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A Conversation with Milton Sobel

Nitis Mukhopadhyay

Milton Sobel was born in New York City on August 30, 1919. He earned his B.A. degree in mathematics from the City College of New York in 1940, an M.A. degree in mathematics and a Ph.D. degree in mathematical statistics from Columbia University in 1946 and 1951, respectively. His Ph.D. thesis advisor was Abraham Wald. He has made substantial contributions in several areas of statistics and mathematics—including decision theory, sequential analysis, selection and ranking, reliability analysis, combinatorial problems, Dirichlet processes, as well as statistical tables and computing. He has been particularly credited for path-breaking contributions in selection and ranking, sequential analysis and reliability, including the landmark book, *Sequential Identification and Ranking Procedures* (1968), coauthored with Robert E. Bechhofer and Jack C. Kiefer. Later, he collaborated with Jean D. Gibbons and Ingram Olkin to write a methodologically oriented book, *Selecting and Ordering Populations* (1977), on the subject. He has published authoritative books on Dirichlet distributions, Type 1 and Type 2 with V. R. R. Uppuluri and K. Frankowski. He is the author or coauthor of more than one hundred and twenty research publications, many of which are part of today's statistical folklore. During the period July 1940 through June 1960, his career path led him to work at the Census Bureau, the Army War College (Fort McNair), Columbia University, Wayne State University, Cornell University and Bell Laboratories. From September 1960 through June 1975, he was Professor of Statistics at the University of Minnesota, and from July 1975 through June 1989 he was a Professor in the Department of Probability and Statistics at the University of California at Santa Barbara. He has since been a Professor Emeritus at UC Santa Barbara. He has earned many honors and awards, including Fellow of the Institute of Mathematical Statistics (1956) and Fellow of the American Statistical Association (1958), a Guggenheim Fellowship (1967–1968), a NIH Fellowship (1968–1969) and elected membership in the International Statistical Institute (1974). He continues to think and work harder than many half his age and still goes to his department at UC Santa Barbara every day. Milton Sobel remains vigorous in attacking and solving hard problems.

The following conversation took place in Room 425 at the Anaheim Plaza Hotel on Sunday, August 10, 1997, during the Joint Statistical Meetings at Anaheim, California.

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EARLY UPBRINGING AND FAMILY HISTORY

Mukhopadhyay: Let us start at the very beginning. Milton, would you give a few details about your parents? Where did they come from?

Sobel: My parents, Samuel and Tillie Sobel, were Jewish immigrants who arrived in America in 1915, immediately following World War I, from the

FIG. 1. *Milton Sobel (the baby) with his parents and older brother Harry, 1920.*

Austria–Hungarian empire which later changed hands a couple of times. At one time my parents' homeland was part of Russia and later on, after World War II, became a part of Romania. The small town from which they emigrated was next to a very forceful and important river called Tisza which runs from Hungary into Romania. Sometimes this area is also referred to as Transylvania.

Mukhopadhyay: Did you lose relatives during the Holocaust?

Sobel: I lost a tremendous amount of family during the Holocaust under Hitler's regime in the concentration camps. I particularly remember that my two grandparents, my dad's father and my mother's mother, perished in the Holocaust. I never got to meet them and I grew up never knowing who my grandparents were.

Mukhopadhyay: Where were you born? What did your parents do to earn a livelihood?

Sobel: I was born on August 30, 1919 in Manhattan, New York, and my parents slowly moved uptown, out of the so-called East Side, to better quarters of New York City, eventually living in the Bronx and Queens. As a child, I lived in four of the five boroughs of New York City. I am a thoroughbred New Yorker in that sense. However, I have not lived there now for more than fifty years.

My father was a businessman. He ran a grocery store first and then owned a gas station and garage. My mother looked after the family and also helped in managing these stores in any way possible. She was a very intelligent lady. I worked with my father both in the grocery store and the gas station at different times. In order to serve the customers, most of whom were Jewish men and women, I had to learn Yiddish, without ever realizing that I was learning another language that would become immensely useful in later years.

During the war, my mother took in a boarder named Ambutter who was a very interesting person and he had some effect on my life too. He called himself the Jewish Caruso. He was a singer and a very nice man. I may add that, in spite of hardship, my parents valued education over anything else.

Mukhopadhyay: Milton, I have found that many Jewish parents consider education as the most important thing in life, even when facing the onslaught of the worst possible oppression.

Sobel: Under Jewish philosophy, our people have always valued education. My family was poor, but I had no trouble going to school or college.

Mukhopadhyay: What can you recall about your life during the elementary, junior high and high school days? Did you start showing any added aptitude in one subject area over others at some stage?

Sobel: I remember some of the schools in the Bronx. I remember getting into trouble in my kindergarten or first grade. One day a zeppelin, a special kind of airship, came along and everybody rushed to the classroom windows to get a glimpse, but none of them really got into trouble, it seemed, except me. I thought that I was a scapegoat. I did not like the idea of being pushed around by the teacher. In the end, my mother had to transfer me to another school because, I guess, I must have sassed the teacher after the incident.

The zeppelin, named after the scientist who designed it, played big roles in some of the ocean crossings and in wars, particularly in World War I. The Germans were quite good at using zeppelins. But then of course there was one big accident in northern New Jersey involving a zeppelin, and after that its production stopped.

Somehow in junior high school I was doing pretty well in mathematics. But mathematics did not strike me at that time as a subject to work on or to earn a living with. Instead, biology meant science

FIG. 2. *Sobel attending school in New York City.*

to me that could lead to a medical profession. When I was growing up, I wanted to be a doctor. But during the years of the Great Depression, there was no money around, and one day my mother told me, "We are not going to be able to afford to send you to medical school," and that was a big blow to my ego. I started looking around in search of alternate career paths. Suddenly, I thought to myself—"Why not try mathematics?"

Mukhopadhyay: Did you happen to meet inspiring teachers in science and mathematics?

Sobel: In junior high school I had very good teachers in mathematics, arithmetic and then in biology and science. I was lucky to have had very good teachers who inspired me. I joined the Biology Club and went on several trips to observe, study and appreciate nature. I remember raising mice at home, collecting frogs' eggs in Yonkers and watching their transition into tadpoles. For a science project at the high school level, I remember looking for and examining crustaceans in a little river in the Bronx near my high school, Evander Childs.

Mukhopadhyay: Milton, let me ask you this. You were born in the United States and you were very comfortable with English as your first language. But at home, your parents probably wanted you to learn and live the Jewish life, its values, discipline and culture, as well as being fluent in the language spoken at home, namely Yiddish. As a child, how did you manage such apparently conflicting demands, so to speak, and did you feel any sense of insecurity perhaps that you might not fit in with other children at the school or in your neighborhood?

Sobel: When I came along, my parents and I were learning English at the same time. My parents could speak Hungarian, and when they wanted to keep something secret from us, they would converse in Hungarian! Hungarian was not that simple for me (or anyone else) to learn! In the grocery store, while serving customers, most of whom were Jewish, I used to make jokes by mixing their words in Yiddish and mine in Latin, which I had just started to learn right around that time. For example, if they wanted potatoes, I would give them, from pateo, patere, patevi, patetis (to fear, in Latin). I was never terribly good with languages, but the Yiddish I learned as a child gave me a strong sense of confidence later. Around 1960 I went to Paris to give a lecture at the Sorbonne. Five minutes before my lecture, someone asked me if I were going to talk in English or French. I thought to myself, "What kind of nonsense is this to ask me that question only five minutes before my lecture?" I said, "Ted Anderson was here a week ago and he spoke in English, and that is what I am going to do." The gentleman said, "There is no need for you to get upset. The language of presentation is after all the speaker's choice. We merely wanted to know your choice." In the next five minutes, I thought about it and when I started my lecture on group testing, I delivered it in French for the whole hour although I did not even know the French translation of the phrase *group testing*, the main topic of the whole lecture. Learning different languages and experiencing different cultures can really build up one's confidence. I did not face any significant difficulties because of differences in emphasis and discipline that I found at home and outside as a child.

Mukhopadhyay: We often hear about courses offered at different universities and colleges on what is known as multiculturalism and diversity. These types of courses are supposed to create awareness about differences among various cultures, ethnic and religious practices, and about accepting people as they are. As a child you seem to have practiced that in a natural way. Is that not the way it is supposed to be taken care of in the first place? How can a course or two on such a topic at a college or university possibly play the role of

something that should ideally be learned by the children at home and in the community so that as grownups they are likely to become more sensitive and tolerant about other customs and cultures?

Sobel: It is hard to understand what would give one the necessary confidence. If you get a letter from somebody that no one else can read and comphrehend but you, then that generates confidence in you, and slowly you become interested in learning more about that language and the culture that goes with it. There is no need to have any conflict here. One thing enriches the other. One does not necessarily take the place of the other. Often some people think that and if you make room for something else, the original identity would eventually get lost. If all the parties involved have enough confidence and self-esteem, one can learn from the other, without feeling threatened by or threatening others, and both would become better for it. But the basic sense of tolerance of people different from yourself has to be implanted in you by your family's views and practices. I am grateful to my parents for their teachings.

Mukhopadhyay: Do you wish to mention your brothers and sisters?

Sobel: Yes. I have two brothers. My sister died at a very early age, so I did not really get to know her at all. My older brother, Harry Sobel, was mechanically inclined and he worked with my father. Chronologically, I am in the middle and I grew up during the Great Depression years. After that, the family's financial situation got a little better and when my younger brother, Garrison L. Sobel, grew up and wanted to go to medical school, there was money available for him to chase the dream. Later, he became a neurosurgeon.

CITY COLLEGE NEW YORK

Mukhopadhyay: In 1936, you graduated from high school. Did you immediately decide to attend the City College of New York (CCNY)? What tempted you to join CCNY? Who were some of your contemporaries at CCNY?

Sobel: The admission to CCNY was given to individuals solely based on credentials and not to all applicants. It was like a prize and privilege for anyone to be selected by CCNY. I was very pleased to be accepted and I found that I was in the company of very smart people, some of whom later became Nobel Prize winners. For example, Professor Kenneth Arrow who received the Nobel Prize in economics was my classmate at CCNY. Herbert Solomon, Herman Chernoff, Harvey Cohn, Frank Beckman and Oscar Wesler were also my classmates at CCNY. I graduated from CCNY in 1940, majoring in mathematics.

Mukhopadhyay: At CCNY, who were some of the inspiring professors in mathematics?

Sobel: Professor Bennington P. Gill, who taught me number theory, was very inspiring; I will never forget him.

Mukhopadhyay: What did you particularly like about the way he taught you number theory?

Sobel: The most important thing was that he made me love the subject. He was always so full of enthusiasm. The incident I remember most clearly about him was the advice he once gave me. I used to turn around the advertising paper from grocery markets and do homework on the blank side. Professor Gill would see the grocery ads on one side and my solutions to homework problems in mathematics on the other side, and this went on for a while. One day he could not take it anymore and became angry. Professor Gill said, "I want you to take mathematics more seriously than this and I want you to use a real notebook. I do not want to see any more of this grocery stuff." He was angry but still he got his message across in a nice way. That fellow had a big influence on lots of people.

There were other inspiring teachers as well. There was Professor Emile Post, who is well known in the field of logic. His name comes up in many books, even more so than Gill's. Somehow I was more influenced by Gill than by Post. From both of them I had learned to appreciate the formal theory of logic and mathematical proof.

Mukhopadhyay: Do you recall other interesting stories from your days at CCNY?

Sobel: I remember very vividly a curious incident. Bertrand Russell was supposed to come one semester and teach a class in mathematical logic at CCNY. However, it turned out that some housewife from Brooklyn (whose child was not yet in kindergarten) read some of his writings about free love and did not really like his ideas. She actually sued New York City and kept Bertrand Russell from teaching a course on mathematical logic at CCNY! This was just astounding. Because of this unknown housewife's lawsuit filed in a court, we were deprived of a lifetime opportunity to hear lectures from a star like Bertrand Russell.

Mukhopadhyay: [Both laugh]. How disappointing! Any other stories?

Sobel: I became interested in complex analysis while reading a book by K. Knopp which at

that time was not yet translated from the German. This came from a series of books, almost like the Springer-Verlag series of German books (called the Sammling-Goschen Series) that were not yet translated into English. The fact that it was written in German led to an interesting incident. I took that book with me when I joined the army. One time a Major saw me reading that book in the morning, out in the field somewhere, and he wanted to know what I was reading. I said, "It is a math book," and he said, "I want to see it." Once he saw the book, he exclaimed, "This is in German!" I replied that I was studying mathematics and German at the same time, and the Major snapped back, "You might be a spy!" Such a line of questioning took place only because I happened to know a little German, that in turn came to me through my knowing Yiddish.

Mukhopadhyay: Did you happen to take any courses on statistics at CCNY?

Sobel: I took some courses in statistics from a professor named Firestone and the book we followed was authored by a Canadian named Keeping. It was a book on mathematical statistics. I remember it very well because it made an impact on my appreciation for the field of statistics.

ON THE WAY TO THE CENSUS BUREAU AND TO MEET DEMING

Mukhopadhyay: What happened after you graduated from CCNY?

Sobel: When I graduated from CCNY in 1940, the country was still in the middle of the Great Depression. One afternoon I told my mother that I was going to see a movie and I left. An hour later she came running after me into the movie theater and said, "Milton, Milton, come home quickly." I asked, "Mom, what is the matter?" She said, "We received a telegram for you," and I sensed that it must be something important. I immediately headed for home with my mother. Once at home, I opened the telegram, and it read "REPORT TO THE CENSUS BUREAU ON SATURDAY AT TEN O'CLOCK IN THE MORNING." But it was already five o'clock in the afternoon on Friday and I was supposed to report to the Census Bureau in Washington, D.C. before 10:00 A.M. on the very next day! I had never been to Washington, D.C. before, and I certainly had no idea about how to get there from New York City within the next seventeen hours or so. That was quite a challenge for me.

Mukhopadhyay: Who sent that telegram? Were you supposed to see anyone special at the Census Bureau?

Sobel: I was not asked to report to anyone in particular at the Census Bureau. I was just told to go there in order to start working on something related to the 1940 census. I *did* manage to get there on Saturday by 10:00 A.M.; that was a big accomplishment for me. I worked there for two or three days and started to explore the facilities within the Bureau. I found an office with the name Dr. W. Edwards Deming (Mathematical Statistician) written on the door.

Mukhopadhyay: Had you heard Deming's name before?

Sobel: At that time I never knew who he really was. I had enough nerve to go up and knock on his door and talk to him. He was very nice to me, and within two or three days after arriving at the Census Bureau, I was working with him. Instead of doing the other work, involving mostly uninteresting coding, I was then working with the great statistician Ed Deming. It was such a thrill for me! This opportunity turned out to be important for me because for the first time I came very close to a real statistician working on actual practical problems in sampling. I told Deming that I had lots of training in statistics and I thought that I could work with him. I immediately started working with Deming on some projects in sampling and this continued for about three months until the project was completed. Later, Deming became my teacher at the Department of Agriculture Graduate School, where Neyman, Madow, Girshick and others also taught.

DRAFTED BY THE U.S. ARMY

Mukhopadhyay: One day you dropped by the Draft Board to find out whether you were going to be drafted and you encountered an interesting situation. Would you like to elaborate on that?

Sobel: After spending nearly three months working with Deming, I worked for the Navy Yard for about two and half weeks. I was working with a lathe machine at the Navy Yard in Washington, D.C., and one day I walked up to the Draft Board to find out what was "cooking." A couple of people were talking inside, and I do not know why, but I started to eavesdrop on that conversation. Someone said, "We got a letter here about some guy who has just started working at the Navy Yard and he has been there for two weeks," and then the other person said, "Oh, then we have to let him stay there." The first person replied, "No, according to the fine print, we can grab him since he has not yet been there for at least three weeks," and then the other person added, "Let's draft him quickly." After that,

the first thing I knew was that I received a letter from President Franklin D. Roosevelt saying, "Greetings. You are now in the U.S. Army." By the way, that is the only lottery that I ever won in my life! On TV Roosevelt pulled out those little pellets out of a bell jar and my number came up in one of the earliest draws. I joined the army in 1940 and I was sent to Baltimore, Maryland, where I spent almost a whole year in basic training.

Mukhopadhyay: Were you allowed to leave the army after a year then?

Sobel: I was waiting for the year to be over so that I could get discharged. Then someone said, "Sobel, have you heard the news about Pearl Harbor?" and I asked, "What happened?" He replied, "We're in the war and you're in for the duration." Thus, I ended up spending four and half years in the army instead of one year.

Mukhopadhyay: Where and how did you spend the four and half years working for the army?

Sobel: Well, that is an interesting and long story. I will try to tell you the story quickly. I was in an infantry outfit within the activated Baltimore National Guard. We happened to be in Philadelphia at that time guarding some electrical installations on Gerard Avenue and I was told that the First Sergeant was very anxious to talk to me. The first thing that came to my mind was along the lines of "What did I do now?" I started to worry, but I was on guard duty at the time and once I was done, I went to see my First Sergeant. He said, "Sobel, what do you know about dietetics?" and I replied, "I'm an expert," thinking that this might be a good opportunity for me to transfer out, although secretly I was quite apprehensive about the possibility of being ordered to cook or do the dishes or even clean the latrine. This fellow knew to cuss better than anyone else in the Army, even better than General Patton. He first cussed me out really strong, and then said, "Sobel, turn in your rifle. You are going on a weapons-carrier tonight." This trip started at midnight (in December, without any blanket!). I became nervous and asked, "Where am I going?" and he replied, "You're in the Army now. You don't ask questions. You just do whatever you're told." Next thing I remember is that I turned in my rifle and blanket, took my duffle bag and got on a weapons-carrier at midnight. I tried to find out about the destination from the driver but he was also tight-lipped, saying only that it was a secret. It was a December midnight's shivering cold! From the glimpse of road signs that I could barely read in the dark, I assumed that we were going toward Baltimore or Washington, D.C. and I was pretty certain that I would shortly end up in a cooking school. Just wait till they find out that I can't even boil an egg! Sure enough, we traveled into an army base in Washington, D.C. and we got off near the kitchen; the first group of people I saw were wearing cooks' hats and aprons. We then got inside and the driver asked me to sit down to have a bite. I was now sure that it was a cooking school and I counted my blessings. Slowly the kitchen area became crowded with people—for some sort of assembly, I thought. Then individual names were being called and I saw them leaving one by one. In the end everybody left the dining hall except me. I thought that perhaps they did not want me there. I stood there waiting for a good twenty or thirty minutes until finally someone came around and said, "Are you Sobel?" and I said "Yes, I am." Then he led me to his office where they were working on statistical work, and not at all on dietician's work. I then realized that my First Sergeant had misread the transfer order—he had confused "statistician" with "dietician!" I took up a statistician's position at the Army War College (also known as Fort McNair) for two years, from March 1942 through March 1944.

Mukhopadhyay: So, now this was a real statistician's job. What was in store for you next?

Sobel: At the time of the Battle of the Bulge, in the winter of 1944, I went to a training camp outside of Paris, France, known as Camp Lucky Strike, with ice and snow on the ground. We stayed there for a couple of weeks and the ice started to melt, and the place became terribly muddy, with slush and mud at least a foot deep. General George C. Patton, Jr., who lost his command in Sicily, Italy where he slapped a soldier, started asking for command and wanted to get back into the war. General Eisenhower gave him the Third Army to lead, so Patton became my General in France. We had to brave some 88 mm artillery fire power of the Germans in order to break through and cross into Germany. While on the road to Berlin (Stalin got there ahead of us), we received a change of order to go to Austria (it was then called the Redoubt area). I became one of the first U.S. soldiers to enter the Nazi concentration camp at Mauthausen, Austria, about thirty kilometers east of Linz, Austria. Austria is where I did a good deal of my military duties. Somewhere along the line, someone asked, "Who knows how to say anything in French?" I sang a few bars of a French song ("Mademoiselle from Armentiers") and immediately became the French interpreter. Then he said, "Do you know any German?" and I came up with a few German words (like "Yavol" for "yes,

of course") and I became the German interpreter on the spot. Next he asked, "Do you know any Russian?" and I said "not much" in Russian (nemnogo). So I also became the Russian interpreter.

Mukhopadhyay: What went through your mind when you first saw the Nazi concentration camp at Mauthausen, Austria? Did you see any children there?

Sobel: I felt the chill in the air. There were lots of living corpses (so to speak) and there were many people in captivity who were very weak. I saw people who were alive, but could hardly move. Many needed rebuilding of bones and flesh in their bodies. There were lots of women in this group, but I did not see many children in this camp. Many children must have perished. My job was to take part in the liberation of this concentration camp and, soon after that, just move on. We did not go there to stay for any length of time. After the victory in Europe, I was sent back to France and soon I got on a troop transport heading for the war against Japan. While we were crossing the Atlantic, however, the news came that the war against Japan had also ended. Eventually, we disembarked at a port in New Jersey, and soon after that we moved to a camp in Tennessee. I received my final discharge papers from Camp Breckinridge, Tennessee.

Mukhopadhyay: I understand that you were drafted in the first part of 1941 and got discharged in 1946. During this period of four and half to five years, how did you prepare yourself for a future career in statistics?

Sobel: I took some correspondence courses in mathematics from the University of Chicago. I learned a lot about complex variables, mostly from Knopp's book, while I was in the Army. There were arrangements already in place between the U.S. Army and the University of Chicago for such course offerings, earlier than with any other universities. So I took several such courses from the University of Chicago through the mail and earned some credits. At the Census Bureau, the contact I made with Deming had created a lasting impression on me. While in the service, I read a lot of Deming's writings, and I got to learn and sharpen the tools and tricks in the area of survey sampling through Deming's collaborations with experts such as Madow, Hansen and Hurwitz. During my service in Washington, D.C., I also took several formal courses in statistics at the Department of Agriculture Graduate School, and I studied a lot on my own to keep up with the literature.

COLUMBIA UNIVERSITY AND MEETING ABRAHAM WALD

Mukhopadhyay: When you were discharged from the services in 1946, your parents were still living in the Bronx and you returned home. Then what did you do to further your career?

Sobel: Yes, I came home and decided to attend the graduate school at Columbia University. I was still in uniform. On the first day of classes, I went there and found a man standing in the hall. I did not know who he was. He asked, "Do you know where you are going?" I said, "I am going to this classroom right here." He said, "Do you know what kind of a class this is?" and I said, "Yes, it's a class on multivariate analysis." He then said, "You are going to study multivariate statistics, but have you taken any courses on univariate statistics?" and I replied "No." The man looked surprised and said, "How can you do multivariate statistics without first learning univariate statistics?" I replied, "I don't know. But I am planning to take both multivariate and univariate statistics simultaneously." I thought that the person I was talking to was possibly only an advisor or a guard. He turned out to be Professor Abraham Wald, who was going to teach the multivariate analysis course. I impressed him quite a bit at that point by showing my ability to do multivariate statistics without previously taking the univariate theory. At the first instance when I saw him standing in the hallway, it did not even occur to me that he would be my teacher and advisor.

Mukhopadhyay: Eventually, Milton, you decided to work with Abraham Wald.

Sobel: He turned out to be my Ph.D. thesis advisor and I was very happy with the way things developed, except for the tragic airplane accident in 1950 in which both Wald and his wife, Lucille, perished.

Mukhopadhyay: At Columbia, who were the other faculty members in statistics at that time? Who were your teachers?

Sobel: Another senior person was there at that time, Professor Harold Hotelling. He did not stay very long after I arrived at Columbia. He moved to the University of North Carolina at Chapel Hill and took some of the better and more senior students with him. I saw Herb Robbins at Columbia, but he was primarily connected with Richard Courant of New York University and joined the staff at Columbia a bit later. Robbins was Courant's protégé. That was the time when Richard Courant and Robbins were writing their masterpiece, *What is Mathematics?* (Oxford University

FIG. 3. *Abraham Wald* (*seated far left*), *his wife Lucille* (*standing far left*), *her mother* (*seated far right*), *and Wald's two children, Betty on the floor and Robert on his dad's lap, Milton Sobel* (*standing far right*) *in California, 1949. The younger lady is unknown.*

Press, 1941), which was translated into many languages, including Chinese. Mathematical statistics was coming of age, and Hotelling was the chairman. Jacob Wolfowitz was on the faculty. Howard Levene had been on the faculty for as long as I can remember. Aryeh Dvoretzky was also on the faculty, and he had already been collaborating with both Wald and Wolfowitz. I attended some of his lectures. Ted Anderson was a junior faculty member at that time. Jerzy Neyman, R. C. Bose, J. L. Doob, W. Hoeffding and P. L. Hsu were all visiting faculty members at Columbia. I remember auditing or taking a course on multivariate analysis three or four times, each time taught by someone different (e.g., Wald, Hotelling, Hoeffding and P. L. Hsu), and these courses were never the same; in fact each had his own set of notes on the subject. I regard Robbins as my teacher too, but he takes offense when I say that. [Laughs]

Mukhopadhyay: As classroom instructors, did Wald, Hotelling and Wolfowitz have different styles?

Sobel: Oh yes! Some incidences will illustrate the differences in their teaching styles. Hotelling gets into a big classroom with two hundred students and asks, "How many people had complex analysis?" Only a few would raise their hands, and Hotelling would reassure everyone by saying, "That's all right. I am going to teach you all you need to know about complex analysis in one hour," and then he would explain a few results on characteristic functions and related things and leave it at that. Hotelling would simply summarize the main tools that might be useful in statistics and then move on. I remember that he always wanted to get to the heart of the matter fast and not "beat around the bush."

Wolfowitz had a habit of choosing one particular student and focusing only on that individual when he was going to discuss a theorem or some difficult result. I remember one day there was a student with a painful neck problem, and that day Wolfowitz happened to focus on him. Every time he made eye contact with this student or asked him whether he understood the logic behind some steps in the proof, the student will nod up and down, and that seemed to give Wolfowitz the indication that the whole class was with him. But because of neck problems, this poor fellow could nod only up and down but not from side to side, whether he understood a particular step or not. Wolfowitz knew nothing about this and kept talking over everybody's head, and in the end he left the classroom thinking that everyone understood the material. But in fact hardly anyone in the class understood the lecture that day. By the way, I should mention this: Wolfowitz was so highly impressed with Kolmogorov that when he went to Russia, he made the statement that Kolmogorov could not possibly be one individual and that he must have been an "institution." He used to bring Kolmogorov's book to the classroom at Cornell and translate (from the Russian) word for word from the book as he taught. That is how impressed Wolfowitz was with Kolmogorov. Wolfowitz correctly claimed that Kolmogorov never got enough praise for his axiomatic development of probability theory.

Wald was such a kind and gentle person. Every time some argument came up, it was Wolfowitz, rather than Wald, who took up the argument. Wolfowitz fought usually to preserve Wald's reputation, often saying that such and such results were first obtained by Wald or these were already in Wald's book, *Statistical Decision Functions* (1950) or in one of his papers, and it would be wrong for anyone to prove or try to publish the same result all over again. Wolfowitz would often quote the relevant page numbers from Wald's work. He continued doing this sort of thing at seminars and conferences even ten years after Wald's death.

In the classroom, Wald was quite terse. He always stuck to the point and he never beat around the bush. He had prepared lecture notes, for example, on multivariate analysis, design of experiments, analysis of variance, and these notes were models of clarity. His book *Sequential Analysis* (1947) was in my opinion the star of his writings. But in Wald's classroom, one had to be bright, study and listen carefully. He was a very serious man. Only once he came to class and veered from the subject for less than a minute. He said, "I suppose you have all

FIG. 4. *Left to right: Milton Sobel, Jim Macqueen, Howard Levene and Herbert Solomon.*

heard the sad news. Our dear President Roosevelt died. Now, consider a sequence of i.i.d. random variables . . . "

Mukhopadhyay: In a classroom, did you feel at ease or somewhat intimidated at times to ask Hotelling, Wald or Wolfowitz questions or seek further clarifications?

Sobel: Hotelling and Wald were more approachable in class. Wolfowitz was different. If someone asked something in the class, Wolfowitz would always ask something else to answer a question, perhaps something like "How couldn't you understand such and such?" and that made one feel a little uneasy about raising questions subsequently. Wolfowitz was a little harder to deal with than the others.

Mukhopadhyay: Who were some of your fellow students?

Sobel: At that time I met Jack Kiefer, Ingram Olkin and Charles Stein. They were graduate students at Columbia. Also, Robert Bechhofer, Frank Beckman, Albert Bowker, Herman Chernoff, Peter Frank, Gottfried Noether, Ed Paulson, Herbert Solomon, Henry Teicher and Lionel Weiss were among the contemporary graduate students at Columbia. I may add that Gottfried Noether was a nephew of Emmy Noether, the most famous female mathematician in all history, who came from Germany.

Mukhopadhyay: Did your professors mostly rely upon their own lecture notes to teach classes or did they follow textbooks?

Sobel: Professor Ritt, a mathematician, used his own notes. He mentioned my name in the introduction to his notes and that raised my level of self-confidence substantially. Wald used his own notes, on both design of experiments as well as analysis of variance. R. C. Bose used his own notes on design of experiments, S. N. Roy used his own notes on multivariate analysis, and P. L. Hsu used notes on his own work. Everyone was doing original work and writing their own notes. The outside material we used was from Feller's book *Probability Theory and Its Applications* **1** (1950) and to a lesser extent a preliminary version of E. Parzen's *Modern Probability Theory and Its Applications* (1960).

Mukhopadhyay: Columbia and Princeton are not far from each other. What was the relationship like between these two schools? Any recollections about S. S. Wilks?

Sobel: Sam Wilks was a highly respected statistician. Ted Anderson, in some sense, was his representative at Columbia; he was a direct Ph.D. student of Wilks at Princeton, I believe. Anderson came from Wilks's school of thought and so he filled an otherwise important void at Columbia. I saw Sam Wilks on many occasions and attended some of his seminars.

Mukhopadhyay: In case you had some difficulty, who could you feel free to go and see among the faculty members for advice?

Sobel: Wolfowitz was not really a difficult person to talk to outside of class. But he was extremely time-conscious. One of his favorite expressions was, "Walk me down to the subway and I will talk to you on the way."

Mukhopadhyay: Milton, you have met many visitors wherever you have been and you have yourself visited many places too. For one reason or another, does any such visitor or visit stand out in your mind?

Sobel: Among the people who impressed me after the visits were J. L. Doob and William Feller. These two people influenced me greatly. When I listened to them, I realized that I was talking with the real masters. But Neyman was a different type of master and so was Wald.

Mukhopadhyay: What kind of a different master was Jerzy Neyman?

Sobel: It is not quite clear how to describe Neyman. I felt comfortable with Neyman, much more comfortable than even with my peer, Herb Robbins, for example. Neyman did not have an ego that I had to fight with! And he had a wonderful relationship with Betty Scott and it really impressed me. It made me think that if she could work with Neyman, then I could work with someone like Wald. One thing I

remember about Neyman and Scott was that they used to have informal gatherings during lunch at Berkeley almost on a daily basis. I felt at ease in some sense to approach Neyman. My friend Henry Konijn (who still visits Berkeley annually) felt the same way about Neyman. He chose Neyman to be his advisor.

Mukhopadhyay: As a graduate student, did you visit some of the faculty member's homes?

Sobel: At Columbia, I do not have any recollections of visiting the homes of Wald or Wolfowitz. I recall going to a few gatherings at Deming's home and listened to some jokes told by many illustrious people. I also heard many serious mathematical discussions at those parties. Herb Solomon and Al Bowker both gave parties and I remember them well. I also have fond memories of parties hosted by Howard Levene.

PH.D. THESIS UNDER ABRAHAM WALD

Mukhopadhyay: After finishing the course work at Columbia, you then decided to work under Abraham Wald for a Ph.D. degree in mathematical statistics, right? How did you proceed to talk to Wald about your intention?

Sobel: I went to tell Abraham Wald about this, and he did not mind at all taking me as his Ph.D. student. He was very nice about it. One day he told me, "I have signed up and committed myself to go to Berkeley for the whole year," and immediately I felt so concerned, contemplating, "How am I going to finish my graduate studies if he spends a whole year at Berkeley?" This was around 1948 or 1949. Wald had left for Berkeley. My friend Henry Teicher and I often talked about this situation, and one day we came up with the idea that perhaps we should also go to Berkeley. We both put on our Army uniforms and went hitchhiking to California. Soon after reaching Berkeley, I caught up with Wald and he asked me, "Aren't you Mr. Sobel?" I said, "Sure, of course," and he asked "How did you get here?" Wald did not have a clue that I was coming to California. I tried to explain to him that I was not doing much at Columbia and I would rather be where my advisor was. He was quite pleased with that statement, but he mentioned that he did not like my hitchhiking all this way. He had just a couple of months left of his visit to Berkeley. Wald said, "How will you get back to New York?" and I was almost going to say "Probably by hitchhiking," but he sensed that and said, "You are coming with me." That summer I stayed very close to where Wald was living with his wife and son and daughter, and I managed to get much research work done. Then I started the journey back toward the east as a passenger in Wald's car. I had already accepted a position at Wayne State University in Detroit. While driving back with Wald's family, I was dropped off near Detroit and Wald went to Columbia. I then started teaching at Wayne State. This was in early 1950. Then suddenly (it seems to me very suddenly), I heard that Abraham Wald had gone to India with his wife, Lucille, and that they had both perished in a plane crash. In fact, I did not know whether he had approved my thesis. I had to go to Columbia to discover, from Wolfowitz I believe, that Wald had approved my thesis verbally before he went on his last overseas trip. So I could get my Ph.D. degree without starting all over again.

Mukhopadhyay: Any recollections about Abraham and Lucille Wald's children?

Sobel: When Abraham and Lucille Wald died, their two children, Betty and Bobby, were very small. Betty was about six or seven and Bobby was probably a two-year-old. Bobby (Robert M. Wald) is now a world-famous physicist at the Enrico Fermi Institute, University of Chicago.

SOBEL THE ONLY PH.D. STUDENT OF ABRAHAM WALD?

Mukhopadhyay: Were you then Abraham Wald's only Ph.D. student?

Sobel: Yes, in some sense. However, because he died suddenly, he did not even have the opportunity to sign my thesis as my major advisor.

Mukhopadhyay: Did Wald have any other Ph.D. advisees? Is it true that Jack Kiefer and Charles Stein were also Ph.D. students of Wald?

Sobel: In some sense, people consider Charles Stein as a possible Ph.D. student of Wald. But Charles Stein was already doing research pretty much by himself, and he already had a famous paper of his written (and possibly published) before he actually came to Columbia. He was not a Ph.D. advisee of Wald in the usual sense of the word even though Wald could have possibly signed some official documents for him toward graduation. Jack Kiefer's thesis advisor was Wolfowitz. In some ways, Herman Chernoff also worked with both Wald and Wolfowitz, but it was similar to the case of Charles Stein. Chernoff had arrived from Brown University already having written a thesis, and he was looking for some competent statisticians like Wald or Wolfowitz to read his paper rather than someone to teach him something that he did not already know. Robert Bechhofer had worked with Ted Anderson,

while Henry Teicher worked with Wolfowitz. There was one other fellow by the name Bruckner who was involved with the so-called "Analysis of Variance Notes" of Wald, but later in life he did not continue in the field of statistics. I was the only student directly "groomed" by Abraham Wald himself. I wrote my thesis under his direct guidance and supervision, and my thesis was the only one like that.

Mukhopadhyay: When you went back to Columbia from Wayne State to inquire about the status of your Ph.D. thesis, what was going on in that department?

Sobel: Wald was undoubtedly the central figure at Columbia before he died. When I went there, I did not know exactly what to expect. I found Anderson making many decisions. There was an organization called the Graduate Students Society that contributed in some important ways, even though they were not a part of the decision-making process.

Mukhopadhyay: Were you not involved in the activities of the Graduate Students Society in its formative years?

Sobel: I may mention that I was the first hired teaching assistant (TA) in that department working for the whole faculty, not just one particular professor. This society helped the faculty to provide TAs for the classes, to write up their lectures, to correct homework, grade exams and so on. I helped to form this student group. After my term was over, Bechhofer became a leader of this student body.

WAYNE STATE TO CORNELL TO NEW YORK UNIVERSITY ON WAY TO BELL LABORATORIES

Mukhopadhyay: Milton, you were awarded the Ph.D. degree by Columbia University in January 1951. Do you have anything else to add to that episode?

Sobel: Dwight D. Eisenhower became the President of Columbia University and he signed my degree certificate. After formally receiving the Ph.D. degree, I continued teaching in the Mathematics Department at Wayne State through February, 1952. I then returned to Columbia as a lecturer from February through June 1952.

Mukhopadhyay: How did that happen?

Sobel: Henry Scheffé was the chairman at that time. When I needed a job just before I went to Cornell, I went to Columbia for six months, and Scheffé was the one who gave me that job. Subsequently I met many people who mentioned to me that Scheffé was very helpful to them also.

FIG. 5. *Sobel at the Bell Telephone Labs, 1959.*

Mukhopadhyay: At Wayne State you came to know Benjamin Epstein. Would you like to say something about your collaborations with him?

Sobel: When I went to Wayne State, I met Benjamin Epstein, who was a somewhat senior person in the department. He was a professor, but not the chairman. We worked together on research problems. Then, in the summer of 1952 or 1953, we both went to Stanford University and continued to work together. Jointly, we published several papers on the theory and applications of reliability. Somehow the relationship later fell apart, but I still have fond memories of working with him. I was particularly pleased with a recent seminar at the Technion when Professor Epstein came out of a rest home to listen to my talk.

Mukhopadhyay: After Wayne State, you held positions chronologically at Cornell University, New York University and Bell Laboratories through the period June 1952 to June 1960. What highlights come to mind about these places and the personalities you met there? Let us start with Cornell.

Sobel: During June 1952 through August 1954, I went to Cornell to work with Bechhofer and Charlie Dunnett on selection and ranking problems. I was guided in this choice of topic by Wolfowitz and Robbins, both of whom thought that selection and ranking would turn out to be a fundamental area for research. Jack Kiefer not only thought very highly of this area, he actually was willing to work with us, too. I went to Cornell with my wife for two years. Our daughter Judith (I call her Judy) was born there.

FIG. 6. *Left to right*: *Mrs. Robbins, Herbert Robbins, Robert Bechhofer, Milton Sobel and Mrs. Bechhofer at the conference in honor of Robbins at Syracuse University, 1989.*

Mukhopadhyay: Can you briefly explain what the area of selection and ranking is about?

Sobel: The area of selection and ranking was born in the first part of the 1950s. Before that, Sir Ronald Fisher's work had a big impact on statistical theory. Many of us felt that "Testing of Null Hypothesis," due primarily to Fisher, was greatly "overused," that is, used even when the null hypothesis was not a model of primary interest. Generally speaking, testing the null hypothesis of equality tells us whether or not a set of real-valued parameters are all equal and this is not of any help at all in ordering the associated populations. Many statisticians including Jack Kiefer, Herbert Robbins, Jack Wolfowitz, R. C. Bose and even Abraham Wald thought (as I also did) that this new area would be a "revolution" in the sense of replacing the general overuse of "Testing the Hypothesis of Equality" by new decision-theoretic models for ordering populations with prescribed confidence in the resulting decision. The new approach would help in finding, for example, the "best" population or the t "best" populations. Sometimes, from the given populations, one may like to find a subset of populations including the t best or find all the "good" populations, and so on. Although the selection and ranking theory has been successfully developed and many students received their Ph.D.s in this area, unfortunately it still is not a standard item in many statisticians' "bag of tricks," nor is it a standard part of the statistical curriculum in many universities.

Mukhopadhyay: In September 1954 you joined Bell Laboratories, right?

Sobel: I joined Bell Labs in the fall of 1954 and stayed there for six years, I believe. I was stationed mostly in Allentown, Pennsylvania and later in Whippany, New Jersey. While in that position, I did lots of work in areas such as group testing, selection and ranking and reliability analysis. Among other things, I managed to bring Shanti Gupta to work at the Bell Labs as his first job. Gupta wrote his Ph.D. thesis under R. C. Bose at the University of North Carolina at Chapel Hill working on selection and ranking procedures. In Bell Labs, I had interactions with people such as Milton Terry, John Tukey, Ross Eckler, Bill Roach, Naomi Robbins, Jack Nadler, John Tischendorf, and there were also others involved in quality control, sampling and related areas. I worked on mathematical aspects but I also did much consulting work with the people at the Bell Labs on problems arising from day to day operations, mostly by handling real data sets.

UNIVERSITY OF MINNESOTA AT MINNEAPOLIS

Mukhopadhyay: You became a Professor of Statistics at the University of Minnesota, Minneapolis, in September 1960 and you continued there in that position until June 1975. Give us a glimpse of how that started and your life there in general during the span of fifteen years in your career.

Sobel: Ingram Olkin and Richard Savage both helped to bring me there. I began this part of my career in the Department of Statistics. (The School of Statistics with its own in-house position of a Director was formed later around 1970.) I had interactions with many important and interesting people at that time. I became close with Gopinath Kallianpur, Harold Rubin, Sudish Ghurye and others. I interacted with Bernie Lindgren, Seymour Geisser and Somesh DasGupta. Ghurye and Meyer Dwass arrived from Northwestern University at about the same time. Dwass left very soon after that, but Ghurye stayed for a while. My research work on the Dirichlet distributions started at Minneapolis with Professor K. Frankowski of Computer Science and still continues. I should also mention that my wife and I became very friendly with George Styan and his wife, Evelyn, at that time. George and I traveled together across the United States and even in Greece, playing chess in spare moments.

Mukhopadhyay: During my visiting appointment in the Department of Theoretical Statistics at Minneapolis in 1977–1978, I often saw Bob Buehler and Don Berry playing chess during lunchtime. I heard that you were an avid chess player yourself and that you originated such lunchtime chess encounters at Minneapolis. Any recollections?

Sobel: I remember playing chess at lunchtime mostly with Bob Buehler. He truly loved the game.

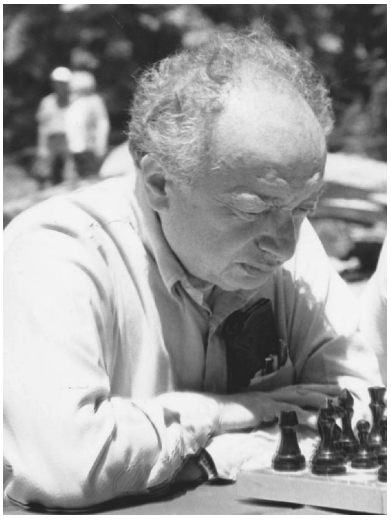

FIG. 7. *Sobel playing chess.*

He was such a happy person and had a very good influence on the department. He was an extremely knowledgable man with a deep grasp of the foundations of statistics. I always enjoyed playing chess or just talking with him. It is unfortunate that Bob is no longer with us.

Mukhopadhyay: You had quite a reputation as a chess player I recall. At Minneapolis, who else did you play chess with?

Sobel: Don Berry was a good chess player and a good Bayesian to have around. I do not recall Bill Sudderth playing chess. Then, there was an algebraist named Warren Stenberg and he was a big-time chess player (possibly a state champion at one point). The funny thing is that even in India I found that I had a reputation in chess. Students were told, "If you want to go to the United States and work with Sobel, you first have to learn how to play chess." [Laughs]

UNIVERSITY OF CALIFORNIA AT SANTA BARBARA

Mukhopadhyay: Milton, in July 1975, you made the move to become a Professor at the University of California Santa Barbara (UCSB). Why did you decide to pack up and move to UCSB after spending fifteen years in Minneapolis?

Sobel: Having lived in a cold place for fifteen years, I wished to move to a warmer climate. After I left Minneapolis, Seymour Geisser one day called his friend Marvin Zelen and told him, "Sobel just went to Santa Barbara," and Zelen replied, "Oh, you will never see him again." Once I got to Santa Barbara, I guess that I hit an absorbing barrier of a Markov chain.

Mukhopadhyay: At UCSB, after spending a few years in the Department of Mathematics, you joined the newly formed Department of Statistics and Applied Probability. This spans a very significant period of your professional life and career. Where would you like to begin?

Sobel: While at UCSB I continued my research with Professor Frankowski and with the late Ram Uppuluri (from Oak Ridge National Labs). I also did some research with S. Rao Jammalamadaka and with one of his students, Marty Wells, who is now at Cornell University. I continued to produce more Ph.D. students like Pinyuen Chen and his wife, Lifang Hsu. Between Minnesota and Santa Barbara I have had about two dozen Ph.D. students, starting with K. Alam, now an emeritus at Clemson University, and including Y. L. Tong at Georgia Tech in Atlanta. My two most recent Ph.D. students are Yontha Ath from Claremont Graduate School near Los Angeles and Hokwon Cho, now at the University of Neveda Las Vegas.

I should add that I am proud of all my Ph.D. students. I am very proud of the fact that Y. L. Tong started writing his own books. I am proud to mention that Asit Basu has done very nice work on reliability and he has been a leader in that field. K. Alam started out as an independent researcher, and I feel very proud of the fact that Alam considers me as his thesis advisor. George Woodworth was another student of mine, and I wish I had maintained a little more contact with him. I am very happy with M. M. Desu's work.

I have continued my working relationships with Ingram Olkin, S. Panchapakesan, Milton Parnes (of Temple University in Philadelphia) and many others. Some of my research has been joint with my son Marc, who is at Temple University.

Mukhopadhyay: Anything else about your ties with UCSB?

Sobel: I did a lot of research with Professor Morteza Ebneshahrashoob who is now at California State University, Long Beach. From time to time we have some very interesting visitors at UCSB; I wish I could have interacted with more of them.

RESEARCH CONTRIBUTIONS: DECISION THEORY AND SEQUENTIAL ANALYSIS

Mukhopadhyay: Now let me direct your thoughts about research areas and the impact your works have had on researchers in both statistics and probability, over approximately the past five decades. We have a lot of ground to cover. Milton, let us begin with the work that you did in your

Ph.D. thesis at Columbia. What was the topic of your thesis?

Sobel: The thesis was on "complete class" results for certain sequential problems. It consisted of purely decision-theoretic results and turned out to be my 1953 solo paper in the *Annals of Mathematical Statistics*. I got considerable guidance and comments from my thesis advisor, Abraham Wald.

Mukhopadhyay: Before your results from the thesis came out in print, you had published a joint paper with your advisor Abraham Wald in the *Annals of Mathematical Statistics* (1949). How did that paper come about?

Sobel: This paper had to do with a sequential test for choosing one of three hypotheses concerning the unknown mean of a normal distribution. The unknown mean may belong to one of three disjoint intervals and we may want to pick the most plausible interval through likelihood comparisons. This was a three-decision problem. I think that it was an important idea. Over the years, various people referred to it and some attempted to generalize our approach.

Mukhopadhyay: You are aware that Robbins's (1970) paper gave a sequential approach to choose among countably infinite number of hypotheses concerning an unknown mean of a normal distribution. This led to the definition of what Robbins had called *sequential distinguishability*, a concept which was further explored by Rasul Khan (1973) and George McCabe (1973).

Sobel: These are very important follow-ups of the 1949 paper of mine with Wald. These days, I find that young people tend to revisit many old problems, but sometimes they fail to mention the names of the original contributors.

Mukhopadhyay: Let me now get your views on something else. From a practical point of view, I find that many applied individuals feel a little uneasy about the fact that in sequential experiments the final sample size is a random variable over whose magnitude they have little or no control. They are often afraid that the sample size for an open-ended sequential procedure might become unusually large. Theoretically, we of course know that how large a random sample size can potentially become does indeed depend on the accuracy levels or optimality criteria one sets upfront before an experiment begins. By relaxing the error requirements prior to experimentation, one can reduce the sample size, right? One may also consider closed sequential sampling plans where the sample size is bounded above by a known constant. Ted Anderson (1960) did some important work in this area. Of course, there is no "free lunch" here, or for that matter anywhere else including fixed-sample-size analyses. But one sometimes wishes to get the best of all worlds in some sense, and preferably for "free." I see it as the combination of a perception problem and the lack of understanding of the basic foundation perhaps on the part of many potential users. In clinical trials, I notice some use of sequential experimentations. Is there a way to project a fair and broader image of this field among the "commoners" today?

Sobel: I do not think that the perception problem arises with the possibility that the random sample size may become too large, but rather the fact that it sometimes becomes too small bothers many users. If a sequential strategy terminates with too few observations, then the users may become skeptical about the statistical findings in the conclusion. So, to circumvent this possibility of early stopping, one should place an acceptable lower bound that is guaranteed for the final random sample size. In other words, an experimenter should be guaranteed to observe at least so many samples in order for the final statistical analyses to remain credible in a particular area of application. I am not convinced that the random aspect of the associated sample size is bothering the potential users. We should look at what kind of lower bound will satisfy people in various applications, so that in spite of early stopping, if it actually happens in some instances, the experimenter would still be able to obtain credible analyses and confidence in the final result.

Mukhopadhyay: But the possibility that sequential sampling may terminate too soon has been addressed in many types of problems. There are reasonable ways to circumvent such a possibility. The fundamental paper of Chow and Robbins (1965) handled this aspect beautifully. One may also include an initial sample size that "grows" in a certain way. Right now, such procedures are quite plentiful in the literature of sequential analyses. Yet we do not see many papers and results with practical applications in the area. Milton, in this light, would you like to expand your thoughts?

Sobel: I seriously doubt if most practitioners are aware of such methodologies. Some may be aware but many possibly tend to avoid this area anyway, and others are probably ignorant and indifferent about such developments.

Consider the famous "birthday problem." If the looks at page 33 of the third edition of Feller's *Introduction to Probability Theory and Its Applications* **1** (1968) or some other book, one will find that asking

twenty-three individuals their birth dates gives a probability of at least one-half that the same birthday is repeated at least once (and any smaller sample size gives less than one-half probability of that event). Now, do you interview each individual one by one separately and wait until you find two birthdays exactly the same? As a teacher, I will perform the same experiment differently in a classroom full of students. I am not going to interview twenty-three students all at once about their birth dates. Instead I am going to accomplish the same goal in a different way. I will pick one student at random and ask him to write his birth-date on the chalkboard, and then all other students in the class having the same birth date will just raise their hands. If no one raises hands, I will pick another student at random from the pool, and ask him to write his birth date on the chalkboard, and again I will turn to the class to see if anyone raises a hand and continue this way. How long will it take me to find a repeat birth date? It will be lot sooner than interviewing twenty-three individuals separately. Actually this strategy, on average, will cut the number of interviews by almost fifty percent! A properly executed sequential sampling strategy is much more efficient in this case! After reading this, some people will immediately protest that, well, the birthday problem is not really a pressing practical problem of the day. But the fact is that there are plenty of practical problems where one attempts to identify similar objects or species, and in such instances the sequential sampling strategy for the birthday problem as I have suggested here could be immediately useful. Wald (1947) had included similar results on efficiency aspects in the context of the sequential probability ratio test (SPRT) for a normal mean and showed that it needed about 50 percent of the sample sizes required by the uniformly most powerful fixed-sample-size procedures with comparable type I and type II error probabilities. The bottom line is that all practitioners, as well as their supervisors, have to be educated first, and we also have to keep bombarding them with notions of efficiency of sequential designs. The area of sequential analysis is certainly very broad and the methodology can be used in many areas of applied statistics. One may browse through the *Handbook of Sequential Analysis* (1991), edited by B. K. Ghosh and P. K. Sen to appreciate what I am talking about. Unfortunately, the users out there continue to use the tools they already know. Differing and newer tools, unless fashionable, do not catch on easily. Changing directions on the part of practitioners requires them to be educated enough about new and challenging ideas and also they must have the courage to break the status quo. It is also perhaps true that we have not been able to market the ideas of sequential analysis forcefully enough, but that is only one component in this equation. A lot of people just simply ignore it without knowing very much about it. Educating the statistical clientele is most urgent in this regard. I do not think that Abraham Wald was a very good salesman for sequential analysis. I do not consider myself a good salesman, either. I do not exactly know what it takes to be a good salesman in this regard. On the point of selling the ideas of ranking and selection, Jack Kiefer, before he died, used to say, "I give up." I go through similar pessimistic emotions sometimes as well, but I must note that many positive things have happened, too. I may add that the journal *Sequential Analysis*, founded by B. K. Ghosh and P. K. Sen, has been growing strong since 1981.

SELECTION AND RANKING METHODOLOGY

Mukhopadhyay: In the field of applications of sequential analyses, you have many path-breaking contributions. Let me start by asking, Milton, what do you think is your most important contribution in this area?

Sobel: I claim that a good deal of my work has to do with applications of sequential procedures for problems in selection and ranking. I used my talent and my knowledge of sequential analysis to solve such problems and others, for example, in the area of reliability and that is my main contribution here, I believe.

Mukhopadhyay: Would you care to add a few words about some of your collaborators in the area of selection and ranking?

Sobel: I worked with Bechhofer and also with Charlie Dunnett and Shanti Gupta from the very beginning when this subject started to unfold. I enjoyed working with all of them. More recently I have been working with my student Pinyuen Chen and also with S. Panchapakesan.

Mukhopadhyay: In the area of selection and ranking, you are one of the pioneers. When and how were you drawn toward this vast area of research that is also a next of kin to multiple comparisons?

Sobel: R. C. Bose had his Ph.D. student, Shanti Gupta, doing some important things on selection and ranking. Wolfowitz was encouraging Bechhofer and me to do some brand-new work in the same area. I would say that there was a small and yet very influential group of people interested in energetically advancing the area of selection and ranking. That group also included Robbins, and every

FIG. 8. *Left to right*: *H. Rizvi*, *M. Sobel, R. Bechhofer and S. S. Gupta at the Selection and Ranking Conference at UCSB, 1985.*

time I met Robbins he would come up with the same question, "Why hasn't selection and ranking caught on?" My answer always has been, "We didn't have anybody to actively advertise the fact that the selection and ranking idea was in fact a revolution in itself." This idea was justifiably supposed to replace the standard approach normally carried out through testing of hypotheses and/or analysis of variance. If we start with five treatments, say, we sometimes know a priori that they are different, and then it would seem fruitless to run the analysis of variance to validate the hypotheses of no difference among the treatments. In how many real practical problems has a scientist accepted the null hypotheses of equality of treatment effects through the analysis of variance anyway? A more basic problem is to identify, for example, the best or the worst among the treatments under consideration with a high prescribed probability of a correct selection. Why should anyone really run the analysis of variance as the first step anyway? It does not make sense in some cases, but people continue to take the analysis of variance route because of habit, I suppose. It is hard to break such a habit, particularly when the alternative approach is not too well known. We lack people who would spread our gospel. All through the years, every now and then, somebody in an applied field rediscovers the ideas and appropriateness of the ranking and selection approach over traditional analyses of variance techniques and later finds to his surprise that a huge branch of statistics has already been in existence for decades to handle such problems.

Mukhopadhyay: How did Jack Kiefer join in the small bandwagon of people interested in selection and ranking problems?

Sobel: Kiefer realized that Wolfowitz and Robbins knew what they were talking about and that those ideas had much potential value. He could foresee the importance of this area. Then, Bechhofer, Kiefer and I decided to form a team in order to work together. After ten years' work, our jointly authored book, *Sequential Identification and Ranking Procedures* appeared in 1968. "It was not exactly bedtime reading," as Herb Robbins used to say, referring to that book.

Mukhopadhyay: That 1968 book was the only one of its kind in this area for quite some time. It continues to be an influential book even today. Any particular thoughts on this that you may like to share?

Sobel: It is unfortunate that not too many people have really read it. I should add that the area of selection and ranking flourished at a number of places other than Minnesota and Cornell. For example, Shanti Gupta had a lot of students at Purdue University working predominantly on the subset selection approach, a fundamental idea that is slightly different from the indifference zone idea used in my 1968 book and has the advantage that it can be mostly implemented with fixed-sample-size strategies. I don't think it is unfair to say that much harder problems arose under sequential sampling strategies in this field and every other field. I feel that people working in sequential problems in general were never given quite as much credit as they deserved, and that is a pity.

Mukhopadhyay: You wrote a paper with Robbins and Starr (1968) that I thought was an important contribution in this area. Any comments?

Sobel: Norman Starr was a very good statistician and it is a shame that our profession ultimately lost him to some other pursuits.

Mukhopadhyay: In the area of selection and ranking, there are several books available now. What is your impression about where we are now, and perhaps more important, where should we go from here?

Sobel: I feel that Ed Dudewicz with S. N. Mishra made some sincere effort to include such topics in a first-year graduate level textbook, *Modern Mathematical Statistics* (1988) and I think that there should have been more books written at that level highlighting the area of selection and ranking in them. The authors of textbooks at the first-year graduate level have to be persuaded that such topics indeed belong to a basic mathematical statistics curriculum. We need to see people in the physical and social sciences using selection and ranking methodology. Until we have that situation, the area is not going to catch on. People have to understand

that selection and ranking ideas form viable alternatives (and often these are preferable alternatives) to analysis of variance techniques. It may be that only then we will make some real progress and see some well-deserved recognition of this subject matter.

Mukhopadhyay: In 1977, you wrote a methodology book, *Selecting and Ordering Populations*, on selection and ranking, coauthored with Ingram Olkin and Jean Gibbons. How did that collaboration come about?

Sobel: In a sense it was due to some backlash. We were trying to sell the ideas of selection and ranking among the masses. Bechhofer was a perfectionist and he wanted to pursue only the best and the optimal before anything could be published, and we were driven by that philosophy while writing our 1968 book. I was getting a little tired of this perfectionism and I turned to Ingram Olkin. He in turn brought in Jean Gibbons to see if we could put out something in a fairly quick manner that would be good for a course in the first-year graduate curriculum.

Mukhopadhyay: In the area of selection and ranking, this 1977 book of yours was very user friendly. Even after twenty two years, the book still remains extremely useful, particularly because it includes all the necessary tables for implementing each methodology. It is refreshing in its applied flavor through and through.

Sobel: I worked very hard to get some of those tables in there. Olkin was a natural choice as a collaborator because he was one of the few people who really understood and appreciated the importance of selection and ranking, and I had already written a few important papers with him on Dirichlet distributions and their applications to some ranking and selection problems. Jean Gibbons strengthened the team because she was willing to learn the subject matter and then could clearly explain the methodologies. She went over everything either one of us wrote and then put these in a textbook form, understandable by any first-year graduate student. She had a lot to do with the examples and exercises in the book. We finished writing that book very quickly, in less than two years, but the strange and miraculous thing about that book was that the sum of the squares of the distances among the three authors was perhaps a maximum for a book written by three Americans. [Laughs]

One author was in Alabama, one in California, and the third in Minnesota! Probably there is no other way to maximize this "distance"! [Laughs]

GROUP SEQUENTIAL TESTS OF HYPOTHESES AND RELIABILITY ANALYSES

Mukhopadhyay: Another large segment of your contributions can be broadly classified into areas involving group sequential tests and reliability analyses. I am aware of the fact that some of your contributions in this area have been seminal for later developments. Milton, would you please mention some of your papers in these areas, which in your opinion really stand out, and give me your thoughts on these in retrospect?

Sobel: The fact that I could develop an optimal procedure in group testing based on an algorithm was gratifying. Superficially the problem may appear simple, but in reality it takes years to master the area. Graduate students have traditionally fallen in love with this type of problem overnight, but they soon realize that the subject is difficult, and the procedure I had originally proposed was not very easy to improve upon. In this area, I am very fond of my papers with Phyllis Groll which dealt with such methodology to eliminate all the defective items in a binomial sample efficiently and I am glad to add that this 1959 paper came out in print in a proper applied setting, namely in a journal published by the Bell Labs, *Bell System Technical J.* I should also mention that I wrote papers on the subject with Satindar Kumar, another Ph.D. student of mine. I do not have any current information on the whereabouts of P. Groll or S. Kumar, both of whom worked with me in the area of group testing.

In the area of reliability analyses, I think that my paper on reducing the experimental time (1956) was an important contribution. But I would say that the three papers I got more recognition from, so to speak, were those written jointly with Epstein which came out in print successively in 1953, 1954 and 1955. These were geared toward developing full-blown sequential methodologies in life testing and reliability problems. On those three papers with Epstein, I received many comments and these have also been referred to by others on numerous occasions. They turned out to be quite important papers at the very beginning of my post-doctoral career.

COMBINATORIAL AND DIRICHLET PROBLEMS

Mukhopadhyay: You also have a zest for hard combinatorial problems. Would you please focus on one or two of your best works in the area?

Sobel: I think that in order to be a good theoretical statistician, a part of one's educational background should include some training in combinatorics. That's my belief. I believe that combinatorics

represent a fundamental basis upon which one can build a statistical career. Ideas from finite mathematics should form a part of the foundation. I have certain amount of that background. I will say that the type of work that I have been doing with Dirichlet distributions is all tied up with combinatorics and also graph theory. I would also add that my work with Dirichlet distributions has far-reaching implications and if these were properly understood by the profession, such topics might then form a part of the elementary statistics curriculum.

Mukhopadhyay: Milton, would you please say a few words about the kinds of statistical applications of the Dirichlet distributions you have in mind? In other words, this is your opportunity to be a salesperson. Let me hear your sales pitch, if you don't mind.

Sobel: Dirichlet distributions appear naturally in problems that have something to do with a multinomial distribution. They appear, for example, in birthday problems or choosing an item from a collection in any way whatsoever. I wrote two books, *Dirichlet Distributions, Type 1* and *Dirichlet Distributions Type 2* [in *Selected Tables in Mathematical Statistics*, **4** (1977) and **9** (1985).] coauthored with V. R. R. Uppuluri and K. Frankowski. I am presently trying to write a third book with K. Frankowski on applications of the Dirichlet. Of course, there are combinatorial problems which do not have any connection with a Dirichlet distribution, and I have been interested in many such problems as well. I feel that my love for combinatorial problems and number theory has given me the foundation for a lifetime career in mathematics and statistics.

I would say that more recently I have been interested in some mathematical problems that have remained unsolved for more than five-hundred years. Recently I wrote an article in *Amer. Math Monthly* (1994) on one such problem's five-hundredth anniversary; the problem is known as a *sharing problem* (or the *problem of points*) which can be explained as follows: suppose there are six individuals each having an ID number from one through six, and a fair die is rolled. If the die comes up with face i, the ith individual wins the first round. Then the die is rolled a second time, and if it comes up with face j, the jth individual wins the second round, and so on. Each round gives one point to the corresponding winning individual and zeros to others. Suppose that the game will be over as soon as one of the players accumulates ten points. But consider a situation where, for some reason, the tournament has to be abandoned in the middle somewhere after a few rounds of play, at which point one individual may have x points, another individual may have y points, and so on where x, y, etc. are all smaller than ten. Now the problem is to design an optimal way to divide up the available prize money among the six contestants, even though there is no clear-cut winner. The significance of the name *sharing problem* is clear, I believe, from the context. This appears to be one of the oldest problems in probability theory. Laplace and DeMoivre both worked on this problem. I have recently received a letter from Lajos Takàcs who is an expert in the history of probability theory (including such problems) and he wrote, "It seems the problem of points for several players did not get sufficient interest since the pioneering works of DeMoivre and Laplace. Subsequent publications simply repeated the old results. Only your recent work with K. Frankowski made significant progress in solving various important problems." (It is called a *problem of points* by some people, but I refer to it as the *sharing problem*.) Again, Dirichlet distributions came into play and we solved the problem completely, actually in a more general set-up, by providing the necessary integrals and tables in order to figure out the optimal way to share the prize money in the middle of a tournament. One may encounter such a situation, for example, in an outdoor tournament when the semis and the final are canceled because of severe weather conditions.

STATISTICAL TABLES AND COMPUTATIONS

Mukhopadhyay: This brings me to another major part of your research contributions, namely, your involvement in statistical computations and construction of many invaluable tables throughout your career. Any thoughts?

Sobel: I should mention two of my students who did excellent work in this area. One of them is Y. L. Tong of Georgia Tech and another is Roy Milton. Both worked on extensive tables of the multivariate normal distribution.

Mukhopadhyay: Your name has been associated with important tables including percentage points of multivariate normal and multivariate t-distributions under various correlation structures. In your view, which one or two statistical tables would you say truly constitute your lasting contribution in this field?

Sobel: I believe the multivariate t-distribution with Charlie Dunnett was an important table, even though E. A. Cornish, a friend of Fisher, published on the same topic at about the same time.

FIG. 9. *Sobel with B. V. Gnedenko (left) in Russia.*

FIG. 10. *In the 1980's, while on exchange visit to Russia, Sobel discovered the grave of Yuri U. Linnik, an hour's train ride outside of Lenningrad (St. Petersberg). The photo shows Sobel at Linnik's gravesite.*

THE BERKELEY SYMPOSIA AND BEYOND

Mukhopadhyay: You attended most Berkeley Symposia from the time when these were organized by Jerzy Neyman. These symposia used to be the meeting places of many great minds in statistics and probability. Any recollections?

Sobel: I met Michel Loève there. He was always thorough and I always enjoyed his lectures very much. I am thinking of Paul Lévy who was actually Loève's teacher! I met Lévy at these symposiums too. I remember that one of my papers (1970) presented at the Sixth Berkeley Symposium had to do with a problem in chess and I wrote it with Leo Katz. I have many fond memories of Leo Katz. So far I had not mentioned one other person who greatly impressed me in my career, and he was Paul Erdős. This was a man devoted to mathematics, more than anyone else in modern history, and he had a big influence on me, too. I remember B. V. Gnedenko from Russia attending the symposia. I did not get the opportunity to know A. N. Kolmogorov very closely, but I knew Gnedenko fairly well and had pictures taken with him.

Mukhopadhyay: After Jerzy Neyman and Jack Kiefer died, slowly the tradition of holding the Berkeley Symposia disintegrated. Any thoughts?

Sobel: Yes, in some sense, the Berkeley Symposium concept unfortunately died with the passing of Neyman and Kiefer. You may know that the people from Stanford wanted to keep it alive, but naturally these Berkeley colloquia needed a tremendous amount of commitment as well as behind-the-scenes administrative work. A common ground of negotiations was never reached with Betty Scott. Even the prospect that the original name of the symposia might actually change to something else disturbed some people. In the meantime, Shanti Gupta started holding the Purdue Symposium regularly. But in my opinion, the big hotshot now is N. Balakrishnan with all his energy. He has the kind of drive that is expected to lead to the eventual success of a large symposium held anywhere on a regular basis. Balakrishnan is a great organizer.

Mukhopadhyay: You have attended many conferences overseas. Any recollection about any of those visits?

Sobel: I remember that when I went to participate in one of the Vilnius conferences, I met so many participants from Siberia and other parts of Russia. About half a dozen Americans were in attendance there as well. It was quite impressive for me to see and realize that there were lots of people in this world of whom I knew nothing, and lots of activities were going on in this world that I was not aware of. The Russian empire was so widely spread out, and the world is so vast. I was pleasantly surprised to personally experience and to take part in one of their organized Vilnius conferences that was almost as big as some of our largest conferences in the U.S.A.

RESEARCH WORK IN PROGRESS

Mukhopadhyay: What types of problems are you working on right now, Milton?

Sobel: Until quite recently I was still working with Professor K. Frankowski, even though he goes off regularly to Poland for a good part of the year. Recently I was interested in something called the *ruin of a gambler*, a classical problem indeed. The

sharing problem I discussed earlier was at least five-hundred years old, whereas the ruin of a gambler problem is at least three- or four-hundred years old. I find that the ruin of a gambler problem has not been extended as much as I thought it was. I have come to realize that very nice extensions and additions to this problem seem quite plausible now.

I also was working on a multinomial selection and ranking problem with a student of mine (Hokwon Cho) until about the end of July 1997 when he finished his Ph.D. degree. In the case of a multinomial distribution, there is an important and open ranking and selection problem, and that is to decide how many cells a particular multinomial setup has. In this situation, what would constitute a natural stopping rule? My current student Yontha Ath is working on finding for given N (number of nodes) and E (number of edges) the most reliable graph if each edge survives a fixed time independently with common probability p. This is an interesting combination of graph theory and statistics.

Mukhopadhyay: Will that not be similar to the discovery of new species? Norman Starr, whom you mentioned earlier, worked on this topic.

Sobel: Yes, the problem is same as discovering new species. I claim that this subject is not yet a closed book. The question is, does one care about all new species in reality? One may not be interested in spending the energy and resources to discover new species if p, the probability of finding such new species, is less than some acceptable small number, say one in ten thousand. Under this scenario, what would constitute a natural sequential strategy? I am looking into different possibilities.

MENTORING

Mukhopadhyay: Milton, let me ask you this. How do you do your research? How do you select your research problems? Can you tell me any secrets about what such processes are all about? I want to learn something about your ways around these.

Sobel: This is not an easy question to answer. It seems to me that one must have some fundamental plan or fundamental contributions to make and one applies such contributions to different areas. That outlook seems very reasonable in order for someone to go about pursuing a research career. I have been personally driven by such a principle. Earlier I had mentioned that I wanted to do something useful by applying sequential methodologies, and so I pursued areas including ranking and selection, reliability analyses, Dirichlet problems, to name a few. There has to be a feeling from inside that one can make fundamental contributions somewhere because of one's specific strength in a certain area of expertise and then apply that strength to as many related areas as possible. For example, I learned and developed a technique of group testing, and I realized that the basic idea in this technique seemed fairly independent of the types of problems on hand. The approach is merely a way of sampling in groups rather than taking one observation at a time. Thus I set out to apply the idea of group testing in different problems.

Mukhopadhyay: You have probably faced a situation like this. A new graduate student walks in and expresses serious interest to work with you. Is there any special way for you to help the student?

Sobel: It is not very easy to break in a new student so that eventually we can work together on something of mutual interest. A student once came to me saying that he would work with me, and so I gave him a copy of my "green books" with a host of Dirichlet problems and their applications. I did not feel that these involved a whole lot of complicated theory at all, and so I wanted him to read this material first. He found the material very hard. I wanted to help him in any way possible, but I simply could not get through to him. In retrospect I find that the subject of selection and ranking, when properly understood, did the work of attracting students for me.

Mukhopadhyay: Is that a typical route for how most of your former Ph.D. students' research first had gotten off the ground but somehow that same approach failed in the case of one particular student?

Sobel: This approach seemed so natural to me. First I pose a problem to a new student, get him interested in the area, so that I can turn him on to look at the problem. My "green books" included lots of interesting but not so hard unsolved problems. This approach had worked fine on a number of occasions in the past. I think that one must build up the ego or confidence in a student. If he could discover an error in someone else's work or my work, then that person's confidence will soar. Somehow the advisor needs to instill and nurture this sense of self-esteem and confidence in a new student just as Professor Ritt (of Columbia University) did for me.

Mukhopadhyay: Do you wish to add anything about your classroom teaching?

Sobel: I wear a hearing aid in both ears. Often students have told me that the hearing impairment I have has not affected my ability to teach in classrooms. I always appreciated such positive feedback

FIG. 11. *Sobel and his son, Marc, at Purdue Symposium, 1986.*

from students and I gathered strength from such comments.

Earlier I was talking about instilling confidence in a student. Let me tell you a story for the record. When my son, Marc, was a student at Berkeley, I somehow got him interested in writing the solutions manual for the problems in the well-known book of P. J. Bickel and K. J. Doksum, *Mathematical Statistics: Basic Ideas and Selected Topics* (1977). I told him, "Marc, if you want to learn the subject, take Bickel and Doksum's book and write up solutions for all the problems in that book." He did so and I worked on the project with him, and then we sent the solutions to Kjell Doksum, and he worked on the solutions too. Eventually, three of us together came up with the solutions manual, but the original book's publisher did not want to print it. But in the process of writing that manual, Marc became committed to statistics. Such activities can also build a core of confidence in a young person.

Mukhopadhyay: In general, the availability of sophisticated computing power is changing the facets of practically all disciplines. Statistics is no exception. Do you feel comfortable with what has been happening? Are we moving too fast? What are your thoughts?

Sobel: I am not against this so called "computer revolution" at all. The technology is here and we must use it to our advantage. But I feel a bit troubled by the fact that a statistician these days finds it very hard to land a good job unless he is at the same time very highly skilled in computing. Even as a graduate student, it seems that one must almost have a double major, one in statistics and a second one in computing. The amount of time a student spends on the graduate program in statistics is, after all, finite and nowadays that total time is divided equally or so between statistics and computing, whereas in the past, students used to primarily learn statistics in graduate school. Now I find

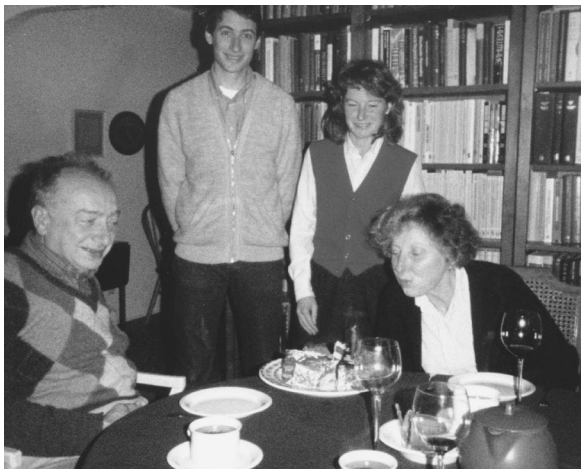

FIG. 12. *Sobel at home with wife, Florence, and two younger children, Eric and Judy.*

that many graduates often end up knowing some statistical computing at the expense of staying very weak in much of the basic understanding of statistics and its principles. Of course one can always find some exceptions here and there. On the other hand, I am commenting on the big picture and the possible danger in the trend of washing down everything and anything in sight with a couple of cycles of computer programs. There is no substitute for hard-hitting brainstorms! Years ago I once asked a student if he knew anything about the square root of two, and he immediately replied that he did not have to know that because a button on his calculator would give the value of the square root of two just like that!

Mukhopadhyay: Sure, but then one will be able to "claim" that the square root of two is a rational number!

Sobel: [Laughs]. How unfortunate that would indeed be. There is something wrong here. We must honestly strive to achieve a kind of balance.

IMMEDIATE FAMILY, HOBBIES AND RETIREMENT

Mukhopadhyay: When did you get married? Would you please give some particulars regarding your immediate family?

Sobel: Before I took the position at Wayne State, right after receiving the Ph.D. degree in 1950, I married Florence Nemet, and we have been married for almost fifty years now. We have two sons and a daughter. Marc is the older one, then came Judy, and the yougest one is Eric. Judy majored in health science and she is with Portland State University in Portland, Oregon, while her husband, Greg Koski,

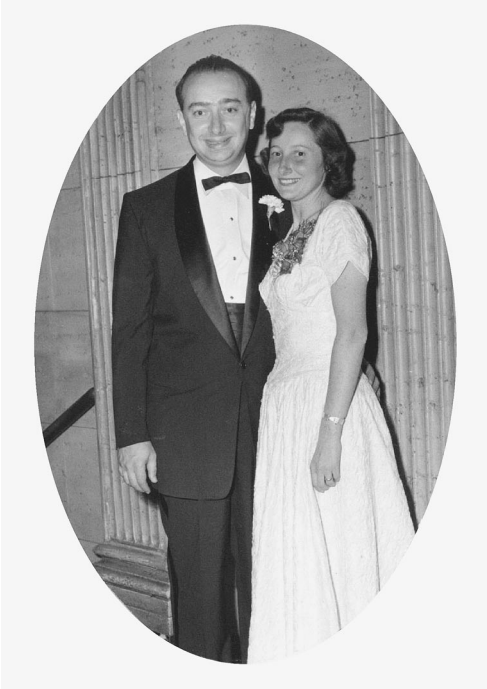

FIG. 13. *Milton Sobel and Florence Nemet's wedding picture, 1950.*

practices in orthopedics. Eric graduated from the Biomath Department at UCLA and he specialized in mathematical genetics. He has just completed a year with the Wellcome Centre in Genetics in Oxford, England. Marc lives in Philadelphia with his wife Marilyn and their two children. He is a professor of statistics at Temple University in Philadelphia. I have four grandchildren, two girls from Judy and a boy and a girl from Marc.

Mukhopadhyay: At home, when you do not think about statistics, what do you do? Do you have any hobbies?

Sobel: I still play chess, and that is my hobby. I usually play at the Chess Club in downtown Santa Barbara. There is always someone waiting there and so I never have to call anyone up in advance to make an appointment in order to play chess. There are several regulars who are normally available at the club. This is my hobby. I never got too much interested in the games of bridge, backgammon or anything like that.

Mukhopadhyay: Milton, you officially had to take the mandatory retirement about ten years ago from UCSB. Do you consider yourself retired?

Sobel: On paper, I had to take retirement about ten years ago, and I became what is known as Professor Emeritus. But, although I do not have an office at the University of California at Santa Barbara, I continue to go to the "office" every day. That is an answer to your question and you figure it out. I have never tired of working in statistics and mathematics in the first place, so why should I have to *retire*?

FIG. 14. *Sobel with his wife, Florence, at a conference.*

Mukhopadhyay: Thank you very much, Milton, for taking the time to have this conversation. Your energy, contributions and career have inspired many. You had also influenced this younger colleague of yours. I wish you a long, healthy, happy and productive life ahead.

Sobel: Thank you, Nitis.

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