Ed_Kaplan Interview by Jim Cochran, November 5, 2018, Phoenix, AZ

Introduction

JIM COCHRAN: Welcome to a conversation with Ed Kaplan. It is November 5, 2018. My name is Jim Cochran, and it is my pleasure today to interview Ed Kaplan for INFORMS history and traditions series.

Ed, welcome to this interview.

ED KAPLAN: Thank you very much. It's a pleasure to be here.

JIM COCHRAN: This is a particularly good time to interview Ed. He's probably still basking in the glow over the Boston Red Sox crushing defeat of the Los Angeles Dodgers in the recent World Series.

Ed, the Red Sox went 86 years between World Series victories, and now they've won four World Series titles in 15 years. To what do you attribute this dramatic turn of events?

ED KAPLAN: It was clearly they were taking the advice of people who are specialists in sports analytics. And the question was, "How do you end an 86 year drought?" And the answer was, "When the season is over, win 11 games-- very simple advice." They followed it.

JIM COCHRAN: I've heard that you've been spotted various times in the stands at Fenway Park. One of your friends asked me to ask you if you're there to apply statistical reasoning to sports problems or if you're just there to devour peanuts in the shell?

ED KAPLAN: I think I can guess who asked that question, because he probably would be my seatmate. That was my former thesis advisor, Dick Larson, from MIT. When I was a graduate student at MIT, Dick and I would go to baseball games in Fenway Park, and we would indeed eat peanuts in the shell as a very sophisticated method for bringing success to the Red Sox.

This came after years of research, that they would perform better after we eat peanuts in the shell than if we didn't. One time, we were sitting at a game and there was a famous Boston player named Jim Rice, you probably would remember him.

He came up and Dick was sitting very, very calmly in the bleachers and he simply pointed and said he's going to hit a home right there. No exaggeration, just very matter of fact that he's going to hit a home run right there.

The people surrounding us in the bleachers, most of whom were drunk, were kind of going, yeah, yeah, yeah. Jim Rice comes up to bat. He hits a home run right there. And they went nuts-- psychic, psychic.

So anyway, peanuts in the shell has a long history with Dick and me.

JIM COCHRAN: Now that we have your historical perspective on Boston baseball, let's proceed with the task at hand. Ed Kaplan is the William and Marie A. Beach Professor of Operations Research, Professor of Public Health, and Professor of Engineering with the Yale School of Management.

He's the author of more than 125 research articles. He's also served as the director of the Law Policy and Ethics core and the Methodology and Biostatistics core for the Yale Center for Interdisciplinary Research on AIDS. Ed has received both INFORMS Lanchester Prize and Edelman Award, two top honors in the field of operations research, among his many other awards.

He's an elected member of both the National Academy of Medicine and the National Academy of Engineering of the US National Academies. He's twice received the prestigious Lady Davis Visiting Professorship at the Hebrew University of Jerusalem, where he's investigated AIDS policy issues facing the State of Israel.

His current research focuses on applications of operations research to problems in counterterrorism and Homeland Security. He has served INFORMS in several capacities, most recently as the institute's Member-in-Chief in 2016.

We're going to have a lot of fun today and learn a lot more about Ed's early life, his education, his career, his influences, and his outlook on the future of the discipline of operations research. We'll get started here.

Early Life

Ed, could you tell us a little bit about where you were born and where you grew up?

ED KAPLAN: They weren't the same place. I was born in Canyon, Texas. So you can tell I'm a Texan because I'm so tall and because of my southern accent. But at the ripe age of four, my family moved almost due north to Saskatoon, Saskatchewan, in Canada.

My father was a professor of music, and he had been teaching as an assistant professor at West Texas State University in Canyon, Texas, which I believe has now become part of the Texas A&M system, actually. But at the time, it was West Texas State University.

And we ended up in Saskatoon, Saskatchewan, Canada, and that is really where I would say I grew up. I grew up in Saskatoon.

JIM COCHRAN: Do you have siblings?

ED KAPLAN: I have an older brother, Jonathan, who lives in Jerusalem, Israel. And I have a younger sister, Sarah, who lives in Cornwall, Ontario, Canada.

JIM COCHRAN: What kind of influences did your parents have on your professional choices and your outlook on life?

ED KAPLAN: I can identify a few things. First of all, as I said, my father was a professor of music, which meant that he spent a lot of his time at the University. Many of the activities that he pursued there happened on the weekend.

For example, he conducted both the Junior and the Saskatoon Symphony Orchestra. And in those days, a lot of the rehearsing took place right on the campus of the University of Saskatchewan. The symphony hadn't moved into their own quarters yet.

And so, I would often come to the university with him on those Saturday mornings. And he would leave me in his office which was really a playground, if you were a youngster, because it had all these incredible musical instruments-- big ones, tubas, French horns, all of these brass instruments.

So while the orchestra next door was trying to rehearse, and I had not yet started to play any instruments, I would explore these things. I would pick up a trombone and go back and forth.

So anyway, the point I'm really trying to make, in spite of just having so much fun, is that as a kid was that universities at a very, very early age felt familiar to me, because I was there all the time.

It was every weekend I would basically spend in my father's office and hang out. And as I got a little bit older, I started to play an instrument myself. I played the violin for many years and became part of the Junior orchestra.

So I spent a lot of time at the university even when I was only you know, six, seven, eight, nine, ten years old. My mother was a psychologist at the-- a child psychologist-- in the university hospital there, and so she was also at the University a lot because the hospital was part of the university campus.

So that's one thing. Because I think being comfortable in any environment is going to make spending your future in a similar environment obviously attractive and doable. And so in some sense, I just became very comfortable in that environment very early in life.

The other thing I would say and I'm quite proud of this actually, I grew up in a Jewish household. I wouldn't say that we were the world's most religious or observant household, but the important values from tradition were clearly passed on at home.

And one thing I remember my mother saying to me when I was really quite young, was it really doesn't matter what it is you choose to do as you grow up, but what's important is that whatever you do, you try to leave the world in a little bit better shape than it was when you found it.

And this is actually something which is part of Jewish tradition. It's called *tikkun olam*, which from the Hebrew translates literally as repair the world. And that was kind of drilled into me from a very early age, and not only at home, but in Hebrew school.

So when I take these two things together, I hadn't really thought of it in a programmed way, but basically, on the one hand, I'm very comfortable in the universities, and on the other hand, there was clearly this advice coming from my parents and coming from my Hebrew school teachers and rabbi, whatever, of just not trying to go out and take over or anything like this, but just trying to make-- try and do things which are good for the world.

And I think those two things actually had a huge influence on me.

JIM COCHRAN: In elementary school, you think your teachers would have described you as a quiet kid or a mischievous kid or a troublemaker?

ED KAPLAN: In Hebrew school I was a troublemaker. My way of dealing with things that I didn't want to do was to basically have fun with it and sometimes that would go overboard and lead to being-- OK, we understand your idea of fun. Now let's go visit the principal's office and see if you appreciate our idea of fun.

But I was a pretty good kid. I didn't get into a whole lot of trouble. But certainly I don't think quiet would be how people would have described me either. I talked a lot. I still talk a lot. But engaged-- that's probably the best way I would say it.

Whatever it was that was happening, I tended to be-- if I was in it, I was engaged, I was into it. Otherwise, I would be totally tuned out and just doing something different.

JIM COCHRAN: Were there any particular subjects that you liked or disliked as you were growing up?

ED KAPLAN: You mean in elementary school, for example?

JIM COCHRAN: Yes.

ED KAPLAN: So first of all, I went to a public school. Hebrew school was an after hours affair. It was like three times a week for an hour and a half each time, either Mondays, Wednesdays, and Sundays or Tuesdays, Thursdays and Sundays. But regular school was regular school.

And, yeah, I did have subjects that I really enjoyed. I very much enjoyed mathematics. I very much enjoyed science. I very much enjoyed history. And it really wouldn't be until high school that you could say I was doing something that could be characterized as social science.

I was always very interested in current events. And then there were also some subjects that I didn't like as much. And in retrospect, I probably would be a better person if I had paid more attention to them, or tried a little harder at them.

I did not like art-- drawing, coloring. And the reason was really quite simple. I wasn't very good at it. I once had a drafting instructor in high school who told me that I was the first person he had met who drew warped stick figures.

So my skill at making images on paper was not great, and as a consequence, it was hard for me to get terribly excited about projects, classes that used that.

I actually was not very fond of English to the extent-- there are different kinds of English classes. There were English literature classes, which I love, where you would read plays or read stories. The English I didn't like was grammar. And days when I was in school, there was a lot of attention paid to proper sentence structure, proper punctuation.

And all of the rules were just presented as this endless flow of rules, which to this day I would-- I'm terrible. I can't conjugate properly-- none of that. It just seemed to me very arbitrary when what was more important was to be able to communicate and that meant being able to talk and being able to write.

And if the way you wrote and the way you spoke didn't exactly match the rules, so what? Now, there's one other thing that probably played into that, which-- I have had a hearing problem from very early in life, and so that was another issue.

Not being able to hear things properly sometimes led me not to say things properly. And sometimes my vocabulary would be a little bit more limited because I just wouldn't hear a lot of the words people were using but would put together the meaning from context.

So anyway, things that were sort of structured around formal English grammar, that wasn't really exciting to me. On the other hand, foreign languages I liked. When I was a kid, I took French. I took German. I did some Hebrew in Hebrew school. And that was more like a game, because I didn't really know what the language was, and it wasn't something that you could speak everyday where I was growing up, and so that became more of a game.

Geography and City Planning

JIM COCHRAN: So you graduated from high school, and you go to McGill, and you received your Bachelor of Arts degree in economic and urban geography in 1977. Was it a direct shot into economic and urban geography, or did you bounce around a little bit? How did you come across that as a--

ED KAPLAN: So to understand that we have to actually go back to high school, because in high school, I had a course in geography that actually stretched with the same instructor over three years. And part of what made this course so special was that there was a group of students, a small group of students, who were interested in really exploring the subject.

Now geography might sound like a strange place to get started, but actually, at least in the British tradition, geography is not simply, “What are the capital cities of the world?” “What is the length

of the Amazon River?" "What's the tallest mountain?" "What country is Mt. Kilimanjaro in?" Things like this.

Geography is actually a very serious social science. Why do people do what they do, where they do it? It's focused on the spatial organization of everything. And if people study physical geography, it's why does stuff in the natural world happen where it does and what are all the processes which make it go?

But in human geography, which was what I was interested in, again, it's why do people do what they do, where they do it? And in particular, I had a fascination with cities from when I was a kid.

My mother used to drive me around Saskatoon. And we would just get on a street and I would say I want to drive to wherever this street ends. And so, that's what we would do. And sometimes, the streets, actually, because this being Saskatchewan, would turn into a rural route, which meant that they would just go and go and go forever until we started to get the idea that, all right, this is just going to keep going. Fine, I get it. Let's go back.

So I was very interested in this. And while I was a high school student, we did several very interesting, what I would actually call research projects, obviously at a level which is appropriate for a high school student. But they involved collecting data. They involved forming scientific hypotheses, and actually doing it at a very elementary level, some basic modeling.

I did this project on why does the Saskatoon central business district look the way it does? And one of the things I did was walk downtown over a period of a few weeks by foot, measured out the actual frontage space of all the different businesses, took notice of the buildings, which ones were ten stories, five stories, three stories, and so on-- came up with a very good understanding of the layout of the downtown and was then able to go back and say so, what's the relationship between the density of buildings and distance from the center of the city, and things like this.

So anyway, where was all of this going? Well, at that point in my life, I wanted to be a city planner. I was kind of in love with the idea of cities.

Now granted, I lived in a very small one. At the time I was growing up, Saskatoon had maybe 120,000 people, or something like this. But it was a complete city in the sense that it had its downtown, it had its industrial areas, it had its residential areas, it had its Arts District, it had the university.

And when the city ended, it ended. You were in rural Saskatchewan. You had to drive 150-160 miles to come to another place of comparable size. And I would read about cities in other places-- Montreal, Toronto. You know, being Canadian, I would mainly see Canadian cities.

But of course, since I was American, I still had grandparents who were alive in Chicago, so we would go and we would visit. Anyway, so I was always fascinated with cities. So what I wanted to do was to become a city planner.

The way you become a city planner in Canada, or one of the ways you do it, is you take a course in urban geography. Another way to do it would be to go more through civil engineering, but I wanted to go the route through urban and economic geography. So I went to McGill very specifically with that major in mind.

JIM COCHRAN: You weren't involved in any way with the effort to bring the NHL to Saskatoon, were you?

ED KAPLAN: No, that happened later. When I was in Saskatoon, the hockey team was a senior hockey team, and it was called the Saskatoon Quakers. And you have to imagine the fear that must have struck opposing players to see the Quaker Oats gentlemen skating at high speed up and down the ice.

So the Saskatoon Quakers was the home team. Later, there was a junior team called the Saskatoon Blades, which for a long time was affiliated with the Los Angeles Kings. I mean, this came much later. And that team, the Saskatoon Blades, still exists, although I don't know who their NHL affiliate is. But actually trying to bring a real NHL team to Saskatoon, I was long gone before that happened.

JIM COCHRAN: So '77, you graduate, and you look around, and how do you decide what to do next?

ED KAPLAN: Well, I wanted to go to graduate school. I knew that. And my original idea was that it was going to be to a city planning program. So I actually was accepted to the University of British Columbia, to the University of Toronto, to leading Canadian schools of city planning.

And they made their admission decisions early. And it looked like I had settled in on the University of British Columbia. And actually, there was some very nice fellowships from the Central Mortgage and Housing Corporation, which is the Canadian equivalent of HUD, the Housing and Urban Development. They gave out a very attractive fellowship, and the university also had one.

When all of a sudden, in rapid succession, I received two phone calls from the two American programs I had applied to. I'd applied to the Graduate School of Design at Harvard, which is where urban planning was, and I had applied to the Department of Urban Studies and Planning at MIT.

On successive days I was accepted to both of these programs. Now, I had already told Vancouver that I was going to go there. And I talk to my advisors at McGill and said, you know, there's something that says, I really ought to go to MIT, but on the other hand, I kind of accepted UBC, so what's going on here? And they said, you know, you like math, and you like building models, and you like doing that much more than the design aspect of things.

Remember, I'm not very good at art. And they said, it's perfectly fine for you to accept MIT at this point. Why do you think the other offers came early in the first place? And I'm totally naive

about this. What did I know about strategies universities use to try to attract students to a campus?

Anyway, long story short, I reneged on going to the University of British Columbia. And I remember getting this letter from them. There is no email at that time, so everything's going back and forth by mail. Are you sure; you realize we have these two $10,000, which in those days, was a big deal. I said, no, I'm going to go to MIT, and so, I ended up going to MIT.

JIM COCHRAN: And you doubled down there. You simultaneously earned a Master of Science in Operations Research and a Master in City Planning, right?

ED KAPLAN: That's right, but it didn't start that way. It started with me being admitted to the city planning program, the Department of Urban Studies. And the degree I was going after was a Master in City Planning, or an MCP, although I am not a male chauvinist pig. But a lot of people hear the initials MCP, and they say, why would anybody advertise that?

But anyway, what happened was Dick Larson, the same person who ate peanuts in the shell with me-- although we are going out of chronological order here-- he also was cross appointed between electrical engineering and the Urban Studies department. And he was also the co-director at the MIT Operations Research Center.

Now at this time in my life, when I first got to MIT, I had no clue what operations research was. I had never heard the words before. However, Dick saw my application, and in my application I had said I wanted to use mathematics and modeling to try and help plan better cities. Basically, get these cities to function better.

And so, Dick calls me into his office. This is the famous first meeting. I had like one jacket, a brown corduroy jacket. So I dress up to come in to this meeting and he's not there.

But then all of a sudden, he comes in, and he had just been playing squash, and he comes in his ripped up shorts and a t-shirt. And his office had this big hammock in it. And so, he's all tired from playing squash, so he goes and jumps into the hammock and stretches out, and says all right, so let's talk about what we can do here. And it was just a crazy introduction.

But Dick, in short order, convinced me that I should think about urban services. And he had just-- I got there in 1977. Dick had just won the Lanchester Prize in 1972 for his book, Urban Police Patrol Analysis. So he gave it to me to read over the next couple of weeks-- this is in the summer, before classes actually started-- so I could decide whether or not this looked like something I wanted to do.

So I remember taking this book to the beach, of all places. And here is a book about policing that's loaded with mathematics-- chock full of mathematics, and mathematics that I didn't understand, by the way, because I hadn't had geometrical probability.

I didn't know anything about dynamic programming. I didn't know anything about optimization. These are all operations research subjects, which I had never studied. But the problems were fascinating.

How do you design a police beat? How do you decide how many officers should be scheduled? What's the probability of intercepting a random crime in progress? And all of these sorts of things-- search theory.

So I was just kind of sold. It wasn't applying models to the design of a city, it was rather applying models to the analysis and design of service systems, in this case policing. But the analogy became clear, there were lots of other problems it would work.

And so, it was at that point that I said aha. I'm going to start taking all these operations research courses. And that's what led to the joint degree. Actually, there's one other thing I should say, because there was one other thing from being an undergraduate at McGill that stands out in my mind. And I didn't have a chance to mention.

When I first got to McGill, as I said, I kind of liked math, but I was not in the math department. I was in the geography department. But I'm going through the course catalog at McGill, and there's a class, and it was called mathematical models and applications. That was the name of the class.

And the subject matter included the use of mathematics for everything from the laws of planetary motion, on the one hand, to traffic, to the spread of disease, and the spread of rumors, to voting models-- all of these things. However, in order to take this class, you had to have a course in differential equations, you had to have a course in linear algebra, you had to have a course in probability.

And again, I'm an incoming geography student. This is not stuff which is being taught. When I say I like math, I like seat-of-the-pants math. If I had to figure out an equation for something, I would just sit down and do it by hand, and draw the points, and literally do everything in about as old-fashioned a way as you could.

So since I really wanted to take this course and I had a limited number of electives within the geography program, I solved-- what I realize now in retrospect-- I solved my very first dynamic program, which is what-- my first dynamic program-- what do I have to do now, here I am in my first year, over the next couple of years in order to be able to take this class at the end?

And so I figured out a way to schedule in all these prerequisites so my senior year at McGill I was able to take this class, and I loved it. And I loved the modeling. And I loved the applications. And I wrote a senior thesis in geography that took these ideas-- and it was a model of travel time in Saskatoon and it tied into the geography really nicely.

So when I came to MIT, I was ready to do modeling, but I had no idea what the options were. And all of a sudden, Dick unfolds this whole world of operations research and it was-- kind of like my mouth dropped. I said this is it. This is it. This is what I want to do.

Graduate School at MIT

JIM COCHRAN: Was there any particular course in your studies at that point that really attracted you?

ED KAPLAN: Well there were several tremendous courses that I took at MIT, but I think the one which pulled it together the best was a course that was called urban operations research. And there's a book of the same name co-authored by Dick Larson with Amedeo Odoni.

And the way that course was delivered, actually, there were-- if memory serves-- four different instructors who sort of each did a quarter of the material. And the instructors who I had were-- I had Dick. I had Arnie Barnett. This is where I met Arnie Barnett for the first time.

Amedeo came in and did some of the teaching. And actually Oded Berman, who was finishing off his PhD and was a doctoral student there-- he was ready to graduate. Now Oded, of course, is a very well-known INFORMS member, operations researcher who is at the University of Toronto now.

But Oded taught a few courses on network modeling, graphs and networks. And so that was the class where there was just this direct one-to-one correspondence between these real problems in the urban services domain and the ability to build models. And so, that had a huge effect on me.

There were other just purely methods courses, which of course, mattered. The probability course that I took at MIT from Al Drake, who passed away several years ago, but was just one of the most exciting courses I think anyone could ever design. It's probability taught from a very, very physical point of view.

There are people who would take probability in the mathematics department, they would learn many more theorems. They would learn more advanced mathematical tools, which would then be used, say, to prove theorems in statistics. But those same people would have very difficult times solving the kinds of problems that were in the Al Drake book because his emphasis was on understanding the physical situation.

What's the sample space? What exactly is the event you're trying to find the probability of? And from there, moving into random variables in stochastic processes and everything else. So that was a very important course to me because it made it possible to do the other stuff.

I actually took these classes out of sequence. I took the urban operations research course before I took the probability class. And it was when I took the urban operations course that I realized I really don't know enough probability. Whatever I had taken back at McGill just wasn't enough. It was more of a statistics variety probability course.

So anyway, those classes really stood out. And I took stochastic processes with Arnie Barnett, and of course, that mattered too. And then there were all the other courses which I took there, but I would say in terms of my own development, those were really the key classes-- the stochastic-- the urban operations research course and probability.

JIM COCHRAN: Then you moved into the PhD program, right?

ED KAPLAN: Yes.

JIM COCHRAN: And while you're in the PhD program, you actually earned another master's degree, a Master of Science in math with a focus on statistics, right?

ED KAPLAN: That's right. At the time, Dick Larson would refer to me as a master of all trades and a doctor of none. But I ended up getting this Master's Degree in statistics for two-- first of all, I did this sort of detour into statistics, because what I was really interested in, as I said, was using operations research ideas, at this point, for urban problems.

But more generally, I would say, I was now broadening out. What about societal problems? Or urban problems that are not immediately recognizable as emergency services. So things like public health or things-- public housing, excuse me, not public health--public housing, at the time, or things that were not so logistical in nature, but nonetheless were very important problems.

And I realized that economics and statistics was really the currency of people who worked on these kinds of problems. There's a lot of people who would do analyses, either experiments or observational data, and they would argue with economic models and with statistics.

So at a minimum, I figured I needed to understand this stuff, and so I started taking some courses. And one thing leads to another, and I'm taking a course specializing in categorical data analysis, or a course in time series, a course in multivariate analysis, where I encountered Herman Chernoff.

Herman Chernoff-- very famous statistician-- was in the math department at MIT. And Herman was looking at me one day, realizing that he had seen me in a few classes. I also took a statistical consulting course with him. And he said, “Hmm, Kaplan, now how many courses have you taken?”

He said, “Do you realize if you just take one more class, and write a thesis, you can get a master's degree in statistics?” So I never actually applied for admission to the math department where statistics was, I simply took all of these classes and was doing well in them, and so finally, at Herman's urging, I said, “OK, I'll do that.”

I'll just take one semester off and take whatever the remaining courses were that I need to take and wrote a thesis in statistics. And Herman was my advisor. It was one of the best things I ever did. Because now, I understood a lot of statistical models. I had a much better background in probability.

I, of course, was in the process of building, more or less, original models in operations research, and it became possible, with the statistical knowledge, to design appropriate ways of estimating parameters, appropriate ways of conducting statistical tests, and so forth. It just made for a great package.

JIM COCHRAN: It comes through in your work. I think you have a much better feel for statistics and probability than a lot of other people in operations research. It makes you more versatile.

ED KAPLAN: Well, I feel sheepish to hear you say that, because there are certainly people at INFORMS who know much, much, much, much, much, much, much more about both of those subjects than I do. But I will say that I find the combination invaluable in going after the sorts of policy problems that I've ended up working on in my career.

JIM COCHRAN: In '84, you received your PhD from MIT in urban studies and planning. Who chaired your dissertation?

ED KAPLAN: Oh, well, one guess. Mr. peanuts in the shell. Dick Larson was my thesis advisor. Arnie Barnett was on my committee. There was also-- the thesis was called Managing the Demand for Public Housing. And the third person on the committee actually was a housing expert, Lang Keyes, who had actually served as an official in the Federal Housing-- Department of Housing and Urban Development, and had also been involved in state and regional housing issues.

And how I ended up working on public housing is one of those wonderful accidents in life, because the problem just fell in my lap. I was in my office one day, and a classmate of mine from urban studies named Christine Cousineau came in, and we had gone through the urban studies doctoral part together.

Her boyfriend was a consulting architect to a group of tenants at one of the local housing projects at the West Broadway Housing Project in South Boston. This was a project, like many public housing projects, had been built after the Second World War, originally to house returning GIs, but had since taken on a life of its own.

It was literally falling apart. The buildings were falling apart. And so, they needed to be modernized, refurbished. But in order to work on the buildings, people who were living in the buildings had to be moved out. In order for people to be moved out, there had to be some place to put them. And you can kind of see what's going on.

They had this gigantic game of musical chairs where the idea was how can we do this as quickly as possible while making sure everybody always has an appropriate place to live? We don't break up neighborhoods. And we satisfy certain physical issues, like the systems aspects of where are the big water and heating pipes going-- that kind of stuff.

So her boyfriend introduces me to the project planner at the Boston Housing Authority. And I go down to the housing authority in a small room, about the size of this room we're in right now, actually. And on the wall, papered around the room, are these big charts where each chart corresponds to one building.

And the rows in the table are the different families that are occupying the different units in the building. And across the columns are proposed moving dates and target locations. So you look at

one piece of paper, and it's building three. We're going to move Mrs. Smith, who right now is in apartment 2g, and we're going to put her in 5h over in building seven, and we're going to do that October 17th.

And they were trying by hand to do this whole thing. And they had gone halfway around and they got stuck. What do you mean by getting stuck? Well, they had someone who required three or four bedrooms and there were no vacant three or four bedroom units in which they could put this person.

And I stared at this thing and I said, how many buildings are there here? Fifteen. I said, and how many different sequences have you tried? Oh, this is the first one. I said, you know, fifteen factorial is a big number. And that's the number of different sequences you could do and you're trying to do this by hand-- I think there's a better way to do this.

And that introduced me to this whole notion of what has come to be known in scheduling as the relocation problem. Not facility location problems, like where do you put the facilities, but actually the relocation problem, meaning that you have to take people or offices or material or whatever-- they are in one place, you have to relocate them somewhere else, but you have to do it in a way which satisfies a whole bunch of constraints.

And what's interesting about this is that the resources for where you put people are themselves endogenous to the whole process because they are the consequences of taking things down and putting things up. So I did put together an optimization model for that.

It solved that problem. It was used many times at the Boston Housing Authority, actually, not only for doing this on-site relocation, but also for solving problems involving scattered sites. To this day, there exists a program called section 8 where people who are lower-income folks, if they qualify, can go into the private market and landlords will have rent subsidized from the government so it becomes possible for people to rent units that otherwise would be out of their income range.

But those units also were falling into disrepair and needed help. So now you had this spatially distributed version where you had to move people around, like the same musical chairs game. The same mathematical model worked just as well for that as it did for when everything was on-site.

And while all of this was going on, I started visiting different public housing projects and learned about some of the other things that were happening. And I got very interested in this question of well, I understand these housing projects, the units are pretty much either all filled or they're completely out of service-- the term was "mothballed." No one could live there. They were not habitable.

Public housing projects don't have the greatest reputation as being wonderful places to live. So I said, well, this is sort of interesting. What's the demand? How many people are trying to get into public housing? All of sudden, I said, oh, this is a big queuing problem.

You have people who are applying for public housing. You have a service rate which effectively corresponds to the unit turnover rate. But with the arrival rate 10 times the turnover rate, it can't be the case that that's all that's going on, because otherwise, the number of people on the waiting list would sort of zoom off to infinity.

Well it was pretty large, but it wasn't infinite. And that's all of a sudden when it dawned on me, oh, that's because people drop out. Unlike the kinds of queueing systems I had studied up until that point, there was reneging here. And reneging from a public housing queue means, typically, you found somewhere else to live.

Either you left town, you moved in with a relative. You actually found someplace that was affordable. You took public housing in a different location. So that's where the whole idea of writing a thesis on managing demand for public housing came in. And the public housing policies that-- it really all came down to this notion of who gets the next available unit-- so-called tenant selection and assignment policies.

They determine everything. They determine the waiting times for the applicants. They determine the demographics of the projects to who gets in where. And this is where it got really interesting. It got really interesting because, of course, the policy that Boston was using allowed for a certain amount of choice on behalf of the tenants.

So if someone applied to public housing, they could say, I'm only willing to live in this one particular project, and say where it is, or I'm willing to live in one of these two, or I'm willing to live in one of these three, or I'm willing to live anywhere. So if you have something like 54, 50-something projects, think of all the choice sets you have.

You have one for the people who want to live anywhere, plus 54, plus 54 choose two, plus 54 choose three. So you have a lot of different types of applicants. Meanwhile, you have all of these service--

And there was no preference ordering. There was no rank ordering. It was just-- your preferences are just expressed in this choice set or not. So who gets the next unit in a situation like that? All right, everyone for whom the unit just became available is part of your choice set, raise your hand.

Now among everyone whose hand is raised, who's been waiting the longest? I had never in my life seen a queueing system like that. And it turns out, nobody else really had either, at that time. That was a problem that actually stayed on my head for 30 years.

And it's only in the last, say, five or ten years, that people really went back and made progress with this problem in the form of matching problems. And actually, I have a paper co-authored with Mohammad Fazel-Zarandi that just came out in *Operations Research* where we show that you can approximate this problem using Ohm's law from physics.

You think of the matching between customers and servers and the flows of applicants that are going to different projects, you think of it like electric current in a circuit network. And it turns out that, that actually gives you a very good answer system.

So here's a case where something just stayed with me for years and years and years, and lots of other people got involved working on that project, and lots of really elegant, beautiful, deep work came out of it, notably papers by Gideon Weiss and Ivo Adan on the one hand, or people like Rene Caldentey who is now in Chicago, have contributed to this area.

There's a lot of interest in matching models totally independently of public housing. They show up in ridesharing networks. They show up in ad placement. Or there's just a lot of these matching models-- kidney matching is a big one.

And it turns out that some of these actually go back and are of exactly the same structure of this original public housing problem I looked at all those years ago-- kind of interesting.

UMass Boston

JIM COCHRAN: No, it's very interesting. You went from there to Boston to UMass, right?

ED KAPLAN: Yeah, right. It's like the other side of town. I did. I was living in Brookline at the time, but right. That's right. I went to UMass Boston. Actually, I spent two years at the Kennedy School, first.

I graduated from MIT and then I-- no, wait. I take that back. I graduated from MIT. I spent one year at the Kennedy School as a post doc slash research associate. And then I went to UMass Boston.

JIM COCHRAN: What was it like to be an operations researcher at UMass Boston at that time?

ED KAPLAN: So you need to understand, Boston has lots of universities. So UMass Boston has a very particular mission. It's a public university and it is really there to serve the, in some sense, either the students who are just not as talented or didn't have the same opportunities to learn as much, so they're not going to get into the Harvards or the MITs, or for that matter, the Tufts or the Boston universities, or they can't afford it-- either way.

So it really has a very urban population, people who are working-- almost all of the students who are going there, also were working. So you have some older students. Totally different kind of a population mix. Now there was-- or there still is a college of management there at UMass Boston.

And within the college of management, there was a management science group. So there actually was a department within a college within the university, but it was by no means research oriented. It was 180 degrees from what I had been used to at MIT or at Harvard.

The standard teaching load at UMass Boston was six courses per year, three courses in each semester. This makes it difficult for someone to do research, even if they want to. And the other thing, I have to say, and this is by no means unique to the operations or the management science group, but overall, I found the faculty at UMass Boston to be quite demoralized.

This is a university which, other than Harvard, had the highest percentage of Harvard PhDs on the faculty, because a lot of people come to Boston, they go to Harvard, they didn't get a job at Harvard, but they didn't want to leave Boston. So a lot of people would end up teaching there.

And if you don't buy into the teaching mission, and if you think that you have some research aspirations, it's almost impossible to do anything. Now I had the good fortune of when I was at Harvard for a year, applied for and received a very small NSF grant to kind of further work in my housing stuff.

It was all of $30,000. $30,000 today, I don't think NSF would even give out a travel award for so little money. But with that $30,000, I was able to buy out teaching in two classes. So I had the enviable teaching load of only two courses per semester, instead of three courses per semester. So that was one thing that was important.

The second thing that was very important, was that Oded Berman, who had been-- he had taught me my year at MIT in the urban operations research course and had left and gone to the University of Calgary, was interested in moving back east. And so he actually was recruited and joined UMass Boston.

So Oded and I together, over a period of a year, I think, knocked out something like four different papers in various areas. So on the one hand, trying to stay active was very important. It was a very challenging environment to do that, and it would have really I think been impossible to have the kind of research career I eventually was able to have, had I stayed there.

JIM COCHRAN: So that was the appeal of Yale, or one of the appeals of Yale, I'm sure.

ED KAPLAN: A huge appeal. Now, I actually had an offer from Yale after I graduated from MIT in 1984. That was when I first had gone on the market. The reason I ended up going to Harvard, frankly, was I wanted to stay in Boston. I didn't want to leave right away. And the reason for that is personal. I had a girlfriend, and I was heavily involved in the chase, as it were.

And so leaving town wasn't what I wanted to do. And UMass Boston also made it possible to stay anyway. So Karen and I ended up getting married while I was teaching at UMass Boston after one year. And having been there for a year already and looking at my second year and what was going on, I quickly realized that I did not spend all this time at MIT and get these degrees-- this is not where I wanted to end up.

And I say that without any malice to my former colleagues at UMass Boston, who were a wonderful group of people. They were doing the best they could under a very difficult set of circumstances. So I went back on the market and Yale remembered me. And so, in 1987, we moved to New Haven.

JIM COCHRAN: Let's backtrack a little bit. How did you meet Karen?

ED KAPLAN: Folk dancing. One of the things I did while I was a graduate student at MIT to keep my sanity, was I participated in folk dancing. Now by this, I don't mean square dancing. I mean very athletic, Israeli and international folk dancing.

On the international side, dances from the Balkans-- Bulgaria-- very, very rhythmic dances where the rhythms are in sevens and 11s and 13s, or you know [RAPID CLAPPING]. And there's-- the steps are-- it's quite athletic. It's quite the workout when you would do this stuff.

Anyway, Karen had moved up to Boston largely for reasons of Israeli dancing, because that was where one of the biggest groups in the country was. We used to have, easily, 150 to 200 people every Wednesday night at MIT dancing Israeli dancing at the MIT folk dance club.

JIM COCHRAN: Wow.

ED KAPLAN: And plus, both of us performed, Karen much more seriously than I did. But just as an added aside, the very first time-- not counting my parents-- but the very first time I had an all-expenses paid trip to Israel was not as an academic, it was as a member of a performing Israeli folk dance troupe. So that's how I met Karen.

JIM COCHRAN: That's terrific. You have children?

ED KAPLAN: We have a child. We have Allison. And Allison is very much, again, 180 degrees different from me. She's into horses. That's her passion. So she takes care of horses. She rides horses. That's what she does.

Arrival at Yale and Research on HIV and AIDS Policy

JIM COCHRAN: So you arrive at Yale-- you and Karen arrive at Yale, and you start engaging in some very interesting and somewhat controversial research. I'm talking about the research you did on HIV and AIDS policy and the needle exchange programs. Would you talk a little bit about how you came to that problem?

ED KAPLAN: Sure. Well, first of all, right when I was going to New Haven, the way I first got involved with AIDS actually goes back to the folk dance club. There were a lot of gay men who were folk dancers, and many of these people were friends of mine, and we're now talking about the mid 1980's. And some people started to get sick.

So that was one thing that was just kind of happening in the community, and it was just on the edge of it spilling over to the point where you would notice it. Where I was, people would talk about it, and in some cases, some of the people who actually were friends of ours, themselves, became infected.

But the other thing was that I had two close friends who were both doctors. One of them was an infectious disease expert, the other was a pulmonary fellow. They were both dancing friends, but they would talk about what was going on with this new disease, with HIV.

And at one point, there was this conversation we had which didn't make sense to me. What happened in those days was that every week there was a publication from the Centers for Disease Control of how many new AIDS cases were being reported. And they said finally, the numbers are starting to go down.

So we think that behavioral change has set in. People are taking fewer risks. Things must be getting better. My response was-- I said, but wait a minute, I thought you told me that it took five years from infection until symptoms. Now, that's what people believed at the time, that it was five years.

It turns out, actually, that that's wrong. The five years came about because of a misapplication of statistics. There were only five years worth of data, basically. So you missed out on it. But I said, but nonetheless, if it was five years, then whatever behavior change you're talking about would have had to have happened five years ago, but five years ago, nobody knew anything.

So either the five years is wrong or what you're seeing in the data is wrong or what you're seeing the data is correct in terms of the numbers, but it's just a random blip. It doesn't really mean anything. Well, we've got to know. We've got to know. Can you help us study this?

So the next thing I know, I'm in the Countway Medical Library at the Harvard Medical School xeroxing page after page after page of all this, because remember, no internet, no data scraping or anything like that.

JIM COCHRAN: No PDFs.

ED KAPLAN: I go home where I had this Sanyo computer, which I was mainly using to play golf computer games on or something. It wasn't really much in the way of a computing thing. I said, what the heck-- I start typing everything in, and trying to see if I could build a little model.

And the model was very simple. It was basically a convolution. It says if you want to know how many AIDS cases there are in a given year, you take a look in history at how many HIV infections there must have been, and then you have to look at the probability that it would progress from the time of infection until AIDS.

And so that gives you a set of equations that you can sort of back solve. It's not a very well posed set of equations, so you have to have careful tricks and stuff for doing it right. Anyway, I'm in the midst of starting through all this by hand, when all of a sudden, a paper comes out in the *Journal of the American Statistical Association* by a very well-known biostatistician named Ron Brookmeyer, and he'd kind of done this exactly right.

The technique became known as back calculation. So suddenly, it was kind of like, oh, well, it looks like I've been scooped. But on the other hand, considering that I knew nothing at all about this, I actually thought well, OK, I think I was onto sort of the right idea.

And so I started talking about what are ways that maybe I could be involved to do something. And I quickly concluded that the operations background, while of course you could contribute on statistical questions and estimation and maybe even epidemiology, that wasn't really where the real value added was.

The real value added was going to be in evaluating policies to try and slow this thing down, to try and prevent infections. So that's the background. Now I get to New Haven. And I had written a paper. It's called “What are the Risks of Risky Sex”.

It was published in *Operations Research* in 1989. But at that time, I had got to New Haven in 1987, of course, it wasn't published yet. It had just been submitted. And I was trying to figure out who at Yale is the AIDS expert. And I start searching around, and everyone pointed me to a professor in the biology department named Alvin Novick.

They said this is the guy who knows more about AIDS at Yale than anybody else. So through the campus mail I sent him a draft copy of the paper I had written. And a week goes by, when the phone rings. Dr. Kaplan? Yes? This is Dr. Novick. I'd like to talk to you about your paper.

I said, oh, that's great. When would you like to do that? How about now? And the next thing I know, this guy, he's running over from the biology department. He had taken my draft and he had gone through every page. He had circled some things and says this is biologically impossible. It can't happen.

It's very easy in a mathematical model to have people recover from a disease, which in practice is not recoverable from. And so that was in the original model. There were people who sort of said, yeah, AIDS. They just suddenly magically got better. So that got rushed out of service very quickly.

But on the other hand, there were parts of the model that he thought were really very important and useful, for example, the basic timescales involved. If transmission was to sort of stop immediately, the time lag that will be required until AIDS cases would disappear.

How good would a vaccine have to be, if there was one, in order to have a noticeable effect on the virus, and so on. So that's the background. So Novick and I became very good friends.

Now, Al Novick was also the chair of something called the Mayor's Task Force on AIDS in New Haven. In New Haven, the real problem with HIV was drug injectors. Back then, something like a quarter of the cases in the United States, maybe 30% of the cases in the United States had something to do with drug injection.

But in the New Haven metropolitan area it was like 3/4 there, so it was a completely different picture than what you would see elsewhere. And Novick and other people who were working with him wanted to have a needle exchange intervention.

The problem was that such an intervention would be illegal, because it involves passing out syringes without prescriptions. So they actually took the strategy of saying, no, we're not going to do this illegally, which is something that was done elsewhere in the country.

They actually tried to advocate and get lawmakers on their side, get the police department on their side, get the clergy on their side, get drug treatment officials on their side. All this is very, very difficult. To make a very long story short, they succeeded.

And actually, the Connecticut legislature passed a bill legalizing needle exchange on a demonstration basis in New Haven. And the governor surprised everybody. The governor than was Governor O'Neil. He signed the bill. Now all of a sudden, New Haven had a needle exchange program.

But part of the bill was you have to tell us whether or not this thing works within six to eight months, or something like that. At which point, Novick comes to my office. Kaplan-- he would call me Kaplan a lot-- he'd say, Kaplan, I have a job for you.

And just like with the original public housing stuff, here is a problem that landed in my lap. It wasn't something that I kind of actively went out and thought of. It was like, this needs to happen. This program needs to be evaluated. I think you're the guy who can do it.

I meet with the people who are running the AIDS division, and originally, my idea was well, you know, what do you do? You take a bunch of people, you test them at the beginning, you put them through the needle exchange program, you test them after a year or something, you see what the difference is. It's just some kind of very naive idea here.

Well that got shot down quite quickly, because the director of the AIDS division, Elaine O'Keefe said, we're not going to let you test anybody. Again, we're talking 1989. There were no HIV treatments. An HIV positive diagnosis would basically lead to ostracism, would lead to losing insurance, possibly losing a job.

There was almost no upside, except for the fact, of course, that if somebody was infected, they could try not to infect other people, and maybe try to take care of their own health a little better. So things were quite different then.

Anyway, so that's how the problem landed in my lap, and things really sort of took off from there.

JIM COCHRAN: You did end up working with Ron on an edited volume, right? Ron Brookmeyer?

ED KAPLAN: Yeah.

JIM COCHRAN: And another edited volume in this area with Margaret Brandeau?

ED KAPLAN: Yeah.

JIM COCHRAN: How did those come about?

ED KAPLAN: Well, let's start with the one with Margaret, because that one came first, actually. So Margaret also got very interested in Policy Modeling around HIV/AIDS questions, and the two of us decided that there was enough interest inside the operations community, and that actually there could be much more interest if we included well-known people from outside operations research who were working on these problems, but from a quantitative point of view.

So we arranged at the San Francisco INFORMS conference-- and I'm probably going to get the date wrong, but I'm thinking 1992, maybe-- we had four special sessions on HIV/AIDS modeling at the San Francisco meeting. And was it INFORMS yet or was it still ORSA + TIMS?

JIM COCHRAN: It was still ORSA + TIMS.

ED KAPLAN: It still might have been ORSA + TIMS then.

JIM COCHRAN: '95 was around when ORSA + TIMS was.

ED KAPLAN: So it would have still been ORSA + TIMS at that point. And so people came from all over. And we solicited these papers. It wasn't something where we-- it was kind of we're going-- it was just Margaret and I basically went and contacted a bunch of people and brought them in.

And the response was fantastic. And the work was really interesting. And Margaret and I looked at each other and said, you know, we shouldn't let this disappear. Let's get it into a book. And not only that, let's also go out and maybe get one or two other people who are well-known in the area who weren't at the San Francisco meeting who could contribute something.

So we did that. And the book was published by a now defunct press called Raven Press. But Raven Press was an important press at the time because they published the *Journal of Acquired Immune Deficiency Syndromes*, also known as the *Journal of AIDS*, and so they knew how to handle this material well.

And so that book came out in 1994, actually. I was in Israel on a sabbatical at the time that, that book was published. That's the first one.

The second one, the one with Brookmeyer, was an outgrowth of a National Institute on Drug Abuse funded project in conjunction with a little group called SIMS, which stands for the Societal Institute of the Mathematical Sciences. The person behind SIMS was a gentleman named Don Thomsen, a mathematician from MIT, worked at IBM for many years, retired from IBM, wanted to use mathematics to help address societal problems.

Again, like with like here. We found each other and we put together a multi-site research project that involved researchers from Stanford, from the University of California, San Francisco, from Harvard, and from Yale, Harvard School of Public Health, and from Yale.

And one of the things we did was we decided that since the International AIDS Conference was happening in Geneva, and this was in now we're up to again-- memory is going to fail here-- but I think it was like 1998, something like this. It was the year France won the World Cup. I remember that. Those of you who are soccer fans, you can work backwards.

But anyway, we had this conference, and the idea was let's have a conference that focuses on quantitative methods for evaluating prevention programs. So it wasn't just going to be epidemiology and statistics. And it wasn't going to be medical research, per se. It was really going to be about evaluating HIV prevention programs.

And it was the work from that conference that led to the Yale Press book *Quantitative Evaluation of HIV Prevention Programs* and that's the volume that was co-edited with Ron Brookmeyer that you mentioned. So that's how that one came about.

Response to Needle Exchange Proposals

JIM COCHRAN: Well, it's interesting to me-- you got a lot of recognition for the work. The 2002 INFORMS President's Award that you received, I believe cited this work heavily. You received the 2008 Shepherd Science Award from the CDC, in part for HIV work. But at the time you were doing it, I know you took a lot of heat for the needle exchange program, and the work you did then. How did you stand up to that? How did you feel about that?

ED KAPLAN: You're absolutely right that we took a lot of heat. And at the time I was doing the needle exchange work, I was not tenured. In fact, I was up for tenure. And so, you have to imagine an untenured junior professor having his name splashed over the national papers by the director of the Office of National Drug Control Policy, who basically was telling the world that you were capitulating to drug use and making things much worse rather than better.

But what I learned from that entire experience was it's one thing to do the research, to build the model, to write the papers, it's another thing to try and really make a difference in the world. And if you want to really make a difference in the world, and if you really believe in what you've done, then you have to invest a lot of energy in communicating the work, in arguing with people.

And there will be people who will attack you publicly. And some of them do so actually not out of a mean spirit, but because they just honestly think you're wrong. But then there are people who will attack you who are very mean-spirited, because they don't care if you're right, they just don't want to deal with the whole subject matter.

And so it was a difficult experience. We were reviewed not once, not twice, but three times. And what do I mean by that? I'm not talking about the papers or journal review or anything like that. After the work was actually published, Charles Rangel, who at the time, was a representative from Harlem and was the chairman of the House Select Committee on Narcotics was outraged

by Yale University having a study that claimed that trading needles with drug injectors somehow or another was a good thing to do because it lowered the rate of HIV incidence.

And so he asked what was then the General Accounting Office, now the General Accountability Office, to investigate us. And the GAO responded, and they sent people up to Yale. And it was kind of like going through my orals again. I had to spend two full days at the blackboard.

They would ask-- they sent statisticians, actually-- where did the data come from? How does this model work? I want to see where this equation comes from? How did you estimate this parameter? How did you estimate that parameter? Like literally walk through the entire thing.

Now the happy ending here is that when the GAO wrote their report up, they said, oh, we think that these people are right. In fact, if we're going to fault them, it's that they were overly conservative. They claimed that they reduced infection by 33%. We actually think it was much greater because they didn't take the impact of drug treatment programs into account.

So that was one review. A second review came from the Centers for Disease Control. They actually did a complete-- it was the same thing. People were reviewing the work. And then the third one was the National Academy of Sciences.

There's actually a volume called *Preventing HIV Infection*-- or *Preventing HIV Transmission, the Role of Sterile Needles in Bleach*. Now, what you have to love about this National Academies volume is that the committee was chaired by a famous statistician named Lincoln Moses. How can you possibly not take anything written by Lincoln Moses as anything but the truth-- from God's mouth to my ears - anyway--

So I had to go and make a presentation to that panel. They also reviewed the work. So all three of these things basically came out claiming that the work was fine. But to go through that entire experience was scary, frankly, at the time. But we believed in what we did.

And we survived. And in the end, it worked out. And laws were changed. And needle exchange programs made it.

JIM COCHRAN: Well, the CDC's review actually resulted in their adoption of your recommendations, right?

ED KAPLAN: Well the CDC itself doesn't the sponsor the programs. It’s local health departments or state health departments, or in some cases, community organizations who do it with funding. What the CDC did say was that yes, these programs, in fact, are--

JIM COCHRAN: They came over to your side.

ED KAPLAN: They are effective. And so there are needle exchange programs in many cities around the country, and indeed all over the world now, and the results from our work were cited in several pieces of legislation that led to enabling laws so that these programs could actually operate legally.

That's not to say that the controversy has totally gone away. There are still some places where, depending upon the political views of who is in charge, it's difficult to do there. One sad case which is relatively recent, was when Mike Pence was the governor of Indiana, there was this horrendous outbreak in Scott County of HIV.

And in the end, they finally implemented a needle exchange, but it was sort of too little, too late, and a lot of people became infected there who wouldn't have become infected otherwise if the intervention had happened earlier. So in some sense, a lot of this work is coming back again now because of the opioid crisis.

Counterterrorism

JIM COCHRAN: That's interesting.

You moved also into counterterrorism, at some point, and Homeland Security, another area that I don't think-- when people started to think about the area, they said this is an opportunity for operations research. This is ripe for OR. But you moved into that area.

ED KAPLAN: Well, so it says a couple of things. First of all, to say that this isn't an area for OR strikes me as odd given that OR grew out of the military. So military applications, to this day, continue to be one of the most important areas where people work. All you have to do is look at the incredible work, which is done by people at the Naval Postgraduate School, at some of the other service Academy departments.

Naval Postgraduate School probably has the largest operations research program in the world, I think, certainly in the country. They have an entire school of operational science. It's a terrific department, and they're doing all kinds of important work. Not all of it is just military. But certainly all the critical infrastructure defense and just work that's been done by many people there over many years.

But back to how it was that I got involved in the whole thing. So it goes back to after 9/11, there were the anthrax letters. And the anthrax letters were sort of like a proof of concept for bioterrorism and people who worry about terrorism in the United States at that time.

There was no Department of Homeland Security. There was a White House Office of Homeland Security. Bioterrorism was very much on their agenda. They worried most about smallpox, frankly, because that-- 30% fatality rate and contagious. Anthrax is fatal, but it's not contagious. Smallpox could spread and that was something that people were very worried about.

So I get this phone call asking if I could come to a meeting to talk about smallpox. And so my first response was I don't know anything about smallpox. And they said, well, but we know that you know how to model diseases, because we know about your HIV modeling. And so that was really the hook, at that point.

Larry Wein and I, together, embarked on an analysis after having a meeting with the government where it became very clear what they-- they had a very specific purpose in mind. The Centers for

Disease Control had on the books an emergency response plan for what to do if there was a deliberate release of smallpox.

And people in the office of the White House Office of Homeland Security, and other people in the military, weren't convinced that this was the right way to go about doing things because it relied on ring vaccination and contact tracing, which in the natural eradication of smallpox, was what people did at the end.

But the issue here is it's one thing if there's a small number of people who are infected and they're kind of running around trying to find out who their contacts are. It's another thing if 1,000 people were infected all in one fell swoop, because of, say, an aerosolized weapon, and suddenly you have large numbers of people.

So that's how the bioterrorism part got started and that led to a whole other adventure involving smallpox, which if you want, we can get into. But the thing is, is that the other thing that happened there was that while all of this is happening, and the war in Iraq started, after 9/11, and in Afghanistan also, I found myself going back and forth to Israel a lot.

Now, I had already established a relationship with many academics in Israel and also government people in the HIV realm, public health people. For example, I wrote the first paper estimating how many HIV infections there were in Israel, for example. And also the issue of HIV infection among Ethiopian immigrants in Israel was something I worked on.

So I started going back and forth to share with them the results of what was coming out of the smallpox work. Now while this was happening, we're now up to 2001, 2002, 2003, 2004, the Second Intifada is happening between the Hamas and the Islamic Jihad and other terrorist groups are using suicide bombers to blow up buses and to basically cause numerous civilian casualties in Israel.

And so I started looking around and saying well, whether or not the smallpox weapon actually exists, we don't know. But suicide bombers exist. They're blowing themselves up all the time and they're killing people. Why isn't anybody trying to understand what the various operational responses are to this? What they could be?

There are very controversial policies in place-- targeted killings, construction of the security barrier, all these sorts of things. So I decided that maybe this would also be a good problem to work on, and that led me to a family of research papers that had to do with what are the best ways to counter suicide bombings.

So that was how I originally got into it and once, of course, you get into any of these areas, you start meeting more people and eventually, I found myself being part of a panel that was studying ways to combat improvised explosive devices and another one on how to use different methods to improve intelligence analysis.

I became very interested in operations models for intelligence that led to this whole terror queues way of thinking about the world, where you think of terrorism as a queuing process, and that

customers are terror plots themselves. And so you can actually, using queuing ideas, estimate how many undetected plots are in progress right now, which of course becomes important in figuring out how many agents you need.

So anyway, there's a whole slew of problems that kind of just start to happen on their own once you get on any one of these segues, or any one of these paths.

Sports

JIM COCHRAN: You've also—to lighten it up a little bit-- you've done a lot of work in sports. What do you think is the most interesting research effort you had in that area?

ED KAPLAN: Wow. So the sports really was just for fun. It started because there used to be a junior faculty person at Yale in our group named Todd Strauss, and Todd came from Berkeley. And they always had this little office pool they would play where the rules were really complicated.

You would get-- you know, this is March Madness, so we're talking about filling out the NCAA brackets. And you would get different numbers of points, not only if you picked upsets, but sort of the magnitude of the upset, like you know if a 16 seed ever beat a 1 seed you would get a million dollars or something like that.

So there have to be better versus worse ways to fill out any draw sheet, and a guy who's a friend of mine at Yale, we always seem to discover the bad way to do it, because we never won. There's something wrong here. We're watching college basketball a lot. We're just sort of big fans. It's a great way to just relax and watch a game.

And then we'd enter this contest and we'd always do lousy. And so suddenly, I said, wait a minute. Why can't we model this? Why can't we figure out a better way to do it? And so, that actually led to a really interesting project, because it turns out that the structure of a single knockout tournament is just perfect for dynamic programming.

The number of games they're having at each round in the tournament. And typically the points are doubling, or something like that. Anyway, so that's that case. That was actually interesting, and that was really a lot of fun.

And people ask me if I ever ended up making money off it and the answer is oh, yeah, I made a couple hundred dollars. Well, how did that happen? I said, well, because UCONN, which has a real basketball team, found out about it, so they invited me up to give a talk and gave me an honorarium. That's how I made money off of basketball.

So I did that. I have some stuff about hockey, and more probability added. I think, probably, the most interesting paper in sports, though, was something that I just did quite recently with a well-known economist at Yale named Ray Fair and it involves distance running.

And the idea is this. The idea is if you take a look at races like the marathon or the half marathon or 10,000 meters, things like this, we know what the world records are by age. So if you start looking at over age 40-- we know what's the marathon record for age 41, 42, 43, 44-- all right.

And the question is, is there any way those data could tell you something-- first of all, what's the natural rate at which people sort of slow down? Well, we're looking at the most elite athletes in the world and we're looking at the world record.

But it turns out, that if you look at things on a relative scale, which is to say you look at percentage decline as opposed to absolute decline, you control for the distance, and you control for the age, then you actually get a very consistent pattern, which is that people slow down on the log scale linearly up to some age, which is typically into the 70's, and then all of a sudden, things start slowing down quadratically.

So that was interesting in and of itself, and it provides hope for people who are runners, because you can say, all right, I'm getting slower. But the question is no, it's not if you're getting slower, it's are you on your curve or not relative to where you should be. Maybe you're running faster, and that should make you happy.

But the really interesting question was then, well, wait a minute now. How fast could you ever go, if we were kind of imagining overall, what's the fastest marathon you could ever run? Is there any way these data could be used to answer that question? People have tried in the past, for example, they just looked at time series and said, all right, here's the world record in a marathon.

You see how it's getting slower and slower over time, and eventually, you think it's going to asymptote. So that's one way to do it. So I had a totally different idea about this. I said, look, a world record by definition is a minimum. It's a minimum, however, over many, many, many people who have been running the race.

This sounds like a perfect opportunity to apply extreme value theory, because what's extreme value theory all about? It's about the asymptotic distributions of minima and maxima. So the idea here was let's imagine that you have some biological frontier which represents as a function of age, the fastest people could ever go. That's just it.

Now a simple way to think about this, is let's suppose you have some chain and it's got a breaking strength. The chain can break if you put different forces on it, but so that there's basically some minimum strength where you know that the chain can at least absorb that much. And then from when you go past that, you get a weakest link model, which is exactly the extreme value theory.

It's going to break when the first link breaks, which is going to be the minimum resistance to whatever the forces you're putting on it. So I thought we ought to be able to do the same thing for running. You imagine that there is this frontier out there, and now what we're going to do is say, look at the existing records. How far are they from that frontier?

If you knew where that frontier was, the distance between the frontier and the fastest time should follow an extreme value distribution. And we're talking about minima, which means it should follow a Weibull distribution. So now the question is can we simultaneously estimate the location of the frontier, instead of trying to get a curve, which in some sense comes this close to the data.

Instead, you're trying to fit a curve where the gaps between the curve and the data are the best fitting Weibull distribution. And if you do that-- and it turns out you can do it-- then, you actually get an estimate in percentage terms of where that frontier is from the existing records.

And it turns out that it works out to be something like 7.5%, or something. So we believe, for example, that the marathon record could drop-- at the time we did the work-- that the marathon record could go down by about almost another nine and a half minutes.

Several years ago, some famous statisticians had published a paper in *JASA* where they have concluded that the marathon would never drop by more than about 20 seconds, from when they did it then. If you remember, just a month or two ago, this-- his name is something, a very famous runner-- in one race, knocked a minute-- almost a minute and a half-- almost two minutes off of the world record, just in that one race. So much for the 20 seconds.

But of course, that still gives us another seven minutes to go, you know? So I don't know if we're right or not, but it certainly seems plausible. It's really interesting. So people are very interested in this, both from the point of view of like how are they slowing down over time and how does it compare to what the model does, but also this idea that maybe it's actually possible to say what the limit of performance is.

Anyway that's my answer to the question.

Risky Research

JIM COCHRAN: OK. Very good. Tell a little more about risky research. What areas of risky research do you think operations research needs to take on, at this point? Can you think of anything that we really should be doing that we're not?

ED KAPLAN: It's not that I would say you should deliberately take on something which is risky. That's not how I would frame it. But what I would say is that if it turns out that working on a problem which hasn't been studied before could conceivably lead to important results, but it also could lead to things just going south, that's the sort of risk you get.

So if you go back to the needle exchange, one of the reasons why the work became so well known was that it actually showed that the program was effective and it reduced infection rates by like 33%. But exactly the same methods and exactly the same data collection and exactly the same models could have led to a result that the program didn't do anything.

And then I probably wouldn't have gotten tenured, to be honest. And there's something wrong with a system where the decision is based on the outcome as opposed to the hunch. Personally, I would like to see junior faculty be rewarded for taking good bets.

So I'm not saying that everybody should go off and just try to work on the stuff that no one's ever done before. Obviously, junior faculty, when they're writing papers, you have to show the world that you're competent with the methods and that you're very systematic in your thinking.

But an important part of being a good researcher is the ability to identify new problems. And I think in operations, actually, operations research, the most important thing is the continued identification of new problems. I don't mean just in the public policy sphere. I mean overall.

So I've been very, very excited when I take a look at what's been going on just in the last decade, say. All the stuff, for example, with ridesharing services, and all the stuff with revenue management, and the economics and operations research coming closer together again for things like kidney matching or other kinds of matching problems. To me, this is terrific stuff.

And I think that what you need in the way of successful applications, of course, is on the one hand, you don't want to have sort of trivial analysis, because then the problem is well, maybe it's important, but people already know what to do. It's identifying these new ways of seeing things and then following through using the appropriate level of methods to do it and completing the package.

Right now, there's just an awful lot of very exciting stuff going on in the field. So I'm like totally bullish on operations research. But what I'm bullish about is I have absolutely no concerns with the methods side. We've got such smart people, much smarter than me, worrying about the mathematics of these problems.

It's the continued identification of the problems which is the key thing. And I would even go so far as to say I think that is the most important thing, is to continue to identify new things to work on where you have this operations mindset, and you can look at the new problem and say, all right, what are the operational components of it that really matter-- staffing, scheduling, managing delays, capacity questions.

These things really, really matter if we're talking about emergency rooms or if we're talking about ridesharing services or if we're talking about matching supply and demand in just the right way in a revenue management situation. It's crucial to get these things right. And operations researchers really contribute to making the world a better place in these commercial areas.

Look how different things are now that you have the Ubers and the Lyfts as opposed to having to wait for a cab. And everybody on their smartphone, has a shortest path algorithm built into their phone. You take it for granted. You don't think of it.

But I just think that operations has had an awful lot to do with all of these problems. And they can obviously make a big difference in the public sector and the policy sphere, as well. Look at

what's going on now in food security, for example. Look at what's going on in some of the work that Larry Wein has done, in terms of social inclusion.

How do you-- how can you make sure that people are who they say they are and make sure that people can be identified in a way that they can receive appropriate benefits and so on? There're just so many important social problems that operations can contribute to.

And the real skill here is being able to identify them and see the operations in them. That's what excites me, and that's why I think that the field is actually in great shape going forward, because people who understand operations methods, they know a lot. We're natural problem solvers.

But now you gotta turn more into identifying what--

JIM COCHRAN: Problem finders.

ED KAPLAN: --what are the big problems to solve. But they're out there.

JIM COCHRAN: Do you think that maybe the scientific community is a little bit at fault for this because we are publishing based on results rather than based on the problem that you look at and your scientific approach?

ED KAPLAN: Well, as I said earlier, it would be nice if people could be rewarded more for taking smart gambles. Now, of course, you'd say, well, what's a smart gamble? And it all has to do with potential. But, yes, I think that by nature the publishing enterprise is conservative, especially if you're a junior person and your goal is to get promoted and essentially get tenured somewhere, there is a real incentive to work on problems which are in some sense safer, that you can publish in a good journal, and they will show that you are methodologically astute, methodologically capable, clever.

And by the way, in terms of identifying new problems that matter, I would argue that, that matters just as much in terms of pure theory, as it does in terms of solving real-world problems. If someone solves a mathematical problem, which is just 100% in and of itself cute and interesting, versus someone solves a mathematical problem that connects to a whole bunch of other mathematical problems that all of a sudden unlocks a chain and makes it possible to go much further, clearly the second sort of a problem, I think, is going to be more added value than the first one.

But I do think that the nature of the game-- and this is not just operations research. I think this is true across the board. If you take a look at a lot of what goes on in scientific publishing, it does tend to be pretty conservative. The emphasis is on getting the right answer for whatever the question is and not arguing so much about whether the question is important or not.

Kind of what I'm suggesting is almost going totally in the other direction. I would rather see someone get a brand new question 60% right with what I would call a very simple first strike model, than take a problem which is 99% of the way there and you're going to make it 99.3% or something.

Now look, there are some real important in life where you have to be exactly right. You know, if you want to land a rocket ship on the moon, you've got to do that calculation right. If you do it wrong, you're going to miss the moon. I don't work on problems like that, because that really does require rocket science. It's not what I do.

So, I get it. There are things where you really do have to just be exact, exact, exact. But there are plenty of problems where you identify something new, you come up with a really good idea, you can kind of get-- make a hefty contribution the very first time you do it, and get people excited. And then the community is going to fill in and the problem will end up getting solved that way.

Travel

JIM COCHRAN: Maybe two more quick questions-- one, where in the world have you been that really excites you the most and where would you still like to visit?

ED KAPLAN: Oh, wow. Well I go to Israel a lot. I do that for several reasons. One, is I have family there. I'm very close with my brother and his family. They're very dear to me. And so, of course, I want to see him. I have a huge number of friends in Israel and most of them actually come from the Israeli operations research community.

I have been going there since 1994. And I just have a terrific set of colleagues in ORSIS, which is the Israeli Operations Research Society-- Operation Research Society of Israel. So at the universities, Technion, Hebrew University, Tel Aviv, Beersheba, or whatever-- it's kind of like a second home. So I just love being with the people. I love the food. I love the whole thing.

So that's a place which I try to get to at least, say, maybe two or three times a year. There certainly are parts of the world where I haven't been. Actually, one place where I have never been is South America, and I have always wanted to go to Rio de Janeiro and have just never found the right time to get down there. But that's a place I would really like to visit.

And perhaps Buenos Aires too, perhaps the Galapagos, perhaps Machu Picchu.

JIM COCHRAN: Easter Island?

ED KAPLAN: I mean, there are areas in South America, which I just haven't been able to explore. But I will say one thing. Because of INFORMS, one of the things I did while I was Member-in-Chief, and actually you're a good reason to blame for some of this, is I got to practice what I would call Star Trek operations research, which is the idea of boldly going where no operations researcher has gone before.

And that saw us traveling to places like Mongolia, places like Kathmandu in Nepal, or I went to El Salvador to help set up the first ever mathematical modeling center in El Salvador, or to other countries-- Taiwan, whatever. And I found it to be incredibly rewarding to go in and meet with people who have a whole set of problems for which we can help, and who just, for whatever reasons, operations research never really developed in those places.

And that was, I think, some of the most fulfilling work I was able to do while I was hoping to run the show at INFORMS. And I have you to thank for that, because you're the one who brought me to both Mongolia and Nepal.

JIM COCHRAN: You did great work in both places, and I appreciated your efforts.

This is probably a good place to wrap up. That's a nice high note, I think. I really appreciate the time you've given us to do this interview, Ed. Thank you very much.

ED KAPLAN: Thanks, Jim. It was a pleasure. And I'll just close by saying, to me, it's just been one adventure after another. I love the field of operations research. I think the future is very bright and people should be optimistic and focus on all the good things that we can do and don't sweat the small stuff.