Matthew J. Sobel Interview by Paul Zipkin, October 23, 2017, Houston, TX

Early Life and College

PAUL ZIPKIN: Hello I'm Paul Zipkin. I'm professor emeritus at Duke University, and I'm here to interview Matt Sobel, who it so happens, was my PhD advisor at Yale. I graduated in 1977.

Matt, maybe a good place to start would be tell us a little bit about your background, where you grew up, and so forth.

MATTHEW SOBEL: I grew up in New York City, in the Bronx. My parents were immigrants. My father's case, his parents, were immigrants, just before he was born. In both cases from Eastern Europe. Our home was modest, my parents were poor as children. We were, I suppose lower-middle class, but everybody in our neighborhood was the same.

As a kid I did well in school, it was New York City public schools. I was very fortunate, to go to a very good high school, the Bronx High School of Science. I was good in just about everything. I particularly relished the writing courses, and literature, and physics. What really enthralled me was Euclidean geometry. Like so many of us I think, it was the first time that I encountered mathematics in a rigorous way. And I thought it was beautiful.

I did the things that kids do, from pranks to sandlot baseball, and at least in New York City, stick-ball. I enjoyed urban sports, particularly handball, and boy scouting, and did stuff that kids do as adolescents.

PAUL ZIPKIN: Tell me about your college years.

MATTHEW SOBEL: I went to Columbia University, first, for 3 years to Columbia College, and then to the School of Engineering. I particularly wanted both liberal arts and engineering. At the time, Columbia's engineering school only had the last 2 years of an undergraduate education, and you needed to spend at least 3 years in a liberal arts college.

Columbia had arrangements with about 20 such undergraduate colleges. One of those was Columbia College, and, that's where I went. It seemed to me to be an obvious combination. My parents certainly could not afford for me to go away to school, and so I commuted and learned to concentrate, because I spent about 3 hours a day commuting, in crowded buses and subways, and I could not afford to waste those 3 hours. I learned to basically blot out the rest of the world for those 3 hours and generally not miss my stop. But sometimes I did. There wasn't much time for extra-curricular activities because of the commuting time.

Even so, I invested myself in the amateur radio club, and in the handball team, and in my studies. During that time, I would say that I was extraordinarily fortunate, in many ways. In particular, vis-a-vis operations research, it turns out that during the 1st or 2nd year, my randomly (I assume

randomly) assigned advisor was George Kimball, after whom the INFORMS Kimball Prize was named.

Kimball was an early pioneer in operations research, co-author of the classic book with Phil Morse. He never mentioned operations research to me, at least I don't recall his doing so. A little later, I took a math course from B.O. Koopman, after which the military section of INFORMS' Koopman Prize is named. Koopman never mentioned operations research, or search theory, but he was a grand instructor.

A little later I encountered Cyrus Derman, who played a major part in my choice of direction; that's an entire story.

Paul, shall I talk about Cy at this point?

PAUL ZIPKIN: Sure

MATTHEW SOBEL: OK, so Cy Derman. First of all I was very young, I was a college student at 16, and I didn't have a clear idea of the direction I wanted professionally. I didn't have a clear idea of how to meld myself, as an individual, in a larger sense. And, I loved academics, I loved classical music, loved athletics.

Cy was an extraordinary man. He could have gone to Curtis Institute as a violinist. He was a great tennis player and he was gentle, candid, honest, and ethical. It was an important combination to me, and I used him very consciously, as one of the people after whom I wanted to model myself. He also thought very clearly. He made important contributions to the foundations of Markov Decision Processes.

I envied all of that, I envied all of that. It came at a time when I was, as I say, trying to figure out up from down. Also, his graduate assistant was Pete (Arthur F.) Veinott, Jr. And Pete was someone who figures subsequently in my doctoral education, so all of this was important to me. I was very lucky as an undergraduate. I should say, that I was not uniformly skillful. I ultimately chose engineering, in particular industrial engineering. But that's besides the point.

In that era-- and this may be strange to people who've been educated in engineering more recently-- every undergraduate engineer took engineering design, I guess it was the kinematics of machine design, and you needed to do engineering drawing, and this wasn't computer aided design. You use T-squares and triangles and India ink, and I was extraordinarily good at getting the India ink to run under the triangle and T-square. Other than that I did pretty well.

First Jobs at Western Electric and the US Public Health Service

PAUL ZIPKIN: So, I understand that after you graduated Columbia, you then worked for a while at Western Electric, and at the US Public Health Service. Why don't you tell us about those experiences?

MATTHEW SOBEL: Yeah. Western Electric Company was the manufacturing subsidiary of AT&T. Now in that era, and indeed until a court order in 1983, AT&T controlled telephone communications in the US, handled maybe 75% to 80% of all of it. Western Electric Company did the manufacturing for all of that. In addition, at the time, it was the largest US defense contractor. So this was a big integrated manufacturing company. I was very fortunate in becoming a member of the headquarters corporate headquarters operations group, in the Wall Street area.

We were in the corporate controller's division. So the perspective, which was representative of industrial operations research in that era, was cost reduction. It was decades later that attention was paid to strategic importance, to revenue enhancement, to competitive effectiveness; it was cost reduction. In a period of about 2 and a half years, I was very lucky to experience a fairly broad diversity of industrial applications of operations research which were representative of that era.

At that point in time, I was married but didn't yet have any children, and so I was draft bait. Had I been drafted, it was pretty clear that I would have spent 2 years doing what to my eyes at the time would have been uninteresting pedestrian, applications of operations research in the Army. The really exciting military applications were not being done by draftees. And so in order to have some control over how I'd spend the next 2 years or so, I enlisted as an officer, as it turns out, in the US Public Health Service.

I was assigned to a project concerned with the planning of water quality improvement in the region of the estuary of the Delaware River, which includes Philadelphia, Camden, Wilmington. This was really exciting because, I had through boy scouting, through other activities, been an ardent amateur proponent of conservation, and improved environmental quality. But I didn't know very much about it professionally.

Here was an opportunity to spend several years that were both professionally rewarding and consistent with my values, and it was a grand experience. We were for the most part, a group of absurdly young people, fairly highly-trained educationally, with no career dependence on the Public Health Service. In retrospect we contributed to revolutionizing not only water quality planning, but regional environmental planning. The procedures we devised persist to this day.

This was around 1962, 1963. Some of what we did resulted in the first use of mathematical programming in regional water quality, or environmental quality planning. And, the effects are still felt globally, in terms of methods. They were the core of Supreme Court Master's Hearings, during the 1970's. We were going up some very powerful corporate interests and governmental interests, and it was very heady stuff, of which I'm still very proud.

Graduate Studies at Stanford

PAUL ZIPKIN: Very good. Now, as I understand, you then went to Stanford, for your graduate studies, right? Tell us about that.

MATTHEW SOBEL: Well, while I was still at Western Electric, and then the Public Health Service, I was doing a part-time master's degree at Columbia University, in Mathematical Statistics. At that time, there was a requirement of an essay, a master's essay, and Cy Derman was the supervisor of that essay. Cy, himself, had obtained his doctorate in Mathematical Statistics at Columbia. So, it was an opportunity for me to immerse myself in Markov Decision Processes, which I did.

Then, I went to Stanford, and who do I find, but Cy's graduate assistant, Pete Veinott, on the faculty. At that time, what I particularly wanted in a doctoral program was comfortable access to a diversity of disciplinary influences. And, I wanted a university that was very strong in the disciplines I foresaw as being germane to my interests: operations research obviously, industrial engineering, statistics, probability, economics.

Economics became a central interest because while in the Public Health Service, I realized that the creation of incentives, the fostering of incentives, were important means by which one could induce behavior, as opposed to mandating behavior. Among the policy-oriented sciences, economics was perhaps most developed in that direction. So that was the origin of my interest in economics.

At Stanford, by that time I had a first child, I was married, and so, I thought that it was important to control the pace of my doctoral education. I thought that I knew a fair amount, and probably most of the material in the required courses, even if I didn't have course backgrounds in the topic, based on the reading I had done while I was at Western Electric and the Public Health Service.

So I thought that the way to waive or justify waiving the required courses was by persuading my advisor, who was Gerry Lieberman, who subsequently supervised my doctoral dissertation, by persuading Gerry, that if I did well in advanced courses, which had the required courses as prerequisites, then the program should waive the required courses. Gerry, being quite comfortable with letting a student choose his own poison, said OK, and so I proceeded to take a boatload of courses, all of which were advanced (with one exception).

For about half a year, I didn't sleep. I mean I nearly died but I somehow survived. And by the end of that year, I was through with coursework, I was through with qualifying, comprehensive exams, and I was free to do really what I pleased. So I spent the 2nd year reading, and exploring research, in a broad diversity of directions. In retrospect, it was probably the most unconstrained research period of my life. I was reading and exploring research in inventory theory, in probability theory, in statistics, industrial engineering, in production scheduling, in many, many directions and it was heady stuff, and it was great. And I kept Gerry Lieberman abreast of what I was doing.

He used a deft touch, and I continued to do more or less as I pleased, but towards the end of that year, I felt keenly the importance of getting on with the dissertation, and converting some of the exploratory research I'd been doing to a finished product. The choice for me was kind of difficult because I felt that, on the one hand, I had this background in environmental quality, water quality in particular, but environmental quality, in which I had a nearly unique comparative advantage.

There really weren't any people yet, in that area, who had a combination of considerable immersion in the phenomena (immersion in water quality yes) and also a reasonably strong methodological background, and so that was an obvious potential direction in which to proceed.

Of course, I felt that, if I didn't do it, others would and that the cream would quickly be skimmed in that area. It seemed obvious to me that there were important worthwhile problems to be solved, and for societal reasons to be solved quickly.

On the other hand, as with any new context-driven area, the most important research is not going to be highly methodological. The major contributions are most likely to be in formulating, in creating canonical arrangements, and describing the research terrain, and making first-cut contributions, in some of those. That kind of research is not likely to be highly methodological, and probably not going to be of great training value to a neophyte researcher.

So for me the tradeoff was, to go in a direction that I thought would train me more strongly, or a direction that I thought might have a more immediate societal benefit, and which might not be there in 5 years. Of course, others would leap into the breach, if I didn't.

Ultimately I opted for the training direction, I opted to do a dissertation in production smoothing, production planning, broadly construed. That was great, it led me into several directions, which I continued to pursue throughout my career. As for the environmental research, the cream has yet to be skimmed. So, I was wrong in my fear.

There was another experience that I had while I was a doctoral student at Stanford that has continuing effects. And that is, when I first arrived, the operations research program was interdisciplinary. There were no physical facilities where doctoral students could hang their hats. So I wormed my way into a small building that housed the mathematical economists and their doctoral students, and it was great.

I thought that occasionally, I might actually inhale some air that Ken Arrow had exhaled. And if so, it could only be to my great advantage, and that was fine. So I rubbed elbows with doctoral students, who became notable economists, over their careers, and with Ken Arrow, and others. Professor Arrow suggested to 2 of us, to David Bradford and me, that we look at a particular problem, a paper in structural unemployment, and both Dave and I decided that it was fascinating, it was interesting, but it invited, it naturally demanded, in our view, an approach for which David and I both felt we were unready. So David went on to do a dissertation on a different subject. He became a very distinguished scholar in public economics at Princeton. And I went on to do my production planning stuff, and queueing optimization stuff.

A couple of years later, I did feel ready to attack the methodological underpinnings that in my view were an appropriate basis for the problem area that Professor Arrow had pointed to. In my eyes that was stochastic games, or sequential games, or dynamic games. So, around 1969, I wrote the first of what has turned out to be a continuing series of papers, right up to the present time, on that subject and its applications.

So you can see that I was very, very lucky in the experience I had as a doctoral student. I haven't talked about the faculty or the other doctoral students, all of whom played a major role in, very positive roles, in my experience then with continuing effects.

First Academic Position at Yale

PAUL ZIPKIN: Great. So why don't you take us through your subsequent career?

MATTHEW SOBEL: OK.

PAUL ZIPKIN: Briefly.

MATTHEW SOBEL: OK, so Stanford begat Yale, Yale begat Georgia Institute of Technology. Well, when I went to Yale, it was at the same time that Harvey Wagner, who figures very importantly in my professional life, went from the faculty at Stanford to the faculty at Yale. And also, Don Topkis, who was in my cohort as a doctoral student at Stanford, went to Yale. The 3 of us arrived at the same time.

Yale for me was an extraordinary experience. I could not at the time nor can I now, envision an environment which for my interests at that time would have been better. It was into a department which housed operations research people, and organizational behavior people. With Martin Shubik as somebody in the middle in certain ways, although Martin also had a very strong foundation in economics and, organizationally, in the Cowles Foundation.

Now, my interests were interdisciplinary; by that time they were pretty strong in what we would now think of as operations management, in methodology of operations research, in environmental planning, in economics, in certain areas of economics, in probability and statistics. That's a really bizarre combination, if you look at a narrowly defined OR department or OM department, or business school.

Even at that time, there weren't many universities and departments where a new assistant professor, quite vulnerable in terms of career, would have been made to feel comfortable. Either I was too thick-skinned to notice warnings and signals or none were given, and none were intended. I did my thing. The doctoral students were superb, you among them. Our careers, and I now include you in this, our careers are mélanges of research, of teaching, of project work, of societal intervention, of intervention and facilitation, in the business of the university broadly construed, of the communities in which we live, and we spend a lot of time and emotion in doing all of this.

At least for me, it was important to feel mellow in the way in which those environments, or at least for the most part, those environments regarded us collectively. I spent a lot of time with and thinking about my doctoral students, including you. One of the enormous gratifications was how much I learned from nearly all of you. It was exciting stuff, still is. You were all on the make, as was I, but we were on the make in the most constructive way. We were trying to do exciting things, we were trying to discover stuff that mattered. And look, we all need arrogance to do this, because we're all working in domains in which particularly after decades of past scores,

hundreds, thousands of people, over centuries in some instances, have been working hard. Some of these folks are awfully bright, were awfully bright. We have the arrogance to think that we're going to do something of value, and the same kind of remark could be made about societal interventions.

Arrogant people can sometimes be a handful. I have to say that my doctoral students weren't. They were a pleasure, an unalloyed pleasure, and still are. The same thing was true of many of my colleagues. I mentioned Harvey Wagner. I mentioned Martin Shubik.

Harvey, who died earlier this year, was a very close friend, for 50 years. Martin, who's 91, is still a close friend. Martin is cranking like crazy. Martin knocked off a very nice monograph last year, and several papers; what a motivation. Martin and I have been much closer intellectually than you would guess from the fact that we've only coauthored, I think it is 3 papers. But, I hope that Martin has learned from me 10% of what I've learned from him.

Martin's interests are extremely diverse, in a disciplinary sense, in a societal sense, and he's a marvel, he continues to be. He's a poet as well by the way.

PAUL ZIPKIN: I did not know that.

MATTHEW SOBEL: I'm sorry, have I answered your question?

PAUL ZIPKIN: So that was Yale.

MATTHEW SOBEL: Oh yes.

PAUL ZIPKIN: From there, you went to Georgia Tech.

MATTHEW SOBEL: Right, right.

PAUL ZIPKIN: Stony Brook.

MATTHEW SOBEL: Yeah.

PAUL ZIPKIN: And Case.

MATTHEW SOBEL: Yes, OK before we leave Yale, I should mention Eric Denardo. Eric, whom I met because he joined the Engineering Research Center at Western Electric while I was still at Western Electric, was recruited to Yale. And Eric is also a very good friend to this day. I should mention the enormous gift that Eric gave me, about 1969. I asked Eric to take a look at a draft of a paper I had written, and he did, and it was if a chicken in the barnyard had walked into red mud, and then walked across every line of that paper. He tore it to shreds! He really did. Now, I should tell you that I fancied myself a very good writer. It was very painful. It was extremely painful, what Eric did to me. And it was a gift beyond measure, gift beyond recompense. It was an extraordinary experience to have someone who had the candor, the thoughtfulness, the insight, to do what Eric did for me.

My family was growing; by that time I had 3 children. Whatever Yale was in that era, it was not a high paying institution. By the time I left Yale, I'd had, I don't know, something like 7 PhD students. Every single one of them, including you, had entry-level salaries which were almost twice what I was earning.

Yale, in that era, was trading on its reputation and felt it could continue to do so. And it would be unthinkable for anybody to leave Yale. In fact, it would reflect badly on anybody who had the poor judgment to choose to leave Yale. Well, that isn't exactly the kind of currency you can use to pay for shelter and food, to educate your children. So, I left Yale and went to Georgia Tech.

Academic Positions at Georgia Tech and Stony Brook

Georgia Tech, when I arrived, was not at all the kind of institution it's become overall now, or in operations research. And one of the very gratifying experiences at Georgia Tech was to be a prime mover in the unification of the collective efforts in the College of Management, the School of Industrial and Systems Engineering, and the School of Applied Mathematics. By the time I left, Georgia Tech was very different institutionally overall, and in particular, in operations research. It was well on its way to becoming one of the dominant institutions in our field that it is today.

My parents were still alive and living in New York City, and they had reached the age and stage where their deteriorating condition really demanded some assistance. I have a younger brother who was living in California, and was not as mobile as I was. I thought that by going to Stony Brook, I would be able to have one advantage that Georgia Tech lacked. In that era Georgia Tech still was manifestly a technological institute, not a university. I thought that Stony Brook was building, and I could become part of the process of building, a superb group in operations research and in mathematical economics. I thought that being within an hour of my parents would permit me to give them the assistance that I thought they needed, and also to make, basically a trade at par, in what I had at Georgia Tech, for what I'd have at Stony Brook.

For a good period of time, that worked very well. And, part of what happened at Stony Brook was that, for the first time, I saw how the combination of ambitious development of a public university, together with vicissitudes at the state level could lead to abrupt changes within a particular state university, and that happened at Stony Brook.

I was at that time sufficiently well-insulated that I could have retreated into a figurative cave, rolled a boulder in front of that cave, and finished out my career, being essentially by myself, doing my thing. I've always felt that for me, academia was a highly social occupation, and that just didn't suit me. I thought that, then and now, a large part of my satisfaction came from immersing myself in a group of people who, as I said before, are on the make in the best sense, and are engaged individually and collectively, collaboratively, interactively, in doing, or aspiring to do great stuff. It didn't seem likely that Stony Brook would be like that for the next decade or more, and so I looked around.

Moving to Case Western Reserve University

I'd had an opportunity to go to Case a couple of decades before, indeed when I moved to Georgia Tech. At that time it looked as if Northeastern Ohio was going to slide into Lake Erie in a cloud of rust. The way in which it prevented that is actually a magnificent saga of regional transformation. But I won't dwell on that now. By the mid 90's, the industrial decline of Cleveland had halted and indeed been reversed. And, for my interests at the time, that was a wonderful place to go. The institution had, and has, a very strong commitment to blurring formal organizational boundaries.

My interdisciplinary interests were well-served there, with one exception, the engineering side, for my interests in engineering were not well served there. But everything else was. My recreational interests: Cleveland is one of the world centers for symphonic music. My recreational interests in road bicycling and cross-country skiing- well served there. I moved and I've had a ball.

From Whales to Elephants

PAUL ZIPKIN: Great. Different type of question.

MATTHEW SOBEL: Yeah.

PAUL ZIPKIN: What would you say are your favorite areas of work that you've participated in?

MATTHEW SOBEL: [LAUGHING]

MATTHEW SOBEL: There used to be a song: if I can't be near the girl I love, I love the girl I'm near. So the analogy, I don't know if it's apt. The analogy is, as I think you can see, I revel in commitment, I revel in collaboration, and I look for it. I revel in being ambitious for accomplishing ends of enduring value. Not that I necessarily fancy myself as achieving that all the time, but I think that the aspiration and the purposeful endeavor, is what, with others, is what I find particularly exciting. And so, I've found that all along, as a neophyte I found that at Western Electric.

In other ways as a neophyte, I found that in the Public Health Service. I found it as a graduate student. I found it at Yale. I found it wherever I was. And I've also found it in the context-driven research I've done, in the organizational work that I've done, in the methodological research work that I've done. Although the world may not think of Paul Zipkin as a math programmer, it was exciting to me, that you, as a doctoral student, were doing lovely work in math programming, aggregation in mathematical programming. And ultimately, we, you and I, found ways to reflect that, in environmental planning, elephant management.

PAUL ZIPKIN: Talk about the elephants!

MATTHEW SOBEL: Yeah, right.

PAUL ZIPKIN: Tell about the elephants, Matt!

MATTHEW SOBEL: [LAUGHING]

MATTHEW SOBEL: OK, OK so one of the people, with whom I've done collaborative work in environmental planning and ecology, and research, and ecology for 50 years, is a very prominent ecologist named Daniel Botkin. Dan, by that point, was on the staff of the Marine Biological Laboratory on Cape Cod. Dan's interests are not just in marine ecology, they're much broader. But Dan saw that a natural way to express his interests, in a way that was organizationally relevant at the time, was to focus on marine mammals, whales.

So, Dan and I were working on modeling marine mammal, and the problem then as now, not just with whales, but with marine resources, biological resources of many kinds, is to balance in some sense, the extraction, the exploitation really, of those resources for the benefits of people who depend on them on the one hand, with, on the other, sustaining the biological resource. So the question arose: how do you harvest these resources, in this case whales, while not destroying the resource?

And, that's the kind of tradeoff problem, on which OR thrives, right? But, one of the devilish issues, with marine resources, whales, fishes, is that age structure matters. So, we encounter the well-known curse of dimensionality. If you're going to build a population dynamics model, where the state is a vector with components for the number, or the biomasses, at each age class, and then you discretize each of those components, you have a nightmare computationally, and you can't get anywhere.

So what you want to do, in that era, is to use a very coarse discretization. And the question arises, how coarse of a discretization can you use, while still being able to glean the essential policy insights? That was a natural connector to your research on aggregation in linear programming. And I might add, to sort of jump from then to right now, this year, a colleague, Jie Ning and I, have completed research that provides some answers to the question of how you can build these models of biological resource management without having to do that discretization.

PAUL ZIPKIN: So how do you get from whales, to elephants?

MATTHEW SOBEL: I think one answer is a couple of million years of evolution, right? But seriously, we were trying to do models of the population dynamics, and the management of whales. But in building models of population dynamics of any population, particularly of mammals, you need to know basic stuff, like the age at which lactation occurs, the length of time of a pregnancy.

Until that time, important assumptions were based on counting the rings in the bones of the ear of a whale. Like tree rings. And I don't remember any longer whether a ring was treated as 6 months, or 1 year, or 2 years. But, while we were doing this it turned out that we were off by a factor of 2. That a ring meant 2 years instead of 6 months, or 6 months instead of 2 years, some enormously sensitive parameter was all wrong. And also, you really weren't able to go out and put a tag on a whale, and track its movements.

So then the question was, what are some populations, which are large mammals, about which we know much more, and about which we can collect data? Elephants, being the largest land mammals, were such a population. And that's how we went from whales to elephants. I can tell you that, at that time, my son, the oldest of my children, was already in elementary school, and when asked in school what his father had been doing the previous summer, said that he was studying elephant management at the Marine Biological Laboratory on Cape Cod. And he said that even at his young age, it was very difficult to say that without collapsing in laughter. [LAUGHING]

Other Favorite Research Topics

PAUL ZIPKIN: So that's one of your favorites.

MATTHEW SOBEL: Yeah, that's one of my favorites.

PAUL ZIPKIN: What else?

MATTHEW SOBEL: OK, since I mentioned Dan Botkin, in the late 1970's, early 1980's, we wrote a few papers on the stability of ecosystems. At the time, there was a widespread notion of a balance of nature that was entirely static. It turned out that that notion is not consistent with observed dynamics in many, many natural settings. We wrote a paper on that, arguing for notions of stability that were consistent with notions of stability in stochastic processes. Those papers have had a profound influence, and I am still pleased with those papers.

Going in a very different direction, the work on stochastic games led to the first paper, with Alan Kirman, on using stochastic games as a framework for analyzing dynamic oligopoly. That has become the standard paradigm now in the economics of industrial organization. Yet another direction, is that when you look at the literature, the research, and applications in dynamic models with uncertainty, the overwhelming majority of it is rooted in a notion of risk insensitivity, risk neutrality.

And yet, in all my experience with people who are in particular context, nobody is insensitive to risk. The way in which they'll express their sensitivity depends on their background, their sense, their education. They talk about eliminating some of the downside risk, even if it means sacrificing what they can get on average, or they talk about mean-variance tradeoffs, but in some way they will show very, very tangibly, that they aren't risk neutral. And none of that, at the time, and now I'm speaking in the 70's and the 80's, was reflected in what we knew about how to do stochastic optimization.

So, I wrote on that topic a little bit, had some doctoral students work on it a little bit, and it dawned on me that the foundations for all of this were very incomplete. There were the very well-known works in utility theory, which focused on risk sensitivity. But what we were dealing with, was a conjunction of risk and time. So the question arose, what is a reasonable framework, a canonical form for making tradeoffs that involve both timing and risk? It just wasn't there.

I'm particularly pleased with some work that I did starting in the 1980's, which was work that was very unpopular. People in preference theory, people in mathematical economics, in game theory, couldn't believe that this was an issue that hadn't been resolved many decades ago. So, it was only published within the last decade, but I'm very pleased with that. That's among the work that has given me great satisfaction.

Evolution of Operations Research

PAUL ZIPKIN: OK, here's a big question.

MATTHEW SOBEL: Yeah

PAUL ZIPKIN: How have you seen the field change over the years, since you first got involved in the 1960's? And, where do you think it is going? And, where do you think it should go?

MATTHEW SOBEL: [LAUGHING] In the beginning, operations research was a label for a fusion, a sociological phenomenon, bringing together people with diverse disciplinary backgrounds, and placing them in the middle of a context-driven issue, or combination of issues, and holding them accountable for making good things happen in that context. Out of that, this was during World War II, immediately afterwards, in such efforts one saw the origins of the methodologies which have now become the principal methodologies in operations research and in operations management. Those are largely the same, but not entirely.

What happened, first of all, was a formalization. I realize now, not as a student but as having had organizational experience, faculty member experience, that people in organizations who make careers need protection, you need to protect students. In organizations, one of the important ways you protect people is by getting budgets. By making sure that by mandating rules of the game, a proper treatment of people in the organization, extend to those people who are vulnerable. But with that kind of formalization, you have an unavoidable loss, which is created by the acknowledgment of the boundaries.

Different universities, different consulting groups, different companies, differ in how aggressively they suppress the awareness of those boundaries, but they're always there. What happened in our field, was that operations research became more and more synonymous with the particular techniques that were created, rather than with the process of bringing together diverse methods for context-driven work.

Then what happened was that within operations research you saw largely a bifurcation. Trifurcation? Math programming was one group, stochastic models, broadly construed, another group. It was no longer the case that individuals in operations research held themselves accountable for being very broadly, as well as deeply, informed across the breadth of the field. At the same time, game theory was sort of blessed, but let go. Game theory left (it remained connected to mathematics), but it evolved, largely transmogrified, into economics.

So operations research for a while was just math programming, stochastic models. At the same time, people in context-driven work, particularly operations, what we think now of as operations

management, were frustrated by the absence of attention, serious committed attention, to the context-driven issues, arising in operations. So they went off their own way, and dismissed operations research, many of them.

It was a disaster in my opinion. What you had was the loss of the intellectual foundation of operations management, and the loss of much of the context-driven motivation in operations research, a bifurcation that was methodological. Well, such a state of disaster couldn't persist fortunately, and what happened was that one saw mostly a renewal of the awareness, in operations management, that operations research had a lot to offer. And, in operations research that there were these fascinating, important issues in operations management. And so, that kind of splintering is far less apparent now, and I think that's all for the better.

But where is the field going? OK, at its roots, what does operations research have to offer? It has to offer a context-driven approach, data analytic, data driven, information driven, approaches. Identification of constraints, make explicit the objectives, as well as the constraints. Identify explicitly the levers, the measures that can be taken, to try to formalize that, use computers, acknowledge uncertainty, acknowledge risk. OK, does that identify only operations research? Of course not.

In the 1950's, it might have; maybe even in the 1960's. Now, you name the field that has a prescriptive arm, any of the policy sciences, any area in engineering, civil engineering, electrical engineering, economics, political science, sociology, drama, forestry.

At one time at Yale, Paul, while you were still there, introduction to linear program was being taught in one way or another, in 12 different departments of the university, including the drama school, and forestry, and on, and on, and on. You don't have to go to operations research any longer to find methodological expertise, to find a knowledgeable commitment to the foundations of operations research. You don't have to go to operations research to find people who are extraordinary creative contributors, and users of queueing theory. You can go to computer science, you can go to electrical engineering, you can go to math, or you can go on, and on.

The point here, is that the touchstones of what we were, and are, are no longer our specialty alone. In fact, even within business schools, certainly it's there in marketing, it's there in information systems, it's there in finance, and in great measure and so on. It's there in accounting, in some schools, it's there in HR in some schools, so organizational behavior. So, what have we to offer that's our comparative advantage? I think that we've won the war, we really have won the war. Whether we can survive in the peace, remains to be seen.

I think that INFORMS has done a splendid job, organizationally, in preserving a framework for us. But that's, I think, a reflection of organizational leadership. We have an extraordinary array of journals, if we think of research. But in terms of the larger society, I really don't see why we should persist, should in the descriptive sense. Obviously, for my interest, for yours, for that of all of us who have allegiances, sure it should persist, but in a societal sense, I don't see a clear argument for its persistence. On the other hand, I've had this opinion for 20 years, 30 years. And so, it could be said that the reports of the death of OR may be highly exaggerated.
[LAUGHTER]

PAUL ZIPKIN: I think that's probably a pretty good place to stop. I hope our listeners have gotten some sense of why I think this is the most inspiring person I've ever met. I'll leave it at that.

MATTHEW SOBEL: Thank you very much, Paul. It's been an honor to know you.

PAUL ZIPKIN: Likewise.