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A Conversation with Leslie Kish

Martin Frankel and Benjamin King

Abstract. Leslie Kish was born in Poprad, Hungary in 1910. He arrived with his family in the United States in 1926 with an English vocabulary of approximately 300 words. Within a year, his father died and Leslie became the principal wage earner in a five-person household. By 1929 he had secured full-time employment as a lab assistant at the Rockefeller Institute for Medical Research. One year later he finished Bay Ridge Evening High School and enrolled in the College of the City of New York evening program. He became a U.S. citizen in 1936.

In 1937, with less than one college year left, Kish joined the International Brigades and went to Spain to fight for the Loyalists. He returned to the United States in 1939, and that same year received a B.S. in mathematics, cum laude, from the College of the City of New York.

Leslie Kish was hired by the U.S. Bureau of the Census in 1940 and in 1941 moved to the Division of Program Surveys of the Department of Agriculture. From 1942 to 1945 he served as a meteorologist in the U.S. Army Air Corps. After the war he returned to the Department of Agriculture, but in 1947 moved to the University of Michigan as a member of the newly created Survey Research Center, which became the Institute for Social Research. While working full time, Kish received an M.A. in mathematical statistics in 1948 and a Ph.D. in sociology in 1952. He became a lecturer at the University of Michigan in 1951, an Associate Professor in 1956, a professor in 1960 and professor emeritus in 1981.

Kish is a Fellow of the American Academy of Arts and Sciences and the American Association for the Advancement of Science. He served as President of the American Statistical Association in 1977. He was elected Honorary Fellow of the Royal Statistical Society in 1980 and was named Honorary Fellow of the International Statistical Institute in 1994. In 1988, Kish received an Honorary Doctorate in statistics from the University of Bologna (900th anniversary) and in 1995 was elected an Honorary Member of the Hungarian Academy of Sciences.

In addition to his pioneering work in the theory and practice of survey sampling, Kish has been responsible for the training of hundreds of practicing sampling statisticians in the United States and in more than 90 other countries.

The following conversation took place at Leslie Kish's home in Ann Arbor, Michigan, on July 22–23, 1994:

EARLY YEARS

King: By the way, happy birthday. I know you have one coming up on the 27th. Leslie, could we begin with some information about your early years? Tell us about your childhood, your education, how you came to the U.S.?

Kish: I was born in Poprad, Hungary. My father was an engineer, who worked for an electric company that moved him to different plants every few years. So we lived in about five different towns before coming to America, always in what was then Hungary, but which became parts of eight coun-

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FIG. 1. *Kish in 1914, Zilah Transylvania, Hungary.*

tries. For example, Poprad, became part of Czechoslovakia, then Slovakia. Our last Hungarian town, Zilah, became Romanian in 1919.

King: Literally eight?

Kish: Eight countries. After graduating from the great Budapest Polytechnic, my father worked during 1906–1908 in the U.S., went home and married my mother in 1909 and I was born in 1910. My life has been dominated by World Wars I, II and the Depression in between. My father always wanted to come back to the U.S., but the First World War intervened. Father and Mother put in their application to come to America in 1919. We couldn't go from Romania to Hungary because Hungary didn't have enough space to hold all the Hungarians. It is still true that one-third of the Hungarians live outside the country.

Why did we come to America? As Simon Kuznets, the Nobel economist, said, "For the usual economic reasons." My father felt that there was no future for us in Romania. We were all prepared to come in 1921 and then Congress passed an act that limited immigration. We finally came in 1926. I was 15 years old, the oldest of four children. My father went to work for Brooklyn Edison immediately. The day after we came, I did two things: I went to school, as did my two sisters and my brother, and I joined the public library.

King: Did you speak English fluently then?

Kish: My father taught us some English—perhaps 300 words—before departing, on the ship *S.S. Berengaria*, and arriving at Ellis Island on March 3, 1926. But I immediately picked up books in English and, luckily, I did the right thing from the very beginning. Instead of using dictionaries, I kept on reading American English until I got the sense of the words, which is, I understand, the best way to learn a language. By the end of that year, I had finished elementary school.

King: Where did your family settle when you came to the U.S.?

Kish: We lived on the lower east side of Manhattan for a month or two. Then we moved to Brooklyn, where I remember Public School 128 in Bensonhurst. In August 1926, my father died and we were suddenly left on our own—my mother and four children. Then we took a vote: Do we go back to Hungary and live with our grandfather or do we stay in America? It was a pretty big decision and at 16 I was, in a way, the top decision maker. We decided to stay here. I went to evening high school and started working in January, as did my sister at age 15. My mother also started doing needlework at home. So we were three people making some kind of a living. Starting in January 1927, I always had a full-time job while going to night school, except during my war service. So the picture is coming from one world war, through a depression and going into another world war.

King: Who was your favorite author in those days? I am curious.

Kish: One of them was Leslie Stephens, a nonconformist humanist author, who wrote books and essays and was extremely popular in England. I changed my name from László to Leslie. Now I have a granddaughter called Nora Leslie Stephens because my daughter married a Stephens. But I have had lots of favorite authors—I have always been quite a voracious reader. Anatole France was a favorite. Also, Shaw, Ibsen, Sinclair Lewis, Dreiser and other humanist–progressives. I also read a lot of science and politics.

THE BEGINNING OF A STATISTICAL EDUCATION

Frankel: Tell us what was your first course in statistics, if you remember, and about your education after eighth grade.

Kish: I finished Bay Ridge Evening High School in 1930 and then started City College in the evening program. I was taking mathematics courses and planned to be an engineer like my father. My acquaintance with statistics had to do with my work at the Rockefeller Institute for Medical Research.

King: What exactly were you doing in the Rockefeller position?

Kish: I went there in 1929 for $70 a month (which is less than I get paid for an hour these days), for 54 hours a week—9 hours times 6 days a week. I first dealt with rabbits, instruments, sharpening scalpels and so on, but after a year or two, it turned out that some statistics had to be calculated. I had two courses of college math by that time—eventually four with calculus—and I started doing the statistics. I was working with some young doctors who had some training—but no courses—in statistics, and there was a book called *Statistical Methods for Research Workers* by R. A. Fisher (1925). So my education in statistics consisted of reading R. A. Fisher and reading it again and again and again—about four times. I also found references to Fisher and "Student" in the Institute Library. I read a book by Yule that became Yule and Kendall (1937) later, there was a book on medical statistics by Raymond Pearl (1930)—you can still find it in the library—and Tippett's textbook (1931). Eventually, there were monographs put out by Henry Wallace and George Snedecor (Wallace and Snedecor, 1931) on the analysis of variance and on correlations. Then also the writings of Karl Pearson and books on probability by Fry (1928) and by Hyman Levy (Levy and Roth, 1936). That was really my education—reading all these very important books.

King: We have to explain that Henry Wallace, who wrote the monographs with Snedecor, was later Vice-President of the United States under Franklin Roosevelt (1941–1945).

Kish: Oh yes, and he will come in the story later too. He was a statistician and founder of the first statistical laboratory in the U.S., at Iowa State, with Snedecor and Charles Sarle.

Frankel: Did you discover these books on your own or were they originally supplied by these doctors who worked with you?

Kish: First by the doctors, because I didn't know statistics; then on my own.

Frankel: Were they doing medical experiments? Were they doing tests?

Kish: Very good question. This was the laboratory of Wade Hampton Brown, and I was working with Paul Rosahn and Harry Greene, both young doctors, interested in tumors and syphilis. They were breeding rabbits with different susceptibilities to tumors, so we were getting into genetics, and I was beginning to work with samples. We were looking at the differences in variation among the rabbits, and we would do some correlations, analysis of variance and chi-square tests with regard to their resistance, say, to tumors. We also weighed their organs and I did some delicate weighing of, for example, the thyroids of rabbits, which are very small. So I also got some laboratory experience in statistics, measuring the variations of these organs. There was analysis of variance: Were their differences statistically significant? And what did that mean?

I'll just say two things about the Rockefeller Institute that are important. One was that Paul Rosahn was very nice to me, especially when we discovered that I could do the statistics better than the doctors because I had some math background and a feel for them. Rosahn and I even wrote two papers (Rosahn, 1935, 1937) together to explain analysis of variance and the chi-square test to doctors to whom they were new techniques in the 1930s. Unfortunately, back then at the Rockefeller Institute, only M.D.s were allowed to author papers—perhaps a Ph.D.—but certainly not a $90 per month laboratory assistant. No way. Rosahn, the author, was really upset about this. Anyway, I got that far.

King: Had you heard then of Jerzy Neyman and Egon Pearson and their work?

Kish: No. Neither Neyman nor Egon Pearson—they came later. I had heard about Karl Pearson—even about the fight between him and Fisher, and I read his *Grammar of Science* (Pearson, 1892) along with some other books. I also read some genetics.

King: Isn't it correct, though, to characterize your studies as a bit of a hodgepodge approach? You were not taking formal courses in statistics.

Kish: There were no courses in statistics at night at City College. By the time I got to taking a course in statistics at City College and one in probability at Hunter College I had already learned a lot of the material that was covered on my own. Remember, I had to take courses at night, except in the summer when I received permission to use my vacation time in small pieces to go to day school.

Frankel: Just to focus on this, you were working at the Rockefeller Institute while you were going to night high school. At some point you graduated from night high school and then you enrolled in college?

Kish: Right. I went to night high school from 1927 to 1930, the last two months while working at the Rockefeller Institute. Much to my surprise I was given the gold medal for best grades in the Bay Ridge Evening High School 1930 graduating class. It came as a complete surprise. In fact, all of my honors have come as surprises, including this interview. I got the medal and then started in City College, uptown, and we moved up there. My family was willing to do that so I could go home from work, eat and then go to college at 7 p.m. When I went to high school, I didn't have college in mind. I just needed the high school diploma. When I finished, I said well, that's not enough. So I went on to college, and it was like that with all of my higher degrees.

Frankel: Was it difficult to get into City College in the thirties?

Kish: No. I could even have had a "Regents' scholarship" if I didn't have a family, because I did very well on the New York Regents' Exam, but I helped to support three younger children and my mother. My mother and one of my sisters also worked. At City College I was interested first in engineering, and I liked math and physics. Remember, this was 1930–1931, the Depression. The guys who graduated in physics had no job of any kind, so I would try one thing after another—physics, then mathematics, also some chemistry. At that time I was not very interested in biology because it wasn't exact enough, and the Rockefeller Institute didn't offer any inducement, as they should have. Let me say one thing that is revealing: Here I was making statistical computations for these very smart young doctors so I wrote a memo to the Rockefeller Institute that said: Look, you have Mr. Hoff, the glassblower, Mr. Darling, the machinist, Ms. Mendelson, the medical illustrator, none of them medical doctors, and each has a shop. You could set me up as a statistical consultant and I could do it for you cheaper and better than the doctors. I think it was a very good proposal because I was earning less than $100 per month. I don't know if anybody ever read my memo, but nothing came of it. I didn't even have a college degree. I was still at least two years away from that. Yet, it still looks like a good idea, even now.

King: You were about 25 years old?

Kish: Yes. I am relating this as a lesson, because this notion of using consultants in statistics and in sampling reappears throughout my career. I just wrote a letter for *Science* (Kish, 1994b) saying that this is what you need for better clinical trials. You need specialists because it's not enough to give every doctor a course in statistics. So my early notion remains a good idea. Eventually, I quit the Rockefeller Institute to go to Spain. Today, of course, Rockefeller University has excellent statisticians.

SPAIN 1937–1939

Frankel: You graduated from City College in 1939? Before that, in the middle of your course work you left the Rockefeller Institute and went to Spain?

Kish: Correct. In January 1937 I went to Spain. I was there for two years.

King: Wasn't your decision to leave mainly for ideological reasons?

Kish: I went there because Hitler announced his plan to conquer Europe and the world, and I just felt something had to be done about it. I told a friend that I wished I could do something, but I don't know anything about weapons, I never shot anything in my life, never held a rifle. She said, "You don't have to, they teach it to you, if you really want to go." I said, "I do." And she said, "Well, you have to wait

FIG. 2. *Kish in Spain, spring 1938.*

at least three weeks to think it over," which I did and then I went. It had to do with the advance of Hitler and fascism. Mussolini had already won his war against Ethiopia. So I was an idealistic fighter against fascism—or farsighted. The FBI called us "premature antifascists."

Frankel: That's when you learned to speak Spanish, isn't it?

Kish: Yes, I learned Spanish and French at the same time. Spanish better. The official language in the International Brigades was French, so I learned the two together.

King: Didn't you tell me that you were in a Hungarian brigade?

Kish: Well, I joined them for romantic reasons. I met some Hungarians in a bar who told me, "You don't have to go through basic training. You go right to the front, they train you with the rifles and you start shooting. And also we have the best cooks." Actually, I had been assigned to work in a hospital because I had worked for the Rockefeller Institute. I was 26 years old and I wanted to fight, so I joined the Hungarians—that's the way things were then. I was with them for six months until I was shot on June 12, 1936 (the day after I met Dr. Norman Bethune). I was in the hospital for a short while and then transferred to an Anglo-Canadian-American artillery unit called The John Brown Battery. I was with them for more than a year until the Spanish Republic retired us.

King: Tell us a little about Bethune.

Kish: He was a Canadian there with his blood transfusion unit. He invented the idea that you could bring blood in cans right to the front, on the battlefield. Before that, half of those who were shot would bleed to death before they could get to a hospital where they could lie next to a donor. Bethune invented the canned blood idea. He was there with his driver–photographer and saw me playing with some village kids. They liked it and took pictures of me for a movie that I never saw. If you ever see the movie called "Bethune," you'll see this driver. The next day there was a battle. I could go through all the details but I won't. I can play the whole thing back in my mind like a documentary film. It was a very colorful day.

King: So after you recovered from your wound, you were transferred to another unit. When did you return to the U.S.?

Kish: In January 1939.

King: Had the Loyalists collapsed by then?

Kish: They finally collapsed in April, but the International Brigades were retired in 1938. I was in the very last shipment because I was in an outfit that was cut off from France, from the Lincoln Brigade and the International Brigades. I was with a Spanish division down south so I was in the very last unit to return on the *S.S. President Harding.*

King: What was the political climate in the United States at that time? Did you have difficulty getting back into the United States?

Kish: No, the government let its citizens come home from Spain. I was a citizen for only six months when I went to Spain in 1937, and my heart was in my throat about volunteering because it was against the announced policy of the United States. Remember, the U.S. was officially against intervention, but we knew that many prominent persons—Eleanor Roosevelt, for example—were on our side. We now know from reading the autobiography of Ambassador Paul Bowers (1954) that President Roosevelt eventually had the same sentiments. Bowers quotes Roosevelt, himself, as having said "It's the only mistake I know I made." So the country was deeply split, as was the *New York Times,* which had two correspondents covering Spain at the same time—Herbert Matthews, who was pro-republic, and another one, who was pro-fascist.

THE U.S. BUREAU OF THE CENSUS AND THE DEPARTMENT OF AGRICULTURE 1940–1942

King: Leslie, how did it come about that you went to work at the Bureau of the Census in 1940?

Kish: When I returned from Spain in January 1939, I went to City College immediately and asked if I could finish the degree requirements by June. They looked at my record and it showed that I had more than enough credits, but just needed some specific classes. I finished in June and even was elected to Phi Beta Kappa—a rare honor at night school. I also took two statistician exams. One for a federal statistical position for the 1940 Census and the other for New York City, which was a little harder and better paying.

King: Did those exams involve statistics as we know it today or were they primarily just tests of mathematical reasoning?

Kish: The best preparation for most statistical exams was to read Herbert Arkin's (1938) statistics outline. He taught at City College and was a good teacher, but by the time I took his course I really knew the material because I had already read his book. That was my preparation, plus a probability book by Arne Fisher (1915), not R. A. Fisher. I took those two exams while holding various jobs, such as a lifeguard at a summer camp, but no real job. This was still 1939, right? Meanwhile the world was getting into the big war: Germany had invaded Poland and Hitler was marching on. I passed both

exams with high scores and got both offers. I decided to take the one in Washington and went there on a train, on April 1, 1940. On that train were Joe Waksberg, Joe Steinberg, Sam Greenhouse, Marvin Schneiderman—all of whom became very good statisticians.

King: You were all on the same train?

Kish: Yes, because we had all passed the exam with top marks, and got the same offer: to start a couple of weeks before the other hundreds of new recruits. The group I just mentioned had been day students at City College and knew each other; I was the only night student. We all went down to the Census Bureau together and became friends. That same summer we talked Bill Madow into giving us a course so we wouldn't "waste" the whole summer without taking courses. I was always taking courses.

King: Who were the leading lights at the Census then?

Kish: Philip Hauser was the senior person, and number two was Ed Deming. Number three, Hansen and Hurwitz, almost one word: Morris Hansen and Bill Hurwitz. We soon recognized that the natural leader of this group in sampling was Hansen. Although he was not the senior statistician, he stood out from the rest. I was assigned to the Census of Agriculture and worked there for a year. We started at $1440 a year, but since we were at the top of the civil service list, we were raised to $1620 by the end of the first month. It was $1620 for the whole year, not per day. So I worked as Chief of Section Two, and then, one day in the spring of 1941, somebody told me about a job that would be a big advancement to Statistician, Grade One. It was a professional and permanent job with the Division of Program Surveys in the Department of Agriculture at $2000 a year. I had an interview with Rensis Likert (after whom the Likert scale is named). That was a fateful day in my life. Likert asked me "How would you like to be our sampler." I said "Well, I don't know anything about sampling." He said "Would you like to learn"? I answered "Yes." He said "All right, more power to you." So I started reading and took a course that fall from Steve Stock, Lester Frankel and Willie Cobb in the U.S. Department of Agriculture Graduate School. I learned later that it was the second course in sampling in the world; the first one was given in the spring of 1939 by Cochran at Iowa State. Cochran was the torch bearer from Yates at Rothamsted.

The basis in that USDA course was studying Neyman's paper on sampling (Neyman, 1934) and his *Lectures* (1939). There were other good things—studying the unemployment surveys and Frankel and Stock's (1942) paper on a Latin square design for selecting counties. Remember there was no survey sampling at the Census at that time—not until 1943, when Lester Frankel brought the sampling design for the Unemployment Survey over to the Bureau in his briefcase.

Frankel: You just mentioned that the Department of Labor was really the place where sampling started in the United States for survey work. The Census had done some samples, but why do you think sampling for surveys started outside the Census Bureau?

Kish: No, the unemployment surveys were not at the Labor Department. They were at the Federal Works Progress Administration, the WPA. I was the first sampling statistician hired by the U.S. government because Stock, Frankel, Frederick Stephan and Ross Eckler were working at the WPA. It was separate, not part of the Federal Government. These statisticians and some very good other people were working for the WPA on this vast sample of the United States. The Census Bureau had only sampled the census schedules in 1940—you know, taking every fifth household. They didn't really have a survey sample in the proper sense.

King: So let's get this straight, this WPA survey was the first large-scale sample survey in the world?

Kish: There had been surveys in India, England, the U.S. and elsewhere, but the Unemployment Survey was the first national probability sample of households in the United States. At the Division of Program Surveys, under Likert, we did surveys that were stimulated and supported by Elmo Wilson and Henry Wallace, the Secretary of Agriculture. Wallace said that if we have all these agricultural programs, we ought to have an agency independent of any of the programs to survey those programs. The idea of an independent survey agency was a vital new concept for evaluations, so our division took samples to survey the ongoing work and reported directly to Wilson and Wallace. At first I selected quota samples of farmers and I could entertain you for an hour with what's wrong with quota samples—I learned that in the first few months.

Then came Pearl Harbor. I was in Greenville, North Carolina and heard about it on my car radio tuned to the New York Philharmonic. When I got back to Washington my division was expanded from agricultural surveys to population surveys. I was at grade P1 and there were also jobs for a P2 and for a P4 in sampling. I knew I could go from P1 to P2 but not to the P4 grade, and I said "I know the guy you want for P4—my teacher, Steve Stock." Within a week, Likert took my advice and then Stock and I worked together. So I hired my own boss. Again, I am working with somebody who is good, learning

about Latin square designs, learning about how to sample properly from frames and listings. We went out and listed cities, blocks, dwellings and even people. That's how I got into sampling, from the time of Pearl Harbor until I fought my way into the Army in June, 1942. How I managed to get myself reclassified from 1B to 1A to join the Army is a funny, but too long story. I was still fighting Hitler. I was in the Army for three and a half years, mostly as a meteorologist, which I got into because I knew some physics and did well on the Army exams. I was attached to various Air Force stations in the U.S. and finally in Hawaii. They kept me from going further overseas, I suspect, because they pegged me as a "premature antifascist."

King: Did you make any use of your statistical knowledge while you were in the military?

Kish: No, but I made use of my physics. I had a Yule and Kendall (1937) in my knapsack all the time, however, and I read that over and over.

King: You must have recognized, though, some interesting statistical issues?

Kish: Oh yes. I was a little bashful about working in the social sciences, but I found that when it comes to difficult problems, meteorology was behind the social sciences. Meteorology has only to deal with 4 or 5 dimensions—3 for space, plus temperature and humidity—while in the social sciences we begin with about 12 and go on from there. So I say that the big difference between physics and the social sciences is the number of dimensions in multivariate problems in the social sciences. I thought about statistics sometimes, predicting the weather, doing risk analysis. In addition to the Yule and Kendall in my knapsack, I was probably reading Fisher (1925) once more. I neglected to mention his *Design of Experiments* (Fisher, 1935), a very important book.

OTHER TEACHERS AND MENTORS

Frankel: Who were your most impressive teachers or mentors?

Kish: As I said, I learned mostly from books on my own, but in statistics my best experiences were at the USDA Graduate School. I particularly remember the courses and contacts I had with Ed Deming. I learned a great deal from him.

Frankel: What kinds of things did you learn from Deming?

Kish: A great deal of scientific philosophy. Deming had an unusual ability to get hold of somebody's important idea and develop it. He invited Neyman to this country to give his famous lectures at the USDA and Deming adopted his philosophy. I learned some quality control by reading Shewhart (1939) on my own. I also must mention Morris Hansen as an important influence. I took two courses with Hansen and Hurwitz, first in 1942 before I went in the Army, and another after I came out in 1945. They were using dittoed notes that ultimately went into their famous textbook, Hansen, Hurwitz, and Madow (1953). And, oh yes, I must not forget Ben Tepping. He already had his doctorate from Ohio State before he entered the Census in 1940 and he was a very good teacher.

BACK AT THE DIVISION OF PROGRAM SURVEYS

Kish: When I came out of the Army in October 1945 I went back to the Division of Program Surveys. Somebody had my job—Beth Morrell—but she graciously gave it up to go to the Census Bureau, without my using my veteran's rights. During the war, at the Division of Program Surveys, Ray Jessen had succeeded Steve Stock, and now Earl Houseman was in charge of sampling. He came from Iowa State. My work with Earl was pleasant and fruitful, and I learned much from him also. He was already involved in policy and let me have a lot of freedom to practice sampling. During those months from October 1945 until I started at Michigan in March 1947 I had three new ideas—you'll recognize all three of them. First, although we had a good sample of households, adults were selected within the households by quota sampling, so I worked up a method for selecting a single adult with equal probability.

King: This is what is now known as the Kish table?

Kish: Yes, the "Kish method" or "Kish table."

King: Which many people think is some kind of delicatessen food (laughter).

Kish: It was just a memo. A couple of years later, Angus Campbell urged me that it was worth a paper, my first (Kish, 1949). Afterwards I decided that if you don't want your own name attached to a disease or to a table, give it a good technical name. Every invention I've had since then I've given a good name like "balanced repeated replication" or "design effect." Now, just writing that memo about within-household selection wasn't the end of it. I had to convince the field section that quota sampling of respondents was not good enough and that they had to use this new table. The field people were not used to it and I had to get the backing of Likert and Campbell to get it adopted.

My second idea was, believe or not, counting nonresponse. If you get a nonresponse for a dwelling, you have to pose and answer the question, "Is this

just a vacant house or a refusal or what"? In other words, you need additional identifications and a classification of the various types of nonresponse. Now I was fighting with the field people, with Charlie Cannell and with George Katona. I had to run again to Campbell and Likert and say "If you want to have a probability sample, you have to do this." Nobody was reporting nonresponse properly on a regular basis—not the Census Bureau—nobody. The argument against it was that if we did it and the competition didn't, we would look bad because we couldn't get past 90% response, whereas the competition didn't have any nonresponse. You know how that sounds. But virtue and full reporting won in the end.

My third idea was that I noticed that the suburbs of the 12 largest metropolitan areas in the U.S. were growing fast. They already accounted for 10% of the population and I expected them to grow; they are now about 40-something percent of our population. The suburbs were so diverse in economic, racial and ethnic composition that I developed the idea for a balanced selection of suburbs, representing size, economics and distance from the city.

King: Was this before you met Roe Goodman?

Kish: Roe Goodman, who came to Michigan later on in 1947, said "Look, if we're going to do balancing, we have to figure out a way of doing it with probability sampling." This was his idea and also the name *controlled selection.* We worked and worked on it for a couple of months. I had done the balancing before, but it was not with strict probability sampling. [See Goodman and Kish, 1950.]

Frankel: These ideas were at the Division of Program Surveys before you came to Michigan?

Kish: Yes, all three of them.

THE UNIVERSITY OF MICHIGAN

Frankel: Why did the move to Michigan happen?

Kish: Because the kind of surveys that Rensis Likert and the group wanted to do—population surveys—did not conform with what was going on in Washington. The Truman administration was closing down anything beginning with the letter P: planning, programs and policy were out. Likert said that we ought to go to a university environment and persuaded the U of M to take us in. Campbell, Likert and Katona had Ph.D.'s. Charlie Cannell, I and others did not when we all moved to Michigan. We were soon joined by some others, including Roe Goodman as my Chief, to establish the Survey Research Center (SRC) in Ann Arbor.

King: Tell us about some of the surveys that SRC was doing in those days?

Kish: We were doing the Surveys of Consumer Finances for the Federal Reserve Board and a few smaller surveys for the State Department on attitudes toward the nuclear tests at Bikini Atoll and the like. The nuclear test surveys began in October 1946, and for these we took national samples of only 600. I am very proud of that, perhaps the smallest national probability samples of households anybody ever selected. One of those samples afterwards became the famous 1948 sample that happened to predict Truman's election victory over Dewey. I said "happened" because, out of the total sample of 600 adults, only about 400 were voters.

King: Was this a full probability sample down to the level of the selection of the respondents?

Kish: Yes. We already had moved to Michigan and had carried with us some sampling materials that the USDA allowed us to take. We brought maps, listings and other items. So in 1948, for example, the election study was based on the same sample that had been used at USDA. We never could have done an election study in a government agency. That's just one example, and there are other examples, which is why we moved to a university. The University of Michigan gave us permission to use its name and gave us about five rooms in a basement, which we painted in as light a color as possible. Likert and Campbell received joint appointments, and Michigan gave us one more important thing: they allowed us to keep our overhead, and from that decision came our great growth. We took a big risk, but also began the most interesting part of the adventure. The Survey Research Center later became our Institute of Social Research.

King: When you were doing these surveys for government agencies, were you competing with other social research organizations in the U.S.? We're talking about 1948 or 1950?

Kish: I would say that when we were part of the government we had little competition. It was more a question of competing for attention to see if the survey could be done. Nobody else was bidding for those surveys. They would come and ask us "Would you do this for us"? Or else Likert would approach them and say "This is something we could do."

Frankel: Let's talk a bit more about 1948. That study wasn't really planned to predict the election. It just happened to include a question about that. Was the response tabulated before the election? I recall hearing that after all the other polls failed, it was only then discovered that the study had been done.

Kish: True. It didn't appear in the papers as a prediction, and most of us tried to play it down because with a sample of only 400 voters, even if you

disregard the design effect, two standard errors is five percentage points.

Frankel: Let's shift back to your situation at the University of Michigan now. You decided to continue your education and get a doctorate there?

Kish: But one thing at a time. First, I felt I needed a master's degree, which I got in mathematics a year later, in 1948.

King: Who were your teachers then?

Kish: There were several, but let me single out Paul Dwyer, from whom I took more than one course. He was a very good teacher. I learned a lot about computing, linear models and that sort of thing at the Statistics Lab, headed by Dwyer and C. C. Craig.

Frankel: There was no Department of Statistics, right? Paul Dwyer was a statistician in the department of mathematics, so your degree was in mathematics.

Kish: Yes. By 1948 I had my master's degree and I was very happy—I loved my job. Even today, I find sampling full of challenges because I have always liked solving puzzles. Every sampling problem is a puzzle for me, figuring out how to do it right. Especially, if it is something that no one has done before.

King: Could you talk a little bit about some of the differences in the "nuts and bolts" of doing surveys and processing the results back in, say, the late 1940s and 1950s, compared to the present day?

Kish: Originally, in the 1940s, we still used those easy sort cards, then we had punchcards and finally we got some better machines, but I still learned from Dwyer how to compute correlations and invert matrices on a Marchant, a Burroughs or a Monroe. The first time I calculated a sampling error was with Earl Houseman at Agriculture and I did more here at Michigan using a Monroe calculator.

King: When was the first computer installed at the University of Michigan and when was it available to SRC?

Kish: Real computers? Somewhere around 1960. Our first use in sampling was when John Sonquist and I worked out a program for sampling errors and design effects. This was before *SEPP: Sampling Error Programs Package* by Kish, Frankel and Van Eck (1972). That was the first book on the subject, but I had already worked out for Sonquist that the necessary and sufficient ingredients were the covariance matrices for all primary selections, for ultimate clusters. The Census Bureau was computing sampling errors before us, but not many others were computing them in those days—maybe nobody else.

When Marty Frankel's thesis (Frankel, 1971) on balanced repeated replication was completed in 1971, he was one of the pioneers in computer studies of sampling. I would say that the computing of valid sampling errors, in general, started somewhere after the early 1970s. So Marty was at the leading edge.

FIG. 3. *Leslie Kish at Ann Arbor circa 1960.*

King: One point I want to make is that many students in the social sciences at Michigan and elsewhere about that time started to use statistical packages like SPSS or OSIRIS, and they were totally ignorant of the fact that the data they were analyzing had not been gathered by simple random sampling and that therefore the canned standard error calculations were inappropriate.

Frankel: They also knew nothing about weighting and were unaware of design effects.

Kish: Most of them still are.

King: As far as I know, none of these packages routinely enables you to adjust for the fact that the data were gathered in clusters rather than by simple random sampling. That is very slow in coming. I think that more and more students are now aware that something ought to be done to adjust the au-

tomatically produced standard errors, especially if they've had a course in survey sampling, but they still may not be sure of what to do, so they leave them alone.

Kish: I lectured on that again last week, but my listeners were a self-selected group who came because they wanted to learn about these things. For the most part, the situation is only changing slowly over the years. I have some simple advice that will be published soon (Kish, Frankel, Verma, and Kaciroté, 1995).

Frankel: Let's finish up. You told us about your master's. What about getting a Ph.D.?

Kish: I decided, with reluctance, that a master's is not enough. I was already 38 years old and although I liked going to school, I had three other jobs. In 1948, my chief was Roe Goodman. Within a year or two he started a graduate course in sampling. We also started the Summer Institute in Survey Research. Goodman then decided to leave and that made me, simultaneously, number one, the designer of samples for the Survey Research Center—that was my main job; number two, a researcher, and I had just started to write articles; number three, a graduate student; number four, I was a graduate teacher—four jobs, all at the same time. And a fifth one, I became the father of Carla and a reasonably good husband. But I worked at the four jobs simultaneously for four years. How I did it, I have no idea.

King: What was your rank at the University, did you have a position like instructor or assistant professor?

Kish: I was a Lecturer in the Department of Sociology when I got my Ph.D., in 1952. They offered me an assistant professorship, but I said that at the age of 42 I was not going to start as an assistant professor, I'd rather just remain a lecturer. So they made me an associate professor; Sociology was generous and far-sighted.

THE STATUS OF SAMPLING IN STATISTICS

Frankel: Your Ph.D. was in sociology, not in mathematics. Why was that?

Kish: I continued in mathematics after my M.A. for a year or two. During that time, and while still working on my master's, I learned from some great statistics books. Cramér's book (1946), for example, was very important for me, and also there was Snedecor (1937). I studied the two big volumes of Kendall's *Advanced Theory* (1945), but most influential were the journals *JASA,* from cover to cover for more than 10 years, some of the *Annals* and articles in *Biometrics*.

Two things happened, however, that caused me to leave mathematics. I knew I wanted to be a sampler and I knew what that involved. First, I was being taught useless things about estimation theory—like best linear unbiasedness. After I had enough of that and similar optimality results that have little to do with statistics as it is practiced, I said "What am I doing in these courses? This is useless for me." The second thing is that I tried to combine sociology and mathematics, but the Mathematics Department would only permit this if I did the full mathematics program. I decided this was not for me. I had those four jobs and I wanted to finish my degree in a couple of years, so I went into sociology. I think that these requirements by the Mathematics Department were wrong. The same thing happened with Marty Frankel and several other excellent students. Even today this is true with many or most statistics departments. So if students want to be practical samplers they go to a biostatistics department instead of a statistics department.

How could I manage four jobs—sampling consultant, researcher, teacher and student? The answer is with wonderful co-workers. First, there was Jane Williams, who left and became Dr. Jane Bergsten. Then Irene Hess came from the Census Bureau in 1954 and we became collaborators for 30 years. She worked with superefficiency for about 27 hours *every* day and on hundreds of survey samples. Irene and I wrote several papers together. We also had some wonderful graduate students working part time who became researchers with M.A.'s and a few with Ph.D.'s who became professors—some of them famous. I learned a great deal from working with them.

In the Department of Sociology, Ronald Freedman and Amos Hawley were both great teachers and great friends. From Freedman I learned population and demography and from Hawley, the chair of my doctorate, ecology and social philosophy. Ted Newcomb and Dan Katz taught me social psychology, and at the Survey Research Center I was working with Ren Likert, Angus Campbell, George Katona, John Lansing, Jim Morgan, Bob Kahn and others.

Was I sad to leave mathematics? Oh, yes. Mathematics is the most noble creation of the human mind. She is so seductive: The more math you have, the more you crave, (Kish's law), but I felt that I would not be a great pure mathematician—applied statistics is more interesting to me than mathematical statistics—and sample designs pose new puzzles for me every week.

Frankel: Let's talk about your teaching. When was the first time you taught sampling at the University of Michigan?

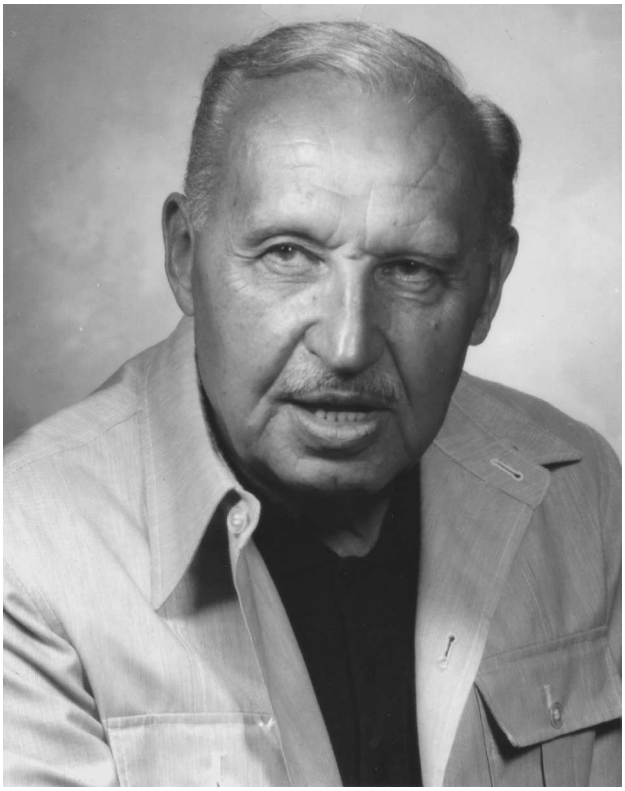

FIG. 4. *Leslie Kish at Ann Arbor circa 1970.*

Kish: 1949 or 1950. Roe Goodman had already departed on his missions to developing countries and I was left to teach the courses he had started. Psychology and sociology were the only departments willing to sponsor the sampling courses in the beginning. I taught sampling from 1949 until I retired in 1981.

Frankel: Let me get this straight. The first course in sampling was taught at the University of Michigan in 1949. You couldn't take it as a student studying statistics in the Math Department. You could take it as a sociology course or psychology course. Did the Statistics Department ever cross-list a course in sampling?

Kish: Much later. A few students came to my course from Statistics, but most of my students over the years came from Biostatistics. Also many were from Sociology and they also came from Political Science, Economics, Business, Education and from other disciplines, especially in the summer.

Frankel: So you've trained a lot of people in sampling as students. We'll talk about some of your Ph.D. students, but for the moment, let's discuss the training program that you put together in sampling statistics. Can you tell us something about that: when it started and how many students you trained?

Kish: The summer program was started by Campbell and Likert one year after we arrived in 1948; Ben Tepping from the Census Bureau and Roe Goodman taught sampling. I started teaching the sampling in 1949 and it's been continuing ever since. The most recent course just ended today. I started my Summer Program for Foreign Statisticians formally in 1961, continuing until I retired in 1981, and it still goes on. I asked myself in 1960: "How can I do good for the world in my profession"? I managed to get some money for one year from the Stern Family Fund and, later on, three five-year grants from the Ford Foundation that carried me through. I used my grants as a "pump primer" and I managed to get most of the fellowships from places like the United Nations, the Rockefeller and Carnegie Foundations, but also from my own funds. Graduates of that program are all over the world, and I see many of them at the biennial ISI meetings.

Frankel: Do you have any idea how many students have studied sampling at Michigan and how many of them ended up working in the field?

Kish: We have had representatives from 94 countries covering all continents, including the latest ones: Albania, Uzbekistan and Slovenia. I would say a total of about 400 students have taken this sampling course over the years. A number of them became directors of statistical offices or work in government departments or agencies. Some joined universities and have continued to do research and write articles. Also, many are members of the International Association of Survey Statisticians.

King: Tell us some more about your students.

Kish: One of the things my course did very successfully was to recruit samplers, including Bob Groves, Steve Herringa, Jim Lepkowski and Marty Frankel earlier, who combined graduate courses with working in our sampling section and later also teaching. But don't give me false credit for "developing" these excellent students. I had to cajole them into sampling, because they also had other talents and opportunities.

Frankel: Can you tell us about some of the samples you've set up around the world?

Kish: I can't give you a complete list, but some of my favorites were in Lima, Peru, Quito, Ecuador, Santiago, Chile—others in South America. Also, later, in Taiwan, Korea, Hong Kong, Thailand, Malaysia and China. My first three foreign projects were in Puerto Rico in 1951–1954.

Frankel: Didn't you help set up the first labor force study in Peru where you stratified initially by size and then by altitude?

Kish: Yes. As Tukey says, "An expert works with other people's brains." Somebody raised the question "What about people living below 10,000 and above 10,000 feet," and I, as an American or European, replied "Nobody lives above 10,000 feet." In Peru, however, more than half the people live in the Altiplano above 10,000 feet and this is an important stratification variable. The same thing is true in Ecuador. We established a sample there in the Peruvian Labor Department which is still going on as a national survey. Indeed, some of the people I worked with are still there.

Frankel: I want to ask you now about your book, *Survey Sampling* (1965).

Kish: Let me begin by saying that I was one of the first people to teach from Yates, (1949), Deming (1950), Cochran (1953), Hansen, Hurwitz and Madow (1953) and the first P. V. Sukhatme (1954). In fact, I taught from the galley proofs for Cochran and Hansen, Hurwitz and Madow. I know these books well because there's nothing like teaching from a book to learn it. They all have different merits, and perhaps Yates (1949) is the best, but for students it's too tough, like Fisher. So I was teaching from all those books right after they came out. I also used Deming's later book, *Sample Design for Business Research* (1960), and M. N. Murthy (1967). As I was teaching I kept putting out my own dittoed notes because some things were not adequately covered. I remember, in Puerto Rico in the 1950s, writing some notes on stratification. Eventually, somebody from Wiley came and asked me to do a book with them. At first, I said "I haven't got time," but he talked me into it. It took me about 10 years from the time I started in 1955 to the time it came out in 1965. It is now available as a Wiley Classic, in paperback, which means that the price will be half and my royalties one-fourth of what they were per book, but I am happy and flattered about all this.

While working on the book I was teaching graduate courses at Michigan in sampling—two courses: one in methods and one in advanced methods and theory—and also writing articles. I would like to add that whenever I developed an innovation, I gave it a name. For example, deff for *design effect*, BRR for *balanced repeated replication* and JRR, for *jackknife repeated replication,* the latter suggested by Tukey at a meeting. Also there is SPREE, for *small area estimation, non-coverage* and now, lately, I am pushing four things: *rolling samples, multinational survey design, multipurpose sampling* and SPD for *split-panel design.* I will lead a session on multinational survey design in Beijing in 1995. Like for the World Fertility Surveys, which were multinational, you design them ahead of time for the various national efforts to be comparable. It basically means that you must make the measurements similar, but the samples can be suited to the local circumstances, provided they are all probability samples.

Frankel: What languages has your book been translated into—in addition to the bootleg copies in Taiwan and elsewhere?

Kish: I have a copy in Spanish. I know it exists in Chinese but I haven't yet gotten a copy of that. There were parts in Serbo-Croatian and some notes in Russian, but I have never seen them.

King: How did you learn about linear models?

Kish: I had a linear algebra course at the USDA in 1946 and linear models were covered later in my Ph.D. program.

King: I mention this because I think that some people—nonstatisticians—don't fully understand the relationship between concepts like stratification and linear regression.

Kish: I feel strongly that a graduate program ought to deal with that. I helped to develop a program for Biostatistics that combined sampling and experimental design because they belong together.

I regret the fact that my course in sampling was never in the Statistics Department. Survey sampling is seldom found in statistics departments. You may have a course on the theory of sampling and on "fundamentals," but that is different from survey sampling. By the way, here is a bit of my philosophy: *fundamental* is the wrong word. I would say that the right word for a discipline is *roots,* because a discipline needs an organic basis that grows down into its foundation at the same time that it grows upward into applications. *Roots* are different from the solid foundation that you lay down like concrete before you can build an edifice. Real disciplines—statistics, physics, biology—grow like organic things. They keep on growing down into the theory and at the same time, they develop branches and leaves grow up toward the light—toward applications. This is a personal view of mine.

Should survey sampling belong to statistics? My answer is definitely yes, because it doesn't belong to any other subject and because statistics is incomplete without sampling. Sampling needs statistics and statistics needs sampling. As I have said time and time again, even in my ASA Presidential Address (Kish, 1978), survey sampling involves statistical issues that are applicable to every area of science. Thus statistical design, sampling design, experimental design—these topics belong to statistics. They belong nowhere else.

FIG. 5. *President of the American Statistical Association, Chicago, 1977, with wife Rhea and daughters Andrea and Carla.*

King: To be fair though, don't you think there are a number of universities where courses are taught with names that imply something to do with surveys and sampling, but which focus almost exclusively on questionnaires and interviewing techniques? These courses really don't have enough statistical theory in them and thereby may lead to bad statistical methodology.

Kish: Yes, I agree. I like and favor courses on *survey methods,* especially on questionnaires and interviewing methods, but I believe that they belong to social science departments. I believe, however, that *statistical design,* as in the title of my book published by Wiley, *Statistical Design for Research* (Kish, 1987), belongs in the statistics departments. It should be there, and a few universities teach it. But *survey design* is not the same as *sample design.* The design of samples is a topic that is "portable" to any discipline, to any country and any field. Survey design—how to ask questions, how to make measurements—belongs to a specific discipline because measurements are different in chemistry, for example, than in social research, health, physics or geology, and they are different in the United States than they are in China. So measurement issues belong somewhere else, not in statistics departments, but sample design does belong there.

King: I think that things have changed in recent years and now many departments of statistics recognize that sample design should be an integral part of the curriculum. I worry, however, about the treatment being too mechanical, especially if the teacher does not have much real sampling experience.

Kish: I am very much for teaching theory and methods, but I believe that theory and methods and philosophy and mathematics are all different things. Mathematics is one of the most beautiful creations of the human mind and it is necessary for probability. Probability is necessary for teaching statistics, but it's not sufficient—you need something else. The main problem is that whereas statistical *analysis* can be made mathematical, it is very difficult to make statistical design, experimental design or survey sample design mathematical. I don't know how much of it could be made more mathematical, but that's a real challenge, and current books don't meet that challenge. They teach formulas without really teaching design.

IMPORTANT SURVEYS IN KISH'S CAREER

King: Please talk about some of the important surveys you've been involved with and their impact on research and decision making.

Kish: Because we were early in the survey business—five years earlier than others at that time—we got into all kinds of interesting things, and we were often the first to do them. I already talked about very small national samples of at-

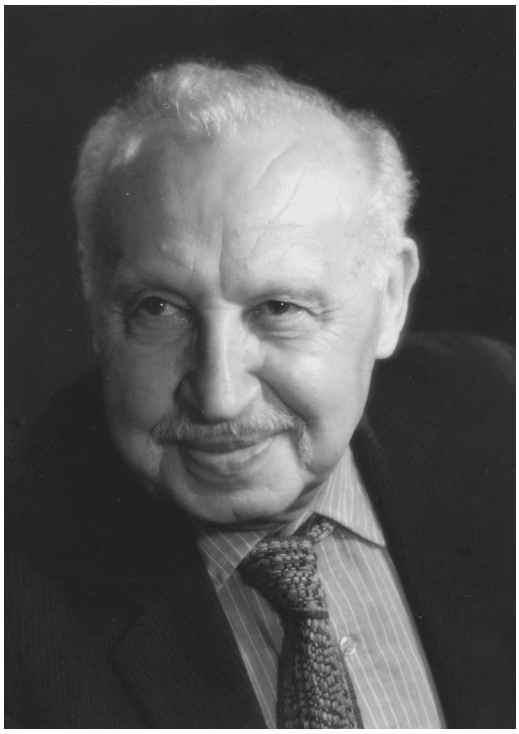

FIG. 6. *Beijing, 1985. Lecturing for the National Academy of Sciences.*

titudes. I am very proud that we could do good national samples of the U.S. of size 600 in 1946.

At ISR we also had the first national study of fertility behavior in the USA—The Growth of the American Family—in 1955. There was a lot of discussion and uncertainty beforehand: Would this be possible? Can you ask people about fertility practices? Can you get it passed? We had a $2 \times 2 \times 2$ Latin square design for four counties where we tried it out first to see if we could get responses. That survey was one of our greater successes. Surprisingly, we achieved considerably better response than we had for some economic data. Our biggest surveys, 3000 households, were the Surveys of Liquid Assets, later called the Surveys of Consumer Finances. These were annual, large, complex, deep and interesting—especially interesting because we found out we could get good data on liquid assets, but that we couldn't get good measures of cash on hand. When we took our survey results, expanded them and compared with benchmarks, we found that we were 50% short on the cash. Too many people apparently kept money in their mattresses, so we finally abandoned the effort.

King: These were the famous Surveys of Consumer Finances. When were they started?

Kish: We started them in the Division of Program Surveys in 1946. They were directed by George Katona, for the Federal Reserve Board. (See Kish, Lansing, Dent and Katona, 1950.)

King: These are different from the consumer expectations surveys, aren't they?

Kish: Right. The two got connected later, but originally they were different. The one for the Federal Reserve Board was our biggest survey. We did 3000 yearly interviews, nationally, and it was our biggest survey resource, intellectually and financially. Later Katona started the famous small monthly surveys of consumer intentions, expectations and attitudes that are still going today by telephone.

King: I don't think there was any survey at that time or even now that has had more of an impact on policy making than the *Survey of Consumer Finances*.

Kish: I remember that Homer Jones was one of the persons at the Federal Reserve Board who started it. Other very interesting people were also connected with it. It was a lot of trouble in the beginning, but it was worthwhile. For example, we had to persuade the Board to drop a contractual requirement of *exactly* 3000 interviews. In those days, the clients did not understand that in complex probability sampling, the number of completed interviews cannot be known exactly in advance, but is a random variable. Since then, I have been lecturing "Don't ever guarantee a fixed sample size."

We also were the first to do national samples of elementary schools for the Fitness of American Youth (not very fit, alas!). Then there were samples of high schools for the Boy Scouts and the Girl Scouts. Such surveys were difficult in those days because the U.S. did not have the good national lists of schools we have today and because it was necessary to cluster the interviews into the primary sampling units where our national field staff was already located. There were also some other surveys of business establishments that presented tricky problems of sample design.

CONTRIBUTIONS TO SURVEY SAMPLE DESIGN

King: Let's continue by talking about your most important contributions to sampling design.

Kish: I believe that the ideas on *multipurpose designs* and *optima and proxima* for them should be useful, and that the idea of a *design effect* is very useful. This is an expression of the effect of the complexity in the sample design on the variances (or standard errors) of estimators. It can be applied to simple estimators like the mean or to more complicated estimators such as regression coefficients. The Frankel thesis (1971) and the Kish and Frankel paper in *JRSS* (1974) were important

contributions. Frankel and I are working now on another related paper. There are several papers written on the proper computation of sampling errors, and this current work continues along that line. In general, for decades I have been promoting methods for proper computations of sampling errors and design effects for complex (analytical) statistics and for complex (clustered) survey samples. I was careful to invent the neologism "design effect" so that it wouldn't end up as the "Kish effect."

King: I hope you don't go as far as the factor analysts, however—Varimax, Oblimax, Oblimin and the like.

Kish: I tried to think of descriptive, not just catchy, names. Design effect is the effect of a sample design. It tells you what it does. The same for balanced repeated replication. That is exactly what it is. It repeats, that is, resamples the replicates, and it is balanced—hence, it is BRR.

King: Tukey must have coined the word *jackknife*?

Kish: Here is what happened. When I presented the paper (Kish and Frankel, 1970), Tukey was a discussant and one of the things he suggested was "Instead of using the complicated 'balanced repetition,' wouldn't it be enough just to leave out one member of the pair of PSU's from one stratum at each repetition and call it a jackknife." So there came *jackknife repeated replication* or JRR, first investigated in Frankel's thesis. Now, let me go on to the method I am pushing now, the *rolling sample.* I want to call it rolling sample or rolling census because instead of taking the sample or census all at once, as you go through successive periods, you roll the samples gradually over the whole population. The name gives the idea. I first used the word *rotating*, but there was a conflict of terminology with rotation design.

King: When I first heard that term, a dozen or so years ago, it immediately connoted rolling stock. The truck coming down the highway accumulating mileage, I think that's an apt metaphor.

Kish: Yes, thank you. It may take two words or three, but it gives you an idea of what makes this method different from the others. *Controlled selection* was Roe Goodman's name, and I would prefer myself *multiple stratification,* which is what I call it in my sampling book (Kish, 1965), because controlled selection is a broader term. There are many ways of controlling samples besides stratification balancing.

Let's see, what are some of the other papers? *Multipurpose design*. I have three papers with that title. You see, every sample is multipurpose. All samples get multiple use, but to *design* a sample to be multipurpose—that's a different idea and I am trying to promote it—to design a multipurpose sample in the first place. I actually wrote such a paper for the first time in *Econometrica* (Kish, 1961), one on multipurpose stratification in *JASA* (Kish and Anderson, 1978) and another in the *International Statistical Review* (Kish, 1988) on multipurpose allocation—so there is yet another name for you.

I am also pushing the idea of multipopulation sampling, and I have a paper on multipopulation survey design (Kish, 1994a). By this I mean something more general than the sampling of many nations that we were talking about before. Here I am referring to the problem that when you survey a country like the U.S., for example, you are also interested in many different populations. The U.S. is composed of various domains and cross-classes that you must take into account when you design the sample. This is an important and practical philosophical issue.

King: You know, that reminds me of some of the things that decision theorists talk about, such as utility functions of multiple argument. Have you ever discussed these things with your friend Howard Raiffa? Didn't you know him at Michigan?

Kish: Yes, we discussed that, and I know Howard well. We were students together at Michigan and got to know and like each other in 1948. We were both working for our master's and we were both dissatisfied with what was being done in the teaching of statistics. He went into mathematics and then into decision theory. I went into sociology and sampling, and we remain close friends to this day. I could talk about my arguments with the Bayesians like Howard Raiffa and Jimmy Savage. I pick very good people to have arguments with.

MODEL-BASED SAMPLING

Frankel: Do you want to talk a little bit about model-based sampling versus what's being called design-based sampling?

Kish: I don't have much to say that is new, but I discussed this in the first chapter of my *Statistical Design for Research* (Kish, 1987). Models can play strong roles in some problems in genetics and in some physical problems, but most often lesser roles in the social sciences. One cannot live entirely without models, however, because of nonresponse, frame problems and because one must always infer beyond the frame population. I tried to say this in that book and here I repeat a specific statement: Models should tell you what variables to put into the regression and, possibly, the functional forms

of the equations. Models will not tell you what the numerical values of the regression coefficients are. You have to compute those. My biggest arguments on these issues were with some great econometricians.

Let me also give a simple example from physics, the distance travelled by falling bodies: $d = \frac{1}{2}gt^2$. The $\frac{1}{2}$ and the t^2 come from a model. The g comes from measurements that vary over the surface of the Earth. You cannot get g from any model; it must come from measurements. The same thing applies to the coefficients in a linear regression. What the dependent variable y and the x's are should come from a model, but not the betas. Those coefficients will vary depending on where you take the measurements. The betas will be different in the U.S. in 1994 than they were in 1993 and they're different in the U.S. than they are in England or Russia in 1994. Furthermore, the estimates of the betas are subject to sampling variations and design effects. To measure them, you have to have the proper probability sample designs.

King: Well, let me just understand this. Are you saying, then, that it is not a good idea to think of "true betas" being generated themselves by some underlying superdistribution?

Kish: You can think of models, but don't embrace them. As Tukey says, "Embrace your data, not your models." There is no "universal beta" you can use now that generates betas for 1993 and 1994 and for the U.S. and the U.K. It would, however, be good econometrics to speculate about them and then test the hypotheses with empirical data. I know that what I am saying is controversial, but it is important. I am taking on the whole econometric and psychometric establishments in denying a universe in which parameters that must be estimated empirically are, themselves, independently and identically distributed.

King: Isn't this intimately related to the problem of the extent to which you can take any finite set of data and treat it as a sample from some universe? As you know, we often apply the traditional ways of talking about random samples to data that are complete censuses or just "chunks of opportunity."

STATISTICAL DESIGN VERSUS STATISTICAL ANALYSIS

Kish: Now we are at a very important point. How does statistical design differ from statistical analysis? The reason you need courses in statistical design is because the universe is not independently, identically distributed (i.i.d.), but complex. All universes—physical, chemical, social—none is a well-mixed urn. The second point is that because we are dealing with these complex universes in a complex way, we must select complex samples from these complex universes. Because we have design effects, we must have large samples. We live and die by the central limit theorem because of this complexity and because we must have roughly normal distributions for inferences based on statistics from complex samples. On the first page of the typical statistics textbook you read "Given n random variables drawn from a population, independently and identically distributed, to estimate $\bar{y}$, etc." Every word in that sentence is misleading. Samples are not given. They must be selected, assigned or captured, and the sample size is not fixed. In surveys, sample size is almost always a random variable, and the data are not i.i.d. And usually, you are not sampling from a single population, but from a composite of different subpopulations. Furthermore, we don't produce a single estimate; we produce a whole host of estimates, so that whole story is wrong. Now, I am not asking the statistics profession to throw out all those books, but I am asking them to make much more room for the teaching of statistical sample design and experimental design. By experimental design—I am quoting Nelder now (Nelder, 1994)—we don't mean just analysis of variance on orthogonal, symmetrical data, but the *design* of experiments. He complained about that recently, and this is coming from Rothamsted, where the design of both experiments and surveys originated.

Frankel: It seems to me that if you really push the idea of model-based sampling, it may just be an apology for quota sampling or a rationalization, because if you could really model things properly, randomization within groups doesn't enter into a model-based design. Would you like to comment on this?

Kish: There's no doubt that, although the model-based idea has been advanced by some very respectable theoretical statisticians, it can also be a cloak for some serious misuses by others. Let's take quota sampling. The model is that if you balance samples by age and sex, then you can afford to disregard how you got people into those classes. Now we know enough about what influence age and sex have on many variables, and it is very little. Their alleged effects have been disproven in the *Public Opinion Quarterly* by Roger Jowell (Jowell, Hedges, Lynn, Farrant, and Heath, 1993), who mocked the quota sampling in the British election polling. I'd like to see more such studies. What age/sex controls assume is that within age and sex the people that you pick are as good as if they were from a probability sample.

FIG. 7. *With the Basque President, October 10, 1986. Lectures in Vitoria, Spain.*

King: Aren't we just saying though that the model is underspecified? We know that there are many other factors that should be in the model, but with quota sampling you get to the point where it's logistically infeasible for the field personnel to administer any more quotas. That's precisely the reason you're getting into trouble.

Frankel: Would it be possible to specify practical models that you actually could use? How many quota controls do you have to use? Do you have to use 10? Do you have to use 20?

Kish: "Underspecification" is an emperor's cloak, because it implies that if we only knew enough, if ... if my aunt had wheels she would be a bicycle .... We have two questions here. One is how many variables could you do and, second, which ones would you pick? Let me give you two answers. The vice-president of one advertising agency told me during an expensive lunch about a plan to start using probability sampling, and he asked me what I thought. I had to admit that it would be more costly and difficult than quota sampling and this vice-president said, "You think quota sampling is easy? You think it's easy to find in New Hampshire a black man, college educated, over 65, who is a widower? You'd be amazed how difficult this is." So that's one answer to the question about multiple controls.

My other answer is this. So far, I have not seen a single case of a quota sample that is supported by a good model-based theory. People have asked me whether there are any good and acceptable quota samples. There aren't any. There are cases in which people have shown that there is not much difference in results between quota sampling and probability sampling—Sudman (1967), for example, and the early investigations by Moser and Stuart (1953). I am prepared to believe that in many cases quota sampling and probability sampling will produce similar results, but when they don't, they can cause a lot of trouble. I still stand by my Section 13.7 in *Survey Sampling* (Kish, 1965).

THE TEACHING OF STATISTICS

King: I wanted to ask whether you were ready to throw away all of the traditional way of talking about statistics that most students encounter in their first course. I've asked myself whether all that was necessary. I think you have to have it as a point of reference in understanding what is wrong with it. The implication is that one course in statistics is really not enough for anybody—even the infrequent user—because if they go out with only that one course, they are going to be doing some rather improper things.

Kish: Now, is a course in statistics necessary? I think yes. H. G. Wells said "Statistical thinking will one day be as necessary for efficient citizenship as the ability to read and write." I believe it's necessary, and as I said in the letter to *Science* (Kish, 1994b) about medical research that I mentioned earlier, such a course would be very im-

portant for doctors, but one course is not enough to design experiments or clinical trials. For any science education—medical, social, physical—a course in statistics would be useful so these people could understand something when they read their newspapers or their journals, and they should know enough statistical language to be able to consult experts for help in designing a study. They shouldn't, however, design it themselves, anymore than they should perform surgery after a single course.

King: Don't you think that sometimes they come out of that first course knowing a lot of the terminology, but not fully understanding what it means and, therefore, doing more harm than good?

Kish: If you're saying that it would be better to teach them nothing, I don't go along with that. For example, is knowing something about i.i.d. samples worthwhile? Yes. It's a simple basis and that's why we use it in defining design effects. Models are also useful. You couldn't do anything without models like $d = \frac{1}{2}gt^2$. It's good to know even if you don't know what g is. It's good to know these ideas provided you're humble about it and know that the real world is different. Now the i.i.d. world is not too bad for some things; for example, for small samples. If you only have 16 cases, you can afford to disregard design effects. You do something else. So I am in favor of teaching from good books, provided the author says somewhere that when you are dealing with data from complex national samples, this is not enough. In telephone sampling, using the simple random sampling formula doesn't throw you too far off. There are problems with weighting, but it's not too bad.

King: You know, of course, that the book by Freedman, Pisani and Purves (1978) was one of the first elementary statistical textbooks to contain cautionary material about exactly this thing. It has an excellent chapter on the Current Population Survey.

Kish: It's a very good textbook. Ted Anderson's new book (Anderson and Finn, 1996), Snedecor and Cochran (1989) and some others also have good chapters on sampling.

King: Freedman's book is great, but it's not widely used—at least not as widely as it should be. In my opinion, it takes quite a bit of understanding of statistics to teach from it, and we all know that the general market for beginning statistics wants a book that will teach itself.

What is your overall opinion, Leslie, of the way statistics is taught today, especially, in the one or two courses that most college students take?

Kish: I don't really know. Are you referring to the one or two courses taken, not by people who become professionals, but by physical or social scientists, or students in health, business or education? A course or two of statistics is necessary for all sciences. I think the courses have improved a great deal, and some use better books. We have already mentioned three good books that caution about complex samples. More of these books ought to have at least a caution about the assumption of independence, because in real life, this will be violated with serious consequences.

King: Now, we're saying that better textbooks are starting to appear, but how much they are being used is another question. We don't know how many adoptions there are, and my personal view is that many places still teach very, very irrelevant and mundane statistical stuff.

Kish: It's not enough to have a good book available. The person teaching from it should have had more than one course in statistics—at least a Master's degree in statistics—and that person should recognize the deficiencies in many of the textbooks. Again, take my example of medical doctors taking a single course in statistics. They should be made aware of its inadequacy for dealing with the kind of samples they will need and they should understand that they will have to seek expert assistance. If this message were widely disseminated, it would be a big improvement.

I am only dealing with problems of sampling, not with all the improvements that could be made in teaching other areas of statistics. One of them, however, would be to go back to the old idea of separating large samples from small samples, which was in Yule and Kendall (1937) in the old days. An interesting question is whether statistics for student scientists is best taught by a fairly good statistician in a science department or by a mathematical statistician in a statistics department or by whom? Each approach has led to good examples and, unfortunately, also to many bad examples.

Frankel: Let's now switch to the teaching of statistics for people who are going to become academic or professional statisticians—those who are going to get a Master's or Ph.D. in statistics. What do you think about the way they are being taught nowadays?

Kish: I am glad that you separated professional from academic, because the majority of statisticians, say, in the ASA, are not academics. They work for market research, pharmaceutical companies or in other industries. There may be more statisticians in pharmaceuticals than in academics, so I think that Master's level courses, particularly, should be aimed at the professional statistician, not the academics. Clearly, the Ph.D. should be directed toward academics and research. It would also be very good

for academic statisticians to go through a Master's program that is professionally oriented, which although primarily serving people for whom this is a final degree, would also well serve as a training ground in real-world applications for academics.

Frankel: What kind of job do you think people are doing teaching these degrees? We should concentrate first on the professional degrees, but, perhaps, we should go to the academic Ph.D.

Kish: More people should be teaching in Master's programs instead of focussing only on doctoral students. They must have the appropriate training. If a course in the Master's program is taught by mathematical Ph.D.'s and all their training has been only in statistical theory, they cannot teach, for example, sampling or experimental design. They can teach ANOVA, but cannot really teach experimental design.

King: You're saying that he or she doesn't have real-world experience?

Kish: Many or most have never had that kind of orientation and experience, so the best thing that could happen to the academic Ph.D. is to first go through a Master's program where they deal with real problems and real data.

Frankel: What about the kinds of research that academic statisticians do? The coverage given to real-world problems versus theoretical problems. Do you have any comments about that?

Kish: When I speak about academic departments, research and publications, I speak as a concerned citizen, not as an expert. Today the great majority of ASA members cannot read *JASA*. I think that's outrageous. They cannot read more than zero, one or two articles in any issue of *JASA*. For about two decades, I read and underlined every paper in *JASA* and many in the *Annals of Mathematical Statistics*. I think the present situation should be changed.

King: Let's give a plug here for *Statistical Science*.

Kish: That's how *Statistical Science* was started. I hope it continues to flourish, but there are other new English-language journals that are mainly theoretical, some in other parts of the world. Now, I am not opposed to theoretical papers, but I am talking about papers that are not chiefly mathematical. I want to separate theoretical from mathematical and methodological. I strongly believe in theory, but I also believe that some journals should have more applied papers, more of the not-too-mathematical kind. Everybody agrees to this proposition. We just don't know what to do about it. I know I can't solve it here and now. I expressed many of the same comments in my ASA Presidential Address (Kish, 1978) and after me, George Box (1979) said much the same thing.

THE CENSUS AND OTHER SURVEYS IN THE 21ST CENTURY

King: Leslie, I want to bring you back to a discussion of surveys, especially, large scale government surveys and even the decennial census and its associated survey operations. What kinds of changes should we expect, as we enter the next century, in the ways that these surveys are conducted and the methodology that will be used?

Kish: I am concerned with these matters, as you know. I believe that having censuses for local data once every 10 years is just not good enough anymore, and it hasn't been for some time. Philip Hauser, for example, said this many years ago in *JASA* (Hauser, 1942). He was proposing an annual Census. Nobody, however, is pushing the idea of an annual census now because even an annual census as small as 10% in scope could cost as much as 50% of a decennial census, so it is not practical. I have, however, written seven different papers, over the past 15 years, under the title "Rolling Samples and Censuses" (Kish, 1990). In the U.S. Census Bureau, Charles Alexander (1993) calls it a "continuous census"—almost the same thing. The idea is that you collect data weekly, say, at a rate of 1 in 520, 52 weeks a year, and then you roll it up into what would be roughly a 10% census for the year. Consider this: Suppose today in 1995, you have a choice of using either (1) the 1990 Census, (2) a 1994 sample—let's say a 10% annual sample—or (3) the latest Current Population Survey (CPS) sample. For hardly any purpose would you use the 1990 Census. For small domains, you might use the 10% sample from 1994. For larger domains you would use the Current Population Survey.

King: But for small areas you're stuck with the 1990 Census?

Kish: For the *very* small areas there are some needs where, as you say, we are "stuck." These would be block-level data. For tracts or wards, I think you might prefer the last accumulated year of the annual 10% rolling sample. For many things, you can get along without small area statistics. For example, suppose you are involved with something about transportation or about pollution. Pollution doesn't come block by block. It comes by some units bigger than census tracts. It comes in neighborhoods. So for most purposes, for transportation say, you don't provide statistics for every block. Only for very few situations do you really need block data. In those cases, by the way, you could use the annual rolled samples and impute the block-level statistics with iterative methods like SPREE (Purcell and Kish, 1980). That would be better than using the last census. Just

FIG. 8. *Receiving an honorary Doctorate in Statistics from the President of the University of Bologna, Italy, on its 900th birthday celebration (1988).*

think of how long it takes now. The monographs based on the 1990 long form are just going to be finished in 1995. That is all that will be available from 1995 to possibly as late as 2005.

King: It seems that the waiting period gets longer and longer each decade.

Kish: I believe that our decennial census was a great invention by either Jefferson or Franklin, or Madison. It was great for its time, but not for today.

King: I don't think you could abandon a decennial census altogether because it is required by the Constitution. There would have to be Congressional action to change the rules or to go from that to a rolling census.

Kish: I am not a lawyer and I don't know whether a 100% census every 10 years is required or if a sample will do. I have heard that it is mandatory, but I am not going to go into that issue. It might be required constitutionally, but it is not good statistically.

CURRENT PROBLEMS FACING SURVEY RESEARCH

Frankel: I have another question. What would you say are the biggest unsolved methodological problems facing survey research today?

Kish: Are we talking to the American audience only or to a worldwide audience? In America, telephone surveys predominate, because we're past 90% coverage of households and Canada is up to 98%. Our biggest problem is refusals.

Frankel: Let's talk about the increase in the nonresponse rate, which is a combination of refusals and not-at-homes. What do you think is going to be done about that?

King: It is also related to the failure to exercise follow-up procedures when you could do it.

Kish: Right. I have always had an interest in nonresponses. I said earlier that in 1946, at the Division of Program Surveys, I insisted on counting, classifying and reporting nonresponses. We were not the first organization to count nonresponses, but we were the first to do it on a regular basis.

King: Who were the other people who worked on developing and counting nonresponse?

Kish: Clyde V. Kiser (1944) was one of them. He was a demographer who wrote in the *Millbank Memorial Quarterly* around 1940. Let me say, however, that I don't have any good ideas about how to cut down on refusals. I have one method called a "replacement procedure" in my survey sampling book (Kish, 1965): You take the nonrespondents from one survey and add them to the selection for the next survey, if you have continuity of methods.

Frankel: What do you think about adjustments for nonresponses? Do you think that too much or not enough effort is being spent on adjusting for nonresponse?

Kish: Perhaps. But there has been a big change because when I started doing surveys the typical response rate was close to 90%. We did some early work in analyzing the results and found that with small samples and high response rates, the differences between adjusted and unadjusted figures were so small as to be negligible. Adjustments were not worthwhile then, and anyway they were difficult without computers.

King: Who were some of the people you worked with then?

Kish: Some research on our small surveys and high response rates was by an economist, Hal Guthrie. Today you have large surveys and low response rates, and the adjustments seem to make for differences. I believe that most good organizations make adjustments. Perhaps too many papers are being written about very fine points concerning differences, and we must remember that adjustments may reduce, but do not eliminate, biases resulting from nonresponse. The study of correcting for nonresponse has become an industry and there is a lot of wheel spinning going on perhaps with respect to exactly how to adjust. I am in favor of adjusting, measuring the effect and publishing.

King: But if your overall response rate is only 30 or 40%, the results of that exercise may be irrelevant. There is only one real cure for nonresponse and that is getting the response.

Kish: You are so right. There are, however, lots of other survey problems. How to reduce refusals, how to get better responses, especially on sensitive variables like abortions, homicide, drug addiction, etcetera. We shall never have methods to solve all those problems.

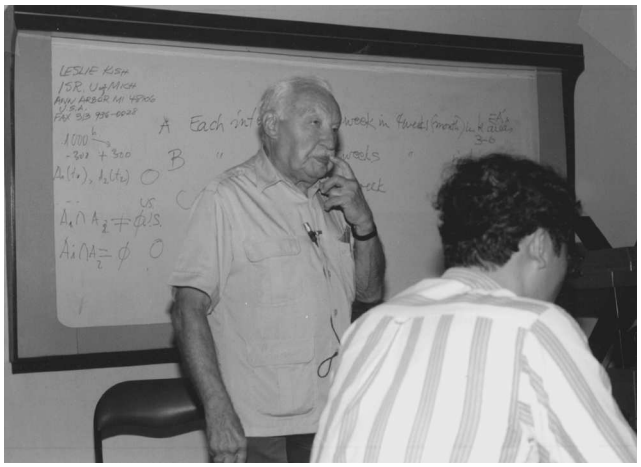

FIG. 9. *Leslie Kish at the Center for Population Institute at Hanoi, 1995.*

THE McCARTHY ERA

King: Finally, before we wrap this up, tell us something about the academic atmosphere of the 1950s.

Kish: About what happened during the McCarthy Era at the University of Michigan? I can't cover what happened in academia in the whole United States. There are several good books about that—one in particular, *No Ivory Tower,* by Ellen W. Schrecker (1986), describes the situation at various universities, including Michigan. Three great people here at Michigan were expelled from the faculty. One was Clement Markert, who became head of all biological societies in the world, now retired in North Carolina, but who is still very active and prominent—a wonderful person. He had also been in Spain. Also Mark Nickerson, who became chairman of pharmacology at McGill in Canada, and Chandler Davis, a mathematician, later a professor at Toronto University.

King: I assume they were fired because they refused to sign a loyalty oath or what?

Kish: Chandler Davis refused to testify, citing the First Amendment. The other two took the Fifth Amendment.

King: Did everybody else on the faculty sign a loyalty oath?

Kish: I don't think that we had a loyalty oath, but a number of persons were harassed. We had a state investigative panel called the Clardy Committee, and people were called to the capital in Lansing to testify. It's a long story. Let me add that for

the last three years we have had at Michigan an annual Nickerson, Markert, and Davis Lecture on Academic Freedom presented by some prominent professor. We are trying to make amends for the events of that evil period. At that time somebody from the FBI came to my office for a long interview. He started asking me questions, first about Spain, and then other things. By the way, this is the way the game was played: When you got before the Committee, if you were defiant, your name was splashed in the papers. If you cooperated, your name was in the papers, but as a cooperator.

King: You did not go before the Committee, did you?

Kish: I didn't. When the FBI agent came to my office, I took the following stand: I said "If you want to ask me about me, I'll tell you anything about myself, but I will not talk to you about anybody else." Now, this was a nonfeasible attitude. It was illegal to do this—to talk about myself and then refuse to talk about anybody else was against the rules of the Committee. You couldn't do that because once you started cooperating you had to cooperate further to show your "sincerity" by giving names. Those were the rules that they laid down. I said "Yes, I know, but that's what I am going to do, because I believe it is wrong to name others."

King: In this initial interview you weren't under oath?

Kish: No. Maybe he had a tape recorder like you have now.

King: Was it sort of a screening interview?

Kish: Yes. He wanted to feel out whether I would make a defiant witness or a cooperative witness, but I took a stand, with a risk, and refused to play their game. He said that I couldn't do it, but I did it anyway. He went away. I went home and told my wife, "I don't know if I have a job. I may have lost it because I took a stand which I think is honorable, but they tell me it's not a feasible stand according to their rules." But you see, I didn't make a good headline. I was neither defiant—because I was willing to talk about myself—but neither was I cooperative by giving other names. So it turned out to be the right answer for me.

Frankel: What did he want to know? He knew you were in the Spanish Brigade.

Kish: Oh yes. He knew a lot about me. You know, the Spanish Civil War was the biggest thing back then. How did I get to Spain and so on and so on. I was willing to talk about that, but then he asks, you know, "Who sent you to Spain?" I said "Nobody, and I am not going to talk about anybody else."

King: Did he ask you directly about membership in certain political parties?

Kish: I think that was a usual question, but you see, having been in the Army, I had gone through two or three security investigations and also for the U.S. Civil Service.

King: The atmosphere must have been chilling.

Kish: Oh, it was terrible!

King: How many academic years did this go on at this intensity?

Kish: The book by Schrecker talks about that. It came to an end when President Eisenhower, after he had listened to McCarthy maligning General Marshall—one of the greatest men this country ever produced, greater than most of our presidents—just couldn't take it anymore. Eventually, Eisenhower said it was too much and he publicly denounced Senator McCarthy. McCarthy was finally censured by his own colleagues in the Senate.

King: Was there any resurgence of this sort of thing during the John Birch period in the sixties?

Kish: Oh, yes. Through the Freedom of Information Act, I eventually obtained the file on myself from the State of Michigan. I also got one from the Federal Government. It is mostly blacked-out, blank pages. So it was nothing really interesting. They both go through the same thing, mostly about Spain.

Frankel: What words were blacked-out?

Kish: How do I know? It was classified, because it would have revealed their sources. I remember one comical thing in the file: There was a meeting in the Ann Arbor Public Library about the construction of air raid shelters. It was an advertised public debate, and somebody—a local John Bircher—took a position in favor of building air raid shelters, and I took the opposite side, saying that they would be useless. This went into my record. Now, of course, the agents got their information from the *Ann Arbor News* because this was a piece of news. So it was in my record that I spoke against air raid shelters in the Ann Arbor Public Library under the auspices of—I don't know—probably the Democratic Party.

King: You were undermining the vigilance of the American people.

Kish: That's right. I was in favor of being annihilated by Russian bombs!

Frankel: Hopefully, that period of American history is now over for good.

King: I regret that the time we have for this conversation is also over. Thank you, Les, for all of your wonderful stories.

Frankel: And for your insights into the history and future of statistical surveys.

Kish: Thank you both. I very much enjoyed our conversation.

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