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A Conversation with Lincoln E. Moses

Byron Wm. Brown, Jr. and Myles Hollander

Abstract. Lincoln E. Moses was born on December 21, 1921 in Kansas City, Missouri. He attended San Bernardino Valley Junior College from 1937 to 1939 and earned an AA degree, earned an A.B. in Social Sciences from Stanford University in 1941 and a Ph.D. in Statistics from Stanford University in 1950. He was Assistant Professor of Education at Teacher's College, Columbia University (1950–1952), Assistant Professor of Statistics in the Department of Statistics and the Department of Preventive Medicine, Stanford University (1952–1955), Associate professor in those departments from 1955 to 1959, and Professor of Statistics in the Department of Statistics and the Department of Research and Health Policy, Stanford University from 1959 until his retirement in 1992. He is now Professor Emeritus. He was Executive Head of the Department of Statistics at Stanford from 1964 to 1968. He served as Associate Dean, Humanities and Sciences, Stanford University (1965–1968 and 1985–1986) and Dean of Graduate Studies, Stanford University, 1969–1975. He was Administrator, Energy Information Administration, Department of Energy, 1978–1980 after being appointed by President Carter in 1977. His many recognitions and honors include being Fellow, John Simon Guggenheim Memorial Foundation, 1960–1961, L. L. Thurstone Distinguished Fellow, University of North Carolina, 1968–1969, Fellow, Center for Advanced Study in the Behavioral Sciences, 1975–1976. He is a Fellow of the Institute of Mathematical Statistics, a Fellow of the American Statistical Association, an elected member of the International Statistical Institute, a Fellow of the American Association for the Advancement of Science, a Fellow of the American Academy of Arts and Sciences, a member of Phi Beta Kappa and a member of the Institute of Medicine. In 1980 he received the Distinguished Service Medal of the U.S. Department of Energy.

The following conversation took place on April 27, 1998 in Byron (Bill) Brown's office in the Department of Health Research and Policy, Stanford, University, Stanford, California.

SCHOOLING IN CALIFORNIA

Hollander: Lincoln, why don't we start near or at the beginning. Would you tell us something about your childhood years and how you got started in Kansas City?

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Moses: Well, I got started in Kansas City hardly at all. I was born there as my mother's relations were there, but I was promptly moved to Kansas as an infant—western Kansas, where the Moses family was staked out in a town called Great Bend. In fact, there were three mutually unrelated Moses families in Great Bend at that time. Then, before I was two, my parents brought me to Southern California where I lived until I transferred to Stanford in 1939 as a junior.

Hollander: What were your parents like?

Moses: My father was a lawyer. My mother was a homemaker until the family split up when I was around age nine, and then she became a schoolteacher. She took a master's degree in Business Education at USC. Seventy-five people got that Master's degree that year, 1931 or 32, and one of them got a job—that was the depression. That one, however, was my mother.

Hollander: Quite remarkable.

Moses: Yes.

Hollander: How did the initiative arise to go to Midland School (located near Los Olivos in Santa Barbara County, California)? I know this school played an important part in your life.

Moses: It did. Well, we were living in Los Angeles and I had been afflicted with asthma throughout all my childhood, and so an inquiry was made of a place where I might thrive better. It was believed that the climate in Los Angeles was adverse for me. There was an old family friend who was a volunteer Assistant State Superintendent of Education. They had several of them in those days. She knew of this school that was starting up and I was actually enrolled as the very first student in August of 1932. I was there for three years, where I became healthy. I entered in August weighing 68 pounds and went home at Christmas time weighing 108.

Hollander: Amazing. Was it mental or physical?

Moses: Mental and physical. In the first year I was being taught arithmetic out of the same book that I'd received instruction in the year before in Southern California. In Southern California I was given more homework than I thought was humanly possible. It was all right to understand arithmetic, but the idea of actually doing all these dreary problems was an affront to human dignity. That was my unconscious analysis. Then I wound up at the Midland School and my teacher at Midland was inexperienced. He assigned much more homework from the same book! He not only assigned it, but he expected it to come in, and he expected it to come in right. And that changed my attitude toward mathematics and computation and what was possible for the human engine to accomplish.

Hollander: He believed in redundancy and repetition.

Moses: I don't know if he believed in it, but he sure did dish it out that year, and probably thereafter. He believed in work.

Hollander: When you graduated from Midland you went on to San Bernardino High School and then San Bernardino Valley Junior College. Would you like to tell us a little bit about those days?

Moses: Well, I didn't graduate from Midland; at that time it ran seventh to twelfth and I went into the seventh grade and I left three years later as an eleventh grader, so I enrolled in San Bernardino High School as a junior. Two years later I was in San Bernardino Valley Junior College. Junior colleges were a different cup of tea in those days. And so of course were the faculty. The typical junior college and the typical small college got their full share of the local talent. That system has been entirely replaced now, as best as possibly can be achieved by siphoning off those who have academic promise into other institutions. Every effort is made to make sure these bright kids are sent to school at prestigious colleges and universities, taking them out of places closer to home. Our class at San Bernardino Valley Junior college was about one hundred and five students. One of them received the President's Medal of Science, which is one of the highest honors attainable.

Hollander: Who is that?

Moses: His name is Earl Stadtman. Another, Frank Gardner, became a professor of medicine. One was the editor of a journal called *Evolution*, and I can never remember his name. Then there was Clifford Evans, a curator of archeology at the Smithsonian Institution. I've left out some people. Surely, it is less likely for distinguished careers to have their roots in junior college today because of our changing structure. In those earlier days it was cheaper to stay at home and go to junior college. And the education was complete. Many of the junior colleges, San Bernardino was one, responded well to the challenge and gave a very good education. When I came here to Stanford, I found that the work was of comparable difficulty and it was slightly easier to make the same grades at Stanford.

UNDERGRADUATE DAYS AT STANFORD

Hollander: How did you decide to go to Stanford?

Moses: It's curious how I decided to go to Stanford. I applied to three colleges. I applied to the University of California at Berkeley, Stanford, and the University of Colorado, which was my first choice. Colorado admitted me as a conditional sophomore because they didn't like my high school English courses and here I was halfway through college. I thought "I'm just not going to be a conditional sophomore," and so I went to Stanford. And thereby the University of Colorado lost this person as an alumnus.

Hollander: Did you have an interest in science and mathematics while you went through Stanford?

Moses: I have always liked science and mathematics. My interest in mathematics got this terrific boost from the man who taught me it was possible to work. I had liked mathematics as a child. My father told me he came home one day; I was lying on the couch and I was all red faced. He came over and said: "What's up?" I said: "Two times two is four, and two times four is eight, and two times eight is sixteen, but I cannot figure out what two times sixteen is." How did you do that? So I guess I've been interested in math all my life, but I'd developed the idea that you couldn't actually do it until seventh or eigth grade at Midland School when I found out I damn well could. And I loved math and science. I chose not to go into science because—it was a very conscious decision in early college—I thought the world doesn't need more science; it needs more social science, so that's what I'll do. And so, I graduated from Stanford in social science. But math and science were what I liked and that is where I wound up. Maybe interest has a dominant, legitimate role in vocational choice. Anyway, when I took advanced psychological statistics from Quinn McNemar, I thought I'd found my vocation.

Hollander: You had this course while you were an undergraduate?

Moses: Yes, I talked my way into it. As I say in the *STATS* (Moses, 1996) article, I had floundered around wondering what in the world I wanted to do, because everything I tried was interesting and I'd always been successful. The first clear glimmer I got was when I took statistics from the track coach at San Bernardino Junior College. I learned I did not want to be a statistician. That was the first clear insight I'd had. Two years later I was taking McNemar's statistics course and I got my second clear insight—which contradicted the first. I did indeed want to be a statistician. McNemar said there was no way to do it, really. I could become a mathematician, or an economist, or a psychologist and then work into statistics, which was approximately true. There was only one statistics department in the United States and I don't know if it gave degrees either, and that was at George Washington University, just before World War II.

NAVY SERVICE DURING WWII

Hollander: You were interested in statistics and social science when you got your degree, and then World War II broke out.

Moses: Pearl Harbor occurred near the close of my first quarter of graduate work—in political science. I was aiming to become a city manager, since statistics was not feasible. In January 1942 I went to work as a clerk with the U.S. Civil Service Commission. After a year of that I was called to duty as an ensign in the USNR Supply Corps. Eventually, I got some sea duty, a year and a half on a floating grocery store: the USS Caelum, AK 106. It was named for a constellation near the celestial south pole—invisible to the naked eye.

Hollander: Was this part of your indoctrination when you came on board?

Moses: No, somehow the captain seemed to know this information. Later I read *Mr. Roberts*. The protagonist, Mr. Roberts, served on the AK 601 not the AK 106. When I read it I could recall smells from the ship. I seriously wondered if it had been the same ship. Many aspects of my ship and of its captain prior to my coming on board seemed to be reflected in *Mr. Roberts.* But I never found any real evidence.

GRADUATE WORK AT STANFORD

Hollander: What did you do after you were discharged from the Navy?

Moses: Well, that was May of '46 and I went right back to work for the U.S. Civil Service Commission, and under the laws of that time that amounted to—that was counted for pension purposes, for example, continuous service with the Civil Service Commission. That was not just for government employment, that was the law of the land for private employment too. You went back to your previous employer and counted your military service as a continuation. You left company "A" and returned to company "A." I left the Civil Service Commission in New York and returned to the Civil Service Commission in San Francisco; it's all in the U.S. Civil Service Commission and there we are. Then I worked for a little over a year. I experienced rapid advancement in that job, partly because I moved to another federal agency called the War Assets Administration. It was the only time in my life when I got two paychecks a month and one of them my wife, Jean, and I lived on and the other we banked. Never happened again. It was a heady and exhilarating experience. I was aiming toward graduate study and we were putting money away in order for me to become a graduate student. During the war statistics had flowered and by the end of the war there were a half a dozen statistics departments in various places in the United States—North Carolina, Columbia, University of California at Berkeley and others in process. And so in September of '47 I stopped banking all this money and became a graduate student in mathematics at Stanford, with a view to enroll in the following year, after I'd gotten my mathematics up, at Chapel Hill, which was the most attractive of the options that I saw. Toward the end of that first year, '47–'48, Stanford founded a statistics department and I stayed here at Stanford. Never have regretted it.

Hollander: Who were the founders?

Moses: Al Bowker. In 1947, Bowker had been placed here by Fred Terman, who had sized him up at the Statistical Research Group during the war. Bowker was first in the Economics Department and then he received an offer from the newly founded Committee on Statistics at Chicago, from Alan Wallis, and he used this to get a promise from Alvin Eurich, Stanford's acting president, to start a statistics department. The first thing he did was to bring Abe Girshick, who was weighing three offers. Girshick didn't have his Ph.D. at the time, but he was offered a full professorship at Chicago in, probably, economics, a full professorship at UCLA in engineering and a full professorship in statistics at Stanford. He chose the last of those. We had visiting appointments in those earlier years, year-long appointments for people like Girshick, David Blackwell, Erich Lehmann and so forth. In those earliest days we had visitors like H. B. Mann (he may have visited the Math Department)—that's Mann of the famous Mann–Whitney–Wilcoxon statistic. Now I left out something, which is how did I start taking graduate courses in mathematics if I graduated in social science?

Hollander: Please tell us how this happened.

Moses: Yes, well, I'd had a little calculus, I'd had one year of calculus and analytic geometry at San Bernardino Valley Junior College while I was trying to figure out what I was going to do with my life. I'd understood it quite well, and I liked it and when I learned that statistics departments were occurring, I immediately subscribed to the *Biometrics Bulletin* and *The Journal of the American Statistical Association* and checked out a calculus book. This occurred about a year before I was discharged. It was Granville, Smith and Longley's calculus book, and I worked alternate problems in that book until I got discharged. If I'd only had another month or two, I would have become a little more adept with multiple integrals than I ever did become, because I stopped studying the calculus when I was discharged. But it was strong enough for me to enter courses like fundamental concepts of analysis, real variables, differential equations, probability and so on and so forth. And I did well in those.

Hollander: Did you have to lobby to get into those courses?

Moses: Actually, it is quite curious, yes. In those days, first of all, I was already an enrolled student at Stanford University (in political science). As a student in political science, I took all these math courses. I remember my advisor; he would look at them and say: "You really want to do this?" I said: "Yes, I really want to do this." And he signed his name. Finally, I thought, in the interest of decency I should go and transfer to the Math Department so I went to interview Gabor Szegö. He could tell that I was not a mathematician and he wasn't going to let me into his department. I said, "I'd like to get a master's degree and then become a statistician." And he said to me, "Do you promise not to study pure mathematics?" You know, English was not his native language. I said: "If by that do you mean, do I promise not to believe that I'm a candidate for any advanced degree in mathematics—all right with me." He said, "All right, then you can come in." Then he said, "By the way, I see you're taking fundamental concepts of analysis; what did you get?" I said, "I got an A." "Well!" he replied, "Perhaps you could study pure mathematics." I had one strange interview after another in those days.

Hollander: Who were some of your fellow students at the beginning?

Moses: Right at the beginning there were a couple of other people. One was a fellow named Ernie. He got a master's degree. I'm sorry, I can't remember his name. There were two or three of us. In '48 the Statistics Department enrolled three graduate students, of which I was the only one headed at that time for a Ph.D. I was the first student to take oral examinations; I was the second student to get a degree. Herb Solomon, a year later, became a graduate student in statistics and though he took his orals after me, he got his dissertation in sooner.

Hollander: It was Herb Solomon, you, and then who was next?

Moses: I believe that Elizabeth Vaughn was the third Ph.D., although that can be checked. She worked for something like the Fish and Game Commission of the state of Oregon, and she got a degree very early.

Hollander: Were Girshick and Bowker essentially the only faculty you could work with?

Moses: Well, Herman Rubin showed up in '49. He was my thesis director, he and Girshick together, but Rubin played a larger role. Another early graduate student was David Haley. He had got a master's degree in math at Stanford (after long service in the U.S. Army) and then gone in 1947 to teach at Acadia University in Nova Scotia. He returned in the summers of 1948, 1949 and 1950, and presently enrolled fulltime for his degree. I well recall the summer of '49 when we shared a room in Berkeley together—a wonderful six-week summer session there. We came back down to Palo Alto on weekends. All travel was done in my surplus military jeep.

Another early Ph.D. was Gerald Lieberman. He arrived in 1950 and received his degree in 1953. I was one of the examiners at his orals.

Hollander: What topic did you work on under Girshick and Rubin?

Moses: The title was "An Iterative Construction of the Optimal Sequential Decision Procedure when the Cost Function is Linear." It confirmed a conjecture of George W. Brown's (orally to Girshick) that such an iterative approach should be feasible.

TEACHERS COLLEGE, COLUMBIA

Hollander: You received your Ph.D. in 1950 and then you went to Teachers College, Columbia University.

Moses: That's right. My first academic appointment was as an assistant professor of education. I was offered it and Bowker said, "That is a good job; you take it," and I did. They treated me very well, and they worked me very hard. I'd had three kids by then and I was assistant professor—it was a tough go. It worked out fine. When they set up the job at Stanford, I was very glad to come. At almost a twenty-five percent cut in pay. Partly because I preferred the field of medicine and partly because I thought I wouldn't live to a ripe old age if I held on to that job at Teachers College. It really was an exceedingly rigorous regimen at the school—large teaching loads and at late hours. It was a pleasure to work as Helen Mary Walker's first junior colleague; she was the first woman president of the American Statistical Association. She was a smart woman.

NONPARAMETRIC RESEARCH

Hollander: Lincoln, I want to ask you about your nonparametric research in the 50s and 60s. Would you tell us about some of that early work at Teachers College when you got involved with nonparametrics?

Moses: This work with graduate students at Teachers College often involved nonparametrics because the problem under consideration might have only seven or eight subjects. There was no real way to be confident of the analysis, other than by nonparametric methods. I'd become interested in the topic somehow before I'd left Stanford. I think I was overly attracted to the exactness of the size of the test. I put rather more emphasis on that than it deserved. The first paper I ever wrote was in the *Psychological Bulletin* (Moses, 1952) in which I summarized a lot of the nonparametric statistical literature. It was the most popular paper I ever wrote. Reprint requests exhausted the reprints, exhausted a reprinting of the reprints, exhausted a reprinting of that.

Hollander: That was about the time that you presented very useful graphical procedures for obtaining exact confidence intervals for location parameters based on inverting the two-sample Wilcoxon rank sum test and the one-sample Wilcoxon signed-rank test.

Moses: Those two graphical procedures for estimating a translation parameter appeared in a chapter that Walker and Lev asked me to write concerning nonparametric methods for their book *Statistical Inference* (Walker and Lev, 1953). This was the first place in the literature where one could find the two-sample graphical procedure. The one-sample graphical procedure was due to John Tukey and appeared in an unpublished memorandum of the Statistical Techniques Research Group at Princeton. I failed to credit him in that chapter as I should have done for that graphical method. (Also see Moses, 1965.)

Hollander: Two interesting papers in the early '60s were your papers on rank tests for dispersion (Moses, 1963) and your paper on estimating the probability distribution of the ratio X/Y (of two independent continuous positive random variables) using Wilcoxon-test theory (Moses, 1962).

Moses: Both of those papers (they are the only two papers I ever published in the *Annals of Mathematical Statistics*) were written during my sabbatical year at Oxford. One of them, I actually wrote in one day. I should have done that again sometime.

Hollander: And not admit it—well I know it wasn't "Rank Tests of Dispersion." You couldn't have written that in one day.

Moses: No, I didn't write that in one day. But the other wrote itself in one day.

Hollander: I thought the dispersion paper was an important contribution in the sense that in that period people were proposing many rank tests for detecting dispersion differences. You made it clear that those tests are very difficult to interpret, actually inadequate, unless one makes the restrictive assumption that the locations of the two populations are equal.

Moses: Well, all I was doing really was developing an idea that I heard from Erich Lehmann. Once he gave a nonparametric test for comparing dispersion in two distributions (see Lehmann, 1951). I think he called it a rank test, and then he remarked that he realized afterward that it was not a rank test at all. I thought about that and realized that the idea of rank tests of dispersion had some intrinsic difficulties.

Hollander: The paper that you wrote in one day, that was a clever idea for estimating the distribution of a ratio by translating the problem into a Wilcoxon-test theory type of problem.

Moses: It does allow easier computation under some circumstances.

Hollander: Would you comment on the paper in JASA (Moses, 1964) on one-sample limits of two sample-rank tests?

Moses: That was written to honor Charles Loewner—a European mathematician who came to the math department and who was a very estimable man with whom I once had an interesting conversation. The substance of that paper grew out of a remark that Charles Stein made once in a series of lectures that I was privileged to attend. He was commenting on the Wald–Wolfowitz runs test, which has very low efficiency, and addressing the question, how is it that it is not a very strong test, although consistent against all alternatives, under certain conditions? He offered the following two thoughts. First, most statistical nonparametric procedures embody test statistics which estimate natural parameters which the runs test does not do. The second remark was that most nonparametric two-sample procedures remain useful if one sample size is allowed to go to infinity while the other remains finite. That was not true of the Wald–Wolfowitz runs test. It was the second remark that I thought about and it led to the development of that paper that you asked about.

Hollander: That paper makes an important connection between two-sample tests and goodness-of-fit procedures, including one you gave in the paper. In fact, in your paper you pointed out how in a situation where you didn't even hypothesize a parametric form for the underlying population, but you just had it described by extensive demographic data, you could proceed.

Moses: Yes. I actually discovered that test in helping a client at the medical school. He came to me back in the days when we were up in San Francisco. He said he'd isolated all of the cases of a certain disorder in childbirth, and the world literature, in English at least, contained eleven instances and it seemed to him that they were an older group of women than you would expect randomly. I thought and I thought and I thought and I thought and I thought and then I thought, well, you can really offer a known distribution of age at childbirth and you can find the percentile rank of each of these eleven ages under the hypothesis that there is nothing peculiar about the age distribution of women with this disease. Then each of those percentile ranks is an observation from a uniform distribution, and the average of them will be well approximated by the normal distribution with mean 1/2 and variance $1/(12n)$ or, in this case, $1/\{(12)(11)\}$. So I did this for him and tucked it in my head, and then when I heard Charles give his remark later on, I developed it further.

Hollander: That test has a very natural consistency parameter, namely, the same as with the two-sample Wilcoxon test. It is $P(X < Y)$ where X is a random value from the distribution you're sampling from and Y is a random value from your hypothesized distribution.

INTEREST IN MEDICINE AND BIOSTATISTICS AT STANFORD

Brown: Could you tell us a little about your interest in medicine? You mentioned it, but we haven't talked about it.

Moses: It had always been interesting. I thought medical research was really something. I might have been influenced by reading Sinclair Lewis' novel *Arrowsmith* during my first year of junior college. I don't know where that interest came from, but of course I thought it was science. Also it appealed to me as being socially constructive. Near the end of my first year of graduate study, I asked my advisor, Al Bowker, if he would approve my taking a minor in physiology. He asked "Why?" Moses: "Because I would like to apply statistical methods to medical research." Bowker: "There is no place for statistics in medical research." Moses: "Well if there is not, there will be." Bowker: "Go ahead, if you want." In the end I was so lacking in prerequisites that I couldn't really do it and decided instead for a minor in mathematics. A year after this conversation Bowker was working with Henry Kaplan in his mouse-cancer research (and I was a junior participant).

Brown: You worked with Henry Kaplan on some radiotherapy problems he had brought to Bowker, and that contributed to your decision to do biostatistical work.

Moses: Yes. Bowker did a lot of consulting. It was a very valuable part of my education. When a member of the faculty would show up in his office to receive statistical advice, Bowker would often invite me in. It was among the most valuable parts of my education, and I regret that kind of thing didn't happen much in later years.

Brown: Lincoln, let's talk a little bit about the beginnings of biostatistics at Stanford with your coming back from Columbia to take a joint appointment in the School of Medicine and the Department of Statistics.

Moses: Well, in 1952 Henry Kaplan, Al Bowker and Rod Beard organized a split appointment between the Department of Statistics in the School of Humanities and Sciences, and Public Health, as it was called in those days, in the School of Medicine. I was appointed to that new post as an assistant professor. At that time it was very unusual, although not unknown, for a medical school to have a statistician on its faculty. Before 1959 I went two days every week to San Francisco and stayed three days every week on the campus. After about four years I was so overwhelmed with consulting work at the Medical School and teaching in the Statistics Department that I prevailed upon the Dean of the Medical School and Al Bowker to establish another split appointment like my own, and I was authorized to hunt for someone. I actually took a trip by rail and air around the United States, and interviewed quite a few people. The one that seemed best was Rupert Miller who was at Berkeley, serving the first year of a two-year appointment. His joining us was a very good thing. I remember a conversation with him on the first day that he sat down at a desk in San Francisco Medical School. I said, "Rupert, I'm awfully glad to have you aboard. I've been practicing medicine in this school for these years, and I've not had a college course in biology, physics or chemistry." He looked at me reassuringly and said, "That's all right, neither have I."

The Medical School moved in 1959 to new quarters on the Stanford campus. Brad Efron was appointed as instructor in the Statistics Department in '64 or '65. Not long after that he was taking part in occasional consulting work at the Medical School and was much drawn to it. Meanwhile, in 1968 we recruited Bill Brown to be a full time Professor of Biostatistics and so we had a three-person unit with some help from time to time from Brad. Time passed and Brad received an offer to go to Harvard and there was an argument over that. The University acceded to his request to modify his appointment so he was half in statistics and half in biostatistics. That brought great strength and a considerable degree of panache to the biostatistics group.

Brown: How did you get NIH training grants?

Moses: One day a piece of paper showed up on my desk, I think from Rod Beard, the department chairman. He said the NIH is trying to foster training of statisticians to work with medical research. I made out an application, arguing as follows. There is a big place for statisticians in medical research and it should happen at Stanford. At Stanford we were collecting magnificently prepared students to study statistics and unless somebody did something, they would all disappear into academic instruction of statistics, or local industry would recognize and be interested in them. We should be enabling students to look at biostatistics. Some of them would be very interested and would then stay in the profession and make it strong. We should recruit from the statistics department, not recruit directly into the biostatistics program, not offer degrees in biostatistics, but offer training in biostatistics and hope to entice well-motivated students to stay in that field. They responded for twenty years and still do.

Brown: And that program worked.

Moses: It did work. Lots of the students became biostatisticians, pre-doctoral and post-doctoral.

Brown: What about teaching the medical students, teaching statistics to the medical students?

Moses: Well, that's a mixed tale. It was legendary how nearly impossible it was to get the attention of the medical students to statistics, but I had pretty good luck for a long time. For a long time I gave the incoming medical school class its first lecture in medical school. Because it was a low-prestige subject, it was scheduled for eight o'clock in the morning on Wednesday and that was the first day of classes every autumn. So, at eight o'clock in the morning on Wednesday, the first lecture they received was from me, about biostatistics. For a long time I was moderately successful, but it fell into other hands when I went into administration and proved to be difficult when I went back to it. I was no longer very well received by the medical students. The course became an elective for a while. That improved things—when it was an elective course, anybody that attended was there because (s)he wanted to learn the subject, but when it was a required course, again it became difficult to teach it. I think in recent years maybe more success is occurring.

Brown: When I came in 1968 it was an elective course and it was a real pleasure. I had 20 or 30 students and it was a lot of fun.

Moses: There is a deeper lesson here about required courses I think.

Brown: We require it now and I think it's taught well by a couple of our faculty here. They get good grades from the students on the teaching of that course in epidemiology and statistics, and I think they have fun doing it. Tell me a little bit about the consulting aspects of the development of the biostatistical division and any interesting experiences and interesting morals of the story that you can relate to us. Henry Kaplan was one person that I had known, and I know that he was instrumental in developing this whole idea of making consultants available to the investigators in the school.

Moses: Yes, and an association with Henry went on for years. Two other investigators were women: Judith Pool, who worked on hemophilia, and Rose Paine, who worked on white cells and their typing. I worked with them, and then both of them worked with Rupert later on. Both of these women became recognized for excellent research. Both of them were wonderful clients to work with. There were many other connections to researchers; indeed, there was a time when apparently a large fraction of the research going on at the Medical School was in touch with the biostatistics group. That's not true today because the medical school has burgeoned much more than the biostatistics group, but contacts are very wide today even so.

Brown: In the history of biostatistical consulting at Stanford, some fundamental ideas have arisen, such as Brad Efron's (Efron, 1971) biased coin design and Rupert Miller's (Miller, 1976) work on regression analysis for censored data. Any other ideas that come to your mind?

Moses: The National Halothane Study comes to mind. The Halothane Study was a cooperative enterprise with much activity at Stanford, but not all of it. Much activity at Harvard, but that didn't exhaust it. It was a cooperative effort at thirty-four medical centers, finally.

Brown: John Tukey was involved.

Moses: John Tukey was involved and John Gilbert (very centrally) and Yvonne Bishop, and still others. Also the Anesthesia Department at Stanford had an important role through John Bunker and Bill Forrest. Bill (Brown), you were in from the beginning, because of your connection at Stanford. It was an interesting study. It grew out of a few clinical cases that suggested strongly that a new general anesthetic, Halothane, could be injurious to the liver of the patient to whom it was applied. These were very vivid cases, but there weren't very many of them, and the anesthetic had spread into wide use because of desirable properties like smelling good, not being flammable or explosive, being well tolerated by patients, who did not wake up with hangovers, replacing the wrestling match that accompanied the use of ether with easy compliance by the patient. A highly popular anesthetic, but a possibly a dangerous liver toxin.

The National Halothane Study was set up by the Committee on Anesthesia of the National Research Council. Fred Mosteller and I were both members of that committee's subpanel on Halothane. The issue was a difficult one statistically, through familiar to statisticians who see it in other guises. Here was a possible causative agent, which might increase death rates, but the difference between various anesthetics and the accompanying death rates would surely be very small compared to the much more influential features of an operation like the age of the patient or whether the operation was on the brain or the big toe, or the physical condition of the patient (very very sick in general? or quite healthy?). So the challenge was to estimate any (tiny) differences in anesthetic death rates in the presence of many other strong factors that influence the death rate. Fortunately, we had lots of data: all surgical deaths over a four-year period in 34 hospitals (17,000 deaths) and 34,000 surgical procedures from the same body of experience, obtained by systematically sampling from the total of 850,000 surgical procedures. On each patient in this study we knew the anesthetic agent, sex, age, two-digit operation code, physical status, hospital, duration of operation. Many statistical devices, some new, were applied to the data, with gratifyingly consonant results. The ultimate verdict was that Halothane was probably as safe an anesthetic as any. The liver toxicity was real—but very rare. We felt confirmed in our early judgment to study death-rate effects which might far outweigh effects of liver toxicity. There was an interesting nonfinding. Because of its spotty and sporadic pattern of application, ether could not be characterized reliably as to its comparative safety. It was the only anesthetic that was not used at all in some hospitals. There were some other hospitals in which it was used more than any other anesthetic. It tended to be used for some operations and not for others. This erratic basis of application led us to recognize that we could not characterize it as either safer than most or less safe than most. The data would not support a finding. The Halothane Study is represented by a thick book full of data and full of interesting and imaginative statistical ideas.

Brown: The book (*National Halothane Study*, 1969) itself is interesting and has not only imaginative, but really innovative, fundamental procedures that other people continue to use. (You authored or coauthored five chapters in that book.) I think another extremely important product was the paper (Moses and Mosteller, 1968) that you and Fred Mosteller published in the *Journal of the American Medical Association* (JAMA) which was really the root of the procedures the U.S. government is now using to rank hospitals in terms of the quality of surgical and other kinds of therapeutic outputs.

Moses: Yes. The JAMA paper, a summary of the National Halothane Study in 1966, does appear to have been influential and I can tell a little story about it. I remember, as I participated in its writing, trying to find a more felicitous way of voicing the following thought: These differences between hospitals in their surgical death rates are too big to be explained by patient risk factors, apparently, or by chance. It appears that there are real differences, and "somebody ought to look into it." Now the idea of publishing a sentence that says "somebody ought to look into it" didn't satisfy me very well and I searched hard to find a better phrase, and failed to,

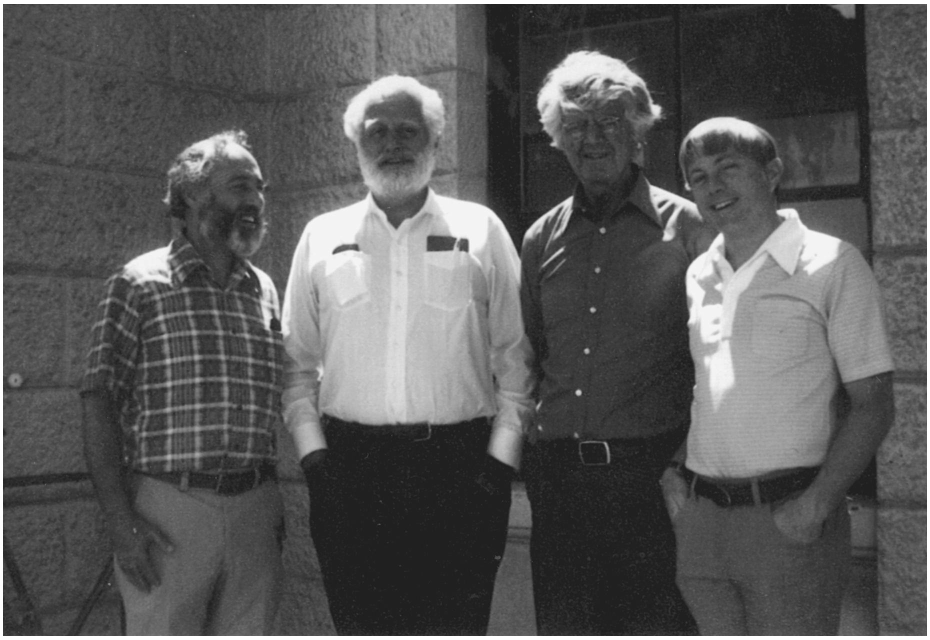

FIG. 1. *Sumner Kalman, Lincoln Moses, David Haley and Donald Bentley, circa 1979.*

and it appears in the article. There was an interesting sequel. I happened to know Phillip Lee, who was the Assistant Secretary of Health at that time, and had known him for a long time. His eye fell on that paper, and he clearly felt someone should look into it. I remember having a conversation with him about how it ought to be looked into. It shows that sometimes if you can't find a felicitous phrase, it doesn't matter too much.

Brown: I didn't know that story. That's nice. Now, some of your other work has also led to some deeper statistical questions, and I wondered about the work that you've done through the committee, various committees, I guess, at the Institute of Medicine in the area of AIDS, needle exchange and so on.

Moses: I think of most of that work as having been less statistical than policy oriented.

TEACHING STATISTICAL ASPECTS OF POLICY MAKING

Brown: I want to get into the idea of policy; it's important in regard to teaching too.

Moses: I am not sure how these assignments came about, but I have three times chaired NRC committees concerned with the AIDS epidemic. Of these the third (Normand, Vlahov and Moses, 1995) concerning the pros and cons of needle exchanges clearly called directly upon statistical expertise, because the question was how to appraise a large and complex body of evidence of data with regard to the question of causation. We had other statisticians—Ron Brookmeyer was on this AIDS study and his statistical expertise stood us in good stead indeed. But still, the main issue was policy orientation and your question allows me to sing a little song that is important to me. Mosteller has pointed out eloquently on other occasions, "It's time for statisticians to focus some of their attention on policy matters." Policy gets worked on and formed (it may not get decided but it gets worked on and formed), with easy frequent advice of lawyers and economists. Statisticians should be present at the same undertaking and for very good reasons. The statistician is more evidence-based than average. He (she) is more likely to ask, "What are the data?" He's (she's) more likely to offer searching questions about the source and meaning of the data. We should be taking all this into account in our education of statisticians. I think the statistics department should address the question: How can we help equip our graduates for participation in policy consultation?

Brown: Can you tell us what might be done in this regard?

Moses: Stanford has done two things that I think are examples of what is achievable. One is that they set up a public policy program for undergraduates in public policy at Stanford. That program tries to equip students with some knowledge of politics and even statistics to some extent, and particular policy areas chosen by the student like education, tax policy, and the environment, among others.

Brown: Did you teach a statistics course connected with that program?

Moses: I taught a core course in that for several years. The second useful thing they've established is Stanford in Washington. It's a residential program of about three month's duration for a student. Students go there, get their education through tutorial and seminar experiences and, in addition, put a major effort into writing up their internship experiences or research accomplishments. This internship takes up maybe half of their hours. This program has just celebrated its tenth anniversary and demands very hard work and is very rewarding for the students who take it. It is to the credit of those who administer it that it has succeeded so well. A critical part of it is generating a stable of contacts in the nonprofit and government sectors where you can place students to get meaningful work to do, as distinguished from, say, licking envelopes in a congressman's office. I'm happy to have taken a hand in the development of that by chairing the faculty committee that designed it.

Brown: What about instruction related to policy analysis for graduate students in statistics?

Moses: Well, if I'm right that we should see more statisticians in the places where policy is considered and fashioned, then it would be desirable to prepare professional statisticians for such roles as a part of their graduate education. How to do that? Internships with local governments or state governments. Especially summer internships might be useful. If a student has interests in policy matters and wants to take some courses related to it, instead of discouraging him or urging more technical material, accommodate that interest and allow him to study such matters. Maybe someday some member of the faculty will be interested in setting up a policy statistics workshop which periodically meets and has consulting contacts, who knows? But a favorable attitude toward it would be something I would urge if I were still an active member of the faculty. There is a particular aspect to this that Mosteller and I have recently written about in a chapter in the book Statistics and Public Policy to honor Richard Savage. The title of the paper was "Experimentation—Just Do It" (Moses and Mosteller, 1997). The statisticians recognize that the most reliable information about an intervention is accessible through experiment. Although that can be difficult to do, it will produce valuable information, often uniquely valuable information. Statisticians tend to know this and will work with it and should tend to push it in the policy circles. Experimentation is substantially underused, and it's a pity. A striking success was the Tennessee class size experiments—much written about recently (cf. Mosteller, 1995) and influencing policy all over the country.

Brown: A very nice example of a very nice chapter.

Moses: Well, we put together a lot of interesting examples in the chapter and tried hard to deal with both theory and practice.

Brown: Yes, there are a number of really nice examples of very economically run little experiments that shed light on important policy matters.

Moses: Here is one of my favorites. When I went up to Sacramento State to give a lecture a few years ago, I learned this story from a person who worked in the Department of Motor Vehicles. The legislature allowed people who had no moving violations and no collisions in the previous four years to renew their driver's license by mail. It permitted the Department of Motor Vehicles to do this. Previously, every driver was obliged to go and take the written test and the eye test and appear in person every fourth year regardless of driving record. And the Department of Motor Vehicles, according to this person, decided that they would hold back on ten percent of the renewals (with the old basis) and in the initial years only apply the mail renewal to eligible people for the other ninety percent. There was a lot of dispute, coming largely from optometrists, about public safety. If you didn't give an eye examination, think of the dire consequences. They were very concerned about this aspect of the public welfare. So ten percent were held back and then at the end of a few years you could compare the driving records of those who had the benefits of the in-person examination against those who had had the mail renewal, and no difference was found. The whole program was put on firm footing because of the experiment.

Brown: There's nothing like data.

Moses: That's right!

ENERGY ADMINISTRATOR

Hollander: Would you tell us some of the problems you got involved with when you were Energy Administrator in Washington D.C.?

Moses: That was a very interesting chapter of my life. I worked about twelve hours a day, but only five days a week, contrary to most highly placed civil servants of that time. In two and a half years I spent less than thirty hours altogether on weekends in my office. I did, however, work terribly hard and only on one thing—the affairs of the Energy Information Administration (EIA). The job was not very statistical; it was mainly organizational and administrative. It was interesting and absorbing work. When I got there, there were essentially no statisticians.

I thought for a long time I was the only statistician in that organization of some hundreds of people. I never revised that opinion by more than one or two, but I changed it. I went out and recruited for statisticians who had two properties: one, a clear, correct understanding of the elements of statistical theory, who, for example, recognized that an R^2 of 0.96 between Y and X did not imply that X causes Y. The second thing that I sought in the same people was a tolerance for working with data. Presently, by experience, I arrived at the opinion that the most propitious place to look for people with these two attributes was in departments of biostatistics, and so I did. My most notable single appointment, or at least my first notable single appointment, was to bring Yvonne Bishop, one of the participants in the Halothane Study, from Harvard's Department of Biostatistics to EIA. I could turn over the recruitment of statisticians to her then. Another notable, but later appointment was Lou Gordon—a magnificent, very helpful addition to that group.

I'll give you an idea of a really puzzling statistical problem. There were, and are, two ways, two statistical systems for estimating the monthly gasoline supply in the United States economy. The Bureau of Mines system for estimating gasoline supply was based on the following reasoning: Find the monthly production of gasoline at refineries, add the net monthly importation of gasoline from overseas, add or subtract the change in gasoline stocks in storage tanks in various places in the country, and there you have the gasoline production. There was also another system for estimating gasoline that entered the market and that was through state taxation. Virtually every gallon of gasoline gets taxed at the point where it leaves wholesale and goes to retail. So we had two ways of estimating gasoline supply, and they disagreed persistently by something like two or three percent, with the gasoline being sold exceeding the gasoline produced according to our Bureau of Mines system. While out recruiting for statistical professionals, I visited the University of Florida at Gainesville, and was delighted to see Bill Cochran was visiting them for some weeks or months. Since he had been an advisor to the Census Bureau and all such things through many years, I recognized in him an experienced person from whom I could get advice I would trust. I raised this paradox to him and his reply was very comforting. He said: "It seems to me that if you have two independently based systems for estimating the same thing, and they agree within two or three percent, you probably have other more urgent problems you should be putting your energy to." This gave me a great deal of comfort. But then, perhaps a year later, President Carter was considering establishing state gasoline quotas in order to be able to control the consumption of gasoline. An early step in establishing such quotas was to be quite clear about the amount of gasoline historically used by the various states, and we had two ways of estimating that, and they disagreed. They no longer disagreed only by two or three percent; they disagreed by about five percent. A horde of angry governors was a threat. Finding out what was going on and straightening it out was a very urgent and stubborn challenge. The solution finally was found. What had happened was that since the Bureau of Mines had established their system, in the 20s or 30s, there had grown up, rather recently with the advent of plastics and so forth, a petrochemicals industry. That industry involved sales, storage, motion of nongasoline products like toluene, benzene, pentane, hexane, etc. But these things could be combined judiciously, supplemented with a little lead and turned into gasoline. Here we now had gasoline not produced at refineries, though it would be taxed. So, not all gasoline that was taxed was produced at refineries. Some came through the unmonitored petrochemical industry. That's the kind of puzzle that can be challenging, stubborn, not very theoretical, but enough to give you a lot of a experience in a hurry.

Hollander: Did your experience as energy administrator set up a statistical legacy? Are there procedures in place now that are being used today in statistical procedures and programs?

Moses: I have not had recent contact with EIA. I stopped being the administrator in the middle of 1980. For the next six years I served on the advisory committee. The technical advisory committee, which with great difficulty I managed to establish, was enormously valuable to me as administrator and to subsequent administrators. I'd like to claim that as a legacy, but it's my understanding that sometime during the Reagan administration that got abolished, so we don't have that for a legacy if I'm right.

ACADEMIC ADMINISTRATION

Hollander: I'd like to ask you about some of your academic administrative activities. You've served about ten years in academic administration; some of it was during a very turbulent period during the Vietnam War and the student unrest then must have posed some challenging situations. Perhaps you can tell us about some of those.

Moses: Well, I was indeed dean there for those years. I think that an academic who has a research interest had better look at the advantages and rewards of functioning as a dean in terms of serving

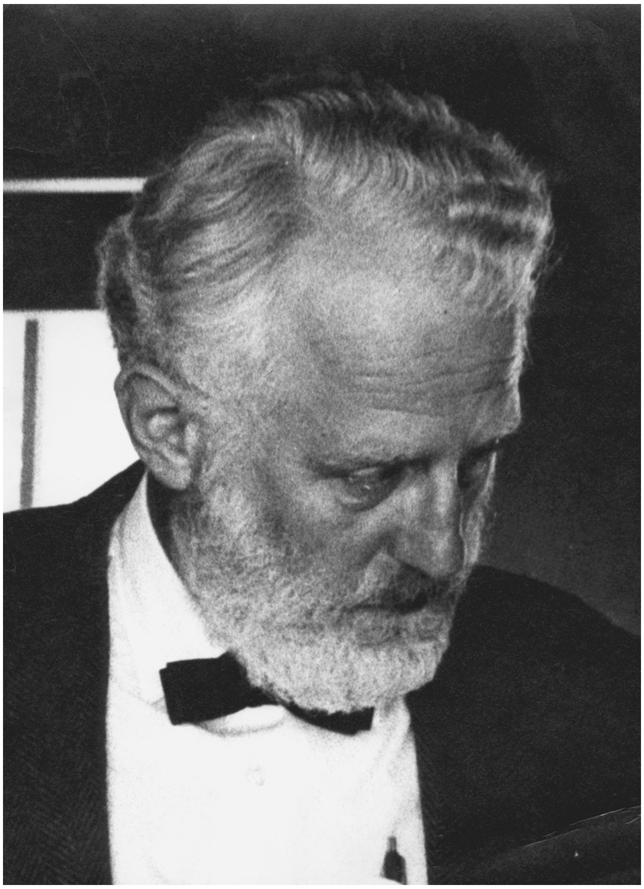

FIG. 2. *Lincoln Moses pondering problems of the Energy Information Administration, Washington, D.C., 1979.*

in the institution. I'm holding back from saying it's dirty work but somebody has to do it, but I feel a little bit like that. There is a distraction from one's career, and it may well be worthwhile, but it should be evaluated in terms of, "Do I wish to serve my institution by accepting this job, or would I prefer to continue doing what I am doing as a professor?"

Hollander: Did you get a chance to keep up your medical consulting during those years?

Moses: Very slight, very slight. I taught one course a year, which was very difficult to do under the circumstances. Later I had to catch up on an awful lot of stuff and had to learn a lot of things that had crept into and through the literature while I was not reading the journals. I am glad I did it. I've never regretted those years.

I spent four years as an associate dean (half-time) and six more years as Dean of Graduate Studies at Stanford. I found the work was interesting—for about ten years. I got acquainted with many other fields and with many interesting people. Occasionally, I was able to help some good thing happen, or derail some bad thing, but I also experienced the truth of Robert Maynard Hutchins' dictum, "An administrator is a person who is paid to be interrupted."

Possibly there are deans who conceive and then develop some broad programmatic themes, but I am not sure I have met any such; in any case I was not of such a mold. Rather, I found my environment was a passing stream of problems, and my aim was to help out. I was often pleasantly surprised to find in others a predisposition to be cooperative. Related to this was my discovery that I found it easier to serve as dean than as department chair. I checked this finding with others who had held both posts; nearly all had a similar perception.

It was hard for me to make the transition back from administration. A lot of catch-up ball. That was a cost I might have foreseen, but I did not. If I had it all to do again I know I would be a (bio)statistician. It is less clear that I'd be a dean.

You ask about challenges in my administrative phase. The years 1965 to 1975 were times of unrest on college campuses and times of anxiety and uncertainty in the administrative offices. The challenge was to maintain balance. Stanford had good leadership in these days and did maintain balance.

Hollander: Now here's a strange one. You were Associate Dean of Humanities and Sciences from '65 to '68 and then after being Dean of Graduate Studies, you come back and were Associate Dean again from '85 to '86. How did that happen?

Moses: That was funny. I came back twenty years later and I went into the same office and I had the same telephone number as twenty years before. It felt a little odd. The reason I was doing it wasn't ambition or anything like that; they simply ran out of associate deans. One left and one got promoted to provost and they were just out of associate deans and they asked, "Would you please come back for a year?" I said yes. During that year, the Stanford in Washington program was born.

BOOK WRITING

Hollander: Starting with the now classic Wiley book, *Elementary Decision Theory* (1959), jointly written by you and Herman Chernoff, translated into Russian, Japanese and Spanish and reprinted by Dover (1987), please tell us about your book writing activities (your motivation for writing, your favorite books, etc.).

Moses: You will note that most of my books have been edited in each instance with one or more coeditors. Of the three exceptions, the best known is *Elementary Decision Theory*, joint with Herman Chernoff (Chernoff and Moses, 1959); it is more his work than mine, and this is a good place to acknowledge that. *Tables of Random Permutations*, with R. V. Oakford (Moses and Oakford, 1962), has just this year gone out of print; it appeared early in the computer age. It provided, among other things, various checks on RAND's million random digits. (They stood up rather well to many tests of randomness described in the preface to the tables.) The only book where I was the sole author was my elementary text, *Think and Explain with Statistics* (Moses, 1986). I like that book well, but the publisher allowed it to go out of print almost immediately. Of the six edited books, Cochran's *Planning and Analysis of Observational Studies* provided the most fun—editing the almost-completed book with Fred Mosteller (Cochran, ed. Moses and Mosteller, 1983).

If any of these edited books were important, it would probably be *Preventing HIV Transmission: The Role of Sterile Needles and Bleach* (Jacques Normand, David Vlahov and Lincoln Moses, eds., 1995). This contribution to the public debate on needle exchange was the report of a National Research Council panel which was a joint undertaking of the Institute of Medicine and the Commission on Behavioral Sciences and Social and Education. I chaired the panel.

Hollander: Your papers and books are marked by clear exposition. In your joint paper with Fred Mosteller, "Safety of Anesthetics" (*Statistics: A Guide to the Unknown*, 1972), one finds the wonderful sentence, "Frequently things that are easy to use actually work better (this is true of sharp knives, fine violins, and easy-to-read instructions), so it might well be that an anesthetic that is easier to use might work better and result in somewhat lower death rates during surgery." Does the analogy extend to easy-to-use statistical procedures?

Moses: It is interesting that you found that sentence. I am even more convinced of its truth now than when it was written. Rodney Beard (who with Henry Kaplan and Al Bowker had brought me to the Stanford Medical Faculty in 1952) was an amateur viola player. One time he had the opportunity to use the viola of a professional violist (first chair in a San Francisco orchestra) and he told me, "Of course such a good instrument *sounds* better. But in addition, I found it much easier to play!" Immediately I sensed a broader truth behind that experience.

Now you ask whether I believe the principle extends to easy-to-use statistical procedures. It is true that where a problem can be validly handled by a t-test, that is likely to be my choice. I have seen situations where over-complicated analyses have generated obscurity; I avoid such analyses if I can. Of course one should seek an approach which is *valid*, rather than easy, or gloriously complex. Among valid approaches, the simpler ones are generally easier to understand, easier to explain, and more likely to be convincing. Simplicity may also give considerable protection against gross bungling.

Hollander: You have greatly influenced statistics and the training of statisticians not just by your books and research, but through your teaching and students. Would you comment for the record?

Moses: My most enjoyable teaching was in the Biostatistics Workshop, begun in 1958 and still going today. For the first twenty-five years or so it was a work-in-progress kind of series, involving many statistics graduate students and their consulting clients. Any applied problem was eligible for bringing to the table. It did not have to be from biomedicine. Several generations of students were backed by participation in this seminar. Originally, it was an undertaking of Rupert Miller and me. In later years Bill Brown and Brad Efron took part, and more recently Richard Olshen has led it. Most of my Ph.D. students participated and most of Rupert's, plus others.

Let me list (from memory) my Ph.D. students: Ed Perrin, Mary Epling, Bruce Hill, Myles Hollander, Mel Klauber, Galen Shorack, Chaim Sternin (with Herbert Solomon), Jean Thiebaux, Kathleen Lamborne (these last two being joint with Rupert), Margie Fuji Peterson, Malcom Hudson (with Charles Stein) David Shapiro, David Wright, Lynn Gale (in another department). These were good students and some of them have gone on to professional distinction. It was a privilege, and pleasurable as well, to bear a hand in their development as statisticians.

The Biostatistics Workshop attracted many other doctoral students; memory brings forth these names: Norman Breslow, Morris Eaton, Jay Kadane, Donald Bentley, Ned Glick, Jim Ware, Marshall Sylvan, David Sylwester, Louis Gordon, Jim Reading, Jim Arveson, Brad Efron, Max Layard, Mel Hinich, Art Owen, Joan Sander Chmiel, Beth Gladen, Sue Leurgans, Leon Gleser, Laurie Beckett, Gerald Chase, Martin Hamilton, Jerry Halpern, Alvaro Muñoz, Rob Tibshirani and Trevor Hastie. And there were post-docs, including David Hoel, Bruce Trumbo, Bill Brown, Jim Boen, Joseph Meiri and David Hill. Occasional master's students also took part. One was Ed Tufte, and another was Janet Elashoff. Another key in the Biostatistics Workshop was Susan Boyle, who for many cheerful years deftly helped staff and students with computations; her contribution was large indeed.

The organizing principle for this workshop was to bring the graduate students and live problems together *early* and *throughout* their Ph.D. training.

EARLY STATISTICS AT STANFORD

Hollander: The Statistics Department at Stanford is now 50 years old. You joined it in '52 so you've been here for 46 of those 50 years. Even more, if you count some of your earlier time. If you count your years as a graduate student, going toward your Ph.D., you've really been here for all but two years of its existence. Would you tell us a little bit about the department's development and how it has blossomed?

Moses: In the early years, there was a great deal of interaction with Berkeley so that Joe Hodges, Jerzy Neyman, Evelyn Fix, David Blackwell, Erich Lehmann, Elizabeth Scott were people that our graduate students would know, having seen them periodically. At Stanford there were wonderful European mathematicians like George Pólya and Gabor Szegö and visitors that they attracted like H. B. Mann, whom I've already mentioned. Al Bowker attracted E. J. Gumbel (of extreme value distribution fame). Many of the visitors spent a year at Stanford. There was just a wonderful traffic in persons who were participating in the development of statistics in those days. Classes were small. If you've got a cadre of six students, then a class might have two or three people in it. There were statisticians outside of the Statistics Department and the Statistics Department itself was rather ecumenical. In its earliest years there were joint appointments which involved actual teaching commitments: Kenneth Arrow, Patrick Suppes, Hirofumi Ozawa, Herbert Scarf. From '51 on we had the fortunate appointment of Herman Chernoff as a core member of the faculty. Charles Stein came in '53. So by the time '53 had arrived, we had Bowker, Girshick, Rubin, Chernoff, Stein, Moses, Lieberman. Emanuel Parzen joined the faculty in '57 and Vernon Johns joined in '58. The use of joint appointments was very prominent. Solomon had a joint appointment with Education, Lieberman with Engineering, I and Rupert with Medicine, Suppes with Philosophy, Arrow, Ozawa and Scarf with Economics. These were very lively times.

Hollander: When did Ingram Olkin come? I think of him as one of the department's pillars.

Moses: Ingram came in '61. He had a joint appointment with Education. We wouldn't dare bring a nontenured person now in a joint appointment because the chance that he could get through two school's procedures is too small. We've lost something. In the earlier days that I've described, you expected that a well-made young appointment would indeed go all the way to tenure, and justifiably so, it did. Another feature of the early Statistics Department, which persists to this day, is the presence of summer visitors. Summer visitors from all over the world show up and a summer in Sequoia Hall can be an enormously stimulating experience.

Hollander: I remember when R. A. Fisher was here in 1961 and you introduced him in the Medical School. His talk on genetics had the one word title, "Junctions."

Moses: Yes, that was the only time I ever met him. Joshua Lederberg had arranged the lectures. Joshua was the Chairman of the Genetics Department, but he asked me to introduce Fisher, which I was pleased to do. And it caused me to have some conversation with him as I went and retrieved him in a car from where he was staying. At that time Fisher was actively engaged in opposing the idea that cigarettes have a causal relation to lung cancer.

Hollander: Let's fast forward from 1962 to 1998. What is your view of the current state of statistics and would you hazard some predictions for the future?

Moses: When asked "What is statistics?" I am likely to reply, "Statistics is a body of methods for learning from experience." As such, it is very important and, I believe, under appreciated. What are some things to do in order to more fully realize the promise of this remarkable "body of methods?"

- Get statistics instruction into the high schools so that it becomes known to the citizenry, broadly.
- Give greater play in our curricula and our own careers to policy-laden applications of statistics.
- Construe our field broadly and avoid narrowing it. The split-off of operations research from statistics diminished both fields. The ties to economics, psychology and industrial engineering have become tenuous. Biostatistics offers a favorable alternative model.
- Graduate training of statisticians should include both lots of mathematics and lots of contact with empirical problems—data from substantive fields like biology, psychology and economics.

With regard to the future, much depends on how statistical computing unfolds. Large-scale simulation promises to open harder and harder problems to solution and to cause statisticians to become practitioners of experimental design, where we were formerly teachers of the subject. A somewhat contrary trend is the growth of statistical packages where

FIG. 3. *The Moses clan in 1994.*

the unschooled user may be overrelying on "default options" that he is unprepared to evaluate.

LEISURE ACTIVITIES

Hollander: Lincoln, during all of your remarkable career, you've managed to lead a very well-rounded life with a lot of balance. You've had relaxing activities that have included biking, bird watching and hiking. Could you tell us about some of these interests?

Moses: Well, I don't bike but I have derived much satisfaction from hiking. I know the Sierra Nevada very well.

Hollander: I have to interrupt for a second. Didn't you bike back from Stanford up to your Portola Valley retreat and then stop at Rosotti's Beer Garden?

Moses: Well, I only did this for a short time, around 1960, and I remember guiltily conflicted sensations as I had in front of me the long census questionnaire and it asked, "By what means of transportation did you go to work the majority of the days last week?" And I put down, for the only week in my life of which this would have been true, "biking." Giving that response drew heavily on my sense of obligation to tell the truth on the questionnaire for statistical reasons. But that's the end of my bicycling.

Hollander: I remember you saying that you biked all the way in from your home, but could only manage to make it halfway back.

Moses: I would go no further than Rossotti's. The road gets steeper there, and good beer beckoned.

I've also had a lot of satisfaction from hiking in the Sierra Nevada and car camping in the desert and from nature study. First just birds, but if you get serious enough about birds, the next thing you notice is what kind of a tree they're sitting on, and from then on it's all down hill. One of the first things I did after retiring was to take the training to be a docent at Jasper Ridge Biological Preserve, which has helped in my understanding of plants as well as birds. I also enjoy chamber music. There was a five-year period in my life when I tried to play the cello. That meant a lot to me at the time, but I could not develop the ability to play rapidly, so I finally gave up.

I'm blessed with five children, Katherine, Jim, Will, Margaret and Elizabeth, who love one another and me and vice versa. They have produced eleven grandchildren, all of whom are outstanding. Mary Lou, my wife of thirty years now, furnished me with four sons additionally, Kenneth, Frank, David and Matthew, and they thrive. So I count myself lucky.

Hollander: Now that you've retired you still seem very active. What are some of the things you are doing?

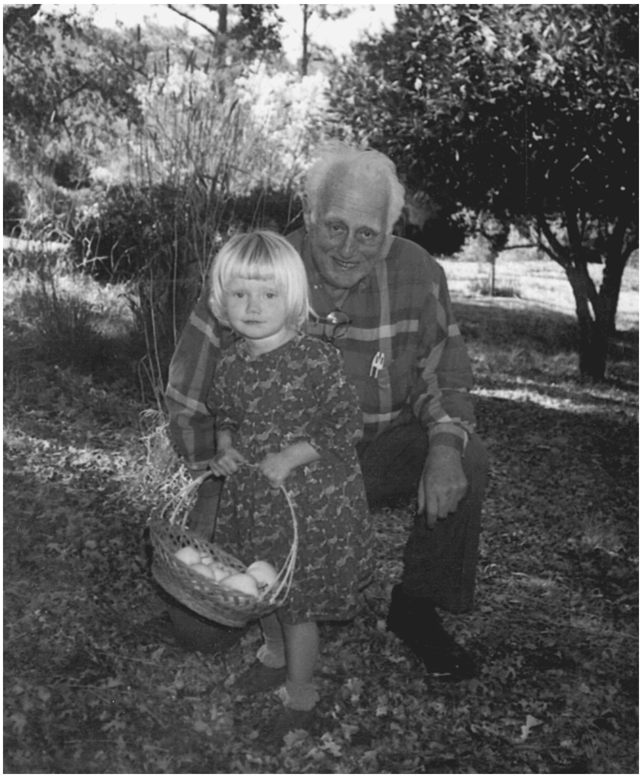

FIG. 4. *Lincoln Moses and his youngest grandchild, Clare Schneider, in 1996.*

Moses: I'm not very active statistically. I read a lot; I give occasional seminars on topics, so I find myself reading up on things I should know but have not previously learned. An interesting example of that occurred about ten years ago during my little statistics seminar at the Center for Advanced Study in the Behavioral Sciences. They leaned heavily on me and said, "Give us some multivariate analysis." I resisted—I never included it because I just resisted it. My reason for resisting it was that, though I'd studied it as a graduate student and had understood it, I had, over years, not found *any* occasion to use it. I had come to sort of dismiss it as more mathematics than statistics and not useful in my experience, and that was why I had not included it. But they leaned on me so hard that I opened some of the more recent books and I discovered that multivariate analysis has become a kit of useful tools for describing data. Not for statistical inference in the presence of a multivariate normal distribution, an emergency that may not arise, but for describing complicated bodies of data. There are now all kinds of interesting, useful techniques that grew out of responding to persons at the Center. I've followed that up, tried to learn more about that.

Brown: You do a little bit of consulting around the School of Medicine though. People wander in and ask you some questions. You spend a fair amount of time with some of those.

Moses: Yes, I encourage old clients to maintain contact with me if they wish, and I've taken an active part in an AIDS research project which operates in Harare in the country of Zimbabwe in Africa. That's been statistically rewarding, and it has been interesting to visit Zimbabwe for weeks at a time on three occasions. I continue to interact with that research.

Brown: It combines well with your interest in birds.

Moses: Yes, wonderful birds!

Hollander: Lincoln, you've had a remarkable career and you've been an inspiration to many, many people. Bill and I are lucky that we can be included in that list. It has been a great pleasure and honor for us to have this conversation with you, and we thank you very much.

Moses: Well, it's an honor for me also. I thank you very much and I hope that our association, which reaches back so far and so pleasantly, will continue for many years.

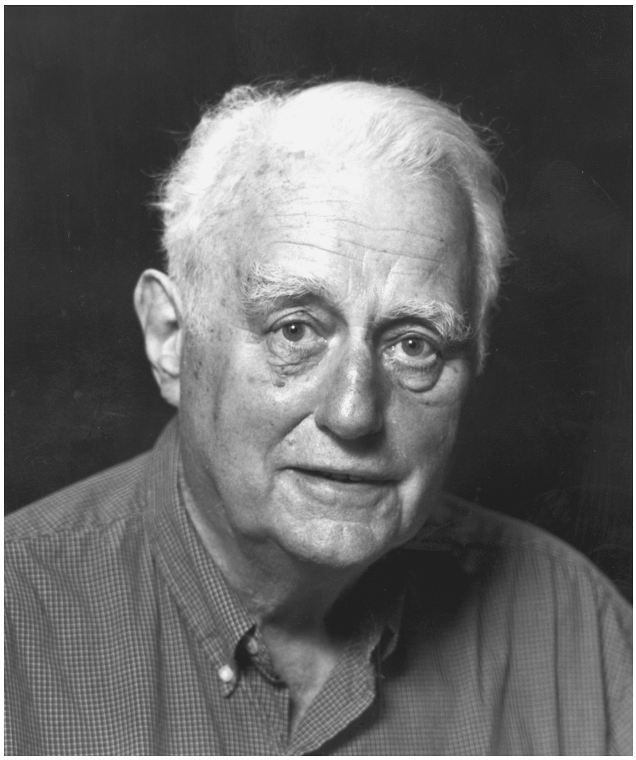

FIG. 5. *Lincoln Moses at the Center for Advanced Study in the Behavioral Sciences, Stanford, California, 1998. (Photo by John Sheretz with the kind permission of CASBS © 1998.)*

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