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A Conversation with Albert H. Bowker

Ingram Olkin

Albert Bowker was born in Winchendon, Massachusetts, on September 8, 1919. He received a B.S. in Mathematics from MIT in 1941, and a Ph.D. in Mathematical Statistics from Columbia University in 1949. He was on the Stanford faculty from 1947 to 1963, serving as founding Chairman of the Statistics Department and Dean of the Graduate Division. In 1963, he became Chancellor of the City University of New York. He returned to California in 1971 as Chancellor of the University of California at Berkeley. In 1980 he was appointed as the first Assistant Secretary for Postsecondary Education in the newly formed U. S. Department of Education. In 1981 he went to the University of Maryland as founding Dean of the School of Public Affairs and later became Executive Vice President. In September 1986, he returned to the City University of New York, and now serves as Vice-President for Planning of its Research Foundation. In 1961–1962, he was president of the Institute of Mathematical Statistics, and in 1964, president of the American Statistical Association. Honors include the Frederick Douglass Award of the New York Urban League; the Medal for Distinguished Service of Teachers College, Columbia University; Shewhart Award of the American Society for Quality Control; Berkeley Citation; Distinguished Public Service Award, Department of the Navy; Order De Leopold II; and honorary degrees from the City University of New York, University of the State of New York (Regents), Brandeis University and Antioch University. He has been a member of the boards of various professional and educational organizations including MIT, the University of Haifa and Bennington College.

The following conversation took place in his home in Washington, D. C. in October 1986.

I STARTED AT MIT

Olkin: Al, perhaps you can begin by telling us about your statistical background before and after your education at the Massachusetts Institute of Technology.

Bowker: In June of 1937 I graduated from Woodrow Wilson High School, which is a block and a half from where we are now sitting in Washington, and that fall enrolled in MIT as a freshman. My original idea was to become an engineer of some sort, although my father and most of his friends had been research scientists at the National Bureau of Standards. But finally, discouraged largely by a drafting and chemistry laboratory, I decided that I wasn't inclined enough mechanically to become an engineer. I then transferred to mathematics.

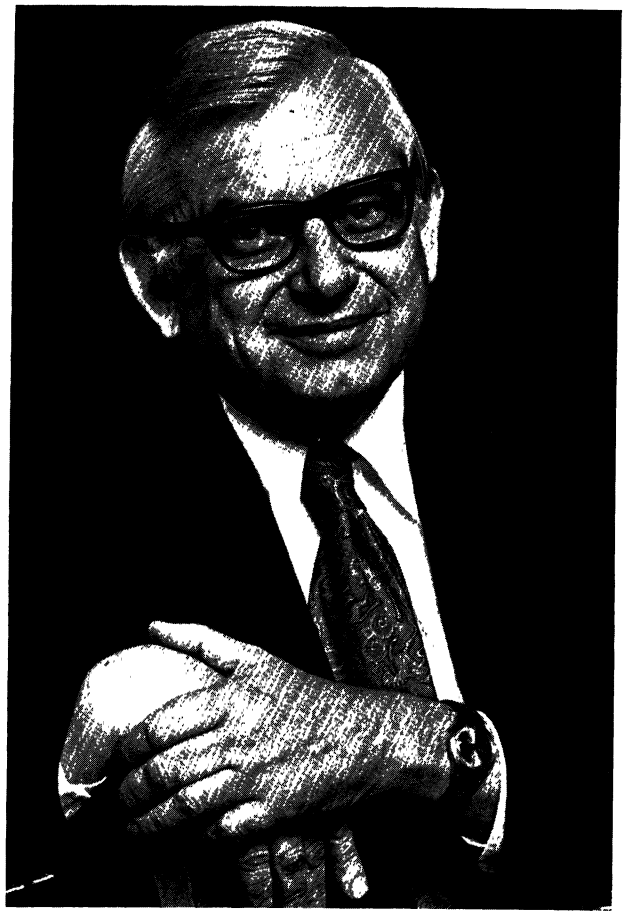
It was very pleasant at MIT because, although mathematics is a very large major today, there were only a few people in my class. Actually, mathematics had graduates every now and then in those days rather than every year. So by transferring as an undergraduate to mathematics, I became part of a small community and was entertained socially by the faculty; I had a small office as a junior.

Olkin: Who were the faculty at that time?

Bowker: The people in statistics were George Wadsworth, who was in the mathematics department, and Harold Freeman, who was in the economics department. I became quite close to both of them as well as with Ken Arnold, who had just finished his doctorate and was an Instructor, and with some of the other mathematicians.

Olkin: Was Norbert Wiener there at the time?

Bowker: Wiener was there, and I took a course from him as an undergraduate. Although I never understood it, I got an A in the course. But it was really Freeman and Wadsworth who interested me in statistics.



Albert H. Bowker, 1980

THE STATISTICAL RESEARCH GROUP AT COLUMBIA UNIVERSITY

Bowker: After I graduated in 1941, I went to work at MIT on a military project, which was trying to use statistical methods for weather forecasting, and in fact was exploiting some of Wiener's ideas of prediction. When it came right down to it, however, these methods were essentially the same as multiple regression. Although I enjoyed working at MIT and in Washington at the Weather Bureau, I became discouraged about the project and moved to the Statistical Research Group at Columbia in 1943.

I must say that the Statistical Research Group (SRG) probably had a major influence on my thinking and career, because I believe it was the most distinguished and creative collection of statisticians ever assembled.

Olkin: For the record there were two statistical research groups, one at Princeton and one at Columbia. Can you clarify that?

Bowker: Yes. We never referred to Princeton as SRG although officially it was SRG Princeton and we were SRG Columbia.

Olkin: So the major center was at Columbia.

Bowker: Yes. The Columbia group was much bigger. For example, Fred Mosteller, who was associated with SRG Princeton, also had an office at Columbia. There was considerable cooperation between the groups.

Olkin: What was the charge to the Statistical Research Group?

Bowker: SRG was set up by the Applied Mathematics Panel of the Office of Scientific Research and Development. There is a very good history by Allen Wallis in the June 1980 issue of the *Journal of the American Statistical Association.*

We worked on military problems that were referred to us mostly but not exclusively by the Navy. I worked a lot on methods of firing various weapons, aerial torpedoes and bombsights.

Olkin: Was this with Harold Hotelling at the time?

Bowker: The three figures who formed the group were Hotelling, Allen Wallis and Jack Wolfowitz, but Wallis was the Director and real spearhead. The other members comprised a statistical 'who's who': Abraham Wald, Churchill Eisenhart, Jimmie Savage, Milton Friedman, George Stigler, Abe Girshick, Ken Arnold, Harold Freeman, Herb Solomon, Ed Paulson, Millard Hastay, Rollin Bennett.

It was a great experience to be able to see significant applied problems analyzed by the best theoreticians in our field given the best available research support and computational facilities. Computing facilities in those days consisted of rooms full of people pounding Fridens, Monroes and Marchants, and my responsibilities included supervising this activity. Most of the young women who worked for us were either from Hunter, trained by Hobart and Jewell Bushey, or from Vassar trained by Grace Hopper. SRG was an open-door operation. I could drop in and talk informally to people who were then the leaders of our field or many who would later become leaders, my contemporaries. And in many ways, the atmosphere at SRG contrasted sharply with the formality of Columbia University as a place to study. SRG was a good model later on for the statistics department at Stanford, hopefully having a series of problems come in from either government or industry, having enough space so that all of the young scholars, graduate students and the faculty could be housed in the same building, easily accessible to each other. I think in the early days at Stanford's Sequoia Hall and before that at the Knoll, we came close to achieving the kind of environment that we had at SRG.

Olkin: Al, let me interject one question here. Physically, was SRG housed separately from the statistics group that was later to become a department at Columbia?

Albert H. Bowker in high school, 1937

Bowker: Oh yes, we were housed at 401 West 118th Street, whereas the statistics department was housed at Fayerweather Hall on campus.

Olkin: So that was close geographically, but off campus.

Bowker: Yes, but both Hotelling and Wald probably spent more time with the SRG than they did in their offices at Fayerweather Hall.

Of course, all those other people were at SRG, so it was certainly the center of gravity of statistics. In fact, students who weren't involved with us were kind of shortchanged at the time. This was wartime and there were lots of other groups around us. We also had a strong group of mathematicians in our building and next door. They had no relation to us. It was just another group of the applied mathematics panel. And we collaborated with them. I worked with Jim Stoker, who at the time was a member of that group rather than Courant's. We worked on several problems, particularly measuring the characteristics of the evasive action of ships bombarded by aerial torpedoes.

Olkin: What was happening in New York at that time? Was there a statistical community or was the activity mostly at Columbia?

Bowker: I don't recall too much activity elsewhere. While I was at Columbia I went down to the New School to hear Richard Courant lecture several times. But he was giving a course on calculus of variations, aimed mostly at teachers. It had very little to do with statistics.

Olkin: Were you formally a student at Columbia during this period?

Bowker: Yes, but I didn't really do anything more than take courses. All the courses then were given in the late afternoon. But when the war was over, I received a National Research Council fellowship to study full time during the academic year 1945–1946 at Columbia. I think that Jack Wolfowitz was on the faculty during those years as well. P. L. Hsu, in particular, was on the faculty, and I started to work on a dissertation with him. He was a great lecturer and one of the clearest expositors in our field. In the fall of 1946 he moved to Chapel Hill, and I moved down there for a quarter. I was also at North Carolina State University in Raleigh the summer before. This was an exciting summer program and a lot of my fellow students from Columbia attended. R. A. Fisher, among others, lectured for the summer. We had an opportunity to hear ideas from the great man. Since we had taken statistical inference, which was based on the Neyman-Pearson theory, from Abraham Wald at Columbia, we were not as respectful

of R. A. Fisher as he might have liked. However, he managed to survive our class.

MY MOVE TO STANFORD

Bowker: In the meantime Allen Wallis, who had been director of the Statistical Research Group, had returned to Stanford where he was a member of the economics faculty. Mina Rees, who had helped Warren Weaver run the Applied Mathematics Panel during the war, was then developing Applied Mathematics and Statistics programs at the Office of Naval Research and had offered Allen a project to help develop statistics at Stanford. I have always thought that Mina and ONR have not been given enough credit for the development of mathematical statistics in this country. In most major universities it is the only new discipline (until the recent addition of computer science) added to the Arts and Science area since World War II; ONR certainly played a major role at Stanford, Berkeley, Chapel Hill, Chicago, even Princeton and Columbia. Largely at Allen's urging, the statistics community at Stanford decided to use the availability of project funds as a base for an academic program in statistics; the move was authorized by Donald Tressider, then President of Stanford, and by the academic Vice President, Alvin Eurich, who also approved an offer to me and asked Fred Terman to recruit me as Allen was moving to Chicago. The mathematics department received me with a certain detachment. Although he became a strong supporter of statistics, Gabor Szegö was then chairman of the mathematics department, and explained to me very nicely that while what I did was very interesting—it wasn't mathematics. So we moved rather quickly to a separate department.

Olkin: So the department was actually formed in 1948?

Bowker: It was announced by Alvin Eurich in 1948, and I was asked to be chairman though still technically a graduate student at Columbia. (Talk about student power!) In the meantime, President Tressider died. Wally Sterling was appointed president, and I think it's fair to say that he reaffirmed the whole decision. I find file accounts of long conversations with him. In some ways a turning point was the availability of Abraham Girshick to join the department. He was then at RAND Corporation. Girshick had a remarkable mind with a deep interest in theory, but firmly grounded in applications from his government experience at the Department of Agriculture and wartime work at SRG. He was a warm and attractive person who drew in other scholars. The Annual Report of the Statistics Department that year lists Meyer A. Girshick, Professor; Albert H. Bowker, Assistant Professor; Zivia Wurtele and Gladys Rappaport (later Garabedian), Research Associates.

Other people were also around. When I first went to Stanford, Herb Solomon came out with me for a year to help on a Sampling Inspection by Variables Project. He went back to the Office of Naval Research, and later moved to Teacher's College, Columbia University. Herman Rubin joined us fairly early on.

Olkin: Was Ken Arrow already on the faculty?

Bowker: No. Allen Wallis had been in the economics department, and I was appointed to the mathematics department. The economics department agreed to this move, as I remember it, on the basis that they wouldn't lose their statistics position. That was also up in the air for a little while, but they decided to recruit someone.

Albert H. Bowker and John W. Tukey, Stanford University, 1954

I strongly urged them to appoint Ken Arrow. There was no one comparable. Kenneth came in with a joint appointment between statistics and economics from the beginning.

Olkin: Where had Ken been at the time?

Bowker: Cowles Commission. Although I was at Stanford first, Ken was very important in the development of the department.

Olkin: Was Pat Suppes already at Stanford?

Bowker: I was not involved with hiring Pat, but I helped keep him at Stanford. He had been a student of Ernest Nagel at Columbia and was interested in logic and the foundations of physics. He didn't find much of an intellectual community at Stanford, and joined us on several projects working on decision theory and inventory models. In June of '49, Quinn McNemar was appointed Professor of Statistics in addition to his appointment in psychology.

Olkin: So that was the composition of the group. There was yourself, Ken Arrow, Abe Girshick, Herman Rubin and, to some degree, Pat Suppes and Quinn McNemar.

Bowker: Lincoln Moses was also at Stanford. He had been a Stanford undergraduate and had come back after the war to study. He was encouraged to come into the statistics department as a student. Our first two doctoral students were Moses and Solomon which may have been a good omen. In any case, both later played important roles in the department.

Olkin: One of the striking features of the Stanford department is that we have a lot of joint appointments. In fact, at one time we had 9 out of 16 faculty joint with other departments. You indicated that to some degree the model came from the Statistical Research Group. Did you consciously think of joint appointments at the time, or was it just a natural evolution?

Bowker: The idea from the beginning was to construct a research laboratory with students and faculty working on problems, many of which would come from applied fields; to treat students as colleagues, with office space; to provide first class computing facilities. Implicit in this arrangement were projects to cover computing costs and stipends for students. Also implicit were joint appointments with other departments. A large ONR project naturally evolved from SRG. It dealt with what is called variables inspection, and comprised a large part of our activity for the first few years.

The policy on joint appointments was also derived from my view that the department ought to dominate, if not be formally responsible for, all of the statistics instruction and that the instruction ought to be in the hands of professional statisticians. This had been a theme of Harold Hotelling for many years. And the easiest way to implement it appeared to be through a series of joint appointments.

In my first few years I tried to spend as much time as I could consulting with other faculty and I worked some at the medical school, which was then in San Francisco. That led to a joint appointment with the Department of Public Health and Preventive Medicine, although most of my work was not with them. It was with other scientists. Lincoln Moses was eventually brought back with a joint appointment with the Medical School.

The School of Education joint appointments came quite a bit later, when an attempt was made to reform the school as a distinguished research school. Joint appointments with industrial engineering always seemed natural because there was a strong quality control activity there.

Olkin: Was Gerald Lieberman in the picture at this time?

Bowker: Jerry came as a student. I was doing some consulting at the National Bureau of Standards and met Jerry and invited him to come as a student. He did and worked on the quality control procedures. After he got his degree in 1953, he accepted a joint appointment with industrial engineering.

David Blackwell was a frequent short term visitor and spent one or two academic years when he, Girshick and Arrow undertook their work on decision theory. This became a major activity for a number of years.

Although few recall it now, Stanford was not an exceptionally distinguished university when I first went there. It certainly was not one of America's great universities as it is today. Most people thought I was crazy in accepting a position there. I remember that one mathematician left Stanford because he thought it had no future.

Although I'm a little ashamed of it, the Berkeley oath controversy, in 1952, actually afforded us a special opportunity. Charles Stein who was on the Berkeley faculty decided that he would not return there. He went to the University of Chicago and then we recruited him to Stanford. Erich Lehmann spent a year at Stanford and also considered moving. Other events were taking place throughout the university. Paul Garabedian came to the mathematics department. Wolfgang Panofsky, later head of the Stanford Linear Accelerator (SLAC), came to the physics department. Other visitors in mathematics came from Berkeley. In particular, I remember Hans Lewy came at that time.

Although not very many people moved, the ones who did were pretty important to the university. Panofsky was to be the key figure in SLAC, which might very well have been at Berkeley.

And there was also a very substantial increase in funds from the Office of Naval Research spurred in

part by the increased military investments due to the Korean War. So when I look back on that period there were a number of incidents that may not have been very good for the country at large but that still helped us.

So Charles joined us, and then we recruited Herman Chernoff. Together with Moses and Lieberman, it gave me, at least, the feeling that we could be as good as any place in the country in our field.

I want to say, however, that the Berkeley people were very cooperative in the '50s. We had a joint Berkeley-Stanford seminar in which everyone, graduate students and faculty, went. It still exists but does not play the key role that it did then. And we did feel part of the same statistical community. Both Jerzy Neyman and I were empire builders and we saw advantages in cooperation.

Olkin: Was Samuel Karlin part of the group at the time, or did he come shortly therafter?

Bowker: Well, he must have come a little thereafter. The roster of the department in '56–'57, the year after Girschick's death and toward the end of my term as Chairman shows Kenneth Arrow, Herman Chernoff, Samuel Karlin, Quinn McNemar, Charles Stein, Gerald Lieberman, Lincoln Moses and Emanuel Parzen.

THE MATHEMATICAL SCIENCES AT STANFORD

Bowker: In the meantime, I had taken a very ambitious role of leadership in the mathematical sciences at Stanford. I was made a member of the mathematics department again and had a lot to do with its direction under Gabor Szegö and later, under Menahem Schiffer when he was chairman. The mathematics department had always been strong in classical analysis. My general notion was to build a mathematics department that emphasized classical analysis and other fields of mathematics that were applicable. I didn't use the term applied mathematics as the center of gravity for the department; I think applicable mathematics is a better word.

Before Karlin came we had brought in Charles Loewner and Stefan Bergman.

Olkin: Of course, George Pólya and Gabor Szegö were already there.

Bowker: Yes. Ivor Stakgold and Harold Levine were brought in as applied mathematicians. Halsey Royden was already there as were Paul Berg and Gordon Latta. David Gilbarg joined us. And in addition to that, I had supported Pat Suppes in his ambitions to build a group in learning theory. We first recruited Richard Atkinson, and later William Estes joined the faculty.

Olkin: I suspect that this was between 1958 and 1962. Although I visited Stanford on sabbatical in 1958–1959, I joined the faculty in 1961 together with Richard Atkinson, who was joint between Psychology and Education, and Kai Lai Chung, who was a full member of the Statistics Department. My own appointment was joint with Education.

Bowker: With Ken Arrow as a nucleus, we had really a very interesting and stellar group of mathematical economists. Marc Nerlove, Hirofumi Uzawa and Herbert Scarf were around. Harvey Wagner, who had been an undergraduate and master's student at Stanford, got his Ph.D. at MIT and came back to join the faculty.

So we had very substantial activity by mathematically oriented social scientists as well as by a group in statistics and a group in applied mathematics. These groups were loosely linked together in the Applied Mathematics and Statistics Laboratory of which I was director. The Laboratory was a kind of holding company for government projects and a unifying force in providing administrative services of a fairly high caliber compared to what most people had available in those days.

Olkin: I meant to ask you whether J. V. Uspensky, who wrote the probability book, was alive at that time? This was a name I recall from his book on probability. It was a rather unique book in containing material that is not readily found in most texts.

Bowker: Uspensky died practically upon my arrival, so I never knew him. Mrs. Uspensky was still alive, and I met her several times.

Olkin: There was another person in Applied Mechanics who was mathematically oriented. This was Stephen Timoshenko. Was he involved with your group?

Bowker: No, he didn't have much to do with us. He had a brother, Vladimir, who was a statistician and economist, I think in the Food Research Institute, and some of those people collaborated with us a little more, such as Bill Jones and Holbrook Working, among others. Holbrook was not actually part of the group, but was associated with us. I don't know why we never offered him a joint appointment since he was a strength to us. Another person in a similar relation to our group was Eugene Grant in industrial engineering.

We never had a very close relationship with the Business School. Although I was friendly enough with the Business School and played a role in several doctoral dissertations, we never got as far as a joint appointment.

Olkin: Another person who was around at this time was George Forsythe.

Bowker: Yes, in the mid 50s I joined forces with Fred Terman, a great friend and supporter since our initial interview, to organize a computer center and get an IBM 650. The computer center led by Jack

Herriott was housed in the Electronics Research Laboratory, but through various transformations was my responsibility until I left Stanford in 1963. I recruited George and I persuaded the mathematics department to take him. He was at UCLA which missed the opportunity to be a leading center of computer science, although it had a lot of money, particularly from the National Bureau of Standards. We hired George explicitly with the idea of starting a computer science department which was later formed with Forsythe as the first chairman.

I am usually given the credit for that action, correctly I think, although I find, as I reminisce about Stanford, that there are a lot of other people who take credit for the things that I think I did. I am reminded of the military saying which John Kennedy used in talking about the Bay of Pigs, "Victory has a hundred fathers, defeat is an orphan." A unique opportunity for Stanford was an enormous mathematically related center of high quality activity. We even chatted a bit about a school of mathematical sciences bringing all these interests together.

I mention all of this because in 1955–1956, I took a sabbatical and went back to Columbia where I did some research. It was during that year that I wrote a paper on a representation of Hotelling's T square and Anderson's classification statistics in terms of simple statistics. This paper appeared in the Hotelling Festschrift volume that you help edit.

It was a year of stock-taking, and I had to decide whether I saw my future mainly in statistics or whether I would go into more general administration.

I was probably offered the directorship of the Courant Institute in that year. I certainly was offered it by Henry Heald, who was president of NYU, and Harold Stoke, who was the dean (and was later to work with me as president of Queens College). I remember visiting Courant in his home, and he showed no sign of being willing to transfer authority. So if I had really pursued it, it might not have worked out. And indeed NYU's directions, although they were close to that of the Stanford mathematics department, were not particularly close to mine. However, I had a number of good personal friends there.

After thinking it over and talking to Fred Terman at Stanford, I decided to return to Stanford first as his assistant (by now he was Provost of the university) and later, when it became available, as Graduate Dean. So from 1956 or 1957 on, I was at least part time, later to be full time, in the Stanford administration.

And I made, I guess implicitly, a decision that I would look for my career in university administration. Although there was a high element of chance in all of these decisions.

Olkin: So your term as Chairman of the Statistics Department ended in 1960?

Bowker: Herb Solomon was a visitor in 1958 and he was appointed chairman the next year. I was then Graduate Dean, but when I was Fred Terman's assistant I also kept the chairmanship of the department until we could recruit someone.

I continued an interest in the mathematical sciences picture, generally. For example, in that year I had organized a committee on operations research with Lieberman, Arrow—I can't remember all of the people—Karlin, Scarf and probably someone from industrial engineering, to look at the future of operations research, which had been started as a graduate division interdepartmental committee; the committee had the authority to grant Ph.D.s but didn't have much structure, and I kept negotiating for newer and bigger machines for the computer center.

To some extent things began to fall apart in these years. The mathematics department, with David Gilbarg as Chairman, decided not to be as specialized as I had intended and branched out more into pure mathematics. Although some of the appointments they made were very good, I have never been sure that this move was wise. I have often thought that mathematics departments around the country, especially the small departments, might be stronger if they had more specialization. I have been particularly critical of my alma mater, MIT, which did not specialize in applied mathematics, and now doesn't have any representation in statistics since Herman Chernoff retired. I've been on several visiting committees there, and found that there are more Ph.D.s in the mathematical sciences outside the department than within it. Much of the basic work in communication theory and information theory there has been done in other departments.

Somewhat later the group around Pat Suppes left. William Estes went to the Rockefeller University and Richard Atkinson became director of the National Science Foundation, but Pat has continued in a very active program at Stanford in computer-assisted instruction and has had a big impact nationally. Kenneth Arrow left and went to Harvard, although now he is back at Stanford, and the relationship with economics continues to flourish. The Department of Computer Science flourished and the Department of Statistics flourished, although there were some moments of trepidation. One thing: the Statistics Department could have incorporated the operations research activity going on in the university and decided not to. I guess this was a mistake.

Olkin: Yes, I remember this decision. The department was offered the opportunity to incorporate operations research, but the departmental vote was very split and it was decided not to enlarge the scope. Then operations research became a separate department in the School of Engineering.

So in effect then, during the late 1950s, two new mathematical sciences departments were formed: the Department of Operations Research and Computer Science.

Bowker: I was anxious to have computer science in the School of Humanities and Science. I gather that within the last couple of years they have made the other decision themselves.

Olkin: They have just moved to the School of Engineering. Did you have anything to do with mathematical biology, which also has a group at Stanford?

Bowker: No, I wasn't involved with that. In the early days I consulted with Frank Weymouth in physiology and Willis Rich in biology. That really led to the medical school relationship there, because Weymouth was in the medical school. When did the mathematical biology group get started?

Olkin: I can't recall exactly, but Luigi Luca Cavalli-Sforza must have come in the middle 1960s, as a visitor I think at the beginning, and then Marcus Feldman joined that group. But that must have been later.

Al, let me come back now to some of the scientific aspects. There is the Bowker-Lieberman book which clearly had a major impact in the industrial field. In fact, it's a book that is still used very heavily. How did your collaboration start? Was this a natural evolution from the Statistical Research Group Columbia to your work at Stanford in quality control?

Bowker: I think so. And Jerry, of course, had started to work both in variables inspection and in continuous sampling in his early days. We were fairly close personally, so we started to write this book.

It was, I think, fairly influential for a while in the sense that I once estimated that about 10% of the engineers in America must have studied out of it. The book is out of date now in that it has never been revised to reflect the impact of modern computers. I have been encouraging Jerry to revise it and he has been encouraging me, and I suspect that we probably won't do it.

But it did have a big impact. I keep bumping into people who studied from it, and I have correspondence every year about an error or a question.

Olkin: I think until that time there was just the book by Harold Freeman, is that correct?

Bowker: He wrote a book with more emphasis on experimental design and experimental statistics. Our book not only had quality control but tried to put in the standard Fisherian techniques.

MORE ON COLUMBIA

Olkin: And your paper that gave the derivation of the distribution of Hotelling's T^2, where did you do that?

Bowker: I did that at Columbia. Actually, I had never had much geometric intuition and my earlier work had been more manipulative in algebraic forms. And I suddenly, thinking about this, as I remember it, visualized the transformation geometrically. It was something new for me and I thought it was clever at the time; it still seems to be. Ted Anderson refers to it in his book.

Olkin: Yes. I think it is one of the nice derivations of the noncentral distribution of the T^2 statistic.

Bowker: Because I had been interested from the early days with P. L. Hsu in putting statistics into a form such that they could be studied asymptotically in terms of asymptotic expansion, and this was part of it. Hsu, although he did everything formally, really did have a fine geometric sense.

Olkin: Before we get into other administrative posts that you had, perhaps you can reminisce a bit about some of the people who were well known at the time, such as Wald or Wolfowitz or Hotelling.

Bowker: Well, Wald of course had a big influence on all of us at Columbia. His lectures were absolutely magnificent. Hotelling was very creative but was not so organized in the classroom, and I think a lot of us were really very attracted to statistics by Wald.

I think, personally, Hotelling was a wonderful person and he helped develop a lot of people, such as Wilks, Girshick, Doob and the Madows for example. He helped get them supported in the days when that wasn't easy. He had us all to his home in Mountain Lakes periodically. Hotelling later went to Chapel Hill and built another statistical community. He had some eccentricities. He always believed Columbia should sell the campus and move to Rocky Mountain National Park. He was a little single-minded in his views of how statistics ought to be organized and taught. In fact, he was at Stanford in 1930, and I found a write-up in a Stanford catalog which was pure Hotelling. Harold Bacon of the Stanford mathematics department had worked with him.

I was a colleague of Jack Wolfowitz at SRG and we got along pretty well. He had never really had an academic post before the war, so he was kind of in between being a graduate student, although quite a bit older, and being a distinguished professor. He had some sense of the lack of recognition of his abilities, which seemed to continue through most of his life.

One of the nicest people that I have ever known in my life was Abe Girshick, who was generous and friendly and open. And I think one positive point about the whole statistics and mathematics group at Stanford was that we all came out to a relatively new community. It wasn't a community in which any of us had family or friends or cousins or sisters. So we quite naturally associated with each other. We had some contacts with the pre-war faculty at Stanford, but there was a substantial age gap and perhaps even some

differences of opinion on Stanford's future. So Girshick's own home became a center of a large number of people in many departments who would just drop in for coffee or a drink or a chat. Many of us went practically every day and certainly several times a week.

Olkin: David Blackwell has very fond memories of Abe and credits him with having a strong influence in his own life.

Bowker: I think Abe encouraged Blackwell to get working in statistics. We were very disappointed not to get David at Stanford. We tried to recruit him but didn't succeed. I was Chancellor of Berkeley later and David certainly has been a great asset to that institution. In comparing Stanford with Berkeley, I think that he felt that his family would be happier in Berkeley where there is a substantial middle class black community and there really wasn't one in Palo Alto. His children have certainly turned out well so I imagine he was right.

The Blackwells were around a lot too, and we also had a policy of having two or three visitors a year. So a lot of the old gang from Columbia was out: Milton Sobel, Ben Epstein came, Z. W. Birnbaum came several times and there were others.

Olkin: Al, let me ask you about what was going on in the East Coast. Was there a group at Harvard when you were at MIT or at Columbia? Or was that quiescent at the time?

Bowker: I don't remember any activity at Harvard. When I was an undergraduate at MIT we had a joint math club with the Harvard undergraduates.

Olkin: But nothing in statistics.

Bowker: There was a group in public health at Harvard.

Olkin: At a certain point Fred Mosteller went to Harvard.

Bowker: Yes, in the Department of Social Relations there was an experiment of trying to combine social psychology and sociology, which I think has now been abandoned. Bob Sears, who later came to Stanford, was chairman of that department.

Olkin: The other group that was thriving at least in terms of students was Princeton. It did not have a big faculty, but it had Sam Wilks and John Tukey. Were you involved with either at the time?

Bowker: I really wasn't. Wilks had done a postdoctoral year with Hotelling once, and I got to know both of them later when I was at Stanford. But when I was a student at Columbia, I didn't have anything to do with him that I remember. In fact, Tukey's own deep interest in statistics came a little later. Statistics got started at Chicago when Allen Wallis went there, so it must have started about the same time that Stanford did.

THE CHANCELLORSHIP AT THE CITY UNIVERSITY OF NEW YORK

Olkin: Al, the next phase of your life, I think, was in administration. After Stanford you became Chancellor at CUNY.

Bowker: The State University of New York (SUNY) had been reorgnized, and some of the campuses were designated as graduate centers. In 1961 the authority to offer graduate work was extended by change in state law to the City University.

I was suggested as chancellor by Mina Rees, who was then a Dean at Hunter College (we had reported to her during the war—she was the Number 2 person to Warren Weaver in the applied mathematics panel), and by Ruth Shoup, a member of the Board of Higher Education, who was a Stanford graduate, as were her husband and sisters. Her husband was the brother of the director of the Stanford alumni asociation. Pat Sears, who was on the Stanford faculty, is one of Ruth's sisters. In any case, there were a lot of Stanford connections.

They hired me at CUNY in part because I was graduate dean. This was an extremely exciting period at the City University. I did organize graduate work,

Albert H. Bowker, New York, 1970

and I was pleased that in the last ranking of graduate programs by the Associated National Research Council, the City University ranked very well. In New York State I would say that it ranked just after Columbia and Cornell.

So the graduate doctoral work was started. But the real problem in New York at that time was that in the period in which there had been enormous expansion of higher education in many parts of the country, particularly California, in which a very large percentage of the high school graduating class was going into some kind of post-secondary institution, the City University had not expanded very much. They had talked about it but it hadn't been done.

So my main job was to build an institution equal to the demands of the population in the City of New York. And I think I really did that. When I took over the institution, there were four senior colleges and three community colleges, and when I left there were 20 institutions. I used to found them at the rate of one or two a year, as I was there only eight years. Some of those were created by the separation of existing institutions and making the components independent. But some were brand new.

This required among other things, a massive building program which is still going on. And the City University construction fund, I think, is one of the best-funded capital programs in the country.

Toward the end of my term at CUNY, we adopted an open admissions policy which offered a place in either a community college or a senior college to every high school graduate in New York. And all during this period, we had a number of additional programs to interest minorities, in particular under-represented minorities, into going on to college. So it was a great expansion of opportunity. The open admissions policy is somewhat controversial, and I think not well understood, but I am not the one to pass judgment on its success.

Olkin: Do you still feel that it served a purpose in permitting minorities and people who would not have had a chance to have an education to get one?

Bowker: Yes. There is no question about it. It has provided an opportunity to an enormous number of people. Also, the school system itself was embroiled in all kinds of controversies during those years. The City University by and large was not an issue in the racial struggles that went on in New York.

Olkin: I was going to comment before that I remember two critical points during your tenure as Chancellor of the City University. One was the budgetary fights with Governor Nelson Rockefeller. And the other was the open admissions policy, which we have discussed. Do you want to comment about the budgetary issues?

Bowker: Well, Rockefeller actually did a wonderful job in supporting higher education. But it was still very important for me to dramatize the needs of the university. One year I threatened not to open in the fall with any new freshmen because we didn't have room for them. That was the year we got the construction fund passed. I used to threaten to close this college and that college. It was all real; we really needed the money. And we got it. Rockefeller would really probably have preferred to bring the City University under the State University. That was discussed a lot in those years, though, and my Board considered it treachery every time it was mentioned, tuition being the big political issue.

In those years the City University was supported by both the City and the State so my demands for money hit the Mayor also. Bob Wagner was and remains both a great friend of mine and the City University; he supported the increased budgets and in his last days in office, intervened in a factional dispute in my Board to make it possible for me to continue in office. John Lindsay endorsed the construction fund and the open admissions policy.

Free tuition was maintained as long as I was Chancellor and somewhat thereafter. It was remarkable how easily it was abandoned during the fiscal crisis of New York. I was always willing to move a little bit on that issue in return for something. But my Board probably wouldn't have been. When CUNY finally gave it up, they got nothing for it.

But those were really creative years, and looking back, I was one of the fairly important people in New York City at the time. I was never listed among the power brokers, but I certainly saw an awful lot of them, whoever they were. I had a lot of good friends in important places and access to all levels of city and state government.

Now, the academic excellence of the City College, in particular, was at its height in the 1920s and 1930s. They had more or less a monopoly on the children of the Jewish immigrants to New York. After the war, the bright Jewish kids had lots of opportunities elsewhere, but many people look back on those days as to what City College ought to be. It just isn't appropriate to run an elitist public institution that is primarily white in the middle of Harlem, in my view anyway.

Olkin: Al, as you look back now from a 15-year perspective, what do you see as the major accomplishments during your chancellorship at CUNY?

Bowker: I think I'd mention the Graduate School and University Center, the policy of open admissions and the Construction Fund; but even more, the creation of new institutions: Herbert H. Lehman College, Bernard M. Baruch College, Borough of Manhattan Community College, John Jay College of Criminal

Justice, Fiorello H. La Guardia Community College, Hostos Community College, Medgar Evers College, York College, Kingsborough Community College, Richmond and helping Mt. Sinai become a medical school and incorporating New York Technical College into the system.

Olkin: Has the enrollment increased considerably in CUNY?

Bowker: It did but it's a little soft right now for demographic reasons though it's holding up better than they thought. There appears to a shift toward older students.

THE BERKELEY YEARS

Olkin: In 1971 you moved back to the West Coast to Berkeley. Tell us about the Berkeley period.

Bowker: Well, Berkeley is of course one of America's greatest institutions, and it was very interesting and a lot of fun to be there. The intellectual life is really outstanding. I was Chancellor nine years, about half of which I had Ronald Reagan as governor and the other half Jerry Brown.

My job really was not to make major changes in Berkeley, though I made a few, but to maintain its excellence and protect it from barbarians at the door. I think I did that reasonably well and was very popular when I left. I left with the respect of the San Francisco community, the philanthropic community, the alumni and the faculty. Even the presidents of the student body gave me a present.

But one thing I did do was start a major fundraising campaign at Berkeley. There really had never been one. We were running about $25 million a year in gifts when I left, up from practically nothing, and my successor, Michael Heyman, has doubled that.

I am also proud of the deans and other administrators who served with me, including the present Chancellor and Vice Chancellor.

Although I count the senior members of the statistics department as close personal friends, and I saw them when I was at Berkeley, I stayed out of the business of the statistics department. I thought they were a little slow in getting into the computer age, and I helped them once or twice. But it's always been my view that the head of an institution should stay out of his own discipline. My predecessor Roger Heyns advised me on this.

Olkin: You were at both Berkeley and Stanford. They have continued to maintain their connection until today.

Bowker: The cooperation between Berkeley and Stanford is practically unprecedented in the United States. I don't mean necessarily in statistics, but between the administrations. For example, there is nothing comparable between Maryland and Johns Hopkins, and in New York the public and private institutions are at each other's throats.

But we could always count on support for our budget and support for many things from Stanford. Different administrators over many years have seen the desirability of this cooperation.

Also Stanford could use Berkeley as a standard when it was trying to achieve great university status. Berkeley could use Stanford as a standard when it was fighting the budgetary doldrums under Ronald Reagan and Jerry Brown.

Olkin: How would you summarize your stay at Berkeley? What do you see as the highlights?

Bowker: When I went into the Chancellorship I said that one of my jobs was to convince the public to have the confidence in Berkeley that Berkeley deserves. It was partly the times but I think partly our effort, that restored the image of Berkeley as a reasonable institution.

When I first went there, for example, people from most central valley towns wouldn't come to Berkeley, a reaction to student violence and so forth. Now Berkeley is incredibly popular all over the country. It's reputation for violence and protest has changed to that of a major cultural center and hub of Bay Area politics.

I enjoyed the friends I made in San Francisco, and the international visitors who came through. Many important people visited Berkeley. For example, a month ago Corazon Aquino finished her tour there. It's true to some extent at Stanford.

Olkin: That takes us almost to the present time. Can you comment about your role in the post-Berkeley period and what you're up to these days?

Bowker: Yes. After Berkeley toward the end of the Carter administration, I was Assistant Secretary of Education for Post-Secondary Education which made me, among other things, the largest loan collector in the United States. That experience was kind of frustrating because we really weren't in office long enough to have major influence on the department.

After that I went out to the University of Maryland and founded the School of Public Affairs, one of the new policy-oriented management schools bringing people from the public sector. It's something like the School of Public Policy at Berkeley, the Kennedy School at Harvard or the Johnson School at Texas. It's an excellent school and it's doing very well. There isn't anything quite like that in the Washington area and it is needed.

BACK TO THE EAST COAST

Bowker: Then I helped in the central administration of the University of Maryland for a couple of years, as Executive Vice President of the University.

But I didn't really enjoy that as much as founding the School of Public Affairs. That reminded me more of my days of building the statistics department at Stanford. So I retired from Maryland last June, took the summer off, and now I'm working for the City University again and looking at their research foundation programs.

I have also agreed to do several things this year for other institutions, so I'm quite busy. I really don't want to work quite this hard.

Olkin: What is your function at the research foundation of the City University?

Bowker: I'm now reviewing how it handles projects and how it is financed. I then want to take a look at some of the problems in the way of getting increased research funds at the university.

I also work for the Chancellor. For example, I work on executive searches. I have given some thought to starting a statistics degree at CUNY. With Princeton changing its emphasis and Columbia in considerable disarray, there is an opportunity to start a strong program in New York. I had encouraged College Park (University of Maryland) to start a program, and I think they will although they haven't made the final decisions.

Rutgers is building up and perhaps that's enough; I don't know. But New York City has always had such a reservoir of theoretically talented people, it just seems to me that it could have a major center in statistics. So I am giving some thought to that as well as to a few other academic programs.

Olkin: You have commented about not wanting to work so hard, which I think you're entitled to. Tell me what you would like to do. What are your hobbies or travels? I know you have a cabin at Lake Tahoe.

Bowker: I love Tahoe and in the last few years while my mother was alive we did not get there very much. My mother died about a year ago so we spent last summer at Tahoe. We just had a very pleasant trip to Israel which we enjoyed a great deal. We are now in a position where we could even stay overseas for some period of time. So I expect to do a lot more travel.

I like to read to keep up the current novels, particularly British, but I'm just dropping behind in that also.

Olkin: Al, from your perspective from the last 40 years in the statistics business, what do you see in the future? What would you like to suggest to people?

Bowker: I have had a little worry about statistics in the sense that so many of the creative people of my generation or the generation of my teachers—I think of Blackwell, Tukey, Hotelling, Wald, Neyman, Wolfowitz, Savage and there are probably many others—were not trained as statisticians, but came with their training essentially in pure mathematics.

There had been a question in my mind whether the statistics departments would attract intellects of the caliber of those people. So far I think the answer is yes. I think some of the 40 year olds at Berkeley and Stanford are very, very good. But I have seen fields, particularly in the applied social sciences such as education, social work and business, decline when they began to hire their own Ph.D.s and not have people from core disciplines. And I do think it's important that statistics keep a flow not only from the theory side but from the side of applications. I must say I have been kind of impressed with what the young people at Berkeley and Stanford are up to.

Olkin: How about the impact of computers; do you see anything special there?

Bowker: Well, I agree with Brad Efron's view in principle that it ought to change everything. So far it hasn't. It seems to me that the young faculty coming out know computing and know computers, and that will probably work out.

Olkin: Al, are there any topics that we have omitted that you would like to talk about?

Bowker: One thing: When I was talking about Stanford, it has always seemed to me that people who studied the history of the development of Stanford have not given enough credit to the intelligent way in which soft money from the federal government was used in its years of big expansion. Indeed, I think the full story of the Office of Naval Research, in general, has not been told. But no doubt, engineering, statistics, parts of physics and parts of many other fields were all built in ways that are not possible today but were then.

I think about the time before the National Science Foundation was formed, the role of the Office of Naval Research in supporting basic research and increasing technical manpower in this country was extremely important in the development of the statistics department of Stanford as well as many of the others.

Olkin: Probably the Courant Institute is another prime example.

Bowker: Yes, Harvard and Berkeley didn't do that whereas Stanford did. Maybe they had enough money. But at Stanford all the philanthropic gifts and all the money from real estate came in much later. It was the incremental overhead money in those years that really pumped money into Stanford.

Olkin: The statistical profession really owes a debt of gratitude to the handful of people who started the many statistics departments during the period from 1945 to 1955 when there was rapid growth and the beginnings of the current expansion era of statistics. And you were one of the key people in that.