FIG. 1. *Marvin A. Schneiderman.*

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A Conversation with Marvin A. Schneiderman

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Abstract. Marvin A. Schneiderman was born on December 25, 1918, in Brooklyn, New York. He received a B.S. degree in mathematics and statistics from the City College of New York in 1939, an M.S. degree in statistics from American University in 1953 and a Ph.D. in statistics from American University in 1961. Additional graduate training and research was done at Ohio State University, Harvard Graduate School of Business and the London School of Hygiene and Tropical Medicine. He is a Fellow of the American Statistical Association and of the American Association for the Advancement of Science. He is also an elected member of the International Statistical Institute, an elected Fellow of the Royal Statistical Society and a Founding Member of the American Society of Preventive Oncology. He has served as President of the Washington Statistical Society, as Chairman of the Committee on Presidents of Statistical Societies, as a member of the Board of Directors of the American Statistical Association and as a Council member of the International Biometric Society. He has been an editor on the editorial advisory boards of several journals, including *Cancer Research*, *Statistics in Medicine*, *Blood*, *Journal of the National Cancer Institute* and the *American Journal of Industrial Medicine*. He was at the National Cancer Institute from 1948 through 1980. He began as a consulting statistician, then was appointed Head of the Controlled Trials Group for Cancer Chemotherapy and later became Associate Director for Field Studies and Statistics. His last appointment at NIH was as NCI Associate Director for Science Policy. He was awarded two of the highest honors accorded civilian employees at the NIH, the Distinguished Service Award and the Superior Service Award. After leaving the National Institutes of Health, he spent a short time with a private consulting firm with strong environmental interests. He then served as a fellow at the Environmental Law Institute before joining the National Research Council/National Academy of Sciences Board on Environmental Studies and Toxicology. He officially retired in 1995. Marvin Schneiderman passed away on April 1, 1997.

Simon: What brought you to the NIH and what did you do before?

Schneiderman: Before World War II, I took a class in sampling theory with Jerry Cornfield and Duane Evans. They were teaching at the Graduate School of the Department of Agriculture and they were in the process of developing a whole new theory of sampling. Once a week we had a class in which Evans and Cornfield described what they had been working on the week before. Every once in a while we would get to class and they would say "Disregard what we told you last week: it was wrong."

I was working as a statistician at the Office of the Court of Master Generals then. I had taken statistics classes as an undergraduate in college. Statistics was so new and unknown a field that the first class in statistics that I took was called "Unattached 15.1" and the more advanced class

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was "Unattached 15.2." because City College of New York did not know in which department to place them. The classes were actually taught by an economist who was interested in index numbers and index number theory.

My undergraduate major was mathematics. Probability theory and several "Unattached" classes were about the only things that I could get in statistics at the time. Then the war came and I entered the Army. I was a Statistical Control Officer, mostly concerned with problems of scheduling and logistics. We applied some life-table theory to the survival of Air Force engines. The question was "When do you pull an aircraft engine out of an aircraft to recondition it?" If you pull it out too early, you have lost some lifetime that the plane might have been in combat, or if you pull it out too late, the engine might fail and the plane might crash. I was stationed at Wright Field (later Wright Patterson Air Force Base) in Dayton, Ohio, at the time.

After the war ended, there was an American Statistical Association convention in Cleveland. There I ran into my old teacher Jerry Cornfield, who had just gone to the National Cancer Institute. His father had died of cancer of the pancreas, and Jerry felt he wanted to do something about this disease because of his father's death. He said that if I were in Washington some time and I was interested in a job, I ought to come and see him. After the war, I took him up on that. I talked with Harold Dorn, a population demographer/sociologist, who had been a Social Science Research Council Fellow in England and had taken classes with R. A. Fisher. I was hired in 1948 to be part of the Mathematical Statistics group that Jerry headed. The group under Dorn was *the* statistical group in the Cancer Institute, and at the time that meant it was *the* statistical group in all of NIH.

Simon: Who were the other statisticians at NIH then?

Schneiderman: Mantel was there when I came and so was Jacob Lieberman. Sam Greenhouse and I came to NIH the same day to work with Cornfield. At that time, Harold Dorn was doing the Cancer Surveys, trying to get incidence data. And Dorn had people working with him on the Ten City Surveys: Sid Cutler, William Grodowitz, Jack Rowan, Donald Loveland, Sam Marcus, Irving Warren. (If there were others, I can't remember their names).

Simon: What was the nature of the interactions with nonstatisticians? How did they know you were there?

Schneiderman: We were brought in to work with the laboratory research people. Our services were free and if they wanted to call us they could. We had a very good relationship with the public information people who spread the word that we were there. The public information people, mostly Ward Gilbert, would talk to laboratory people about something they were working on; if it seemed to Gilbert or to his associates that the lab people had a problem that the statisticians might help with, Gilbert would say "There are statisticians here at NIH in the Cancer Institute and they might be helpful to you. Why don't you call Jerry Cornfield?". Although we were in the Cancer Institute, we worked with people from any Institute who wanted to talk to us. That led to my working with the group of hematologists under George Brecher. Brecher had a great feeling for statistics and mathematics. In the pre-Hitler era in Germany he had been a student in mathematics. He became a medical doctor because his father said "What is going on here in Germany is such that if you were a medical doctor you will be able to make a living all your life; if you are in mathematics you may or may not." I did a fair amount of work with Brecher. It all started when someone had built a mathematical model for the life and death of red cells. As a hematologist, Brecher was interested, and I had the good fortune to work with him, in trying to interpret the mathematics. We had a good working relationship for many years. Some other working relationships with laboratory researchers were not so good.

Brecher and I followed up the model building and set up more efficient techniques for counting red cells and platelets than the ones in use at the time. Until then, the counts had essentially been done visually, by technicians who would almost go blind doing these extensive counts. Brecher had done a little work on the counting and had come to the conclusion that most people were lying about the reproducibility of their counts. There should have been at least Poisson variation from count to count, and Brecher showed that it was substantially less in the reported data. It turned out that Brecher was right: the people who were doing the counting did not believe the variation should be as large as their counts had indicated, and they found all sorts of reasons for discarding information, because it appeared to be too variable [6].

Simon: So, clinical trials were not the dominant theme then.

Schneiderman: Clinical trials did not begin at NIH until later. Jerry Cornfield was involved in the first of the clinical trials, a trial in the treatment of leukemia. The people who were conducting the trials, mostly under the direction of Gordon Zubrod, were numerically oriented. Zubrod had worked with

Shannon, the NIH Director, in the development of the antimalarials. Zubrod was receptive to statistical ideas and concepts, and, with several people, including Jerry, they designed the first of the randomized trials in acute childhood leukemia. Jerry pointed out this was an good disease to be studying because we had an objective measure of the number of cells, and it was not a question of asking does the patient feel better or does the patient look better. I got drafted into the clinical trials area after Jerry had set the whole thing up. I knew absolutely nothing about clinical trials and only a little bit about randomization.

The Cancer Institute had invited Bradford Hill to come and talk about controlled trials, following what looked like success in the early trials that Zubrod and his group had conducted. Hill gave such an interesting talk, and excited so many people that we asked him if he could come and spend a year with us at the Cancer Institute. He said no, he couldn't, but he had a young statistician on his staff who probably could. We were fortunate to get Peter Armitage for a year.

While Peter was with us, he helped set up the cancer chemotherapy activity, both clinical trials and the screening process for evaluating drugs. Peter had done some work in sequential analysis, and we set up some two- and three-stage schemes for determining which drugs might be effective [4]. After Peter Armitage returned to England, I had the good fortune of getting a Rockefeller Public Service Fellowship that enabled me to go and spend a year with Peter and the rest of Bradford Hill's department at the London School of Hygiene and Tropical Medicine. And it was there that I did the research that led to my Ph.D. I did the thesis research on the closed sequential schemes. Peter had some closed sequential schemes. Irwin Bross also had some which he had computed, almost by brute force. I had some interest in sequential analysis because when I was in the Air Force and working as a statistician I had come upon Wald's work, and it looked like a reasonable kind of thing to use in some of the destructive testing that was being done. If you were testing ammunition for its quality, obviously you had to fire the bullets, and that destroyed them. Therefore, you wanted to have a scheme in which you had a minimum sample size before you could come to a conclusion. Wald's schemes were open schemes. Peter Armitage had a closed scheme but it did not have very good operating characteristics under the null hypothesis. If one of the two treatments was effective, the schemes worked nicely and you got out with a very small sample size, but if the treatments were roughly equal, you went on for a long time. The closed schemes, which Bross had developed, had better operating characteristics, but there were very few such schemes. I spent my year at the London School of Hygiene working with Armitage and developing closed schemes with much better characteristics if the two treatments were equivalent. They allowed you to get out of the trial much sooner, almost as quickly as you did with the Wald open schemes [5].

Simon: NIH became a major center for biostatistics. Did that happen gradually?

Schneiderman: No, it happened very quickly, and for two reasons. It happened because we had a critical mass at NIH: five mathematical statisticians who were working together. Sometimes it sounded as if we were screaming together, shouting at each other; but we really worked together. I became aware of this when we would go to the American Statistical Association conventions and listen to the papers that people were giving, and I once remember remarking to myself "There isn't a subject that these people have been talking about that we haven't worked on!" Cornfield and Mantel did some lovely stuff in probit analysis [1] and ways of computing the relative potency of materials [2], substantial improvements over what had been done before. The only other person that I remember from that time who seemed to be doing work similar to what we were doing was Joe Berkson at the Mayo Clinic. Berkson had done some things in potency determinations and was also working on controlled trials. It just seemed that a burst of energy occurred when our group of statisticians was brought together by Harold Dorn at the Cancer Institute.

Simon: Was it difficult to sell a medical and biological organization on the usefulness of creating a statistical group like that?

Schneiderman: Well, both Dorn and Cornfield were very good at doing that kind of thing. We were not without our problems. I remember one of the Institute Directors got intrigued by what somebody at his Institute was doing with Cornfield, and he approached Cornfield to see if he could come to his Institute and create a statistics group. I don't remember exactly what government salary grade Cornfield was classified as, but probably a grade GS-12 or GS-13, with a GS-15 as a maximum. The Institute Director said to Cornfield "What grade are you?" and Cornfield told him and the Institute Director was astonished, and he said, "Oh! I was thinking you were a GS-7." Then a similar thing happened to me. I was invited by one of the Laboratory Chiefs of the Cancer Institute to talk with his staff about what we were doing. Apparently it went very well; it was supposed to be a one-hour session, but it went on for about two hours. I had a lot of questions about some of the problems that these guys were having in their labs, and whether we could help them. When this was over, this Lab Chief thanked me very much for having come and spent the time, and then said, "You know, what you guys are doing is quite interesting, but if it were really important, I would be doing it." And I didn't hit him!

Simon: Cornfield finally left Cancer and went to the Heart Institute?

Schneiderman: Yes, in 1960.

Simon: Was that at a time when multiple groups were being set up?

Schneiderman: Yes, many of the other Institutes become aware of what the Cancer Institute was doing. Cornfield had been working with several people, and many of the other Institutes' problems were brought to him.

Simon: You were able to keep your independence?

Schneiderman: One of the lovely things about Harold Dorn was that he made it quite clear that what we did was entirely up to us, that we could work on anything we wanted to work on; but it had to be good, it had to be effective. We not only had authority to work on what we wanted, but we had to take responsibility if it didn't work. At the same time, we were also quite independent from medical groups, although we were deeply involved in the controlled trials.

Simon: It seems like you had a very good balance between applications and developing new methods.

Schneiderman: We would get into problems that nobody in statistics had ever worked on before. Laboratory scientists had a problem and we would have to develop a new statistical technique. The creativity was almost forced upon us by the problems. For instance, Sam Greenhouse worked with John Dunn in Cancer Control in establishing the characteristics of a diagnostic test, with the concepts of the false negatives and false positives [3]. I may be giving Sam and Dunn more credit than they deserve, but I think they really started statistical research in the area of diagnostic tests.

Simon: Do you have any closing comments?

Schneiderman: Yes. I remember going outside of the NIH and expressing the excitement that I felt about working at the Cancer Institute. "In all my life, I had never been in a place in which the intellectual level was so high as it was at the National Institutes of Health and the National Cancer Institute." I wasn't bragging about the quality of my colleagues, I was just reporting on them. And, I still feel that way. It was the most exciting intellectual experience in my life.

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