A Conversation with George Fishman

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Abstract The following are excerpts from an extended conversation between the authors and George Fishman that was recorded over the period October 25–26, 2007.

1 Education and Early Career

This section concentrates on George's education and his experiences in industry prior to his academic career. The speakers are indicated by the initials of their first names.

C: Good morning, George. We are very pleased to see you. This is part one of your interview, and we'd like to start with your educational background.

G: In the early 1950s, there was considerable interest in science and technology, and high school students were not immune from that enthusiasm. Those who did well in technical subjects were often encouraged to go into sciences or engineering. Because I was living in Chelsea, Massachusetts, MIT was the closest university that had a good reputation in both disciplines and I decided that I would like to go there. Of course, it became clear that if I was going to have an interview, I had to know what field I wanted to enter. For reasons that still elude me today, I chose aeronautical engineering. The admissions interview committee included an admissions officer and a single faculty member. We had a long discussion about my interests, my grades, my extracurricular activities, etc., and then they asked me what I saw myself going into. I said "aeronautical engineering." Immediately the faculty member's eyes lit up. It turned out that he was in that department. At that moment I was sunk because I knew nothing about aeronautical engineering except that it had to do with airplanes and avionics. Although I thought I had flunked the

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interview, I was admitted shortly thereafter. No more than four or five months after matriculating, I decided that engineering was not my true interest.

Economics was something that I always liked and at that point in time MIT had the leading Department of Economics in the United States. Paul Samuelson was the reigning authority with a widely adopted introductory textbook in economics. I enjoyed the economics courses that I took but, in order to graduate from MIT at that time, one was required to take a minimum of about 60% of one's courses in either science or engineering. Therefore, I also took a wide spectrum of science and engineering courses. These included quantum physics with a well-known physicist, John Slater. Although I've had zero use for this course during the last fifty years, it was more enjoyable than one might think to learn about Schrödinger's equation everyday for six months. [Laughter.]

My principal engineering courses were in electrical engineering, and that's where I learned about networks. Instructors in both economics and electrical engineering encouraged us to take probability theory and statistics. I received my degree from MIT in Course XIV, and my diploma reads economics, political science, and electrical engineering. [Laughter.] Technically, it's all three of those, but I focused on economics.

D: They are really all in the same physical location?

G: No, most lower-level economics courses were taught on the main MIT campus. More advanced ones were offered at the Sloan School, some distance away. I would leave a main-campus class immediately after the bell rang and in ten minutes would have to get to the class at the Sloan School. I usually sat in the back of the class because the teachers didn't like my heavy breathing from rushing from the main campus. Because I finished my in-class course work in December of 1959, I had six months before I could begin graduate school. Therefore, I began working for several MIT economists. These included Robert Solow, who eventually won the Nobel Prize in economics. Another, Maurice Adelman, was an authority on the automobile and oil industries.

When I asked for additional work hours, they referred me to the Political Science Department, where I went to work for Professor Ithiel de Sola Pool. Although this sounded to me like a very nonquantitative area, it turned out that Professor Pool was a protagonist for quantitative methods in political science. His main interest was in content analysis, which had to do with the propagation of information as it passed from one person to another—how fast it spreads in different kinds of societies, things of that sort.

I'm trying to get to the point of what all this had to do with simulation. It was the presidential election year, 1960, and Professor Pool was intimately involved with the Kennedy campaign. He and his collaborators, including Robert Abelson who was a psychologist at Yale, were doing studies of peoples' responses to a variety of political and economic issues. It was the first election in which that was done, and much of the work involved random sampling on a computer by very primitive methods.

When I began this work, I found that the computer programs in use were written in SAP, then the assembly language used for IBM computers. It consisted of three-letter codes, and the program, if stretched out linearly, would probably have covered the entire floor in the computing center. Moreover, it was not executing correctly and no one had been able to debug the program. At my skill level, I knew that I would not be able to as well. Fortunately, during my last semester, I had taken a course in computer programming languages. Although it focused on SAP, the instructor had mentioned that a new language called FORTRAN was considerably easier to use. I began studying the few available FORTRAN manuals and eventually suggested we rewrite the SAP programs in FORTRAN. A long debate ensued as to whether we should do this and whether or not FORTRAN was a good choice. Once we made the switch, the new program executed with a minimum of difficulty, so my opinions became more credible to others on the project.

When I announced to my employers that I had been accepted at Stanford in economics, Professors Pool and Solow told me that they were good friends of Kenneth Arrow, a leading mathematical economist at Stanford. He later also became a Nobel Prize winner. I had not thought to ask them beforehand to write recommendations for me. Once they learned of my decision to go to Stanford, they wrote letters of recommendation to Professor Arrow. When I got to there, he offered me an assistantship. Because he was (and still is) a theoretician, the amount of computation he did was minimal. He assigned me to Professor Marc Nerlove, a twenty-six-year-old econometrician who was already a full professor. He is now at Maryland. I also worked for Irma Adelman, a professor of economic development at Berkeley who was visiting Stanford that year. She had published one of the first studies on economic development using simulation.

Although I was not part of Professor Adelman's simulation work, I did learn from working with her how to go about testing models. Professor Nerlove had developed an interest in applying frequency-domain analysis to economic time series. Previously, these series were analyzed almost exclusively in the time domain. Working with him, I began to cultivate an interest in spectrum analysis.

D: Had you had any spectral courses back at MIT?

G: You can never tell which college courses are going to be helpful. Having taken electrical engineering courses, I was familiar with frequency-domain analysis. Therefore, I found it relatively easy to integrate myself into Professor Nerlove's research. We had a good working relationship in which I felt that I had something more than merely programming skill to offer. Conversely, I learned a lot from him on how to conduct a quantitative analysis, never overlooking contradictions and always giving explanations that would hold up under scrutiny. It was a good relationship and I have always been grateful to him for his guidance.

At the end of my second year at Stanford, I was offered a summer internship at the RAND Corporation, based on recommendations from Professors Arrow and Nerlove. Both had been RAND consultants. That August (1962), I was offered a permanent position at RAND which I accepted. My completed credit hours at Stanford earned me a master's degree in economics that Fall. At RAND, I joined the Logistics Department. To a great extent, logistics involves microeconomics and that fit with my education to date.

Logistics is a major component of all military organizations. I didn't know that before I went to RAND. As Jim has been in the service, he can appreciate this fact. The Air Force, RAND's principal client in 1962, had challenging problems in reliability, maintainability, inventory management, and facilities location. These intrigued me.

D: Can I make you do an aside for a second? I should know this, but how exactly is RAND related to government organizations?

G: RAND came into existence as a consequence of the actions of farsighted people in the scientific community and the U.S. Air Force. During World War II, groups of scientists had been set up in Washington, D.C., to work on defense related problems. The leader of the entire scientific effort was Vannevar Bush, whom you may never have heard of.

C: No, he's well known. I have read about him in history books.

G: Bush had become head of the Carnegie Institution in Washington, D.C. President Roosevelt made him his science advisor. He was the liaison between the scientific community and Roosevelt, and basically mobilized a considerable amount of the scientific war effort. Bush recruited people like Phil Morse from MIT to work on a whole host of problems. The Air Force recognized the value of this work; in particular, General "Hap" Arnold. He was one of a handful of five-star generals during World War II.

C: I think I have read about Vannevar Bush in relation to George Dantzig.

G: That's possible. Dantzig was at a considerably more junior level. He worked for Marshall Wood, who led a group doing analysis for the Army Air Force. Wood was one of the people who were instrumental in the armed services becoming interested in operations research. He recognized the value of OR techniques and assigned Dantzig a variety of problems related to aircraft scheduling, airlift scheduling, transportation, etc. That's where many of the problems on which Dantzig focused originated. But getting back to your original question, two important events happened at the end of the war. Bush recommended that an organization be set up with sufficient funds to sponsor a wide range of basic research. That organization came to be known as the National Science Foundation.

D: That's where NSF came from?

G: Right, it came from the recommendation that Bush made to President Truman. Bush went on to become the Chairman of the Board of MIT. I have to tell you an anecdote. Bush went to the same high school as I, where we had a high school play focused on famous alumni. Guess who I got to play? Vannevar Bush! [Laughter.]

To return to the discussion, General Arnold recognized that the Air Force would benefit from technical assistance in many areas. He arranged for the Air Force to give a contract to Douglas Aircraft to set up a research group. It was called the Research and Development Group, which we now call RAND. Starting about 1947 or 1948, the group occupied a building that Douglas Aircraft owned in Santa Monica. Several years later, RAND moved into newly constructed buildings close to its current location on Main Street in Santa Monica. At approximately that time, H. Rowan Gaither, a name that you won't recognize, served as Chairman of the Ford Foundation and as Chairman of the Board of RAND. I may be off in the date but he certainly was a presence in RAND's early development. To answer your original question, RAND was a nonprofit corporation with buildings put up with Air Force and the Ford Foundation financial assistance.

It soon became apparent that RAND's aims and objectives, especially with regard to long-term research, differed from those of Douglas Aircraft, a profit-making company, that was considerably more task-oriented. Here's what we have to do, and here's the data. RAND was not designed that way. To his credit, General Arnold recognized that you had to put people into an environment in which they could think more comprehensively. That was the RAND atmosphere that I found when I got there in 1962. For me, that was a very good experience. As I said, I was in the Logistics Department and worked on a variety of problems. In retrospect, none of my contributions were significant for solving Air Force problems. I was learning how to be a researcher and paid considerable attention to how more senior members of the research staff went about their work.

Towards the end of 1963, Murray Geisler became the head of the Logistics Department. A statistician, he was active in management science and had written several papers on simulation. RAND used simulation to study a variety of Air Force logistics problems. I never became an integral part of that effort. As chairman, Geisler had many responsibilities one of which was to assign referees to Logistics Department documents that were going to appear as external RAND publications. Each was internally reviewed by two people. I was assigned a paper having to do with methodology for running simulations, which focused on the batch means method.

I wrote a brash report that focused on all the issues that the authors had overlooked. Very brash. [Laughter.] I wouldn't write a report like that today. Geisler told me that he liked the report, which was very reassuring. He also encouraged me, saying that simulation was an emerging area and suggested that I should devote time and energy to it. Then he said "To get you started, here is an internal RAND document by Ken Arrow that's never been published."

That document gave me a considerable understanding of the area. Conceptually, Arrow understood exactly what the issues were. Over the years, I've come to realize how farsighted he was in terms of what the methodological challenges for simulation were. That's how my serious research interest in that area began. At the same time, Phil Kiviat came to work at RAND. After getting his master's degree in operations research from Cornell, he had spent two years at U.S. Steel where he developed the GASP discrete-event simulator, which was FORTRAN-based.

At the time, Cornell was, without question, the university where the basic concepts of discrete-event simulation were put into the classroom on a formal basis. Dick Conway and Bill Maxwell were responsible for that. They saw simulation as a legitimate area of inquiry, an opinion not widely shared then by others in the academic community.

D: This would be around 1963?

G: Yes, that's my recollection. When I would sit down with Phil, I didn't get the idea of a "piecemeal" field that was being put together by people with limited technical skills. It was a formal field, in which language was the major formalism.

Although his training was as a mechanical engineer, he had gravitated to the area of simulation languages. He encouraged me repeatedly, telling me there was a great need for people in the area of statistical methodology. So that was the area in which I chose to work. Moreover, the paper that I had refereed motivated me to try another method for estimating the variance of the sample mean. I took what I knew about spectrum analysis and began doing simulation experiments. Phil supplied many ideas and we worked well together. That collaboration led to our 1967 *Management Science* paper. Although that paper shifted attention from *ad hoc* evaluation to more of a methodological approach, it required considerable computing time relative to the time spent simulating.

When I first came to RAND, the computation of the spectrum at any point was a quadratic sum calculation done by computing the autocovariances and then taking their Fourier transform. That was an order n^2 operation. Since it took much longer to analyze the data than to generate them, the method had limited appeal. But in 1965, the Cooley-Tukey algorithm for the fast Fourier transform came along. It transformed not only simulation, but also many other areas. You could then perform those computations very rapidly. As a result, the technique developed a broader appeal.

Actually, Jim, I'm going to have to tell you a story about your colleague, Salah Elmaghraby. The first public presentation of the spectrum analysis paper that I gave was in Vienna, at a meeting of the International Institute of Management Sciences. The talk was limited to about fifteen minutes, restricting what I could say about the method. Salah was in the audience and, after I finished, he said to me: "That's a wonderful paper. What is it all about? You have to explain these details." [Laughter.]

D: He was around when Maxwell, Conway, and all these guys were starting to think about these concepts at Cornell.

G: Oh yes. He was one of the first Ph.D. students in the Cornell OR Program. Previously, Salah had worked for Western Electric and then went back to graduate school at Cornell.

After publication of our paper, I concluded that, computing cost aside, the setup cost to learn about spectrum analysis was too large to make the method widely attractive for estimating the variance of the sample mean in simulation experiments. I knew that an autoregressive scheme had an easily computed rational spectrum and thought this alternative approach might offer a convenient approximation for the spectrum of a queueing process. Although these processes generally do not have linear autoregressive representations, their spectra can be approximated to some degree of accuracy by rational spectra. Also, this approach got the analysis out of the unfamiliar frequency domain.

I wrote my paper on the autoregressive method in 1968 and through Phil Kiviat was invited to present it at the Second Winter Simulation Conference in New York that December. Because I was unaware that this conference was a succession of single-session events, that is, no parallel sessions, I was surprised to find an audience of three to four hundred people waiting for me to speak. This was intimidating to someone more accustomed to fewer than ten or fifteen attendees at a presentation in one of RAND's conference rooms. My talk was well received. During the presentation, I had felt that I was not making sense to the audience. But the response afterward indicated otherwise. Most of all, they were happy to see someone talking about statistical methodology. That's where and when I finally accepted Murray Geisler's assertion that statistical methodology was an important component of simulation.

In the late 1960s, Phil Kiviat and I decided to write a series of papers on the methodological aspects of discrete-event simulation. We called the series "Discrete-Event Simulation." Each would be on a different topic. I wrote one on statistics, he wrote one on languages and another on modeling alternatives, which I regard as one of the best papers in the simulation literature. That paper formalized the difference between the event-scheduling approach, the process-interaction approach, and the activity-scanning approach. I adopted many of those concepts in my 1973 book on simulation.

Most of Phil's papers never became academic publications. Some of mine did. As to the balance between languages and statistics, there's no question that when I came into the simulation field, the emphasis was on language. Moreover, the distinction between model and language was not clear. People had unusually creative ideas about languages and modeling, but the focus was on the issues of how to make event lists function efficiently.

SIMSCRIPT was an attempt to make simulation modeling more accessible by being more conversation-like than FORTRAN. By making it more forgiving during compilation, it made it easier to code, in principle. It might have prevailed as the dominant simulation language at that time, except for the fact that IBM had come out with GPSS. GPSS had two advantages. It was an IBM software product and it offered a more attractive environment for modeling. The user sat at a remote terminal—that's what we called them at the time, basically a teletype machine—and merely interacted with the code. Because of its structure, successive interactions occurred without a full-blown recompilation. No recompilation meant faster interactive responses. At that time, compiler-based simulation languages took a long time to compile, an unappealing property for people who had thousands and thousands of statements in their simulation code. GPSS internalized much of this modeling effort by using "off-the-shelf blocks" and interpretively executing the simulation program made up of these blocks.

Although eliminating compilation gave GPSS an edge, it had other limitations. I was present at many discussions about its slow execution and its lack of flexibility. Most notable were its inefficiencies in processing the current and future events chains that contained transactions that were waiting to execute.

Harry Markowitz and Phil Kiviat were the principal developers of SIMSCRIPT II at RAND. Harry had left RAND shortly after I arrived and gone into business with Herb Karr. However, he remained a consultant. He and Phil would get together several times a month. My first exposure to Harry actually was at Stanford where I took a course that Arrow offered on portfolio analysis in 1961.

D: That's what Markowitz was famous for though, right?

G: Right, the principal topic was Markowitz's book on portfolio analysis. Arrow cast the topic in a more formal setting focused on utility functions and nonlinear optimization. But Markowitz's book was definitely the essential feature. When I got to RAND, I was puzzled by what Harry was doing working on simulation. It turned out that he had very broad interests, including discrete-event simulation. His Nobel Prize award testifies to his accomplishments.

By the end of the 1960s, I realized that RAND was a plateau type of environment. Many staff were at my level, but few were senior people. I equivocated in my own mind as to whether I would enjoy being a senior person. I wasn't sure I was suited for it. The only option for me was to find something else to do. Several times, I had been encouraged by RAND colleagues to teach. I actually did teach introductory statistics at UCLA in 1965. In 1967, I finally acknowledged to myself that the only way I could teach at a university was to get a Ph.D. So here is the answer to your question about why I studied biostatistics. There were two major universities in the area that offered Ph.D. programs. UCLA which was twenty minutes away, and the University of Southern California which was thirty-five minutes away. Several RAND staff in the Logistics and Mathematics Departments had recently earned their Ph.D.'s through the Biostatistics Department at UCLA. You may know Stan Azen, who has served as the editor of the *Journal of Graphics in Statistics*, and Craig Sherbrooke, one of the major contributors to multiechelon inventory management. Both had gone through the biostatistics program. I saw that they were able to balance the demands of full-time work at RAND and the graduate program. So I enrolled in 1967 and completed my degree requirements in March 1970.

By then the RAND environment had changed dramatically. It was no longer the research organization that I had joined in 1962, partially because of the change in funding arrangements. RAND had done a good job of educating the Air Force in using analytical techniques and so it now was capable of doing analysis for itself. Therefore, its level of dependence on RAND had become less. RAND sought other sources of funding. It solicited support from nondefense government agencies whose interests were more task oriented. That implied shorter research time horizons and less time to indulge one's interest in more conceptual research.

I decided to look for an academic appointment in the Spring of 1970. I was invited to visit Northwestern University for an interview in the Department of Industrial Engineering. In late June, the department offered me a position. The chairman sent me a handwritten offer for a tenure-track position. He apologized for the informality, but student campus protests had effectively shut down the university. Shortly before that, I had received a call from Harvey Wagner at Yale University, whom I had known at Stanford. He was familiar with my published research, and we knew each other casually. He said "Would you like to be considered for a position here?" So early in July of that year, I visited New Haven, Connecticut, and gave a presentation to the faculty of the Department of Administrative Sciences. The department comprised the disciplines of organizational behavior and operations research. Shortly thereafter, Bob Fetter, the department chairman, called me from a public pay phone on a highway in New Mexico to offer me a position as an associate professor. I had made it clear to both Northwestern and Yale that I did not want to begin as an assistant professor. One of the benefits of RAND was that I was able to write a 1969 book on spectral methods in econometrics, which was basically an outgrowth of the work that I had done under Marc Nerlove. So between that and my journal publications on simulation, both places were willing to offer me an associate professorship.

The informality of both offers made me wonder as to how these universities worked. That was compounded by my next experience. I can still recall that when I got to Yale, I asked Bob Fetter "Okay, what am I to teach?" He looked at me with a smile and said "What would you like to teach?"

2 Academic Career

In this section, George discusses his first academic appointment at Yale University and his move to the University of North Carolina.

C: We are now moving to the second part of the interview which concerns your arrival at Yale and eventually the move to UNC. So you have the honors.

G: I wasn't ready for Bob Fetter's offer to teach whatever I wanted, nor was I ready when I first got to New Haven for the fact that I was going to have to prepare teaching materials. I assumed that Bob would pick the subject and that there would be a standard textbook for it. He wasn't suggesting anything—all courses were covered—and he said "Teach whatever you wish." I responded with "What if I offer a course in spectral methods in economics?" He said "That's fine, but remember we hired you principally for simulation." I said that I would do that in the spring semester, and that was agreeable. That Fall, I lectured three times a week to three or four students on spectral methods.

J: So this was the Fall of 1970?

G: In the spring semester, I taught the simulation course. During the fall semester, I had looked at the potential textbooks, but none struck me as acceptable in the methodological sense. The closest one was Tocher's book. But that was so tightly written, that it would be very hard for students to get the meaning of what was really a very rich book. So that Fall I began writing notes. When I look back, it's hard to believe that during the four fall months I prepared 400 pages of typed notes for the spring semester. Needless to say, during my first semester of teaching simulation, I was merely ahead of the students by a few hours, so to speak. They were kind, helping me with typos, etc. Those notes formed the basis for my 1973 book. It focused on modeling, programming languages, and statistics, ideas that I had adopted from my association with Phil Kiviat. Because of the separation of modeling and languages, language did not dictate modeling, but modeling dictated what features a simulation language needed.

I should say that one of my responsibilities at Yale—the way that I was hired—was as the associate director of a health services research project, a joint effort of the School of Public Health and the Administrative Sciences Department. So I had some administrative duties as well, and taught a seminar in health services research using quantitative methods.

D: Were you involved with health systems back then?

G: Yes. I taught a seminar in it. Lee Schruben was one of my students. In fact, if I remember correctly, when we recruited Lee as a student, we offered him a fellowship in the Health Services Program. Wayne Winston of Indiana University was also a member of that program.

Several things occurred during those years. The OR simulation research community became much more active. The Winter Simulation Conference was instrumental in that. It created a focus. In particular, it established an accommodation between industry and universities, which in many ways continues to this day, perhaps in different proportions. It was well understood that both domains would be respected and be part of the annual conference. The first several conferences were unusually successful. Its sponsorship progressively got broader. For a number of years, the then National Bureau of Standards was a cosponsor. The name of the fellow there slips my mind. He was very instrumental in helping with it.

J: Paul Roth?

D: He's listed on the program even to this day.

G: Yes, he arranged for the facilities of the Bureau to be made available for the conference. You've all been involved with the conference so you know it needs some kind of continuity from year to year. Well, that was one of the difficulties when it first began, first through Julian Reitman's efforts and then with the help of Arnie Ockene of IBM. In the early 1970s, the conference was attracting more academics from diverse fields such as civil engineering. Joe Sussman at MIT, whose research was in transportation, did a lot of simulation in civil engineering.

There were also more people getting interested in the statistics of simulation. I neglected to mention that during my RAND years Alan Pritsker was also instrumental in making me see the value in simulation statistical methodology as a field of research. Alan was a consultant to RAND. I often worked in my RAND office in the evenings and Alan was always there. We had many conversations and I could see that his ideas were similar to Phil's. It was not difficult to see that Alan, like Phil, had a well-thought-out view of modeling, language, and statistics as applied to discrete-event simulation. While their views may have differed in emphasis, their conceptualizations made it considerably easier for me to see where my interests could fit.

There were also people working on variate generation and random number generation. In the early 1970s, Ahrens and Dieter published several papers that described variate generating algorithms with bounded computing cost, independent of distributional parameter values. Those were intellectual ideas which I don't think are fully appreciated today, because they're lost in the mix over time.

My first sponsored research proposal was to do simulation analysis graphically on a monitor. [Laughter.] It was not funded. Although well regarded by reviewers, NSF rejected it. Afterwards, one of the reviewers told me that he recommended rejection because my proposal wasn't feasible. [Laughter.] At that time, he was probably right. Tektronix offered the most advanced graphical capability. One could take a body of data and put it on a screen, but if you tried to add or subtract something from those data, you had to rewrite the entire screen. No addressing of individual pixels or anything like that. No animation. It was in primitive form. I had not realized how slow the entire interactive process was until Tektronix gave a demonstration at Yale. By the end of the decade, graphics had become a standard part of discrete-event simulation. Graphics devices were being improved and new language constructs were making it easier. The introduction of PCs in the late 1970s made the interaction more productive per hour of effort. So I might have gone in an entirely different direction, had those developments occurred a decade earlier.

My interaction with Matt Sobel came about in an interesting way. During my first Yale year, he had been away at CORE in Brussels. In the Fall of 1971, Bob Fetter mentioned to the faculty that the department had received a request from the New Haven Housing Authority for help in managing their inventories and maintenance. Matt and I were the only faculty who expressed interest in the project. He and I spent three years consulting for the Authority. The first day we arrived on site, the director said to us "Where did you park your car?" We said that we parked in the street, and he said "Go out and get it and bring it inside the fence, inside the barbed wire." [Laughter.] The Authority was in a tough section of town. It had wonderful problems for anyone interested in OR. To put it concisely, they had the funds to buy supplies, but needed guidance on how to set reorder levels.

For example, they had a ten-year supply of Moen cartridges. [Laughter.] Moen cartridges were not in common use in those days. However, the Authority had learned that when they used regular washer faucets, leaks were not reported punctually and, as a result, it was paying for a lot of needlessly leaking water. By using Moen cartridge faucets, they could eliminate the leakage. However, that policy led to a substantial inventory of Moen faucets.

C: Did that involve simulation at all?

G: No. This experience broadened my understanding of what OR could do for people. At about the same time, I was asked to consult for the RAND Institute and the Ford Foundation in New York City. The RAND work never panned out, because we were unable to identify a specific problem calling for my expertise. Al Madansky, a statistician whom I had known at RAND, was responsible for my involvement with the Ford Foundation. Al had become the chair of the Computer Science Department at CUNY. He was also consulting for the Foundation. He felt that my interest in simulation would be helpful on one of its studies of the performing arts. Every several weeks, a group of consultants would meet at about 4 p.m. at the Foundation's headquarters in a beautiful building close to Second Avenue in New York City. The 4 p.m. time allowed those who were academics to travel, after class, into the city from their respective universities. I recall that the consultants included faculty from Yale, Princeton, CUNY, and possibly Columbia. I never saw how simulation could be a major contributor. The principal focus was on the analysis of data. Eventually, I helped edit one of the studies. Seeing how the Foundation operated was an eye-opener. Although it did not lead to much fundamental research, it did provide good conversation and many good dinners at the Foundation's expense. Dick Shelton, the Ford study leader, arranged these memorable occasions after our working meetings. On one occasion, I recall that the arrival of the bill prompted the invention of two additional attendees to justify its size. [Laughter.]

In conversations with Matt Sobel, whose interest was in queueing control, I began to think about how queueing properties affected statistical behavior in discrete-event simulation. I realized that the time-dependence within queueing sample paths was unconditional, but that there were conditions under which successive segments of the time path were independent. For example, entry into the empty-and-idle state. I also raised this issue with Madansky who pointed out to me that the empty-and-idle state is a special case of the more-general concept of renewal processes. These interactions motivated me to read about renewal processes and led to my first paper on the topic of independent sample-path segments.

I had also received a grant from the Office of Naval Research (ONR). One of the grant's stipulations was that all those who were supported by ONR in a common research area were to interact with each other. Interaction meant that you sent each other technical reports. One of these grants was at Stanford, and the only person on that grant whose name I recognized was Gerry Lieberman. I sent him a copy of my paper on what's come to be known as the regenerative method, and he promptly sent me back a paper by Don Iglehart and Michael Crane on the same topic. The topical match between them was a big surprise—two groups had come up with the same idea at the same time. Although each paper had a different twist, there was no question about the commonality of the idea. Both papers were published roughly at the same time. In the Fall of 1973, Don Iglehart and I were invited to present our papers at a TIMS meeting in San Diego at an OR-sponsored session.

By then—well, much earlier than that—I had decided that I wanted to leave Yale. I had come into a department that was truly a "warehouse" for two disparate disciplines. Early in the days of operations research, there had been this concept of having it interact with the psychology community, particularly organizational behavior. The concept of man-machine simulation was big—it was a major topic in the Logistics Department when I arrived at RAND. The Administrative Sciences Department at Yale had been established as a home for organizational behavior and OR. But the two disciplines had fundamentally different views of what constituted research. This led to a tense atmosphere that I did not enjoy. Although I suspected that it could eventually be at the expense of junior faculty members, in retrospect, I don't know of any junior faculty whose progress at Yale actually suffered because of the conflict.

In 1973, the Administrative Sciences Department was incorporated into Yale's newly established School of Organization and Management. That arrangement led to other conflicts. I decided to look for a new position. After the San Diego meeting, I gave talks at several universities. Maryland's business school and UNC's newly established Curriculum in Operations Research and Systems Analysis expressed interest.

C: But the Curriculum was still housed within the Statistics Department.

G: No. The previous year, Jim Gaskin, the Dean of the College of Arts and Sciences, had established the Curriculum as a separate academic entity.

C: And they were located in the Phillips Annex?

G: Yes. Originally, Jerry Gould was the chairman, but by then he had departed for the University of Chicago, and Jack Evans was the chairman. My interview was at the height of the 1973–74 oil crisis and gasoline was hard to come by in Chapel Hill. So I agreed to take a bus from the Raleigh-Durham Airport to the Holiday Inn in Chapel Hill. That gave me the equivalent of a Cook's tour of the Triangle, Durham, and Chapel Hill. Jack Evans met me at the motel and said "Before dinner, we'll take you for a drive around so you can see the town." And I said "Oh, will we have time?" I had no concept how small the town was!" [Laughter.]

C: What year was that?

G: 1974. Shortly thereafter, UNC offered me a position as a tenured full professor. Although Maryland was still mulling over what they wanted to do, I had already decided that Chapel Hill was a better place for my family and me. We moved here in July, 1974—I, my children, Becky and Matt, and my wife, Sue. I quickly learned that circumstances were not as I had originally pictured them. The Curriculum was a separate freestanding unit, and it did have two tenure-track positions. But it was still not a department, and therefore, whenever we would go to college-level meetings, there were people who would say "It's a curriculum. What are you doing here?" or "Operations Research? I thought that was part of Statistics!" The OR program did not have much status on campus. Moreover, there were people who saw no reason to continue the program. Although these attitudes made me uncomfortable for a number of years, events in the late 1970s gave reason for cautious optimism.

Even though Dean Gaskin kept his commitments to the Curriculum, he wasn't providing the additional resources that the program needed to grow. A newly appointed dean, Sam Williamson, a military historian, was considerably more of an activist. Through his affiliation with the military, he knew what OR was. He recognized the peculiar situation of the Curriculum and raised the question "What should the future of this program be? Should it be eliminated or continued?" At roughly the same time, Phil Manire was appointed as the dean of the Graduate School.

C: What year was that?

G: Probably 1978 or 1979. Dean Manire was a microbiologist who had been a guiding force behind the development of the Microbiology Department at UNC. I attribute the survival of the Curriculum to him more than to anyone else. He appointed a committee to advise him on the future of the Curriculum. It consisted of faculty from the Mathematics, Computer Science, and Statistics Departments and the Business School. Jack Evans, who had left the OR program to become the assistant to the chancellor, encouraged Manire to keep the program. Also, the chairman of the Mathematics Department, Bill Smith, felt that this was a program worth supporting. John Tolle now had a joint appointment in Mathematics and in the Curriculum and Bill was familiar with John's interests and work.

Manire recommended to Williamson that the Curriculum be continued. Williamson did that and more, an action that led me to understand what a good administrator does. Not only did he allow the program to continue, he provided additional resources for it to reach its potential. In 1980, I was asked to be chairman. Before accepting I met with Williamson and asked for additional resources, and these were also granted. The Curriculum had already gotten new space (the Smith Building), new equipment, and additional positions. That was a very successful period and the next dean of Arts and Sciences, Gillian Cell, continued that support. Upon meeting her for the first time, I learned that she was a historian whose area of specialization was Labrador. I kept saying to myself, how was a person who specializes in Labrador going to know what operations research is? [Laughter.] Well, it turned out that she was also a conscientious historian. Prior to our meeting, she had read the College's file on our history.

I should also mention one other thing about Phil Manire which forever endears him to me, besides his fair-mindedness. In discussion with him, I expressed my frustration at the slow progress in recognition that the Curriculum was making on campus. I asked him how that was done. He said "By advertising. You have to keep on going around and introducing OR, showing what it can do for people on campus and stressing its academic accomplishments. No money ever flowed to a department on this or on any other campus where the chairman did not push the department." Manire exemplified that policy. When he first came here, Microbiology was also a curriculum. He turned it into a substantially first-rate department. Most UNC AIDS research is done there.

I took him seriously, and tried to take that stance whenever I was in a meeting with other groups; not to look like the junior partner but to speak with confidence. Jim, you probably found this in your own experience that when you talk like a chairman, others treat you like a chairman. If you act like a supplicant, that's how you're going to be treated. If you talk like you deserve to be there, you find faster acceptance. [Laughter.]

D: Could I ask to step back for a second, because I might have missed something here. When you got there, who was the Curriculum answering to? They were not part of another department were they? So it was treated as kind of a minidepartment then, right?

G: Right.

C: But some statisticians were also part of the OR Curriculum.

G: Well, on this campus the concept of a curriculum was as an interdisciplinary group. Other departments were encouraged to contribute faculty time. In the early 1970s, the Business School contributed courses taught by Jack Evans, Roger Blau, and Dave Rubin. At that time, its dean, Morris Lee, believed that OR was a worthwhile discipline.

Computer Science allowed its faculty to participate, but not to teach our courses. Fred Brooks, its chairman, was supportive. Don Stanat, whose area was languages, and Vic Wallace, whose interest was decomposable Markov chains for network analysis, were part of the Curriculum and had research interests that overlapped with OR. Several OR faculty including me sat on dissertation committees in that department.

We also had Jon Tolle joint with the Mathematics Department, Walter Smith from Statistics, and Dick Shachtman from Biostatistics. As I've said, the Curriculum received its first two tenure-track positions in 1973. When several other departments found out about this, they were puzzled because this wasn't the conventional UNC definition of a curriculum. In conversation, some told me that we should not have received tenure-track positions. Never mind that they were talking to one of the tenure-track faculty. [Laughter.] That issue eventually became less of a topic of conversation.

To get back to the simulation side, issues that I and others had raised began to attract more attention. Better random number generators were materializing as were better variate generation methods. More academics were expressing interest in simulation statistical analysis. Jim was one. Lee Schruben was another. Lee's appointment to Cornell in about 1976 gave me considerable satisfaction. It meant that Cornell took seriously the area of simulation statistical methodology. When Don Iglehart, with a substantial reputation in applied probability, expressed interest, that was additional verification that Murray Geisler, Phil Kiviat, and Alan Pritsker had wisely advised me.

What I'm saying is that the analysis of discrete-event simulation has matured. However, I did have a concern. Although the statistical problems were well understood, I felt the area of output analysis was becoming one of diminishing returns. I didn't see that I had much more to contribute. In conversations with my UNC colleague, Scott Provan, I revived my interest in networks. A decade earlier, Alan Pritsker had aroused my interest in the network formulation to discrete-event simulation. If Alan could have had his way, discrete-event simulation would be taught principally through a network formulation.

J: Oh yes, I think that's true.

G: What impressed me about networks was that if you started out with this intrinsic structure, you could exploit it for variance-reducing purposes. I worked with several creative students and we produced a series of papers on variance reduction in networks, in particular, on antithetic variates. Christos worked on one of the more interesting ones, the max flow–min cut distribution problem, which to this day is still an extraordinarily tough one to analyze analytically.

C: I actually still find it the toughest one I ever worked on.

G: It is, it is. Many people had useful insights that didn't carry over to great generality. One had to cater too much to the particular network being analyzed. Nevertheless, I saw that this area was worthwhile and the nature of the problems motivated me to move more toward Monte Carlo sampling methodology. I began reading the proceedings from old conferences on simulation and Monte Carlo. These were held in the late 1940s and early 1950s, some at UCLA, at IBM in the New York area, and I think, at the National Bureau of Standards, today called the National Institute of Science and Technology (NIST). Attendees came from many areas and organizations, including RAND, IBM, and the Bureau. Von Neumann's contributions were presented by George Forsythe, then of the Bureau. Ted Harris gave a talk. To a great extent he was a theoretician. Have you ever heard of Harris recurrence? That's the same Harris. [Laughter.] When I met him some years later, he was chairman of the Mathematics Department at RAND. It was clear that his was an entirely abstract view of these problems. But in his earlier incarnation, he had a much more applied view.

During the 1960s, unflattering remarks about simulation were common. Many felt that only analysts with limited analytical skills resorted to simulation for problem solving. The remarks were usually made by individuals who had not confronted problems as complex as those under study. That attitude continued well into the 1970s. However, I was reassured by the knowledge that people like Harris, von Neumann, Arrow, and Markowitz had interests in the area and recognized the challenge. Another man who was involved with it was Herman Kahn. Although you may remember Kahn from his work on variance reduction, he achieved his greatest notoriety from a book he wrote while at RAND, *On Thermonuclear War*. When I interviewed at RAND, I wondered why people were picketing outside its entrance. Kahn's book spoke of surviving a thermonuclear war and that motivated the protest.

From reading these people's remarks on simulation and Monte Carlo methodology, I realized that they didn't think of simulation as something to be tried if all other methods failed. They saw it as a methodology that could provide flexibility. That convinced me that if you started doing this in any number of problem areas, for example in networks, you ought to be able to formalize ideas, which would have much more generality for a wide range of problems. That's what encouraged me to focus more on networks and Monte Carlo.

3 Life After Being Department Chair

This section focuses on George's research during the 1990s, following his tenure as chair of the UNC Department of Operations Research.

C: We are now moving to the 1990s, and the emphasis on computational issues.

G: As I read more about Monte Carlo, I decided that I wanted to write a book to get it into an easily understandable form and to describe what was going on in particular areas. Monte Carlo was a collection of techniques, but as a formalism, it lacked coherence. My 1996 book was an attempt to overcome that limitation. In retrospect, the book turned out to be a compendium of techniques rather than a pedagogic device. Nevertheless, the compendium gave a comprehensive picture of the area.

With the batch means method—which, as I said, was around from the beginning of simulation statistical methodology—it occurred to me, as to many other people, that it was much easier to understand than autoregression or spectrum analysis. Interestingly, there is a statistical paper by Champernowne in the 1950s in a British journal that describes a variant of the batch means method.

J: I've heard of this paper but I've forgotten much about it.

G: Some of the ideas there seem to be very much related to the time series ideas that I had seen in Maurice Bartlett's papers. I wrote a computer program for my simulation class to implement batch means that allowed a user to progressively monitor convergence of the estimate of the variance of the sample mean as the sample path increased in length. I published a paper on that approach in 1978 in *Management Science*.

At that time, Lou Moore was a doctoral student of mine. We began talking about speeding up the procedure. Lou actually wrote a program that did that. For reasons that are not clear to me, we did not pursue this speedup method and I cannot recall what happened to that computer program. It was a first-rate attempt to accelerate computation.

In the early 1990s, my student Steve Yarberry got interested in this problem and we talked about how to increase batch size while reducing computing time. We came up with the square root rule as the crucial element for doing this. Our 1997 journal article describes the technique in detail. Our LABATCH and LABATCH.2 software is based on this approach. Every so often, I hear from people with diverse backgrounds who use LABATCH and LABATCH.2. They usually contact me about technical features of the code. I don't have a good picture of the extent to which it is used, but I do know that it continues to be used.

During the summer of 1986, Russell Cheng arrived for a yearlong visit to UNC. I told him that I contemplated writing a book on Monte Carlo and he was encouraging. Work on the manuscript was slow and halting at first. By then, the OR program had become a department and the demands of chairmanship made it difficult to write as much as I wanted to.

At the end of my second term as chairman in 1990, I went on a yearlong sabbatical. Because my wife, Sue, and I concluded that the time was not opportune for taking our children out of the Chapel Hill schools for a year, I searched for a local opportunity. I approached John Geweke [now at the University of Iowa] who was the chairman of the Institute of Statistics and Decision Sciences at Duke, and I asked him if he could provide a desk. John had come over to UNC several times and we had talked about simulation. He was most cordial and kindly arranged accommodations for me at the Institute. When I arrived, I learned that John had accepted a professorship at Minnesota. [Laughter.]

I can't say enough for the faculty at Duke. They were extremely warm and welcoming. Although they were not directly interested in Monte Carlo, they used it, understood the ideas, and offered many suggestions. So I began to realize that it was now becoming part of the statistics milieu. I had many talks with the Duke faculty, especially Michael Lavine. I didn't agree with everything they said, but it all had relevance.

This exposure led me to cast what I was writing into a broader format in terms of problems and techniques that would make my book more appealing to statisticians. Although I didn't want to move too far away from OR, I included examples like the eye-hair contingency table problem in Diaconis and Sturmfels.

It took several more years for me to get to the publication stage. The reality was—you may not believe it—but there were actually more manuscript pages on several different subjects that I chose not to include in the published book. I was fearful that potential publishers would be uncomfortable with a book of more than the 700 pages that I submitted. Of all my books, it's the one that's sold best. It continues to sell a substantial number of copies in Europe, and I am mystified as to who's buying them. By now there are books for statisticians that are more focused on their interests.

C: We are moving to the later stages now.

G: By the early 1990s, interactive modeling had become an essential pedagogic device for teaching discrete-event simulation. However, I wasn't prepared to make that the focus of a simulation course. I preferred to use different languages to demonstrate their features to students, because I still thought at that point that it was very important for them to understand the limitations of individual languages. Therefore, I used a mixture of SIMSCRIPT and Arena, which by then was in a form that students could easily use. If we had had an engineering audience on the UNC campus, I probably would have moved faster. Our students principally were mathematics majors. In the late 1990s, I decided to convert simulation class notes I had recently prepared into a book. My 2001 book, *Discrete-Event Simulation*, was the result. In reality, the material in the book was dated, partially because I used SIMSCRIPT as the prototype. Although it remained a good teaching device, SIMSCRIPT didn't have the many conveniences of other online languages. By then, students were accustomed to more immediate real-time interactions with their programs.

C: Actually, the military still uses it.

G: That's because of their association with CACI, the company that owns SIMSCRIPT. In order for the students to go out and be marketable, they have to know something about a modeling language and how to use it. I picked Arena to provide a more highly interactive experience for students. Had I continued teaching, I would have switched to AutoMod. It's hard to write a well-rounded book on simulation because the language/modeling part has again become the central focus, and anything of a statistical nature is definitely peripheral. In many cases, people don't bother with it at all. Early on, they didn't bother with it, but for different reasons. Now they don't bother with statistical procedures although they are actually accessible with merely a few keystrokes. For example, Arena and AutoMod both offer these procedures.

During my retirement which began in 2001, I decided to write a lower-level book on Monte Carlo. The book was published in 2005. I made it heavily example-oriented. It seems to interest those in a wide range of disciplines. I continue to get inquiries about issues it raises. This book, as well as my 2001 one, have one particular advantage over the others I've written. I can maintain their errata online. [Laughter.]

C: So do you believe that the 2005 book will be the most read because of the wide area it's covering?

G: I hope so. However, others also are putting out lower-level books. At the time I wrote the book I looked for an example in genomics. I picked one on protein folding and felt I had to make the example clear in terms of biology. I gave a very elaborate description of the structure of proteins and how all this works in three dimensions. Since then I have come to realize that most people don't bother reading that. They rely on a more concise approach to it, and I probably could have gotten by with a much shorter account. I think I made the description a little heavy-ended.

C: Do you have a few comments for your research associates and students?

G: I was fortunate to have good students. None at UNC shortened my life. [Laughter.] Andy Seila was my first. With him, I had the benefit of someone who was very focused on getting through. He wrote a nice dissertation that had to do with quantile estimation. It looked into an issue that had been raised by Iglehart and his colleagues.

When I first came to UNC, the Curriculum essentially had a volunteer faculty. As the only full-time professor in the program, I was the most visible faculty member. By 1978, I simultaneously had four dissertation students, Andy Seila, now retired from the University of Georgia, Veena Adlakha, now on the faculty of the University of Baltimore, Bao-Sheng Huang who went to work for Bell Labs, and Lou Moore, now at the RAND Corporation. Since each had a different thesis topic, on any one day I could easily start talking about the wrong problem with a student because I was thinking about one of the other problems. [Laughter.] Although I found simultaneously advising four theses demanding, I look back fondly on the experience.

In the 1980s and 1990s, I again had a collection of good students, including Ken Risko, now at Deloitte & Touche L.L.P., Tien-Yi Shaw, now at SAS, Christos, now on the faculty at Georgia Tech, Steve Yarberry, now at Practice Plus/Arkansas Health Group, and Cristina Arguelles Tasker, who is now at i2 Technologies in London.

4 The Future of Simulation and Operations Research

In this portion of the interview, George discusses potential future research directions for the field of simulation and offers an assessment on the status and future of operations research in academia and industry.

C: Well, that brings us to probably the last two topics. The first concerns the future of simulation. Where do we stand as a research community compared to other communities, with regard to two streams: the modeling side and the theoretical side. Any comments?

G: The modeling area of discrete-event simulation is essentially cast in concrete, principally because of the substantial investment that's been made in existing proprietary software.

J: What about Petri nets or event graphs?

G: Although Petri nets have been around for some time, they have not become a central concept. There may be room for modeling using network formats, but recall that a well-established software exists for some network problems. Some of you may remember the network program called SPICE, a creation of the 1980s. One would have to go up against well-entrenched software to motivate people to consider new concepts.

J: In fact, SPICE is still very heavily used by chemical engineers everywhere.

G: Right. Many of its users have no idea of its internal structure. [Laughter.] The same is true of much of the proprietary discrete-event simulation packages. I'm saying that it's very hard to gain acceptance for new modeling ideas.

With regard to statistical methodology, the picture is mixed. For example, many people thought that the regenerative approach was going to change how the statistical analysis of discrete-event simulation output would be conducted. But anyone who experimented with the method early on realized that was not to be, especially in highly congested systems where the regeneration period got very long. Although there were proposals to increase the frequency of regeneration, discrete-event simulation often requires one to maintain the fidelity of the local rules at each point on a sample path, a limitation to increasing regeneration frequency. However, other uses of Monte Carlo do not impose that requirement. The idea is to just to come up with an end result. There are many ways to do this. A paper by Brockwell and Kadane describes how to induce more frequent regenerations. They also give an example of its use. It's an interesting approach and in certain respects is different from earlier attempts. It relies on adding an extra state to the system, and to make use of that state in a particular way. Essentially you are dealing with an augmented chain.

J: An old trick.

G: I've written a set of notes on this method and other new methodologies but have no idea what I'm ever going to do with them. A good idea is not enough to have an impact on either discrete-event simulation or Monte Carlo, more generally. You have to make a concept or idea implementable to get it adopted. This attitude was justifiable in the past and even more so now. Today we have considerably more capacity than in the past for expeditiously translating ideas into usable products and testing them.

C: As a summary, do you believe that there is a good future for the statistical side of the simulation community?

G: Only if simulation methodologists broaden the problem set on which they work! For the last twenty years, we have been working to the point of diminishing returns. You may not want to hear that but unfortunately that's the truth. It's harder to get something new in these areas implemented today because the off-the-shelf products that are often available do a reasonably adequate job. Certainly in random number generation, we now have at least one random number generator that's equidistributed in 624 dimensions, a world apart from where we started years ago. That's been a big contribution.

J: What about the larger future of operations research itself, not just the field of simulation? What's your take on the future of OR as a discipline?

G: As a discipline, OR receives less visibility today than it did thirty years ago. At universities, it's been merged with other programs. I cannot explain why. To a very great extent, business schools have abandoned OR or at least incorporated it into their multifaceted quantitative methods courses. Some have eliminated their quantitative methods courses, replacing them with hands-on experience on a computer.

In engineering circles, there are specific classes of problems that rely on OR techniques. There's a healthy respect across engineering disciplines for what OR can do, provided it's oriented towards their problems. As an overall area of methodology, OR doesn't seem to have the visibility that we'd all like it to have. That's certainly true of simulation methodology. Many of the developments of the last forty years in discrete-event simulation hardly, if ever, get acknowledged, particularly in computer science and statistics.

C: Well, this brings me to the last question that I had, which returns to the status and visibility of the simulation community. When you talk to people in statistics or stochastics, they tell you that simulation is an applications area. For instance, statisticians will tell you that batch means or other output analysis methods are simply L_2 estimation. You talk to computer scientists, and they tell you that you are doing statistics. We're right in the middle, and I'm very concerned about what we need to do to shake this perception.

G: We're in the middle because different people have carved out sections of what was once our discipline. They often have good ideas and make important contributions. But there's still room for new OR-related ideas to play a role. The problem that I'm looking at now—counting using simulated annealing—is an example. The computer science approach focuses on complexity rather than the intrinsic opportunities within the problem for devising a near optimal sampling plan. More of a focus on OR may well lead to more appealing solutions.

So Jim, to answer your question about the profession, I think OR, regrettably, has not received the recognition that it deserves. Certainly it's realized its potential in some areas. The names of several OR techniques have become common "household terms" so that we and others no longer assign authorship or provide citations to many of them.

I think the professional societies have tried to do something about it, but I wonder how successful it's been. It's been pointed out to me that the way to judge the success of a profession is by how good the salaries are that its students receive when they enter professional life. By that standard, I think all is fine. [Laughter.]

D: How do we compare to other engineers?

J: Industrial engineers and OR types compare very favorably to civil engineers, for example, and electrical engineers. I don't really know about chemical engineers.

G: Industrial engineers, for many years, were at the top.

J: Well, they are, certainly, in terms of starting salaries, at least at NC State. They compare very favorably against almost all disciplines, including computer science, interestingly enough.

D: Not at Georgia Tech.

G: Computer science has basically held its own to a great extent in terms of what they can command, and resources they demand when they go to universities as a prize for coming there.

D: Although, apparently, computer science degrees have gone down a little bit in the last few years; I don't know why.

J: Not just a little bit.

C: Well, we're getting towards the end. Let me just ask a question. You've had a distinguished career. It's a fact. Going back, is there anything you would've done different?

G: I've been very fortunate in as much as a lot of good things came my way. I happened to have been in places where I could benefit in one way or another from contact with many accomplished people. My experience has been more favorable than others I know who didn't have the same good luck.

In terms of what I would've done differently, there were times here at UNC that I wish I had done things differently with regard to the OR program. Perhaps I should have encouraged a different academic emphasis for the program. I focused on becoming a highly methodological department at a point in time when there was a major shift towards PCs and hands-on work, and we didn't make that transition as rapidly as we perhaps should have in many areas. We didn't have any part of the manufacturing activity of the 1980s. We didn't have any part of the financial modeling of the 1990s. If I were to do things again, I would try to reconfigure the faculty into a form that would've allowed us to move more easily into those and other applied areas. Not having done so eventually became a limitation for us in terms of what our reputation was and our ability to attract people.

J: I've got one last question that I'd like to ask. What sort of advice would you give to people pursuing careers in operations research and simulation? Do you have a set of principles that you'd offer someone to bear in mind in pursuing a career in that area?

G: I am not capable of doing that. To my mind, my career was a series of good opportunities. It was partly—I have no illusions about it—attributable to the times. I was graduated from college right after Sputnik. Funding had grown considerably for research. Suddenly, the concept of a son doing operations research bordered on having a son that was a scientist, a doctor, or a lawyer. I'm sure your mothers would appreciate that. [Laughter.] So that made my life easy. There was a demand for what I wanted to do. Nevertheless, there were times that I had doubts. My first day of academic life began with a memo from the president of Yale saying, because of a budget deficit, he was freezing salaries. [Laughter.] It soon became apparent that budgets were universally tight across all academic institutions, including places like RAND. Therefore, I stuck with it, and the situation improved.

Today university life is not as I found it when I began. It has a much higher level of accountability. I can't speak for all disciplines, but from what I've seen in the mathematical sciences at UNC and other places, there are many more demands placed on faculty, and especially junior faculty. Regrettably, the junior faculty today have little awareness of the lower level of accountability of the past, and so they don't know what they're missing. You are all old enough to appreciate that difference. It's lamentable, but the truth is that future university life will continue to diverge from my experience.

I don't think I would necessarily encourage a young person to go into academia. Nor would I discourage them. I've had this debate with several people in the department who have encouraged students who seem perfect for academic life to go into it, although the student is not inclined to. I don't think that faculty encouragement serves students well. It overlooks the fact that students may have a considerably better perspective on what's right for them. They observe departmental and university governance which are much different today than when we first came to academia. I encourage students to keep their eyes open.

C: Let me end this conversation as the former student. George, it was an honor for us to have you speak with us. You and I have had parts of this conversation several times, but it was the first time I was able to get the whole nine yards. Due to the digital recording, it will live for eternity. So it was wonderful. Thank you so much for the hospitality.

G: Well, I'm glad you enjoyed it.