

A Conversation with I. Richard Savage

Allan R. Sampson with Bruce Spencer in attendance

I. Richard Savage was born in Detroit on October 26, 1925. He attended Northern High School in Detroit and received a bachelor's degree in mathematics at the University of Chicago when he was nineteen. Subsequently, he received a master's degree in mathematics from the University of Michigan (1945) and a Ph.D. in mathematical statistics from Columbia in 1953. While at Columbia, he met JoAnn Osherow and they were married in 1950. They have two daughters, Martha, born in 1951, and Donna, born in 1953.

His career began with a three-year (1951–1954) stint as a mathematical statistician in the National Bureau of Standards, followed by a three-year visiting appointment in the Department of Statistics at Stanford University. From 1957 to 1963, Savage was a professor of Statistics, Biostatistics and Economics at the University of Minnesota. He then spent the next eleven years on the faculty of Florida State University in the Department of Statistics. From 1974 until his retirement in 1990, he was a professor in the Department of Statistics at Yale University.

His research interests include rank order statistics, statistics and public policy and Bayesian statistics. He served as Coeditor of the *Journal of the American Statistical Association* (1968), as Editor of the *Annals of Statistics* (1974–1977), and as President of the American Statistical Association (1984). Because of his commitment to advance proper use of statistics to shape public policy, he has been heavily involved with the Committee on National Statistics of the National Research Council. The book *Statistics and Public Policy*, edited by Bruce Spencer, was published in 1997 in honor of Savage's contributions to statistics.

This conversation took place July 14 and 15, 1995, at the home of Richard and JoAnn Savage in New Haven, Connecticut. Because of its length, some editing has been required.

Sampson: Richard, in preparing for this conversation Bruce and I have independently charted your life in different ways. We may know more about you than you know about yourself!

Savage: Ah, I doubt that, but maybe in a different way. I've just finished 800 pages of autobiographical essays, so I've done a little bit of thinking about my life also.

Allan R. Sampson is Chair, Department of Statistics, University of Pittsburgh, 512 Thackeray Hall, Pittsburgh, Pennsylvania 15260. Bruce Spencer is Chair, Department of Statistics, Northwestern University, Evanston, Illinois 60208-4070.

Sampson: Any intentions to publish them?

Savage: Not really. They're the family record. Although they do include professional material.

Sampson: Tell us some of your early family history.

Savage: The records are very meager. My paternal grandfather died in 1900. I know little about him. My father's mother had two husbands in the course of her life, and it turned out that her first husband was related to my mother. My maternal grandfather devoted his resources to bringing his family from Eastern Europe to America.

Sampson: Your brother, Jimmie, was born in 1917, and your sisters Joan in 1921 and Barbara in 1922. That makes you the youngest?

Savage: Right. Actually, Jimmie wasn't [legally] born in 1917. My mother was very ill when he was born, so they didn't get around to naming him. In

1920, my father changed his name from Ogushevitz to Savage, and it turned out under the Michigan law that his minor children's names were not changed. So in the Second World War, when Jimmie was doing classified work, he had to go before the court to petition for his name. Thus Baby Ogushevitz became Leonard Jimmie Savage. I wasn't born at the time my father changed his name, so mine was automatically Savage.

EARLY EDUCATION

Spencer: Where in Detroit did you attend high school? Central High?

Savage: No. My brother went to Central High. My older sister went to Central High. Barbara and I went to Northern High School. At that time, Central High was a very academic-oriented public high school with mostly Jewish students. Both Jimmie and Joan had rather unpleasant experiences there. Jimmie, in particular, couldn't get into any college at the time that he graduated.

Barbara and I went to Northern High School which was, in theory, in a very affluent part of Detroit, but a tremendous transition had occurred in the five or six years before we went there, so it was now on the border of the Puerto Rican community, black community, and somewhat of a white community. The students were mixed almost in equal proportions. It had a faculty left over from the twenties, so the faculty were the best in Detroit in terms of their training. It had a very rich program. You could study anything from classic Greek to anything else you wanted to study. I didn't take advantage of the classic Greek, I assure you.

Sampson: If you could find some of your teachers now, would they say, "I remember Richard Savage. That guy was a star"?

Savage: No. I was never really a very good student, particularly in elementary school. When I got to intermediate school, for some reason Jimmie sent me off in a peculiar direction, into a noncollege preparatory program.

Sampson: Why would he have given you that advice, particularly as he was then finishing at the University of Michigan?

Savage: He may have come to the conclusion that some practical things were more useful than some theoretical things. So I took interesting courses like shop, business, electrical wiring and mechanical drawing. Then I went to high school, and in high school I shook off this particular piece of advice of Jimmie's. Instead, I did something else that was peculiar. I decided that I wouldn't loaf through high school, so I compressed the three-year program into two years. And did a slipshod



FIG. 1. *Richard Savage in his youth.*

program. I don't think any of the teachers would particularly remember me.

Spencer: Why didn't your parents intercede, when Jimmie gave you this advice in junior high school?

Savage: Other than Jimmie, I doubt the family was interested in this decision. Jimmie was my guide in science and education. I think Jimmie's advice probably did more good than harm. But maybe not in the way that Jimmie was thinking about; I don't know.

Sampson: What made you think it did you more good?

Savage: I think it is useful to know how to make out a checkbook by the time you are thirteen or fourteen. I never became a proficient typist, but I was better than a one-finger typist. After age twenty, a lot of the normal experiences that people have with mechanical devices and so on, I didn't have. But the experiences I had before age twenty, I remembered very vividly. So that I still have a very good mechanical sense. I know how things are done and should be done. So it turned out well.

Sampson: I was reading what Wallis (1981) wrote in the Jimmie Savage volume concerning your upbringing. There was something about your dad having a fear of kidnapping at that time, and building a house with a big wall around it. Can you say anything about that?

Savage: I was extremely young. I'm sure he bought the house rather than built it. It was a fortress. It's very interesting where that house actually is. It's on a street called Virginia Park. Virginia Park and Woodward Avenue were very close to the center of the nation's worst race riots that occurred in Detroit in the 1940s. There's a whole book by John Hershey [*The Algiers Motel Incident*] on the subject of that little area. I barely remember the house; we moved out of there when I was about five. My father had had some business dealings that may have made him apprehensive. When he was a very young man, he was a bail bondsman. My father decided that being a bail bondsman was not the best job for a nice young Jewish boy.

Sampson: Growing up, did you have any thoughts about going into business like your father?

Savage: I followed Jimmie. We didn't want to do it. I did obtain a real estate salesman's license in Michigan. I had an extremely high score, but knew absolutely nothing about selling real estate. The University of Chicago had taught me how to pass an examination of any kind.

COLLEGE

Spencer: You mentioned that Jimmie couldn't get into college right out of high school.

Savage: Right.

Spencer: Why exactly was that?

Savage: Jimmie was truly a polymath from a very young age. He was a brilliant child, but he paid no attention to what was going on in school because he couldn't see what was going on in school. The teachers thought he was more or less feeble-minded. So that when it came time to go to college, there was no way. He finally got admitted to Wayne University (before it was called Wayne State), and then he got transferred to Michigan. He had decided to become a chemist, and that didn't work out too well because he didn't see what he was doing in the laboratory. There was a math professor at Michigan who had a big influence on both Jimmie and myself, a man named G. Y. Rainich. He rescued Jimmie and brought him into the Math Department and got him going in the right direction.

I found Rainich a very kindly man. I took a seminar on geometry with him when I was at Michigan. At the first meeting of the seminar there were four

or five students. Rainich looked at the bunch of us and said that he would teach this geometry seminar in total darkness, "just like in Russia." He said that they had always studied geometry the last thing in the day, in darkness, because they couldn't afford a candle, and thus it had "to be purely mental." And his reasoning for doing it with this group was—there was myself, who was moderately visually disabled, another graduate student who was quite a bit more visually disabled than I was, another student who was totally blind, and the only other student I can remember was a priest who I think had normal vision, but I'm not sure. So we had a blind seminar, and Rainich was very good. The three visually disabled students all went on to receive doctorates.

Sampson: How much of a disadvantage was your eyesight when you were going through public schools or college?

Savage: I think eyesight was both a disadvantage and an advantage. I never met a blackboard that I could see, and, you know, I don't make eye contact with people. A lot of things are missing in my view of a scene. On the other hand, extreme myopes, like Jimmie and I, could read forever with great comfort. And so, both Jimmie and I did a tremendous amount of reading.

Sampson: How did you manage when the teachers wrote on the black board, sit in the front row, or copy notes from your fellow students?

Savage: In college, I'd certainly copy notes. The best strategy is to be prepared before you go to class.

Sampson: What were your undergraduate years like at the University of Chicago?

Savage: The University of Chicago, in the war years, was a pretty empty place, and I kept my nose to the grindstone as a result of two events early in my career. My first day at the University, I went to play some chess, and I discovered that the big boys played chess at a different level than I did. So I never played a serious game of chess again for the rest of my life. Also I went to try out for the University of Chicago's swimming team, and I discovered that the big boys swam much faster than the high school boys. So I stopped competitive swimming. And then I concentrated on my mathematical education—pretty much to the exclusion of everything else. I spent two years at the University and the curriculum was rather thin. I only had one lecture in calculus in my career there. The professor told us the definition of derivative, then the rest of the quarter we solved problems. When it came time to take integral calculus, it wasn't being offered in the summertime. So I took it by mail order, which was one of the businesses of the University of Chicago at the time. The only instructor

I had for extended periods was A. Adrian Albert, who bored me to death. I must have taken five or seven courses from him, and they were all the same subject, mainly matrix theory. I made the same mistakes in each course and got nothing out of it.

Sampson: You knew at that point that you wanted to go to Michigan, again for mathematics?

Savage: Right. It just seemed right to move on. I tried to get a job before I left the University of Chicago, probably with the Manhattan Project, I'm not sure. But, when the interviewer asked me about contour integration, I said I would have to go home and study it. I realized that I was out of my league, again, and so I went on to graduate school. I spent a year at Michigan. Rather a happy year. A lot of people whom I met there, I saw again later in life. Paul Erdős was notably present and influential on all the younger people there. I took oddball courses. I started comparative anatomy and gave that up. I spent a lot of time in electricity and magnetism. And I wasn't getting a very deep mathematical education. The one thing I learned was the real variables course, which is now what we call advanced calculus. But I skipped the modern algebra because I couldn't read German, and van der Waerden [*Moderne Algebra with Artin and Noether*] was the only available text. And somehow I didn't get around to taking topology.

Sampson: But you decided Michigan was not the place to stay?

Savage: Well, Jimmie was already established in New York and had become very enthusiastic about statistics. He was employed at the Statistical Research Group. So, Columbia was my choice.

Sampson: There's a distance of five or seven hundred miles between Detroit and New York, and your brother is finding statistics. And here you are still fooling around in mathematics and anatomy and electricity and magnetism. How did he influence you at that distance to become interested in statistics?

Savage: It must have been very subtle and very minimal. I don't think we were doing much letter-writing at that time, probably not much use of the telephone. But these influences somehow get transmitted. Jimmie might have raved about Wald as being a great man. I don't know.

POLIO

Sampson: Again, when did you contract polio?

Savage: 1945. After we dropped the bomb at Hiroshima, gas rationing was removed, and my mother and I drove to Toronto to go antiquing. There were no hotel rooms in Toronto and we

stayed at a tourist home on one of the islands in the Toronto Bay, where I presume I picked up the polio bug, and then took it with me to New York. And when I got to New York—I came by train—I felt rather poor. I went to see a doctor. I don't know how I got to this particular doctor, but he was near the apartment, Jimmie's apartment, where I was going to stay. He assured me I had the flu and sent me home, and told me to take a few pills. Jimmie and his family were away from New York. The apartment had no telephone. And basically, when I woke up in the morning, I had polio. The problem was, how do I attract anybody's attention? Because I was totally paralyzed. And I sort of lay in this bed all day calling for help. Finally a man named Maxwell heard me. Maxwell's problem was this. He was about 6'6"—I never met him. He'd been in a terrible automobile accident, and he was learning how to walk on crutches, but he wasn't very good at it. So, he would slide along the wall, with his ear on the wall. And he heard my moaning and crying for help, and he called Allen Wallis. The reason why he called Allen Wallis was that the offices for the Statistical Research Group were in the next building and he somehow knew that Jimmie was employed there, and so Allen was called. Then I saw the first real stroke of Allen's genius. He calls Columbian-Presbyterian-Harkness and says, "Send an ambulance. Hurry. One of your medical students is dying." Well, you get very good treatment when you're a medical student. And, Allen's theory was that if I'd gone to the Contagious Hospital, they'd have killed me. So that was my first real introduction to Allen. I should add that the same day that I saw the doctor, after arriving in New York, I also saw Harold Hotelling, and talked about my program for the fall.

Sampson: When did you go to Warm Springs [Georgia]?

Savage: A year later, fifteen months later.

Sampson: How much time were you in the hospital for?

Savage: I was in Columbia-Presbyterian-Harkness until just before Christmas. Then we came home to Detroit, and dammit if I didn't end up in a contagious hospital there.

Sampson: Going to Detroit?

Savage: My father had a very close friend who was a pediatrician. He understood very well what medical problems were, but he wasn't a specialist in polio. He hired as the person to take care of me the chief of Detroit's Hospital for Contagious Diseases. Well—[there are] very few villains in my life—[but] the chief is very low on my list of esteemed people. He insisted that I go to his hospital. When one

of my—I have two men that I call sort of brothers and I'll have to explain them in a minute—when one of my brothers came back from the German theater occupation—he'd been to Buchenwald and other places—he took one look at me in the hospital and told my mother, “They're killing him; it's like Buchenwald.” So we insisted that I get out of the hospital and go home. I have two brothers that I haven't mentioned who are not really my brothers. In the mid-1930's, two young Jewish refugees came, more or less, to live with us. The younger of the two, Henry, actually did live with us for a year or so. The older one, Arnold, worked for my father for many, many years. I don't think he ever really lived with us, but he ate at our house very often and we saw a lot of him. Arnold was very supportive of me after polio, after I came back from New York and after I came back from Warm Springs. He died seven or eight years ago. Henry is still very close to all of us.

Sampson: Tell us about Warm Springs.

Savage: I spent about eight months there. Warm Springs was an essential part of my education. It was the first time where I was involved with nonacademic adult living. Warm Springs had a very peculiar sociological selection of people. The people who were close to me at the hospital were physicians, priests and people from the foreign service. So it was a pretty sophisticated crowd. I did my first teaching there (tutoring in high school mathematics to postoperative patients), and I probably met the best teacher of my life there.

Sampson: Who was that?

Savage: Her name was Miss Collins. She was the teacher of walking, and she taught people how to walk who had absolutely the most minimal musculature to accomplish walking. Everyone, no matter how poorly they were endowed with muscles, were on their feet. And practically everyone learned to do some walking. She was quite astounding. It was good to meet her.

Sampson: Did you study any mathematics at that time, or read any statistics?

Savage: Well, up to this point, technically I hadn't studied any statistics at all, but Jimmie must have provided me with a copy of Cramér (1946) and I carried Cramér around with me all the time I was at Warm Springs. I had two things on my lap most of the time, Cramér and my knitting.

Sampson: The knitting was for hand therapy?

Savage: Right. I did much better with the knitting than I did with Cramér.

Sampson: Maybe you picked the wrong career, Richard.

[Laughter]

Savage: Maybe. I just couldn't believe that a simple idea like an additive family of sets was really important. So Cramér just couldn't get into my head. When I got back to Detroit, after Warm Springs, I had a very important educational experience, again purely by accident.

Sampson: Now, this would have been the summer of 1947?

Savage: Right. Well actually the academic year of 1947–1948. I met a Professor Max Coral at Wayne University. He was a professor of mathematics. He did everything for me that a good statistics department could do for an individual. I took a couple courses from him. (They were very traditional.) He got me a teaching job. I taught solid synthetic geometry. He got me involved in a little bit of consulting. And he socialized a little bit with me. So it was very good and a little astounding that out of his beginning class not only did I go ahead and continue graduate work in statistics, but there was a woman, Mrs. Moy, who also went ahead and got a doctorate degree. So, he gets two doctoral students out of a very beginning mathematics course. He did something right.

COLUMBIA

Sampson: You were finally able to return to Columbia in 1948. What was Columbia like at the beginning of your graduate study?

Savage: There were many students around, many veterans. We began in the summer of 1948, with Jack Wolfowitz teaching, about ten hours a week. We went through a good part of Uspensky [*Introduction to Mathematical Probability*] and a good part of Cramér in the first summer. It was very intense. Fortunately I had had this little bit of preparation from Wayne University. I think everybody had a little background, and we started to work like dogs. My Columbia career was checkered in the sense that I avoided all of the really difficult courses, such as Ted Anderson's multivariate analysis course and the serious courses in decision theory. Howard Levine, who was then a rather junior faculty member, was my mentor. Howard got his degree at Columbia, and he is part geneticist and part statistician. His career is the glue that held the Columbia Department together for forty-five years. He's one of the few people who have been chairmen of departments while an emeritus professor. He has been the faithful supporter of Columbia statistics, both in terms of tenure and in terms of his energy. As a teacher he had good ideas, like not making it too complicated. So, when you got done with a course in genetics or whatever with

Howard, you understood what he tried to teach. He didn't try to teach too much. And I took both genetics and elementary nonparametric statistics from him.

Sampson: Did you write your dissertation with Howard?

Savage: Right. Well, it was my impression that at that time at Columbia, dissertations were not really supervised. It was sort of the standard that people left Columbia without a dissertation and they did them in the field. The one big exception while I was there was Stanley Isaacson, who wrote his dissertation with Henry Scheffé. In rapid time, he left the University with his dissertation and we were all quite impressed by that. But people like myself and Bill Kruskal left with little or no work on their dissertation.

Over the years, I met the three Kruskal brothers. The middle brother, Martin, who is an applied mathematician at Princeton, and I were undergraduates at Chicago. Bill and I met as soon as I got to Columbia and we became lifelong friends. I met Joey in Washington. He was a student of Jimmie's. A Kruskal nephew was a student in the Yale Statistics Department. So the Kruskal's and the Savage's have been friends and colleagues of each other for more than fifty years now.

NATIONAL BUREAU OF STANDARDS

Sampson: Did you go to the National Bureau of Standards with a dissertation topic in hand?

Savage: Not even that. I'd started to think about a dissertation while I was at Columbia. Somehow I would do something on the Kolmogorov–Smirnov statistic. But by the time I got done thinking about it, I realized it would have to be more mathematical than I wanted. So I got to the Bureau with no topic. I became very busy at the Bureau in the sense that I did a lot of work there. In the Bureau, you're supposed to do a lot of consulting, and I didn't do very much of that. Just enough so I could say that I hadn't cheated.

I did one piece of consulting with Marvin Zelen. We studied the variation between rolls of film used in x-ray diffraction analysis of the structure of portland cement. And it turned out that the between-roll variations were very large and made the analysis very difficult. My main consulting work at the Bureau was with defense projects, and ultimately those were the things that made me leave the Bureau.

Sampson: Why is that?

Savage: Well, it's just that defense work always has a bad taste to it. The thing that really turned

me off the most was one day when I was at Fort Deterick, the biological testing ground. They were telling me how they had set their computer up so they could put in the data from an experiment and do the analysis of variance and never look at what had been done. And, I said to myself, "This is a total disaster. We'll never know what data are being collected from now on." And that turned me off completely.

Computing was just emerging as a powerful tool. Part of that defense work, I did with Alan Hoffman on the largest computer available. He was the programmer, and I was observing the data as it came out. And that's the only time in my life where I had to sit up all night with the computer.

Dan Teichroew was the authority on statistical computing at the Bureau, and he did some wonderful computations of the probabilities of rank orders. He actually did it for a different purpose, relating to the work of Bob Bechhofer. But it was quite astounding that he could do these computations as accurately as he did. A few years later, Roy Milton at Minnesota did these computations in a much more extended form and showed the power of modern computing for doing statistical tables, I thought. I had a student from Antioch who was on a work-study program at the bureau. I was going out of town for a few days, and I suggested to him that he compute some tables of the expected value of $1/x$ for Poisson distributions with the zero class missing. And the same for the binomial distribution. He did these computations and when I got back, I asked Lolla Deming, who was our woman of all chores, to check the work. She came back and told me, "Well, there are a terribly large number of errors in it." So she had gone ahead and recomputed everything that the student computed. And by the time we had finished this little homework exercise, we had a lot of time and money invested in it. So I think that might be the first published paper that I have; I'm not sure (Grab and Savage, 1954).

Sampson: Frank Proschan was one of your colleagues at that time; is that correct? (See Hollander and Marshall, 1995).

Savage: Right. Frank and I shared an office. Above our office door we put up a sign saying "See us first." Frank was a very active consultant compared to me. We didn't do very much work together. But we wrote a short paper together.

Sampson: "Starting a control chart," (1960), with Frank Proschan.

Savage: I don't think it was a very important paper. But I had one other paper published in that journal, and I'm not sure it's in my vita. It is actually a letter to the editor, and the letter was signed

“Ann Landers.” The letter was prepared by Herman Chernoff and myself. When Ben Epstein came to Palo Alto, he looked for this Ann Landers woman who had such a smart idea, and we didn’t enlighten him.

Sampson: Did you do your dissertation at the Bureau working by yourself or were there people you talked to?

Savage: All the serious work I’ve ever done, certainly by myself, tends to be out of the mainline of what other people are doing. The dissertation was written in a week or so, but it took a little while to finish it off. I think it was the week that my wife, JoAnn, was confined with the birth of Donna, our second child. I was kicked out of my office because they were doing some renovations. And I was in a relatively isolated office—I worked very hard. I had prepared for the dissertation writing, in the sense that I’d done very extensive reading in nonparametric statistics. At that point, there were very few review articles or summaries of what was going on. And I just started collecting a great big bibliography.

Nonparametric statistics at that time had nothing to do with what is now called nonparametric statistics. It was focused on rank orders and their properties. My idea was to see how much of the standard theory of statistics could be applied to the rank orders rather than to the underlying random variables. And in the end that’s what the thesis was all about.

Sampson: What was it about working with rank orders that so caught your fancy?

Savage: Well, they are very, very discrete. There’s no limit theory involved. And yet there’s a certain amount of interesting structure. One of the games I liked to play was to use new branches of mathematics on these trivial-appearing problems. Later I have a paper in the *ISI Review* (Savage, 1964) on the use of lattice theory in the analysis of rank orders. I got some interesting results there. But it was a disappointment that I could never make the lattice theory computational methods work for the computation of the number of rank orders corresponding to a particular value of a Wilcoxon statistic. The main result of that paper is that the typically well-known statistics like the Wilcoxon statistics are the natural measures of distance within the lattice, which is the cute result I mentioned. So for years I was very much obsessed with trying to know everything there was to know about nonparametric statistics and rank orders. I published the bibliography in *JASA* [Savage (1953)]. A few years later, the citation bibliography was published by Harvard Press (Savage, 1962).

Eventually, in 1974, I cut the cord by leaving the whole file with Myles Hollander. And I hope he just threw the whole thing out. I don’t know what happened.

Sampson: Myles is too compulsive to throw anything out.

Savage: Well, anyhow, at that point I really stopped and didn’t try to keep up in any way.

Sampson: When you were preparing these bibliographies, did you use computers?

Savage: No, we didn’t use computers. I had some small five-by-eight file folders printed with the outline of the bibliographic information on the outside, to be filled in on a typewriter. Then you would throw the citation slips inside the file folder. All of that was done by hand.

Sampson: I know you. I imagine you read all these papers.

Savage: Oh, yeah.

Sampson: That’s a prodigious task.

Savage: Well, at one point I called Jimmie and myself “reading machines.”

Sampson: That’s just the eye lens you are talking about. I’m thinking about the brain power that it takes.

Savage: Well, I understand, but you don’t have the distractions of playing baseball or tennis or whatever. You read.

Sampson: I know as editor, associate editor, referee you’ve handled a very large number of manuscripts. I imagine that you also read every one of those and were very thoughtful about it.

Savage: Right. A major part of my career was editorial work. I started it at the Bureau. Allen Wallis was then the editor of *JASA*. As he became editor, there was a large backlog of manuscripts that had been hanging around, and he felt they had to be cleaned up. He picked me to be the clean-up man. So he just gave me a stack of manuscripts to work on.

Sampson: That’s a tremendous talent, to be able to do that, Richard.

Savage: I think the talent was Allen’s, to figure out that he could find someone to do the work.

Sampson: That’s his talent, maybe.

Savage: There’s a little point to that story in that I was very fortunate to be part of the old boy network from a very early age. I paid for it in the sense that I did the work.

STANFORD

Sampson: How did your move to Stanford come about?

Savage: The idea to go to California came up one evening when I was driving back from Fort Deterick

with a biologist from the Smithsonian. I asked the man how he could stand working on such terrible problems and he said that when he went home he spent his time in the backyard growing tomatoes. I came home and told JoAnn we were going to go to California to grow tomatoes. Then I started to look for a way of going to California. And, again, things were easy. I wrote to Stanford, and they said "Sure, come on; we'll pay you a little bit of money." We left Washington with furniture and everything for a one-year appointment in Palo Alto.

Sampson: It actually grew to a three-year appointment.

Savage: We ended up spending three years in two houses. It was a very good time. Al Bowker and Jerry Lieberman were the directors of a project on quality control, and I think they paid a large part of my salary most of the time I was there. The big intellectual part of my Stanford stay was the paper with Herman Chernoff (Chernoff and Savage, 1958). Herman remembers the history better than I do.

Sampson: What is your recollection of it?

Savage: My recollection is that by then I was very deeply immersed in the nonparametric business, and I felt that there should be a unified asymptotic theory for rank-order tests and that the unification would come from expressing the test statistics as integrals, and then using whatever mathematical theory that was applicable to the integrals. Meyer Dwass had some contribution to the formulation. Herman did most of the hard work and the actual writing. Most of my effort was to keep very close track of what Herman was doing. It's a very long, tedious proof. There were a lot of places where things had to be modified and proved as the proof was growing. I had nothing to do with the calculus of variations argument, which is one of the more interesting parts of the paper, although in terms of its usefulness, the basic limit theorem is the central focus of the paper.

Sampson: You did some consulting at the Center for Advanced Study in the Behavioral and Social Sciences?

Savage: Right. That started in the second year, and I had an office up there and at Stanford. It was by chance that I became the statistical consultant at the Center. The chance happened because Howard Raiffa was a Fellow at the Center. He was being asked to do too much statistical consulting work, and he didn't want to do it. That wasn't what he was there for. So he helped to create a job which I got. There were many people at the Center who knew much more about specialized techniques for the social sciences than I ever knew. People knew factor analysis, scaling models, all kinds of things.



FIG. 2. *Richard Savage at the Center for Advanced Study in Behavioral and Social Sciences, 1970.*

But I was there simply as a hand-holder and as an aid to what might be going on. The only long serious research I did was the paper with Karl Deutsch (Deutsch and Savage, 1960).

Sampson: Your Bayesian outlook—how did it enter into all of this, if at all?

Savage: Not at all, really. I was thinking Bayesian, but I wasn't using Bayesian. I had a little run-in with Marvin Zelen while we were still at the Bureau. He forgets it all. But you have to remember that we were at the Bureau in the early 1950s in the days of Joe McCarthy. Marvin called me un-American because I was Bayesian, and I was undermining the United States Government. But I don't think that, operationally, Bayesian ideas became at all important to me until I got to Florida State.

MINNESOTA

Sampson: What took you to Minnesota?

Savage: I went to Minnesota at the invitation of Leo Hurwicz, who was an economist I'd met at the Center. Leo was the driving force at Minnesota to get statistics organized and going. He, of course, was the driving force in economics and elsewhere. Leo was very thinly spread. But he was effective, and he secured the position for me at Minnesota.

Sampson: Your vita shows you listed as being on the faculty in three departments: statistics, biostatistics and economics.

Savage: That sounds about right. The first time I went to my office in the School of Business, a tall guy came in and said, “What do you want to be, an economist or business administrator?” And, I said I hadn’t ever thought about the subject. He said, “Well, you’d better become an economist.” This was Walter Heller, who became the chairman of the Council of Economic Advisors. He said, “Since your appointment, the School of Business has become departmentalized, and you have to choose one department or another.”

Sampson: There was no Department of Statistics at that time at Minnesota?

Savage: Right.

Sampson: Was there a Department of Biostatistics?

Savage: Yes. I also had a joint appointment in the Department of Biostatistics in the School of Public Health.

Sampson: Who were some of the statisticians in economics and in biostatistics at that time?

Savage: The real statisticians on the campus were John Neeter, who was well established in Business, and Leo. Eventually, and maybe even at the time I came, there was John Chipman who was a statistician/economist/econometrician. And later there were many econometricians in the Economics Department. Palmer Johnson was the head of the Ed Psych Department—very well established. Also there was a young man named Ray Collier, a very good applied statistician who stayed for many years.

Over on the agricultural campus, there was Charlie Gates and a senior person, I think his name was Ralph Comstock, whom I didn’t interact with. The strong people in the social sciences had disappeared before I got there. Leon Festinger was gone by the time I got there. I met him at Stanford. And Louis Guttman had gone to Israel before I got there. In Biostatistics, there were Jacob Bearman who was the director, and Richard McHugh. Byron “Bill” Brown was, I think, an instructor, and Eugene Johnson and I were his dissertation directors. The University was large, and I cannot recall all of the faculty with a strong interest in statistics. When I came there, the math group included Monroe Donsker and others in probability theory.

Sampson: You were there when the Department of Statistics was started in Minnesota?

Savage: Yes. When I came, there was a thing called a Committee on Statistics. And near the end

of the time I was there, maybe the last two years I was there, the department was created.

Ingram Olkin was appointed during that time and was there when we did the *Index to the Annals of Mathematical Statistics* (Greenwood, Olkin and Savage, 1962). This was a major activity and very supportive of creating a statistics group because we had lots of work for lots of people. While Ingram was there, we invited Meyer Dwass and Sudesh Ghurye, and as I was leaving, Theophilos Cacoullos and Robert Buehler joined the faculty. Bernie Lindgren was there when I got there. He’s a mathematician by training, but he’s a statistics educator by choice.

Sampson: Milt Sobel must have been there?

Savage: Milton came at some point; I don’t quite know when. But he was there for a good while when I was there. He enjoyed defeating me in board games, and I was impressed with the enthusiasm of his seminar students.

Sampson: What was the driving force to get that department started at that time?

Savage: Well, I think Hurwicz was the original pusher, and then he was the only one that had influence on the administration. After I left Minnesota and was in Tallahassee a few years, Bryce Crawford, who had been a dean, happened to come by. He asked me, “Savage, why are statisticians so hard to do business with?” Which really epitomizes the problems of Minnesota and probably other large universities in getting a statistics group organized.

Sampson: Who was the first chairman at Minnesota?

Savage: That’s the reason why I left. I was on leave at Harvard when time came to elect the first chairman. (Or maybe the second chairman; Leo may have been technically the first chairman.) I thought there had been a gentlemen’s agreement that I would be the Chair, and all of a sudden I got the message that Leo would be the chair. I felt quite put out about that. Leo was very thinly spread around the University, and I thought it was an error to make the Chair a person who was not focused on the growing business of the department. I came back for a year, and then I went to Florida.

Sampson: Not an unusual scenario. What was your relationship like with Leo?

Savage: Well, up until that point, it was very cordial. We socialized with each other. He’s a very bright person. He gave me good advice, taught me a lot of things. But he was well known in the University to lead a multiple life. He had so many offices in the University just so nobody could ever find him. He could always say, “I missed this appointment because I’m in some other appointment.”

Sampson: Sounds like a good trick to be able to do.

Savage: He was an expert. My remarks aren't meant to be particularly critical. It's just that that's a way of life that I found incompatible.

Sampson: Let's come back to the index that you did with Olkin and Greenwood for the *Annals*. How did that get started?

Savage: I'm not sure of the exact history. I think there had been some talk around the National Bureau of Standards. And John Tukey was involved. Maybe John actually got the money; I'm not sure. Ingram and I met with Tukey in Princeton, and he patted us on our backs and told us to go for it. It was a horrendous project. As far as I know, no other scientific journal has ever been indexed in this way. It's a subject matter index, a citation index, it's complete and it was all done by hand on old-fashioned electric typewriters, with multitype sizes, with columns adjusted to be same length and so on. Greenwood was a fanatic for detail. Ingram and I supervised a horde of students, working with them in preparing the index words for all the abstracts, as well as all the articles. It was a very, very costly project.

Sampson: Who funded it?

Savage: The National Science Foundation.

Sampson: Richard, when you look back at that, what did you see as the benefits of doing the *Index*?

Savage: Well, we ran a whole statistics department off the project for a year or so. We supported all the graduate students, if nothing else. But I thought of it, still think of it, as an experiment in information. Now the *Current Index in Statistics* is a much more cost-effective way of doing almost what we did. But the *Current Index* doesn't have a citation index with it. So you have to use the *Science Citation Index* and the *Current Index* to accomplish what we were doing. But it's so much more efficient that it makes sense. I was never the Chair of the *Current Index* committee, but was a long-time member of it. One of the things I pushed for was to use that data base to generate other subject matter indices.

FLORIDA STATE

Sampson: How did your move to Tallahassee in 1963 come about?

Savage: Ralph Bradley asked me if I wanted a job.

Sampson: Did Ralph know you were looking to leave?

Savage: I'm not sure he was actually aware. But maybe I'd grumbled a little bit too loudly. They invited me down there in the springtime.

Sampson: Springtime in Tallahassee!

Savage: Nothing is more beautiful and attractive than springtime in Tallahassee.

Sampson: Florida State at that time was a provincial university, and Ralph was just starting the department.

Savage: He had some unusual people there. Shri Katti and Frank Wilcoxon were there.

Sampson: Statistics eventually became one of the exceptional departments at Florida State.

Savage: There was a quite clever administrator who decided, presumably with a lot of cooperation, that they would develop a few departments, some cheap, some not so cheap. So they bought a chemistry department of national stature. That is, they went out and hired a whole bunch of guys from one or two places and created a major chemistry department as a major investment. Then, as relatively minor investments, they created other departments such as Meteorology.

Sampson: I'd like to talk about Frank Wilcoxon and your interactions with him.

Savage: Well, my main intellectual interaction with Frank happened before I met him and indirectly. Let me tell you my social interactions with Frank before I tell you the intellectual. Frank was a wonderful colleague. Did you know him?

Sampson: No. He died before I got there in 1970.

Savage: He was just a wonderful, friendly, warm—a little childlike—person. On my interview trip, I had lunch with him. I was talking about how cold it was in Minnesota, and then he told me a Jack London story almost verbatim that he had read sixty years earlier. I was so overwhelmed with his recitation of this story that I fell in love with him, and that may have been the real reason why I went to Florida. While we were there we saw a lot of him. But I actually did very little, if any, work with him.

The big work that I did was around him. While I was visiting at Harvard, I met Bob Berk, who was a second-year graduate student. Berk told me that he had spent the summer working with Frank Wilcoxon at American Cynamide. They had tried to work on, or at least discuss, the problem of the termination of sequential probability ratio tests based on ranks, and they'd made no progress. When I got to Tallahassee, Ralph and some of his students, maybe coauthored with Frank, had published some very partial results of the easy case, mainly where they break the sequence of observations into small subsets and then can do the analysis without any serious problems. But I was intrigued by the problem and wanted to solve it, and I couldn't get very far.

Shri Katti came back from a trip to California, and he says to us “Jeez, I met this wonderful guy in Palo Alto, and I took it upon myself to invite him over here for a seminar.” Well, we had a seminar budget about enough to get a person from Atlanta to Tallahassee, and he invites someone from Palo Alto. So I met Jayaram Sethuraman, and I told Sethuraman what the problem was. He says, “Well I’d been thinking about that for a long time, but I don’t quite know how to put it together.” I said the way you put it together is that you use the representation that I’d used with Chernoff on the structure of the integral. “Oh,” he says, “Well, if we do that, then why don’t we finish the paper this weekend?” This is one of the most satisfying collaborations that I have ever had. It had a real kick to it. It wasn’t working for months with Herman on tedious hard analytical problems. We just, on a weekend, put the whole thing together and it was done.

So this peculiar thing that Katti did was probably one of the great things that ever happened to the Statistics Department in Tallahassee. Then a few years later Bob Berk came down. The problem Sethu and I solved only handled a special case of rank order statistics [Savage and Sethuraman (1966)]. Bob and I did one general problem in a frantic way [Berk and Savage (1968)]. We worked on it all summer. The last night before he left, he finally got the last little term to behave properly, and that was nice. So that sequential analysis problem was a big problem, and that’s what my one paper with



FIG. 3. *Richard Savage and J. Sethuraman in Tallahassee, 1970.*

Jimmie is about also (Savage and Savage, 1965), in a much more simplified version.

Sampson: In what sense?

Savage: We do easier things. We don’t try to prove the hard limit theorem. We only want to prove that things do terminate, not to actually get the rate of convergence or anything like that.

Wilcoxon was at least indirectly involved with all of this research.

WILCOXON

Sampson: I’d actually like to go back and get some more of your reactions to Frank Wilcoxon.

Savage: Let me tell you about the last time I saw Frank. We were standing around our front yard, and Frank drove up with his van, or maybe it was a station-wagon, with his kayak on top. He said to us “I had actually this wonderful afternoon out in the swamps in the kayak,” and then he added parenthetically, “but you know, I think I overdid it.” And then a few days later he died of a heart attack. So he was going full tilt until the day he died.

Sampson: Was he involved with a lot of research with people at that time in Tallahassee?

Savage: No. The students loved him. He was supportive of them. I think he may have had only one student who wrote a dissertation with him.

Sampson: But what brought Frank Wilcoxon to Tallahassee?

Savage: He had retired several times. Ralph most likely approached him. The Regents didn’t want to make the appointment; they were getting a little sick and tired of hiring retired colonels and



FIG. 4. *Frank Wilcoxon.*

other old geezers. And then somebody on the Regents looked very carefully at Frank's curriculum vitae, and in there is a little item about patents, and he's a copatent holder of a chemical called parathion, which is the primary chemical for spraying citrus crops. So as far as the Regents were concerned, they were hiring God—they weren't just hiring a statistician.

Sampson: I never knew that story.

Savage: So he was welcomed by the Regents. He worked like a dog. I suppose he was paid halftime, but he was around the Department more than anyone else. Probably his last summer he was at home in Nyack. When he came back he said he spent the summer learning to read Dutch, and here is a man in his late sixties, maybe seventies. He spent the whole summer reading Dutch. I asked "Well, why did you do that, Frank?" He said that he'd done some consulting on spotting fraudulent data, and the basic papers were in Dutch. So, he thought it was time to learn Dutch.

Sampson: If my memory serves me correctly, in the Wilcoxon Reading Room [at Florida State] there's a picture of Frank, with a long scarf and motorcycle.

Savage: I don't remember the scarf, But he certainly had a motorcycle. I think he used the motorcycle the first few years that I was there, but he must have given it up at some point.

Sampson: Do you want to talk about your relationship with Ralph Bradley? He built a tremendous department at Tallahassee, and it seems that he, in many ways, was the main stimulus of that with his energies.

Savage: I think that's true. He took advice occasionally, not always.

Sampson: I always had the sense that there were very few people there that Ralph really paid a lot of attention to, to get their opinion.

Savage: Sethuraman, myself.

Sampson: And then later, I thought perhaps Frank Proschan.

Savage: I can't vouch for that. I must add that Ralph and I always had a positive working relationship.

HÁJEK

Sampson: I met Jaroslav Hájek in Tallahassee when he visited several times. You had a strong connection with him. That's my impression.

Savage: We became very good friends.

Sampson: That was my sense.

Savage: We didn't do any work together. I can tell you a couple of stories about Hájek. At one point

we arranged a very large dinner party at our house. Fred Mosteller was going to be in town for a seminar. There was a visiting person who was interviewing for a job, a man whom I'll call the "Bad Czech." When I told Hájek about this dinner and who was going to be there, he said, "Well, the 'Bad Czech' has recently defected from Czechoslovakia." Hájek had explained earlier to us that he [Hájek] would have to return to Prague because of health reasons. He had inherited kidney disease. He felt that he was terminally ill, and he couldn't leave his wife and young daughters in the United States where they had no connections. So it was absolutely mandatory that he go back because of his health concerns. He said, "I really can't come to such a dinner party because it is surely going to be known that I've been socializing with a defector." As I was leaving the office the night of the party, he said, "Well, is the invitation still open?" So he came, embraced the man, and they sat the whole evening talking to each other. So his rational behavior was overcome by affection for a countryman.

The last time I saw Hájek was much more dramatic than that. I was in Prague and we had dinner at his house. It was very nice. They had a very modern, little, tiny apartment. Dinner was lovely. The dessert was fresh plum dumplings, rolling around in butter and covered in powdered sugar. Betty, his wife, explained that due to his health problems, Jaro was not allowed to eat fresh fruit, but this was an exception. So you know, we all had two dumplings or so. God knows how many Jaro ate. He was anything but a glutton, but this was just too tempting.

I made a subsequent luncheon engagement with him. He had one of his assistants take me from the lecture hall to the dining room in the institute where he had been teaching for many years. The idea was that after lunch he would accompany me back to the entrance of the building. He would go for dialysis, and I would meet JoAnn at the entrance. On the way to meet him, the assistant and I had a fair amount of trouble. We found the elevator and it was a very small elevator. So she had to take the foot-rests off my wheelchair, collapse the chair, take the chair up without me, come back for me, and then take me up, and it was quite a struggle for this young woman [assistant]. She got me to the dining room and there was Jaro and we had a very nice lunch. Now it was time to go back. We come to the elevator and I looked at the elevator, and said, "Jaro, it doesn't quite look like the elevator I came up on." He said "Well, there could only be one elevator." "What the hell," I said, "Well, it might be. It probably doesn't make any difference anyhow." Well, it turned out this elevator was not the one I'd come up on; it was a little bit

smaller, and we had to struggle. By this time he was feeling very terrible, because he was late for his dialysis appointment. Finally, we got down to the level that we thought was the correct level, and I said, "Which way do we walk?" He said, "Well, I don't know. I'm a little bit confused." I said, "I'm sure that's not the right elevator." He stopped a student and asked how many elevators there are. The student said, "Oh, there's just that one elevator." "Which way is the entrance?" Well, he's not too sure which way the entrance to the damn building is. The building is about the size of the Pentagon and linear. Jaro's nonplussed; he doesn't know what to do. There were only two directions to walk, and we tried this one direction. As we walked, we eventually came to the other elevator, the one I had come up on. We kept walking and walking, and finally, when he was ready to collapse, we got to the entrance. We'd often talked about Kafka and our last meeting was such a Kafkaish event that we were overwhelmed.

Sampson: For how long did he visit Tallahassee?

Savage: I'm not sure whether it was six months or a year. He had no duties. But Ralph had a student whom Ralph really couldn't finish. He gave the student to Jaro, and Jaro saw him through until the end.

Sampson: Did Hájek come there because of his desire to work with you?

Savage: I don't think so. We certainly didn't do any work together.

Sampson: I know, I understand that. That was my next question. Why do you think you guys didn't work together?

Savage: Doing collaborative work to me is like sex or something. Its got to be spontaneous.

Sampson: The chemistry has got to be there.

Any other thoughts about your Florida State years?

Savage: By the early 70s, the Department was flourishing with senior faculty, outstanding junior faculty including Hollander and yourself, and a good number of students. I must say we enjoyed the University. I also enjoyed Minnesota and Tallahassee as real universities where people did talk to each other, and where you saw people from other departments regularly. That's true at Palo Alto, too. Yale left me isolated.

YALE

Spencer: You left Florida State around 1974 to come to Yale. Why did you want to come to Yale?

Savage: I think our reasons for coming to Yale were strictly social and had nothing to do with the

intellectual climate of Yale. We wanted to be closer to our families, closer to the center of modern art, theater and so on. In hindsight, the year I visited Yale [1967–1968], was not too promising. The Statistics Department is a very small department, and I found it not very stimulating. I don't know the exact record. I don't think there were any joint papers among members of the faculty in the department before I retired in 1990.

It's my impression that Yale is extremely isolating, in that the individual departments are treated as individual kingdoms, and there are very few joint appointments of any serious consequence, relatively little interaction of the faculty. Further, it was difficult to get around on the campus with a wheelchair.

The real intellectual drive of the Statistics Department was the interaction between the faculty and the students. That's where all the energy went. And, from the education viewpoint, that might be a very happy way of running a university.

Spencer: While you were at Yale, was there a danger of the department being shut down? It's a precariously small department and later there were pressures.

Savage: We were always aware of that possibility. As a faculty, we took seriously the responsibility of doing a lot of teaching outside of the department and trying to do committee work or whatever was asked of us to make us good citizens. We were very self-conscious about that.

Sampson: When were you Chair at Yale?

Savage: I don't know... from 1976 to 1988, or something like that.

Sampson: What were your duties as Chair?

Savage: None.

Sampson: None?

Savage: There are three officers in the Yale Department. There is the Director of Undergraduate Studies (which I often was), but we didn't have any undergraduate majors. There was the Director of Graduate Studies (DGS), who is responsible for maintaining all aspects of the graduate programs. I did that for one year. I think I was not too competent, so I was made the Chairman, whose responsibilities were minimal. The DGS is the important person.

Sampson: If you look back over your chairmanship at Yale, what do you think your accomplishments were?

Savage: Well, I think there were very few accomplishments as Chair. The department did not grow, the number of graduate students did not grow. We mainly maintained our status.

Sampson: Princeton's statistics department was lost around that time.

Savage: Columbia also was threatened. Yale had great trouble in getting graduate students, but we kept turning them out—you know—rather good students, throughout the period.

Sampson: Why did you have trouble getting graduate students?

Savage: I've been impressed that at Minnesota, Florida State, and Yale (but not Stanford) we do not recruit a substantial number of students from major universities. Our American students tend to come from relatively small schools, not particularly high on the pecking order; it's much harder for me to evaluate where the foreign students really came from. When we did our screening of incoming graduate students during the entire period that I was at Yale, we had barely enough qualified applicants to fill the available fellowships.

One year we came up with the brilliant idea that we would have a statistics day at Yale and invite college seniors from the Northeast to come look us over. We had a smashing program, with several people giving miniseminars, and lunch—a real high-class presentation. We got about twenty people to come. Of the twenty, there was not a single one who was qualified to go to graduate school in a mathematical subject. It was a total bust. We only tried it for one year.

LONDON

Spencer: You visited London for six months around 1975 and split your time between Imperial College and the Office of Population Censuses and Surveys. What were your impressions of the Office of Population Censuses and Surveys?



FIG. 5. *Richard Savage, 1975.*

Savage: Well, it was a revelation to me. I enjoyed it tremendously. I don't really know the basis of comparison. But I had the feeling that, in England, government statistics was a small organization compared to the River-Rouge-plant-type census bureau activities in this country. They treated me in such a wonderful way.

I was essentially a visitor to the senior staff. Philip Redfern was the associate director or deputy director and acted as my host. I saw him almost daily. We went to lunch regularly. We talked about all the problems they had. I saw the director, I saw the leaders of the various sections, I saw their working papers. I was part of what was going on at the senior level. While at the Office of Population Censuses and Surveys, I thought I made very little contribution, and yet my recent correspondence with Redfern suggests that maybe I'd made more of a contribution than I thought. I came to London and started talking to him about undercount problems in the United States, and they assured me that in England it was one-tenth of one percent at most, but the undercount in England is now creeping up to around the two percent level and they are beginning to understand what's going on. And I think I helped to alert them to this problem. One can't be sure.

One of the things that I think is of particular interest to Americans is that in the British civil service system for statisticians, a basic tenet of the system is that as an individual gains experience, he must move from agency to agency. The people that I met in the British office, for instance, spent three years in Housing, three years in Energy and then, maybe on their third or fourth tour, settled down into their careers. By that time, they're senior people who know quite a bit about what's going on and how it takes place. I believe that sort of professional training of statisticians is missing in the United States.

Spencer: In the United States, I think it's up to the individual to request a transfer to another agency, but it doesn't seem to be part of the standard training.

Savage: I consider my stay at the British census to be a great success since I have Redfern as a friend for life as a result of that visit.

PRESIDENT OF ASA

Sampson: Let's talk about the presidency of ASA. Did you want to be president?

Savage: I was quite shocked when I was nominated, and of course you know that you don't campaign for this office. But I think I would have



FIG. 6. *Richard Savage, late 1960s.*

been very disappointed if I hadn't been elected, and I imagine the person I was opposing was disappointed. I enjoyed most of the presidency. Let me start with the bad things first. I came on in the middle of the fundraising for the new ASA building, and because I came from a family of builders (I liked my father's favorite line that the "Rabbi had an edifice complex") I thought that the then current location of the Association in a nice office building—a little decrepit, but a nice office building—a block from the White House was just "Jim Dandy." And I couldn't work up any enthusiasm for the building fund. So that was hard on me. The president of the American Statistical Association has very minimal influence on certain important aspects of the Association. I think that is still true. One of the things that really runs the organization is the Publications Committee. I don't know if all my ideas were bad, but I always found the Publication Committee stonewalling in terms of any changes. As you know, everybody who belongs to the Association used to get the *Journal*, but most of the people who belong to the Association can only read the book reviews in the *Journal* and nothing else.

Sampson: That's changed, though. They have the smorgasbord option for which journals one wants to receive.

Savage: Well, that was the kind of thing we were trying to think about more than ten years ago, and the Publications Committee members were just absolutely intransigent. The idea of maybe putting out a separate journal of book reviews was totally unacceptable. There had been a government publication called the *Statistical Reporter* that summarized what was going on in the government in terms of major statistical programs. The government wanted

to give this publication up, and the Association said they wanted to extend their influence over government statistical workers. But the Association absolutely refused to pick up this journal. Consequently, it disappeared. The Association wanted to become a popular association, yet they let some commercial publisher start *Chance* because they wouldn't touch it. Then, subsequently, they went ahead and bought the magazine.

The Budget Committee was very professional and somehow everything was a *fait accompli* by the time the president saw what was going on. I was lucky in the sense that during my presidency, and when I was past president, I think the budget was balanced. Then about a year or so, after I was really out of office, the Association experienced a financial disaster, partly because of overexpansion and partly due to losing some of their windfall profits. Fred Leone was the general secretary, and Fred and I corresponded a tremendous amount during that year on many, many programs. I don't know if any of my advice was taken, but anyhow I tried. Finally, there was the series of tours in which the officers of the Association visit the chapters. I found these visits physically very demanding, and I've never been able to talk at a nontechnical level, so it was pretty difficult going. I didn't make many of these visits.

Sampson: What were some of the pleasurable aspects of being President?

Savage: The thing I enjoyed most about the presidency was the preparation of the Presidential Address and giving it and seeing the consequences of it. I saw an immediate consequence. That evening, or maybe it was the next evening, a young man approached me as I was leaving the hotel and said "I just loved that; where can I do more?" and introduced himself to me as David Banks. I said "Go meet Tom Jabine" and Banks and Jabine have worked together ever since [on human rights].

Spencer: And what about the ASA committee you formed on National and International Security?

Savage: That's a consequence of that talk. Nancy Spruill led it for quite a few years and it had some impact. In addition to that, I was invited to Los Alamos and gave what, I think, was a very important seminar.

Spencer: What did you say in your seminar?

Savage: The way I structured the talk was that I had asked them for a few of their current technical reports, and then I had prepared a review of each of these technical reports. I had about four or five of those prepared, but I only spoke about one of them because there was so much discussion. When Alan Hoffman and I were at the Bureau in 1952 or 1953, we had been concerned with simulating

the defense of a battleship or small area with Nike missiles and had done many computer simulations. At Los Alamos, they were doing almost the identical type of work in the Star Wars defense of the country, but with the same kind of computations. Their computations were more deterministic, less probabilistic, than what we had done, only infinitely larger because they were using a Cray. They might have been using several of them, whereas we were using what was called the SEAC [Standards Eastern Automatic Computer]. So I explained to them in great detail how (1) they hadn't done anything technically different, and (2) how being so overdeterministic was totally misleading as to what they were doing. They were rather put out about all that discussion. I pointed out that they didn't have any feedback at all as to what happened to their reports. They wrote them, and that was the end of it. They had no idea of what impact they had.

Sampson: Richard, I'm going to wade into some difficult territory. I want to warn you up front.

Savage: Okay.

Sampson: Bruce and I both benefited very much from your critical comments at influential times in our careers. Yet, you have a reputation of saying honest and direct things to people without as much diplomacy as others might use. You have already brought it up. I can envision what this seminar might have been like at Los Alamos. I'm sure you're aware that people feel that your criticisms are sometimes on the harsh side. Do you want to say something about that, or what's been your philosophy of that over the years?

Savage: I don't think I ever realize when I'm being harsh. Starting at the Bureau, when I became an associate editor of *JASA*, and for the next forty years, I was an editor, more or less. I have no idea how many papers I handled, but it's more than a thousand. It's probably more than two thousand, including the *Annals*. When you do that much, I suppose you become a little callous, and you just don't pussyfoot around too much. You think you try to be a little bit polite, but I suppose you can't really consider people's feelings as much as you should. I don't think I ever intentionally used tactics of humiliation and devastation.

Sampson: I've never seen that, but I've seen you ask very pointed, hard questions.

Spencer: I talked to Steve Fienberg one time. He had sent us some correspondence on the great "machine" by Bishop, Fienberg and Holland [*Discrete Multivariate Analysis: Theory and Practice*]. I think that you'd sent some of your comments on the book to him. I recall he and I had been talking about the way you would give very detailed penetrating di-

rect comments on things, and he quite appreciated it. He thought that it was great to get this kind of close reading and feedback, but his colleagues were somewhat offended.

Savage: I think people are astounded when people actually read anything they write. Some people are very pleased, and other people react, "Why is he interfering with what I'm doing?"

Sampson: What were your interactions like with students?

Savage: In teaching, I think I was rather an ineffectual classroom lecturer. My first class in Minnesota was an undergraduate course in business statistics, and I followed the syllabus very carefully, and told the students I was following the syllabus very carefully. The syllabus laid out in great detail what the prerequisites were. Well, I, of course, ended up giving most of the students C's and D's. The Dean of the Business School asked me what happened, and I explained. He said, "Savage, you're not to be trusted. I'll never let you teach another undergraduate course in the Business School."

The only time I really thought I taught well was in the one-on-one probing, give and take, seminar-like class, where the students are put on the hot seat and are continually manipulated and maneuvered to react in the course. It doesn't always have to be done in a very small group. At Florida State I did this a couple of years with the large first-year graduate class in applied statistics, where we read Snedecor and Cochran [*Statistical Methods*] together.

Spencer: Yes, I'm a survivor from that experience.

Sampson: I see Bruce is smiling; I think maybe he can offer you a counter-point.

Spencer: I remember it. We went in and there was the scary professor who would call us up to the blackboard, one by one, and ask questions. I remember one student in particular shaking as she tried to write on the blackboard. And I was so scared I went and prepared questions to ask Richard ahead of time so he wouldn't call on me.

Savage: But I think it was an extremely effectual course in that I didn't lose very many students, and they knew, not necessarily the contents of the whole volume, but they knew what was pushing what in the volume. Those were probably the largest courses that I taught.

Spencer: It was a good experience. We basically tried to derive Snedecor and Cochran.

Savage: There's a second-year course in practical statistics at Yale, and the way I structured it was that we would take a particular problem and try to learn the background literature of the problem. One year we did lead poisoning, another year

we did small area statistics, and another year we started to prepare a bibliography on the methodology of the AIDS epidemic. These were wonderful courses to teach, but with just five or six students.

STATISTICS, SOCIAL SCIENCES, AND POLICY

Spencer: Richard, I'd like to shift gears a little bit. When did you get interested in statistics and the social sciences? How did your interest arise?

Savage: I've been trying to figure that out a little bit myself. I had no formal training in the social sciences, apart from a one-year survey course in college. While I was at the Center for Advanced Study in the Behavioral and Social Sciences, I didn't study any particular area of the application of statistics to the social sciences.

The next important connection of mine with the social sciences again seemed coincidental. I don't know where or how it came about. I was asked to be a member of what was, I believe, the first National Research Council panel on the census undercount. I wouldn't be surprised if the hand of Fred Mosteller was involved there, but I just don't know. That was my first introduction to Washington, and my first work in public policy. I found that panel pretty interesting. It struck me almost from the beginning that nobody was looking at whether it was important whether there was an undercount or not. In the panel report, there's no discussion of the implications of the undercount. It's all concerned with the origins of the undercount and mainly how to get rid of it.

Spencer: So you got interested in how the statistics get used?

Savage: Right after that, I became involved with other National Research Council panels.

Spencer: Also there was a meeting at the East-West Institute. Can you tell us a little about that?

Savage: That was during my 1970 visit to the Center. The purpose of the meeting was to develop an interaction between statisticians and demographers. At that time, there was very little interaction between the two groups. At the meeting, there were some statisticians sitting on this side and some demographers on that side and nothing much was happening. Finally, Nathan Keyfitz took over the leadership role, and an interesting dialogue and set of activities were developed as a result of the meeting. A vivid event that I remember from the meeting is that I went bird-watching with Lincoln Moses.

Spencer: Not too long after the East-West Institute Meeting, Keyfitz worked on the accuracy of population forecasts. In the early 1970s he started publishing on that subject in *JASA*.

Savage: Well, Keyfitz was the giant of the meeting; there's no doubt about that. Although the proceedings of the meeting did not appear, it may be that the meeting was helpful to him in going ahead with his own agenda. The meeting didn't directly impact on my work.

Spencer: Then there was a study at the National Academy of Sciences on setting statistical priorities.

Savage: Right.

Spencer: In discussing that study, Margaret Martin described (Straf, 1994) that someone at HEW wanted to know what statistics to collect in education, and so she looked in the statistics textbooks for the answer.

Savage: And came up empty, more or less.

Spencer: You chaired that study panel?

Savage: I chaired the panel, and the thing that's astounding to me is that I have absolutely no idea who was on the panel. I hate to say it, but I presume the panel was very ineffective. The project was almost a total disaster. This was, I think, the first panel of the Committee on National Statistics and the original director of the panel was extremely well qualified to do the work, yet he nearly sabotaged the whole project. You know what those jobs are like—you're to go out and build a liaison with the sponsoring agencies, develop the resource material, get the agency to focus on what the real problems are and help the panel to digest the material from the agencies. He had the habit of going out and having a fight with the agency members, coming back with no material at all, theorizing about what the whole problem might be and never following instructions from me or from Margaret. Eventually he was removed and Margaret essentially became the panel study director. The report of the panel is a very mild suggestion that costs and benefits of statistical programs should be considered—in a very mild nonstructured way. We thought that this was a reasonable suggestion. There was a formal hearing of the Advisory Committee to the Department of Education that was concerned with our report. The chairwoman of that committee held a McCarthy-like hearing. I was never so floored in my life, and the report was just torn apart.

Spencer: A hearing on your study?

Savage: On the study and how asinine the study was and how inappropriate it was, etcetera. Then nothing came of the study. Except, I (and I think others) have often found the study a useful reference for how to mildly do cost-benefit analysis. In the Reagan administration, all programs were in theory to be justified by cost-benefit analysis, and this study indicated how extremely difficult it is even to do that in a mild sort of a way.

Spencer: Had you worked on cost-benefit analysis before? I'm interested in how that ties into your Bayesian interests or your interactions with Howard Raiffa or Wald at Columbia.

Savage: I think it was just a natural thing you want to do. These governmental agencies have tight budgets and they have to try to figure out which programs to do and try to figure out which ones will pay off.

ASA COMMITTEE ON DISABILITY

Sampson: My recollections were that you were the person who motivated the establishment of that ASA Committee on Disability?

Savage: Well, I don't really know. I was Chair for a long time and Vincent Hodgson was Chair for a very long time. My own attitude towards accessibility was that for about thirty years or so I didn't think about it. After I left Warm Springs, polio was just an incidental background annoyance. The first contact I had with disability issues was at Florida State where they already had some federal money, and there was a Committee on Disabilities, and I was a member of it.

I think my first Washington involvement was on the AAAS Committee on Disabilities, Women, and Minorities, and I enjoyed working with the two AAAS project directors. The project directors were high-class people who were working hard and they were successful. They were establishing the accessibility norms for professional meetings. When I became more involved at ASA, I tried to bring what I learned at the AAAS to the Association.

Sampson: You did the "Ability testing of handicapped people"? [*Ability Testing of Handicapped People: Dilemma for Government, Science and the Public* (1992)]

Savage: I was on the panel for that. I don't know whether I was on the panel because I was a statistician or because I was disabled. It was a good panel. I remember working on this panel quite hard, and we came up with a serious agenda, things that really needed to be improved in the ability testing program. Until 1990, the ability testing groups—the commercial groups that do it—had stonewalled to prevent any modifications that would tend to improve the situation for the disabled. A negative thing they do is that they allow disabled people to take tests under nonstandard conditions. But then the test scores are flagged when they are sent to institutions, where the flag says that these scores are not reliable and should not be compared with any other scores. Over the last number of years, I've lost contact with the problem and I don't know if changes have occurred.

JIMMIE SAVAGE

Sampson: Richard, you mentioned earlier in our conversation some things about Jimmie.

Savage: Well, I mentioned his peculiar advice in junior high school.

Sampson: Right.

Savage: While I was in junior high school, I visited him in Ann Arbor. One day we looked at the plaster mathematical models of conic sections and such things. I was terribly impressed with the beauty of all of that, although I'm sure that neither Jimmie nor I could see any of the detail of these models. It was very striking to me. Then I spent the next year or so plotting sine curves and other trigonometric things associated with astronomy. I did these drawings for a class, but they were inspired by Jimmie. Certainly, Jimmie's interest in statistics—well, first in mathematics—made me major in mathematics at Chicago and Michigan. I don't think I would have taken a course in Michigan in electromagnetism if it hadn't been for Jimmie's interest in physics. I followed him and modeled after him quite closely, and I certainly wouldn't have gone into statistics without Jimmie. I certainly wouldn't have gone to Columbia, except that Jimmie was associated with Columbia at that time.

Sampson: Why did you choose to model yourself after him? Was it his being an older brother or could you say even more than that?

Savage: I don't think I could say much more than that. I don't know how your education is, but to me and I think to many other young people, there aren't many people around who are doing anything very interesting academically or intellectually. So if you have one, that might be the model you follow. Jimmie, for example, I think was somewhat influenced by my cousin, Julius, who was a civil engineer.

I think I became a non-Neyman–Pearsonite without Jimmie's help. The first time Wolfowitz told me what a confidence interval was, I knew it was an absolute joke and that nobody in their right mind would ever interpret a confidence interval in the mathematically correct way. But then as Jimmie was working on his *Foundations*, I listened to it carefully. Didn't really absorb all of it. But it just made more sense than Neyman–Pearson theory could make. I don't know how blinded I am. At one point at Florida State, I took *Statistics: A Guide to the Unknown* (1978), which has maybe fifty or seventy essays, and I analyzed the language of the essays in terms of their statistical foundations. Every essay in *Statistics: A Guide to the Unknown* uses a Bayesian vocabulary. There's not a single es-



FIG. 7. *Richard and Jimmie Savage in Tallahassee, 1964.*

say written in the Neyman–Pearson tradition. It’s astounding because most of the authors have little interest in Bayesian statistics. So I was always looking at what Jimmie was doing. I wasn’t necessarily understanding it. I’ve never been comfortable reading Jimmie’s *Foundations*. I find it just too hard to read. The notation is so beautifully simple that it eludes me.

Sampson: Did he try to directly influence you?

Savage: No. Just purely association.

Sampson: How did he react to you as a person who was in some sense following in his footsteps?

Savage: He began to treat me as an equal, I think, fairly early. The first professional interaction I had with him was when I was still a graduate student at Columbia and Jimmie was Chair of the Statistics Department at Chicago. He asked me who Bill Kruskal was, and I wrote a letter of recommendation for Bill, which I think was read seriously.

Sampson: And before then. How would you characterize your relationship?

Savage: First of all, you have to remember that he was eight years older than I, so we weren’t close. Not physically close from the time of my Bar Mitz-

vah forward. In fact, the only time after I was ten or eleven years old that we ever lived in the same town was six months in New Haven when I visited here in 1967.

Otherwise I would just see him occasionally in Chicago or elsewhere. We corresponded a bit, mostly about family business. The only time we worked together was at an IMS meeting in Tallahassee, where we wrote a little paper for the *JRSS*. And that paper was all my idea, and Jimmie just made it beautiful. We did it in two or three days.

The first meeting we went to together was an AAAS meeting in Cleveland. This must have been about 1948, and as I recall we didn’t go to any of the talks. Instead, we went to the Cleveland Art Institute. Jimmie had heard they had a great El Greco collection. We asked where the collection was, and somebody told us to go to a particular room. We got to the room and there were no paintings on the wall. There were a stack of canvases on the floor. All the El Greco’s were on the floor, and we put on our own El Greco show by turning these canvases over and looking at them as close as our noses could get.

Another meeting that we went to that was very memorable was an IMS meeting at Ithaca. Jimmie came one way, I came another way, and we met very early in the morning at a lecture hall. We must have spent at most five minutes in the lecture hall and then we went outside and sat on the piazza. And we spent the rest of this beautiful spring day on the piazza. At this meeting, Jimmie introduced me to Egon Pearson. That was the only time I met Pearson. John Tukey was there. It was a very memorable day. It was the day of the Bay of Pigs invasion, so you can date it rather precisely [April 17, 1961]. I was supposed to spend two or three days there, but Jimmie went home and I came back to Harvard with Howard Raiffa and Robert Schlaifer.

Maybe those were the only meetings where we were together. The day of his fiftieth birthday, when we were all here at Yale, was pretty nice. The whole family met, and I had bought twelve tickets for the Harvard–Yale game. We sent everybody off to the game and my father, Jimmie and I stayed at home. We had a very pleasant, wonderful afternoon. The next morning in the *New York Times*, there is a fish-eye photo of the Yale bowl with all 90,000 people there. You know in the fish-eye there is only one person in the foreground that you can really see. It was a full portrait of one of my nephews. That was the last time that we were all together.

Sampson: I was struck, Richard, that both you and your brother dedicated your books to your father. [Savage (1968), Savage (1954)]

Savage: Our father was very influential. He was not an educated man. He had only gone through the third grade, but he was very successful in his ability to do business. He had the respect of both the business and legal community in terms of how to do real estate business. And he enjoyed it; he enjoyed the business, not the respect so much.

Sampson: I'm just trying to understand. You and your brother are really so strongly influenced by your dad. There are lots of people that have fathers who are successful in business careers, and yet that doesn't reflect in the upbringing of their kids. There must have been something more that he brought to you that left you with such a strong feeling.

Savage: He was bright. He was ethical. We didn't want to work for him because he dominated people who did work for him. That we understood.

Sampson: He must've been supportive of your intellectual activities. What did he make of the two of you studying mathematics?

Savage: Oh, I'm sure he was proud as all hell.

Sampson: Was he active in the household when you guys were growing up? Did you see a lot of him?



FIG. 8. *Louis Savage, father of Jimmie and Richard.*

Savage: Well, he worked long hours, but he wasn't out gallivanting at all. So, you know, he was home every night. But we didn't particularly chum around. I think the most intimate thing we would do together is listen to Amos and Andy. Breakfast time often included his business associates. On many Sundays, I went with my father to look at property and to observe the efforts of the salesmen.

STATISTICS TODAY

Sampson: I want to come back to your thoughts about Bayesian statistics today. I assume that you stay current with that.

Savage: Oh, you're absolutely wrong. I have read no statistics for five years.

Sampson: Let's go back five years then. What was your feeling, at that time, about Bayesian statistics?

Savage: Go back a little further. By the time I was in Tallahassee, it was pretty clear to me that any kind of statistical inference considered in anything like a formal way should be Bayesian. And, in fact, any kind of statistical presentation that was ever made was at least informally Bayesian.

Sampson: And five years ago, what did you think of the state of what people collectively were doing under the umbrella of Bayesian statistics at that time? Did their advances, their influence seem more



FIG. 9. JoAnn and Richard Savage afloat, 1980.

than you would have expected forty years ago? Or hasn't it gone as far as you would have expected, given its naturalness?

Savage: I don't think I really have a well thought-out, or poorly thought-out, idea on that. But I can tell you what my impression is of statistics five years ago. Not just Bayesian statistics. Things were getting too technical for me. It was no longer possible to casually read statistical literature. It was totally out of hand. After I got polio in 1945, one of the first things I did as I recuperated was to join the American Mathematical Society and become a subscriber to *Mathematical Reviews*. At age twenty, with a master's degree, I could browse with pleasure in *Mathematical Reviews*. At age sixty-five, with forty-five years of experience, I couldn't even read the specialized reviews within statistics. Things changed in terms of specialization and intellectual level. One has to remember, why did people go into statistics? The reason why statistics was appealing was that it was the one field in which a dilettante could earn a livelihood.

Sampson: I share that with you.

Savage: But now, it's not so easy. If you want to be a Bayesian statistician from a technological viewpoint, you've got to carry a awful lot of heavy apparatus with you.

Sampson: And do a lot of computing. How has nonparametric statistics changed?

Savage: Remember we touched on the fact that rank order statistics was nonparametric statistics in 1950. The first time I saw the word "nonparametric statistics," used in a way that I failed to recognize what was going on, was in the book by Grenander and Rosenblatt (1957), when they start talking about nonparametric spectral analysis. That just threw me for a total loss. It was foreign, and of course today, or five years ago, when you speak of nonparametric statistics, it has nothing to do with rank order statistics. In fact, my interest in rank order statistics was a finishing touch on a dying field.

Sampson: Any final thoughts?

Savage: Early, you started to ask how did disabilities affect what I did and what I didn't do. And I think that eyesight was more important than mobility. Partly that's because JoAnn has been absolutely devoted to being with me. She made sure that I got around as much as I wanted to get around. I may not have done as much consulting as the other people at the Bureau, but I did enough. I may not have visited as many departments at Stanford as everybody else did, but I had a wide circle of friends. I'm sure I've been to as many meetings in Europe as most of my colleagues. Mobility always required a certain amount of planning.

Sampson: I understand that.

Savage: JoAnn's devoted efforts have made a rich life for me.



FIG. 10. Richard Savage, Bruce Spencer and Allan Sampson in New Haven, 1995.

Sampson: Richard, this has been an extremely enjoyable conversation. Both Bruce and I want to thank you for your open and honest responses and comments.

Savage: You're welcome!

ACKNOWLEDGMENT

The transcription of what were originally more than four hours of tapes and the preparation of subsequent multiply edited versions were only possible with the dedicated help of Diane Hall.

REFERENCES

- BERK, R. H. and SAVAGE, I. R. (1968). The information in a rank order and the stopping times of some associated SPRTs. *Ann. Math. Statist.* **39** 1661–1674.
- CHERNOFF, H. and SAVAGE, I. R. (1958). Asymptotic normality and efficiency of certain nonparametric test statistics. *Ann. Math. Statist.* **29** 972–994.
- CRAMÉR, H. (1946). *Mathematical Methods of Statistics*. Princeton Univ. Press.
- DEUTSCH, K. and SAVAGE, I. R. (1960). A statistical model of the gross analysis of transactional flows. *Econometrica* **28** 551–572.
- GRAB, E. L. and SAVAGE I. R. (1954). Tables of the expected value of $1/X$ for positive Bernoulli and Poisson variables. *J. Amer. Statist. Assoc.* **49** 169–177.
- GRENANDER, U. and ROSENBLAT, M. (1957). *Statistical Analysis of Stationary Time Series*. Wiley, New York.
- HOLLANDER, M. and MARSHALL, A. W. (1995). A conversation with Frank Proschan. *Statist. Sci.* **10** 118–133.
- GREENWOOD, J. A., OLKIN, I. and SAVAGE, I. R. (1962). *The Annals of Mathematical Statistics Vol. 1–31 Index*. IMS, Hayward, CA.
- PROSCHAN, F. and SAVAGE I. R. (1960). Starting a control chart. *Industrial Quality Control* **17** 1–2.

- SAVAGE, I. R. (1953). Bibliography of nonparametric statistics and related topics. *J. Amer. Statist. Assoc.* **48** 844–906.
- SAVAGE, I. R. (1962). *Bibliography of Nonparametric Statistics*. Harvard Univ. Press, Cambridge.
- SAVAGE, I. R. (1964). Contribution to the theory of rank order statistics: applications of lattice theory. *Rev. Internat. Statist. Inst.* **32** 52–64.
- SAVAGE, I. R. and SAVAGE, L. J. (1965). Finite stopping time and finite expected stopping time. *J. Roy. Statist. Soc. Ser. B* **27** 284–289.
- SAVAGE, I. R. and SETHURAMAN, J. (1966). Stopping time of a rank-order sequential probability ratio test based on Lehmann alternatives. *Ann. Math. Statist.* **30** 1154–1160.
- SAVAGE, I. R. (1968). *Statistics: Uncertainty and Behavior*. Houghton Mifflin, Boston.
- SAVAGE, L. J. (1954). *The Foundation of Statistics*. Wiley, New York.
- SHERMAN, S. W. and ROBINSON, N. M., eds. (1982). *Ability Testing of Handicapped People: Dilemma for Government, Science and the Public*. National Research Council, National Academy of Sciences, Washington, D.C.
- SPENCER, B. D., ed. (1997). *Statistics and Public Policy*. Oxford Univ. Press.
- STRAF, M. (1994). A conversation with Margaret Martin. *Statist. Sci.* **9** 127–145.
- TAMUR, J. M., ed. (1978). *Statistics: A Guide to the Unknown*, 2nd ed. Holden-Day, San Francisco.
- WALLIS, W. A. (1981). Memorial service tribute. In *The Writings of Leonard Jimmie Savage—A Memorial Selection* 11–24. Amer. Statist. Assoc. and IMS, Washington, D.C.