

Chapter 1: Initial Introductions to Optimization

IRV LUSTIG: So we're here today with Professor Harold Kuhn of Princeton University. And to start off our discussion, how did you end up choosing optimization and math programming as a career?

HAROLD KUHN: I didn't choose it. I think it chose me in one way or another. I've told this story several times on several occasions, that I was a very impecunious graduate student in the spring of '48, and went to Al Tucker, who was Associate Chairman of the math department asking for summer work.

It was a very lucky time, because he'd just met with George Dantzig a short time before and discussed the brand new subject of linear programming with him. And Dantzig had been to see Von Neumann, and Von Neumann had suggested there had to be a relation between linear programming and the theory of zero sum two person games.

And this led to the idea in Al's mind that he'd start a project to study that relationship between linear programming and the theory of games. And it was a period in which it was easy to get money for support for such projects. So he rounded up money for a summer project, and Al Tucker and David Gale and I started to study the theory of games, a subject we knew nothing at all about. It was an informal kind of affair.

We just took the Bible, von Neumann and Morgenstern, and divided it up into three parts. And every day one of us would lecture on whatever part we'd chosen at that time. We worked our way through von Neumann and Morgenstern.

And the result of that research project was, of course, papers on duality theory of linear programming, which was presented in Symposium Zero in Chicago in 1949.

And that's how I got started. It was economic necessity.

IRV LUSTIG: Now you were a graduate student at the time at Princeton, with Al Tucker as your supervisor.

HAROLD KUHN: No, not really. I was simultaneously working on my PhD thesis, which was an entirely different area. It was in an area that borders algebra and topology. My thesis is called Subgroup Theorems for Theorems Defined by Generators and Relations. And my thesis adviser was Ralph Fox, a topologist.

And I proved something which is known as Kurosh's Theorem using covering spaces, homotopic groups, fundamental groups, complexes, entirely topological algebra, entirely in a different area from the linear and nonlinear programming that we were doing simultaneously.

IRV LUSTIG: So while you were working on your thesis in that area, at the same time you were working on the work related to games and duality.

HAROLD KUHN: Absolutely. Right. My thesis was finished in the summer of 1949. And that's

exactly the period in which Tucker and I started working on nonlinear programming. He went off to Stanford in the year '49-'50. So I was working simultaneously on my PhD thesis and working in this area.

IRV LUSTIG: So the first projects were really some of the key theorems that began the field. Were there some applications that people were talking about at the time?

HAROLD KUHN: Not enormously. I think the first applications I ever heard about-- we met George Dantzig in the summer of 1948. We went down to Washington and visited him and were regaled with his account of what the simplex method was.

And his wall was papered with the diet problem, which was, I think, the first problem that was really of any significant size that he solved with the simplex method. So the diet problem was one application, So the standard minimum problem that people would attack.

I think through von Neumann I learned about the assignment problem, on which I worked later on. And transportation problem surely was-- I'd heard from Koopmans and in discussions with others. But those were the primary applications.

Simultaneously, of course, Charnes and Cooper were starting out oil application to petroleum industry. But that's a little bit later, really. There weren't any computers to--

IRV LUSTIG: To compute on.

HAROLD KUHN: To compute on.

Chapter 2: Initial Computational Work

IRV LUSTIG: Yeah, I've heard some interesting stories about from George and Al and Phil about some of the early days, how people were computing.

HAROLD KUHN: I have a little story I'll try out on you anyway.

IRV LUSTIG: Go ahead.

HAROLD KUHN: In the summer of '53, I spent the summer at the Institute for Numerical Analysis at UCLA. And the INA had a computer in which the total memory was 256 Williamson tubes, that is, you had 256-word memory which were essentially CRTs, just round television tubes in which the memory was held.

And Tommy Tompkins was trying to program this computer to enumerate all permutations of 10 objects, and he was having a devil of a time doing it, just so that he could solve the assignment problem on 10 jobs and 10 people by brute force, just enumerate all the permutations. And I decided I would-- I took an interest in this at this time. And indeed, that was the beginnings of the Hungarian algorithm for the assignment problem.

But I also was interested in the traveling salesman problem at that time, and to try to establish what the polytope was for the traveling salesman problem. And this polytope is spanned by

extreme points, which are the tours, the tours that a traveling salesman can take. And so for a five-city case there are 24 tours. So you got 24 points sitting in 11-dimensional space which spanned this polytope.

And I had a bright idea I could find the faces of this polytope by sitting at the center of it and firing a gun at random. If you sit in the center of a polytope and fire a gun at random, you're going to go out through one of the faces. And this is a simple linear programming problem to maximize the distance from the center, or scalar times the vector from the center, and stay inside the convex hull of these 24 points.

And it turned out to be a program with about 25 variables and 25 constraints. And there was only one program in the whole world that was programmed to solve a linear program with 25 variables and 25 constraints. And that was the Standards Eastern Automatic Computer, called the SEAC. And I had a friend, Alan Hoffman, who had connections there, and Saul Gass was working there, whom I knew.

So I sent these problems by telex. There was no fax, no email. Telephones were too expensive. But I sent these problems by telex back to Washington, and they indeed solved 20 of them. That was about as much as we could afford to try to solve it. 25 variables and 25 constraints. That's ridiculous.

IRV LUSTIG: Well, in fact, I was reading this morning the paper by Alan Hoffman on the first tests they did with the simplex method against two other methods that were infinite methods, which were on the SEAC. And the interesting thing is, they came from games. And the largest problem they solved was a 10 by 10 game, which took 15 minutes for 11 iterations of the simplex method.

HAROLD KUHN: That's exactly the kind of story.

IRV LUSTIG: And they figured that 80% of the time was spent reading data in and out, working memory in and out of tapes.

HAROLD KUHN: Well, the SEAC had a liquid mercury memory. And don't ask me what a liquid mercury memory-- I think it was sort of baths of liquid mercury that were tilted back and forth and somehow kept data-- I don't know exactly how. But the SWAC, which was the one at the INA, the Standards Western Automatic Computer, couldn't handle anything of that size.

IRV LUSTIG: And this is 1953.

HAROLD KUHN: This is '53.

Chapter 3: Mathematical Programming and Optimization

IRV LUSTIG: You were involved at the beginning. Dantzig's first paper was Programming in a Linear Structure. And I know there's some stories, and you were around with Al Tucker, on how the terminology became over to linear programming and math programming and having a linear program. Can you say some about how that evolved?

HAROLD KUHN: I think Koopmans played a role in this, and I'm not quite sure what it was. Koopmans really argued for the use of the terms activity analysis in the description of this new subject. And since George came out of a planning environment, he liked the word programming as a synonym for planning. It probably was a bad choice, given the confusion over computer programming and planning as--

IRV LUSTIG: Today.

HAROLD KUHN: Programming as planning. But somehow George won out in that fight between him and Koopmans, and activity analysis never really took on at all. But linear programming was so simple minded that it caught on. It was natural for us to coin the term nonlinear programming. And I think possibly Bob Dorfman is responsible for the name mathematical programming in kind of a survey article that he wrote at one time, which was to cover the whole field.

IRV LUSTIG: Now when would you say that-- now today we call it optimization, which has helped us sell the stuff, because people understand that word. Would you say that there was a time where that became introduced to sort of include math programming as well?

HAROLD KUHN: Yes, well, optimization is a very old subject. I mean, every student of the calculus learns maximum and minimum problems. And in retrospect, I'm somewhat astounded that we didn't use just optimization, linear optimization, non-linear optimization or something like that, rather than coining these new terms. I think George was partly responsible for it, coming out of the planning activities of SCOOP in the Air Force, which he really started the subject.

I rather like optimization, too. And I think it's a concept that people understand. Doing things better, doing things in the best way, is a natural kind of thing to explain to students, in particular, what they're about.

IRV LUSTIG: What kind of people-- there was a number of mathematicians that you were working with at the time. What kinds of backgrounds were people coming from that were then working in math programming?

HAROLD KUHN: That's a very hard question to answer, because they came from all kinds of backgrounds. In the period of the late '40s, in the math department here, a lot of people were coming back after war service. That's one fact of life.

David Gale is a bit older than I am. And he'd worked at the radiation laboratory at MIT. Other graduate students, contemporaries of mine, had spent considerable periods working in applied areas during the war, and then they came back to finish up their graduate study.

I had spent two years in the army studying Japanese, strangely enough. But it meant that a lot of people had some practical experience. But I think in the math department at Princeton, people didn't come back to do applications.

There has always been-- this has to be said-- there's always been a class distinction within mathematicians between the ones who do pure mathematics and the ones who do applied mathematics. And the pure mathematicians always look down somewhat on the applied

mathematicians.

And I think at Princeton, we were safe because von Neumann was a mathematician of such eminence, and such a national figure and such a respected figure within the mathematical world, if he did game theory, if he took interest in linear programming, then it was OK.

But if you didn't have that kind of-- similarly, Lefschetz, who was the chairman of the math department, was doing work on nonlinear differential equations and nonlinear oscillations, very practical subjects which became enormously practical for control theory and rocket management and whatever.

The fact that mathematical giants like Lefschetz and von Neumann took an interest in applied things was a defense for some of us, because as a young man, you know, you're looking for a job. And it's the hot subjects that get the good jobs.

Chapter 4: Pure and Applied Mathematics

IRV LUSTIG: Now would you describe yourself-- I guess then compared to now-- as more of a pure or applied mathematician?

HAROLD KUHN: I think I've always been more of a pure mathematician. Occasionally-- I've done a lot of algorithmic work that is working on ways to get constructive solutions to problems. But by and large I've been attracted by the intellectual challenge of getting sort of the best algorithm to solve something, not applying that algorithm after it's been found, to solving real problems.

I've had a split personality, as you must realize. For somewhat over 20 years, I was very deeply involved with a consulting company here that I helped found, Mathematica, Incorporated, so I could one day wear my hat as a pure mathematician, on my next day wear my hat as a principal of this consulting firm who had to take an interest in clients and what kind of problems they were solving.

At one stage, our consulting work for the National Biscuits Corporation involved solving the largest linear programs that were being solved at that time. We did production scheduling for National Biscuit, which was gigantic linear programs.

IRV LUSTIG: And when was that?

HAROLD KUHN: Well, this had to be-- must have started about 1963 or '65, something of that sort.

IRV LUSTIG: And do you recall about how big the problems were at that time?

HAROLD KUHN: They were very large. They were largely transportation problems. They could run as high as a million variables and 30,000 constraints. Good size. They were good size. We also did work with Mobil Oil. We did work with a number of corporations with very large-scale linear programs.

IRV LUSTIG: Now Mathematica is still here today, right, the firm?

HAROLD KUHN: Mathematica was sold to Martin Marietta in the summer of 1983. It had three divisions-- actually four, in one way or another. The division that Martin Marietta wanted was the software division. We'd developed a database management program called Ramis, which was the Cadillac of database management systems at the time. In effect, they bought us because they wanted that particular part of the company.

We had an industrial consulting group which was called Math Tech. And the principals of that company bought their way back from Martin Marietta several years after the sale.

There was a third division which was called Mathematica Policy Research, which did research in such things as the negative income tax. We had a contract with the Labor Department in which we gave about \$40 million away to poor people to see how they behave if you give people a negative income tax.

The fourth division was a lottery division. We were principals in the first successful lotteries in the country. We designed the New Jersey lottery, which was, in many ways, the first successful lottery in the country.

But in short, in my capacity, I was Scientific Director, I was Senior Consultant. I worked on all kinds of projects there and was exposed to lots of practical applications of optimization theory, of game theory, of many subjects that have little to do with either.

Chapter 5: Early Career in Math Programming

IRV LUSTIG: I'll wait for the plane to pass over. When would you say that you knew that math programming, that this field was going to be a successful endeavor?

HAROLD KUHN: You're going to have to rephrase that question.

IRV LUSTIG: Well, I mean, when did you say, yes, this could be the foundations of a career, and know that this was going to be something that was going to be a lifetime's work?

HAROLD KUHN: That's a very hard question to answer. Many things in life are somewhat involved with chance. And I think if you deliberately say, I'm going to do a life of work in mathematical programming--

I came back to Princeton in 1959 after having taught for six years at Bryn Mawr College. And I came back to Princeton to a joint appointment teaching both economics and mathematics. I've never had a course other than a 10-week introduction course at Cal Tech when I was an undergraduate in Economics.

And it's rather strange, but I've taught a wide variety of courses in the last 30 years, everything from graduate courses in international trade to undergraduate courses in price theory. And that, clearly, was a choice on my part, that I was going to undertake to teach economics, although my research was mainly in the far reaches of theoretical mathematical economics. And that was somewhat by choice and somewhat by chance.

I think, really coming to the heart of your question, I knew very early that mathematical programming was a very solid subject. I worked very hard on the assignment problem and algorithms for it. And when they were published, you'd get a fantastic reception. I would get letters from all over the world how the Hungarian algorithm was applied to scheduling empty freight cars in Romania to solving textile plant problems, switchovers. You realize, suddenly, that something you do, without any particular practical application in mind, is hitting a note of resonance all over the world.

IRV LUSTIG: Right. And that must be pretty exciting.

HAROLD KUHN: It is exciting.

IRV LUSTIG: That sort of leads me to my next question. What project or event that's occurred over time has made you proud to be part of the field?

HAROLD KUHN: Well, I think the collaboration with Tucker was the first such event, when we wrote Nonlinear Programming together. It was exciting to realize that the duality of linear programming carries over in very large measure to nonlinear programs, that there are dual variables, Lagrange multipliers, call them what you will, that play the same role. And they lead to necessary conditions for nonlinear optimization within equality constraints, which parallel almost exactly what you see in linear programming. That was an exciting moment.

I think we fully realized that this is something that had to have real significance in the long run. We didn't do it for algorithms, but of course, the conditions which are now called, properly, the Karush-Kuhn-Tucker conditions, because Karush did the same work in 1939, are the basis of many algorithms, which are practical and in use. And I think we recognize that, in the same way you understand that when you teach an undergraduate in calculus to set the derivative equal to zero, that's teaching him how to solve minimization maximization problems, so that we knew the role of that.

I've always been extremely proud of the Hungarian method because I think-- when I worked on that method, I was working in an attic office in the little farmhouse outside Philadelphia where we were living.

And I realized I could solve larger problems than any machine in the world could solve, and I could do them by hand. I knew I had a good algorithm when I could solve a 12 by 12 assignment problem by hand in about an hour of hand computation. There was no computer in the world that could do that.

IRV LUSTIG: Now just to frame this for the audience, the time of that would be about when?

HAROLD KUHN: The fall of 1953, beginning of 1954. And so the computers weren't in the race, but they were going to be very shortly. So if you've got a method in which you can beat any computer in the world by hand-- of course, that could have been programmed, once you had a computer to program it on.

There are moments like that in which you realize you've done something which will last, and that's very exciting.

IRV LUSTIG: If you could go back into the past and you were starting all over again, do you think you would make the same choices career wise?

HAROLD KUHN: I don't think I'd change very much. Being an academic gives you the freedom to pursue the problems that you like and are interested in, irrespective of-- you don't have a client. You're your own client.

And fundamentally, the main part of my life, separate from the consulting work, has been that. And I've been able to choose my own problem set with a great deal of freedom and independence. It's a kind of freedom that not many people have in this world.

I can give you an example. A fair amount of my research activity in the '70s dealt with computing approximations of fixed points. Now this has a relationship to economics because calculating general equilibria had been recognized as something that could be done by fixed points very early in the game by von Neumann and others, Kakutani.

And on the other hand, the attraction of this, for me, was the abstract problem of applying certain combinatorial results, like Sperner's Lemma, to these very practical computational problems. And I got a great deal of pleasure out of developing algorithms and programs that could be explained very easily, almost down to the high school level, for solving these problems. And it wasn't done with any aim of application, although I knew there were applications in the background, and people do compute economic equilibria for policy purposes.

I loved the mathematics. I'd learned Sperner's Lemma first from Tucker in a graduate course-- actually, an undergraduate course in topology years before. The idea of using beautiful, pure mathematics to solve algorithmic problems is probably been the theme of the best work I've done in my life.

Chapter 6: John von Neumann and Al Tucker

IRV LUSTIG: What interesting stories can you tell us about what it was like to work with von Neumann?

HAROLD KUHN: Well, you see, most of my contacts with von Neumann were when I was a graduate student. And we would occasionally-- by we I mean Al Tucker and I would make an appointment and go see him about some subject. We wanted either advice or support or something of the sort.

And I'll tell you one story that Al Tucker told me later that I'd forgotten. Once, as a graduate student in one of these meetings with von Neumann, I pointed out an error that von Neumann had made in his expanding economy paper. He'd had a fraction in that paper in which the denominator could go to zero. And he'd not realized that, and it led to very severe difficulties.

And Tucker told me years later that he blew up at having this error pointed out to him. He was

an extraordinarily vain man in one way or another. But very generous with his time and all the rest.

Together with Tucker, I edited two of the volumes called Contributions to the Theory of Games in The Annals of Mathematics Studies. And von Neumann had done an algorithm for solving matrix games using differential equations. And a mathematician named George Brown, who worked for the Rand Corporation, had done a similar algorithm.

So I was able to organize a joint paper and travel back and forth between Princeton and Santa Monica to get this paper written between the two of them. And von Neumann was very cooperative in this sort of regard. He would come to the seminar, gave superb seminars. He would erase what he'd written almost as fast as he wrote it, which made it hard for you to follow sometimes. But that's the way his mind worked.

I think it's a pity. Toward the end he spread himself too thin with his consulting and with the Atomic Energy Commission. I think he might have done some more great mathematics if he had not spent as much time serving the government in that capacity. And of course he died really very, very young of cancer.

IRV LUSTIG: Now I know I've heard you speak of Al Tucker numerous times. But for the pleasure of our audience, tell us about what it was like working with Al.

HAROLD KUHN: First of all I'd say Al Tucker was a Canadian of English background, which says a lot. His father was a minister. and Al was given a very strict upbringing. And hence, to many people, he might appear uptight. But in fact, he was really very relaxed, and warm and friendly with his graduate students. I recall that he would often ask graduate students to come out to dinner with him and his wife and his family.

And I think on one of those first occasions, going over to New Hope to a restaurant, he said to David Gale and me, I want you to call me Al. I don't want to be called Professor Tucker. Now this is a man probably in his forties at that point, and hence, to a graduate student he seemed like a very aged gentlemen. But this was typical. He wanted to be called Al, and we always called him Al. And that was the way it was.

Very generous with his ideas, but a perfectionist. He published very little in his lifetime because he was such a perfectionist. Final work, jointly with Evar Nering, Linear Programming and Extensions, a beautiful textbook. Took them nearly 18 years to write because they kept changing and modifying and perfecting and whatever. And that's kind of a pity, because I think it deprived the community of many books and papers that could have been written had he not been such a perfectionist.

IRV LUSTIG: What would you say his reaction would be to, say, the growth of practical optimization that's occurred?

HAROLD KUHN: I think a bit of surprise, as we all-- I think we're all a bit surprised at the pervasiveness of techniques throughout industry and whatever.

IRV LUSTIG: Why is that so?

HAROLD KUHN: He was attracted to it, I know for a fact, because he recognized parallels with things that he'd understood as a young man. He analyzed Kirchhoff's Laws, analyzed electrical networks, and studied their generalizations to topological subjects such as betting numbers and homology groups. That's where he started. He started as a pure topologist as a student of Lefschetz. And I think he got into the field just because of these lovely analogies that he recognized.

I think some of his major contributions were in expository material, that is, he made sense of areas of linear programming that were not well taught or well written before he got his hands on them. I don't think he ever took an interest in applications at all, not in the slightest.

Chapter 7: Applications of Optimization

IRV LUSTIG: Why would you say that you're surprised at the pervasiveness of the applications in industry?

HAROLD KUHN: I guess because anything you work on becoming so important just runs counter to your original feelings about it. That's the best answer I can give.

I mean, I don't know whether-- if you put the same question to George Dantzig, he might say, yes, when I was working for the Air Force, I thought this is going to be the method that's going to cure the industrial world, and I saw the full development right from the beginning. But I don't think many of us saw that.

IRV LUSTIG: Well, I think he-- I asked him something along these lines, and he was saying that-- well, actually, Phil Wolfe was saying that there was-- what hurt some of the growth was oversell, so that there were people who were promising too much. And you were only able to deliver so much.

HAROLD KUHN: But I think it is a fact that no self-respecting company can get along without having some form of optimization in its planning structure or some kind of optimization in its day-to-day operations. Otherwise they'll go out of business. They won't be in competition with the others any more.

IRV LUSTIG: Right. Yeah, we like to sometimes say that it's recession proof.

HAROLD KUHN: In firms like petroleum firms, it just-- how could you get along without it? And you go down to more pedestrian sort of things like animal feed mixing. You can't be in the business without optimizing. There's just no doubt about it.

IRV LUSTIG: How do you explain optimization and math programming to people who haven't heard of it?

HAROLD KUHN: With difficulty. I'm more often called upon to explain game theory than optimization, to tell you the truth, because in one way or another, game theory has had a hold on the popular imagination that, perhaps, optimization has not.

IRV LUSTIG: So what do you use to explain-- say you're at a cocktail party, and how do you explain game theory to the artist who's there?

HAROLD KUHN: Again, it's a difficult task. You try some interesting example that you know that might appeal to them, like the prisoner's dilemma or battle of the sexes or something like that. But I use the examples. Happily I've not been in this position too often.

It's hard. If you take nonlinear programming and try to think of some really good example which really affects their lives-- I try to use balancing of power supplies over generators and across state lines, which is something which clearly is of practical importance to states like California right now. They enter into long-term contracts to either buy gas supplies or electric supplies.

And these are large-scale nonlinear programs on the part of the people who are solving them. They're applying Kuhn-Tucker conditions 100 times a minute in their calculations. And I think people can understand how you have to do planning in these situations. I don't think they ever get any of the sense of what the difference is between nonlinearity and linearity, or even have a grasp of how large these problems are. It's a rare scientist who's seen problems this big.

Chapter 8: Teaching Students

IRV LUSTIG: Now you've taught some of the intro courses in the math department at Princeton. And in those courses, what do you think motivates the students to get excited about it?

HAROLD KUHN: Different strokes for different folks. I think the mathematicians really like the mathematical theory. The mathematical theory is beautiful. And you can bring that out. Computer scientists have always been algorithmic-oriented. You usually get a student who's already encountered shortest path problems or something like that.

Economists, I think they do it because it's something like spinach. It's told to them it's good for them, and they'll eat it. I think, by and large, the best students I've had have been computer science. They really get caught up in the development of algorithms and in the applications, and often in the theoretical ends of it as well.

I had a brilliant student, David Shmoys, who was an undergraduate at about the time that the Khachian algorithm was devised for linear programming. And he got deeply involved in that and applied it to perfect graph problems and program things which were far ahead of most of the professional postdoctoral people were working with him on. They get very excited about both the practical applications, the algorithmic applications. David is now a professor of operation research at Cornell.

IRV LUSTIG: Right, yeah. I know him.

HAROLD KUHN: You do? You know him. The best students clearly were always the computer science.

IRV LUSTIG: Now that's interesting. I mean, does that sort of sense that maybe there would have been even more excitement about math programming if it aligned itself closer with

computer science, as opposed to Mathematics

HAROLD KUHN: It has aligned itself closer already. I don't think-- there are very few pure mathematics departments in the country who teach it seriously. I think they're either operation research or computer science departments where it's taught seriously and at a high level. We still give-- there's still an introductory course in the mathematics department. But I think the serious courses at Princeton are in engineering.

IRV LUSTIG: Right. Which is where I used to sit. Let's see.

Chapter 9: Some Early “Stories of Practice”

IRV LUSTIG: What would you say is the most invalid criticism of optimization?

HAROLD KUHN: Invalid criticism. You encounter a certain amount of skepticism, or I have encountered a certain amount of skepticism in my consulting work that optimization theorists take into account enough of the institutional detail, so that the structure that's important to a practical person, that there's a built-in bias that you, as a mathematician in particular-- if you're identified as a mathematician-- or as a computer scientist, won't understand my real problems and will ignore many important aspects of it.

I think as the computers have gotten larger and the modeling has gotten more sophisticated, this is a totally invalid criticism. That is, if you'll tell me what your problem is, I'll model it for you. But time and again you encountered practical people who said, oh, you're from cloud nine. You don't understand what my problems are. And it's not even worth talking to you. I think that's, in my experience, been the most invalid.

Of course, at a certain stage-- I'll tell you a story along that line. This goes back to 1958-59. I was spending a year in London, and I was contacted by an airline pilot who is very high in the airline pilots' union who had heard of optimization theory and hoped that we would be able to give him some help with the scheduling of pilots' schedules. They're subject to a variety of complicated rules, so many hours on, so many off. And you'd like to satisfy their desires to have a home city where they can have a family and a life. And yet you'd like to conform to all of the complicated rules of the airlines.

Now in '58-'59 the computers were not capable of doing this. I could model it, but nobody could solve it. It was a complicated mixed integer program, in large part. And there just were no algorithms. And Gomory's integer programming had barely been invented. And branch and bound had hardly started, but there were no computers that could handle it.

And by now, I think they do scheduling problems like this on a regular basis. And I think the most astounding thing has been the progress in the computing power that's available. That's a real surprise. And I'm not sure I've even kept up with this. How can one?

IRV LUSTIG: The line I use is, the reason for our success is that hardware's gotten faster, our software and algorithms have gotten better, which is where we've made a big contribution. And then thirdly is the availability of data. So, you know, if you asked somebody to plan something before and you said, what is your current inventory level, they couldn't tell you. They couldn't tell

you how much it cost to buy a part.

HAROLD KUHN: That's right. I remember a consulting assignment for a manufacturer of many household products who were concerned because their inventory level had soared. And when we went into this production facility, we found out there was one woman keeping all the data in her desk drawers.

I mean, there was no real data collection anywhere. And she was sort of talking every afternoon, at the end of the afternoon, to the salesmen and to the production people, and doing most of the coordination by hand.

But all of the data was flowing through this one woman. There was no point at which anybody was keeping any data at all. So the first advice was, start keeping data about what your demand and what your sales figures are for various products. They'd merely noticed the place was bulging at the seams with over inventory.

And they didn't know what the hell was going on. She was just making very bad decisions on the basis of data which was totally inadequate.

Chapter 10: Symposium Zero

HAROLD KUHN: I have a story which won't fit in the sequence, but maybe you can add it in.

IRV LUSTIG: Go right ahead, because we will end up doing that.

HAROLD KUHN: I looked at George Dantzig's interview on your site, and he was talking about Symposium Zero, which was held in Chicago.

IRV LUSTIG: And that was 1949.

HAROLD KUHN: 1949. Exactly so. I took part in the planning session for Symposium Zero. As a graduate student, I sat in a meeting in Cleveland which contained Dantzig, Leontief, Morgenstern, Tucker was there. I can't remember who else was there. And the thing that-- Koopmans was definitely there-- strikes me from that meeting was that Morgenstern and Leontief got into an enormous fight over what comes first. Theory or data gathering? Theory or practical application?

And Leontief being much on the side of, he would gather national accounting data and make big matrices of input output between industries, and then do some mathematics after he'd gathered all this data. And Morgenstern was saying, no, that's dumb. You have to have the theory before you can start to gather the data and know what you're going to do with it.

And here I'm sitting there as a graduate student, watching these two giants scream at each other, till finally Koopmans calmed them down and said, we can do both. We can build theory and we can gather data at the same time.

And if you look at the volume, Cole's Commission Monograph Number 13, which was the proceedings of that Symposium Zero, you'll see that Koopmans' attitude is reflected. There are

papers on national accounting, Leontief types of papers. But there are lots of theory papers in there, too.

Chapter 11: The Future of Optimization

IRV LUSTIG: Well, actually, I mean, it's kind of interesting. One of the questions I had written down-- was what do you think there is in terms of the future for optimization, in terms of, is the growth going to continue in that way where there's continued work, growth in theory that will lead to applications, or applications leading to more theory?

HAROLD KUHN: I think there's been surprising vigor in the last decade due to the new methods for nonlinear programming, interior point methods of this sort. I see a great deal of vigor in the subject still, devising new algorithms which handle problems which hadn't been handled before.

I think we're nowhere near the limit of improvements in computers. Computer people continue to astound us by finding faster ways to do things. And there are exciting possibilities in quantum computing which may lead to fantastic leaps forward in the kind of problems we can solve which were considered impossible. I think the future is very bright. I don't think it's dead. The subject isn't dead yet.

There is something very exciting about the beginning of a subject, as you must understand. Those of us who were lucky enough to be in at the beginning come along and cream off a lot of easy results. And that's, as I said in Atlanta last August, that's being in the right place at the right time.

But I think there are loads of problems out there to be solved. Just using the computers that are available in the right way is a challenge for young people. It doesn't have the attraction of the great, pure mathematical results. I think the subject changes in its nature as time goes on. But I think that there are plenty of good problems still around to be solved, and the improvement in the computers will lead people to do some very interesting things in the future.

Chapter 12: Communication in the Early Days

IRV LUSTIG: Now you mentioned there about some of the excitement of being there at the beginning. And I know I heard your talk in Atlanta. Maybe you could convey, for the purpose of our interview, some of that for us.

HAROLD KUHN: Well, we were very lucky in Princeton to have a group of people, both in optimization and in game theory, who were very, very bright and working very closely together. Von Neumann is one who's been mentioned. Tucker's one. But we had young graduate students like Shapley and Nash and Gale.

And we had all kinds of visitors here who came through. Office of Naval Research was extremely generous in supporting our project from 1948 to 1972. I think that's the gas pipe company examining their pipelines. It's a helicopter that does fly through occasionally. I'll wait for this one to go over, too.

It was a period in which the community was smaller, so you knew almost instantly when

somebody had discovered something. Our relationships with the Rand Corporation were very close, and people went back and forth between here and there. The Institute had von Neumann and other visitors there. Kaplansky was a name that comes to mind, somebody who did work in game theory very early.

We had a weekly seminar in which it seemed like every week you heard a great new result that you hadn't heard before that somebody proved. And Abraham Wald would come down from Columbia to lecture. Von Neumann would lecture. And the group of young people were producing things, both in game theory and in mathematical programming that were absolutely fascinating. We had Phil Wolfe, of course, came to our project here after working with Dantzig in Washington.

There's an example of the typical kind of visitor. Alan Hoffman was at the Institute. That's where I first met Alan Hoffman. Partly, the world was smaller.

IRV LUSTIG: Well, it's interesting you say that, because in some sense, while it was smaller, because there was a small group of people working at the birth and evolution of the field, there was geographical dispersment. The Rand Corporation was in California. So what was the methods of communication? You're, saying the results, you'd know about results. But it wasn't like we had the email and the Internet to transport them all.

HAROLD KUHN: That's a very good question. People traveled. I made several trips out to the Rand Corporation and talked to people about game theory, or Lefschetz financed some trips for me out there to talk to people.

IRV LUSTIG: And the travel then, were there plane flights going across country then, or you had to go by train?

HAROLD KUHN: I actually flew. As of '53, you could fly. But it was uncomfortable and took a long time. The last time I took a train trip across the country was in the Army, when I got out of the Army in 1946. I went across the country by train. I did that several times in the Army, but never post-war, thank you very much.

Actually, coming to Princeton for graduate study in '47, I came by train. But I think-- I know I flew from Chicago to New York just as a luxury instead of doing the last leg by train.

It is a good question. Tucker and I developed the nonlinear programming paper by mail between Princeton and Stanford. I mean, we traded the drafts back and forth. And that's an extremely clumsy way to write a paper given the quality of the mail.

IRV LUSTIG: Well, were you-- I mean, there was the writing of the paper. Was the theorems and the work already proved, and it was just a matter of coming up with the exposition? Or you were actually working on the mathematics back and forth that way?

HAROLD KUHN: I've told this story several places. Tucker was on leave in Stanford, and he wrote to both Gale and me asking whether we would like to join him on a paper, really, on quadratic programming. And Gale had had it with programming. He wanted to go back to pure

mathematics and had an appointment at Brown, and he said no.

And I said fine. But I said to Tucker, I think we ought to really do nonlinear programming in general, rather than doing quadratic programming. We'd had some conversation in the summer before he went out to Stanford for the year, because I'd recognized the saddle point properties of the duality of linear programming, and had suggested in a little one-page note to him that I thought those could be generalized to nonlinear situations.

But by and large, I made one trip out in spring of 1950. I was newly married and wanted my wife to meet my parents, who lived in Los Angeles, so managed to boondoggle to take me on to California. But it gave us an opportunity to work on the paper then.

I think we framed the ideas in that visit. But then the actual writing, I think we had pretty much all the ideas together, in person together, and then drafts went back and forth.

IRV LUSTIG: And that was your primary mode of communication, because you mentioned earlier that phone calls, long distance calls were very expensive.

HAROLD KUHN: Phone calls were expensive. And they were a no-no. If somebody had a tragedy, if a child was sick or something, you might make a long distance phone call. But it was not a mode of communication. It's hard to believe today.

IRV LUSTIG: Well, I mean, you do. I mean, it's so fascinating to hear that here's this very key theorem of the field, and in some sense that it was developed in a bicoastal way when communication was much less instantaneous. We take so much for granted in today's world.

HAROLD KUHN: Al and I wrote, through the years, a number of joint papers. And we got along quite well in that regard. And I would very often do a first draft.

We wrote the Encyclopedia Britannica article on the theory of games together in 1956. And he was in Princeton and I was in Bryn Mawr. And we may have had a face-to-face meeting only once or something, in which we quibbled over did Emile Borel broach the theory of games, or did he start the theory of games, or did he make a contribution to the theory of-- fine points could be discussed in person.

IRV LUSTIG: But it's interesting, because in today's world, because of things like--

HAROLD KUHN: That's your tape.

IRV LUSTIG: Let me try to pause this one.

HAROLD KUHN: This table was being delivered in New York last Friday. And it turns out, I think I've exchanged five emails today back and forth between the people in Italy over when the table's going to be delivered and who's going to pay what charges. This is something which is literally incredible, to get mail delivered there and answered five times in a day.

IRV LUSTIG: So we were talking, before we changed the tapes, about at the time, that

communication essentially was via surface mail. And you would then send a draft off, and I guess have to start working on something else while you waited for an answer.

HAROLD KUHN: That's about it. That's absolutely right. It's hard to explain to a younger generation about carbon copies, mail instead of email. It's amazing the paper got finished. I know it was delivered at the symposium in the summer of '50.

And I was getting ready to go off for a year on a Fulbright in France '50-'51, and had just taken the flu shots for the trip to Europe. You took a whole battery of shots in those days before you'd go into the dreadful health conditions, sanitary conditions of Europe.

I was feeling very, very low from these shots. But I remember the galley proofs for the paper came from the Berkeley Symposium to be corrected before we got off on the boat in October. You know, just simply sending galley proofs across the country. Everything can go by electronic file.

IRV LUSTIG: Oh, yeah. I mean, I think still galleys-- at least I've seen, they'll send you the paper.

HAROLD KUHN: I suspect they still send paper.

IRV LUSTIG: I still get paper on that.

Chapter 13: The Traveling Salesman Problem

IRV LUSTIG: Earlier I had asked you about the most invalid criticism of optimization. I want to turn the question around a little bit and say, what would you say is the most valid criticism of optimization?

HAROLD KUHN: I think some problems are oversold. And I'll take one of which I'm very fond, and that's the traveling salesman problem. Now I've worked on the traveling salesman problem for over 48 years. I gave a lecture in Santa Monica in 1953, a symposium with the Math Society on applied mathematics.

And the title of the lecture was The Traveling Salesman. I know I gave it because people referred to it, and indeed Dantzig, Fulkerson, and Johnson, in their solution to the 49 city problem referred to it in one way or another.

At that time I thought, gee, this is really good applied problem, and a really serious and interesting theoretical problem. I don't believe it's either right now. I think the application-- I've cited applications. Laser drilling for computer boards, or design of lenses. These are the canonical applications that people are solving, 15,000 city problems now routinely.

But the amount of improvement they get over heuristics, and the quality of the data they have for these problems means they're not really going for a pure optimum. It's not making that much of an improvement. I'm a practical man. I might take dirtier approaches and get just about as much improvement as I need. I think sometimes exact solutions are over sold when approximate solutions will do. And that's a little bit of overselling.

IRV LUSTIG: That's a very interesting insight.

HAROLD KUHN: I think also that-- I love the mathematics of the traveling salesman. I still think about some aspects of it, and I try to contribute. But I think a valid appraisal is that it's had very little effect on any other areas of linear, nonlinear, integer programming.

IRV LUSTIG: Actually, I think there have been some things in the algorithmic sense, where-- I know, because I worked with Bixby-- there's been stuff that's he's done with Bill Cook, who's now at Princeton. They've done things there, where there's work that's been done and thought of in the context of the TSP, and we've carried over to the IP solver.

HAROLD KUHN: Some of these ideas do carry over. And I search for relaxed problems, which are good approximation to the original problem. General ideas. But those are very old ideas already. And the precise investigations of new classes of constraints for the traveling salesman problem has not really lent that much. Occasionally we oversell ourselves that way, but everybody sells their own field.

I think the other theme, though, is a serious one, getting the right match between the degree of precision that you need and the quality of the data. It's a serious one for practical people, and I think practitioners who are advising people should be very sensitive to that.

Chapter 14: The Nobel Prize and Concluding Remarks

IRV LUSTIG: Earlier you mentioned Nash, who was one of-- I guess was a student at the same time you were at Princeton. And he's won a Nobel Prize. So do you think that optimization should get a Nobel Prize, or is there some other recognition that it should receive along those lines?

HAROLD KUHN: Along the way to the Nash prize, I learned a great deal about the politics of the Nobel selection process. Among other things, I don't think optimization is likely to win a Nobel Prize in any foreseeable future. You know, the prize that he won, that Nash won was in economics.

And Assar Lindbeck, who was chairman of the selection committee for about 30 years-- not quite that long, but a major part of the time that the prize has been in existence-- has always considered that Dantzig's work, for instance, is mathematics and not economics. So he would never consider Dantzig for a prize in economics.

When Koopmans and Kantorovich were awarded the prize, Herb Scarf wrote a very angry article in the New York Times about how George should have gotten the prize. And I think that was the last chance optimization had. And in some sense, we made it. I mean, Koopmans and Kantorovich are very fine examples of optimization people. Not the same as George.

IRV LUSTIG: Would you say that there are any areas of optimization that are overlooked?

HAROLD KUHN: Overlooked, I can't say. I really have nothing to contribute on that.

IRV LUSTIG: OK. I think I've covered the things I want to cover. I guess I want to also ask if there are some things you want to show, like physical things you might have, artifacts or anything.

HAROLD KUHN: No. I've got a von Neumann prize medal. I've got a honorary degree from Bergamot with a beautiful gold medal, Colleoni, the condottiere who fought for Bergamot and for Venice, honorary degrees in this and that. But I don't think I want to show them. Thank you anyway. They're nice, but--

IRV LUSTIG: OK. And is there anything else that you'd like to add extemporaneously?

HAROLD KUHN: I think I'd really like to end with a sense that people who are able to do what they want to do, what they like to do, what they enjoy doing, are very lucky in this world. And if, by some odd chance, it also happens to be useful to other people, that's a little extra blessing.

I've had a very happy life, being free to do exactly what I want to do and pursue the problems I like. And if they've had a certain coherence and a relationship to an important field, that's very pleasant, but it was never why I wanted to do it in the first place.