

Chapter 1: Discovering Game Theory

IRV LUSTIG: OK, we're here with Philip Wolfe. And my first question for you is, how did you get involved with optimization?

PHILIP WOLFE: Ever since I was a kid, I wanted to be a scientist. For a long time, my principal interest was physics. I grew up in the Bay Area in California and lived in Berkeley. Shortly after I started college at the University of California. Physics was a big thing on the Berkeley campus in those days. The people on the Hill had a lot of Nobel prizes and were celebrated for any scientist that made an attraction. I worked away on that for a while.

But after I graduated, I looked over my academic career. I was astonished to find that I had effortlessly made a beautiful record in mathematics and had rather a struggle with physics. This partially decided me to go into the mathematical area. My interests were quite broad. I'd heard about computing machinery, and that sounded fascinating.

IRV LUSTIG: And this was in what year?

PHILIP WOLFE: Got my A.B. in 1948.

IRV LUSTIG: OK.

PHILIP WOLFE: Made a trip to Los Angeles where they had a copy of the Bush Differential Analyzer, and I took a course in that. Maybe nobody knows what that is.

IRV LUSTIG: No.

PHILIP WOLFE: That is a great big machine bit about 20 feet by 50 feet full of shafts, gears, and things. You set up the shafts to represent a variable changing in time connected with gears so that you can multiply it by a linear factor, put it into something called an integrator so that you can integrate the result over time, and in effect, do derivatives. You can plug in a large differential equation to this machine and let it crank out graphically the answers to it.

IRV LUSTIG: So in that sense, it was an analog computer.

PHILIP WOLFE: Definitely an analog computer. One was used very, very extensively during World War II, and probably the biggest strain on computing funds until they started with the digital machines.

IRV LUSTIG: OK.

PHILIP WOLFE: Well, I didn't pursue that very far. Actually my main interest in mathematics were kind of in what you call the foundations of logic, set theory, and metamathematics I found very fascinating, because they have the appeal of sort of getting down to the very essential ingredients of science. But I got distracted in 1949. I was a regular science fiction reader, and I have here a copy of Astounding Science Fiction magazine for October 1949.

IRV LUSTIG: Can you move back a little bit? And so there you're holding it up, and now I'm going to try to zoom in on it.

PHILIP WOLFE: It's a pity the cover's damaged. But I always had some inspiration.

IRV LUSTIG: Let's see. I think I can get it on that date. There it is. Perfect. So we see that it was published October 1949 for only \$0.25.

[LAUGHS]

PHILIP WOLFE: Yes.

IRV LUSTIG: OK.

PHILIP WOLFE: And this was the principle, almost the only outlet in science fiction in that time. There's a story here called "The Finan-Seer" spelled S- double E- R. It's not much of a story. It's about a group of college professors who learn a new subject called the theory of games.

And they use this to beat the stock market and build up the endowment for their university. I'd never heard of the theory of games, of this little tiny two-person, zero-sum game here used as an example. I was fascinated by this.

IRV LUSTIG: And so this is very interesting. This is the theory of games in a science fiction story in October 1949, and game theory was only a few years old at the time.

PHILIP WOLFE: Right. Yes. The Von Neumann Morgenstern book was published I think first in 1945 and then the second edition in 1948. I was an avid game player. Chess, Go were my favorite board games. I played a lot of ping pong too. But somehow the idea that there was a mathematical theory of games just absolutely fascinated me. I rushed out and bought the Von Neumann Morgenstern book and started studying it. Realized there was some real mathematics in this, and decided that that's what I wanted to do my graduate work in. Took me a while to find somebody who would agree that that was a reasonable topic. And there was just one guy on the Berkeley staff, Edward Rankin in the Statistics department who'd done a little work in linear programming. He was acquainted with the theory of games and he consented to act as my thesis adviser.

Chapter 2: Research for a Ph.D. in Game Theory

IRV LUSTIG: Now when you're saying he was acquainted with linear programming, this would be in what year?

PHILIP WOLFE: 1949. Maybe the early 1950s.

IRV LUSTIG: Now being in the West Coast and the activity that Dantzig was behind was happening on the East Coast, where you didn't have the internet or other ways of communicating, how was someone like that able to find out what was going on in linear programming?

PHILIP WOLFE: Rankin was basically an East Coaster. He took his A.B. at Princeton. Don't remember where he did his doctorate. But he maintained Princeton connections. At that time he was pretty well acquainted with George Dantzig and Albert Tucker and a number of the other figures in the field. Don't know what kicked him off in starting linear programming, but he was doing work in the mathematics of that. Taught a couple of courses in the Berkeley mathematics department. One was on convex cone sets and functions.

IRV LUSTIG: OK.

PHILIP WOLFE: And a couple of others on basic mathematics. I and a friend of mine, Dean Gillette, both enrolled as students of his. Dean did a very nice thesis in repetitive games, games whose play is followed by one of a number of other games. I forgot now the official term for that sort of things-- recursive games. It's a nice word there.

I chose a kind of recherche topic. Games of perfect information as created by Von Neumann and Morgenstern have, of course, stop rules. They don't go on forever. And when they stop, the payoff is determined. My background in more abstract mathematics and set theory caused me to pose the question of, what would happen if a game had no stop rule? Suppose you could play a game infinitely long? Of course, you'd have to have a referee decide who won when he saw this infinite sequence of moves. As far as set theory goes, that's not a problem.

I set out to investigate games of perfect information of infinite length. My thesis adviser was pretty sour about that. He didn't like the theory of games much anyway. He had some objections to the Von Neumann Morgenstern theory of utility that he felt was much too cramped and rigid to be a useful model of social behavior. I always tried to avoid discussions of that with him, but wanted to return to the mathematics, in which he wasn't very interested.

But anyway, I posed that question, got some preliminary results. Showed using a fair amount of set theory that games of infinite length and perfect information were not strictly determined, unlike games of finite length. That is, if I knew what your plan was, then I could win. And if you knew what my plan was, you could win, even though the games were supposedly of perfect information.

Much later in work that I haven't followed very well with that turned out to be something-- well, I proved that using the axiom of choice. It turned out, as I understand, the proposition itself was more or less equivalent to the hypothesis of the continuum. Anyhow, it's way out there.

IRV LUSTIG: OK.

PHILIP WOLFE: As I say, my adviser didn't like it very much. But I pursued it to the point also looking for subclasses of games that would have a value in a Von Neumann Morgenstern sense of numbered results. Then sometime in '53, I was startled to have a paper published by David Gale and a student of his in which they'd laid out the foundations of the theory that I was working on. And I thought I was scooped until I discovered that they said, well, now here's an interesting open question. And I had already answered that open question. So Rankin had to

agree that my thesis was publishable. And it was, and I did. And I took my degree in 1954.

Chapter 3: Degeneracy and the Simplex Method

Meanwhile, back in 1951, Rankin did something really lovely for me. I think I was running out of funds at the time. My atomic energy commission probably should have expired, and I was looking very hard for something to do in the summer of 1951. He contacted George Dantzig at the Pentagon and got me a job there for the summer. So I went from Berkeley to Washington. Met George, met the group of people at Project SCOOP, Scientific Computation Of Optimum Programs under the Air Force in 1951. And I worked there all summer.

George handed me a partially solved problem of trying to straighten out an algorithm for handling the problem with degeneracy and the simplex method. Degeneracy, you have a lot of zeros on the right hand side. Has a theoretical possibility of cycling when you use the simplex method and never terminating.

IRV LUSTIG: So this, to give a background, we actually did speak about this with Alan Hoffman. But to keep this recording independent, we'll say that this at the time, nobody knew that the simplex method would actually work, because there was a concern that it could go on and on forever.

PHILIP WOLFE: Right.

IRV LUSTIG: So the problem as Dantzig was giving it to Alan Hoffman as Alan told me was, is it possible that this could happen? And I think with respect to the question he was asking to you was, well, if it could happen, what could we do about it?

PHILIP WOLFE: Right.

IRV LUSTIG: OK.

PHILIP WOLFE: Alan answered the first question beautifully by presenting us with an example in which the simplex method would indeed cycle along without ever coming to a stop. My angle on it was to read what little had been written about it. And there was a paper of I guess a student or at least an acquaintance of Dantzig's who had proposed a certain kind of perturbation of the right hand side involving polynomials with little epsilons in them that was theoretically promising.

What I did, and using something I'd learned from my study of logic as a matter of fact, was to rephrase this argument in a very crisp, mathematical form using what is now known as lexicographic ordering of the elements with the simplex method. And that sort of put this proposal on firm footing and gave a method that made the simplex method secure. If you use this procedure, it would terminate after a finite number of iterations. So that was a great summer in 1951. Hoffman and I settled both ends of this issue. And of course, we met and we've kept up a friendship ever since that time.

IRV LUSTIG: Now when this occurred, the important questions were theoretical, to say, here was a method for doing it. Did the lexicographic method then end up being implemented in the Project SCOOP or in the coding that was being done at the time?

PHILIP WOLFE: I don't believe it did.

IRV LUSTIG: OK.

PHILIP WOLFE: But I don't know for sure. In my mind, it's kind of laborious. Quite a bit later, about around 1963 or so, it occurred to me there was a much simpler way of doing the same thing, which I'd never published. And I believe I called it the ad hoc procedure, which really does perturbation, but greatly shortens the work and actually is practical. It uses a number of computer routines.

IRV LUSTIG: Have--

PHILIP WOLFE: Oh.

IRV LUSTIG: Go ahead. Oh, go ahead. Please, go ahead.

PHILIP WOLFE: Back in Berkeley in 1951, I slowly put together my work on the theory of games, and included my work on degeneracy and linear programming as two parts of my doctoral dissertation. I got my doctorate in 1954.

Chapter 4: Al Tucker and Princeton

The other big sort of stroke of luck or nice thing that happened for my career was also due to Edward Rankin I think. He contacted Al Tucker at Princeton and recommended me for instructorship there. For several years Tucker and his associates had been very much interested in the theory of games. I was probably one of the very few potential instructors in the world who'd done anything in game theory at that time. So he hired me. And that was a terrific experience for me.

IRV LUSTIG: So this was at Princeton in, I guess, starting in 1954.

PHILIP WOLFE: Right. I was there from 1954 to 1957. At that point, as it happened, Tucker needed a right hand boy. Lloyd Shapley, who worked for him for a couple of years as an instructor. And Shapley, I'll mention in passing if it doesn't come back, I think is the second to von Neumann as most important in the theory of games so far. He'd left to go to the RAND Corporation. Tucker needed somebody to help out with a number of small tasks, and I happened to fit in that very well. So I worked quite hard for him for three years and developed a very long friendship that carried on to the end of his life.

Something that happened not far before the end was I think maybe for the first time I told Al what attracted me to game theory, namely the story in Astounding Science Fiction. And Al said, oh yes. The author of that story got the idea from a public lecture of mine that I gave early in 1949.

IRV LUSTIG: Oh.

PHILIP WOLFE: And Al said, as a matter of fact, I've got a copy of the magazine. Would you like to have it? And this is that copy.

IRV LUSTIG: Oh, OK.

PHILIP WOLFE: I thought that was kind of a wonderful--

IRV LUSTIG: Great way of closing the circle. Right.

Chapter 5: The RAND Corporation

So now in 1957, when you left Princeton, you went where?

PHILIP WOLFE: To the RAND Corporation.

IRV LUSTIG: And what was the group there?

PHILIP WOLFE: Dantzig was a big attraction. There were a lot of really good people there. Shapley, Gale visited for a year. Other names, possibly haven't gotten world renowned, like Oliver Gross, Selmer Johnson, of course Ray Fulkerson. They were all there at the time, and they made an extremely attractive environment.

IRV LUSTIG: Now, was it a pure research environment?

PHILIP WOLFE: Almost entirely pure. George Dantzig had, I think, the wisdom and the foresight not to rely entirely on the Air Force or on the scientific achievements, but had a lot of contacts in the industry.

And something I remember being kind of astonished by that sort of influenced my approach for a long time was this little story that George told me in the very early days about giving a speech before some consortium of oil companies in Los Angeles. At the end of the speech, being practically awarded a contract or being invited to this consultant to work for them, and told how great his speech was, and how little bullshit there'd been in it. Because he presented what he thought linear programming could do in the way of improving production in the oil refinery. And somebody said, well, how much of an improvement do you think you could produce? And George said, oh, somewhere between 1% and 3%. And the audience felt, well, here's somebody who is certainly not bullshitting us. We believe that.

IRV LUSTIG: Right.

[LAUGHTER]

Now what would you say was the motivation for RAND to form a research group at the time?

PHILIP WOLFE: I can't answer that question very easily. I was never that familiar with the whole structure of the RAND Corporation. It was set up as I understood it as a research group given a great deal of freedom to decide what sorts of problems would be in the national interest.

IRV LUSTIG: OK.

PHILIP WOLFE: And I think maybe due in large part to John Williams, who was director of the mathematics department for a long time. They felt that the theory of games had sort of an obvious potential, military application. And certainly the sorts of things that had gone on in the Air Force in the linear programming area in the way of improving the efficiency of operations could be of great general interest.

IRV LUSTIG: Would you say were there any applications during the early part of your career that you personally were connected with?

PHILIP WOLFE: There were several. To a large extent we, I think, acted as free consultants to people from some industries. The oil industry was, I think, the big earliest source of interest.

One connection I had for a couple of years was kind of fun was with the Kern County Land Company. They have a herd of-- I forgot the numbers now-- of the order of 10,000 to 20,000 cows that they use for meat and milk, and of course had to feed every day. They came from RAND, and having heard about linear programming, asked us if we could help with the classic diet problem.

Their diet problem was their dietitians had a whole set of requirements about how much salt, how much protein, how much carbohydrate, how much of each of a whole bunch of ingredients that their cow herds should ingest every week. And they wanted to go out in the market every week, take the current prices of the possible food stuffs, and make the best deal they could. So for about a year I was taking their weekly data, putting it into our computer, and giving them the answers about how they should treat the cows.

IRV LUSTIG: Oh, OK. OK.

Chapter 6: Linear Programming on Computers at RAND

PHILIP WOLFE: My own involvement at the RAND Corporation-- and again, I'll say what a wonderful place to work it was-- sort of the only requirement I was given was to do useful stuff, preferably in the areas of linear programming and game theory, even though that wasn't a strict constraint. And I took advantage of that to explore quite a few directions. I had a real fascination with computing equipment. At Princeton, I made a connection with the Institute for Advanced Study Computer project. That was the machine that was built from von Neumann's specifications. An amazing machine in today's terms. Had a total memory of one kilobyte of 40 bit words.

IRV LUSTIG: Did you remember the name of it?

PHILIP WOLFE: Maniac.

IRV LUSTIG: Maniac. Because I know there was a whole series of names.

PHILIP WOLFE: That was one name for it. Formally known as the Institute for Advanced Study Computer.

IRV LUSTIG: OK.

PHILIP WOLFE: And when you programmed for it, you took punch cards now called, unit records. Also, time has remembered them-- 80 columns by 12. And you punched your own program in binary on this card.

IRV LUSTIG: With a pencil, basically.

PHILIP WOLFE: No, no. There was a nice hole punch.

IRV LUSTIG: Oh, there was? OK.

PHILIP WOLFE: Yes, it had 0 to 12 on it. And every time you punched the card, it moves over a line.

IRV LUSTIG: OK.

PHILIP WOLFE: And that's how you wrote your program, fed it in the machine and see what happened. And it was a fascinating process, and even at that time, it wasn't very advanced. When I came to RAND Corporation in '57 I had to upgrade a lot of my understanding of computers. The IBM 701 that just appeared. There was one homegrown machine called JOHNNIAC that was one of the first transistorized computers. And I worked at that quite a bit.

IRV LUSTIG: Could you make any comments relative to the connections between the development of linear programming, game theory, and optimization, and the development of computers at that time? In other words, how much did math programming and optimization influence some of the developments that were occurring with computers?

PHILIP WOLFE: Oh, an untold amount of knowledge. I was never aware of any influence from mathematical programming to a computer. We certainly used a lot of computer time, and economically maybe that helped boost the industry. But I'm not aware that--

IRV LUSTIG: Well, even-- but even in that sense.

PHILIP WOLFE: I'm not aware that any design judgements were ever made to benefit work in mathematical programming. On the other hand, of course, you couldn't possibly do linear programming without a computer.

IRV LUSTIG: Right. Right. I mean, it's one of the true practical methods that comes from mathematics where people can say, look, it's saving the company money. What would you say, as you've lived through the history of the field, what project or event has made you proud to be part of the field?

PHILIP WOLFE: I can't think of any specific giant breakthrough. Oh. Well, I guess I can reply again with some personal reminiscences.

IRV LUSTIG: Sure.

PHILIP WOLFE: One bit of fun I had was deciding I wanted to really get into the computation of linear programming. What we had at that time was a pretty elaborate routine that was written by Bill Orchard-Hayes for the IBM 701. Very effective linear programming routine written in machine language. I wanted to use it and I wanted to get into its innards, and I absolutely couldn't understand it. It was way over my head, and maybe I was reluctant to learn that much machine language at that time to find out how it worked.

Bill had just quit when I arrived in 1957. I think at that time he went to then form the company CEIR, Corporation for Economic Industrial Research that later Les Ford joined. Any case, he'd left, and I was, to some degree, given the responsibility for trying to do something of carrying on the software effort in linear programming that George Dantzig was very interested in carrying on, but not something that he was going to do personally.

I worked on that code for a while, and then finally gave up. I'd heard about FORTRAN, and it seemed like a wonderful idea. As we now say, a higher level language. You could write something like mathematics and have it translated. Nudged the RAND Corporation into installing it and worked with it for a long time amidst the quiet sneers of my colleagues who were doing computing. They thought this was a very effete and probably very ineffective way to go about writing computer programs. I persisted anyway.

And one of my great moments was after putting together linear programming code that I thought was pretty good. Talking with friends at Standard Oil company of California in San Francisco who were bragging about an assembly language code they had that was very good. And I said, well, why don't you try FORTRAN? And I sent them the results of the test problem I'd run. They refused to believe it because I was getting a better time than their program was. But finally they checked it all out and indeed I was. That was a time of great pleasure.

IRV LUSTIG: Great.

PHILIP WOLFE: Winning a race like that, which was really kind of outside of my field as a mathematician was a real pleasure.

IRV LUSTIG: OK. So you mentioned earlier the application that you worked on with the cow feeding.

PHILIP WOLFE: Yes.

IRV LUSTIG: Was that your first practical application?

PHILIP WOLFE: Yeah. That was my first. And by golly, it was probably pretty close to my last.

IRV LUSTIG: OK.

[LAUGHTER]

Chapter 7: The Frank-Wolfe Method for Nonlinear Programming

PHILIP WOLFE: Almost all of my time was spent in trying to do algorithmic research, find ways to improve the simplex algorithm, do other kinds of programming problems. Probably one of the things that made it rather easy for RAND to hire me was work that I did in my last year at Princeton. And let me go back to this little anecdote. At Princeton, working on Tucker's logistics project, I and somebody else supported by the project, Marguerite Frank, started to look at nonlinear programming problems.

IRV LUSTIG: And this was about what time?

PHILIP WOLFE: 1955.

IRV LUSTIG: OK.

PHILIP WOLFE: Wow, there's so much history involved here. Let me go back a little bit farther.

IRV LUSTIG: OK.

PHILIP WOLFE: Bob Dorfman, an economist at Berkeley, had been interested in mathematical methods in economics for a long time and worked with Edward Rankin especially on the quadratic programming problem, quadratic objective function to minimize under linear constraints. And Dorfman used the then very new Kuhn-Tucker theory for nonlinear programming to show something that was really kind of exquisite.

If you wrote the Kuhn-Tucker conditions down together with the original constraints of the problem, you had a big set of linear equations that looked a lot like a simplex method tableau. As a matter of fact, you can show that the solution of the quadratic problem was an extreme point of this tableau.

IRV LUSTIG: OK.

PHILIP WOLFE: It was really kind of exciting. The trouble is, we never really knew how to find one of those extreme points. Unlike linear programming, we didn't have any regular way of getting there. Marguerite Frank and I devised a somewhat clumsy method of doing it, which has become known as the Frank-Wolfe Algorithm. And while it's pretty poor for quadratic programming, it's turned out to have a lot of applications in other areas where you have a messy objective function under linear constraints.

We submitted this for publication in the Journal of the Office of Naval Research, the Naval Research Logistics Quarterly. Alan Hoffman was an editor. At the same time, we received a manuscript from Harry Markowitz on portfolio selection and quadratic programming. And because these were both sort of similar subjects, he sent the Markowitz manuscript to me to referee, and he sent our manuscript to Markowitz to referee simultaneously.

It turns out when I was talking to Harry later on, we both initially did the same sort of thing. Looked at it, couldn't understand it very well, but it sounded like competent mathematics, so we turned in our referees report saying, publish this. Now I have some twinges of conscience about that that I think Harry didn't. I knew I hadn't really understood what he did.

And about a year later, I went back to it and tried to really figure out what was going on. Now I think I came close, but I was unfortunately struck by a creative idea at the moment. It sounded like he was doing something a little bit like the simplex method. And I wanted to see if I could model the steps he was taking in the simplex method.

Well, I never made a perfect model, but I made a version of the simplex method to solve a quadratic programming problem. And that was kind of a breakthrough at the time. I remember sending a copy to Dantzig at RAND. This was in very early '57. And I have his letter back saying, "this is a terrific result, if it's true."

[LAUGHS]

I gather you omitted reading the proof.

IRV LUSTIG: Right.

[LAUGHTER]

PHILIP WOLFE: But, hey. Anyhow, I think that was part of the reason I was very welcome at RAND, and I went to join there in 1957. So I worked on that algorithm and polished it some. The connection with Dantzig was terrific for me.

Chapter 8: The Dantzig-Wolfe Decomposition Algorithm

Another occasion a couple of years later, George would often called me in and just kind of rap about something that was on his mind. This day he told me about a couple of things, a couple of problem areas, something that Fulkerson had worked in, something else whose details I forgot. But in both cases, he was using the linear programming model and the simplex method on a problem that had a tremendous amount of data in.

And he in one case, Ray Fulkerson in another case had devised a sort of ad hoc method of creating the data at the moment that was needed to fit into the problem. After George told me about this, I reflected on it for quite a while. And it suddenly hit me that they were all doing the same thing of they're essentially solving a linear programming problem whose data, whose columns I would say, being the important part of the data, were just too many to ever write

down, but for which you could devise a procedure for creating one when you needed it, and creating the one that the simplex method would choose to work with at that moment. I won't drag you into more detail.

IRV LUSTIG: No, that sounds great.

PHILIP WOLFE: Call it the column generation method. The immediate lovely looking application of what was to a linear programming problem in which you had a number of separate boxes for separate linear programming problems connected only by a small number of constraints. That fit beautifully into the pattern. It was a way of decomposing such a problem, so we referred to it as the decomposition algorithm. And that rapidly became very famous.

IRV LUSTIG: Right. And it's known today as Dantzig-Wolfe decomposition.

PHILIP WOLFE: Right

IRV LUSTIG: And then even column generation methods today are still quite popular in applications.

PHILIP WOLFE: Yeah.

IRV LUSTIG: So with the number of these contributions, what would you say is the one that would you consider your main contribution to theory and practice?

PHILIP WOLFE: The two I just mentioned.

IRV LUSTIG: The Frank-Wolfe algorithm and the Dantzig-Wolfe--

PHILIP WOLFE: Well, not so much that as the simplex method for quadratic programming and the work on decomposition.

IRV LUSTIG: OK. OK. What people in the field have you admired and why?

Chapter 9: Al Tucker and George Dantzig

PHILIP WOLFE: Been quite a few of them. Let me start with Albert Tucker. As I said, I worked closely with him for three years and then maintained a lifetime friendship. He's almost the only person I know who I can say was consciously and rationally good. He had somewhere hanging up in his head a code of ethics, a very nice one, and he observed it.

I don't have a lot of anecdotes to give. You'd have to sort of know him and work with him to see it in operation. But you could feel with it. If he made a decision about something, that decision had nothing to do with the interests or rogue passions of Al Tucker. But with whatever institution or cause he was working with, it would be in its best interest.

IRV LUSTIG: Wow.

PHILIP WOLFE: Really a really remarkable guy. And there are a lot of us who I think would pay him that kind of tribute. I know last August at the Founders Awards Ceremony-- where you there?

IRV LUSTIG: Yes. Yes. That's what gave me this idea to do these interviews.

PHILIP WOLFE: Do you remember Harold Kuhn's talk?

IRV LUSTIG: Yes. Yes.

PHILIP WOLFE: And at the end of the talk he gave a tribute to Tucker and he started to cry.

IRV LUSTIG: Yeah, it was very moving.

PHILIP WOLFE: I think that we all felt that kind of affection for him. But jeez, so many other people. George Dantzig, our acquaintanceship has been very long. At times, a little bit stormy. But generally like, say, in doing the decomposition work we've been sort of a perfect pair. George is a genius.

IRV LUSTIG: A modest one, at that.

PHILIP WOLFE: A modest one. But he has real originality.

IRV LUSTIG: Yes.

PHILIP WOLFE: It seems to me when he wades into a problem, he's-- and I mean this sincerely-- unencumbered by the kind of baggage a lot of us are. He'll sit down with the problem and let the problem speak to him in his own language and he'll go to work on it. I, on the other hand, I had a wonderful mathematical education. And when I sit down on a problem I start saying, well, what is there in my known bag of tricks that I can apply to this? So George tests new ideas and in the collaborative work we did, I was the guy who put the polish on it and put it in really nice mathematical form.

IRV LUSTIG: Well, it's interesting that you describe it this way. Because your interactions at the time, which was 1960, '61, I think was the decomposition work, and later, he was my thesis adviser 25 years later. And I would describe the experience the exact same way.

PHILIP WOLFE: Yep.

IRV LUSTIG: Very similar way. Because it was even then, I thought this was an evolution. But here you're describing it as this is how he had always been. It's very interesting.

PHILIP WOLFE: I'm sure of it. Yep

Chapter 10: Looking Back and Looking Forward

IRV LUSTIG: If you go back to your past and start all over again, do you think you would choose

the same career?

PHILIP WOLFE: Yes.

IRV LUSTIG: So where do you see our field going?

PHILIP WOLFE: Gee, that's a tough one. I've seen our work in optimization become sort of an integral part of business planning, of industrial engineering, a very solid part. I can't imagine this ever going away. Sometimes we wonder why it hasn't become more widespread, why more people aren't using it.

IRV LUSTIG: Well, what do you think-- I had this as a question here as, what would you say has held optimization from being more widespread?

PHILIP WOLFE: I don't have a firm opinion on it. It might have reached its natural limits of growth. It may be handling almost all of the problems that are really worth handling with these particular tools. I haven't a lot of consultant work here, but when I have it, I've rarely found an area where I felt that there was tremendous gap that we could just wade in and fill. Seems to me that enough people nowadays know roughly the capabilities of optimization. There aren't a great deal of missed opportunities.

IRV LUSTIG: How do you describe optimization to someone who doesn't know what it is?

PHILIP WOLFE: I give an example like cow feeding via the diet problem. Or a little more obscurely I refer to twiddling all the bells in the refinery so as to get the maximum output. And then saying, what you need are the equations that describe the operations of the refinery, and then you need the mathematical method that will tell you how to set the numbers for those valves in such a way to give the function its best possible value.

IRV LUSTIG: OK. What would you say is--

PHILIP WOLFE: Cocktail parties are hell, though.

[LAUGHS]

IRV LUSTIG: Well, I have some stock examples that I use that seem to give people the picture of things, to recognize that it connects with daily life and that things like airline fleet scheduling or crew scheduling. That, you know, what made that reason you flew on that plane was because optimization decided it should be that plane, and usually then that gives them a connection to how things are going. What would you say is the most invalid criticism of optimization?

PHILIP WOLFE: I'm going to have to pass on that one. I haven't heard any criticisms for a long time.

[LAUGHS]

IRV LUSTIG: Well then, let's turn the clock back. What would you say were the criticisms that

you were hearing as the field was developing?

PHILIP WOLFE: Oversell. The number of operations research practitioners just promising the world, and to think that they could revolutionize the business if they had the right tools, and then finding out that you really can. To me it was a little bit more dramatic in game theory. I think there was a time when a number of us felt it was really going to revolutionize life. The military would use it all the time. So would businesses. It could kind of drive society. On the whole, it's been a flaw.

IRV LUSTIG: And why do you think that's so?

PHILIP WOLFE: Because it can't answer enough questions. You do a great job on a zero-sum two person game, but that's not what life really is. Life is a very large number of person non zero-sum game, and people were still twiddling with solution concepts for that and still working on the theory of trying to get some rationality into it, and it hasn't been done.

IRV LUSTIG: Then that's with respect to game theory, but would you say the same things, types of things apply with regards to linear programming and other areas of optimization?

PHILIP WOLFE: I don't think so much. I think probably most practitioners are really aware of such limitations there are and how to use them, on how to use the tool. As I said, I think there was perhaps a serious period of oversell in the operations research community. But my sense is that's died off now. We kind of know how to use the tool.

IRV LUSTIG: OK. Do you think that there's some valid criticisms of the technology?

PHILIP WOLFE: Oh gee, there must have been, but I don't have any stories that come to mind.

IRV LUSTIG: OK. OK. Would you say that there are some misconceptions then, about optimization that exist?

PHILIP WOLFE: Again, I don't think there's anything serious.

IRV LUSTIG: OK. OK. So in some sense, you're saying time has made people learn more of what are the limits and realities of what we can do?

PHILIP WOLFE: Yes.

IRV LUSTIG: OK. Let's see. You have anything else you want to add maybe? Some stories and this and that? Because I actually, I love the way this went. I really am happy with it, where you really gave good flavor of what was going on. So I'm going to offer the opportunity for you to tell any other anecdotes.

PHILIP WOLFE: I think I've told you all of them, all the ones I had prepared in advance.

IRV LUSTIG: Oh, OK.

PHILIP WOLFE: And let me just summarize I guess by saying, I am very pleased to have been there, to have shared in that excitement. The idea that the theory of games might spread over the world and solve all our economic and social problems, that was terrific. That was really inspiring.

Oh. Oh, that reminds me. Just a one time anecdote. When I showed up in the summer of 1951 at that job at the Pentagon, first thing I did was to go around for a couple of days and have short interviews with the other people in Project SCOOP. And I remember going to the office of one economist and introducing myself and wanting to sit down, talk to him. Well, he didn't want me to sit down and talk. And what he said was, look. I'm making a detailed model of the American economy. So you're going to have to excuse me, because I'm terribly busy.

[LAUGHTER]

We thought it could be done, I think. We had that feeling. It was very exciting. The theory of games, linear programming, and computers sort of all coming online at the same time was really a strong motivation. And it was a lot of fun.

IRV LUSTIG: And you think you if you rewind the clock, you'd relive it in the same way? Or want to?

PHILIP WOLFE: There are four or five results now. If you'll let me keep my present mind, I'd like to get them in print before somebody else does. But I think that's the only change I'd make.

IRV LUSTIG: OK. All right. Well, thanks a lot. This has been great.

PHILIP WOLFE: Thank you.