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A Conversation With Herbert Solomon

Paul Switzer

Abstract. Herbert Solomon was born in New York City on 13 March 1919. His parents had arrived in the United States at rather young ages around the turn of the century, and like many Jewish immigrants from Russia they came to find a better life. He profited from the New York City public education system, receiving a B.Sc. from City College in 1940 with mathematics as his major subject. In 1941, he completed a master's degree in mathematical statistics under Harold Hotelling and Abraham Wald at Columbia. The Second World War intervened at this point, delaying a Ph.D. in statistics until 1950 at Stanford.

Through Hotelling he secured an appointment with the Mathematical Research Group and subsequently, the Statistical Research Group at Columbia, both of which were engaged in military research during the Second World War. From 1948 to 1952, he served in the newly established Office of Naval Research, where he was named the first head of a newly created statistics branch. Some 25 years later he was invited to serve as Chief Scientist for the 2-year period 1978 and 1979 for the Office of Naval Research in London.

In 1952, he accepted an associate professorship at Teachers College, Columbia University and was promoted to professor in 1957. This position provided him with opportunities for research in statistics in the behavioral sciences, and an affiliation with the Department of Mathematical Statistics kept him in touch with theoretical and methodological issues in statistics and probability. After a sabbatical year at Stanford, 1958–59, he was invited to serve there as chairman of the Department of Statistics. He held this post for 5 years. During his initial chairmanship the number of master's and doctoral students grew dramatically. He also was chairman from 1985 to 1988.

Solomon has enjoyed a wide variety of research interests in statistical and probabilistic methodology and in their applications to engineering, the behavioral and social sciences, marketing, law, education, health and military issues. He is a fellow of both the American Statistical Association and the Institute of Mathematical Statistics, for which he served as president in 1964–65. In 1975, the American Statistical Association awarded him the Wilks Medal for his contributions to statistics, and in 1977, the City College of New York presented him with the Townsend Harris Medal for his contributions to knowledge. The Secretary of the Navy awarded Professor Solomon the Navy Department Distinguished Public Service Medal in 1978 for his research contributions and for his leadership in furthering basic research in the academic community for Navy Department programs. This is the highest civilian award offered by the Navy Department to an individual not employed by the department.

Solomon has authored or co-authored about 75 papers and several books in statistics and probability.

He married Lottie Lautman, a violinist, on 1 January 1947. Their daughter, Naomi, is a vice-president in database management for a large bank in New York City, and their two sons, Mark and Jed, are lawyers in the San Francisco area.

Paul Switzer is Professor of Statistics, Stanford University, Sequoia Hall, Stanford, California 94305-4065.

This interview took place during November 1990.

Switzer: My name is Paul Switzer. Herb, it's been my privilege as your colleague over the last 25 years to have listened to many of the very interesting and sometimes amusing stories of the statistics world as it developed from the 1940s to the present day, and I hope that you will share with the rest of humanity some of these recollections. During this interview I am going to be asking you about your earliest contacts with statistics and statisticians, your days at City College, the first time you spent at Columbia University, the people you met there and life as a graduate student there. Then we'll move on to your wartime work, your first period at Stanford, your work at ONR, your return to Columbia and then finally your return to Stanford. I'd like to begin by asking you about your earliest contacts with statistics.

Solomon: I did well in mathematics in high school, and so I chose it as my major subject when I entered City College in New York City in early 1936. This was at the height of the Great Depression, but fortunately, tuition was completely free, except for a charge of $1 a year for use of the library. In fact, in my first years there textbooks were loaned to students to be returned at the end of the semester. The National Youth Administration was a Federal Government agency with funds to help somewhat indigent students, and this translated into a monthly stipend for me in the amount of $15. It was reduced to $10 a month when a younger brother entered City College, since he was also going to get $10 a month. Admission to the college was very selective and depended very much on high school grade point average. In my class year of 1940 there were Ken Arrow and Milton Sobel and others who also went on to careers allied to ours. And interestingly enough, there is a group photo of the City College Mathematics Club taken in late 1939 in which one can find, among others, Kenneth Arrow, Herman Chernoff, Paul Burke, Lowell Schoenfeld, Harvey Cohn, Harold Shapiro, Joshua Barlaz, Milton Sobel and a number of others who went on to distinguished careers in mathematics and statistics. In those days, City College was an all-male school, which accounts for the lack of females in the group photo. Unfortunately, I was not in school the day the group photo was taken, and I have always regretted this.

A mathematics major had to complete a large number and wide variety of courses. Among these, there was one course available in "Mathematical Statistics." This was taught by Prof. Selby Robinson from a monograph by H. L. Rietz, which was the closest to a textbook at that time. J. V. Uspensky's introductory book on probability came out in 1937, and I do not recall whether this was brought to our attention immediately. Robinson had a Ph.D. in Statistics from Iowa.

FIG. 1. *Herbert Solomon at age 50.*

I was one of a very large number of students over the years who were introduced to mathematical statistics in this way. Some noncalculus statistics courses were offered by Economics faculty under a somewhat interesting title of Unattached, with a capital "U." These courses were typically taught by John Firestone of the Economics Department, who also brought Statistics to the uninitiated in this way. The book used in these courses was authored by F. C. Mills; another popular elementary text was one by Croxton and Cowden. At any rate, Robinson and Firestone kindled my interest in Statistics and a desire to continue its study in graduate work. And I know that they did this for a large number of students. I would like to mention here my thanks to the public education system in New York City, which provided a free and excellent education through college.

Switzer: This interest in statistics, kindled by Firestone and Robinson, induced you to enroll in a graduate program in Statistics at Columbia University. Can you tell us about that?

Solomon: I entered the graduate program in Statistics at Columbia in 1940, essentially the end of the Depression and the beginning of the defense preparedness era in this country. My choices were quite limited, mainly for financial reasons. I had heard of Iowa and Iowa State, but Columbia was in the heart of New York City and accessible by public transportation. Despite its proximity, the tuition per year in which a Master's degree could be earned was $400, an exceedingly large sum to my family. However, both my father and mother were insistent on my continuing my education. A loan of $400 from one of my mother's uncles made it possible for me to earn my Master's degree in Mathematics at Columbia, and I also, because of the exigencies involved, finished it in 1 year. During this year I recall taking 10 courses, about five each semester in Mathematical Statistics and in Mathematics. At that time, degrees were not awarded in Statistics or Mathematical Statistics even though a program existed in these fields.

Switzer: I think we would very much like to hear about the kinds of courses that graduate students in Statistics were taking at Columbia in those years and the kind of books that were used and what was demanded of students in that period.

Solomon: In that 1-year period, I took a large number of courses with Harold Hotelling and Abraham Wald and two or three courses in the Mathematics Department, for example, "Probability" with B. O. Koopman and "Integration in Finite Terms" with Joseph Ritt. Courses with Hotelling included "Statistical Inference" and "Regression and Correlation," and with Wald, the "Analysis of Variance," "Design of Experiments" and a course on Pearson curves.

Switzer: Were those courses taught from textbooks?

Solomon: There were no textbooks available on the graduate level, except possibly R. A. Fisher's early book, *Statistical Methods for Research Workers*. At times there were handouts from the instructor and of course, in practically all courses, copious notes were taken by the students. In a number of cases, these notes were bound together to serve as a textbook for future generations. Unfortunately, not too many of them came to fruition in those years. One had to wait for some time before textbooks as we know them now appeared.

Switzer: Who were some of the other students at the time at Columbia University doing graduate work in Statistics?

Solomon: Some statistical students who preceded me by several years were Edward Paulson, William Madow and M. A. Girshick. Howard Levene and Albert Bowker were roughly contemporaneous. When the Department of Mathematical Statistics was formed in 1946, Ralph Brookner was the first Ph.D. The usual graduate social groups that form today did not exist at Columbia, mainly because we were a subway school. In fact, many courses were given after 5:30 p.m. As I recall, there was usually a course from 5:30 until 7:10, and then another course from 7:30 to 9:10. This did not permit too much getting together as we now see, for example, in Sequoia Hall. I should add that at that time, the Master's degree was not the kind of terminal degree as we regard it now. It was another step on the way to achieving some reward in higher salaries or eligibility for jobs.

Switzer: Do you have any stories to tell us about, say, Harold Hotelling and Abraham Wald as lecturers and personalities?

Solomon: I got to know them as personalities much better around the middle of the 1940s, when I worked in the Statistical Research Group. During my Master's degree there (1940–41) I do remember Hotelling as a very stimulating teacher. A number of my peers did not think so because he did not seem to be organized in his lectures, but yet I found this characteristic did make them more provocative. Abraham Wald, on the other hand, was very methodical and precise in his lectures. One could almost count on the starting bell ringing when he came into class and the closing bell ringing when he left the classroom. This did not permit too much time for questions since it would interrupt the cycle he had set for himself. However, the amount of detail he poured out in that one hour was large. I still have my classroom notes from both, and I now find it much easier to go back and see what Abraham Wald did and what Hotelling had attempted. Hotelling, who lived in Mountain Lakes, New Jersey, reserved the second Sunday of each month for an at-home get together. This gave us an opportunity to meet our peers and senior statisticians in a more social setting. Hotelling and Wald were fine human beings. The former was especially supportive of those who were first-generation Americans, and I sincerely appreciated it.

Switzer: How hard did they make you work in those days? Did they pile on the homework and the examinations?

Solomon: Coming out of the City College environment it did not seem to me to be too much work at the time to take five courses in one semester. Now, I think, most of us would throw up our hands and say that's absolutely ghastly! However, the thought in my mind was to get the Master's degree as soon as possible and then elevate myself somewhat more professionally and financially.

Switzer: When you finished your Master's year at Columbia and had elevated yourself professionally, what was your next step?

Solomon: It may seem ridiculous right now, but my first job after my Master's degree was a messenger for a Federal Agency. However, this lasted just a short time because we were entering the World War II period. During the War, I found myself working for the Army Quartermaster Corps, the U.S. Army Air Force and, then, for a few years in the Statistical Research Group at Columbia. Just prior to my entering that, I

FIG. 2. *New York City College Math Club, 1940. Seated (left to right): H. Soodak, K. J. Arrow, P. Burke, L. Schoenfeld and F. Beckman. Middle row: O. Wesler, J. Barlaz, J. Engel, H. Shapiro, not known, S. Katz, M. Sobel and H. Mintzer. Back row: J. Cherry, J. Blum, S. Tetenbaum, H. Cohn, H. Chernoff, not known and S. Rosen.*

was in a similar group called the Applied Mathematics Group, both under the umbrella of the Office of Scientific Research and Development. A principal personality in this OSRD program was Mina Rees, whom I mention now because of my relationship with her a little later.

Switzer: Let me ask you a little more about the wartime work at Columbia. Who were some of the people there with you, and what kind of work were you doing there?

Solomon: Hotelling was one of the principal investigators on the Columbia contract under which the Statistical Research Group operated. It was through his good offices that I was invited to join the Applied Mathematics Group to work with Churchill Eisenhart. The immediate problem we had was in connection with pursuit curves, and let me say a word about this.

Flexible gunners in bombers were being taught sighting rules about how to aim their machine guns to knock down oncoming enemy fighter planes whose pursuit path was determined by aiming its fixed guns at the bomber. The rules given to the flexible gunners, if executed perfectly, would by our analysis result in no hits. This suggested some drastic modifications, and so quite a bit of effort was given by us and others in similar programs for determination of the best possible rules that would still be simple and that would be effective. To do this we had to look into a notion of the pursuit curve. Interestingly enough, an early paper on pursuit curves appears in the 17th Century in a French journal in connection with a privateer chasing a merchantman, namely, what would be the actual course of the privateer. Of course, in our case, we were also interested in some stochastic elements. One point I remember vividly was the fact that in all the earlier U.S. work in World War II no account was taken of the speed of the forward motion of the bomber and adding it to the path of the machine gun bullet fired at the fighter plane. Once this was done, things got much better. In fact, rules were suggested that included aiming in back of the fighter and that it wasn't just like shooting a duck. The British had already accounted for this by 1942. We also did some data analysis of cargo ship tonnages lost to submarines in the North Atlantic. In addition to this, we also did some work on the statistical analysis of accuracy of height finders and range finders. In fact the first paper I wrote, with Churchill Eisenhart as co-author, had to do with an extension of Cochran's test for equality of variances which was an example directly of interest to us on these devices. Interestingly enough, Michael Stephens and I extended this work of 45 years ago in a paper published in the *Journal of Industrial Quality Control*, 1990.

Switzer: After you finished your work in Eisenhart's group, you moved to another group. Can you tell us about that?

Solomon: Eisenhart and I, who were in the Applied

Mathematics Group, were moved to the Statistical Research Group. Actually, it meant no physical move at all, because we were both in the same apartment building just a block or so off the Columbia campus. This was about 1944. I'm not sure anymore. At the time we joined the Statistical Research Group it was replete with a large number of statisticians, essentially all of whom became very well-known and leaders in the field over the next 40 to 50 years. Among these were W. Allen Wallis who was the leader of the group, Milton Friedman, George Stigler, Edward Paulson, Abraham Girshick, Kenneth Arnold, Al Bowker, L. J. Savage, J. Wolfowitz and, of course, Hotelling and Wald. I apologize for any I have omitted.

Switzer: What kinds of projects were these people involved with? Was it one big project, or many small projects?

Solomon: There were a large number of projects, but I guess one that was a major effort and included a large number of staff was the newly emerging sequential analysis. In fact, there was some sort of unpleasantness about who initiated and developed the work on sequential analysis, which led to a little scurrying around to see who should get credit. But somehow or other, either it was never resolved, or the passing of time took care of it.

Switzer: Was the work of this group being published?

Solomon: At the time, essentially all of the reports we prepared were classified. In fact, it was a long time before a number of these papers were unclassified. Most of them exist now in the National Archives.

Switzer: Could you tell us a little bit about Wald's work in sequential analysis during this time?

Solomon: I was not directly involved in this effort. Wald apparently did a lot of work on it in those years, and prepared a technical report which essentially became his book on sequential analysis. Also at the Statistical Research Group another group was attempting to produce a companion volume called *Applications of Sequential Analysis*. As I recall this appeared in a looseleaf binder with each chapter discussing another application; for example, testing a binomial, or testing a difference of two binomials would be two different chapters. I should add Wald was a pioneer in operations analysis through a paper he wrote on the vulnerability of aircraft to flak damage. This paper which was originally classified was recently circulated to the public by Allen Wallis. It showed how to measure vulnerability from damage data on returning aircraft.

Switzer: What became of this group as the war ended?

Solomon: When the war ended in August 1945, the group disbanded. A few stayed on for maybe 3-6 months to finish up work and submit final reports. In fact, a book called *Techniques of Statistical Analysis*, published by McGraw-Hill (1947), in which the editors were Eisenhart, Hastay and Wallis, was one of these products. The book *Sampling Inspection*, also published by McGraw-Hill (1948), edited by Freeman, Friedman, Mosteller and Wallis, also came out at this time.

Switzer: Did the group disperse at this time?

Solomon: Yes, the group dispersed almost immediately. People went back to their home institutions if they had them. Some were not so fortunate and were looking around for jobs. For the period from about 1945 to 1950, there was probably much movement in and out of jobs. In other words, being in the group was not an automatic ticket to a job, and the job market was spotty. In my own case I had about five jobs in this period. Among the members of the group, I found any number of fascinating and bright individuals. Wallis has reported on this in *JASA*, June 1980. While we were all somewhat young, as the saying goes, "some were younger than others." Among the juniors, including myself there, were Al Bowker and Ed Paulson, plus young women in their early 20's who served as research assistants. Others who were probably 5–8 years older seemed like senior statesmen to us. Frequent lunches over a 2-year period with Wallis, Friedman, Wolfowitz and others prepared me well for future efforts in scientific administration, which constitutes part of my career.

Switzer: Well, what happened to Herb Solomon after the group disbanded at the end of the war?

Solomon: I bounced around a little, teaching at City College for a short time and then came out West in January 1947 to work on an Office of Naval Research project on sampling inspection at Stanford University. Al Bowker had taken an appointment at Stanford a few months earlier and had invited me to come along with him to keep working on the kinds of topics we had studied during the war. He arrived in the fall of 1946, and I came out in January 1947, having been married just 2 weeks earlier to a very pretty and talented young violinist named Lottie Lautman. I mention her here because she served as hostess at many statistical gatherings over the last 45 years and has been my traveling companion to many statistical meetings.

Switzer: And what was going on at Stanford in Statistics in early 1947?

Solomon: Let me put this in some context. Fred Terman, who was then Dean of Engineering, was anxious to have Statistics develop at Stanford. I believe he felt that Engineering would require modern probability theory and statistics for its graduates. And so he was very helpful in getting things started, and as you know new programs are always hard to start in universities. At the time I do remember either a formal or informal committee of individuals from various departments who served as a committee on Statistics. These included Quinn McNemar in Psychology, George Polya in Mathematics, Holbrook Working from the Food Research Institute, Willis Rich in Fisheries, Eugene Grant in Quality Control and someone whose name I cannot remember who came from Medicine. These were essentially the germs of the joint appointments which the Statistics Department encouraged all through the years.

Switzer: Well, it's clear that Stanford must have made a good impression on you because you were to return later. But first you went back East. Can you tell us about that?

Solomon: We returned East for personal reasons, but by early 1948 I accepted a post with the Office of Naval Intelligence. In the latter part of that year I transferred to the Office of Naval Research at Mina Rees' invitation. I joined the Office of Naval Research in the Fall of 1948, although I did not begin there until early 1949. At that time, Mina Rees was head of a Mathematics branch in this new fledgling agency which was to support basic research to meet the needs of the fleet and served as a forerunner to other agencies that came later in the Department of Defense and other federal agencies such as the National Science Foundation and the National Institutes of Health. In the Mathematics branch there were several programs: computers, statistics, mathematics, logistics and applications. What was interesting is that these programs were quite apart from what the mathematical community thought the Navy should be doing along these lines. Despite a lot of pressure, Mina surrounded herself with such informal advisers as Sam Wilks, Richard Courant, John Von Neumann, among others, who could bless the new Navy mathematics. These programs were to take mathematics out of the mainstream of pre-war American mathematics, and so funding for more typical efforts would suffer. Mina kept these programs going. For instance, in the computer program, a large sum over time (I believe $1 million) was going to be available to develop the Whirlwind computer at MIT which everyone hoped would be a great success. Fortunately, it was and it is now, or at least has been, on exhibit in the Smithsonian Institution.

Switzer: Did the Office of Naval Research have a large budget in those days, and how were decisions made about funding of research proposals?

Solomon: I don't recall the budget for all Math branch programs, but I believe the Statistics and Probability core program probably had over $200,000 annually, in 1948. In addition, it was possible to encourage other agencies to contribute to the ONR Math program, so that I think by the time I left in 1952 it was at least double whatever it was when I joined, mainly by a large infusion of funds from other Defense Department agencies.

Switzer: How many researchers could be supported annually from this budget?

Solomon: A large budget in those days, 1948, might be on the order of $40,000 a year. A few were about this size at the time. Funds were available for students and summer faculty support. There was the Wald program at Columbia. Of similar magnitude were the Wilks program at Princeton, the Neyman program at Berkeley, the Feller program at Cornell, the Hotelling program at Chapel Hill and the Bowker program at Stanford. As you can see, we were essentially taking leaders in the field and encouraging research and development programs in their universities under the supposition that work in statistics and probability would serve the mission of the fleet and societal needs. Somewhat smaller programs were started at the University of Washington, University of Oregon, Johns Hopkins University, University of Chicago and others I can't recall now. In addition to what might be called these basic research programs, it was intended that these also train graduate students, in the sense that a scientifically trained force would be of much help to the country. There was an attempt by ONR and others to continue the work that had been done on sampling inspection and quality control in the war to finish up a number of unresolved problems and also put together acceptance sampling manuals to be used by the Defense Department in the purchase of their equipment. To do this, ONR, which developed the plan, was designated as the coordinating agency. Each of the three services was asked to contribute to a $100,000-a-year budget to help this along. There's a funny incident here because when the initial request was made to the Pentagon no answer came back for some time. Upon checking we learned that there had been some typographical error in the request. Instead of a sum of $100,000 it went in as $1 million a year, and the Pentagon kept asking, "What are those jokers going to do with all that money?" However, once this was brought to their attention, the money did come forward. This led essentially to having the work continue with those who had been working on it during the war, which, at Stanford, translated into a large contract to continue what had been going on at the Statistical Research Group.

Girshick and others worked on this, and in the early 50's, a large share of the Stanford effort went into sampling inspection and quality control and aid in the development of a lot of Defense Department manuals still used to this date for sampling inspection by attributes, sampling inspection by variables and continuous sampling plans. I worked on the latter with Gerald Lieberman.

In the early days of World War II there was legislative activity to bring government into science. In 1942, the first Kilgore science bill was introduced having in mind a "National Research Foundation," essentially a precursor to the National Science Foundation, except that it was geared to industrial research. Dr. Herbert Schimmel, a physicist, and later a bio-statistician, was a senior staff person to Senator Kilgore and his committee. At about the same time, Senator Magnuson introduced a competitive bill slanted more to an NSF program. The NSF-type bill had slow going and failed until years later. However, Admiral Bowen of the Office of Research and Inventions and his staff decided after the NSF bill ran aground to create a pilot NSF, that is, an ONR using $20,000,000 of unspent Navy appropriations. Both Bowen's staff and the Kilgore committee staff were in favor of this. While the NSF had many obstacles in achieving passage (it finally occurred about 1951), the ONR sailed through earlier on August 1, 1946. This made it the first of the federal funding agencies for science and engineering. It was strange that a military agency was taking on research for the nation. However, ONR jumped in quickly and established a reputation as the premier basic research agency. The other science funding agencies were still about 5 years away. Much of what happened was due to legislative analysts such as Dr. Schimmel. Others were Dr. E. Lowell Kelley, a psychologist, who served with Admiral Bowen as a Lt. Commander. A number of others were involved, and I leave it to historians of science to give more details. I believe there are written histories of this now.

Switzer: Do you suppose that the interest of the Office of Naval Research in sampling inspection was possibly responsible for the founding of the Statistics Department at Stanford?

Solomon: It definitely played a major role. Allen Wallis and Mina Rees thought that work along these lines should continue after the war. Upon his return to Stanford, Wallis did not stay long, but wheels had been put into motion to have some kind of sampling inspection contract at Stanford. However, Wallis left before the department ever got going. He had been a professor of Economics at Stanford and went to Chicago.

Switzer: Can you tell us a little about the very earliest days of the Statistics Department at Stanford?

Solomon: Bowker had come as an Assistant Professor of Mathematics in late 1946, but with an informal mandate to develop statistics. He also became the Principal Investigator of the ONR program at Stanford in Sampling Inspection. With the success of the project and funding continuing, the administration agreed to initiate a Department of Statistics. This augured well, because by the early 1950s there was another large infusion of funds from the Defense Department through the Office of Naval Research.

Switzer: Who were some of the founding faculty of the department, and some of the early graduate students?

Solomon: By the early 1950s there were Abe Girshick, Herman Rubin, Herman Chernoff, Charles Stein and, of course, Bowker. Among the early graduate students, I was one; Lincoln Moses, David Haley, Craig Magwire, Steve Allen and Jerry Lieberman were others.

Switzer: Could you tell us a little more about the Office of Naval Research in Washington, your colleagues there, and the problems and excitement of the job at that time?

Solomon: I have already mentioned Mina Rees and the Mathematics branch. Well, this quickly developed into a Mathematical Sciences division, and what had been programs before, such as the Statistics program, became branches by 1951. This made it easier for us to do our business, and our business grew rapidly. Heading the Mathematics program was F. J. Weyl who later became Chief Scientist of ONR and a Dean at Hunter College in New York City. Fred Rigby ran the Logistics branch, which provided a forum for all the kinds of mathematics that were being newly developed in linear programming, inventory control and so on. Charles V. L. Smith headed the Computer Program.

In the Mathematics branch with Joe Weyl was my colleague Arthur Grad who had obtained his degree at Stanford in Schlicht functions and whom I had met at Stanford in 1947. I mention him particularly, because I somehow or other kindled an interest in him in Statistics programs, which stood us in good stead when he became Head of the Mathematics program at the National Science Foundation in the late 1950s. Up until the time he took over at NSF there had not been much funding, if any, allocated to Statistics and Probability. I always like to believe that it was my influence that taught him to see the wonders of our discipline. He and I have a joint paper on quadratic forms in the 1955 *Annals* which arose out of some military applications.

Switzer: When did the National Science Foundation get started?

Solomon: I believe in 1951. In fact, a large number of people from the Office of Naval Research moved over to the National Science Foundation, including the Chief Scientist, Alan Waterman.

Switzer: Did they have a Probability and Statistics program at NSF from the beginning?

Solomon: No, they did not. I don't recall any Statistical grants in those years.

Switzer: When were the first NSF grants made in Probability and Statistics?

Solomon: I think they started in the very late '50s or possibly early '60s.

Switzer: So the Office of Naval Research was the principal research funding organization for a long time?

Solomon: Certainly for 5 years after which the Army and the Air Force also had Mathematical Science programs and were funding proposals in Statistics and Probability. They started a number of years after ONR did and used ONR as a model, except in one important way which I think you brought up earlier, namely, the procedure by which a proposal was approved or not. The Army and the Air Force used the National Research Council to review proposals. At the Office of Naval Research the procedure was much more informal in the sense that there was no formal screening committee or approving committee except of course for the upper echelons in the agency itself. That meant each program monitor had a lot more to say in what should be funded and what should not be funded. This may have defects as well as advantages, but this is the way it did operate. I think it operates in similar fashion today.

Switzer: Was it very competitive? Was it difficult to get a grant for someone who was active in research in those days?

Solomon: Strangely enough in the early days, several people had to be coerced into accepting grants, as I recall. There was a concern by some that if a grant were approved and initiated and that if at the end of a year or two, it was terminated, there would be staff to pay and no funding to pay for that staff. In some cases universities would not provide adequate space for the research effort. So there was really a reluctance by some to even get involved with, what were then, very new programs in the Federal Government. It's interesting that they would feel this way because some years earlier, if we go back to the middle of the 19th Century, the government was funding university programs and building up A&M universities to improve agriculture and the mechanic arts. In fact, only too few of us know, one of the earliest of these statistical research centers is at Iowa State, which was funded by the Federal Government through these A&M programs. One of the early elementary books in our field, still around in some form or other, was written by Snedecor and Henry Wallace, who later became Secretary of Agriculture and Vice President of the U.S. In fact, what we now call Snedecor and Cochran derives from that first book by Snedecor and Wallace.

Switzer: Let me ask if you have any interesting acecdotes from those ONR years in Washington.

Solomon: One thing that comes to mind was a trip I made to Berkeley, probably around 1950, to see Jerzy Neyman and others. I recall letting him know that one of the technical reports he had sent in contained some results which I sent on to a Navy group which then had used the procedure and found it excellent. I just wanted him to know that, thinking that this would be good for him to hear. When I told him this, he looked at me wryly, and said if I would give him the name of the author he would see to it that he was fired immediately.

Switzer: Why was he so upset?

Solomon: I think he was putting me on. Because he certainly was one who did give a lot of attention to applications. As I recall, people from his group did visit Navy installations, and no doubt some of his reports which I have now forgotten have derived from problems that were posed to him on these Navy trips. We should recall that in the 1930s what is now known as Neyman allocation in stratified samples was developed by him in response to a query. Everything was not all roses. There were a number of universities, usually not as well developed in statistics as the ones I've referred to, who were interested in getting going, and who felt they were unfairly being squeezed out. This came to my attention in not a nice way for me, when I was at a meeting in Black Mountain, North Carolina, sometime around 1950, and I was on a program in which I was supposed to talk about the agenda of the Office of Naval Research. When I got through with my talk, the chairman asked if there were any questions, and a gentleman and colleague from a university in Virginia got up and asked why was it that his university, among others, had never received any funds. He, in fact, didn't wait for any answer, but immediately gave four reasons, each of which did not make me look too good as to why we were not funding him. Since this was embarrassing to me, I called upon some piece of trivia from my youth, and looked at him and said, is it or is it not true that in 1861 your faculty took an oath to overthrow the U.S. government by force, and that it's never been rescinded?

At ONR we were always subjected to some opposition from within the Navy itself. There always were attempts to hit our budget, even though we came under a different statute than the other research agencies in the Defense Department. I recall once there were some complaints from the Chief of Naval Operations, possibly through their Operations Evaluation Group. An admiral was assigned to look into our programs as a committee of one and make recommendations as to what should be done with ONR. Now, you may think it a little odd that just one Admiral was assigned, and not a committee. The admiral's name was Badger, and I believe he was the only admiral who won the Congressional Medal of Honor while he was an admiral. This caused some concern around ONR, and the Chief of Naval Research, who was an admiral, but somewhat junior to Admiral Badger, began to get a number of us together to prepare some dry runs in anticipation of Badger's visit. Well, the Chief of Naval Research came to the Math program, and selected a subject in a big book he had, and said, "topology." "O.K., now, Joe," speaking to Dr. Weyl. "What are you going to tell Admiral Badger if he asks you about what topology is doing for us?" Joe spoke, "Admiral, I would put it this way. Suppose you plant a seed, a seed sprouts roots, the roots have rootlets. Then there's a tree that starts growing. There's bark and then there are branches, from the branches we get branchlets, and so on to the notion of research spawning everything." The Chief of Naval Research looked at Joe Weyl and said, "You can't talk that way to Admiral Badger. He'll think you're crazy. You better change that; you better think of something else just in case this comes up." Well, there came this great day when we appeared before Admiral Badger. And he said, "Now, Dr. Weyl, what can you tell me about this program here, topology, and what it means for the Navy?" Before Weyl could answer, the Chief of Naval Research jumped in and said, "Well, suppose you plant a seed . . . and the roots grow up, and the roots have rootlets growing up." He had the whole tree upside down. As he kept talking, this admiral kept staring at him. Finally, he just quietly stopped talking as Admiral Badger kept staring at him. The admiral looked at Joe Weyl, who, in his precise, multisyllabic yet flowery prose for which he was well known, regaled Admiral Badger. When he finished, Admiral Badger looked at Joe Weyl. "Tell me, young man, have you ever sold refrigerators?" Joe Weyl said, "No." "Well, you would make an awfully damn good salesman." Admiral Badger then wrote a report saying, "You don't want to get rid of ONR. You need more of them." A few years ago, I had a visit from someone who was writing a history of the Office of Naval Research and asked had I ever heard of the Badger report? Had I ever heard of it? Darn right, I heard of it. I told him the story. Apparently it had not been found. But I don't know if it ever came out anywhere. There is a book on the history of ONR. I believe I have already mentioned some anecdotal accounts of my career in ONR that may have been interesting or, at least, silly. Perhaps I should also talk about another incident which had to do with some work that Z. W. Birnbaum was doing at the University of Washington. Birnbaum was interested in getting the exact percentiles for Kolmogorov-Smirnov tests when the sample sizes were small. In order to do this, he needed some computer resources. We are now talking about the period of the early 1950s. I knew that there was a very good computer that was controlled by the Air Force at the Pentagon. And so I got in touch with my colleagues in Operations Analysis in the Air Force to ask if they could help out in having some of this work done on their computer. They liked the idea very much, and so we sent it over. Unfortunately, much time passed without any kind of response coming back. When I looked into it, I learned from my colleagues in the Air Force that the whole program had been held up by several senior Air Force officials because they were wondering why we were trying to do things to help Russians. Fortunately, cooler heads prevailed and the work was done, and I believe it appears in an article by Birnbaum in the *Annals of Mathematical Statistics* in the early 1950s.

Shortly after the outbreak of the Korean war (June 1950), I developed a program to preserve the identity and functioning of some groups in Mathematics and Statistics for work on problems stemming from direct military applications. A Joint Services Advisory Committee for Applied Mathematics and Statistics was established to administer the program. This committee consisted of two delegates from each of the services, and I served as the first chairman. The manner of organization was somewhat similar to the way in which the Applied Mathematics Panel groups served the OSRD during the war. We were faced with two issues. One was that the country was under partial, rather than full-scale, mobilization, so that scientists did not feel obligated to participate. In addition, many scientists were participating in military research but were being compartmentalized so that their talents could not be brought to bear on either more global or more dramatic problems then facing the nation. The original groups were formed at Princeton, Chicago and Stanford. At the end of hostilities in Korea, I believe that Princeton dropped out, and Chicago and Stanford continued in this program. Also other universities have been added. Over many years, a number of interesting problems have received resolution and an appreciable number of graduate students have been exposed to and worked on these problems. Over the years, a large number of agencies of the three services have provided problems or areas of research.

About 1949 there was a growing desire to continue the research in quality control and sampling inspection that had been ably started under OSRD and continued in a very small way at Stanford, but the vehicle for doing this required some attention. The Research and Development Board and the Munitions Board were the agencies who were relevant on the Defense Department level, but the funds to do the job had to be supplied by the services. There was a lot of leg work by me followed by discussions I organized among Navy, Air Force and Army groups which resulted in a directive by RDB that joint services research be established. Finally, each service put up about one-third the cost. The original annual level was about $90,000 (I believe the Navy allocation came in $10,000 chunks from three Navy bureaus). This program continued up until several years ago. The results of the research of this program have been implemented several times in catalogs promulgated by the Defense Department to be used in inspection procedures for items procured by Defense Department agencies. I don't recall all that have been issued, but there is one on variables sampling called Mil. Std. 414 and one on continuous sampling called H-106, both of which have been issued by the Office of the Assistant Secretary of Defense (supply and logistics). These catalogs are all based on techniques and procedures developed under the program. Other research reports have been valuable to specific service agencies.

I was at ONR in its very early days, now some 40 years ago. It was a very exciting environment. Each of us, at least in the Mathematical Sciences Division, were given much freedom by Mina Rees. This permitted me to deal with other programs in ONR and programs in other Defense agencies. About 1950, together with the Naval Air Branch we jointly supported an initial contract in operations analysis at Lockheed in California. This may well have been the first federally sponsored contract in what is now an ever burgeoning field. I also had close ties with the Psychology Division, especially in psychometrics studies. I dealt with John Wilson, who subsequently became a Deputy Director of the National Science Foundation and the President of the University of Chicago.

Switzer: How long were you at ONR altogether? I understand you also had connections with ONR later in your career.

Solomon: I was at ONR about 3½ years after spending a year in another Navy agency, the Office of Naval Intelligence, which gave me about 4½ years of full-time work for the Navy Department. When I left in 1952 to go to Columbia, I did not realize that my career was not ended there, because some 25 years later I was invited to serve as Chief Scientist for the Office of Naval Research branch office in London, where I spent 2 years. It was a privilege to serve overseas, and it gave me a chance to see firsthand what was going on in Statistics and Probability in all of Europe and the Mid East. While I did not get to Iran myself, some of my colleagues in other disciplines did get there just as things were beginning to get pretty hectic politically. I should add that I was active in ONR research programs in between my assignments with ONR directly.

Switzer: Let me now ask you, Herb, about your career after you left ONR. I believe you left ONR to go to Columbia.

Solomon: In 1952 I accepted an appointment in Teachers College at Columbia. A major part of my effort was dealing with students and scholars in education and psychology. However, at this time, I also spent quite a bit of time with the Department of Mathematical Statistics. In fact, I was trying to bring the two groups together to do what we would call in those days, and I believe it is still called now, Mathematics in the Social and Behavioral Sciences. In fact, what happened there led to a number of things of interest to me to be worked on again when I returned to Stanford in the late 1950s. We had a project at Teachers College with the U.S. Air Force School of Aviation Medicine that included Gustav Elfing, Raj Bahadur, Herb Robbins, Ted Anderson, Al Bowker, Rose Sitgreaves and Howard Raiffa.

Switzer: What was the nature of this project, and was it a long-term project?

Solomon: The project actually outlasted me at Columbia. The Air Force Contract had to do with looking into item analysis – essentially, how many items should there be on a test to classify an individual, and what should the characteristics of those items be? It was the kind of activity indulged in by the Educational Testing Service, with whom we tried unsuccessfully to coordinate seminars. One result of this project was a book I edited, *Studies in Item Analysis and Prediction* (1960), which had chapters by essentially all the above named individuals, including me. More directly on the behavioral and social science front, we were fortunate to secure from the Psychology branch of the Office of Naval Research some funds to do mathematics in the social sciences. Here, one of the chief architects was Paul Lazersfeld in the Sociology Department at Columbia. Others involved were Ted Anderson, Ernest Nagel in Philosophy, Bill Vickery in Economics. One of the earliest workers on this particular project in the mid-1950s was Duncan Luce. Another young researcher on the project was Jim Coleman, who in later years prepared the well-known report on high school achievement. The project on behavioral and social science research led to the publication of three volumes: one by Luce and Raiffa on games and decisions, and two others that looked into a number of topics in these fields – one edited by me and one edited by J. C. Licklider of M.I.T. The book I edited contained discussion by me on factor analysis, Adams on utility, and Coleman on group performance, all under the title *Measurement of Behavior.*

Switzer: Can you say a little bit about your connections with the Mathematical Statistics Department at Columbia?

Solomon: Informally, we had a very close relationship. In the formal way, I gave some courses in the department, and also, as can be seen from what I have already discussed, a number of individuals in the department became research associates on projects at Teachers College.

Switzer: Was Herbert Robbins at Columbia at that time?

Solomon: When I got there in September 1952, Robbins was not yet at Columbia. However, there was some recruiting for another appointment in the department, and I recommended him very highly for the post. From what I knew of his work, it seemed to me he would make a good appointment, especially since he would answer the concerns of a number of individuals in the Mathematics Department who felt that the only individuals who could receive the appointment in Statistics were Doob and Feller. I recall Robbins putting together all his work in probability and marching it over to B. O. Koopman's office to reassure him that he really knew some probability and could teach the course.

Switzer: Who were the other people in the Columbia Department of Mathematical Statistics at that time?

Solomon: As I recall, it was a very small department, consisting of Henry Scheffé, Ted Anderson and Howard Levene, and visitors. When I left, some 7 years later, it was the same group, except that Robbins had been added. Hotelling had departed in 1946. Wald died in 1950. So, in a sense, Robbins was filling the Wald post. I believe Henry Scheffé left in 1953, and I do recall how happy he was about the publication of his analysis of variance paper in *Biometrika* at that time.

Switzer: Can you expand a little bit on your role at Teachers College?

Solomon: I was brought in in 1952, 5 years before Helen Walker was to retire, to serve as her replacement. My immediate predecessor, as it turned out, was Lincoln Moses, who had spent 2 years at Teachers College, and then gone to Stanford. There seems to be a Solomon-Moses package that travels around the country. Helen was quite interested in bringing everything new and most modern in Statistics to problems bearing in Psychology and Education. She was very proud of her efforts, and her elementary textbooks to this day are exceedingly useful in non-calculus statistics classes. In fact, I think they are still used right here at Stanford. She had spent some time in London with Neyman and Pearson and Fisher, as I recall her circulating amongst them and having tea on different floors depending on which one she was visiting. In the 1930s she was on leave and spent a year, at least 1 year, in London with the great statistical scholars of the day. She was always very proud of the fact that she had been elected President of the American Statistical Association in the 1940s. I seem to have been ahead of my time because in two very important posts my bosses were women, namely, Mina Rees at the Office of Naval Research and Helen Walker at Teachers College, and I don't think I suffered from it. Perhaps they did.

Switzer: Do you recall who some of the students were at Columbia during this period?

Solomon: In the late 1940s and until the mid 1950s, there were a number of present-day distinguished statisticians who were getting their Ph.D.'s at Columbia, for example, Bob Bechhoffer, who spent most of his career at Cornell, Bill Kruskal, who spent his career at Chicago, Jack Keifer, who was mainly at Cornell and briefly at Berkeley, Richard Savage, who spent his career at Yale. I should not neglect Milton Sobel, who actually had started with Wald and may have finished it with him, but he also emerges in this period. There were a number of others.

Switzer: You left Columbia in 1958, I believe, to come to Stanford. Can you tell me the circumstances of that change?

Solomon: In 1958–59 I spent a sabbatical year at Stanford, and was never to return to Columbia, for in September 1959 I assumed the post of Chairman of the Department of Statistics at Stanford. I replaced Al Bowker, who had been Chairman since the late 1940s. Bowker had beome Graduate Dean and could no longer serve in both posts.

Switzer: Can you tell me how the group of statisticians at Stanford had changed from the first time you were there in 1947 until the time you returned in 1958?

Solomon: Well, Girshick had died a few years before, and this was a tragedy for the department and a personal loss. There were some new appointments, namely, Vernon Johns and Rupert Miller, who actually came the same year I did. Mannie Parzen came around the mid-1950s. Chernoff and Stein were the seniors in the department. Sam Karlin had a half-time appointment in the department, the other half being in the Mathematics Department. There were also Gerry Lieberman and Lincoln Moses, both of whom were following in what became a Stanford tradition of having as many joint appointments as possible—Lieberman in Industrial Engineering and Moses in the School of Medicine. Rupert Miller also had a joint appointment with the School of Medicine, and, of course, I have already mentioned Karlin as being half-time in the Mathematics Department. These joint appointments have flourished, and there is a rather healthy contingent now in the School of Medicine that derives from the original Moses appointment. In fact right now we have Efron half-time in the School of Medicine and Iain Johnstone half-time in the School of Medicine. As in any number of universities, there were other statisticians on campus, for example, Quinn McNamar in the Department of Psychology, Kenneth Arrow in the Department of Economics, Pat Suppes in the department of Philosophy. Along these lines I recall a department meeting at Columbia once where some individuals were being considered for appointment to the Statistics Department. Robbins in his inimitable manner said that he had looked around the university and noted that every department had a statistician, and he felt that it was about time that the Statistics Department hired one.

Switzer: Could you say a little about Stanford's statistics programs at the time you got there in the late '50s, both its undergraduate and graduate programs?

Solomon: The department had a lot of vitality when I arrived. It had profited from government funding for a number of years, which made it possible for us to have Ph.D. graduate students and many visitors. In fact, we always prided ourselves on the number of

FIG. 3. *1960 Berkeley Symposium at Stanford. Seated (left to right): E. Cramér, A. Spacek, A. H. Bowker, J. Neyman and G. Polya. Standing: M. Fisz, H. Solomon, L. Solomon, H. Cramér and H. Hotelling.*

visitors and the international coloring they provided, both in the academic year and summers. It was hard at one point to go around the world and find someone who had not spent some time at Sequoia Hall. This was a period on the American scene when Mathematics Programs and universities were in a kind of turmoil because World War II had brought about the existence of other mathematical disciplines not usually in the sway of the American mathematical establishment. I may have mentioned earlier that at ONR we felt the impact of this in the sense that the establishment was very concerned about the fact that we were doing finite mathematics and statistics and inventory control, and so on, and not doing very much in terms of funding pure mathematics. In fact, as time went on, we funded less and less pure mathematics, especially with the arrival of NSF. This led to some problems at Stanford over the next few years, as it did in other universities around the country. Another factor that may have led to turmoil was the fact that we had gotten a lot of government money in the 1950s, and this outside funding increased to about $500,000 in the early 1960s. It was a period when government monies were becoming more abundant, and those who were aggressive and had the talent and the credibility could tap into these funds.

Switzer: Did the flow of funds during this period create any special problems?

Solomon: I think it wasn't so much the amount that created problems as the fact that the Statistics Department became the headquarters for the kinds of things that might have been in the Mathematics Department or other departments but in many cases turned out not to be so. This was true around the country. For example, the work we were doing in Operations Research, Mathematics in the Social and Behavioral Sciences, had tugs on us that made it difficult to run a narrow Statistics Department. So questions of new kinds of administrative machinery were always present. Should Operations Research be another department? Should Computer Science be another department? These led to all kinds of, at best, discussion, and, sometimes, hostility, which fortunately does not exist anymore.

Switzer: I believe you were Chairman for about 5 or 6 years when you first came to Stanford. Can you tell us a little bit about some of the new people that you brought in and some of the changes that were made in the department?

Solomon: Because of the turmoil I just mentioned in about 1961, Karlin transferred to a full-time appointment in the Mathematics Department. Also Chung, who had just been appointed, decided to become a full-time appointment in the Math Department, rather than any kind of joint appointment. An appointment in that period was Ingram Olkin, who held a half-time joint appointment with the School of Education and does to this day. This was part of a build-up in social science that also brought in Richard Atkinson, who is now Chancellor of UC San Diego. From the inception of the department, and for a while, we tended to appoint our own Ph.D.'s to the department, for example, Moses, Solomon, Miller, Efron. Also, in this period we added Paul Switzer on a half-time basis, joint with the School of Earth Sciences, a post he holds to this day. We have a policy of rotation for Chairman in our department, and after a number of years as a regular faculty member, I was asked to serve as Chairman again for a 3-year term in 1985. During the 20 years between my two periods of chairmanship, I was essentially the typical faculty member doing teaching, research and developing Ph.D. students. I feel very happy about the fact that I did have about 20 Ph.D. students.

Switzer: Can you tell us about some of your students during this period?

Solomon: Some of my students are now out in the university world, for example, John Lehocsky, who is Chairman of the Department at Carnegie, Alan Gelfand, who is a senior person at the University of Connecticut, Andy Siegel at the University of Washington, Ed George at the University of Chicago, Tony Kuk, University of Hong Kong, Satish Iyengar, University of Pittsburgh, Fred Huffer, Florida State, Donald Hoover, Johns Hopkins, Cliff Sutton, George Mason University; others are in government and industry. Paul Zador wrote a thesis on quantization (1964) that serves as a pioneering paper for electrical engineers. It was published some 20 years later after being referred to constantly as an unpublished paper.

Switzer: What about some of your research collaborators during this period.

Solomon: Over the years, I have had a number of visitors on research contracts and grants, and one who has become almost a perennial visitor here is Michael Stephens of Simon Fraser University. As a result of his visits, we have a number of joint papers, and I see no reason for the relationship not to continue. Other visitor collaborators are Mark Brown, Alan Gelfand, Shelly Zacks and Don Jensen.

Switzer: You served a term as president of the IMS. When was this?

Solomon: I served as President of the Institute of Mathematical Statistics in 1964–65. There were two issues at the time. One concerned the relationship between the institute and the American Statistical Association. For example, should the societies merge, or continue as they were? Not much of anything was done on this point. By the early 1960s, several successful IMS regional meetings had been held in Europe. Several European statisticians objected to an American association essentially invading their turf. The IMS had stressed its international flavor, but this was not selling. At a regional meeting in Berne in 1964, I met with David Kendall and David Dugúe and urged the Europeans to hold their own meetings. I believe this led to the organization of the European Meeting of Statisticians. It is interesting to note that the 1990 annual meeting of the IMS was held in Uppsala, Sweden.

Switzer: I now would like to ask you about the main themes of your research career.

Solomon: As I look over my work in the past, which has led to about 75 papers and several books, I note that my interests are rather eclectic, and I am motivated usually by problems that are brought to my attention, that is, applied problems, rather than extending theorems. In this way, I have gotten into geometrical probability, usually through some military or biological applications, the distributions of quadratic forms, acceptance sampling models, statistics in legal settings, clustering and classification. I became interested in statistics and law, not because of any specific problem thrown at me in the legal setting, although I have had a number of these. It began when I served as a foreman of a grand jury in Santa Clara County, and I kept asking questions about why are we 23, and why are juries made up of size 12? To look into these led to some model building, building on Poisson's work in the early 19th Century, and also led to some papers very early on about measurement of evidence in essentially, well, criminal and civil cases. In fact, a very early paper called "Jurimetrics" appears in the Neyman Festschrift volume edited by F. N. David in the early 1960s, which seems to be overlooked by those who are always talking about the first papers in the subject. Another way I have run into problems to look into has been through consulting. Over the years I have gotten involved in, in addition to any number of contacts with lawyers, problems posed to me by people in marketing and advertising, which led to a number of multivariate analysis papers on classification, clustering and factor analysis. My work on continuous sampling derived directly from specific queries raised by people in the Department of Defense to look into the work of Harold Dodge on continuous sampling and, if possible, to extend it, which Gerry Lieberman and I did do. Another area of consulting was provided by problems arising in pharmaceutical research, mainly getting new drugs approved by the Food and Drug Administration. This kind of work has died out because the regulatory attempts by the Federal Government have diminished, throwing out of work not only statisticians but lawyers.

Switzer: Well, Herb, this has been a fascinating review of a lifetime involvement in the Statistics profession, and I would like to ask you now what advice do you have for the younger members of our profession?

Solomon: Statistics is so pervasive in modern life and science that it can only get bigger and bigger and more and more of us will be required. It will be interesting to see the future development of computer-intensive procedures such as the "bootstrap" and where work on Bayesian procedures will lead us. Each of these efforts will thrive if employed in a wide variety of applications.

Switzer: I am also interested to hear your opinion about future prospects for the funding of Statistics research in this country.

Solomon: The National Science Foundation and the National Institutes of Health loom in the near future as those who will provide most funds for statistical research. I am a little concerned about the former because the NSF does not have a mission in the typical sense, as the National Institutes of Health and, say, the Office of Naval Research has, where one is interested in medicine and public health for the U.S. population and the other is interested in the mission of the fleet and the national security of the nation.