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A Conversation with Ralph A. Bradley

Myles Hollander

Abstract. Ralph A. Bradley was born in Smith Falls, Ontario, Canada on November 28, 1923, but grew up in the village of Wellington on the shores of Lake Ontario. He graduated from Queen's University in 1944 with an honors degree in mathematics and physics, was in the Canadian Army, 1944–1945 and returned to Queen's to complete an M.A. degree in 1946. In 1946 he entered the then new doctoral program in theoretical statistics at the University of North Carolina and received the Ph.D. degree in June, 1949. His first academic position was at McGill University, 1949–1950, followed by nine years at the Virginia Polytechnic Institute, 1950–1958. Bradley moved to Florida State University in 1959 to found a Department of Statistics there, heading the department until 1978, with the exception of ten months in Egypt in 1966 as a consultant to the Ford Foundation and the Institute of Statistical Studies and Research of the University of Cairo. He moved to the University of Georgia as Research Professor of Statistics in 1982. Although he retired in 1992, he continues to participate in activities in statistics there. He has been named Professor Emeritus at both Florida State and Georgia Universities.

Bradley has played a leadership role in the professional societies. He was Editor of *Biometrics*, 1957–1962, and Vice-President and President of the Eastern North American Region (1963–1965). He served as Vice-President (1975–1978) and President (1981) of the American Statistical Association (ASA). He headed (with Margaret E. Martin) the Coordinating Committee of the ASA Building and Development Fund (1982–1988) and was a member of the Future Goals Committee (1980). He has also served on various committees of the Institute of Mathematical Statistics, been active in the International Statistical Institute, the Gordon Research Conferences, the Southern Regional Committee on Statistics, and the National Institute of Statistical Sciences. He has served on the editorial boards of *Biometrics, the Journal of Statistical Computing and Simulation*, and *Communications in Statistics-Theory and Methods* and acted as an editorial advisor on the Wiley Series on Probability and Statistics (1954–1998).

Bradley has been active in research throughout his career. He has over 110 research papers in such areas as design of experiments, nonparametric statistics, methodology for sensory evaluations, sequential analysis, multivariate analysis, probability theory and computing techniques. Most of his papers stemmed from his statistical consulting on applied problems, or from the need to develop new theory to solve such problems. Bradley's consulting with General Foods on statistical methods in product evaluation was particularly influential in stimulating his own research and the research of others.

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Bradley has also been extensively involved with statistical consulting on organizational matters. His work in Egypt led to requests for his participation in regional conferences and reviews of statistical programs in the Middle East. He has been instrumental in establishing statistical consulting units and in obtaining funds to support research and graduate students in the departments where he had leadership roles.

Ralph Bradley has received numerous recognitions for his contributions to the discipline. He was elected Fellow of the American Statistical Association (1957), the Institute of Mathematical Statistics and the American Association for the Advancement of Science (1963) and became an elected member of the International Statistical Institute (1970). He has received Founders Awards from the ASA and the Southern Regional Committee on Statistics, (1992) and the (1994) Paul Minton Service Award of the latter committee. In 1970 Bradley was named the R. O. Lawton Distinguished Professor, Florida State University, an award made to only one faculty member per year and the University's highest faculty honor.

The following conversation took place at the Department of Statistics, Florida State University, Tallahassee, on June 11, 1999.

GROWING UP IN WELLINGTON

Hollander: Ralph, it's a pleasure to have you here today, and I'd like to start out with your childhood. What was your life like growing up in the village of Wellington?

Bradley: Growing up in the village of Wellington was pleasant. Some 800 people lived there, mostly retired farmers, merchants, fishermen and providers of village services. At that time there was commercial fishing on Lake Ontario and some boat-building in the village. Wellington and the county had some of the best beaches in Canada and water temperatures reached 70° F near shore in midsummer. Streets were oiled in summer to reduce the dust and I was young when the highway was first paved. I can remember when the main highway to Toronto, 120 miles away, was unpaved with hills that were very treacherous after a rain and worse in the winter.

Hollander: What did you do as a young boy?

Bradley: We had a lot of freedom. I was involved with the Scouts. I also liked boats and fishing. Spring fishing was particularly good in West and East Lakes and most evenings there would be 8 or 10 boats at the Wellington end of West Lake. These lakes were separated from Lake Ontario by a long sand bar and seldom had rough water. Once in a while I would go out on one of the commercial fishing boats for the fun of it. Then there would be rough water and lots of fish taken with Seine nets.

Hollander: Did you ever get paid by any of those boats to tag on and be a hired mate?

Bradley: No. It was just an adventure to me. I did buy a boat when I was about 12 years old. I paid $10 for a used boat, a flat bottom, wooden punt with no oars so I had to have new oars made. I sold the boat to a friend when I went to college.

Hollander: You went out alone at age 12 in this boat or you had friends with you?

Bradley: Both. My brother, Bruce, went with me occasionally and my parents sometimes went on a Wednesday half-holiday. Also I had a fishing buddy. He was a third-grade teacher at the school and boarded next door. He loved to fish. I had the boat and he had a car, so he'd drive us to the boat and we'd go out nearly every Saturday morning in the spring. We did a lot of rowing in that boat!

Hollander: What else was happening in Wellington?

Bradley: Wellington had a "consolidated school" with a school bus system and grades K–13, I think the first such school in Ontario, and a break away from the storybook one-room school. There were few school activities, some field and track but no organized teams, no dances and no graduation ceremonies.

Wellington had the only skating rink in the county, so all the hockey games in the region were played there. The games were well attended and provided entertainment.

We were members of the Wellington United Church of Canada and attended church and Sunday school regularly. The church also provided social activities that were needed. Villagers looked forward to the several church fundraising dinners each year. In practice these dinners ended as pie-eating contests, particularly by the male teenagers. Eventually it became apparent to the women that it was cheaper to contribute the necessary funds

than to produce the baked goods and this was the end of the dinners.

I did a lot of bicycling, often visiting school friends and helping with farm chores; students from surrounding farms missed a lot of classes because they were needed at home, particularly in the harvest seasons, and many did not complete high school. Occasionally I would bicycle with a friend 10 miles to the county town of Picton for a Saturday matinee at the only county theater.

Hollander: What did your parents do to make money?

Bradley: My father was the village dentist and the village wasn't big enough to support a dentist very well, so it was always a struggle. These were depression years and nobody had much money. Many paid for dental expenses with subsistence items: butter, eggs, fowl, cord wood, etc.

Hollander: Your mother was a homemaker?

Bradley: Yes she was. Before she married, she worked in Toronto as a bookkeeper in a department store. She developed rheumatoid arthritis at age 20 and suffered greatly from the disease most of her life, but with some periods of partial relief.

Hollander: Your father's experience as a dentist couldn't have inspired you to be a dentist.

Bradley: No, and he wasn't very happy with dentistry either. I decided very early that there was nothing for me in a town like Wellington as far as any future career or activity went. It was clear to me that, if I were to have some kind of a useful and interesting career, I'd have to get out of Wellington. The only way to get out of Wellington was through education. With my father's concerns about money, he couldn't quite imagine how he was going to get two boys through college. That worried me and it worried him. I needed money. One of the first things that I did was get a paper route. I delivered the *Toronto Star*, a liberal paper, and my father gave up the conservative *Toronto Globe and Mail*, something of a sacrifice, to be one of my customers. I had 60 to 70 customers. The papers came in on the bus from Toronto, arriving about 7:00 at night, and then I had to get them around town. I had a special big basket on the front of my bicycle and I rode that bicycle up and down those streets—I did 7 miles, 6 days a week, to deliver the papers. I'd go a mile into the country to sell one more. I first made 3/4 and later 4/5 of a cent per paper. That was a lot more per hour than working in a cannery—which we also did with no child labor laws.

Hollander: How many hours did it take you to complete that paper route?

Bradley: I could deliver the papers in less than an hour in good weather, not so quickly in a blizzard in the middle of the night when the papers usually arrived late. I rode that bicycle in snow, rain, and sleet; on rare occasions my father would use his car and help me.

Hollander: You must have developed hardiness and self-confidence from doing this.

Bradley: Well, I had a deal with my father that he was going to keep feeding me and I was putting the money in the bank for college. I think that the boat purchase was the only major expenditure that I made with that money, but I did have a number of welding jobs done on the frame of my bicycle—the hazards were ice and dogs. I did other things to earn money. I went from door to door at appropriate times selling Christmas cards, Easter cards, seed packets, and *Liberty* magazines. The other way we made money was in the harvesting of crops and canning the produce. The season started with the picking and canning of peas, beans and strawberries and ended with tomatoes, corn and pumpkins. One could make 20 cents per hour in the canneries and sometimes have a 20-hour day because the produce on hand had to be canned the day of arrival if at all possible. The other money- making side of it was to pick berries, tomatoes, cherries, apples, ... They paid 2 cents per quart for picking raspberries and few could pick more than 100 quarts per day.

Hollander: Were you a typical Canadian youngster who played hockey?

Bradley: No, I had a problem. I played a little hockey, but I was busy running those papers around in the evenings when the hockey practices were on. I did play intramural hockey for four years in college. We scored our first goals in the last game of the final year, but still our opponents were undefeated.

UNDERGRADUATE YEARS AT QUEEN'S UNIVERSITY

Hollander: You mentioned your fishing buddy was a schoolteacher. At the school you attended?

Bradley: Yes.

Hollander: What was that school like in academics?

Bradley: I have noted that it was the first consolidated school in Ontario. Ontario had a 13-year school system. The 13th year was supposed to be equivalent to the first year of college. It was interesting to me, but it was disappointing, too. I went to Queen's University, obtained credits for my Grade 13 courses, and was plunged into chemistry, physics and mathematics for which I was quite unprepared. It turned out that what I'd had in high school wasn't even close to being equivalent to what I'd have had if I had taken the same courses at the university.

I just didn't know anything about chemistry even though I had made an "A" grade. Chemistry was the big problem of my freshman year.

I had thought we had a pretty good set of teachers in high school in Wellington, but it turned out that I wasn't getting important information at all, especially, from the point of view of advice and counseling. There were a lot of Provincial Scholarships in those days in the Province of Ontario. I didn't know of their existence. When I got to Queen's, I found that nearly every would-be math major, except me, had a Provincial Scholarship. As it turned out, most of these scholarship holders dropped out of mathematics and went to some easier subject, and I was one of only two people in the freshman class who graduated in mathematics and physics four years later.

Hollander: How did you choose Queen's?

Bradley: It was good, it was close, and it was cheap. There's a simple answer.

Hollander: Probably many students had the same motivation.

Bradley: Most people went to the nearest place. The other near university was the University of Toronto, which was large, and had all the professional schools as well as the basic Arts and Sciences.

Hollander: You found yourself a little bit behind when you first entered Queen's in terms of your ability to pass exams, but you pulled yourself up by the bootstraps.

Bradley: It was a very difficult first year, it really was. I quit going to the chemistry class about November. This was a class labeled Chemistry II and it was a class on semimicro qualitative analysis. I bought the textbook for Chemistry I, and studied that from November until sometime in January by myself, and then I went back to Chemistry II. I never did quite know what was going on but did master some of the topics. These courses were all year-long courses, no quarters or semesters, so one was responsible for a whole year of material and took the final exam in the spring for each of these courses. That's a lot more difficult than a quarter system. Somehow I passed that final exam.

Hollander: You ended up getting a B.A. in math and physics. How did you specifically get interested in these subjects?

Bradley: I enjoyed mathematics, found it easy, and the puzzles of finding solutions to problems intrigued me. I wanted to major in mathematics and minor in chemistry but was immediately told that physics and economics were the only acceptable minors with mathematics because one could then become a high-school specialist in two areas. I delayed a decision by deciding to do two minors, physics and chemistry, but gave up on the second very early because of my noted problems with Chemistry II. The teachers in mathematics were good, but they were more authors of high school texts than researchers.

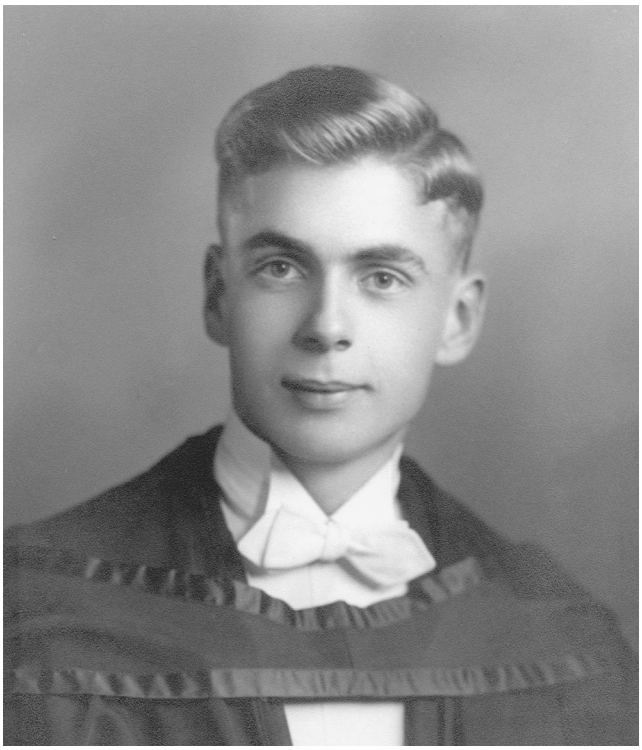

FIG. 1. *Ralph Bradley at Queen's University graduation, 1944.*

WORLD WAR II SERVICE

Hollander: Ralph, you're studying at Queen's and World War II is going on. How did this affect your studies, your life, and your involvement with training for the Canadian army?

Bradley: Well, it was required that every able-bodied male in those years take military training at his university—what we called the COTC, the Canadian equivalent to the American ROTC. So we put in quite a few hours a week, perhaps 8 or 10, of military training. There was a big shortage of male students in the university. Intramural sports were organized on the basis of class and year. I was in Arts 1944, the Arts and Sciences class scheduled to graduate in the spring of 1944.

We had intramural teams and we played similar teams from other years of Arts and Sciences and from Engineering and Medicine. We only had about 10 willing male students in the group, so we all played every sport. We weren't too good in any sport. We played hockey for four years. We had no money to rent the rink to practice, so we'd practice

hockey on Lake Ontario. For the last game of the fourth year we all played a position different from that which we had ever played before. My role in that game was to camp in front of the opponents' goal and I scored twice. As I mentioned earlier, we did not win a single game. But we tried everything. We did win the college softball championship that year.

Hollander: Was it difficult to be in the university while others were at war?

Bradley: There was an element of embarrassment... sitting around the campus studying when all our contemporaries were off to war. We had had no choice as it turned out. Canada entered the war in 1939... a couple of years ahead of the United States. The local regiment went overseas in November of 1939; a lot of the people I knew went then. The government became aware in another year or so that they were losing their best young people in the war. Young Ph.D.'s in mathematics were charging off to fly fighter planes in the Battle of Britain and the losses were terrible. So they decided that this couldn't go on. They formed what was called a Wartime Bureau of Technical Personnel and everybody in science, engineering or mathematics was under this wartime bureau, including undergraduate students majoring in these areas. We were all frozen in what we were doing. We couldn't enlist if we wanted to. When we graduated, or just before we graduated, a committee was sent to interview all of the relevant graduates at each university. There were representatives of each of the armed forces, the Army, Navy, and Air Force. There were representatives from industries needing technical personnel. There were representatives from other government agencies that needed people. They interviewed students and assigned each to a job—just as simple as that—"You report next Monday" or whatever. I had a reserve infantry commission in the COTC. I was picked to become an artillery officer with a view to learning gunnery and then going on to research in ballistics. That was the game plan. There were about 40 of us when we finally showed up for artillery training. By the time I was getting close to being trained in artillery, which I sort of enjoyed, the war was nearly over. Canada had artillery officers all up and down the east coast at antiaircraft batteries, there to worry about air and sea invasions, but doing very little. Then they decided that this was nonsense. So they sent all the artillery officers back to be retrained as infantry officers. That was not good. Artillery officers, when they hear a gun go off, pop up their heads to see where the shell landed or to correct outgoing fire.

Hollander: That's the wrong thing to do when you're in the infantry.

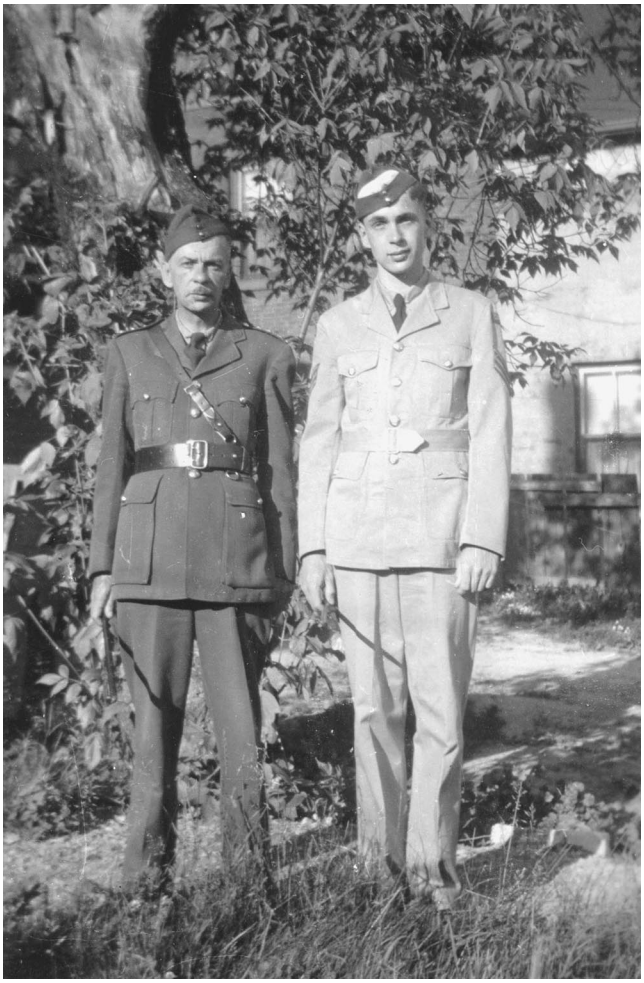

FIG. 2. *Ralph Bradley with his father, Alva, in Kingston, Ontario, 1944.*

Bradley: That's the wrong thing to do in the infantry. The few who got into action, as infantry officers didn't do well. The whole thing was disgraceful. Now there we were, 30 or 40 young people, training to be artillery officers in a camp with 10,000 artillery officers being converted to infantry. We weren't too popular!

Just to finish my great war experience... I had to resign my Second Lieutenant commission to go into the artillery and start over as it were. After a year and a half in the army, I graduated from officers' training, not in artillery, but in infantry. We had been converted to infantry also, because neither more artillery officers nor ballistics research was needed. I started as a second lieutenant and regained the rank at the end of my military career—so not much progress was made. I was sitting in an army camp in August of 1945 with little to do; we would sign up for more motorcycle training most weeks as something somewhat interesting. The

FIG. 3. *Ralph Bradley, motorcycle training on active service, 1945.*

army was developing a point system for discharge (this was after VJ day). Points were to be recorded for length of service, whether or not one served in a war area, and this or that. I was sitting there with zero points! I had visions of being five years in an army of occupation in Germany. I was wondering "Now how do I get out of this mess?"

Hollander: Obviously you did get out of it because you went back to Queen's.

Bradley: That's right. Inspiration. I called Queen's, called the Head of the Mathematics Department. I said "You must just be desperate for people to teach mathematics to the returning veterans." "Yes," he said, "I don't know what to do." I said, "I volunteer." I was out of the army in two days. Unbelievable!

Hollander: One of the wisest decisions you made.

GRADUATE WORK AT QUEEN'S

Bradley: When I got back to Queen's, I went around to Professor George Edgett, the University's only statistics faculty member, to see if I could do a Master's degree under his direction. He had all the Master's candidates in the department and everybody wanted to do statistics at that point. He was not an outstanding teacher, being mainly a researcher, but he was so enthusiastic about the subject that students wanted to study with him. But he immediately refused to have me on the grounds that he had so many students already that he just couldn't possibly handle another one. So I started a Master's thesis in finite projective geometry under the direction of Professor Israel Halperin. Later on, I could see that this geometry had applications in the construction of experimental designs and that a measure in real analysis could be a probability. I have never been sure whether teachers of such material were unaware of these associations or thought them to be obvious. What a pity that they were not able to better motivate their students.

Hollander: I noticed in some of your papers that you showed a lot of geometrical skill in deriving distribution theory.

Bradley: Yes, I later learned how R. A. Fisher used geometry to derive the Student t-distribution and things like that. I had worked with Professor Harold Hotelling by then, and he was one of the best in utilizing mathematical techniques to obtain results in statistics and economics. But let me return to Queen's and Halperin. He had gone into the army at the beginning of the war. He was only at Queen's the year when I began undergraduate study. He had been promoted rapidly and was a Lieutenant, acting Major. He got back to Queen's about the time that I did. He was very enthusiastic about having me as a student. He had me struggling along with a big volume on finite projective geometry by, I think, Veblen and Young. About the first of November, he left Queen's on very short notice and did not return that academic year.

Hollander: What happened to him?

Bradley: What happened was never fully clear to me. The other funny thing was that he'd approached me earlier, just before the end of the war, while I was still in officers' training, to go to Ottawa and help him with his ballistics research. This would have required that I resign from the Officers' Training Program, but he offered me an appointment as a Warrant Officer First Class, quite an unusual offer. Because I had gone through the tortures of officers' training in the dead of winter in Canada and was nearly finished, I decided not to drop out of the training program and so declined.

Hollander: Was he disappointed?

Bradley: I think that he was and I think that I would have enjoyed working with him. But I have an innate dislike of leaving things unfinished.

Hollander: Going back to your studies, when Halperin left you needed a major professor and thesis director. Was there someone else who could help with the geometry thesis?

Bradley: No, there was no one else, but it may have been a blessing. I had been having difficulty in becoming motivated in the geometry thesis and had no specific problem defined. I am not sure that I would have succeeded in producing such a thesis.

I went back to Professor Edgett and made a deal with him. He would suggest a topic in statistics, I would write a thesis with minimum help from him, and he would read it and approve or disapprove. This worked. I think that I met with him only once or twice. I started this effort in January and turned in a completed thesis in April.

Hollander: Ralph, I know from the times we have spent together that you met Marion during your days at Queen's and you've been married for over 53 years. She has been your stalwart partner in all of the building you've done in departments in the South. Would you please tell us how you met Marion?

Bradley: Well, Queen's had a small campus. There were only about 1,500 students at Queen's during war time, maybe fewer. There were dances every Friday night where everybody could go. It cost almost nothing to get in. I think we paid a dollar to help with expenses. I knew who she was for a while before I dated her. One of the things I was doing in the Fall of 1943 was tutoring in mathematics. I had a small group of students and two or three of them were girls. Marion was a friend of one of the girls that I was tutoring. That's how the connection was made. Also I was a Company Commander in the COTC at this time; we held training near the main women's dormitory in late afternoon and this drew an audience.

Hollander: You met her in 1943 and married three years later.

Bradley: Yes. It was early in the fall of the academic year 1943–44 and she was in her first year. Then I was off for my year and a half in the army and she was continuing work on her Bachelor's degree. My military training started at Petawawa, well north of Ottawa, and continued at Brockville, east of Kingston, where Queen's was located. Marion and I got together when I could get leave in Ottawa or Kingston. I got back for my Master's degree in the Fall of 1945 and we became engaged later that year. We both graduated in the spring of 1946. Following graduation I worked for her father, who was a very successful patent attorney, in the summer before continuing graduate study. Mathematics was good training: writing a patent claim is much like writing a theorem. Both require precise statements of minimum necessary conditions and the broadest sustainable conclusions. It was a pleasant summer and pleasant employment.

DOCTORAL STUDIES AT THE UNIVERSITY OF NORTH CAROLINA, CHAPEL HILL

Hollander: Ralph, how did you decide to join the new department at the University of North Carolina, Chapel Hill (UNC), as a member of the first group of graduate students?

Bradley: I knew I wanted to do further study of statistics, and the question was "Where?" Iowa State was about the only place I had heard of for graduate study in statistics. I applied to Iowa State and was accepted. The name that stood out there was W. G. Cochran, and they had a good faculty generally. So, I'm about ready to go to Ames in August and I discover that Cochran has moved to North Carolina State College (NC State). Since I didn't know anyone else, I wanted to follow Cochran, applied to NC State, got admitted at the last minute, went down there and talked to several of the faculty at NC State. They said, "You don't belong here." Cochran was there, but I don't think he was at hand when I was at the interview. They said, "You should be in Chapel Hill at UNC." They had a brand-new program in theoretical statistics in a Department of Mathematical Statistics. So there was a program in each of the two universities, both state universities, only about 35 miles apart.

Gertrude Cox had gone to NC State to found a Department of Experimental Statistics several years earlier. She had other goals too and the ability to achieve them. She formed an Institute of Statistics in the two universities and obtained money to support it and its three departments from the Rockefeller Foundation (the NC State Department of Genetics had become a part of the Institute). She was Director of the Institute, and W. G. Cochran and Harold Hotelling were, respectively, Associate Directors and Department Heads in Raleigh and Chapel Hill. Students commuted back and forth as necessary. All the statistics courses in Raleigh were taught on Monday, Wednesday, and Friday, those in Chapel Hill were on Tuesday and Thursday. NC State had three-hour courses and UNC had five-hour courses. I commuted every day that first year because the only housing we could find was in Durham, as U.S. veterans were given priority for on-campus housing. Marion was able to obtain employment with the North Carolina Council of Churches located on the Duke University campus.

Hollander: Who were your teachers as you shuttled back and forth between the two campuses?

Bradley: P. L. Hsu was at Chapel Hill that first year, along with Hotelling, H. E. Robbins, G. E. Nicholson, and W. Hoeffding. E. J. G. Pitman was a visiting lecturer for part of the year. I took courses from Hsu, Hotelling, Robbins and Pitman at UNC and Rigney, Cochran and Cox at NC State. I was most excited with the work of Hsu and arranged a dissertation topic with him before leaving to teach astronomy at Queen's in the summer of 1947. I was also trying to learn enough German to pass a required reading examination; I had demonstrated reading ability in French on arrival at UNC. When I returned in the Fall, Hsu was missing, having returned to China to visit his mother. Much later the department had a message from him that he believed that it was his duty to remain in China. Again, I was having trouble with major professors, but Professor Hotelling agreed to serve. Later, on a visit to China, I learned that P. L. Hsu had died and that Peking University was planning a celebration on what would have been his 70th birthday if he had been alive. He had continued an active research career under difficult conditions and was highly regarded in China. I learned that during the Cultural Revolution in China he simply moved his classes to the little apartment that he had on the campus of Peking University and was little affected by the Cultural Revolution. He hadn't married and didn't have a family.

Hollander: Hsu's courses were in multivariate analysis?

Bradley: There was a sequence. The first course was called Correlation, Contingency and Chi-Square. It featured bivariate distribution theory, the related theory of inference and some limit theory as it relates to contingency tables. Then the follow-on courses provided state of the art material on multivariate analysis. P. L. Hsu was a marvelous teacher. He never wasted a word, but never left anything unsaid that was needed. I was very disappointed not to be able to do a dissertation with him. I did do a dissertation with Harold Hotelling, and he was the most distinguished of the people in the Institute at that time. He'd done important things in mathematical economics, multivariate analysis, mathematics and physics. He was a little difficult to work with, not because he was unpleasant or anything like that, but he was too busy and doing too many things and not as available as I would have liked, but otherwise fine.

Hollander: What was your dissertation topic?

Bradley: Hotelling was interested in the effects of nonnormality on the Student tests. He had developed an adjustment for the effects of nonnormality on the significance level of the t-test for high significance levels. That was the idea. He had a clever way of getting an adjustment to correct for the effects of nonnormality. The adjustment, of course, was zero for the normal distribution. So, I took the idea, which he hadn't fully developed at that time, and generalized it for other cases. Both of us were working with geometrical approaches to the problem. I took also an analysis approach that permitted higher-order corrections to the adjustments of Hotelling.

Hollander: I believe that work was the basis for your 1952 *Annals of Mathematical Statistics* paper, "Corrections for non-normality in the use of two-sample t- and F-tests." I looked at that paper. It contains neat geometrical arguments and a hint of the thinking that would lead to your work on paired comparisons and rank tests.

Bradley: Yes, but a rather better paper appeared in the 1952 *Virginia Journal of Science*, "Corrections for non-normality in the use of the two-sample t- and F-tests at high significance levels."

Hollander: You were doing your Ph.D. under Harold Hotelling. Do you want to mention how Herb Robbins got in the picture?

Bradley: Professor Hotelling was ill at the time I was trying to finish my dissertation, which I turned in on the last day that it could be accepted for graduation in June 1949. There was a fuss with the graduate school. They were trying to tell the department that I wouldn't be able to defend the dissertation until my major professor was available. I understand that Herb Robbins went to bat for me and spent a lot of time arguing with the graduate dean in Chapel Hill to get permission to do the exam. R. C. Bose presided over it. It was a funny exam. Robbins himself started it with a question about the dissertation which I didn't understand. It went back and forth. I clearly wasn't answering the question he thought he was asking. Finally, I said "Well if your question was such and such, the answer is 'Yes'." He said, "Well, good, that's what I wanted to know." That was about the end of the discussion about the dissertation. At this point, Bose spoke up as acting chairman of the committee and said, "Well, I believe we are also supposed to test a candidate for the Ph.D. on his general scholarship." And he asked for questions from the rest of the committee. I got rescued. One of the committee members, Jack Rigney from NC State, an agronomist and applied statistician, said "I have a question," and he went into a twenty-minute explanation of a research problem in agriculture to design a potato digging machine. He explained what was needed in a general way. Then Robbins, a probabilist, said, "Let's suppose there are three potatoes in every hill." That was his starting point, declared totally unrealistic by Rigney. They got into a huge argument that

went on for about an hour. I left in the middle of the argument, seemingly being excused. But then it was decided to have a faculty meeting, so I sat for about two hours while they were in their faculty meeting. Finally, the departmental secretary went in and demanded to know whether I had passed or not. I had.

Hollander: You entered UNC in 1946 and finished in 1949. That's very fast.

Bradley: There were lots of things I could have done. I should have taken more courses in mathematics. I refused any financial assistance because related duties were likely to slow me down. I had the Canadian equivalent of the GI Bill which predated the American one. So I had that kind of support and I had some scholarships from both Ontario and Canadian governments, having found out how the system worked.

Hollander: Well, maybe this is good advice for some of our young readers, when they're eager to leave after, say, three or four years of graduate work and they have their Ph.D. nearly completed. It might be wise to stay in school one more year, write some papers, take some more courses, and shore up on mathematics, computing, and statistics.

Bradley: I have very strong views on one aspect of this. Too many students are tempted to take employment when they have completed all degree requirements except the dissertation. If possible at all, my advice to students is to stay in graduate school until the dissertation has been accepted. The pressures of a new job and other responsibilities often make further progress on a dissertation almost impossible. Sometimes would-be employers assert that completion of a dissertation is not important to them. I assert that it will be important to the student who seeks to change employment later.

Hollander: Ralph, who were your fellow students during your North Carolina days?

Bradley: The first students were mature and well motivated; many had been delayed by military experience. A number of excellent students followed Hotelling from Columbia University to finish dissertations in progress under his direction. Some names that will be recognized are Dorothy Morrow Guilford, Albert Bowker and George Nicholson. Some of my friends in the first UNC classes were Uttam Chand, Paul Minton, Milton Terry II, Glen Burrows, Max Halperin, J. Edward Jackson, Isadore Blumen, Bob Tate, Geoff Watson, and Bob Hader. Please blame a failing memory for omissions.

FACULTY POSITIONS AT MCGILL UNIVERSITY (ONE YEAR) AND VIRGINIA POLYTECHNIC INSTITUTE (NINE YEARS)

Hollander: How did you decide to go to McGill? That was your first appointment as a young Ph.D.

Bradley: I had a sense of responsibility to contribute to Canadian education. I had accepted a lot of Canadian funds to assist me in graduate work and felt that I had an obligation to go back to Canada. I went to McGill University, which is in Montreal and was one of the best universities in Canada. I had the usual beginner's job of teaching elementary classes for about 18 hours a week. My students were mostly freshmen and the classes were large and at the high school level by Ontario standards. For example, I had 85 students in a course on trigonometry. It took more effort to maintain some element of discipline than to teach the subject. It was an amazing experience. At that time Quebec had a four-year high school system so the freshman students were a year behind and a year younger than similarly classified Ontario students. I taught trigonometry. I eventually got to teach a half-year course in statistics. That was the only kind of statistics or probability course that I taught at McGill, although I did have an opportunity to teach an evening course to a group of mature engineers. I was very busy with course preparations and grading for the large classes. Support for faculty was almost nonexistent compared with American universities. The department had no secretarial help and no graduate assistants. The prospect of developing degree programs in Statistics, mentioned during preemployment discussions, seemed forgotten. It turned out that the department was also a base for theoretical physicists who competed for any prospective new faculty positions. I wanted to write papers based on my dissertation, but I never wrote a line.

It was clear to me by the end of the Fall term that I was going to be professionally dead if I stayed at McGill and continued with this kind of activity for another year. I went to the annual meeting of the Institute of Mathematical Statistics in New York in December, 1949. That was in the days when meetings were held between Christmas and New Year. I talked to people about the possibility of moving. I ran into some of my friends from Chapel Hill, and Milton Terry said that the Virginia Polytechnic Institute (VPI) had started a new department of statistics the preceding year. He said that they were recruiting faculty and asked me to visit there. I think that I also told Professor Hotelling that I needed to move and he arranged an offer from Northwestern University. Northwestern just

looked like another McGill to me, in the sense that it offered a lot of teaching in a mathematics department. VPI, with a statistics department that was a year old, was more interesting. Boyd Harshbarger, the head of the department, had become interested in some design of experiments problems related to taste-testing. He'd persuaded the U.S. Department of Agriculture to give him a three-year contract to do research on taste-testing. Now he never intended to do it himself, so he turned the whole project over to me. Milton Terry worked on it with me, but I was the faculty member with the major responsibility because Terry was still working to complete his UNC dissertation. I had really lucked out. I received an associate professorship, a research contract, and three-quarters time for research, albeit on a topic about which I knew nothing. While VPI was not as prestigious as Northwestern, it was the right place for me in Statistics. I needed to determine that I could develop a research career and/or become a consultant in statistics.

At VPI, we started with four faculty members: Boyd Harshbarger, Milton Terry, David Duncan and me. We offered a good Master's degree program from the beginning, and obtained approval of a Ph.D. program by the fourth year. We recruited good students and produced some outstanding graduates. We were a hard-working faculty. Harshbarger had done his doctorate under the tutelage of Cochran; Duncan, an Australian, had developed methods of multiple comparisons working with Alex Mood at Iowa State and Terry was nearing completion of his Ph.D. on nonparametric statistics under the direction of Hoeffding at UNC. John Tukey was the leader of research on multiple comparisons, a hot topic of the day, and visited VPI frequently, partly because he was courting his future wife in southwestern Virginia. This activity in statistics took place in an institution that had not yet offered the B.S. degree in Mathematics; indeed, the Mathematics Department was organized only to provide the minimum instruction needed to train engineers.

There were other stimulating activities also. We held several regional meetings of the Institute of Mathematical Statistics and the Biometric Society. We sponsored Summer Research Conferences at Mountain Lake, Virginia, near VPI. The department was one of four statistics departments supporting Southern Regional Graduate Summer Sessions on Statistics in four-year cycles. These activities involved visiting faculty members as well as the regular ones. M. G. Kendall was our most frequent visitor; R. A. Fisher was also a participant on occasion.

Hollander: VPI was the right place for you and, looking at your CV, it's clear to me that your research flourished during your nine years at VPI. You did many different things including significant work on paired comparisons with Milton Terry. Please tell me how that paired comparisons research got started.

Bradley: Well, it started this way. I had been accused during my preliminary exam at UNC of not doing a thorough enough review of the literature on my dissertation topic. Hotelling had told me there was nothing in the literature on the subject. But then one of the other members of my committee said that I just hadn't researched the field. I decided that a thorough review of the literature of taste-testing was mandatory. Terry and I read every available journal on food technology, psychometrics and statistics looking for relevant articles on organoleptic testing, the conduct of sensory evaluations and nonparametric statistics, and wrote careful abstracts of articles of importance. We published technical reports on our literature reviews and sent them to the U.S. Department of Agriculture to distribute to interested personnel. We made a very careful start.

It was clear that one of the problems in taste-testing was that a taster could not taste very many samples at a sitting, or what was called "taste fatigue" could set in. Tasters wouldn't be very discriminating then. There was also a problem of carryover of tastes from one sample to the next. It seemed, from this reading, that one needed to use an incomplete block design of some kind. There was a lot of work that had been done on the use of artificial scoring systems which might be used in an incomplete block design by treating the scores as measurements and using standard statistical techniques. The smallest incomplete block design would have one observation on each of two treatments per block, a paired comparisons, taste-testing design. We devised a model for this type of design based on orders, not artificial scores.

Hollander: The now famous Bradley–Terry model, which is referenced in many books, including Herb David's book on paired comparisons (David, 1963). It is also cited, for example, by Critchlow and Fligner (1993) who refer to it as one of the most commonly used paired comparison models. Critchlow and Fligner (1993) also cite two of your papers (Bradley, 1976, 1984) as excellent introductions to the paired comparisons literature.

Bradley: Right. Well, as happens many times in statistics, Terry and I were not the first to suggest the model. The earliest reference seems to be to Zermello (1929), a publication in German. Zermello was interested in the rating of chess players. In addition, the model has been reinvented by various authors

several times following my 1952 paper with Terry, with the main motive being parameter estimation of rating scores and not the development of statistical inference. Just last week, I was handed a copy of a paper from *Chance*, Glickman and Jones (1999), by one of the faculty members at the University of Georgia. This paper is concerned also with the rating of chess players and references Zermello and Bradley and Terry. Glickman and Jones are interested in the scores of players playing in a peer group of chess players and correctly understand that the scores would be group related. No comparisons of players in two distinct peer groups is possible. In the language of design of experiments, the two distinct, peer-groups data arise from an experimental design that is not "connected."

Hollander: The paired comparisons and taste-testing was a theme with you for many years. Seventeen years after the Bradley and Terry paper in *Biometrika* (1952), you did multivariate paired comparisons research with Roger Davidson that was also published in *Biometrika* (Davidson and Bradley, 1969). I remember that at a conference on nonparametric statistics, organized by Madan Puri at Indiana University in 1970, the related paper by you and Roger Davidson (Davidson and Bradley, 1970) was praised as being very useful and important.

Bradley: Now, that's interesting. I don't remember attending that conference.

There has been other research on paired comparisons that should be mentioned. L. L. Thurstone (1927) presented a model based on a normality assumption and F. Mosteller (1951) devised methods of analysis. Basically, the difference between the Thurstone–Mosteller model and the Bradley–Terry model is analogous to the difference between probit and logit models in bio-assay but data analysis is easier with the latter model (See Bradley, 1965, 1976).

When we were developing the paired comparisons work, one of the problems was the computing. We were trying to do this on Monroe desk calculators, and we needed better computing equipment. VPI didn't have anything but tabulators to keep track of the registrar's records and things like that. I wrote a proposal to develop tables for analysis of paired comparisons and I didn't know where to send it, so I sent it to the one place that was using paired comparisons seriously, the U.S. Army Quartermaster Food and Container Institute in Chicago. They were doing a lot of sensory evaluations of items and were clearly interested in using paired comparisons. I thought they'd like to have tables, so I sent my proposal to them with no concern on my part about whether they were a research-granting agency or not. I got a letter back from someone there who rejected the notion that they'd like to do that. But he made a mistake—he went through my proposal and went through line after line where he claimed I was wrong. That made me angry because I wasn't wrong. Otherwise I might have simply accepted the fact that they weren't going to give me any money. I went in to tell Boyd Harshbarger, my department head, about the nonsense I got back on the proposal. He said, " I'll call Senator Byrd and see what can be done." Byrd was the senior Senator from Virginia and the senior member of the Senate Armed Forces Committee. Well, he called Byrd's office and explained the problem to one of his aides who said he'd look into it. Harshbarger got a call back from the aide within an hour saying "I've made an appointment for you with the Quartermaster General. Can you meet him at 8 A.M. in Washington tomorrow morning?" We agreed.

Hollander: You had the U.S. Senator working for you.

Bradley: We charged off to Washington. We were met at the front gate of the Quartermaster Corps Headquarters by a colonel who introduced us to the Quartermaster General and then turned us over to someone who was going to handle the issue. We entered a military conference room. There were about 25 people there. They had brought in people from all over the United States who were doing or were responsible for sensory evaluations in the Quartermaster Corps. I had prepared a good presentation but they didn't want to hear it. The word had come down from the Quartermaster General and Senator Byrd to support our proposal. We had it won before we opened our mouths. They sat there for most of the morning with us present arguing about whose budget to take the money from and give to us. The Food and Container Institute was chosen.

We had obtained VPIs first computer. It was a bicycle chain driven IBM 602A, but we did the computing. Dick Cornell, a graduate student working with me, sat up night after night running that machine, cranking out our tables. (As you know, Dick Cornell was here at Florida State during the early years.) Every time we sent in a technical report or a progress report on this project, we'd get some kind of flak. The people in Chicago were not happy about us using their money. But it is an impressive little story about how knowing the right people helps. The desired tables were produced with considerable difficulty and published; see Bradley (1954). Now it is easy to program a personal computer to iteratively estimate the

parameters involved and do a complete analysis in seconds.

Hollander: You had acquired some support for computing. Did you acquire continuing support for your research?

Bradley: The original research contract was for three years. As the end of this support approached, I submitted a proposal for continuing research support to the Office of Naval Research (ONR). This was approved and was the beginning of some 20 years of ONR support, including research assistantships for two or more senior students. Also we were in a period in which traineeships were offered by the National Institute of General Medical Sciences to provide training to the doctorate in biometry, and we competed successfully in this program.

Hollander: Had you already started your consulting with General Foods? That consulting seemed to drive some of your research in paired comparisons and taste-testing.

Bradley: Yes, it certainly did through the years. The people at General Foods (GF) picked up the first *Biometrika* paper with Terry, and were very interested in it. So their Director of Research came down to VPI and talked to Boyd Harshbarger and me. They asked me to visit GF and give them some help with paired comparisons. That's how I got started, and I guess that was just within a few weeks of publication of that first *Biometrika* paper. Mavis Carroll was the statistician at GF and Cuthbert Daniel was a statistical consultant. I got to know him for the first time and learned from him. Mavis Carroll and I worked well together for some 20 years; I would visit GF, we'd do statistics all day, and then play duplicate bridge every night.

Hollander: Your paired comparisons research is nonparametric in spirit. You did some other work on nonparametrics at VPI. You developed a rank test for dispersion with Ansari, Ansari and Bradley, (1960), and that test still is used quite a bit today. It is referenced in many nonparametric statistics books, and it's typically included in software. I sometimes call it the Ansari-Bradley-Barton-David-Siegel-Tukey test.

Bradley: Siegel and Tukey (1960) had a paper on the subject in press, essentially the same time that my paper with Ansari was in press.

Hollander: The Siegel–Tukey test has the convenient feature that the null distribution of the test statistic is the null distribution of Wilcoxon's two-sample rank test, Wilcoxon (1945), so additional tables were not needed. Barton and David (1958) independently proposed an equivalent test. Klotz (1962) detailed the relationships.

Bradley: Yes, Siegel and Tukey were clever. I think we had, at the last minute, inserted a cross-reference in our paper to the Siegel–Tukey paper and vice versa, but they were basically published at the same time.

The whole introduction into the taste-testing business and into consulting at GF, almost all on taste-testing, was interesting. GFs greatest interest was in improving the quality of its Maxwell House instant coffee. But, being in this environment, I ran into all sorts of little questions in applied statistics that didn't seem to have answers. There were situations where the treatments in the paired comparison experiments were factorial arrangements. Concentrations in different aspects of the formulation, coffee strains, roasting procedures, and so on led to use of factorials following the research of Bradley and El-Helbawy (1976). Sometimes I recognized that different sets of data were simply parts of a factorial experiment. Being in this environment was a great stimulus. Some things arose that were not related to paired comparisons. For example, I have a little paper on sequential triangle tests (Bradley, 1953). It was a screwy little paper, but it worked very well and they were using triangle tests a lot at General Foods. A triangle test involved two samples of one product and one from a variation of it, and you could check the taste acuity of a taster by giving him a series of trials wherein he is to pick the odd sample from the three. This was used quite widely and the training and quality of tasters were important factors in the assessment of product improvement.

Hollander: A relatively simple application of the binomial distribution.

Bradley: That's right, and we made it sequential and we terminated the sequential test by a very arbitrary procedure. We changed the control lines from the parallel lines of Wald (1947) to a triangular set of lines that came to a point, and, if you got out of that area, the test was terminated. We did some work on average sample numbers and powers and this and that, a nice little procedure that was used at GF.

I got to the point where I could go in to General Foods and say "Look, I have a good idea for something or other. Let's do an experiment to see how it works." Mavis and others would cooperate. We did work on many products, including dog food at one stage. This was a stimulating little business. I used to go to GF at Tarrytown, New York about every three months, for two or three days. When I returned home, I would prepare reports on my activities. We did quite a bit by mail, too.

FIG. 4. *Early faculty at VPI: Ralph Bradley, David Duncan, Milton Terry and Boyd Harshbarger with Maurice Kendall, 1952.*

FOUNDING A STATISTICS DEPARTMENT AT FLORIDA STATE UNIVERSITY

Hollander: You were very productive in the nine years you were at VPI and then in 1959 you picked up and headed further south to head a new Department of Statistics at Florida State University in Tallahassee.

Bradley: I'd like to comment on that. I was enjoying the work at VPI. It wasn't a very stimulating place to live, but it was a nice place to live. Our two children were born there, Allan in 1951 and Linda in 1956. But I was in a situation where there was nowhere to go. I had an excellent salary that included some funds from the VPI Research Foundation. Boyd Harshbarger was head of the department and it was clear that he was going to be head of the department for quite a few more years. It looked to me that it was time to move if I could find the right situation. I went on several interviews at universities looking for department heads and one of them was Florida State University (FSU). FSU was firmly committed to the formation of a department of statistics but had almost nothing on which to build. I had a nice visit there and was impressed by the determination of the faculty and the administration to build a great university from what had been the Florida State College for Women. I was offered the position, but in the end turned it down. It was far from clear how the details were going to work out. FSU called me again in about six months and asked me to visit again and give advice on how they should proceed. I think this was a ploy to get me back, because I wasn't asked much about possible appointees. I did become convinced that they were really serious this time and that there was a real challenge. I accepted the position. Many of my contemporaries were struggling to get departments approved and this hurdle was not in question at FSU.

Hollander: Let's discuss your early development of the program at FSU.

Bradley: I came to FSU in September, 1959. They weren't very well prepared for me. I inherited two faculty members from the Mathematics Department, so there were three of us. The two faculty members from the Mathematics Department weren't interested in the research thrust that I expected in the Department of Statistics. I made it very clear that I was going to build a graduate program first and do our best to fulfill the other needs of instruction in the university, but that research was the priority. I assisted them both to get better jobs in industry and we stayed good friends through the years. The first faculty members I hired were S.K. Katti, who came about halfway through the first year, and R. G. Cornell from VPI, who was employed by the National Center for Disease Control and had given leadership in the polio vaccine trials. I also tried to hire Frank Wilcoxon who was well known at General Foods and whom I had met at Gordon Research Conferences. He was spending the winter somewhere in Florida in happy retirement and I had quite a lot of trouble locating him. He said he couldn't possibly join us that first year, but he agreed to come the next year. Frank came the next year in September, complete with a made-to-measure bicycle and several kayaks. He could only take a half-time job because if he wanted to go kayaking on a nice afternoon he was going to go, and he didn't want to have a guilty conscience. Well, it turned out to be great to have him at FSU. He had a lot of research ideas that he hadn't been able to work on during his industrial career, so he and I particularly got together on some of these projects. Sequential rank tests was one of his ideas, and this worked out very well. Frank was somewhat self-trained in statistics. He had some very ingenious ideas. He was all for quick and easy methods for data analysis.

Hollander: As you well know, he was trained as a chemist. He earned a Ph.D. in chemistry from Cornell in 1924. I shared an office with him for three months in 1965. He was determined to solve real problems.

Bradley: I could tell you lots of early history of Wilcoxon, but that probably doesn't belong here. He was a fascinating man. We hit it off. He was a great help. He taught a course on applied statistics for the physical sciences. Everybody who ever knew

FIG. 5. *Ralph Bradley at Florida State University, circa 1959.*

FIG. 6. *Frank Wilcoxon and Ralph Bradley in Tallahassee, 1963.*

Wilcoxon loved him, and again it was partly tied up with his complete enthusiasm for the subject matter. He had students lined up all through registration to get in his classes, even though he gave zero marks for exercises where the correct methodology was used but the numerical answer was wrong. He became well known very quickly.

Hollander: The work you and Frank did on sequential nonparametric procedures, cf. Wilcoxon, Rhodes and Bradley, 1963; Bradley, Martin and Wilcoxon, 1965; Bradley, Merchant and Wilcoxon 1966 may have partially inspired Richard Savage and Jayaram Sethuraman to work on termination of sequential probability ratio tests (Savage and Sethuraman, 1966).

Bradley: I think it did and it was certainly good for all of us. Frank was really pleased because he didn't really have the technical background to worry about the termination of sequential tests. Things that he wanted to see done in statistics got done with his leadership and the help of others.

Hollander: Frank had keen insight and he had those two beautiful ideas which were the catalysts, really the driving point, for the flourishing of nonparametric statistics: the Wilcoxon rank sum test and the Wilcoxon signed rank test (Wilcoxon, 1945).

Bradley: One of the other topics that interested Frank was multivariate nonparametric tests. I did get a little bit done on that problem with Dennis Wackerly and Kanti Patel, two of my Ph.D. students

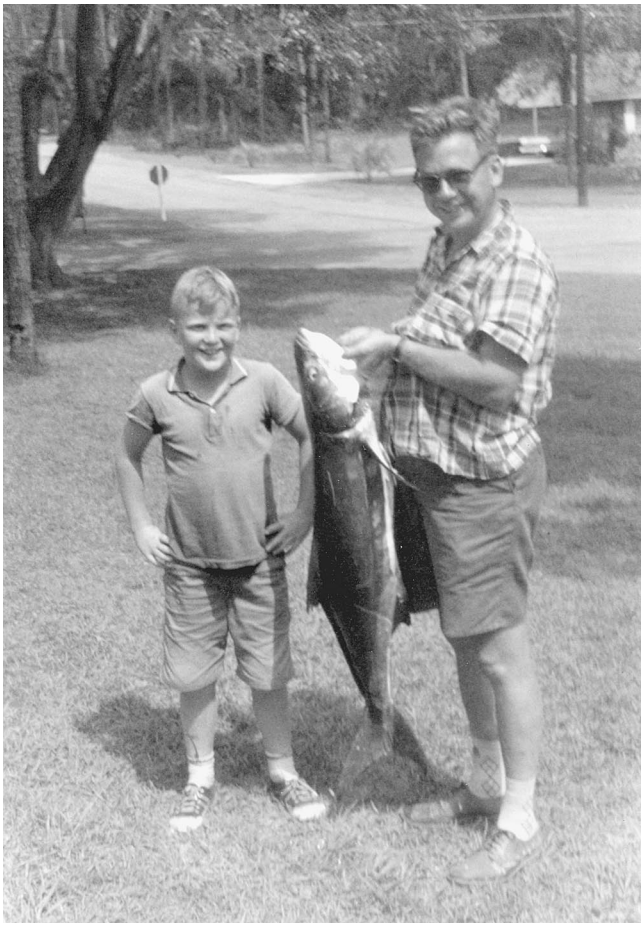

FIG. 7. *Ralph and son, Allan, displaying a big one, Tallahassee, 1963.*

(Bradley, Patel, and Wackerly, 1971). Multivariate nonparametric tests seemed a little hard to devise because it's hard to model the correlation problems. But we did some things, and they worked reasonably well.

Hollander: Multivariate nonparametrics was an active area in the 1960s and 1970s. Puri and Sen wrote a useful book on the subject (Puri and Sen, 1971).

We've mentioned Richard Savage. In 1963 you made a major appointment by bringing Richard Savage to FSU from the University of Minnesota.

Bradley: That's right. It was a great appointment for the department and I think it was a great appointment for him. He liked it at FSU. The climate was a lot better for him. He built a house with a swimming pool and generally had more mobility than in Minnesota. It looked like he was settled for life, except that, when his older brother, Jimmy Savage, died, Richard was offered the position Jimmy had held at Yale. Richard just couldn't turn it down and he left FSU for Yale in 1974.

We should say a little bit about the way I tried to go about building the department at FSU. We've mentioned Richard Savage, Frank Wilcoxon and S. K. Katti. We wrote joint proposals together and so on. But those days, the early 1960s, were in many ways the golden era of the sciences in the United States. There were grants, training programs and NDEA Fellowships. Everything was available. It was the post-Sputnik period. The United States was going all out to develop science. The Department at Florida State was formed at exactly the right time to benefit from all of this. My general effort in building the faculty was to go for the best available—that's certainly how I got Richard Savage and it wasn't easy either—I even had the President of the University participating in the interview when he visited the campus. That impressed Richard.

When I came to Florida State I was out to build one of the great programs in statistics. That was my goal. I was willing to approach anybody who might help us. I was also looking for bright young people. I brought in Shri Katti from Iowa State and Dick Cornell from VPI in 1960, Vincent Hodgson from the London School of Economics in 1961, Bill Thompson from North Carolina in 1963, Duane Meeter from Wisconsin and Fred Leysieffer from Michigan in 1964, you from Stanford and Morgan Hanson from New South Wales in 1965, Gerald van Belle from the University of Toronto, Ismail Shimi (who earned his Ph.D. from UNC) from Ain Shams University in Egypt, and Bob Serfling from UNC in 1967. (Bob Serfling was a coinvestigator on one of my Office of Naval Research grants for some time, and then he obtained an ONR grant of his own.) Jayaram Sethuraman (Sethu) came from the Indian Statistical Institute and Pi-Erh Lin from Columbia University in 1968, George Kimeldorf from Michigan in 1969, and Allan Sampson from Stanford and Doug Zahn from Harvard in 1970. Doug Wolfe came from Iowa for a postdoctoral appointment in 1969, and Frank Samaniego came from UCLA for a postdoctoral appointment in 1972.

In the early years, Sethu visited the department and gave a couple of outstanding seminars. He was still based at the Indian Statistical Institute at that time. I think he was visiting at Stanford when he came to FSU.

Hollander: That's where I met him. I became friends with Sethu in 1964 at Stanford. It was clear that he was exceptional.

Bradley: I decided he was a person we had to have.

Hollander: He was Raj Bahadur's first Ph.D. student at the Indian Statistical Institute. He already had an excellent reputation in probability and limit theory by the time he was at Stanford.

Bradley: Raj Bahadur was a contemporary of mine in Chapel Hill.

Sethu entered the department in September 1968. He was late getting to FSU because his father, a religious man, wanted him to travel on a propitious day.

Hollander: Fortunately for us, there were many days on the calendar that were propitious. He immediately had a very strong influence on dissertation direction and interacted with the faculty, especially with Richard Savage.

Bradley: Sethu was a tremendous asset to the department because he could do everything well. His research was broad, varied, and solid. He interacted with Savage. He interacted with you and so on. I think he was one of the greatest appointments we ever made. He was everybody's consultant. If we had trouble with a theory or a problem, we could ask Sethu how to go about it.

Hollander: He is extremely generous with his time and he brings a lot to bear on problems. If you present a problem to him, he solves it.

Bradley: That's right. He knows how to solve problems. He is unbelievably broad, able to comment on almost any problem anybody puts to him. I presume he's still acting in that kind of a role in many ways.

Hollander: Yes, he is the guru of our department.

In some of your years as department head, Fred Leysieffer was your associate head and he went on to a fine career in administration. You helped

train him. He's currently Associate Vice President for Academic Affairs here at FSU.

Bradley: Fred Leysieffer made a big contribution to the department. No question about that. I struggled for a while in the earlier days in the department. I was trying to do too many things. I could not get an administrative assistant from the administration, so I asked Fred to take on some of these chores. I got a little bit of criticism for that, for using a faculty member in this way. But I had to do something. I think it helped Fred. I think, in a sense, that it made Fred's career, not as a researcher, but as a good solid administrative person. He was very helpful to me. We sat down and wrote training grant proposals together. Dick Cornell had been so involved earlier on the first one. Frank Proschan came along not too much after Sethu.

Hollander: Frank came in 1971 when Boeing was phasing out their research labs. Frank brought a great presence in the area of reliability.

Bradley: Well, I know you were close to that work. I was not close to it at all and didn't attempt to be at that stage.

Did you know that I hired Jerzy Neyman at one time?

Hollander: I didn't know that. I remember when he came to Tallahassee to give some lectures and I got to talk with him. I picked him up one evening at the Travelodge Motel for a dinner party and he was clearly working on statistics in his room right up to party time.

Bradley: Well, he liked to visit FSU. In my weather modification days, we became fairly close. I had some money and a position open. I invited him to come as a visiting professor. In fact, I invited him to come and join the faculty. He did accept the visiting professorship. I think it was on a science development grant position that we hadn't filled with a permanent appointee. He liked to go down to the coast for seafood dinners and things like that. He accepted the position for three or four months, but he couldn't come in the end, because he needed back surgery rather unexpectedly.

This conversation does bring back memories. There are two references on the history of the FSU Department of Statistics that are fairly complete, Bradley (1990) and Leysieffer (1990). These references have further information on faculty appointments, graduates, training grants and research support.

Hollander: You've just mentioned your weather modification days. Please tell us about the types of statistical procedures involved.

Bradley: In the 1970s there was considerable interest in cloud seeding to create weather modification, usually more rainfall. Dubious claims for enhanced rainfall were made and questions arose on the nature of weather modification experiments, the definitions of experimental units and their selection, blocking, measurements of rainfall, seeding procedures and the validity of statistical methods as applied. Experimental variation was large relative to the effects produced. Cloud seeding was attempted in Viet Nam for military purposes. The Office of Naval Research had supported some studies of cloud seeding and desired to settle the disagreements and determine the validities of claims. Jerzy Neyman led a group at Berkeley interested in these problems, but their comments were not generally well accepted. I had served as a discussant at a symposium during this period but declined further participation until the Office of Naval Research asked me to take on a contract to appraise the literature of weather modification and make recommendations on experimental designs. Several symposia were held at FSU. Some careful experimentation was being done on winter storms at Santa Barbara and we had access to their data. We made our own analyses of these and other data sets. We started using parametric analyses, which drew few fans, and then went to two-sample nonparametric permutation tests, tests based not on the huge numbers of possible permutations but on samples of the permutations. These analyses had few assumptions and were consistent with the parametric analyses. A main reference is Bradley and Scott (1980), whose references lead back to our other publications on this project.

FIG. 8. *Wiley editorial advisors Ralph Bradley, Stuart Hunter, David Kendall and Geoffrey Watson with Wiley Editor Beatrice Shube, 1964.*

A YEAR IN CAIRO

Hollander: Ralph, please tell us about your year in Cairo, 1966–67. I know you went there under the auspices of the Ford Foundation to help establish training in statistics and computing.

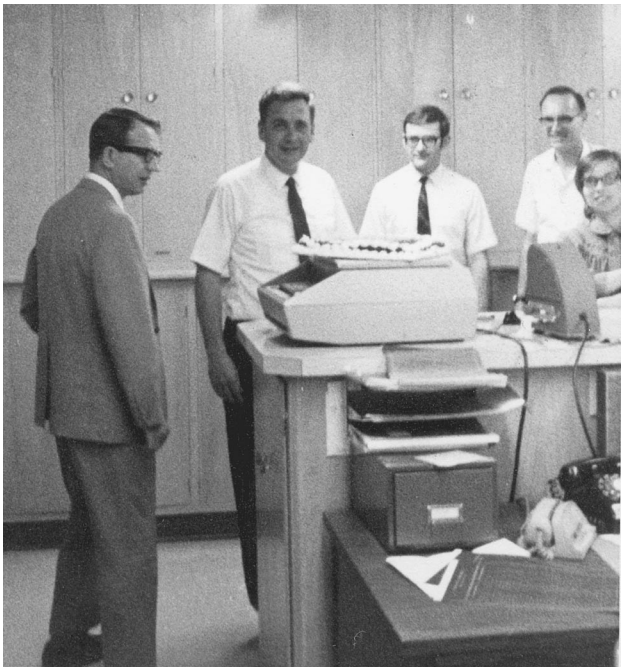

FIG. 9. *Jaroslav Hájek, Ralph Bradley, Fred Leysieffer and Dick Cornell at FSU, 1972.*

Bradley: I went to Egypt for 10 months, starting in the Fall of 1966. They had an Institute of Statistical Studies and Research (ISSR) at the University of Cairo and had some good senior people to help start that. A. E. Sarhan was Dean of the Institute when I went there. Gertrude Cox, the first consultant to the Ford Foundation, two years earlier, had persuaded the Ford Foundation to help build the Institute. Egypt had resources for local currency needs but almost no money for foreign exchange. The Ford Foundation provided funds to support a library and for appointments such as mine. I was a Visiting Professor at the University of Cairo and a consultant to the Ford Foundation. Gertrude Cox had persuaded the Ford Foundation to assist in the building of a scientific computing center at the University of Cairo in association with ISSR. Nothing had happened. The Foundation needed a viable proposal to send to New York for approval. ISSR personnel were never going to write a proposal. They just weren't set up that way—weren't thinking that way. So I arranged many meetings within ISSR until I gained an understanding of what was needed and talked with Ford Foundation personnel to understand what might be possible. I wrote a proposal that suited ISSR and took it to the Ford Foundation and approved the proposal as a Ford Foundation consultant—this was pretty easy. That was one of the interesting things that happened.

Because a British company was the only one willing to accept partial payment in Egyptian currency, I spent some time in England visiting university computing centers and trying to evaluate British equipment. Acceptance of some Egyptian funding made a substantial difference in the quality of equipment we could acquire. Plans for a new building to house the proposed computing center and ISSR were developed. The computing equipment was ordered. Construction was about to start when the June war of 1967 disrupted the effort. It is remarkable that the building was built, the computer installed, and an international conference convened in December 1969. At that time Milton Terry had succeeded me as the consultant and visiting professor, chosen because of his computing experiences at Bell Labs. He and I jointly organized the conference and the selection of the participants with the help of Sarhan, who had done an incredible job of proceeding with our plans in the turmoil in the aftermath of the 1967 war.

Hollander: I remember a photograph of you on a camel. Did you enjoy riding camels?

Bradley: Up to a point. Let me explain. We did some touring through Sinai. We went to St. Catherine's Monastery in the shadow of Mount Moses in the Sinai Peninsula. We rode camels through the Sinai to look at various ancient sites. Sitting on a camel all day is not great fun, but we did it for a couple of days. We were going up and down rocky mountain trails on camels, their feet seemingly slithering in all directions even though they were somewhat stable. Looking down 100 feet from the side of a camel on a mountain ledge isn't fun. These camels also had old Bedouin saddles and there were a lot of rivets sticking up in those saddles and that's not fun.

Hollander: You were playing Lawrence of Arabia?

Bradley: A little bit of that, yes. We also became refugees at the time of the June War and left Egypt about a month earlier than intended.

Hollander: You brought back Ismail Shimi from Egypt.

Bradley: Essentially. Shimi had gone to Beirut and was stuck there awaiting a U.S. visa. Providing him with a FSU appointment expedited the process. An American official assisted by letting me correspond with Shimi using a diplomatic pouch. I got lucky that year. I was still trying to hire Sethuraman so I corresponded with him from Egypt and somehow convinced him to come to FSU. Thus two good appointments were made.

Our period in Egypt was fascinating and any expedition, if only to the bazaars, could become

an adventure. We did visit Jerusalem, the Sinai, Alexandria, Luxor, Aswan, El Alemain, Hurgada, Gisa, the Faiyum and Petra.

EDITORIAL WORK WITH WILEY AND BIOMETRICS

Hollander: Ralph, you've done a lot of editorial work in your career, including 44 years as an editor for the Wiley Series in Probability and Statistics. You were editor of *Biometrics* for five years. You became a Wiley editorial advisor very early in your career, in 1954. How did that come about?

Bradley: In a sense, I hardly know. Marion and I went to New York in December, 1949 as has been noted. I met Sam Wilks and this trip got me to VPI. I think I was introduced to Wilks by Hotelling. We visited for a little while. He was obviously an important man in the discipline. It was a moderately casual meeting with Wilks. Somehow I must have impressed him. I know I did make an effort to visit with him for a few minutes at any meeting I went to thereafter, because I thought he was great. And he must have thought I was sensible because he put me on two or three important national committees. He was chairman of practically every committee related to statistics in the United States Government. He did a tremendous amount of that kind of work at that time. He succeeded Walter Shewhart as the editorial advisor to the Wiley Series on Probability and Statistics. He did this for several years and then needed help. So he appointed three of us to help him as editorial advisors. Two were Princeton people, Geoff Watson and Stuart Hunter. I was the third. I think he picked all of us because he thought we had some different attributes that would help the Series. Stuart Hunter was working in applied statistics in industrial areas and in the applied sciences. Geoff Watson was more theoretical, but he was working in some special areas that looked helpful. I don't know why he picked me, perhaps because of my activities on some of his committees.

Hollander: You were working in experimental design, nonparametric statistics, and distribution theory.

Bradley: Yes, I was more theoretical in some ways than the other two. Watson was fairly theoretical, but perhaps somewhat more specialized. Geoff was very good. I had some good discussions with him, through the years.

Hollander: Three years after you became a Wiley editor, you became editor of *Biometrics* for a five-year term.

Bradley: That was an interesting thing. Some of us, and Gertrude Cox was one of them and I guess I was one of them, suddenly realized that *Biometrics* wasn't being published anymore. That was a little startling. John Hopkins of the National Research Council of Canada was the editor at that time. John was interested in applications of paired comparisons, so we had some correspondence on various issues related to that. John had become ill and he kept thinking that he was going to get back to it but never did, apparently, so the days went by and *Biometrics* didn't get produced. Allyn Kimball, who was the international treasurer of the Biometric Society, and I, with the blessings of Gertrude Cox, and maybe her urging, went to Ottawa to see what was going on. We spent several hours at Hopkins' office, but he wasn't there. We talked to his secretary. We looked at the *Biometrics* files in his office and we were able to pull out enough accepted papers to produce the next issue of *Biometrics* which was overdue. We took those papers back with us, organized them into an issue, and got *Biometrics* back into publication. Sometime at this stage Gertrude asked for advice on who should become the next editor of *Biometrics*. It was pretty clear that Hopkins wasn't able to go on with it. I volunteered, so it was fairly easy to get permission to become editor of *Biometrics* given the circumstances.

Hollander: What did you look for in *Biometrics* papers? What was your approach, your philosophy as an editor?

Bradley: I liked *Biometrics* and I liked the fact that it had good coverage of design of experiments and basic applied statistics, particularly applications in agriculture. The journal was well received and continuity of its characteristics seemed important. I don't think that new editors have much immediate control; there are papers in the pipeline that were submitted by authors who considered editorial policy to be consistent with their needs. Future authors are influenced by their perceptions of the editor's interests. I think the fact that I was the editor continued the interest in papers on design of experiments, nonparametric statistics, and related topics. I think also that it was important to retain and appoint associate editors with expertise in important areas like statistical genetics, biological assay, sampling theory, estimation of population sizes and so on. David Finney had edited a section on Queries and Notes and continued this at my request. The section was of considerable interest to the readership.

Hollander: How many papers a year were submitted?

Bradley: I think that more than 100 papers were submitted each year and that we published 40 or 50 papers a year. You can estimate the acceptance rate

from this but I no longer have records to be more explicit. The acceptance rate was as high as it was because we often suggested revisions that improved papers so that they became acceptable. Also we did substantial editing on many papers and published after changes were accepted by authors.

Hollander: It takes a lot of work and patience to rescue good ideas but it is well worth the effort both for the authors and the journal.

ACTIVITIES FOR THE AMERICAN STATISTICAL ASSOCIATION

Hollander: Ralph, let's talk about your many activities for the American Statistical Association. You've been heavily involved in service to ASA in your career, including being Vice President of the ASA from 1975 to 1978 and President in 1981. What were your main thrusts during that presidential year?

Bradley: My main thrust related to the acquisition of an ASA building. This came about because at the end of a Board of Directors meeting, perhaps fairly early in my year as President, there was discussion of the renewal of the ASA lease on the property used as our headquarters office. Near the end of this meeting, I raised the question of building acquisition because it had come up every 10 years or so as lease renewals were considered. I felt some obligation to raise the question, and was a little surprised that everyone present was so enthusiastic about the acquisition of an ASA building. ASA didn't have much money and we were going to need some kind of a fund-raising campaign if we were to have a building. That's how it got started. The result was that a committee was appointed to investigate the acquisition of an appropriate building, and I was asked to chair this committee. I hadn't had that in mind. It turned out to be an arduous task. I got some wonderful help from Margaret Martin. She became cochair of the committee and she was on hand in Washington, and that helped a great deal. We acquired a building somehow or other.

Hollander: A very handsome, useful building in Alexandria!

Bradley: I like that building. I thought we were lucky in that we acquired a very nice building within our price range, one that was designed for completion to suit the buyer. I'm very pleased with the outcome. It is not easy to extract money from statisticians!

Hollander: The second oldest professional society in the United States (after the American Philosophical Society) really needs a building.

Bradley: That's right. Not many people know that it's the second oldest, but it is.

FIG. 10. *Ralph Bradley at the University of the Arab Emirates, with a committee to improve statistics in the School of Business, 1979.*

Hollander: I remember that you went to China during your presidency. What were the circumstances?

Bradley: We had an ASA group arranged by Fred Leone, but, I guess, under my chairmanship. We went to China to see what interactions between Chinese and American statisticians might be beneficial. We spent time in Beijing and we were able to talk to the leaders in statistics there. China is a little like many other countries. They have a central statistical office and we visited it to find out what was going on. They had a number of research institutes with statistical overtones that we visited and one or more of us spoke at each of these institutes. I spoke at one of the research institutes. I think it was helpful. We did a little bit of touring in the sense of going to see national sites and treading the Great Wall of China. One of the things that I insisted on

FIG. 11. *Ralph Bradley and Feng-Yi, China's Vice Premier for Science and Technology, China, 1981.*

FIG. 12. *David Cox, Nancy Mann, Dick Anderson and Ralph Bradley at the ISI meeting in Madrid, 1983.*

was that we visit an agricultural experiment station, and, after a fair bit of persuasion, we did. It was interesting. The meeting started off in the usual way, each group sitting along one side of a room staring at the other group and having the usual tea. Each of the Chinese people, and the Americans too, explained a little bit about their interests in statistics and what they were doing, and we got people talking in that way—but not communicating very well. We said we wanted to see the labs and the experimentation going on. This was just after the cultural revolution and they were at early stages of starting new programs. We went out and looked at some rice experiments. I did the right thing there. I went down and walked along the edges of the irrigation canals, looking at the young plants, and then the rest of them joined me. When it was time to go back to the lab, everybody was talking with everybody else and very friendly conversations were under way. The fact that I got mud on my shoes apparently helped.

Hollander: Were they interested in your ideas on specific subjects?

Bradley: We didn't get into much depth. We had encouraging preliminary discussions. Later in the year the Chinese made a return visit to the United States and attended the 1981 meeting of the ASA at which some sessions on Chinese statistics were held and reported in the ASA 1981 *Social Statistics Proceedings*, 112–118, 1982. Our Chinese guests also visited several U.S. universities, Bell Labs, several industries and Disneyland. More than a year later, at a meeting of the International Statistical Institute (ISI) in Madrid, I think all of the American group in this China expedition were present, and many of the Chinese, so we had a reunion dinner with them in Madrid. There has been an ongoing benefit from this.

A MOVE TO THE UNIVERSITY OF GEORGIA

Hollander: Ralph you had an extremely productive 23 years at Florida State. Nineteen of those years you were the department head, then all of a sudden in 1982 you were attracted by the University of Georgia. What happened?

Bradley: What happened? I don't know. I guess I thought it was time for a change in the sense that I'd been pretty fully involved with the activities at FSU. Greener pastures maybe and new stimuli. Also, I didn't want to seem to be looking over the shoulders of new chairmen. I could only hope that my leadership had been effective and that I was leaving a strong, self-sufficient department.

Hollander: When you went to Athens, Georgia, Lynne Billard was chair and then in later years Bob Taylor. Their leadership greatly strengthened that department, but you also played a big role in that department becoming a major player in the statistical community. In fact, after you went there, the

FIG. 13. *Four past Presidents of the ASA: Dick Anderson, Bill Kruskal, Ralph Bradley and Margaret Martin at the dedication of the ASA building in Alexandria, Virginia, 1988.*

Department had two presidents of the American Statistical Association, you and Lynne.

Bradley: We actually had a third at Georgia. John Neter also was president of ASA about that time. John was in the School of Business Administration. I don't know how UGA had three ASA Presidents so quickly. I think John was well known because of his successful texts and his service to ASA particularly. I suppose I went to Georgia feeling that I might be helpful in the development of another department and I was trying to help Lynne Billard. It had some personal advantages.

Hollander: What type of activities did you get involved with in Athens?

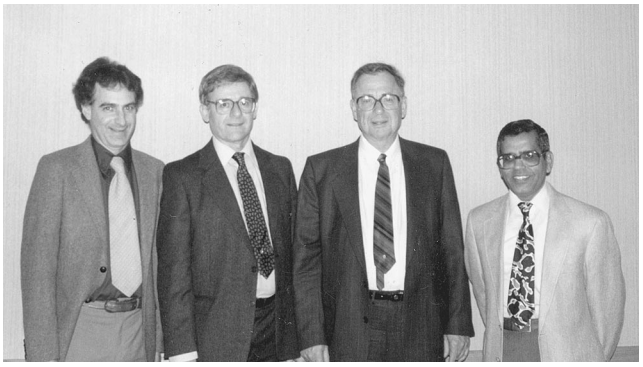

FIG. 14. *Four past chairmen of FSU's Statistics Department: Myles Hollander, Fred Leysieffer, Ralph Bradley and Jayaram Sethuraman, 1988.*

Bradley: Well, early on I went back to doing some consulting in agriculture, which I had always enjoyed at VPI. I had one paper with a master's student in agriculture almost immediately.

Hollander: You're speaking of "Expeller Optimization for Peanut Oil Production" (Bradley, Sivakumaran and Goodrum, 1985).

Bradley: Yes. Somebody at Georgia got the notion that they could make motor fuel out of peanut oil, and that was true. They were worrying about fuel shortages in this country around that time. This particular paper involved the use of response surface designs to determine optimum settings for an expeller designed to extract peanut oil from peanuts under various temperatures and pressures, the resultant oil product being capable of use as a diesel fuel in farm tractors.

Hollander: I also want to ask you about your research in experimental designs, particularly trend-free designs with Yeh (Bradley and Yeh, 1980, 1983) and Notz (Yeh, Bradley and Notz, 1985).

Bradley: These papers were based on work done at both VPI and FSU. The 1980 *Annals of Statistics* paper with Yeh was a nice paper. At VPI I went to look at the agronomic field experiments every once in a while. I didn't do it often but an agronomist friend, Clarence Genter, asked me to go out one day to look at a corn yield experiment, a big one in progress.. He wanted advice on how to deal with it. He had an incomplete block design, a balanced lattice, laid out in the field. But he had the plots within blocks end to end in one row of plots that extended over hill and dale. Then he'd have another row, another block, that would just be parallel to the first one, and so on. So, I thought that was staggering in the sense that there was obviously lots of soil heterogeneity from plot to plot in each row or block that could produce a large experimental error and low efficiency in the experiment. He had some counterarguments such as ease of management of the experiment. So these blocks had parallel plots. It looked to me that there might be some kind of a common fertility trend over the plots in each block that might be exploited through use of a restricted randomization of treatments (varieties) over plots within blocks so that the trend effects on each treatment were nearly equal. So, it just didn't look like a good experiment to me.

It was about 1955 or 1956 that I got this idea on managing the fertility trend. I had a couple of students on this through the earlier years, but they never quite came through with anything useable. C. M. Yeh and I worked on the problem and Yeh was very good. We published the papers that you cited,

the third with Notz. We developed trend-free block designs that in the simplest case positioned the treatments on plots within blocks so that there was no linear trend effect on any treatment. It turned out that Cuthbert Daniel and Frank Wilcoxon had similar concepts about trend-free designs. They didn't develop them in quite the same way. Pearce and Moore (1976) also had the concept, but the approach of Yeh and myself was more comprehensive.

Do you remember Bob Odeh?

Hollander: I remember Bob. He visited FSU and he is very good at computing. I note that you and Bob had a paper dealing with producing an algorithm for generating trend-free designs (Bradley and Odeh, 1988).

Bradley: Well, something bothered me about that. I wrote a program in Basic to generate trend-free block designs when they existed and it worked very nicely. I submitted it to *Communications in Statistics*, and they rejected it out of hand because I hadn't written the program in Fortran. I was stymied. I called Bob and said "How would you like to do a joint paper with me?" He converted my basic program into Fortran.

I have another nice program that I wrote some time ago in Basic that permits input of data from an experimental design as yields by blocks for each treatment on its specified plot; the computer now has complete information on the nature of the experimental design. The computer need not do anything complicated, no fancy formulas, but. . . .

Hollander: It works.

Bradley: It works, and you can put any design you can come up with into it. At the time I had an old IBM PC. It worked, but it was slow. Then I compiled it as a DOS program, and it ran quite rapidly. Now new desk top computers are so much faster that the computation for an experimental design is almost instantaneous. In addition, the program checked to see if the design was trend free and adjusted the analysis to remove trend effects.

Hollander: Is this being picked up by some of the experimental stations?

Bradley: Not to my knowledge. I haven't published it. I have a paper on this subject that is 90% complete and hope to finish it soon and possibly place the program on the Internet.

Hollander: Did the trend-free material lead in any way to your work on designs with Frances Stewart at Georgia?

Bradley: Well, it's part of my renewed interest in experimental designs. Think about agronomic experiments. They'll block the experiment to reduce between-plot heterogeneity in the field. Now you harvest the stuff and you need a couple of biochemists to do analyses giving the desired responses. Now you've got a second blocking criterion, the chemists, and something else about the conduct of the experiment could lead to a third blocking criterion. So now you've got a multiple blocking design—blocked once in the field, blocked once by chemists and blocked once again by something else. We handle some of this in Latin Squares, which is a start. But there has been no coordinated thinking about multiple blocking. I don't think it's done very often. I think there's a big potential gain in experimental efficiency through multiple blocking. That's the sort of thing we were doing. But among other things, Frances was generating experimental designs that permitted multiple blocking in designs with reasonable properties and balance. There's quite a bit that can be done. I don't know whether anybody else has ever picked this up or not. I think we're deficient somehow about thinking about sources of variation in many dimensions.

FUTURE OF STATISTICS

Hollander: Let me ask you a general question. You've been practicing statistics for fifty-three years and you still have some papers that you are working on and thinking about. How do you see the future of statistics and do you have some advice for up and coming statisticians?

Bradley: I guess to some extent, I'm a bit puzzled about what should go on in the future. We're getting more and more computing power and that lets us get around some of the problems that plagued statistics in the past. Somehow or other there seems to be a trend toward simpler experiments, not more complicated ones.

Hollander: I think the extra computing power has energized the trend toward more interdisciplinary work. Whereas maybe 20 or 30 years ago we were looking inward and getting involved with some esoteric questions, now the movement is to work in teams and really bring the power of statistics into other fields.

Bradley: I think there's some tendency to rely on the extra computing power and to pay less attention to good experimental design. Then its necessary to do a lot more computing than we could have done in the past to eliminate the same kinds of effects of variation. But it seems to me to be done in an awkward and unbalanced way and I'm a little concerned about that.

Let me limit this discussion on the future of statistics and refer you to my ASA Presidential Address, Bradley (1982a), as it seemed to be well accepted at the time. The title was "The Future of Statistics as a Discipline."

A BALANCED LIFE

Hollander: One of the things that always impressed me about your career, Ralph, is that, while you were very active in research, departmental leadership, teaching and consulting, getting grants, you maintained a remarkably balanced life. You participated in many recreational activities, tennis, softball, fishing, boating. You do antique auto restorations, sometimes with your son Allan. In your house you have beautiful woodworking projects. How did you achieve this? Where did you find the time to do all these things?

Bradley: Well, some of it was done at odd hours of the day and night. I'm sort of a one-track guy. If I've got some project going, I've got to finish that one before I think much about another one, whether it's statistics or woodworking. I guess I've put in quite a few evening hours at things like woodworking. But I enjoy it. I got hooked on woodworking in a manual training course in grade school. I didn't have any power tools, but I created some power tools, homemade ones using old sewing machine heads. Now you've got a bearing system and a shaft and you can put a pulley on a shaft, you can even make a pulley and put it on a shaft, etc. I did a lot of that kind of messing around. I had a homemade power jigsaw and a homemade lathe and these kinds of things. It was sort of fun. I made some decent stuff when I was pretty young.

Hollander: I remember many delightful evenings that Glee and I spent with you and Marion playing tennis, and you were quite competitive in all sports.

Bradley: Well, I guess everything you do, you try to do your best. My tennis was a little bit aggressive for some people. They didn't like bobbing and weaving too much. But we had a lot of fun playing tennis. We had a lot of noon-hour tennis right here outside these windows at FSU which was very good. You and I were involved in a lot of that.

Hollander: When Glee and I played you and Marion in doubles, we usually split sets. You and I would have preferred to win both sets, but Marion and Glee were more diplomatic. If the Bradleys won the first set, the Hollanders would typically win the second, and vice versa.

Bradley: I don't remember that. Let's switch to fishing. Fishing has been right there since my youth. In the fishing season, I'd adjust my paper route so it ended right where I had the boat. I kept my fishing pole underneath the boat while it was on the bank, so I could get out on the water in a hurry. The fishing was pretty good.

Fishing from Tallahassee was good, but somewhat seasonal. I used to do a lot of fishing on the Gulf of Mexico when I first came to FSU. There could be fabulous fishing there but one needed to watch the weather.

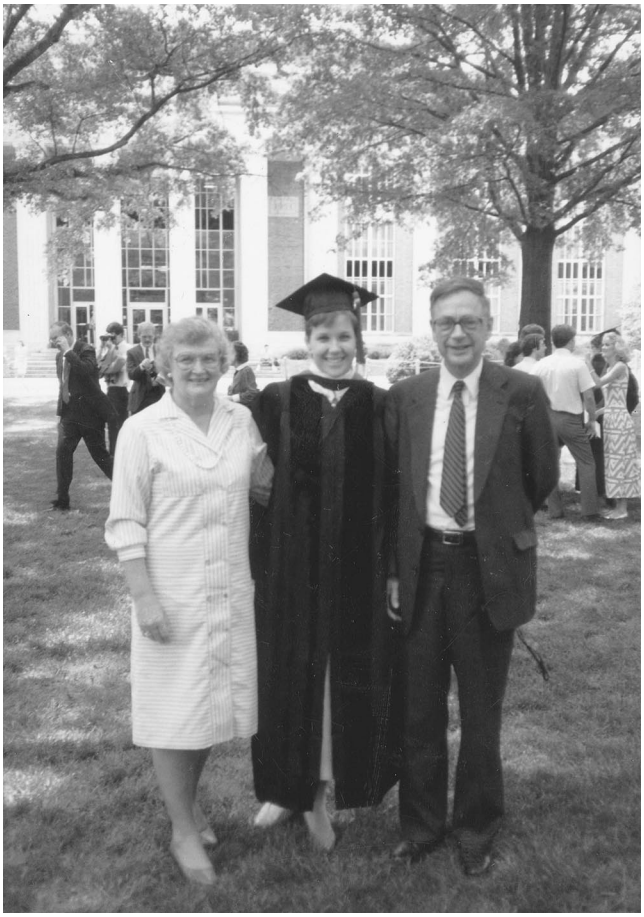

FIG. 15. *Marion, daughter Linda, and Ralph at Linda's graduation from Law School, University of Georgia, 1986.*

Hollander: You used to go fishing with the Associate Dean of the Graduate School, Tom Lewis.

Bradley: Yes, we fished together a lot. Larry Charmers and I fished together most. He was in Psychology when I came to FSU. After becoming Dean of the Faculties at FSU, he left to be Chancellor of the University of Kansas. Ken Brewer and I did a little fishing together.

Hollander: You and Marion engaged in enormous amounts of travel and one of the things you do now is collect antique books.

Bradley: That's right. Very specialized books. It's really her project. She thought that one up. She thought, if we were going to travel, there ought to be something we were seriously looking for but not filling the house. Steel engravings by a man named Bartlett and others are quite nice. In the 1830s, steel engravings were in and quite a few of them available. This was before photography. People, apparently, were very interested in travel in

FIG. 16. *Ralph's brother, Bruce, Bruce's wife Edythe, and Ralph, Stoney Lake, Canada, 1994.*

those years, but individuals didn't have the means to travel much, so they liked to have books with pictures of places of interest. That 's how steel engraving started and became the means of reproducing pictures. Also, they could take a major piece of art and an engraver could copy it onto a steel plate. Some of the very best art was reproduced that way. We have a set of three volumes of the steel engravings of paintings by J. M. W. Turner (1775–1851) entitled *The Turner Gallery*. Turner was one of the great English artists of his time. These are just gorgeous books.

Hollander: I've had the pleasure of seeing some of them and they are remarkable.

This is really a softball question, Ralph. How are you and Marion enjoying your grandchildren?

Bradley: We enjoy them very much. On the other hand, neither pair is close enough that we see them very often. Our son, Allan, and his wife, Millie, have two children, a daughter, Allison, just enrolled at Texas A.&M. University, and a son, Jonathan in senior high school in West University, Texas. Linda and her husband, Tom Stirling, have two children, Alexander and Caileigh, ages 7 and 5. Linda is a lawyer with the Security Exchange Commission in Washington. Allan is a senior executive of the Coastal Corporation, seeking corporate opportunities around the world. He's done an incredible amount of traveling. Both families are doing well and both families are happy families.

Hollander: Ralph, you've played a tremendous role in the development of statistics everywhere,

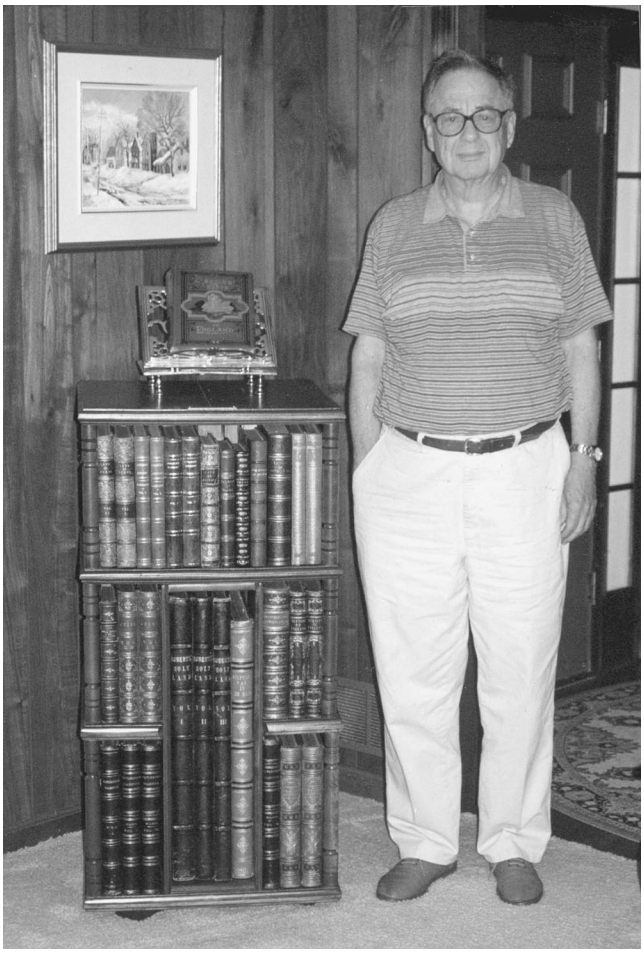

FIG. 17. *Ralph Bradley and some of his and Marion's antique books, 1999.*

and particularly in the south where you have left a very strong mark on three departments, those at VPI, FSU, and UGA, and you founded the department at FSU. Many students and colleagues who worked with you are grateful for the excellent standards you set. You actually taught many of those people how to be professionals.

Bradley: I'm proud of that, Myles, because a number of them have had careers similar to mine, and have been willing to contribute to the profession at large, head up departments, and things like that—you're certainly one of them. Something is carrying over to these students, and, apparently, they liked the way I did things and tried to emulate them.

I have to add that the greatest rewards for a teacher are the successes of one's students. I have been particularly so rewarded. I'll list my doctoral students below, again risking possible omissions.

VPI: A. R. Ansari, R. Chaddha, R. G. Cornell, J. J. Gart, J. E. Jackson, C. Y.

Kramer, R. N. Pendergrass, T. S. Russell, R. E. W. Schumann, H. A. Still, H. C. Sweeny, R. E. Walpole; **FSU:** D. A. Brindley, R. R. Davidson, A. T. El-Helbawy, D. W. Fairweather, D. C. Martin, K. M. Patel, T. C. Redman, D. A. Rollier, B. N. N. Young, D. A. Wackerly, H. D. Weed, Jr., C. M. Yeh; **UGA:** F. P. Stewart.

Hollander: On behalf of all these people and myself, personally, I thank you for your dedication to the profession. It has been a great honor and pleasure for me to have this conversation with you.

Bradley: I greatly enjoyed it.

ACKNOWLEDGMENTS

Ralph Bradley and Myles Hollander thank Leon Gleser for a careful reading and helpful comments and Pam McGhee for transcribing this conversation.

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