Statistical Science
2005, Vol. 20, No. 1, 89–110
DOI 10.1214/088342305000000133

A Conversation with Shelemyahu Zacks

Nitis Mukhopadhyay

Abstract. Shelley Zacks was born in Tel Aviv on October 15, 1932. He earned his B.A. degree in statistics, mathematics and sociology from Hebrew University in 1955, an M.Sc. degree in operations research and statistics from the Technion in 1960, and a Ph.D. degree in operations research from Columbia University in 1962. He is perhaps best known for his groundbreaking articles on change-point problems, common mean problems, Bayes sequential strategies and reliability analysis. His lifelong enthusiasm in handling difficult problems arising in science and engineering has been a primary inspiration behind his most important theoretical publications. His studies on survival probabilities in crossing mine fields as well as his contributions in stochastic visibility in random fields are regarded as fundamental work in naval research and other defense related areas. Professor Zacks' authoritative book, *The Theory of Statistical Inference* (1971), and its 1975 Russian translation have served graduate programs and researchers all over the globe very well for over 30 years. He has written other books and monographs, including *Parametric Statistical Inference: Basic Theory and Modern Approaches* (1981b), *Introduction to Reliability Analysis*: *Probability Models and Statistical Methods* (1992), *Prediction Theory for Finite Populations* (1992), co-authored with H. Bolfarine, *Stochastic Visibility in Random Fields* (1994b) and *Modern Industrial Statistics: Design and Control of Quality and Reliability* (1998), co-authored with R. Kenet. He is the author or co-author of more than 150 research publications. During the period 1957 through 1980, his career path took him to the Technion (Israel Institute of Technology), New York University, Stanford University, Kansas State University, University of New Mexico, Tel Aviv University, Case Western Reserve University (CWRU) and Virginia Polytechnic Institute and State University (VPI). During 1974–1979, he was a Professor and Chairman of the Department of Mathematics and Statistics at CWRU. In 1979–1980, he spent a year in the Department of Statistics at VPI. In 1980 he moved to State University of New York–Binghamton (now called Binghamton University) as Professor and Chairman of the Department of Mathematical Sciences, and he has continued in the department as Professor and Director of the Center for Statistics, Quality Control and Design. For nearly 20 years, Professor Zacks worked as a consultant for the Program in Logistics at George Washington University. Professor Zacks has held a steady stream of editorial positions for such journals as *Journal of the American Statistical Association*, *The Annals of Statistics*, *Journal of Statistical Planning and Inference*, *Naval Research Logistics*

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Quarterly, *Communications in Statistics* and *Sequential Analysis*. He served as the Executive Editor for *Journal of Statistical Planning and Inference* during 1998–2000. He has earned many honors and awards, including Fellow of the Institute of Mathematical Statistics (1974), Fellow of the American Statistical Association (1974), Fellow of the American Association for the Advancement of Science (1982) and elected membership in the International Statistical Institute (1975). He regularly travels to scientific conferences as an invited participant, works harder than many half his age, and continues to inspire through his writings and uniquely affectionate presence.

The following conversation began March 16, 2000 at the Radisson Hotel in San Antonio, Texas, during the international conference, *Statistics: Reflections on the Past and Visions for the Future* (March 16–19, 2000), on the occasion of the 80th birthday of Professor C. R. Rao.

UPBRINGING IN TEL AVIV

Mukhopadhyay: Shelley, let us start at the very beginning. Please tell me about your parents and where you were born.

Zacks: My parents emigrated from Russia to Palestine after World War I. The country was at that time under British rule (Mandate of the League of Nations). There were only about 600,000 people in the Jewish community and close to two million Arabs. I was born on October 15, 1932 in Tel Aviv and grew up there. It was a difficult period. During the years 1936–1939 there were armed attacks led by Arab gangs against Jewish communities. The British Army fought these gangs. These skirmishes stopped when World War II broke out in 1939. I still remember vividly those difficult days during World War II. The tension was very high when the British Army, under the leadership of General Montgomery, fought the German Army in the African desert. We were afraid that the Nazis would conquer the Middle East and annihilate the Jewish community.

My father, Yechezkiel Zacks, was an elementary school teacher. My mother, Devorah, was a homemaker. We lived in a small house with a garden and some citrus trees. The family used to grow some vegetables and raise chickens. I had a sister who was eight years older than I and a brother who was four years younger. My brother is still living with his family in Israel. My mother's side of the family was large and they lived in and around Tel Aviv.

Mukhopadhyay: Did you see your grandparents?

Zacks: I only saw my maternal grandmother. My maternal grandfather tried to flee Russia but he was caught at the border. He perished in a Russian jail. The grandfather from my father's side did not leave Russia and died there from natural causes.

Mukhopadhyay: What subjects did you enjoy in school? Do you recall any particularly inspiring teacher?

Zacks: Mathematics came easily to me. At age 11 I started taking music lessons, playing the violin. After school hours, I devoted a lot of time to music. I continued studying violin until I finished high school. In elementary school I had a great teacher, Mr. Halevy, for five years. He had a big influence on me. He emphasized humanitarian values, and the value of rebuilding the nation of Israel and creating an independent state for the Jewish people.

The standard of living during those years was relatively low. People had to work very hard just to make ends meet. Nevertheless, we got our education all right.

HIGH SCHOOL LIFE AND THE WAR OF INDEPENDENCE

Mukhopadhyay: During the post-World War II period 1946–1950, Shelley, you studied at the famous Herzelia High School in Tel Aviv. What do you remember from that period?

Zacks: When news about the Holocaust reached us we were all very much affected. Many families lost relatives in Europe during the war. This led to the fight of the Jewish community with the British government to bring the Holocaust survivors to Israel. The struggle to achieve independence for a Jewish state began. I remember the declaration of the United Nations for partitioning of the country. This was in November 1947. The Palestinian Arabs did not accept this UN resolution and started immediately an armed struggle against the Jewish community. My education was obviously disrupted. We became active members of paramilitary units. The British regime left our country on May 15, 1948 and the State of Israel was born. The

FIG. 1. *Shelley Zacks playing violin at the age of 16 in Tel Aviv.*

war of independence ended some time before I graduated from high school. Immediately after graduation I was drafted into the Army and I served in the Israel Defense Force for two years, and later I belonged to a reserve unit.

Mukhopadhyay: What subjects did you particularly like in high school?

Zacks: There was one major track in natural sciences and mathematics, and another one was in the humanities. I opted for sciences and mathematics. I found these subjects fascinating. In addition, I continued to devote a considerable amount of time to music. By the time I finished high school, I also graduated from the Conservatory in Tel Aviv. Actually, some of my teachers at the Conservatory wanted me to continue my education in music. I was offered a scholarship in music, but by then I had decided not to become a professional musician.

UNDERGRADUATE AT THE HEBREW UNIVERSITY, JERUSALEM, 1952–1956

Mukhopadhyay: In the fall semester of 1952, Shelley, you joined the Hebrew University in Jerusalem in its undergraduate program. What kind of a curriculum did you pursue?

Zacks: Normally, a student majoring in mathematics would have selected physics as a minor subject. I decided to choose the tracks of mathematics and sociology instead.

Mukhopadhyay: Why did you want to study sociology?

Zacks: I read influential books about social structures and social history. I thought that it would be interesting to formally study sociology. Eventually, I took mathematics and sociology along with a minor in statistics for a four-year undergraduate degree. Among the mathematics professors, A. Dvoretzky greatly influenced me. I also had the privilege to take some courses from Professor A. Halevy Frankel, a very famous set theorist. I enjoyed the thorough training I received in the foundations of mathematics.

I also took some statistics courses. The Department of Statistics was very small. It offered an elementary methods course and another one on demography. An introductory course on probability and mathematical statistics was offered in the Mathematics Department.

Mukhopadhyay: Did you have prescribed textbooks?

Zacks: My professors gave lectures primarily using their own notes. Of course, from time to time we referred to some classics such as Richard Courant's book, *Differential and Integral Calculus* **1**, **2** (1936). I learned real analysis, complex analysis and probability theory from A. Dvoretzky. He was my first teacher in probability. I studied mathematical statistics from Harald Cramér's *Mathematical Methods of Statistics* (1946). I often read from books such as Hansen, Hurwitz and Madow's *Sample Survey Methods and Theory* (1953).

Mukhopadhyay: What convinced you to be a statistician?

Zacks: In 1953 or 1954, the Department of Statistics held a seminar and I volunteered to give a talk on the analysis of variance (ANOVA). I had not even learned "linear models" or the "analysis of variance" by then! In 1952 A. Hald's *Statistical Theory with Engineering Applications* was published. I went through that book and gave a lecture on the topic. Hald's book was an eye-opener for me. This was the first time I got the taste of how statistics could be useful to solve practical problems in engineering and science. I felt the excitement and thrill of it. I had no idea then how this seminar talk might affect my future.

FIG. 2. *Shelley Zacks as a student, Hebrew University, Jerusalem, 1952.*

THE MAKING OF A CONSULTANT: A STATISTICIAN AT THE RESEARCH COUNCIL OF ISRAEL, JERUSALEM, 1955–1957

Mukhopadhyay: Hald's book really touched a chord!

Zacks: Yes, certainly. This seminar presentation actually led to my first consulting job. N. Landau, an agronomist at the Research Council of Israel, had just arrived from the famed Rothamsted Experimental Station, England, and was conducting experiments regarding the influence of photosynthesis on various measurements on plants. But there were all sorts of confounding effects; for example, the number of hours of exposure to light per day was confounded with other factors. On each plant Landau observed variables such as the length of stems and length of leaves. He called the Statistics Department at Hebrew University looking for someone to help with the statistical analysis of his data. The department referred Landau to me as someone who knew the area of analysis of variance. God knows what would have been my career if I had not volunteered to give that seminar! (Laughs.)

I went to talk to Landau, thinking that I would probably work with him for a couple of hours and carry out some elementary statistical analysis. I told you that my knowledge was limited to what I had taught myself from Hald's book! Landau asked, "Haven't you heard of R. A. Fisher's *The Design of Experiments* (1935) or Snedecor and Cochran's *Statistical Methods* (1946)?" and I said, "No." He reached out, grabbed his copy of Fisher's book and gave it to me. He said, "Here, take a look at this book." I immediately started reading about designs and analyses of experiments. The statistical analysis for the problem on hand required some serious computations. So I taught myself some basic numerical algorithms and statistical computing with the help of purely hand-operated *Facit* machines to come up with appropriate data analysis. Landau was very pleased, which meant that he gave me more applied work. My involvement with Landau's projects continued for several months. Afterward I got the position of a statistician in the Departments of Biology, Chemistry and Physics at the Research Council of Israel in 1954–1956.

Mukhopadhyay: This was a big break for your career, I am sure. Did you get involved in other exciting "research apprenticeship" positions?

Zacks: Dr. Neeman, an organic chemist at the Research Council, was developing compounds which were going to act as synergists to DDT. He was working with an entomologist who performed the bioassays. He needed help with the analysis of data. At that point, I picked up D. J. Finney's *Statistical Method in Biological Assay* (1952), taught myself some basic biostatistics and also looked at several journal articles. I remember writing to Finney and he was very kind to send me some reprints of his papers. Eventually, I was able to carry out some interesting analyses of the data, involving statistical modeling of synergistic effects in a situation where the interaction terms were of primary importance.

I loved those opportunities to work directly with scientists. They were not working on routine problems and, hence, any conventional analysis was rarely appropriate. A scientist would laugh if I had said, "Look, your F value is significant and so you have significant effects." He would respond and say, "Do you think we design and perform an experiment if we do not anticipate significant results?" I realized very early in my career that experimental errors ("background noise") might sometimes overshadow treatment effects. When that happened, it would indicate to a scientist that he should rethink the experiment itself! In a scenario like this, the experimenter would go back to the drawing board and carefully reexamine the problem. On the other hand, a significant F value from the ANOVA indicated that a careful response surface analysis or multiple comparisons might be pursued.

Mukhopadhyay: Any other projects?

Zacks: J. Frenkiel, a physicist at the Research Council, had set up windmills in several locations of the country to study wind speed during different seasons. Wind speed turned the blades of windmills quickly and that generated electricity. For this purpose, the wind speed has to be above a threshold specific to a windmill. I was helping Frenkiel in modeling the distribution of wind velocity during a given period and predicting the amount of electrical energy that could be extracted. Data were collected continuously for several years at different sites. There were a lot of available data. One of the Pearson distributions with different parameter values for different sites fitted the data very nicely. Having fitted these distributions, Frenkiel was then able to predict the amount of generated electricity based on the wind speed.

Mukhopadhyay: These collaborations sound very exciting. Did you have any publications from these projects?

Zacks: Frenkiel and I published a joint paper (Frenkiel and Zacks, 1957) on wind-produced energy and its relation to wind regime. I published several joint papers in scientific journals, for example, in the *Annals of Botany* (Poljakoff-Mayber, Mayer and Zacks, 1958) and the *Journal of Pharmacology and Therapeutics* (Blum and Zacks, 1958). I learned from scientists that a final solution to a practical problem must be consistent with the scientific objective and, hence, that a model should not be built around some mathematical analysis just for the sake of doing mathematics alone.

INDUSTRIAL STATISTICS AT TECHNION AND A MASTERS PROGRAM IN OPERATIONS RESEARCH, 1957–1960

Mukhopadhyay: How did you land a statistician's position at the Technion?

Zacks: One day I saw an advertisement in the newspaper to fill a statistician's position at the Building Research Station of the Technion (IIT). I applied for the position and was offered the job. This was in 1957. Later, I became an Adjunct Instructor of Industrial Statistics at the Technion, in the Faculty of Mechanical Engineering. There was a Senior Lecturer of Statistics, Dr. Paul Naor, who needed assistance in teaching some of his courses. I taught one class of mechanical engineering students and he taught the other class. I organized a very nice course on industrial quality control based on the first edition of the famous textbook of A. J. Duncan, *Quality Control and Industrial Statistics* (1952), plus other sources. I worked in Technion for three years, until I was sent by them to get my Ph.D. degree from Columbia University.

At the Building Research Station, I collaborated in various projects associated with materials science and building technology, and developed useful statistical methodologies. I learned many aspects of the interface of statistics and engineering. In later years, I have used my own experiences from those collaborations to come up with real examples from engineering and science.

Mukhopadhyay: Shelley, in 1960 you received an M.Sc. degree from the Technion. How did the regimen of your M.Sc. degree fit within the demands and priorities of your other duties?

Zacks: I knew that one must have a Ph.D. degree in order to be hired as a regular faculty member in the Technion. I thought about advancing my career. At one point I got admission to the Operations Research (OR) program at the Technion in its newly formed Faculty of Industrial and Management Engineering. Ultimately, this led to my master's degree there.

Mukhopadhyay: Was the master's degree awarded on the basis of course work, thesis or both?

Zacks: There was some course work, but a written thesis was also required. I took a number of courses, including design of experiments, linear and dynamic programming, and queueing theory. A course in engineering was required too. Because I was not trained as an engineer, I decided to take an interesting course on "switching networks" that was offered in the Electrical Engineering Department. This course was based on Boolean algebra and I truly enjoyed the material. I learned the rigorous concepts of engineering optimization of switching networks. I also took a very interesting course in computer programming offered in the Electrical Engineering Department, but unfortunately we did not have a computer. So, we invented on paper a small computer, designed in principle, and programmed it in machine language.

I had a statistics professor, Sylvain Ehrenfeld, who came as a visitor from Columbia University in the United States. He was teaching linear models and design of experiments. I wrote my master's thesis under his supervision on randomized factorial designs. Later, before I received my Ph.D. degree, Ehrenfeld and I published a joint paper in *The Annals of Mathematical Statistics* on this topic (Ehrenfeld and Zacks, 1961).

THE PH.D. PROGRAM AT COLUMBIA UNIVERSITY, 1960–1962

Mukhopadhyay: Why did you choose to go to the United States for your Ph.D. degree?

Zacks: Technion authorities decided to send me to Columbia University and earn a Ph.D. degree under the auspices of the U.S. Agency for International Development. I planned to return to Israel thereafter.

Mukhopadhyay: Was this fellowship directed exclusively toward Columbia University?

Zacks: I could have chosen to apply to another university. However, I had already collaborated with Ehrenfeld, who had returned from the Technion to the Department of Industrial Engineering (IE) and OR at Columbia University by this time. Hence, I sought admission in the IE department at Columbia University.

Mukhopadhyay: Was this a smooth transition for you?

Zacks: Even though I came to the IE department, I really did not want to do a doctoral degree in engineering. I wanted to pursue a Ph.D. degree in the graduate school. So I applied for a transfer from the engineering school to the graduate school. A committee approved my transfer. In the graduate school, students could take courses from any department to count toward a Ph.D. plan of study. The graduate school at Columbia was very flexible at the time.

Mukhopadhyay: Who were some of your teachers at Columbia?

Zacks: I took courses from Herbert Robbins on sequential analysis and optimal stopping. I received a heavy dose of "optimal stopping" from him. I took a course from Lajos Takacs on stochastic processes and Markov chains. I took a course from Arthur E. Albert, who had just arrived from Stanford University. It was a hard-hitting course on statistical decision theory, involving topology, functional analysis, and the Blackwell–Girshick book on statistical decisions and game theory. I also took a course on the philosophy of science, taught by Ernest Nagel, from the Department of Philosophy. In the IE department, I learned Markov processes from Cyrus Derman, industrial quality control from Sebastian B. Littauer and extreme value theory from Emil J. Gumbel. I finished my Ph.D. program from Columbia in 18 months.

Mukhopadhyay: J. Wolfowitz had moved to Cornell University by the time you came to Columbia. Did you meet him earlier?

Zacks: When I was at the Technion, Jacob Wolfowitz came to spend a sabbatical year. I had the privilege of attending a course on statistical decision theory that he taught.

Mukhopadhyay: Briefly, what was the big discovery in your Ph.D. dissertation?

Zacks: I wrote my Ph.D. dissertation on optimal strategies in randomized factorial experiments. For example, I proved that the usual randomized fractional replication was minimax. I included different kinds of Bayesian and sequential results too. From my dissertation, I subsequently published three papers in *The Annals of Mathematical Statistics* in 1963–1964 (Zacks, 1963, 1964; Ehrenfeld and Zacks, 1963).

POSTDOCTORAL YEAR AT STANFORD 1962–1963

Mukhopadhyay: After breezing through the Ph.D. program, did you look for some postdoctoral experiences in this country?

Zacks: Yes. First, I decided to spend the summer of 1962 as a postdoctoral research associate at New York University to continue doing research with Sylvain Ehrenfeld, who had moved there. I also received an offer for a postdoctoral position in the Statistics Department at Stanford University. Prior to this, J. Wolfowitz offered me a postdoctoral position at Cornell University, but I decided to accept the offer from Stanford. I spent the following year, September 1962–August 1963, at Stanford.

Mukhopadhyay: If I may ask, what made you decide in favor of Stanford over Cornell?

Zacks: At that time the Statistics Department at Stanford was one of the best in the country and I felt that a year at Stanford would be very beneficial for my career. I saw it also as an opportunity to spend a year on the West Coast. While at Stanford, I mainly worked with Herman Chernoff on the change-point problem. I met Charles Stein there. Jack Kiefer, Bob Bechhofer and Milton Sobel were also spending sabbatical years at Stanford at the time, collaborating on their monograph, *Sequential Identification and Ranking Procedures* (1968).

Mukhopadhyay: What were your primary responsibilities at Stanford?

Zacks: When I arrived at Stanford, my only duty was to do research and nothing else. Life just could not get any better than that! This gave me also an opportunity to take more courses. I attended a one-quarter course from Samuel Karlin on stochastic control. I sat through a two-quarter course from Kai Lai Chung in stochastic processes and martingales, a course in decision theory from Charles Stein and a one-year course on large sample theory from Herman Chernoff. Stein shared some of his early path-breaking results for the very first time in departmental seminars. In the Mathematics Department, I sat through a course on functional analysis and operator theory taught by Karel de Leeuw. The research environment was thriving. It was an exciting period.

Mukhopadhyay: Shelley, what were some of your major research accomplishments during those Stanford days?

Zacks: At Stanford I completed several projects. One project led to my paper dealing with a tracking problem, jointly written with Chernoff, that appeared in the *The Annals of Mathematical Statistics* (1964). We included a related change-point problem and derived a new Bayesian test statistic for change points. This paper, especially its part on the change-point problem, has earned quite some fame. I also wrote a paper on sequential design of fractional factorial experiments that was later published in *The Annals of Mathematical Statistics* (1968).

Mukhopadhyay: How would you explain a change-point problem to a layman?

Zacks: In a simple situation, suppose that one is observing a sequence of independent random variables $X_1, X_2, \dots, X_n$. The question is whether all n random variables have the same distribution $F(x)$ or whether $X_1, \ldots, X_\tau$ have an identical distribution $F(x)$ and the common distribution of $X_{\tau+1}, \ldots, X_n$ happens to be $G(x)$, where $F \neq G$. Here, $1 \leq \tau \leq n$ is called the change point. In a change-point problem, one may like to check whether $\tau = 1$ or $\tau = n$, and in either situation one would say that the common distribution of $X_1, X_2, \ldots, X_n$ did not change. But if $1 < \tau < n$, then there was a change and one may want to estimate the location of τ, that is, the point where the change took place. In this one change-point model, one may also like to examine how different F and G are.

Mukhopadhyay: What was the motivation for your work in the area of detection and estimation of change-points?

Zacks: As a result of some query connected with naval tracking of missiles and fast detection of any malfunctioning, Chernoff and I worked on a problem that was intertwined with a change-point problem. The widely used tracking procedure at that time implemented linear filtering methods of Kalman and Bucy. We approached the problem of estimating the current position of a process by combining a linear filter, which is a Bayesian estimator, with estimation of the location of the change-point. We first treated a simplified version of the problem and obtained a nonlinear filter that was previously unknown. A fallout of this problem turned out to be the change-point problem. The CUSUM procedure of Page (*Biometrika*, 1955 and 1957) was already well known, but our approach gave the first sound Bayesian formulation to solve the problem.

Mukhopadhyay: This paper has been very influential over the years. I recall that in 1991 you reviewed this area in a lengthy article.

Zacks: I wrote a chapter on sequential aspects of detection and change-point problems, which appeared in the *Handbook of Sequential Analysis* (1991b). A related paper for the general exponential family of distributions, jointly written with Kander, appeared earlier in *The Annals of Mathematical Statistics* (1966).

Mukhopadhyay: It appears that change-point problems have appeared in a lot of guises in your work. Can you indicate some examples of interesting statistical problems that are formulated as change-point problems?

Zacks: I worked on the early detection of the entrance to the wear-out phase of increasing hazard rate and published in *Operations Research* (1984). Ben Boukai, my former Ph.D. student, worked on the related sequential burn-in problem in reliability. It is much cheaper to test and eliminate parts in a plant rather than repairing the failed parts in the field after installing them within a bigger system. The burn-in is a procedure in which parts of a system are tested in a plant in order to eliminate those parts that are likely to fail early. A crucial component failing inside an aircraft's engine, for example, can be very costly! The question is this—When no failures are in sight, how long should a test run so that one would have enough confidence that a questionable part will not fail too soon? This has to do with early detection of the epoch of change from a decreasing to a constant hazard rate (Boukai, 1987).

JOB CHANGES: ISRAEL AND RETURN

Mukhopadhyay: After spending a little over a year at Stanford, I assume that you had to go back to Israel.

Zacks: Yes. By the time I finished my assignment at Stanford, I had already spent close to $3\frac{1}{2}$ years in the United States, counting my days at Columbia. I came to Columbia University with an exchange visitor's visa, having a stipulation that after finishing my assignment in the U.S., I must immediately go back and work in Israel for at least two consecutive years. At that point, the Technion hired me as a Senior Lecturer in the Faculty

FIG. 3. *Shelley Zacks with his wife, Hanna, and their older son, Yuval, in Haifa, Israel, 1964.*

FIG. 4. *Shelley and Hanna Zacks with their sons, Yuval and David, in New Mexico, 1968.*

of Industrial and Management Engineering. I was there during 1963–1965, and I taught both statistics and stochastic processes. In the meantime, I was invited to participate in the 1965 Berkeley Symposium, but I could not attend it because of complications arising from immigration rules.

Mukhopadhyay: Then, you came back to the United States. How did that come about?

Zacks: I forgot to mention one important piece of information. When I was at Columbia, my wife Hanna studied in the Department of Philosophy in order to fulfill the requirements for her Ph.D. degree. She finished her required courses as well as both the written and oral parts of the qualifying exams before we had left for Israel. She had to finish her degree and we decided to return to the US. During 1964–1965, I received an offer for the position of Professor of Statistics from Kansas State University (KSU) in Manhattan, Kansas, which I accepted. I resigned from my position at the Technion and joined KSU. The KSU administration took care of all the details for our permanent residency in this country. I spent three years at KSU from 1965 through 1968.

Mukhopadhyay: And then where did you go?

Zacks: In 1968 I moved to the position of Professor of Mathematics and Statistics at the University of New Mexico in Albuquerque, New Mexico, and spent two years in that position. At that time, Julius Blum was the chairman of the department.

Mukhopadhyay: Why did you move?

Zacks: I wanted to move to a more exciting place where I could pursue mathematically challenging and more fulfilling research. I hoped to have research collaborations with Professor Judah Rosenblatt, who was in New Mexico. He also received his Ph.D. degree from Columbia University, but he was ahead of me by a couple of years. He already had some important papers in sequential analysis and I thought that it would be nice to do research with him. Unfortunately, by the time I arrived in New Mexico, Rosenblatt was preparing to leave the University of New Mexico and move to Case Western Reserve University in Cleveland, Ohio. (Laughter.)

Mukhopadhyay: Did you then move to CWRU?

Zacks: Yes, after spending two years at the University of New Mexico, I received an offer from CWRU for a professorship in mathematics and statistics. The department had a number of very good mathematicians, including Lajos Takacs. I moved to Case in 1970 and stayed there until 1979. In 1973, I took a one-year leave of absence from Case to work in Tel Aviv University. In 1974, immediately after I returned from Tel Aviv, I became the Chairman of the Department of Mathematics and Statistics at Case.

Mukhopadhyay: In retrospect, was it a good idea to become the chairman at CWRU?

Zacks: I think that it was probably a mistake to become the chairman of the department at that time. I was serving as Associate Editor of both the *Journal of the American Statistical Association* and *The Annals of Statistics*, and had to devote much energy fulfilling my editorial responsibilities. At that time, I was also

FIG. 5. *Shelley Zacks at Case Western Reserve University, Cleveland, 1975.*

at the peak of my ability as a researcher and I was busy with research initiatives in many directions. On top of these activities, the chairmanship demanded serious time commitment, and that naturally hampered my scholarly pursuits. But, because I had agreed to be the chairman in the first place, I continued in that position for a full term of five years until 1979.

MOVE TO SUNY–BINGHAMTON

Mukhopadhyay: And then you moved to Binghamton?

Zacks: Not quite. In 1979, I was offered a position of Professor with tenure in the Department of Statistics at the Virginia Polytechnic Institute and State University in Blacksburg, Virginia. I took a one-year leave of absence from Case to explore the position at VPI. When I was about to finish a year's stay at VPI, the faculty from the Mathematical Sciences Department at the State University of New York (SUNY)–Binghamton approached me. They were hoping to fill the position of chairperson. In the fall semester of 1980, I joined SUNY–Binghamton as a Professor and Chairman of its Department of Mathematical Sciences. I chaired the department for three years from 1980 through 1983. I have stayed in this place (now called Binghamton University) ever since as a Professor of Mathematical Sciences.

Mukhopadhyay: I realize that the chairmanship at Case was very time-consuming. Thus, why did you move to SUNY–Binghamton as the chairperson of yet another large department?

Zacks: I moved for both family and academic reasons. I wished to move and live in a larger town, but had decided not to return to Case from VPI. Also, I knew that SUNY–Binghamton had an excellent undergraduate school and it had a respectable mathematics department. So, for me, this was the right opportunity for a suitable move.

Mukhopadhyay: After joining SUNY–Binghamton, you led the Center for Statistics, Quality Control, and Design within the university. How did it originate?

Zacks: At that time, there were serious discussions within the profession expressing concerns that academic statisticians did not have sufficient understanding of the special needs of industrial researchers. Many argued that the important issues and problems arising from industries were not being addressed or reflected in the mainstream statistical research programs or publications. The American Statistical Association was pushing the idea to build stronger and lasting partnerships between academia and industry. I thought of actually doing something to address this problem.

FIG. 6. *Shelley and Hanna Zacks in Storrs, Connecticut. October 27, 2000.*

Mukhopadhyay: I presume that statistical needs of the nearby IBM plant had a major role in this.

Zacks: Yes. Close to our campus, there is a big IBM plant in Endicott, New York. The head of the quality control division of that plant had visited some Japanese companies and wished to follow the Japanese example by requiring all its engineers (and many of its technicians) to have some basic understanding and working knowledge of statistical quality control. IBM managers visited SUNY and asked if we could set up instructional courses for their employees. We could not fit this within the rigid framework of regular academic programs, but there was a possibility of doing this under the auspices of the Public Service Center at the university. This gave me an opportunity to establish a Center for Statistics under the jurisdiction of the SUNY Research Foundation. Within that framework, we could sign contracts with IBM, and later with other industries, to provide them with the desired service courses. Some research projects also ensued. The offered courses were not similar to some of our typical courses found in the university's catalog. They were organized along the lines of workshops in which participants got first hand experience in industrial statistics by problem solving.

Mukhopadhyay: How many participants did you normally have?

Zacks: Twenty participants attended the university for four weeks and worked within a "workshop environment" for four hours every morning. The workshop environment was different from a regular series of intensive classroom instructions. A topic was presented

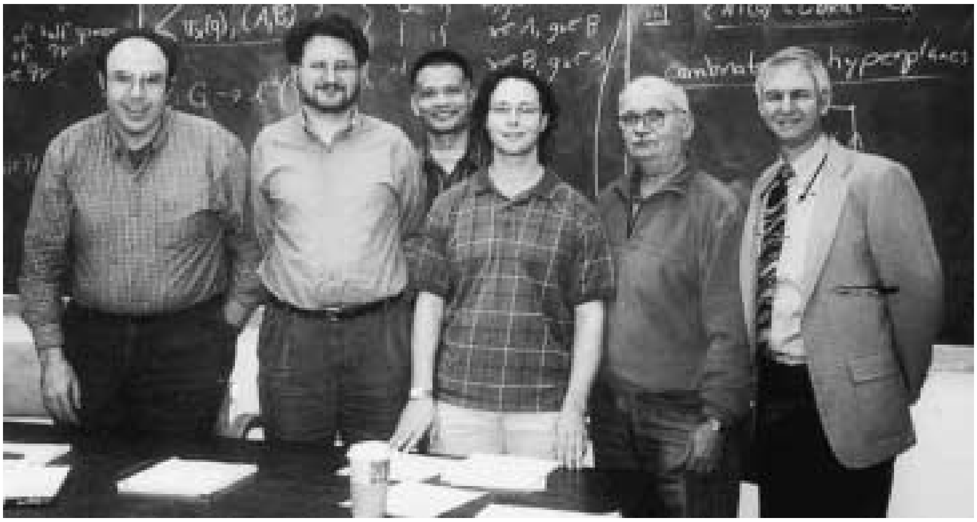

FIG. 7. *From right to left*: *S. Polachek, S. Zacks, M. Haner, Q. Yu, A. Schick and M. Arcones, shown immediately following the final dissertation defense of Matthew S. Haner* (*adv. Zacks*) *at Binghamton University, December 2002*.

and discussed by an instructor for a short time and then the participants immediately started working in pairs on PCs in solving problems connected with these topics. We started these workshops in 1982 with the first generation IBM PCs for which we had to create some special software. This crucial software, called MIPSAQ, was similar to MINITAB, but was created long before any PC version of MINITAB became available.

Mukhopadhyay: How did you prepare to run these workshops?

Zacks: I prepared special notes for these workshops on many aspects of statistics. A special game involving computer simulation was created by a crew from the engineering school under the supervision of Don Gausse, a specialist on industrial simulation. This game simulated a production line with many kinds of "disturbances" that had to be detected by the trainees. Analogs of monetary reward and penalty were embedded in these simulations. Teams of four participants played the simulation game for three days. In the end, the team that made the highest profit was declared the winner and awarded a prize. After successful completion of each workshop, every participant was awarded an appropriate diploma. When a group of 20 participants finished a workshop, another group came aboard. This continued for more than two years. IBM employees from other plants also joined some of the workshops.

Mukhopadhyay: I suppose that you did not offer identical courses to engineers and nonengineers alike.

Zacks: The nonengineering technicians were offered different types of courses in which they learned simple data analysis and statistical graphical techniques for process control. The technicians normally used hand-held electronic calculators rather than PCs. Workshops for the engineers were held on varied topics, including statistical quality control, reliability and design of experiments. My 1992 book, *Introduction to Reliability Analysis: Probability Models and Statistical Methods*, evolved from these lecture notes.

Mukhopadhyay: Did your clientele go beyond IBM?

Zacks: Eventually, with significant support from IBM, the Center brought the much-needed financial leverage to create other positions and invite visitors. It also encouraged participation from other high tech industries including General Electric, Universal Instruments, Dupont and NCR Corporation.

Mukhopadhyay: Did this commitment diminish your effectiveness as the chairman?

Zacks: Due to my involvement with the Center, I was forced to devote significantly less time to oversee some of the routine departmental affairs. So, after chairing the department for three years, I decided to step down in 1983. My colleague, David Hanson, became the new chairman.

MORE CONSULTING PROBLEMS: NAVAL RESEARCH LOGISTICS AND BEYOND

Mukhopadhyay: Shelley, how did you embark upon consulting activities at the federal level?

Zacks: I frequently visited the Statistics Department at Stanford. I began working on different projects with

Professor Herbert Solomon, who had strong ties with the people in Washington, DC, and he recommended me in 1967 as a consultant to the Logistics Research Program at George Washington University (GWU). Solomon told me, "Anytime you go there, you must be fully prepared. Work on their projects intensively as if you are working on a paper. This will ensure your success there." I took his advice very seriously and they were happy with my work. Thus, my consulting activities with them lasted for 20 years. I have flown from my workplace in Albuquerque, Cleveland or Binghamton every second week to Washington, DC, to work on projects in logistics research.

Mukhopadhyay: Did you get into military consulting through your GWU consulting?

Zacks: Not entirely. In 1976, when I was the Chairman at CWRU, one day I received a call from Pete Shugart who worked at White Sands Missile Range in a military operations analysis group (TRASANA). He said, "I read your joint paper with Goldfarb in the *Naval Research Logistics Quarterly* (1966) on survival probabilities for particles crossing a field having absorption points. I also saw your paper in the *Naval Research Logistics Quarterly* (1967) on sequential strategies. Clearly these can be applied to address problems for crossing minefields and I think that you can help us."

Mukhopadhyay: Did you start working on these problems right away?

Zacks: Pete Shugart came to visit Case and explained the types of problems in this area that their agency was trying to resolve. The discussions were fruitful and I started helping them. Later, I wrote a paper in the *Naval Research Logistics Quarterly* (1979) on this subject. Ultimately, they came up with some funding for research contracts through the Office of Naval Research and the Army Research Office. My collaboration with Mr. Shugart continued more than ten years.

Mukhopadhyay: You have worked extensively on modeling and analysis of stochastic visibility problems. Please explain some of the specialties in this field.

Zacks: When a guided missile is launched, one must at all times see both the missile and its target to maneuver the flight path. This was the technology at the time. Inclement weather conditions coupled with excessive dust and debris from heavy shelling or trees and other objects might obscure visibility of the target some of the time. At the ground level, in a thick forest or wooded area with many trees, one loses visibility

FIG. 8. *Micha Yadin (on left) and Shelley Zacks in New York City, 1976.*

fast. The point is that some such random phenomenon may obscure the "target" and many complicated stochastic visibility problems arise.

Mukhopadhyay: Were random visibility problems not similar to coverage problems?

Zacks: Yes, but the models were often more complicated than the ones usually found in the literature. I started working on these stochastic visibility problems with a colleague of mine from the Technion, Micha Yadin. Yadin and I first gave a solution (*Journal of Applied Probability*, 1982) for the stochastic visibility in a random Poisson field in a circular region. Then we followed up with subsequent developments for nonhomogeneous random fields (*Journal of Applied Probability*, 1985). The Office of Naval Research and the U.S. Army Research Office supported related projects for a number of years. Yadin and I were planning to write a book together on this subject, but he passed away prematurely. These ideas and approaches were put together in my 1994 monograph, *Stochastic Visibility in Random Fields*, which I dedicated to Yadin.

Mukhopadhyay: This monograph is more than a simple synthesis, is it not?

Zacks: Yes, it includes explicit methods for determining visibility probabilities of single points, the simultaneous visibility probabilities of several points, the distribution of the total lengths of visible segments of a curve, visibility probabilities in three-dimensional spaces and the distribution of the length of nonvisible segments. The book came with a diskette in which computer codes could be found for the direct computation of visibility probabilities. It was very important for me to equip the users with appropriate computing tools so that they could easily use the otherwise complicated theory to solve real problems. The field of geometrical probability has since been growing very fast.

Mukhopadhyay: Who were other notable contributors in these fields?

Zacks: I am thinking about the period from 1970 through the early 1990s. Herbert Solomon made fundamental contributions in geometrical probability. He also published a very influential monograph, *Geometric Probability*, in 1978. Peter Hall's important piece of work, *Introduction to the Theory of Coverage Processes*, appeared in 1988.

Mukhopadhyay: The problems you had tackled were slightly different though, right?

Zacks: You are correct. Our problems were different and these needed special stochastic analysis. Yadin and I specially tailored the proposed methodologies so that we could appropriately take into account the restrictions put forth by the practical nature of real-life problems.

Mukhopadhyay: You worked in both biostatistics and gene ordering. How did those projects come about?

Zacks: At one point I developed adaptive designs related to dose-escalation schemes in Phase I cancer clinical trials. After meeting Loren Cobb, a biostatistician at the Medical Center at Charleston, South Carolina, I became interested in the applications of catastrophe theory for statistical modeling in biostatistics. Cobb and I worked together to publish an interesting paper on this topic in the *Journal of the American Statistical Association* (1985). I remember that a referee remarked, "I wish that I had written this paper." That made my day!

Mukhopadhyay: Did you not have significant collaborations with colleagues at Fox Chase Cancer Center in Philadelphia?

Zacks: Yes. Some years ago, a group of clinicians and biostatisticians at Fox Chase Cancer Center showed interest in innovative ideas on dose-escalation schemes for Phase I clinical trials. Andre Rogatko, a statistical geneticist, and Jim Babb, a former Ph.D. student of mine, were both working at the Cancer Center. I knew Rogatko from the time I had spent at the University of Sao Paulo, Brazil. The three of us developed an important methodology (*Statistics and Probability Letters*, 1998) and also gave a Bayesian sequential search procedure to determine an optimal dose (*Statistics in Medicine*, 1998).

Rogatko was also investigating the role of Bayesian tests for gene ordering. Years later, he and I started collaborating on a project on ordering genes through sequential techniques with the objective of controlling the decision-error probabilities. We published one joint paper in the *American Journal of Human Genetics* (1993). Later, Rogatko, Rebbeck, and I published another article in the *American Journal of Medical Genetics* (1995).

COMMON MEAN AND BANDIT PROBLEMS

Mukhopadhyay: Shelley, in your view, which one or two papers of yours have been most influential in statistical inference?

Zacks: I will say that my paper on the common mean problem and the other one with Chernoff on the change-point problem have been most influential in statistical inference.

Mukhopadhyay: A number of your major contributions in statistical inference evolved from your attempts to answer questions raised by scientists or engineers. Did the common mean problem arise from practical considerations?

Zacks: In 1964, I was at the Technion and one day a professor of soil engineering walked into my office. He asked a very simple question, "How does one estimate the common mean of two normal distributions?" I asked him, "How do you know that you have two normal distributions having a common mean to start with?" After some discussions I realized that he knew what he was talking about. Soil samples were going to be collected from two sites and it was known that the average percentage (μ) of a certain mineral was the same in both sites, but consistency of the soil differed from one site to the other. It made sense to think of two normal distributions having a common mean μ, but with unequal and unknown standard deviations σ_1 and σ_2. He wanted to find (1) how many observations he would have to take from sites 1 and 2, and (2) once the observations were gathered, how to estimate μ?

It is clear that if the variance ratio is known, then a uniformly best unbiased estimator of μ exists. I wished to attack the estimation problem in the case when the ratio σ_1/σ_2 was unknown. I located two or three references, but none provided a satisfactory answer.

Mukhopadhyay: Did you pursue the method of maximum likelihood?

Zacks: Yes indeed, and I immediately came up with a preliminary solution. I also wrote a letter to Herman Chernoff asking him whether he knew any other answer that could be better than my simple-minded one. Chernoff replied, "No, I don't know any other method.

But, your procedure is probably a good one." I worked on the problem for small sample sizes and published in the *Journal of the American Statistical Association* (1966a).

Mukhopadhyay: Later, did you not extend the results under Bayesian and fiducial approaches?

Zacks: The Bayesian and fiducial approaches were published in *The Annals of Mathematical Statistics* (1970). Subsequently I developed a sequential design procedure for this problem that appeared in the *Journal of the American Statistical Association* (1973).

Mukhopadhyay: Others followed with decision theoretic considerations. Any comments?

Zacks: A. Cohen and H. Sackrowitz from Rutgers University became interested in the common mean problem. Cohen and Sackrowitz (1974) obtained decision theoretic results such as nonminimaxity and nonadmissibility properties of some of the otherwise natural estimators. Tatsuya Kubokawa, a Japanese colleague from the University of Tsukuba, wrote a Ph.D. dissertation (1987) extending the original ideas in several directions.

Mukhopadhyay: Herman Chernoff's (1972) monograph, *Sequential Analysis and Optimal Design*, had interesting expositions of the common mean problem. In broad terms, would you please contrast your approach and Chernoff's sequential approach in the common mean estimation problem?

Zacks: There are similarities between the two approaches. In a design context, one would prefer having all observations from the normal population that has a smaller variance. Since we do not know which population has the smaller variance, we essentially have a two-armed bandit problem. In other words, at every step we wish to make a sequential adaptive decision regarding the population from which it is more worthwhile to take the next observation.

Mukhopadhyay: I recall that in your 1973 paper, a sequential strategy was laid out to do just that.

Zacks: Yes. My objective was to come up with an estimator with minimal variance at termination. I studied the case where one variance was known. The goal was to obtain sequentially a fixed-width confidence interval estimator of the common mean with the smallest expected sample size and an approximate prescribed coverage probability.

Mukhopadhyay: Is it fair to say that the success in this area has spilled over into other research areas?

Zacks: I would like to think so. I recall that you published a sequential two-armed bandit version generalizing my one-armed bandit design (Mukhopadhyay and Narayan, 1981). Together with P. F. Ramig, a colleague at CWRU, I developed methods for estimating the common variance of k-dependent normal populations, assuming an equicorrelated model. My Ph.D. student, Daniel J. Ghezzi, generalized these results in his dissertation (2001).

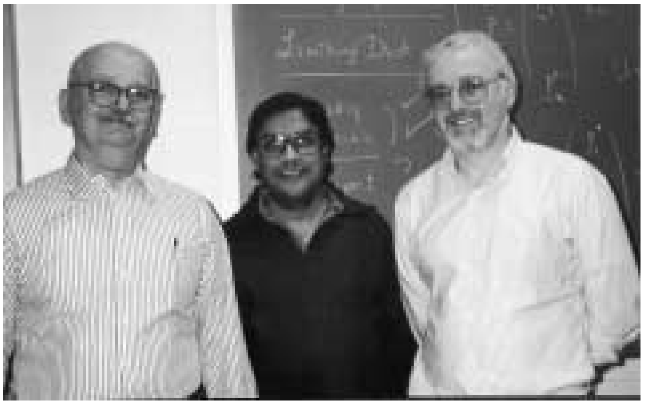

FIG. 9. *Standing from left to right*: *Shelley Zacks, Nitis Mukhopadhyay and Joe Glaz during Shelley's visit to the Department of Statistics, University of Connecticut on October 27, 2000.*

Mukhopadhyay: In the variance component problem you originated novel ideas. Any remark?

Zacks: Jerry Klotz, Roy Milton and I showed that the conventional estimators of variance components were inadmissible (*Journal of the American Statistical Association*, 1969). I later derived the Bayes equivariant estimators of variance components. It is noteworthy that the invariance structure cannot eliminate the nuisance parameter, namely the ratio of the variance components. H. Sahai, a former Ph.D. student of R. L. Anderson, extended some of my results in his thesis and published an extensive bibliography (Sahai, 1979). Searle, Casella, and McCulloch's (1992) *Variance Components* now serves as a reference guide.

Mukhopadhyay: In some situations, you have mixed a bandit problem with a change-point problem. Am I correct?

Zacks: Yes, Nitis, you certainly are. As an example, let us consider a two-armed bandit machine. Arm 1 has a fixed known probability of success p_1, but arm 2 starts with a known probability of success θ_2, which may change at an unknown epoch to a value φ_2 (known), where $p_1 > \theta_2$, but $p_1 < \varphi_2$. The epoch of change is unknown!

Mukhopadhyay: In a clinical trial, one may face a similar situation too.

Zacks: Yes, of course. This problem has a dual in clinical trials since at some unknown epoch, an inferior treatment once modified may exhibit superior characteristics when compared with the standard treatment. Now, what should be the optimal strategy if a finite number of trials are allowed and the objective is to maximize the expected total number of successes? E. Lamprecht, a Ph.D. student of mine, wrote her dissertation on this subject. A Bernoulli two-armed bandit problem involving a change-point in one arm appeared in the *Journal of Statistical Planning and Inference* (1998).

BAYES SEQUENTIAL INFERENCE

Mukhopadhyay: The breadth of your contributions in sequential analysis is amazing. Would you care to discuss some of your favorite contributions in this area?

Zacks: Chow and Robbins (1965) gave a general recipe for constructing confidence intervals with prescribed fixed width for the mean of any distribution having a finite but unknown variance σ^2. But in constructing a confidence interval for the standard deviation $\sigma (> 0)$, it is natural to expect a confidence interval to be short or wide according as σ is small or large. That is, the accuracy of the interval should be then proportional to σ. Thus, the fixed-proportional closeness idea sounded more appropriate here than the fixed-width criterion. I worked out a sequential procedure for estimating the mean of a lognormal distribution with two unknown parameters and proposed a confidence interval having prescribed fixed-proportional closeness (*The Annals of Mathematical Statistics*, 1966c).

Mukhopadhyay: But, why did you settle on a lognormal distribution?

Zacks: It is interesting. In my earlier involvement with the Building Research Station at the Technion, I noticed that the compressive strength of concrete cubes was routinely assumed by the engineers to follow a lognormal distribution. Initially I thought of pursuing a two-stage or some other similar procedure like you and others had studied (Mukhopadhyay and Solanky, 1994, Chapter 2; Ghosh, Mukhopadhyay and Sen 1997, Chapter 6; Mukhopadhyay, 2000, Chapter 13), but then I decided to proceed with a purely sequential sampling design where the Fisher information matrix played an important role. The associated techniques were substantially different from those in Chow–Robbins. Rasul Khan, a student of Herbert Robbins, generalized these results (*The Annals of Mathematical Statistics*, 1969).

Mukhopadhyay: This area as such was not entirely new or was it?

Zacks: The distribution of stopping times is an essential ingredient for evaluating the mean, variance and other characteristics of a random sample size in a sequential experiment. The distribution is also crucial for determining a confidence interval of a parameter estimated after stopping. In the case of SPRT (sequential probability ratio test) procedures, the original set of useful approximations was given in Wald's classic text, *Sequential Analysis* (1947).

Mukhopadhyay: What was your initial motivation to work on the distribution of stopping times?

Zacks: My motivation for working with stopping times for Poisson processes with linear boundaries came from a reliability problem. At the Center of Statistics, I consulted with engineers from a nearby NCR plant on sequential testing of electronic equipment and they were interested in estimating the reliability after sequential testing. I set out to derive the fixed-precision confidence intervals for the reliability parameter after stopping. For this purpose I needed the distribution of the stopping time corresponding to a time-homogeneous Poisson process crossing one of two parallel linear boundaries. From this distribution, I could construct a confidence interval for the parameter of interest and evaluate the coverage probability (*Communications in Statistics, Stochastic Models*, 1991a).

Mukhopadhyay: One of the first papers along these lines was due to Dvoretzky, Kiefer and Wolfowitz (*The Annals of Mathematical Statistics*, 1953). How was your approach different?

Zacks: Dvoretzky et al. gave the Laplace–Stieltjes transform of the stopping time through appropriate difference-differential functional equations. I was looking for an explicit solution and so I stayed away from these established techniques. Personally, I was not very satisfied with approximations through Brownian motion. Instead, I exploited the properties of sample paths, strong Markovian properties and complex analysis. Using the fact that a Poisson process is strongly Markovian, a closed formula can be derived for the probability distribution of the stopping time.

Mukhopadhyay: I remember that your final explicit solution was indeed very pretty. But, then what led you to extend these approaches in the case of multiple Poisson processes?

Zacks: The generalization was motivated by a problem in genetics involving the ordering of three loci on a chromosome (Rogatko and Zacks, 1993, *American Journal of Human Genetics*). At each time point, one could observe sample paths of all three processes.

The stopping time was reached when the difference between the lower and the middle sample path reached an integer threshold value c for the first time. I considered three independent Poisson processes and found the distribution of the first time at which the difference between the second and first order statistics exceeded a threshold (*Communications in Statistics, Stochastic Models*, 1994a).

Mukhopadhyay: Have you pushed these ideas through compound Poisson processes?

Zacks: In collaborations with colleagues D. Perry and W. Stadje, I have obtained results on the distributions of stopping times when we have compound Poisson processes with positive jumps. Two papers appeared, one in *Communications in Statistics, Stochastic Models* (1999a) and the other in *Queueing Systems Theory and Applications* (1999b). This is a very active area and we have followed up with few more publications in recent years.

Mukhopadhyay: When you began your career, statistics was dominated by frequentist ideas and principles. But you have pursued Bayesian techniques through and through. How did that happen?

Zacks: I am not purely Bayesian in my approach to practical problems. On many occasions, I have analyzed data by the frequentist methods. But, I have been keenly aware that in certain problems in multiple comparisons, for example, the classical non-Bayesian approach might result in conflicting conclusions, whereas this will not happen within a Bayesian framework. Moreover, from a decision theoretic point of view, one can claim that Bayesian decision rules are generally admissible. These gave me the motivation and instilled in me the confidence in using Bayesian analysis.

Mukhopadhyay: What if the statistical inferences drawn were sensitive to the choice of the prior?

Zacks: The Bayesian paradigm is quite useful for statistical inferences made adaptively. In a sequential setup, one moves forward as one observes a process step by step, and hence what is assumed a priori in the initial stages should become practically irrelevant after adjusting for the incoming data a number of times. So a sequential experiment is a perfect scenario for applying Bayesian techniques, because here one automatically achieves a sense of robustness of the inferences made even if the initial prior is somewhat off base. This is why I have written extensively on Bayes sequential methods. Theory rather than the philosophy of Bayesian analysis has guided me as a statistician and so you may possibly label me a "practical" Bayesian rather than a "religious" Bayesian.

Mukhopadhyay: Shelley, you are sounding more and more like an empirical Bayesian.

Zacks: Nitis, you are correct. I really believe in the empirical Bayes methods that Robbins (1956) had developed. I think that many inferential problems should be formulated in an empirical Bayes framework. Statistical modeling should increasingly follow this approach. Of course, an empirical Bayes technique in the presence of a large number of parameters would become quite messy.

EXPERIMENTAL DESIGN: ADAPTATION AND DOSE ESCALATION

Mukhopadhyay: Early in your career, you had worked with weighing designs. How did that come about?

Zacks: Having worked with Ehrenfeld as my adviser, I was initially trained to do research in experimental design. During my tenure at Kansas State University, I met Kali Banerjee who had published extensively on weighing designs in *The Annals of Mathematical Statistics* (1948, 1949). I saw his work, had some discussions with him and decided to pursue ways to apply randomized procedures to fractional weighing designs (*The Annals of Mathematical Statistics*, 1966b).

Mukhopadhyay: How did you move into the area of adaptive designs?

Zacks: Adaptive designs are very important because these have direct practical relevance in clinical trials for problems associated with dose-escalation schemes and the determination of an optimal dose. I started working in this area in the early 1970s when I was at Case. From the very beginning, this area seemed wide open to me.

In Phase I clinical trials, the problem of dose escalation involves the determination of the appropriate dosage levels of a drug. The objective is to guess the highest possible dose that does not generate a lethal level of toxicity. The available methodology at the time consisted of a conventional "up and down" technique that was essentially nonparametric in nature and did not have any control on the level of risk. B. H. Eichhorn and I decided to approach the dose-escalation problem parametrically.

Mukhopadhyay: What kind of parametric models did you two have in mind?

Zacks: Consultations with physicians and pharmacologists led us to assume that the toxicity level, measured by a quantitative variable, had a lognormal distribution and we developed a sequential search procedure to determine the maximum tolerated dose. At

FIG. 10. *Shelley Zacks and P. K. Sen during the taping of the "Research Panel Discussion" organized by the Students' Seminar Series, University of Connecticut, on May 4, 1990.*

the same time, Eichhorn and I controlled the probability of toxicity above some allowed threshold (*Journal of the American Statistical Association*, 1973).

Mukhopadhyay: How was the parametric approach received by your peers?

Zacks: In March 1973, J. N. Srivastava organized an International Symposium on Design and Analysis of Experiments at Colorado State University. In the first plenary session, I gave a lecture emphasizing parametric modeling of the adaptive process. Many in the audience appreciated the newly proposed approach. I remember that J. Neyman, J. Kiefer and M. Zelen were in attendance. Neyman asked, "Why didn't you use stochastic approximation obtained via Robbins–Monroe process?" My response was, "The Robbins–Monroe process provides a nonparametric framework. The parametric adaptive procedure is more efficient as long as the underlying model is justified and approved by the clinicians. The justification depends on the type of toxicity measurement taken on each subject. Our approach relied heavily upon the clinicians' beliefs and input."

SURVEY SAMPLING

Mukhopadhyay: Early on, you developed influential Bayes sequential designs for sampling from a finite population (*Journal of the American Statistical Association*, 1969). I also note that you have repeatedly returned to finite population sampling. How about some of your decision theoretic results?

Zacks: I found that the literature was filled with many interesting results for estimators of the population total τ or mean μ on the basis of samples drawn from a finite population. While the estimated population standard deviation $\hat{\sigma}$ was routinely used to provide estimated standard errors for $\hat{\tau}$ or $\hat{\mu}$, I quickly realized that one traditionally used a plug-in type estimator for σ. My primary interest was to estimate the population variance σ^2 itself. I proceeded to locate "good" estimators of σ^2 from a decision theoretic point of view, and discussed the class of Bayes equivariant estimators of the population variance and derived an explicit formula for the example with exponential priors (*Communications in Statistics, Theory and Methods,* 1981a).

Mukhopadhyay: You have worked extensively on model-based inference in a finite population. Would you contrast the ways inferences are drawn using a more traditional approach and a prediction model?

Zacks: The traditional approach considers a finite population as a static collection of units. A probability space is defined by considering the sample space of all possible samples, and a probability function is defined on the corresponding algebra of events. This probability function, called the *sampling strategy*, yields the randomization technique for selecting a sample.

The model theoretic approach assumes that the values of the variables, observed on the units in the population, are themselves a sampling realization from some hyperdistribution. This is the "superpopulation" approach. In this framework, the population itself is treated as a random sample (with replacement) from the superpopulation itself and the observed data are actually a subsample. The quantities of interest in the population, such as the population mean, are now statistics of interest. We do not estimate statistics, but rather predict them. That is why this approach is referred to as *prediction theory*.

Mukhopadhyay: Your collaboration with colleagues from Sao Paulo in Brazil has been very fruitful, has it not?

Zacks: Yes, indeed. I did some work with Jose Rodriguez on utilizing the (expectation–maximization) EM algorithm to predict the values of the unseen portion of the finite population (*Statistics and Probability Letters,* 1986). Subsequently, J. Rodriguez, H. Bolfarine and I worked together on some asymptotic results for finite population sampling (*Statistics*, 1993).

Mukhopadhyay: Did H. Bolfarine spend some time in your department when you two were collaborating on the Springer monograph, *Prediction Theory for Finite Populations* (Bolfarine and Zacks, 1992).

Zacks: Bolfarine took a one-year sabbatical leave from Sao Paulo and spent it with us in Binghamton. The 1992 Springer book was a direct outcome of our joint work pursued during that sabbatical visit. In the process of writing this book, we discovered many new and interesting results. For example, we found a way to deal with equivariant prediction of a population variance under location-scale superpopulation model (*Sankhyā, Series B*, 1991) and worked on Bayes and minimax prediction problems (*Journal of Statistical Planning and Inference*, 1991).

Mukhopadhyay: Now, let me hear your thoughts about some of the foundational issues. Where do you wish to begin?

Zacks: The theory of finite population sampling has many foundational issues and some of these are deeply rooted, but two names, V. P. Godambe and D. Basu, must be mentioned before some of the issues are discussed. When I joined the University of New Mexico, Basu was in the faculty. At the time, I was searching for the right way to formulate a Bayes sequential sampling design in a finite population. Basu and I used to go to lunch together and discuss theoretical issues of survey sampling. During that period Basu was vigorously writing lengthy articles to raise deep foundational concerns and challenge the status quo head on (Basu, 1971; Ghosh, 1988). I recall that the Bayes sequential formulation that I had ultimately proposed (*Journal of the American Statistical Association*, 1969) was shaped and influenced by those discussions. Basu's approach has mainly been Bayesian in nature, whereas Godambe's approach has largely been non-Bayesian.

Mukhopadhyay: Did you get a chance to interact with V. P. Godambe?

Zacks: I met Godambe at Johns Hopkins University in 1968 when I went there to give a talk. We began exchanging ideas, and a few years later I was invited by Godambe to visit him for several months at the University of Waterloo. We worked together on some fundamental inference problems and investigated their roles in sampling from finite populations.

Mukhopadhyay: An important question is how one may characterize the units one should select as random samples from a finite population?

Zacks: According to the model-based approach in sampling, one should select only those units that are associated with the smallest prediction risk or perhaps the largest Fisher information.

Mukhopadhyay: Is it fair to say that you normally prefer a superpopulation model-based approach?

Zacks: It depends on the problem and the amount of available prior information. In many situations, a superpopulation model-based approach in finite sampling might be preferable. Under this framework, one's objective is to predict population quantities (for example, the population total) with the smallest predictive mean square error. In some sense, the traditional (equal probability) random sampling, that is, the SRSWOR (simple random sampling without replacement), can be shown to be a minimax strategy. But, if one has a priori expert opinions available, then one may decide to implement a Bayesian (or an adaptive Bayesian) sampling strategy. This might provide substantial reduction in risk compared with the minimax approach. I think that one would opt for a (equal probability) random sample only when a priori one knows very little about the population under consideration.

Mukhopadhyay: But, then I may ask who is an "expert" and who is not? Whose opinion or information should one trust?

Zacks: In a Bayesian framework, one can provide a resolution between alternative models suggested by experts. One considers a likelihood function that is a mixture of the likelihood functions of different models, where the mixing probabilities are obviously subjective ones. One may assign more weights to the models provided by the experts. If one cannot decide a priori whether one model is preferred to another, then all possible competing models may be mixed with equal probability.

If one presumes that any mistake due to subjectivity of the choice of a model can be too costly or risky, then quite possibly one would instead use the minimax methodology, which will essentially coincide with the SRSWOR.

BOOK WRITING

Mukhopadhyay: You have published six books. Let us begin by discussing your gigantic 1971 monograph, *The Theory of Statistical Inference*. Why did you embark upon writing that authoritative book at the time when you did? How long did it take you to write it from start to finish?

Zacks: In the 1960s there was no advanced book on the theory of statistical inference in the market. We relied upon Cramér's *Mathematical Methods of Statistics* (1946), but it was almost 20 years old and, hence, did not include many important topics that had been developed. I was already preparing my own lecture notes for a number of years, so I thought that it was the proper time to write this book.

It took almost five years to write the book using the lecture notes I had. This was long before the phrase "camera-ready copy" came along. Regrettably, LaTeX was not available at the time. Such a lengthy text unfortunately had many composition errors and I had to correct these errors by hand many times over. But at the galley-proof stage, I found to my surprise that many corrections suggested earlier were not incorporated. This aspect of authoring a book was quite frustrating for me.

Mukhopadhyay: Among all the books that you have written, would you say that you are perhaps best known for your 1971 book on statistical inference?

Zacks: I should probably answer your question by saying "yes."

Mukhopadhyay: Any reader will quickly realize that practically in every major area of your research interest, you have also published a book. Why is that?

FIG. 11. *R. C. Bose Memorial Conference in Fort Collins, Colorado, July 1995. J. N. Srivastava* (*first row center*), *C. R. Rao standing diagonally on Srivastava's left, Shelley Zacks* (*with dark glasses*) *standing behind C. R. Rao, and others*.

FIG. 12. *Shelley Zacks with a group of musicians in a recital, Binghamton University, 1997.*

Zacks: You may say that this is one of my characteristic features. I believe that I prepare good class notes, give clear lectures and I think about sharing these lecture notes with a larger audience, so I tend to write books in areas that are of interest to me. But, in a book the reader finds a synthesis, that is, a sense of how some of the different approaches fit together within a field. That is why I have written a number of books.

Mukhopadhyay: Throughout your career, you have shouldered heavy editorial responsibilities. In particular, you have been associated with the *Journal of Statistical Planning and Inference* (*JSPI*) for a very long time.

Zacks: Yes, I have been associated with the *JSPI* since its inception, first as an Associate Editor and then as its Coordinating Editor and Advisory Editor. In 1982 P. K. Sen and I became Co-Chief Editors of the *JSPI* and shouldered this responsibility for nearly three years. During 1998–2000, I served as the Executive Editor of *JSPI*. This is a very demanding and time-consuming position.

Mukhopadhyay: How much time do you normally spend on this and other editorial commitments?

Zacks: All these editorial jobs consume a lot of time. I viewed commitments to referee or handle papers in a timely fashion as absolutely essential for the existence and growth of journals and for the ultimate dissemination of knowledge. I feel that I have devoted a significant portion of my time and energy to these important activities during the last 40 years. I hope that I contributed to the growth and welfare of statistical science.

Mukhopadhyay: The statistical community remains grateful for your extraordinary service.

FIG. 13. *Shelley and Hanna Zacks with Yuval after Yuval was commissioned as an officer in the U.S. Army, 1983.*

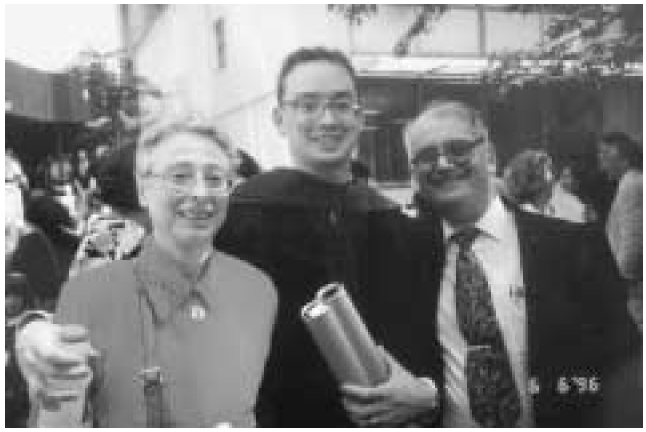

FIG. 14. *Shelley and Hanna Zacks with David after David's graduation (both M.D. and Ph.D.) from Albert Einstein College of Medicine, 1996.*

IMMEDIATE FAMILY

Mukhopadhyay: When and where did you get married?

Zacks: I married Hanna A. Bilik in 1955 in Jerusalem. We met as students at the Hebrew University. Hanna received her B.A. and M.A. degrees in philosophy, history and education from Hebrew University. As I previously mentioned, we came together to the U.S. in 1960 for our doctoral studies.

Mukhopadhyay: What was the topic of your wife's Ph.D. thesis?

Zacks: Hanna received her Ph.D. from Columbia University in 1965 based on her dissertation, *Henry Bergson and Allied Philosophers*. After several years of teaching philosophy, she pursued psychotherapy in the School of Applied Social Research of Case Western Reserve University and received a second Ph.D. degree in 1980. During the past 15 years, Hanna has been a psychotherapist in the Broome County Mental Health Clinic.

Mukhopadhyay: Do you wish to mention your children?

Zacks: In 1962, when we were visiting Stanford, our son Yuval Joseph was born. In 1968, our second son, David Noam, was born in Albuquerque.

Mukhopadhyay: What do they do professionally?

Zacks: After graduating from William and Mary College in Williamsburg, Virginia, Yuval was commissioned as an officer in the U.S. Army, Artillery Branch, and he served both in South Korea and Germany. Yuval presently holds the rank of Lt. Colonel and serves as U.S. Assistant Military Attaché in New Delhi, India. He is married to Heidi Vierow who received her Ph.D. in classics from Duke University.

David graduated from Cornell University in biology and received both M.D. and Ph.D. degrees at Albert Einstein College of Medicine. He did his residency and fellowship in ophthalmology at the Massachusetts Eye and Ear Institute, Boston. He is now an Assistant Professor of Medicine at the medical school of the University of Michigan. He is married to Susan Harris, a pediatrician.

Mukhopadhyay: Do you have grandchildren?

FIG. 15. *Shelley and Hanna Zacks with their granddaughter Gabrielle, 1995.*

FIG. 16. *Shelley Zacks in Storrs, Connecticut. October 27, 2000.*

Zacks: Yuval and Heidi have one child, Jacob Elkana. David and Susan have two children, Gabrielle Sima and Daniel Soul. Hanna and I visit them as frequently as we can in order to enjoy being with our grandchildren.

Mukhopadhyay: That sounds wonderful. Do you have any hobbies?

Zacks: I play viola. I am the principal viola player for the Binghamton Community Orchestra. I have continued playing chamber music regularly. I read books as much as I can and I especially enjoy reading modern literature in Hebrew. Hanna and I both love to travel.

Mukhopadhyay: What are your plans for the future? Is retirement on the horizon?

Zacks: I am not thinking of retirement right now. I hope to be able to devote more time for research.

Mukhopadhyay: What research areas do you have in mind?

Zacks: I would like to address distributions of stopping times arising from stochastic processes. I also wish to apply some of these results to risk analyses in actuarial contexts.

Mukhopadhyay: Shelley, anything else?

Zacks: I will probably stay away from additional editorial responsibilities. This way, Hanna and I hope to have more opportunity to travel and also spend time with our family.

Mukhopadhyay: On that happy note, we will end this conversation. Thank you very much, Shelley, for taking the time to have this conversation with me. I wish you a long, healthy, happy and productive life.

Zacks: Thank you, Nitis.

ACKNOWLEDGMENT

I thank Professor Leon Jay Gleser for valuable suggestions and thorough editing of the manuscript.

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