely. One of the things that I remember vaguely is that my wife and I went "punting," as it's called, on the river. I'm not sure, maybe it's the Cam River, I'm not sure. It goes through the college of Cambridge.

W: Well, shall we close for today?

B: Sure.

[end of interview]

INTERVIEW 7

MAY 23, 2002

B: Are we ready?

W: I think so. I just wanted to ask you some questions about being here at UC Berkeley. I wanted to ask you first, was it hard to leave Howard and come to Berkeley?

B: No, I don't think so. I liked Howard a lot, but I knew I would like Berkeley.

W: When you came here, and your family got settled, was your wife part of the Faculty Wives club?

B: Yes, I had forgotten about that, but she was quite active in the Faculty Wives club for several years. I don't remember just what they did. Seems to me one of the things that they did was to run a store where graduate students could rent or buy items at a very low cost, cooking utensils and such things. I'm very vague about it, but that was one of the activities I remember.

W: When you came here, was there a faculty member and his wife that kind of took you up and showed you around?

B: No. Essentially, we were members of the statistics department and so we just participated in the social activities in the statistics department.

W: And do you remember did she form friendships with other faculty wives?

B: I don't remember any in particular, no. She was about equally friendly with all, I think. As I said, there were a lot of departmental parties and we would all go to them. Sometimes, we would have parties at our house.

W: Did you have other close friends outside of the university in the Bay Area?

B: No, I don't think so.

W: And I realized, I was also wondering if there were any of your fraternity brothers that you were still in touch with, or new ones that you were in touch with?

B: I see some of them from time to time. I ran into one of them last weekend at a retirement party for another faculty member. But I have not been at all active in the Alpha fraternity.

W: Since graduate school?

B: Right.

W: I went to the Alpha web site and I learned that John Hope Franklin, who you've mentioned several times, was also an Alpha, which I didn't realize.

B: I didn't know that, but I'm not surprised. We expect good black scholars to be Alpha men.

W: Okay. And can I ask, who was the other faculty member who was also an Alpha who you met at that retirement party?

B: The retirement party was for Oily Wilson, but the other Alpha man that I met was Jimmy Johnson. I first saw him when he was running the quarter mile on the Cal track team many, many years ago. He's now a very successful real estate man. I hadn't seen him for many years, and I ran into him at Oily Wilson's party.

W: So I have a question that I neglected to ask previously, with regard to the fact that your coming to Berkeley in 1954 coincided with the Brown v. Board of Education, which desegregated public institutions.

B: Yes.

W: I just wanted to ask you, were the two connected in your mind at all?

B: No, except that I knew that something important was happening before the Brown v. Board of Education decision was announced. You know, the lawyers at Howard University had an important part in pleading the case before the [U.S.] Supreme Court. They were very excited at that time at Howard. They knew they were doing something important. I didn't know exactly what was going on, but it was just in the atmosphere. It was an exciting time.

W: When you came to Berkeley, did you watch this institution first, were there aspects of Berkeley that were segregated, and did you watch them desegregate?

B: Would you ask that again?

W: Were there parts of this university that were segregated, and did you watch the process of desegregation occur? As a result of Brown v. Board of Education?

B: Oh, I don't know that the decision had any immediate effect. When I came to Berkeley, it wasn't formally segregated, of course, there were just very few black people around, and that didn't change suddenly, but [long pause]. It's hard for me to go back to those days. That was such a long time ago.

W: I wonder if when you came here, were people talking about that decision? I understand that people were talking about it at Howard, or it was in the air at Howard, but I wonder if it was in the air here at all.

B: I don't remember much discussion of it, no, no. It took some time for the decision to penetrate. But, as I say, the lawyers at Howard knew that something big was happening.

W: Yes. Okay, [pause] Do you think that that decision, Brown v. Board of Education, had any impact on UC Berkeley's hiring you decision to hire you that year, in 1954?

B: No, I don't think that race or that decision had very much to do at all with my being hired except that it gave Jerry Neyman a special pleasure to hire me because I'm black.

W: Was that because of his politics or where he came from?

B: Well, certainly his politics entered into it. Ever since I've known him, he's been interested in the problems of black people, and he's wanted to do things.

W: Over the course of your relationship, did you have conversations with him about these issues?

B: We had several conversations about what could be done to help black people, yes. [pause] Neyman started a program here that's still going on. I can't think of the name of it.

W: I'll find out.

B: Okay.

W: What do you think it was about his background? Was it because he came from another country, or was it something in his background that he identified with the issues that confronted black people here in the U.S.?

B: I don't think of it as being his background; I think of that as just being his personality, his makeup.

W: I wanted to ask you this question about when you came to Berkeley, after were the Evans still around?

B: Oh, yes.

W: Yeah.

B: They were friendly to me and my wife. In fact, they invited us to their house.

W: Oh!

B: A lot of things changed between 1942 and 1954. Big change in attitude.

W: Do you mean in this society, or just with the Evans?

B: Oh, big change in society. In 1942, segregation was the norm. In 1954, it was still widespread, but not quite respectable any more.

W: And to what do you attribute when you think about how that happened? How do you think that occurred? What brought about that change?

B: I don't know, but I'm sure World War II had a lot to do with it. The black soldiers coming back from World War II regarded themselves as full citizens.

W: I have a question also about this question is also kind of centered on your wife and family I wanted to ask what role has your wife played in your career?

B: That's so big!

W: It is big. [laughs]

B: I don't know what I would have done without her, what do you mean, she ran the family... That's just too big.

W: Yes, and was that a pretty big job, taking care of the family, making sure everyone was okay?

B: I think so, but she never complained about that. She always seemed to have lots of energy.

W: When it came time for your children to go to school and you sent them to Berkeley public schools, were you glad of that choice?

B: Why, yes. The only time that they didn't go to public schools was when my daughter Sara was just five years old. But she could already read pretty well, so I thought she was ready for first grade. But the public schools were very rigid on that, "No, no, she was born too late." So we sent her to a private school, Bethlehem Lutheran School, right around the corner from where we live. And that was very successful. But still, after that, we put her back in the public schools.

W: And were you and your wife teaching your children to read?

B: Not intentionally, but we read to them a lot. And I know that Sara learned to read from Dr. Seuss books and other picture books that we read to them. She had memorized what was on each page. So, she simply...

W: picked it up.

B: Picked it up.

W: When you came to Berkeley, were there any other black professors here that you remember?

B: There were two black faculty people that I remember. But they did not, I think, have the rank of professor. One was a man named Staten Webster, who was in the education department. I'm not sure what his rank was. And the other was in the School of Engineering. But he was, again, not a professor. And as far as I know, those were the only other people that had any kind of black only other black people that had any kind of faculty status.

W: I've only heard of Joseph Gier in engineering.

B: Joe Gier! That's right. Was he a professor?

W: Well, it's been very confusing for me, it's very confusing. I've been trying to determine if you were the first black professor, ladder-rank. There's an "In Memoriam" for Joseph Gier where it says that he was the first tenured black professor.

B: Yes, I wouldn't be surprised. He was certainly here when I came. And he had some kind of faculty position, there's no doubt about that. You have to be careful when you talk about "firsts." [chuckles]

W: Yes, I definitely, I understand that. So, did you communicate very much with these other two faculty?

B: No. With Joe Gier, I think I just had one or two conversations with him. I did talk to Staten Webster several times over the years. I don't remember much what it was about, but I know that I had some interaction with him, yes.

W: What was it like to be one of three black faculty on a campus of hundreds of faculty?

B: There was nothing special about it. My main associations were with the other members of the statistics department.

W: When did you begin to see other black faculty in more significant numbers?

B: I don't know, it was just gradual and it happened without my being particularly aware of it. For instance, Bill Shack [William A. Shack] was on the faculty for a year or two before I even knew that he was here.

W: Did you often face from people, from other faculty and administrators, the expectation that you as a black man would be very concerned with issues that had to do with diversity and race?

B: I'm not sure what their expectations were. When Walter Knight asked me to chair this committee about the Afro-American studies department, I'm sure that the reason that he asked me was that I am black, [long pause] It's hard to know how much blackness has to do with things.

W: For example, I'm thinking about how Leon Hinken and Jerzy Neyman really suggested that you teach the program for disadvantaged persons. And I was wondering if that was kind of often, where people said, "Hey, you know " Kind of often times expected you to take up issues specifically because the issues were black and you were black?

B: In both cases, I don't think they particularly expected me to do anything, but they knew that I would be interested.

W: Right.

B: It's hard to know what other people's expectations are.

W: I understand. In math and statistics, in the two departments, in math and statistics

B: Yes.

W: I haven't located other African American professors. Were there others?

B: No!

W: In general, in the fields of math and statistics, do you feel it is a field that African American people go into?

B: For a long time, it was not. And I have my own explanation for this. I'm not sure how much validity there is to it. For a long time, the idea was that if you study mathematics, the only thing you can do is to teach mathematics. And teaching had, and I think maybe still has, a low status among black people compared to being a doctor, or a lawyer, or an engineer, or a politician, or a preacher. Teaching is just low status. And I think that caused many black people who could have done very well in mathematics to go into other fields. Just because, as I say, if you're a mathematician all you can do is teach, and teaching is low status.

W: Oftentimes low pay compared to being a doctor or an attorney.

B: Low pay, but I have the feeling status was more important than pay. For instance, when I was at Howard I used to ask students, "What would you like to do after you graduate?" And a very common response was, "Anything but teach." And a lot of them did go into other things.

W: I remember when you were telling me about that, when I was asking you how you brought your students at Howard into an academic career of teaching and basically learning math. And you were saying, "They generally wanted to go into other areas, that was their priority." And you're saying that now things are a little different, that there's more black people in math and statistics?

B: Oh yes, especially in mathematics. Still not very many in statistics. It's kind of hard to get into statistics. Most people come into statistics from some other area. Either mathematics or biology or something else. But yeah, for a long time, I actually knew personally all the black mathematicians with PhDs in mathematics. The number was that small.

W: In the United States?

B: In the United States, yes. [laughter]

W: Oh my goodness!

B: Yes, we all knew each other. But that day has long passed.

W: Was there ever a time when everyone got together and had a conference? Or did everyone just kind of move in the larger world of math and statistics?

B: Well, there were two worlds. The world of mathematicians who taught in the black colleges, and the others. All of us who taught in black colleges would see each other at national meetings of an organization whose name I forget now. It was black professors of mathematics and science. And we would all see each other once a year at those meetings. And the few black PhDs who were teaching in other colleges I don't know, we just sort of knew each other.

W: When you went to these conferences every year, were there people who you were particularly very excited to see and catch up with? People whose mathematical activity was very exciting to you? Or just friends?

B: Just friends.

W: Yes. I'm wondering if there's any people in particular that you remember?

B: No, I'm kind of vague on that.

W: Okay. And in these conferences, what kinds of issues were discussed? Was it all about was it focused on mathematical activity?

B: The focus was on science. And mathematicians were there just because mathematics was considered a part of science. But there wasn't much mathematics in the meetings. It was just a chance to see your colleagues from other colleges.

W: Okay. I'm also wondering about was there any conversation about strategies for bringing more black people into the sciences?

B: No. This is going to sound strange. That wasn't regarded as a problem. The problem was to open opportunities and then leave people free to choose. It didn't bother me that people trained in mathematics went into law, for example. But it certainly bothered me that there were not more opportunities.

W: How could one change that in that time?

B: Just by continuing to knock on doors and make noise.

W: When I was talking to Al Bowker, he said . He was talking about you and he said, "David is of the opinion that it will be another generation before race relations improve." And that's a very relative comment.

B: Yes.

W: Do you know what he was talking about? What he was referring to?

B: Not exactly. But I guess my feeling is that attitudes change not so much because individual people change, though they do change to some extent, but because new people come along with different attitudes.

W: Have you watched that occur in your lifetime?

B: Well, how can you miss it? [laughs] Just look at the way race is treated on TV, for example. It's treated as if life is integrated.

W: Do you see that as reflecting a reality?

B: Well, sure. I see it reflected in my own children. They're pretty integrated.

W: When you say that, do you mean that they are moving

B: Oh, some of their mates are white. And they just don't seem to make a big distinction.

W: In the beginning of this interview, I remember I was asking you these questions. I was like, "What was it like for you as an African American in graduate school?" And after the interview you said, "Nadine, this question you're asking ." You said, "Nadine, I'm always black, everything I do is black," or . You had this very interesting way of saying it, which I thought was very important. Partially because it just kind of illuminated the difference in the way you and I talk about race, but also because it was a... I just thought it was a very important statement.

B: Well, it's just so obvious. It colors everything you say, do, and think.

W: Yes, it does.

B: Just as being female colors everything you say, do, and think.

W: It really does.

B: You can't avoid that, you don't want to avoid it.

W: I have a question also, and this is focused on UC Berkeley and it was . It comes out, again, out of a conversation that I had with Al Bowker. I was wondering if you remember when Harry Edwards he was a professor in sociology, and I believe he was up for tenure or he was...

B: Yes, yes, I remember that vaguely. And I remember that Al Bowker was the one who got him tenure. But I don't remember the details at all.

W: Okay. I think what Bowker said about it, he said that there was a whole . There was actually a big controversy around Harry Edwards tenure. And he said that many of the African American faculty wrote letters, and that you felt it was very important not to lean on your personal relationship with Al Bowker to make this happen, which I thought was very interesting.

B: I don't remember that. Al and I have been friends for a very long time.

W: I know. He also told me this interesting story about how you I guess probably when you first meeting, it was in the very beginning of your friendship about how you drove up to Cornell together.

B: Yes. I don't remember where he was going or where I was... I don't remember at all how it happened. I remember this long automobile trip that we took together.

W: I think you were going to a conference at Cornell or something of that nature. When did you meet Al Bowker?

B: I think I met Al Bowker at an IMS meeting at Cornell in 1946.

W: What did you notice about him? His appearance and personality? His mathematical work?

B: Two of the things that struck me about Al were his calm, imperturbable demeanor and his interest, even while a graduate student, in academic administration

W: How did you become involved in the chancellor search committee in 1971? Who else was on the committee with you?

B: Before being appointed to the chancellor search committee, I had been active in the Academic Senate, serving as chair of the committee on committees. And I expect that being black had something to do with my appointment to the search committee.

W: What role did you play in bringing Al Bowker to Berkeley?

B: As a member of the search committee, I wrote to several people for suggestions. One of them was Allen Wallis, president of Rochester University and former head of the Statistical Research Group. He recommended Al Bowker, describing him as "far and away the best man you're at all likely to get." I showed this letter to the committee, and they started looking at Al.

W: Have you worked or socialized together over the years?

B: Al and I have been close, personally and professionally, since 1946, at Stanford and Berkeley.

W: Did you ever give him advice or feedback when he was chancellor? Do you have a perspective on his actions when he was chancellor?

B: I wouldn't dream of giving Al advice. I think that he became bored with being chancellor after a couple of years, finding it not challenging enough. Al once said that his policy, when confronted by the demands of black activists, was "instant capitulation." I don't think he was joking, though he may have been referring to his policy as chancellor at CUNY.

W: I'm wondering if you can tell me before I move on to some of the awards and honors you've gotten, I have this photo that I found. And I wanted to know if you could tell me a little about it, it looks like it's from 1960.

B: Yes. There was a big army base down at Fort Ord, I believe. And the people in this picture were on some committee to evaluate something that was going on at Fort Ord. And I remember that we spent two or three days down there. This is my old friend, Kenneth Arrow. And this is Bill Shockley. You know about him as a racist and as a prize-winner for inventing the transistor or something. He had strong racist views and he somehow wanted to get me in a discussion and maybe an argument. So, whenever possible, he would sort of seek me out. But it didn't bother me, I just treated him in a friendly way and refused to argue with him.

W: Yes, you said you two, surprisingly you got on well together.

B: Yes!

W: I'm adding the "surprisingly."

B: What?

W: I'm adding the "surprisingly;" you just said you got on well together.

B: Yes.

W: Yes. I understand he was kind of active in . He was thinking hard about theories associated with eugenics, and it sounded like he wanted to make some mass generalizations about people's intelligence and abilities.

B: Yes. Once, for instance, at some meeting several years after that. It was a big meeting and we were sitting down to dinner, he just came over and sat next to me and started a conversation with me. He wanted to talk to me about race, [laughs]

W: What kinds of things would he say to you?

B: I don't remember, but ... [chuckles]

W: Did he regard you as a friend?

B: What?

W: Did he regard you as a friend?

B: No. But he didn't regard me as an enemy either. He just wanted to talk about race.

W: That's very interesting. How did you manage to not start not be in arguments with him? How did you land of deflect his line of conversation?

B: I don't remember.

W: Okay. Is there anyone else in that photo who you ? You really just marked Kenneth Arrow and Shockley. And then there's you, looking very serious.

B: [laughs] That's right!

W: Right next to Shockley.

B: Um-hmm. Well.

W: That's a wonderful photo.

B: Good.

W: I wanted to ask you some questions about some of the awards you've won, because you've won some pretty big awards, and I have the list of awards here. This is your biography, and it doesn't include all your honors and awards, but it includes some of the bigger ones, so . It goes from this page to this page. In particular, I wanted to ask you about being elected to the National Academy of Sciences in 1965. I was wondering, how did you hear that you'd gotten that honor?

B: The way I heard it was I got a telegram from [President] Clark Kerr congratulating me. [laughs] He, well, it was just . Presumably, one of the many things that he did was to find out which Berkeley members had been elected and send a telegram to each one of them. So that's how I found out about it. I somehow knew that I was under consideration, but I didn't expect anything to happen because most people who are under consideration don't get elected. And I hadn't realized what an honor it was.

W: Yes.

B: I was very surprised at how much of a fuss was made over it.

W: How did you come to realize what an honor it was?

B: After that, whenever I was introduced, "And a member of the National Academy of Sciences!" [laughs]

W: Yes.

B: It's not something that you aim for. It's just if it happens, that's great.

W: Were there other honors that you got that meant a great deal to you, as well?

B: Yes, there were two honors that I'm really proud of. One was my honorary degree from the University of Illinois. And the other was my honorary degree from Howard University. I've gotten a number of honorary degrees.

W: They're all here.

B: Well, there are some that are not there, too.

W: Okay.

B: But there were two places where I was that I stayed at a long time. I was at the University of Illinois for six years. And I was at Howard University for ten years. So, when you get an honorary degree from a place where the people really know you, that's something to be pleased with, I think. As I say, I was very pleased to get a degree from Illinois and to get one from Howard.

W: I have a question about the degree from Illinois.

B: Okay.

W: So you had a degree from Illinois.

B: I had three degrees from Illinois.

W: Three degrees from Illinois.

B: Right.

W: And they gave you an honorary one in addition to that.

B: Yes.

W: As just a way of honoring you, yes. Okay. Can I ask you about that Von Neumann theory prize, to me that also sounds like a pretty big deal.

B: No, it's not, though. The Operations Research Society gives one or two awards every year. And I think they awarded it to me for my work in dynamic programming and optimization theory.

W: And in 1988 you got the Berkeley Citation as well?

B: Yes, that's just recognizing lots of activity on the Berkeley campus, teaching a long time, and serving on a lot of committees, being useful to the university, that's all.

W: There also was an honor that I understand is a very important one is to be you were also a Faculty Lecturer?

B: Yes, every year there is a committee that selects now it's two, it used to be one- member of the faculty each year to give a special lecture. And, I was very pleased to be chosen for that.

W: It's definitely recognition by your peers.

B: Yes, that's right. Again, recognition by people who know you and your work.

W: Do you remember what you chose to... What did you choose to lecture on?

B: Sure, I talked about game theory, [laughs]

W: Good. There was one other thing that I was really interested in that we never got the chance to talk about which was in 1954 or 1955, I'm not sure which, but you went to the Netherlands, I think?

B: Yes. This was... There was an international mathematical society, I've forgotten its name. And I . I got some money from some government organization to go to that meeting and. I was invited to give a half-hour lecture, which was an honor. I enjoyed it a lot and met a lot of the people whose names I had seen.

W: Yes.

B: And I took a picture of my thesis advisor, Joe Doob, talking to Kolmogorov.

W: Very special, very special picture.

B: Very special picture. Not a very good picture, but a picture that was special. I sent Joe a copy of the picture. He never acknowledged it, but maybe thirty years later I happened to visit him at his house in Urbana and went in his study and there were three pictures in his study. And that picture was one of them! That really pleased me.

The other thing that I remember about that meeting has nothing to do with mathematics. All the people at the meeting got a bus pass that enabled you to ride free on the Amsterdam buses. There was some place that I wanted to visit in Amsterdam, and I wasn't sure how to get there by bus. I asked a man how to get there by bus and he said, "Come on and I'll take you there." He got on the bus with me and rode with me to the building that I wanted to visit. That was such a special courtesy that I couldn't get over it.

W: He dropped you safely off?

B: Oh yes, yes. I think I was wearing something that showed that I was a visitor or something.

W: Beyond those trips, I know you've been to Europe, I think several times.

B: Yes.

W: And though the only two that we've really talked about was London and this one, but I wonder did you ever visit any of the places that were, like, kind of like the birthplaces of your discipline in some ways? For example, I'm trying to think of what would be a good example. The place where R.A. Fisher and Pearson ?

B: Well, I certainly . I visited Bayes burial place and took pictures of his tomb. In fact, I was looking at the picture of Bayes tomb this morning, oddly enough, [chuckles] One other place in Europe that I visited for sort of professional reasons was in Paris I visited the place where a man named [Wolfgang] Doblin had gone to school I believe it's l Ecole Normale Superiore, because I learned a lot about Markov chains from a paper that Doblin wrote. And I developed a special affection for him. I'd never seen him, but I admired him a lot.

W: You liked the way his mind worked?

B: Yes! Yes. There are... Well, at that time, I recognized two basically different approaches to probability. In one approach, if you have a probability problem, you change it into an analysis problem as soon as possible. And you use all the techniques of analysis to solve the problem, and then you translate the results back into probabilistic terms. But in the other approach, you never leave probabilistic thinking. You think probabilistically all the way. And that was the way I like to think and that was the way Doblin thought. I could follow his way of thinking and I'd learn something every time I read something that he wrote. With the more analytic way, you can follow the steps. You say yes, yes, that's right, but it is not clear what each step has to do with the original problem anymore. So, I admired Doblin a lot.

I may have told you that one of my papers was a solution, was an answer to a question that he had asked. And so I wrote that paper for this one man, for Doblin, but alas, he never got to read it. He was either killed or committed suicide in World War II. I think the Germans were about to capture him and he committed suicide.

W: I think I've heard of him, I think I read a bit about him. Can you tell me what was the question you were responding to?

B: I could tell you, but it wouldn't make much sense. The question was whether anormal chains existed. He proved a lot of beautiful results that were true for normal chains, and went on to say that he didn't believe there were any chains that were not normal. He didn't believe there were any anormal chains. Well, I constructed one and wrote a paper. The only one who would have been interested was Doblin and he was dead, and I didn't know it. I'm sure nobody paid any attention to that paper, [chuckles] How'd we get into that? Oh, we were talking about Europe, I guess.

W: Yes, and you were telling me about places that were important to you that you visited.

B: Yes. [pause] Well, I visited a lot places, but those were two that had a special meaning for me because they were associated with people that I admired.

W: I have a question which is about Bayes. When was Bayes mathematically... When was he active? When was he alive?

B: Let's see. I believe he died in 1761 .

W: Oh!

B: [laughs]

W: I think I had him in my mind in the nineteenth century.

B: No, no, no.

W: No, no, no, okay. You were describing Doblin s approach and your approach. I wanted to ask you a bit about intuition. And, I wanted to ask you what role do you think intuition plays in the way you approach your mathematical activity?

B: I don't know quite how to answer that. I always start out . Or I usually start out with the idea that something is probably true. And that is surely just intuition. And then you try to prove that it's true and sometimes you discover that it isn't. But I think I'm never really neutral. I don't just start at, "Is this true or is this not true?" I always start out with an attitude, it's true. I think it's true, let's try to prove that it's true. But occasionally, in trying to prove it's true, you keep bumping into obstacles and then you decide, "Well, maybe those obstacles are really there, let's try to use them to find an example where it's not true." But again, until you actually nail it down, it's just intuition, just a guess.

W: So when people in math say intuition, what do they mean?

B: To me it's not much different than a guess, a hunch.

W: I've heard the word applied to your work and also Paul Levy s work, [pause]

B: Well, I'm flattered being mentioned in the same sentence with Paul Levy. Yeah, he's another example of the way I describe Doblin. His thinking is almost all probabilistic. He does analytic things, but you learn a lot of probability by reading what he does.

W: When you say "probabilistic," what does that mean?

B: That's hard to describe, [laughs] Thinking about coin tossing, for example. Just imagining tossing a coin a lot of times and thinking about what is probably going to happen, what is likely to happen.

W: Yes, Leo Breiman has said that working with you around information theory, he really credits you with turning on the left side of his brain, basically. And teaching him about intuition.

B: Oh, the left side is the intuitive side?

W: I'm not sure about that. I think people generally associate the left side of the brain with creativity and art. But I was just taking his turn of phrase and bringing it over. He kind of located himself in these two traditions of Michel Loeve .

B: Loeve?

W: I think it was. He locates himself as the product of two traditions, and yours is one.

B: And Loeve is one?

W: Yes, I believe so.

B: Yes, that's right. Loeve was more analytic and I was more probabilistic, intuitive.

W: It's interesting, those words that come into math and mean different things. Words like intuition and fair and different words.

B: That surprises you?

W: A little, yes. I actually find it very delightful. Because I like when people bring words and kind of valence them in new ways, I think that's very wonderful.

Yes, Breimen told me this story that you would slip him notes during boring seminars suggesting that he prove these fundamental aspects of information theory, [laughs]

B: Yes, Leo could do things that I couldn't possibly do. He... And sometimes, I had a feeling that he could do them!

W: How did you come to recognize the importance of information theory and the relevance of information theory? I understand you were one of the first.

B: Jimmie Savage told me. He said something like, "You ought to look at Claude Shannon's paper." I don't think he said much more than that, but I trusted him, and did. As I told you, I learned a lot from other people, my contemporaries, people just a little ahead of me. Those are the ones that I really learned from.

W: I guess last week I emailed you this question about what are interesting questions that you would like to see posed to mathematicians? And I was wondering if you had any?

B: Yes, I don't have any now but I used to have questions. In fact, one of my strategies was this: if I'd worked on a question for a while and wasn't able to get anywhere, but I couldn't stop thinking about it, I would go around and bother other people with the question hoping that one of them would be able to answer it, so that I could think about something else. And that happened at least a couple of times. There was this question on comparisons of experiments.

W: You told me about that.

B: I told you about that. And so finally Charles Stein answered the question. Partly, I think, to keep me from bugging him about it, so to speak. And there was a question of the determinacy of Borel games. David Gale originally raised the question, but I was extremely interested in it, and one of my students, Morton Davis, made some progress in solving it. I kept bothering people about it, and finally, a logician named Tony Martin settled it. That all Borel games are determined. So, when I have a question, it's not so much that I need to solve it, I just want it solved. I want to know what the answer is, and if I can get somebody else to do the work, that's fine with me.

W: It's funny, you answered that question in a very different way than I had asked it, and I appreciate that.

B: Good.

W: You know you mentioned one of your students, and I wanted to ask you... This is a big question, what did your relationships with your graduate students mean to you?

B: Each one was different, [pause] I can't say anything general about that...

W: We were just talking about your graduate students and I asked this very general question. I'm wondering if there are graduate students that stand out in your mind.

B: Well, as I said, each one is different. Let me give you an example. Dorian Feldman. He didn't do very well on his oral examinations and people considered cutting off his support as a graduate student, but I argued in favor of giving him another chance. The basis was not that I had any statistical contact with him, but that he was far and away the best Kriegspiel player in the department. And on the basis of my recommendation made because of his Kriegspiel he was kept on for another year. One day, during Kriegspiel, I happened to mention a statistical problem to him, a problem that I had worked on a long time without success. And he said, "I want to work on that problem." And he worked on it, and he solved it beautifully. I and several other people that I thought were far more talented than Dorian Feldman had worked on it unsuccessfully. But Dorian took the problem, saw it as one that he liked, and solved it. Now, that's just one example, and no other case would be like that at all, you see. Each graduate student is different.

W: What was that problem? Do you recall?

B: No, it had to do with which of two treatments you should give to a sequence of patients in certain circumstances. It was phrased in terms of one-armed bandits which arm should you pull? I don't remember the exact problem.

W: Okay.

B: Again, I guess that's another example of my bugging other people with problems. I have a problem and I've worked on it, I can't solve it, I keep telling other people about it.

W: Well, it also seems as if you just had an atmosphere that was really full of math and statistics problems, and if people came into your orbit, then they were that was part of the air they were breathing as a result of it.

B: You're right. My Kriegspiel partners had been all statisticians and game theorists so it all gets mixed in. That's right.

W: I think you have maybe over a hundred graduate students who worked with you.

B: I don't think it's anything like that number, but maybe fifty.

W: Oh, okay. Sorry. Wrong number. So, maybe fifty or around that?

B: Uh-huh.

W: And I know there were a lot of you know, that everyone was very unique in their own way, and I'm wondering about Jonathan Nkwuo.

B: I don't remember very much about him.

W: Okay. And the other one is Wesley Thompson.

B: Yes, Wesley was in the mathematics department. I don't remember how I happened to have him as a student. I don't remember what his thesis was about anymore, except I liked it a lot. It was a very good thesis he was a very talented man. He was drowned a few years later. I don't remember now exactly what happened to him.

W: Those are just two that I found on the web site.

B: Those are what?

W: Those were two that I found on a web site your graduate students. When people say, "Someone is a student of mine," or a chip you know how there are mathematical terms like "This is my mathematical parent, this is my mathematical son or daughter" more often son.

B: It simply means that he wrote his thesis under my supervision.

W: Okay.

B: That's exactly what it means. And every person has exactly one thesis supervisor. As I say, the relation between the supervisor and the student varies a lot from case to case. Sometimes, the student simply writes a thesis and looks around for someone to sign it as his supervisor and there is very little interaction. Sometimes, it's the supervisor's work that the student just adds a little to, and there are all kinds of gradations in between.

W: If you think of graduate students that you would particularly like to talk about, then please do when you think of some. I have a question about what I've been reading is that people often connect music to math.

B: Uh-huh.

W: And, I wanted to ask you, what do you know about this? Have you heard of this as well?

B: I don't understand the connection myself, but I have the impression that a lot of mathematicians are musical, more so than say chemists. But I don't know why that should be the case.

W: I understand. I think the question that I was trying to get to, which is that I've heard you are a lover of jazz music.

B: Oh, yeah. Except the last my taste cuts off at about 1940. I have a theory that I haven't really tested very much: that you can't really appreciate music that develops after you reach the age of thirty-five, or something like that. It sure is true with me. All jazz music, up to about 1940, I really like. After that, it seems to me that it deteriorated.

W: Who are your favorites?

B: Oh, well, let's see. Louis Armstrong, Count Basic, Fats Waller, Sidney Bechet, one maybe you never heard of Don Ewell you never heard of him.

W: You're right. You got me.

B: His piano style is, to me, just perfect. Another one of my favorites is James P. Johnson. Did you ever hear of him? You had heard of the others?

W: I'd heard of all of the others.

B: Poor James P. Johnson. He's, I think, one of the most neglected people in the history of jazz. He was a New York piano player. He was Fats Waller's teacher. And he was just wonderful.

W: I have been told that your wife is also a beautiful jazz pianist

B: Yeah, that's right. Alas, she is not well now. She doesn't play much any more at all. But, oh yeah, she was especially good with her left hand. She has large strong hands and the way she plays those tenths with her left hand [imitates the sounds]. Oh boy, that can make you jump up.

W: In the beginning of our interview, we talked a little bit about your parents, but I just wanted to ask you a little bit more about what you remember of them and their personalities.

B: That's so general.

W: I know, [laughing]

B: I don't know, if you ask something very specific, I could probably answer it, but

W: I'll try this one. It might be again too general. What are your memories of your father?

B: First of all, we called him Pop. Pop's first love was the Illinois Central Railroad. He had just one job with the railroad all his life, and he loved those steam locomotives. Did I tell you about his \$25,000 accident?

W: No.

B: No? His job was to take the steam locomotives from the engineer and put them into the roundhouse. Now, to put a steam locomotive into the roundhouse, you run it into a turntable. The turntable rotates, and when the engine is facing an empty stall, you stop the turntable and drive the engine off the turntable into the stall. So, Pop was doing that, but he somehow neglected to line up the turntable with the tracks. So, he ran the engine into the roundhouse and the engine went down a big incline. Bamm! My father was hurt slightly, but more important, from the railroad's point of view, he did \$30,000 worth of damage to the steam locomotive and the turntable and the roundhouse.

W: Hmm. That's a lot of money, then.

B: It was a lot of money. That's right. So, they sent him home. And told him not to come back. So, it was panic in our house for thirty days. After thirty days, they told him to come back to work. That really hurt him. He couldn't stand the idea of not going to work with those steam locomotives he just loved it.

W: Uh-huh.

B: I used to go down there and he would take me for rides on the engine with him. And we would go for a joy ride, two or three miles down the tracks, and come back. Work was a big part of his life. He was extremely good to me. He used to take me with him many places. People said that I look like him. He grew up in a small town called Mounds, Illinois, about a hundred miles south of Centralia. I used to spend the summers in Mounds with some of the people that he grew up with. And I remember once I was walking down the street and a man sitting on the porch who I'd never seen before in his life, said, "Hey, little Graver!"

I went over and he told me that he had known my father when my father was my age, and I looked just the way my father looked then. Well, this story has no place in a document like that, but I just you asked me.

W: I did ask you.

B: Okay.

W: I really did. I'm really glad you responded. And your mom, I'm also wondering what kind of person she was, what her personality was like?

B: She was a very strong and independent, well-organized woman with lots of ideas. She had everything to do with running our house, including some of the things that you might think of as a man doing. For instance, if there were any repairs to be made around the house, it was her job to find a carpenter to repair them. Pop just had nothing to do with any of that. His job was to work on the railroad and bring home the money, and it was her job to manage it. Mom owned a couple of rental houses and ran them. She was very active in the church.

My parents gave us a lot of freedom and we loved it. I was just free to wander all over Centralia, for instance. I did. You didn't have to account for where you were. You just better not get in any trouble.

W: How would you describe yourself as a child?

B: I liked to play games. Checkers, chess, marbles, and more active games like baseball or softball. Track and field we used to organized track meets. I think I may have mentioned this before, but the children organized things that the adults had nothing to do with. We had a Southtown baseball team that played the Northtown baseball team. And adults had nothing to do with that at all. We organized it ourselves and played ourselves. Organized track meets ourselves. When I look at how much parental supervision there is nowadays, I feel sorry for the poor kids. They don't know what independence is like. I didn't want to grow up. I really enjoyed being a child.

W: That's pretty wonderful.

B: Well.

W: Yeah. This is a very small aside, but were the Pullman Porters present in Centralia?

B: Oh, yes. One of the most important black people in Centralia was a Pullman Porter. Yeah. That was regarded as a very good job. How did you happen to ask about the Pullman Porters?

W: Mostly just because I know about railroads and when you said that there's the East Coast terminus, there's a West Coast terminus, and it was where the two met, then it made me realize that that probably was a factor in your community.

B: Yeah. Oh, it was a big factor, yeah. I still get a special feeling every time I see a picture of a steam locomotive.

W: That's why I brought you this, [laughter]

W: I wanted to switch over to something I had wanted to ask you about. You told me this runny story about Centralia, about how there were two parts of Centralia and they were kind of in competition with each other?

B: You mean Northtown and Southtown?

W: Yes. Can you describe that for me?

B: Yeah, sure! Now, this is purely among black people that I'm talking. Most of the black people in Centralia lived in Northtown. And where they lived, there were practically only black people. But there was a few black people who lived in Southtown and down there, there were mostly white people. And there was a kind of a rivalry between the Southtown black people and the Northtown black people. I think the Southtown black people thought that we were better.

W: So you were staying in Southtown.

B: Yes. And the Northtown black people knew that we thought we were better, [laughter] So there was a certain amount of competition and rivalry.

W: Was it organized in any kind of formal sports or anything like that?

B: Oh, sure! We, the Southtown black people, had a softball team and the northerners would organize a softball team and come down and play us or we'd go up there and play them. Sort of a friendly rivalry, but rivalry, nevertheless.

W: Understood. Was the Northtown community also characterized by people who worked on the railroad?

B: Yes. Most of the black people in Centralia who worked for the railroad. Yes, that was by far the biggest occupation. I think that was another reason why we felt superior. Because we lived a mile and a half closer to the shops than they did. And it didn't make sense to live so far from where you worked. I mean, they had to walk about three miles each way, every day to go to work.

W: How did that Southtown community start, how did it begin? I mean as you are describing it to me, I'm realizing that it was a community that was new and distinct.

B: Well, most of the black people in Centralia came there, or their ancestors came there, in 1912 or in 1919 as strike breakers. The railroad workers at the Illinois Central struck, and the railroad replaced them with black strike breakers. So that's how most of the black people got to Centralia. My father came there in the 1912 strike. I guess many of the Southtown people came there in the 1912 strike.

W: I didn't know that before.

B: As the result of that, I grew up with an anti-union attitude because the union didn't accept black people and the black people were there as strike breakers. But then, there was a conflict.

W: Did that come to shift for you eventually?

B: Not completely, [chuckles] I saw something similar in Washington, D.C. When I first went to Howard, there were no black bus drivers in Washington, D.C. And the management said that they couldn't hire blacks because the union wouldn't accept them. And the union said, "We can't accept blacks because the management won't hire them." Then, during the war, there was a strike, and the management hired blacks to replace the white strike breakers. That's how blacks got started as bus drivers in Washington, D.C. So that sort of confirmed an attitude that I'd already had. That unions are sometimes good and sometimes not good.

W: Understood, within the context of the history.

B: Yeah.

W: When you describe Southtown being a sprinkling of black people within a larger white community, was there any kind of memory of that within the community? Of the strike and breaking strike? Was there any kind of friction around that history?

B: There was no friction between black people and white people. There was not a lot of interaction; but there was no friction.

W: Well, is there anything else you want to say about Centralia today?

B: I think, sometimes, when you ask me questions, things pop into my mind.

W: I think the reason I asked you that question is because you said with a great deal of strength that the railroad had been very important for your community when you were growing up, that it was just very central, no pun intended. So that's why I return to it. Thank you for sharing that.

B: Always a pleasure to talk about Centralia. [laughter]

W: You've been interviewed several times and I was wondering I know Al Bowker told me he'd asked you several times to do a biography for the Sloan Foundation. And I was wondering, why did you consent to do this interview?

B: Because Russ Ellis kept bugging me.

W: Yeah?

B: Yes! [laughing]

W: And I understand that Russ would say, "Please do this for us."

B: I don't remember what he said. I remember more that he kept saying it.

W: So you agreed?

B: Yeah.

W: And I wanted to ask you, what has this experience been like for you? Being interviewed by me, but just the process of thinking about your life.

B: Why, it's been completely pleasant. The only thing that concerns me is, it's sort of irrelevant. I'm still not clear why we've done this, [laughs] but it's been fun.

W: I knew I could count on you for an honest response, thank you.

B: Well, you're welcome, [laughs]

W: This is kind of a big question. When you think about the contributions that you've made that you want to share with others, what would you say are your contributions?

B: [pause] I don't know that I've made any contributions. I'm very serious about that. I did things that were interesting at the time, not just to me, but to some other people. And we enjoyed doing it. But, if you look at what's being done today, you can ask, "How many of the papers that are being written today cite anything that was written thirty years ago or even name people who did it thirty years ago?" Very few papers get cited, but the people who get named are the people who have made real contributions, lasting contributions, and there are very few of them. My papers don't get cited anymore. Well, a couple of them do. But mostly not. Mostly you're working just for the benefit of your contemporaries and people who come just after you. It sort of builds up.

And I worked in a certain area of statistics that you might call statistical decision theory. Now I think people understand just about everything there is to be understood about that, and that was the case thirty years ago so people just don't work in that area anymore.

W: You really have talked about your contributions within a context of math and statistics. And I'm also thinking about you as a teacher, and a husband, and a father, and a person.

B: Well, thanks, but I'm not going to touch that one. [laughs] I just don't have anything .

W: Okay, I want to ask you about your grandchildren.

B: Well, I have about ten or twelve. I don't see them very much. They just pop in from time to time, they're scattered around. One of them, for example, my granddaughter Christa, who lives in Sacramento, just came by yesterday afternoon. She happened to be in Berkeley on her way from someplace to someplace else and I get along well with them, but I don't see them very often.

W: Do they live in the Bay Area?

B: Yeah. Let's see. Tommy lives in Sacramento. Wabby [Walter Johnson III] lives in Florida. Stacy lives in Oakland. Christa lives in Sacramento. Lisa lives in Oakland. Rocky lives in Washington, D.C. Nate lives with us. Sean lives in Oakland. Kasheen and Jules and Richy live in Oakland. And Braden and Jordan live in Houston, Texas. I have named them all. So they're more or less around.

W: The picture of your granddaughter who was in the Montclarion.

B: Oh yes, oh yes, that was Richy, yes, oh, she's a real cutie.

W: Yes, she is. Yes.

B: I recommend having grandchildren.

W: Okay.

B: Are you married, by the way?

W: No, I'm not.

B: I recommend that, too.

W: Well, you know, hopefully that will happen.

B: [laughs]

W: So what do your grandchildren know of you?

B: Oh, they don't know anything specific. But they think, "Oh, he's done something very important." But, they don't know what it is. [laughs]

W: Okay. Well, I think there's a couple questions I'm forgetting that are pretty huge. But I feel it may be important, it may be okay for us to close for now. The questions I wanted to ask were actually about JFK [John F. Kennedy] and Martin Luther King, big questions. And they're totally achronological since we've left that time.

B: You can ask them, since this is our last meeting. Go ahead and ask them, but I don't think I'm going to have much to say.

W: Well, I guess with JFK my basic question is, do you remember when he was assassinated?

B: Yes, let's see. No, I actually don't have a clear memory of that. I have a clear memory of when Martin Luther King was assassinated, though.

W: Will you share that with me, please?

B: Well, simply I was at a meeting at the Claremont Hotel and a man announced that Martin Luther King had been shot, and it wasn't clear whether he was alive, and that the meeting was terminated. They didn't quite know what to do, but it was clear that this was a big event and that people's minds would not be on the meeting after that. That's about all I remember about that.

W: Had you been watching the Civil Rights Movement unfold?

B: Somewhat, certainly. I had a tremendous admiration for Martin Luther King. I admired nonviolence a lot. And I felt, and still feel, that Martin Luther King and Gandhi had the right idea. And I would like to see more nonviolence today.

But I think that's partly just because I'm anti-war and have always been anti-war. For instance, one man that I admire a lot is Muhammad Ali. He refused to fight and thereby gave up a lot. He gave up his championship and he gave up the admiration of a lot of American people. But he said something like, "I don't have nothing against them [Viet] Cong." Although he was the heavyweight boxing champion of the world, he was a man of peace. And as I say, it's easy to give up war if it doesn't cost you anything, but it cost him a hell of a lot. But he did it.

W: That's very true. I think so few of our athletes today are using the power that they actually hold.

B: That's right. But you don't expect them to. That's why I admire Muhammad Ali a lot. He did something that you don't expect them to do.

W: I read that there was also this time when Muhammad Ali hooked up with Malcolm X.

B: I don't know much about Malcolm X.

W: There's been two films out about Muhammad Ali, and one is this documentary which is incredible, with incredible footage of when he went to Africa. And the other one is the one that Will Smith is in, which is a main very much a blockbuster from Hollywood.

How were you connected with civil rights movements, were you involved with civil rights movements?

B: I don't think so. I'm sure that I've taken actions but I don't remember what they were, nothing big.

W: Well, I think we've done okay, I think we've done pretty good.

B: Okay. Good.

[end of interview]

INTERVIEW 8

SEPTEMBER 11, 2002

W: So, I had a couple more questions about university history.

B: Sure.

W: I wanted to ask you if people kind of approached you when, in the 1970s and eighties when they were forming affirmative action policies, active outreach and recruitment and admission of persons of color did people approach you to get your thinking on that?

B: I don't think so. My memory is not very good, but I don't remember anything on that.

W: When you think about what needs to be done to recruit and retain faculty of color specifically in the hard sciences and mathematics what do you think could be done?

B: [pause] I think the important thing for black people is to see that the opportunities are there. I'm thinking specifically about mathematics. Black people tend to think that the only thing mathematicians can do is teach, and they are largely right, but not completely right about that. Teaching has a low priority among college black people. So, many of them, who could have been very good in mathematics, have gone elsewhere.

W: Yes.

B: I don't have any deep thoughts on that. To me, the important thing is that each person is free to go in his own direction. When I was coming along, black people were not free to go in their own direction. For instance, mathematics teaching was limited to black colleges, pretty much. That has changed now. [pause] I haven't really thought a lot about it. I've been more concerned with just doing my own small bit. For instance, many years ago, I used to have summer programs where I brought black students from Southern colleges here for a summer. And that made a small difference.

W: Very interesting. When you say that, when you were coming along, "black people were not free to go their own direction." When you say that, what do you feel was a major determinant for your being able to go your own direction?

B: [pause] I went in the one direction that was clearly open to me and I was lucky in that it was the right direction for me, namely college mathematics teaching.

W: Over the years that you've been here at Berkeley, kind of watching things unfold, did you have the opportunity to watch the different debates, and policies that came out of affirmative action here, and the work that went into making that happen?

B: No. I did participate briefly in the development of the Black Studies program here.

W: We talked about that.

B: Except for that, [pause] I can't think of any other involvement that I've had.

W: Well, mostly I'm not asking so much about involvement as I'm asking for your opinion of how well it's gone, in terms of and when I say affirmative action, that speaks to a specific policy, but really what I'm speaking to is this university's effort to bring both faculty and students of color into its within its environs, this institution.

B: Nadine, I just have big impressions and I don't really know enough about it. For instance, my big impression is that the College of Engineering has been much more active than the other colleges on the campus in the attempt to recruit black students. And I think they have had some success. As I say, that's just my impression. I haven't been close to it.

W: Were you in Berkeley when Proposition 209 was passed in 1996?

B: Remind me what Proposition 209 said.

W: Okay. Basically, it kind of reversed affirmative action and brought the university to a place where it practiced color-blind admissions, which on one level sounded good to people but the way that it translated, it was basically prohibited to actively recruit students of color from disadvantaged backgrounds, and it really has impacted on campus the numbers of black students, which of course have diminished hugely, and Latino students, where I think Asian and white students are kind of on the rise.

B: Well, all I can say is I personally think that race should be taken into account. You need to look at the whole student, and his race is an important part of him. Probably, the main argument is over how much it should be taken into account not whether it should be taken into account. Of course, what I really regret is that there have to be exclusions at all. I would like to see enough universities so that every student who wanted to go could go, and you wouldn't have to reject students. It's that way now in grade school, and high school, and in junior college, and I would like to see it that way at the University of California.

W: It's interesting that you say that because that's actually part of the argument that Troy Duster makes that the issue of race in admissions has only become an issue as scarcity has become an issue. And that's when all these different kinds of rhetoric are mobilized around race, is when that becomes because there was plenty room for everyone who applied.

B: Well, no, at the University of California, admission has been restricted the upper 12.5 percent for many years I think ever since I've been here. If you go back to the 1930s, I expect all high school graduates were admitted to the University of California. And I'm sure that all high school graduates were admitted to the University of Illinois. When I went to Illinois, anyone who had a diploma from an Illinois high school was admitted. That would be expensive now, but I would like to see it. I think it would be worth it. Yeah, Troy is certainly right that it's scarcity that makes race a factor.

W: I wanted to ask you about when you came from Howard to UC Berkeley. How was it different from being at primarily a teaching institution to a research institution?

B: It narrowed my view. At Howard, I taught whatever mathematics courses were offered. That meant that I taught courses in algebra, courses in topology, courses in analysis, courses in probability. And so I learned a little about a lot of different areas. Here, I was restricted to statistics and probability. So, I taught much more deeply in those areas, but never went outside them. So, it was a different focus. And I wouldn't say one is better than the other. I enjoyed both.

W: I understand. Did it mean that you were able to kind of set your priorities in different ways, in terms of how you spent your time?

B: Yes, perhaps so. I expect that at Howard there was more emphasis on teaching and less on research. And here, it was probably reversed.

One thing that was very different was that here there was some emphasis on getting research support from the federal government. At Howard, when I was there, there was practically no emphasis on that. Of course, things were changing, and probably if I'd have stayed at Howard, I would have noticed that same shift there.

W: You said that when you were at Howard, right before you left, you had one small contract.

B: Yes.

W: What was that for?

B: I don't know, just researching probability, I guess. Yeah, I'm sure that's what it was for.

W: One other kind of larger question I wanted to ask you is, near the end of our last conversation, you were talking about being anti-war?

B: Mm-hmm.

W: What does that mean to you? And where did that come from for you?

B: Well, probably where it comes from is my early childhood experience. In my uncle's living room, there was a picture of World War I soldiers fighting, jamming bayonets into each other, Germans and Americans, blood here and there. It was a very vivid, strong picture of what hand-to-hand combat is like. And my uncle and my father had both participated in the army, and they both didn't like it, tried to get out of it, they were just anti-war.

W: During World War I?

B: During World War I, yeah. So, I just grew up with the idea that war was dumb. And it just never changed.

W: Can you tell me a little bit more about your father and your uncle's participation in the military, and what that meant for you?

B: Well, I wasn't born during World War I, but my father and my uncle told me a bit about it.

My father managed to get dismissed by pretending to be sick. I may have told you about that. Well, there were a lot of people in his group who tried to get out by pretending to be sick. But the others, when time came for them to get leave and go home for a visit would get well. That's when my father would get sick. So, he got sick and had to stay on the base when he could have been at home. So that apparently helped to persuade them that maybe he really was sick. But, in fact, he was just trying to get out of the army.

It's just the crude idea that killing people is wrong and getting killed yourself is even wronger, and there are better ways to settle things. I know that sounds very simple, but...

W: And today, it seems our country is on the verge of going to war with Iraq.

B: Yeah, I know. And you notice that the people who are pushing it are the people who haven't been to war very much. Colin Powell, who has the most military experience of all of them, is very cautious about it. Earlier, Eisenhower was very reluctant to involve the United States in war, because he had had experience. He knew what it was like to order men to go their deaths. To me, what's going on now is just crazy.

W: For me, when September 11 happened last year, the thing that it changed in my life is that I had felt very kind of alienated from making change in this society, and it reminded me of my responsibility to be present and to participate in things that I thought were important. So that's what changed in my life last year. But this whole thing, the way that we're moving again, toward war with Iraq, it brings back for me again that feeling of powerlessness, and feeling like I'm not really able to impact any of the major decisions that occur here in this country.

B: Not much, but you can affect it by voting and by talking about it. I'm surprised and pleased at how much open anti-war sentiment there is in this country. Many people are saying, "Go slowly. Be cautious. Aren't there other ways? If Saddam is such a menace, why are we the only ones that are talking about doing something about it?" How'd we get on this subject, anyways?

W: Anti-war.

B: Okay, [laughing]

W: And that uncle, was that on your mother's side or your father's side?

B: It was on my mother's side.

W: That was your mother's brother?

B: My mother's brother.

W: Okay. Two questions. And I wanted to ask you this broad question. How did having children change the way that you engaged your career? Or your mathematical activity?

B: [pause] Why, it simply getting married and having children stabilized my life. It's sort of hard to say. It all happened at about the same time. I got married, I went to Howard, and that's sort of when my real professional career started. Up until then, I'd been one year here and one year there, just having fun.

W: Did thinking about your children's future and providing for your family impact the way you made choices?

B: [pause] It may have influenced my decision to move from Howard to Berkeley. I may have thought that the educational opportunities for my children would be better here, than in Washington. My wife and I discussed that some. And, we thought that probably that would be true.

W: I don't have children of my own, but I imagine that having children would kind of make you start dreaming about what you want them to have, as human beings.

B: I think that what I wanted was for them to get a good education and be free to make their own choices. But I didn't really think a lot about such things. I think my wife thought about it more than I did.

W: What did she think?

B: Well, just that the choice of schools was very important, [pause]

W: Well, let's be done for today.

B: Okay.

W: Is there anything else you want to speak to?

B: No, can't think of anything,
[end of interview]

NADINE ERIKA WILMOT

Nadine Wilmot is the project coordinator and lead interviewer on the African American Faculty and Staff Oral History Project. Prior to coming to ROHO, she was lead interviewer and coordinator of the Oakland Oral History project about the changing racial and ethnic composition of Oakland during and after World War II. She has a master's degree in City and Regional Planning from UC Berkeley (1998) and a BA from Wesleyan University in African American Studies.