Chapter 1: First Job at National Bureau of Standards

IRV LUSTIG: So we're here with Alan Hoffman, one of the founders and trailblazers of optimization.

ALAN HOFFMAN: What a nice statement.

IRV LUSTIG: And so I'd like to start and ask you, how did you choose optimization as a career?

ALAN HOFFMAN: Well, actually, I didn't. In a certain sense, it chose me. Shall I tell you how I got into the subject?

IRV LUSTIG: Absolutely.

ALAN HOFFMAN: I had graduated from Columbia in 1950 and spent the next year at The Institute for Advanced Study with the anticipation that after that fellowship was over, I would get an academic job. But it turned out that was a bad year for academic jobs and I got the notion that maybe I could work for the government. The reason I had that notion was Marshall Hall. I don't know if you know the name. He was a professor at Caltech for many years.

He had come around recruiting for The National Security Agency and boy, the idea of working for Marshall really appealed to me. But my wife said, listen, if Marshall Hall is coming around recruiting, there may be better places that don't have to recruit. Wise. And I found out about The National Bureau of Standards, it is now known as The National Institute of Science and Technology, NIST. In those days, it was NBS. And I wrote, asking if they could be interested in me even though I knew no applied mathematics whatsoever. I think I wrote on a Thursday and on Friday, I had a call offering me a job, which parenthetically, I remark would never happen any place in the world now. But in those days, people, even government employees, move with abandon and confidence.

So I was hired and I thought they must have been terribly impressed by my resume, but in fact, what they were really impressed by was that I was breathing. They had a contract with the Air Force to do good work in linear programming and they didn't have enough people to staff that contract. So any reasonable person who applied would have been hired. And I was a reasonable person.

So I came to Washington. I had never heard of linear programming before. In fact, nobody had heard of it. But the circumstances were that at the Bureau, first, their computing machine called SEAC which, of course, is long dead, had been built mostly with the sponsorship of the Air Force. And secondly, there was a continuing contract between the Air Force officer of the air controller, which is where George works--

IRV LUSTIG: Now this is what year?

ALAN HOFFMAN: Well, I don't know when that--

IRV LUSTIG: I mean, when you were first at NIST.

ALAN HOFFMAN: '51.

IRV LUSTIG: 1951.

ALAN HOFFMAN: And that contract provided that the Bureau would do good work in computing and other things for the Air Force. So that's why I was hired and a number of other people as well. And so I met George. I met Emil Schell Alex Orden, and George's whole crew. And they came over, and explained to me what linear programming was, and how useful they thought it was going to be, and so forth. I was absolutely dazzled.

Not, I should say, by the novelty and brilliance of it, but by their incredible ambitions that they actually had developed, or were developing a mathematical tool which would really be useful in planning. I thought, these people must be out of their minds.

IRV LUSTIG: Now at that time, how were they describing those ambitions that made it seem so exciting?

ALAN HOFFMAN: Well, they describe the background of it in Air Force planning. And I think there was already some use in petroleum refineries, I'm not sure. I don't know when Abe and Bill entered this subject.

IRV LUSTIG: That would be Abe Charnes and Bill Cooper.

ALAN HOFFMAN: Bill Cooper. Exactly.

Chapter 2: Cycling and the Simplex Method

And there were certain models that they were working on. Arranging for building a certain number of Air Force wings over a period of time, and considering the requirements in purchase of aircraft, training of crews, training of teachers for the crews, and so forth. I didn't know anything. And George gave me the Activity Analysis of Production and Allocation book to read, and I read it, and then he discussed with me the question of degeneracy. And he said the proof that the Simplex Method works depended on a assumption of non degeneracy. That is to say, speaking semi-technically, that certain matrices were of full rank rather than of rank less than what you would normally expect.

It's analogous to this situation. If you're looking at a polyhedron in space, in general, you would expect that a vertex to that polyhedron would be the intersection of three planes. But it might be the intersection of four or more planes. By accident, some planes might have come together, more than three in order to generate that vertex. And if you look at the algebraic analog of that situation, it might lead to the possibility that the Simplex Method, which George had developed, would not work. It might cycle. You might repeat yourself.

Anticipating this, George and other folks had developed devices to prevent this occurring of cycles, but they didn't know whether it was necessary yet. So he poses the question to me, could it happen? And I worked on it for a couple of weeks. On Mondays, Wednesdays, and

Fridays, I was always sure it couldn't happen. On Tuesdays, Thursdays, and Saturdays, I thought it might happen. On Sundays, I read The New York Times. But eventually, I found an argument to show that it could happen. That impressed George, I think, because it was something he hadn't done, nor anybody else. And in addition, my example was phrased trigonometrically.

To be rigorous about it, I had to put down formulas and show four of the entries of matrix. So I did. But the formulas were capturing something which I had visualized. And it was useful to write these formulas in terms of sines and cosines of some angles.

IRV LUSTIG: Right. Now I remember this example because its in George's book, right?

ALAN HOFFMAN: Yes. Actually, I didn't think anything of it particularly, but a few years afterwards, people asked me to write it up. So I wrote it up as a report, of which I have a copy. It's in Saul Gass's book and other people's books.

IRV LUSTIG: To get the picture of the significance of this is that at the time that we had the Simplex Method, we did not know that it would work. That it would actually ever terminate because it wasn't sure--

ALAN HOFFMAN: Yes. This is as a theoretical question.

IRV LUSTIG: As a theoretical question.

ALAN HOFFMAN: Whether it would terminate.

IRV LUSTIG: And the question was, well, maybe there's no problem out there where this would occur and you discovered the first problem that would cause the cycling?

ALAN HOFFMAN: Yes. There was also a theoretical question, they had a way of overcoming this phenomenon, if it did occur. The issue, which was tantalizing and titivating, was could this phenomenon of cycling recur. So I showed it could and that was fun. And eventually, it appeared in various people's books. I only wrote a report about it.

IRV LUSTIG: Now there was also some computational work that you were involved in.

ALAN HOFFMAN: Yes. I will come to that presently. But I do want to make a comment concerning-- I found the first example, other people found other examples. A couple of years ago, Jon Lee wrote an article in the SIAM journal trying to recover and, I think, succeeding in what my mental processes were at the time. Since my write up occurred long after I thought of the example and people asked me, how did you think of this? And I couldn't really reconstruct it, but I think Jon succeeded.

Now getting back to cycling. There was a lady named Betty Jane Gassner, who some years later had found cycling for the transportation problem, a much more difficult question. And I was very impressed by that. She was really very bright. And her career did not flower for various

reasons and I sort of have big regrets about that. End of that digression. Now we're moving on to what my jobs were.

Chapter 3: Evolution of the Hoffman-Wielandt Theorem

At the Bureau, there was a lovely atmosphere. So that I think of that period as golden days and I think people who weren't there on Saint Swithin's Day should regret it. We were a happy band of brothers and everything was good. I had, in particular, the advantage of having a boss named John Todd and a kind of mother superior named Olga Taussky, who was Jack's wife. And Olga sort of looked after the young mathematicians, and taught us the culture of the subject, and encouraged our careers. And Jack was the boss. He was in charge. He was very fair, and friendly, but firm. He was just wonderful. For example, there were certain times when we had a disagreement. I thought one thing, he thought another. But we had the disagreement openly and we remain friends. He was really very nice.

So the contrast was that Olga treated me as if I were a talented kid, which, in fact, I was. And Jack treated me as if I were an irresponsible adult, which was also true. And in fact, I think both things are probably still true, to some extent, today. Anyway that was a marvelous period. And one of the things which happened, which was very influential for me, is that I found there were two ways in which linear programming was relevant to things other than planning.

And one was an estimation of eigenvalues of a matrix. We had a visitor named Helmut Wielandt a German mathematician, whose major interest was in group theory, but who had, during the war, become interested in numerical analysis, numerical calculations because the German government had discovered he was a scholar, and had taken him off his motorcycle, and put him to work in a laboratory. And Wielandt and I contemplated a problem which other people had looked at. Namely, you have two normal matrices here-- unavoidably, I have to be technical. And the question was, could you estimate how distant the eigenvalues of one matrix were from the eigenvalues of the other, in terms of how distant the two matrices were from each other?

Now of course, these terms-- well, we have technical meaning, but it was an interesting question. And what was interesting was we solved this problem. And we solved this problem using linear programming. That is what we used as a fundamental part of a proof, was the fact that every doubly stochastic matrix is the convex hull of permutation matrices. Or to put it in a gentler way, if you look at the subclass of linear programming problems, which are called transportation problems, then if the data consists of integers, then the answer won't be fractional. It will be integral. And we exploited this fact in order to prove this theorem on estimation of eigenvalues. This theorem has its own life. It's known as the Hoffman- Weilandt Theorem and there are people who only know me through this. They don't understand anything else I've ever done.

So that was a thrill for me because I saw that you could use linear programming for other parts of mathematics. And in fact, if I may digress further, over the years, I returned to this subject of estimation of eigenvalues. And I do very well in it compared to my competition, if I may speak in all honesty, because I know the concepts of linear programming better than they do.

Chapter 4: Duality and the König-Egerváry Theorem

Then the other connection was linear program with combinatorial problems. I had difficulty understanding the duality theorem.

The duality theorem is a fundamental theorem in the subject. It says that two linear programming problems whose data are related in a certain, technical way, have their answers related in a certain way. And this theorem is frequently explained in textbooks, it was explained to me by George, in terms of the following artificial problem. You want to buy food in order to satisfy the requirements of nutrition. You need so many units of protein, so much vitamin A, so many carbohydrates, so on and so forth. And each kind of food you buy has a certain fraction of it composed of nutrients of the different types. And subject to the restriction of satisfying the nutritional requirement, what's the cheapest diet, the cheapest way you could satisfy this?

In fact, this was a classic problem. A famous economist named George Stigler had written about this.

IRV LUSTIG: When I met with George, he of course talked about the diet problem as one of the first problems that was solved.

ALAN HOFFMAN: Yes. And it was solved by people working with hand computers, which is an interesting-- And then there is the dual problem and this has the following interpretation. You have a drug manufacturer who is going to be selling you pills corresponding with the nutrients. And the question is how we should price these pills in order that he should make as much money as possible, but people shouldn't feel that their paying more for the pills than they would pay for the food. And then he introduced, or some economist introduced the notion of a shadow price, all of which I found, you should pardon the pun, penumbral. I mean, I didn't really understand this concept at all.

But I found out that the-- in fact, I noticed without really breaking a sweat that the so-called König-Egerváry Theorem does follow from the duality theorem. Now I will remind you-- although, of course, you remember very well from my class. Irv and I were together at Stanford and I taught him this material.

If you have a matrix, that is say, some array, a rectangular array of zeros and ones, and consider the following two questions you might ask about this rectangular array of zeros and ones. The first question is what is the largest number of ones you could select from this matrix in order that no two are in the same row or column, but you get the largest number? That's a maximization problem. And the dual problem, in the sense of the duality theorem, to which I alluded previously, would be the following. You want to choose the smallest subset of rows and columns which collectively contain all the ones in the matrix. I could draw a diagram on the board, but I don't write very legible as Irv can certainly attest.

So the famous theorem of König-Egerváry was that those two numbers were the same. The smallest number of rows and columns which together covers all the ones in the matrix is equal to the largest number of ones, no two of which are in the same row, or column. And I saw I could do that by the duality theorem. And that was a tremendous thrill for two reasons. One is--

and I wanted to study the duality theorem. This pill manufacturer was mythical, but in the case of this combinatorial problem, both the two problems-- the primal problem, the dual problem, each were vigorous, virile, if you will forgive the expression. They were manly, they weren't retired. So that was reassuring. And I really learned what the duality theorem was when I thought of that proof.

And the other thing was-- I said, my goodness, I can do a lot of combinatorial things using linear programming, which in fact, I proceeded to do. So in those days, besides working on actual problems and in particular, investigating how well the Simplex Method in comparison with other proposed algorithms for solving these problems, the extracurricular interests were in two directions. One was in estimate of eigenvalues and the other was in using linear equalities to prove combinatorial theorems. And my interest in the latter combined with Harold Kuhn and David Gale and Al Tucker as sort of the birth of what later became identified as a subject called polyhedral combinatorics. I also had that Jack Edmonds came to the Bureau after I did and, of course, he did absolutely brilliant work in the subject. So that's how that subject started, really.

Chapter 5: Computational Comparisons with the Simplex Method

IRV LUSTIG: So now turning onto some of the practical things--

ALAN HOFFMAN: Well, excuse me. You asked about computing and I give you-- you can even keep it. I'll give you two things, one of which is embarrassing because it's so poor. This is a copy of that paper which we wrote on comparing the Simplex Method with other stuff.

IRV LUSTIG: We've got it in the light?

ALAN HOFFMAN: You can keep it, if you wish. The other thing--

IRV LUSTIG: Go ahead.

ALAN HOFFMAN: I give you absolutely without shame.

IRV LUSTIG: We'll take some pictures later.

ALAN HOFFMAN: I organized one of the early linear programming conferences. Not the zeroeth, and not the oneth, but the twoth. And it was in the early '50s Bureau. And at that time, I had been in charge of the computing work, which we were doing with the simplex code that had been written under Alex Orden's direction. So in the sense of supervision, I have probably supervised more actual calculations than anybody in the world, with the possible exception of Ragnar Frisch, the Norwegian economist, who had basic ideas about the interior point methods without working on a rigorous theory.

So when I organized this conference, I felt obliged to speak myself and I also felt obliged to speak about how wise I was concerning techniques for computing the answers to linear programs. I am so ashamed of that paper, you have no idea. I have looked at it. All of the predictions I'd made about the future were wrong, all the comments about the present, were

naive. I confess. I keep it to remind me of how dumb I was, at least in that area. I mean, I didn't have a flair for computing at all.

IRV LUSTIG: But let's speak a little bit about the paper, when you were comparing the different methods. I think I once heard a comment, maybe from you directly, if it wasn't for that paper, we might have just disbanded the Simplex Method right away.

ALAN HOFFMAN: No. I don't think I said that. This paper-- there were an enormous number of requests for reprints and this was the days before people had duplicating machines and so forth. And I couldn't understand it. Harold Kuhn said, it's not that they're so interested in what your results are, it's just that there a lot of places that are writing their own Simplex codes and they want to have answers to problems to make sure that their codes are performing correctly. Yes. But it got a tremendous number of requests for reprints.

It also has some celebrity in the Mathematical Programming Society, which has a subcommittee on algorithms. And they established something, which is quite sensible. Criteria for what articles on computation should contain.

IRV LUSTIG: That's right.

ALAN HOFFMAN: And they always cite this as a noble example because we gave so much information. The reason we gave so much information is we had accumulated so little that we felt obliged to say everything we knew. Nevertheless, I think we designed some reasonable experiments and I think we said what we knew with some grace.

Chapter 6: An Early Combinatorial Auction

IRV LUSTIG: Were there lines of study, at that time, that didn't work?

ALAN HOFFMAN: That's a good question. Let's see. In terms of mathematics, almost nothing works. For your audience, I will tell them the secret, which is one of the secrets of the profession. A mathematician lives in constant failure. 98 % of the things you try, fail. But you haven't made a big investment in your laboratory, or equipment and so forth, so you just let it pass by. But you live with the failure of your ideas so constantly that it doesn't bother you the way failure would bother you in other professions. So are there lines, which didn't work out? Oh, countless, but it's hard for me to remember them.

Actually, the earliest problem, which is still on my list of unsolved things which I really would like to do, but haven't, is not more than 20 years old. The stuff going back then, I either forgot, or-- there was one interesting experience I had in terms-- oh, here is something which I think is germane. One of the projects on which we were engaged arose in connection with the armed forces. There was, at that time, an organization called The Armed Services Textiles and Apparel Procurement Agency, ASTAPA, whose functions later went back to the quartermaster core. At that point, it was a joint effort of several services. And their job was to buy stuff for the services.

So I'll give you an example. Suppose the army felt it need 25,000 tents. And so they would issue an invitation to bid and the manufacturers would respond. And then this organization had

the responsibility of selecting the bidders with the least cost to the government. Now it was a little bit complicated for the following reasons. They needed these tents in different quantities at different warehouses around the country. And the cost of the shipment from the manufacturer to the warehouses was borne by the government. Secondly, the manufacturers, in general, would quote a price, but they might quote different prices to different places, if they wished.

What was more common is they would change the price per tent, depending on how many tents you buy. Oddly enough, the price would up with increasing quantity rather than down, which offended me, as a citizen, but pleased me in terms of computing because that made it possible to use techniques of the transportation problem, which a decrease in cost would've made more difficult. But occasionally there was decreasing cost.

They also had sometimes a restriction-- I'll make 15,000, or none. Or I'll take everything, or none. Or I'll take it most-- the common thing was, I'll take most, so and so, and here's the price. Well, someone had heard about us and someone else had realized that this was, in principle, a transportation problem, so we went up and worked on this. And there was a sad period during which we couldn't find out anything we wanted from the accountants who were in charge of this work because they sensed, I think, quite properly, that technological unemployment was around the corner.

There was one guy who helped us out and with his assistance, we found out what the problem was. And then entered the period where we were engaged in the social competition with the accountants. Our object was to satisfy the legal requirement for awarding the contracts at least cost to the government and so we competed with them on 10 bids. And roughly speaking, on two of them, everything was the same. Same amount for each manufacturer-- they solved these probably by an informal iteration. Accountant A would do the best he could, then he'd pass the result onto Accountant B, who would want to see if he could beat Accountant A, and so forth. That was until they converged.

We were using the Simplex Method. So we knew we were right. Given the assumptions about car load lots, that the rate-- what the freight rate would be. Now on the other contracts, we saved the government some money. You may ask how much. Well, on contracts of the order of several million, we might save $100. Of the order of $1 million, we might save $10. It sounds ludicrous. But on the other hand, the bidding was very close, so it might have made a big difference to the manufacturers. But in terms of the cost to the government, it is, by every standard, negligible. Still, we were satisfying the legal requirement, doing it at the least cost for the government, and it was some publicity for us, and some publicity for the government agency. And so we did it. We kept on doing it. I don't know at what stage that work is now.

Technically, there's an interesting aspect of this, which I didn't appreciate at the time. These restrictions that the manufactures imposed are very analogous to what occurs in so-called combinatorial auctions. Do you know those?

IRV LUSTIG: Yes.

ALAN HOFFMAN: They're identical. Now furthermore, the techniques which we used were an informal kind of branch-and-bound. And at certain stages, we said to ourselves, these are nice

rules we've concocted. Sometimes we take a chance that the linear programming answers is going to satisfy the restriction. And sometimes we win and sometimes we lose. And had we bothered to cognify the games we were played with the data, we would have been the creators of branch-and-bound, which I remind the lay part of your audience is one of the techniques for solving a very hard class of linear programming problems, those in which a variable has to be a whole number. Or more commonly, those in which you either do something, or don't do something. You don't say do it 70 %.

IRV LUSTIG: Half way.

ALAN HOFFMAN: That's an interesting project.

Chapter 7: Solving Linear Programs on SEAC

IRV LUSTIG: Now in this project, obviously there was a data collection aspect in formulating the problem, but then now you have to go about and solve it. Were you using computers, or was it done by hand?

ALAN HOFFMAN: Certainly, we were. We were using this computer, the SEAC, which we had at the Bureau of Standards. This machine is now dead. It had a 1024 memory. That was all the memory we had. Some of it was mercury tubes, the other were Williams tubes, cathode ray tubes. It was not very fast. It was a four address machine, there was no high level language. You wrote-- and it was a significant game to save one order. And the it was a kind of competition the programmers had, is to have-- now the way we did the Simplex Method, and I have nothing to do this, it was all Orden was that we had these two tapes. And you'd read in the last iteration, from the tape into the computer, then you would do your transformation, then it would read out on the other tape.

The tapes were not rigid. They were in these bays behind glass fronts. And they just descended in crinkly fashion. You could watch them for hours. To see the different shapes that the metallic tape would take, just beautiful. You could also, since the machine was sufficiently slow, actually watch the iterations taking place. See the flashing red dots, and so forth, and know when the cycle of calculations was reaching a certain stage. And it was loads of fun.

An interesting anecdote in connect with that period is a group, not the optimizing group, but another group, was doing the calculations for Los Alamos. And every night, the person who was doing the calculations would take off the teletype. The input and output were on teletype. And roll it up, and put it in the mail, and send it to Los Alamos. One night when the programmer came in, he discovered that we didn't have any teletype paper anymore. He didn't want to lose the night's work, so he went to the bathroom and put a roll of toilet paper-- these calculations were printed on the roll of toilet paper and mailed to Los Alamos the next day. I think this fellow's name is Charles Swift and I don't know whether he ever got a special medal for it, but he surely should have. I cannot tell you how much fun it was to be there.

IRV LUSTIG: Well, that's one of the reasons I wanted to interview. It was for you to really--

ALAN HOFFMAN: The site was beautiful. The programmers with whom I worked were such

dear guys. Sol Pollack, Leon Gainen, Frank Meek, Bill Hall. Really wonderfully-- I remember when I had this cycling example. They wanted to see if it worked on the computer, too. And finally, Sol Pollack did it. And he said, Alan, it's going crazy. It's repeating itself. Now the funny thing about that code, which will actually shock you, is that it was not coded for sparseness. We transformed the whole tableau in every iteration.

IRV LUSTIG: They do it with the Revised Simplex Method. You weren't using that yet.

ALAN HOFFMAN: No.

IRV LUSTIG: Because that was done by Orchard-Hayes.

ALAN HOFFMAN: Yes, he put it in, but, of course, the ideas were known before. Let's assume they didn't know it initially, but they certainly knew it after. Now I'll tell you some regrets I have about that period, which is on the basis of entering the subject at a relatively early stage, though not in the first chapter of Genesis. I got to know all the players except one. And that was Frank Hitchcock. Now there's something called the Hitchcock-Koopmans transportation problem.

And everybody knows Tjalling Koopmans. He was a very fine fellow, who developed his version of the transportation problem in some secret memorandum, which he wrote during the war where some shipping administration, which was concerned with getting all the shipping around the world on time and in the face of obstacles, like submarines--

It takes some intellectual effort to convert what Koopmans wrote into the transportation problem that you and I know and love. On the other hand, earlier than that, Frank Hitchcock, who was a professor at MIT-- I think not necessarily in mathematics. Possibly in some other subject. He had written in the MIT Journal of Math and Physics, surely not an obscure source, a description of the transportation problem in exactly the terminology, which we have. And he had shown, he had described this example with sellers and buyers of warehouses.

He had described a method for solving it, namely the Simplex Method, and most impressive of all, he had made the following comment. I paraphrase, but I'm giving you the essence of his comment. Degeneracy may be a problem and I do not know how to handle it. Now layman may not appreciate this, but there's just such a marvelous honesty in that statement because someone of a weaker intellectual fiber might have waved his hand and said, degeneracy could easily be handled. But he just threw it in reverse.

Now for some reason, which I can't figure out, Hitchcock was never involved in the further developments, but he surely should've been. He also made a fundamental contribution to three dimensional tensors. I am told by various people who are experts in matrix multiplication that it's quite possible that the later developments in finding the minimum number of operations needed to multiply two matrices together, which depend on the manipulation of three dimensional tensors, might have and probably surely would have been done by Hitchcock, if he had known about the problem. So that's sort of a loss to the subject. He never became a member of the fraternity.

IRV LUSTIG: Continuing back on the stories related to computing, were there some connections

between the development of computers at the time and what was happening with linear programming, or were they sort of occurring in parallel?

ALAN HOFFMAN: Yes. I alluded to the fact that the financing of that machine, I think the SEAC, was, I think, largely done with Air Force money because they had it. That's my version. George can either verify, or negate that, but I'm pretty sure I'm right.

IRV LUSTIG: George was alluding to it, but he's always been very modest about how--

ALAN HOFFMAN: About how much money?

IRV LUSTIG: Well, no, modest about trying to say what the connection was.

ALAN HOFFMAN: I don't know if he showed you, but there are these old Air Force memorandum in which, in the bowels of the Pentagon, George's buddy, bless his heart, is making a presentation to go the air controller, that is to say, the general who was in charge of the Air Force money. He was describing what they were doing and then making this arrogant statement that we think that the work we're doing on planning is as good as anything going on in the world. That seems ridiculous. You see this on mimeograph paper and here are the snot noses, or government employees making this statement. But it was actually true. They were doing the best work on planning anywhere in the world.

And I remember when I saw that. It was absolutely a thrill to realize, retrospectively, how great it was. I confess that, at the time, I didn't realize it. I just thought, this is fun. But it really is an astounding achievement.

Chapter 8: Explaining Optimization

IRV LUSTIG: If you're meeting someone, like our--

ALAN HOFFMAN: Rebecca.

IRV LUSTIG: Like Rebecca here. And you're explaining to them optimization in layman terms, how do you do that?

ALAN HOFFMAN: I avoid it. In fact, there's only one-- no one ever really asked me. There's only one piece of work I ever did in mathematics, which was capable of being fully explained to laymen. And that had to do with scheduling round robin tennis tournaments. You have N couples and you want to arrange matches, so that the husband and wife never play with each other, or against each other. And you meet every player-- these are mixed doubles. And you meet every player of the same sex exactly once in a match. And every player of the opposite sex, you play with once as a partner and once as an opponent. Now I've spoken swiftly, but if I spoke more slowly, that concept, or that set of ideas, is perfectly plain and intelligible. And that's the only thing I've ever done, which I felt I could explain.

IRV LUSTIG: But do you find yourself talking about-- myself, I always end up talking about examples. That I haven't necessarily worked on, but others have done.

ALAN HOFFMAN: Yes. I sometimes use this example with the contract awards. I used an example once from a project on which I worked having to do with planning dams on the Missouri River. They wanted to know how high they should make the dams and how they should operate them. And you needed to have certain amount of flow, but not too much. A certain amount of irrigation, but not too much because you worry about flooding. So I got involved with this project. In fact, it was interesting. I got involved sort of consulting, but then when it came time to recommend how they should compute, I said, well, you should really compute here at the Bureau. We have the best stuff for it. And I therefore, removed myself from this consulting contract.

Chapter 9: A Historical Perspective

IRV LUSTIG: If you look at the over 50 year history--

ALAN HOFFMAN: It's wonderful, isn't it?

IRV LUSTIG: That we have. What would you say, a project or event, has made you proud to be part of this field?

ALAN HOFFMAN: In this field. Oh! In general, I think the growth of it. I guess when the meetings started to get so large and I could see there was this enormous fraternity of people that were interested. And interested in various ways. There were people like me, whose principal interest was proving theorems. There were people whose principal interest was in developing algorithms. There were people whose principal interest was in programming algorithm. There were people whose principal interest was making applications in the real world. So each of these four sub-fraternities is substantial in its own right. And to see that each of them was large, was thrilling.

From the point of view of my personal interests, I felt the development of polyhedral combinatorics gave me lots of pleasure, particularly since it attracted guys of the genius of Jack Edmonds, and Lex Schrijver, and in particular, Laci Lovasz and Paul Seymour. Now Laci and Paul are peerless. And they got interested in the subject, too. And then, I'm also pleased with the friendships. I got to know Joe Kruskal. We were buddies. I remember when he was a graduate student, along with Bob Aumann, at Princeton and I was at ONR in Washington. And I got to know them. There was a project they were doing for the Navy. They were little kids, graduate students. The work they did was absolutely splendid.

And getting to know Keith Fan with whom I had a common interest in eigenvalues and convex functions. And that occurred as an outgrowth of the involvement in linear programming. It was a general thrill about the development of the subject and in particular, about the development of the areas in which I was interested. But with respect to the application to eigenvalues, that never caught on the way it should have.

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

ALAN HOFFMAN: That's a good question. Practically everybody, I would say. Well, let me

mention, first of all, Al Tucker. Everybody who tells you about Al-- you haven't met, Al, I don't think? Or maybe at one of George's parties?

IRV LUSTIG: I did. Actually, I invited George to Princeton once and one of my enticing things was to visit Al. And so I drove George, he gave a talk, and then I drove him over to Al's house. And I was all set to leave, and let them be, and Al was saying, no, you stay. And I have to say, it was probably one of the most wonderful hours in my career as they just reminisced about the history. And it was fascinating and it was maybe a few years before--

ALAN HOFFMAN: Before Al died.

IRV LUSTIG: I'm trying to think. It was in '88, or '89.

ALAN HOFFMAN: He was a man of--

IRV LUSTIG: He was incredible.

ALAN HOFFMAN: Terribly nice person. I mean, exceptionally nice. There are so many instances of him being nice. I'll tell you one of them, which affected me. I've had gone to ONR London, I guess in 1955, or 1956, and I came back, and I went to visit Al at Princeton. Al Tucker was then and for many years preceding, the chairman of the math department at Princeton. And I was telling him about the things I had done in London having to do with the use of linear inequalities to do combinatorial things. He did the following unbelievable act. He was scheduled to speak on this subject at a American Mathematical Society symposium. He got in touch with the organizers and said, you should really ask Alan to speak on this subject rather than me. He has better stuff.

Now the general ego of mathematicians is a sense of nature that his act of generosity was really incredible. And in fact, it affected me both internally and externally in the following way. From that point on, I thought of myself as a senior person. And I think other people thought of me as a senior from that moment on. Perhaps it's all an illusion on my part. But that was one of his-- there were other things he did.

And one of things I'm most proud of is that one of my students, Alan Gewirtz, became quite friendly with Al. Independently of me, but he had met him. And they wrote a book together and he was in assistance to Al in his years of declining physical strength. So the person for whom I have the greatest admiration as a person is Al Tucker. But I liked practically everybody. I even got along with Abe Charnes, with whom many people had disagreements, but he and I were buddies.

George's position of the subject is sort of exceptional. It's suey generous, it's like the big meal you get at the Chinese restaurant. Harold is a nice fellow and David Gale.

IRV LUSTIG: That's Harold Kuhn?

ALAN HOFFMAN: Yes. Harold Kuhn. You're doing your job well there. All the guys I knew. What I admire about all of these people is that they were both nice and smart. And it's nice to deal

with people who are nice, and smart, and generous, and fun to talk with. Harry Markowitz. A guy of incredible versatility. People don't realize how versatile he was. He is one of the principal sparks of taking advantage of sparseness in linear programming calculations. He was one of the founding fathers of computer simulation. Beside the stuff, which got him the prize.

IRV LUSTIG: In fact, we have him on the list to interview.

ALAN HOFFMAN: And he's a mature, responsible adult, unlike some. And so Harry's a terrific guy. So many people are terrific. There was a colleague I had, first at GE and then at IBM, named Burt Grad, who put together the first linear programming codes at the IBM company. This would be in the early '60s.

IRV LUSTIG: Do you remember the name of that code?

ALAN HOFFMAN: No, but one of the guys who worked on it is still with the company. Maybe I should invite him over for lunch. His name was Bill White. The man who organized it is Burt Grad, who is no longer with the company, but I believe he's still alive. Take Lex Schrijver's book on theory of linear and integer programming. That's wonderful. And he's done really nice stuff.

Here's something I really regret, now that you mention it. I wrote a paper in those early days called, "On Approximate Solutions of Systems of Linear Inequalities," in which I consider the following question. Suppose you almost satisfy the system of linear inequalities and you know there is at least one point which does satisfy them, how close are you to some point which satisfies them? And I proved a theorem of the following nature. Suppose your given point is x and the nearest point to the polyhedron is specified by the system as x naught. Then distance from x to x naught is bounded by a constant depending only on the matrix of the inequalities times the norm of the vector of residuals. Now if the inequalities are-- forgive me, folks, for being technical. ax less than, or equal to b. If you look at the vector ax minus b, if a coordinate is negative, that's good. You've satisfied. If the coordinate is positive, so to speak, that's bad.

So what I do is I take the size of the vector of bad coordinates-- in other words, I take the norm of the positive parts of the vector ax minus b. And now to recapitulate, the distance of the given vector x to the nearest vector is bound by a constant depending only on the matrix a times the size of this vector of positive parts ax minus b. So this is known as Hoffman's Lemma and so forth. And my original write up is absolutely dreadful. I mean, I've looked at it since it became quite popular in circles where people are interested in convergence of methods not only for linear programs, but for convex programs. I wanted to see what I could do with it and I couldn't understand anything that I had there. It was completely unintelligible. And I think maybe that was the reason why it took 20 years for it to be discovered, or rediscovered.

So I rewrote it. That is, Osman Güler a man you probably don't know.

IRV LUSTIG: Yes. I've met him.

ALAN HOFFMAN: You know him. So Osman and Uri Rothblum and I, we all had ideas about it and we decided to put them into one paper. But there are lots of papers about that. Now my regret is this. That was in fact, when I think about it, a fairly penetrating observation. In fact,

other people have found it a fairly-- I did nothing with it beyond that. So I could have been a contender, in Elia Kazan's language, in that area, if I had pursued that further. Possibly. So that's one of the regrets.

Chapter 10: Looking Forward

IRV LUSTIG: So let's now move from the past more towards looking forward. How do you see optimization being used in the future?

ALAN HOFFMAN: Listen, before I comment, I want to express the following caveat. In this terrible paper, which I waved in front of you before, I had these predictions about the future. Every one of which was terrible.

Furthermore, if I could have foreseen the future, I would be a very wealthy man today. I wound have invested in IBM in 1951. So do not rely on me as a prophet. I think at one stage Mr. Watson Senior said, perhaps there's room for five computers in the country. There was only 4 before that. I agreed with him. So you're really asking the wrong man. Here's one thing which I think ought to happen, I don't know if it will. I'd like to see a closer connection between Departments of Statistics and Departments of Operations Research.

IRV LUSTIG: This is actually a very insightful comment because we see today the connections between forecasting and optimization. Yet to do good optimization, you may need to be able to do good forecasts in the future, which requires good statistics.

ALAN HOFFMAN: Yes. And it has various virtues. One of which is the intellectual content. In order to get Ph.D. Program in Operations Research, you go into more detail in some of the branches of the subject than you would, for example, in mathematics. Suppose you're getting your degree in mathematics, you would certainly know a lot about your speciality, but for other subjects, you just learn what the basic stuff was so you wouldn't be entirely ignorant if you came across a discussion of it. In Operations Research, there needs to be more. In Statistics, there needs to be more, too. And since both subjects are concerned with the use of quantitative methods in the management of organizations, they really ought to be combined at the university level and, I think, in the industrial level, too. They shouldn't be separate groups.

And, for example, at the Bureau we had a statistical laboratory. I must say, an extremely capable one, which was a sister organization of our group, which was called Numerical Analysis. So that's one thing which ought to be true pedagogically. And I think it ought to be true outside, in the world, in organizations, like, for example, here. Well, we are together, but somehow, I think we should have-- well, I don't know. In every instance, I think greater integration would help. I think each side has something to learn from the other.

Now what else do I think we should have. I spent a period in General Electric, in a group called Operations Research and Synthesis, where among my colleagues were Martin Shubick and Harlan Mills, and George Feeny and so forth. I mean, people-- including the fellow who might have gotten to be the chief executive at GE at the time. They chose John Welch, but he was also in the running of the three guys. Now one of the things emphasized in this group was that mathematics was only a part of Operations Research. And in fact, in the work we did, it was

emphasized that it was only a part. And I think in real world experience, it's only a part. Now those of us who have specialities might get a different feeling because we're not called in on all that many kinds of questions where our specialties aren't germane, or useful.

But in the world at large, if you're giving quantitative advice to business, mathematics may not enter much. And how does that affect things? I think that, in a certain sense, limits the possibilities for what optimization can do. It's going to be a long time before you're going to have confidence that the data is OK, that the world's sufficiently stable, that things aren't changing. It's hard. I'm not as optimistic as other people.

There is a group in the world, including some people in the company, who anticipate that optimization will do wonders and we won't need to drive our cars. They'll be carrying along electronically and various other things. The preparation of our meals and the cultivation of helpful patterns of sleep, and all of that will be settled mathematically and in many cases, through optimization techniques. Perhaps all of this will occur, but considering my age, I don't think it will occur in my lifetime. So I'm not a good prophet.

Chapter 11: The Growth Of Optimization

IRV LUSTIG: What would you account for as the growth of applications that we've seen over the years?

ALAN HOFFMAN: My guess would be education in the following sense. First of all, guys in business schools were learning more. And secondly, guys were getting jobs. Let's go back to the business school thing. Many years ago, the amount of mathematics that one learned in business schools was very tiny. And a concerted effort was made by the Ford Foundation, under the leadership of my college classmate, Bud Harkavy, to rectify that. They felt that business school students should learn more mathematics.

And there was a fellow named Howard Raiffa who was in charge of this. And it's called Institute for Mathematical Applications in Business, IMAB, or IMBAB. I used to speak of Howard as the Iman of IMBAB, or whatever it was. Anyway, this really had an affect. Business schools got to do more mathematics. For example, I was teaching at Yale Business School for a number of years. And it was certainly true at Stanford and all over the place. So that had an influence, that people who were graduating from business schools knew there was something there.

And then of course, some of the people who were in business schools really were good at the mathematics and stayed in it. So there was a connection of the culture with the development of the subject. And then the codes got better and each fed on the other. There was a need for better programs to solve larger and more interesting problems and so forth. So this interplay, or to use this deplorable word, synergy, truly had an effect.

I'm trying to think, did there have to be a reason? In some respects, there may not have to be a reason, but it's just the fact that it's there. For example, I have a friend who consulted me once. He says, I'm going to start this journal, do you think we'll get contributions, that authors will submit? So my reply was the same thing that was given years later in the movie, Field of

Dreams. Start a magazine and you'll get contributors. And not only that, the creators of the magazine will stimulate work in the subject. It's just the fact that it's there, and people could see their name in print, and so forth. So many things contributed. I think the creation of the organization, which really had to happen.

IRV LUSTIG: The organization being, The Mathematical Programming Society.

ALAN HOFFMAN: The Mathematical Programming Society. I, originally, thought it was ridiculous. I was wrong. And I must say also that its greatest period of growth occurred during the tenure of my friend and colleague, Phil Wolfe, because it did much better. I hope I'm not offending anybody, any of his predecessors. But it did much better from the time Phil became president to later. But Phil also took an active role in initiating its various projects. More than anybody else I can think of, he's responsible for the success of that organization as an organization.

IRV LUSTIG: So you wanted to add--

ALAN HOFFMAN: I wanted to add to the list of people I admire. First of all, my relation to Phil Wolfe is of such a nature that's on a case of admiring. I think of him like a younger brother. We've known each other a very long time. I always felt, just technically, he was the number two man on the subject after George. And just as a person, he's absolutely marvelous. Open, and sincere, and a great guy. And I also wanted to tell you about Sol Pollack, who was one of the programmers for the simplex code at the Bureau.

IRV LUSTIG: Now was he involved in the paper?

ALAN HOFFMAN: No. He wasn't involved in the paper, but this is what he did. In 1954, around the middle of the summer, Sol thought it was possible that the Giants might win the pennant. The rest of us offered to bet him 100 to 1 that the Giants would not win the pennant. Well, on the last day of the season, or the day after the last day of the season, as you know, the Giants won. And I have this vivid picture in my mind. There's Sol and a bunch of us listening to the game on the radio. And then Thompson hits the home run. And the look of ecstasy on Sol's face when the new comes over the radio. And he raises his fist, and starts running, and we're all running after him. And then what Sol did was he said, I can't take $100 from each of you guys. I know you didn't really mean it, but if you just run a beer party to celebrate the occasion. I thought that was really nice of him. That would have been $1000, which, in 1954, was really sizable money.

IRV LUSTIG: Great.

ALAN HOFFMAN: So he was a nice guy. All the people were nice. Particularly, I wanted to mention Phil, with whom my relationship was more personal than with anybody else.

Chapter 12: Criticism of Optimization

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

ALAN HOFFMAN: Invalid criticism. Well, I can tell you the things which were said in the days that I was at GE, which was that there were certain things which it didn't take into account.

IRV LUSTIG: Such as?

ALAN HOFFMAN: Personalities of people. For example, there was once a question of where to locate a new plant for one of the departments of the company. And so we figured out what would be good from the point of view of transportation and so forth, but they decided to build it someplace else because the person who was to be nominated to be the general manager of that department wouldn't live in that city. Period. So in fact, they could have saved themselves the effort in the first place.

So there's a criticism I've heard about practitioners of optimization, or Operations Research generally, which is there's a tendency for some people to work silently until they make a presentation of the results of their spectacular research. It's really important, I think, that the sponsor of the research, the people who are paying for it, become partners in it at every stage. You check that the view point of the assumptions are genial to them, you make them feel that it's their contribution. Instead of just saying, well, remember, we had this conversation two months ago? Well, here's the results and admire us. That's bad.

IRV LUSTIG: So this is more of a criticism of the people--

ALAN HOFFMAN: No. How can I criticize? I can't criticize-- Is the mathematics correct? Yes, the mathematics is correct. Is it relevant? Well, that's a separate issue. People might say-- for example, there are some cases where people use optimization in a model where it's sort of inappropriate. The knowledge is too flimsy. They don't know enough about the numbers associated with it. And sometimes there's a thought that this uncertainty can be quantified and you can be intelligent in the face of uncertainty. I think this is certainly true intellectually, but I don't know if it's true in practice. That is to say, I'm going to say I think the probability of this scenario is 0.85 and that scenario is 0.12. I mean, come on. I'm uncertain of that.

IRV LUSTIG: I think in my conversation with George, he was saying he thinks is the next--

ALAN HOFFMAN: He thinks that?

IRV LUSTIG: No. He thinks that programming under uncertainty is always the big thing. And I would bring up this issue. I would say I think the issue is data.

ALAN HOFFMAN: Yes. I'm on your side rather than his.

IRV LUSTIG: I think the success of optimization that we've had is due to data availability. That's one of the real key success factor. I say it's due to data availability, improvements in hardware, and improvements in software. And you need all three to be solving the kinds of problems we solve today.

ALAN HOFFMAN: Yes. That's true. And in particular, one thing which has happened, which absolutely astounds me, my colleague, John Forrest, is an absolute whiz. I mean, his skill is

unbelievable. He can make things happen and do problems of an enormous size by one kind of cleverness, or another, which would just be way beyond me. So that's been a big thing. But I imagine, of all the things we are speaking of, the improvement of machines is probably the least significant.

IRV LUSTIG: It's a combination of things.

ALAN HOFFMAN: Because you would solve relevant problems with more amalgamations, so the size would be smaller, I think.

IRV LUSTIG: Yes, but the fact that we can do less amalgamation means that we can be more precise in the answers we put forth.

ALAN HOFFMAN: More precise, if your knowledge supports that precision. That's the point you were making before.

IRV LUSTIG: Yes.

ALAN HOFFMAN: And I agree with you completely.

IRV LUSTIG: What would you say has held optimization technology back from growing more popular?

ALAN HOFFMAN: I don't know what has held it back. What? You tell me what's held it back.

IRV LUSTIG: I'm asking for your opinion.

ALAN HOFFMAN: I haven't the slightest idea. Actually, just speaking personally, some things take a long time to be noticed. For instance, in 1960, or '61, I wrote a paper which introduced this concept of Monge sequence. M-o-n-g-e, named after the famous French mathematician, Gaspard Monge, who was the founder of the Ecole Polytechnique. And in a famous pamphlet, he wrote, shortly after the French Revolution, described a kind of transportation problem having to do with the construction of forts-- f-o-r-t-s. Which led me to take the essence of his idea and use it to formulate a notion that certain transportation problems could be solved in greedy fashion. And so I named this idea after Monge as I hoped that because he was a famous man, the word might catch on. Point of fact, he wasn't so famous. And I can tell from the expression on your face--

IRV LUSTIG: I've heard the name. I've heard the name, but I didn't know the connection.

ALAN HOFFMAN: Anyway, it took 20 years before anybody noticed it. Some of these things take a long time to be seen. Also, I've never been very skillful at placing papers in places where they're likely to be noticed. But that's just from the point of view of theory. I think the progress in theoretical developments has been absolutely spectacular. That is related in someway to competition. I think the development of semi-definite programming has been wonderful. The development of approximation methods using linear programming and semi definite programming, as in the work of Dave Williamson and Goemans, has been a spectacular

development.

I remember with great joy when I first heard about the Howson-Lemke algorithm, This was a method that's related to complementarity and therefore, in your years at Stanford you had to have been exposed to it.

IRV LUSTIG: Yes. It's going back.

ALAN HOFFMAN: Shorter for you than it is for me. Anyway, I remember this so well. A fellow here named Jim Griesmer and I, and Abe Robinson at Yale had made an observation that if you're solving a bimatrix game and the data is rational, then there's always a rational equilibrium pair. And that was sort of a mild argument, but we polished it up. We were thinking of sending it into a journal when, at a meeting of some organization, it was probably the Operations Research Society, or some such organization, Carl Lemke, he came over and told me about his algorithm for solving bimatrix games. And what a thrill that was because although it made my paper with Jim and Abe--

But as I said before, although it made the paper we had planned to send in absolutely superfluous, it was such a thrill to hear this thing, which was a new idea. A new idea of a beauty and penetration of the sort I hadn't heard of for sometime. It was just lovely. By the way, Carl Lemke is a guy who's made a lot of contributions to the subject. And he isn't appreciated enough, if I may make that comment. Maybe we should quit at this point, unless you have--

IRV LUSTIG: No. I was going to say--

ALAN HOFFMAN: What?

IRV LUSTIG: There was one of the other questions here that I do want to ask. Would you say that there's some misconceptions about optimization that exist?

ALAN HOFFMAN: I haven't the foggiest idea what the answer would be.

IRV LUSTIG: That's OK.

ALAN HOFFMAN: I'm not living in a world where optimization is around me daily. I am living in a marvelous world where I have absolutely marvelous colleagues, but not necessarily just optimization wise.

Chapter 13: Concluding Remarks

IRV LUSTIG: So now there were some things that you had that you were interested in showing.

ALAN HOFFMAN: Oh, I have-- yes. This enormous pile which you haven't gotten to. I have correspondence from my early years, some of which is fun. A letter exchange with Karl Menger, for instance. Letters to and from Harold Kuhn, and David Gale, and Joe Kruskal, and Phil, and Ray Fulkerson, and George.

IRV LUSTIG: Are there two, or three, which you would like to share?

ALAN HOFFMAN: I'd be willing to share any of them, the only problem is, I haven't looked through these to pick out the ones. And you don't want to undertake the effort of going through these. It's too much.

IRV LUSTIG: No. I think that's something we should do one day.

ALAN HOFFMAN: Yes. I'll tell you what we might do, which is I'll browse through these in the next few days and if I see there are some-- unless you want to call it quits now? If I see there are some which might be interesting for you to put on the screen--

IRV LUSTIG: You can just photocopy them and I can take pictures.

ALAN HOFFMAN: And then I brought in, as a sort of recollection, copies of early papers. For example, the paper Harold and I wrote when we first got the idea that we could use linear programming to do combinatorial things. So they never sent us reprints, but I have like 100 copies of The American Mathematical Monthly containing this. So if it's of any use to you, all of those papers over there, you can take. But you may not want them. Since this isn't your life work.

IRV LUSTIG: Yes. That's your life work. Actually, one of the things I did notice here when I was looking at the paper on the first computational experience in solving linear programs, is that this was in the first SIAM issue. SIAM, volume number one.

ALAN HOFFMAN: I think it was solicited.

IRV LUSTIG: Was it actually solicited at the time?

ALAN HOFFMAN: It's possible that Harold Kuhn was the president of SIAM then and asked me for it. But it wasn't an accident that it was in the first one. So either it was solicited, or I thought, boy, here's a chance to get something in before the refereeing standards get too strenuous. It was one, or the other.

IRV LUSTIG: Now here's a society that clearly was in its infancy at that point, what do you think was driving the creation of that society?

ALAN HOFFMAN: I think the guys felt that the math society was paying insufficient attention to applied mathematics. And I think the case was well made.

IRV LUSTIG: Because what's interesting here is that here in the first issue, there was a paper on solving linear programs. And actually, when I started being involved in the societies, optimization really almost wasn't on the radar screen at SIAM in the late '80s.

ALAN HOFFMAN: Isn't that interesting.

IRV LUSTIG: Now there's an activity group, and there's more activities, and they have regular

meetings, but it's always been that the society, at least from my point of view, seemed to be dominated mostly by people doing differential equations.

ALAN HOFFMAN: Sure. And models of physical phenomenon. Well, Harold Kuhn was an officer, I think the president of the society at some point or another, and I think during that period, most of that activity-- I didn't know that it had disappeared from the screen at a certain stage.

IRV LUSTIG: Maybe this was just my sense.

ALAN HOFFMAN: Of course, one of the things which might have happened is that the optimization fraternity had acquired several journals of its own. Not only the Mathematical Programming Society journals, but others. And then there were the Operations Research journals and all of that. So running SIAM became less attractive. That's a possibility. Do you think that?

PHILIP: Yes.

ALAN HOFFMAN: And people like to write where they're going to get readership, which reminds me I said to you the other day. If I write a paper and 30 people read it, really read it, I'm delighted. That's a big readership. If I were to write a program and less than 10,000 people-- you know, like for personnel, or hiring, or how to fix something in Windows. If I were going to write a program and less than 50,000 people use it, I'd be disappointed. It's just a different kind of audience. People get used to it.

IRV LUSTIG: Yes. That's true.

ALAN HOFFMAN: So perhaps, Philip, if we wanted to have an impact, we might have been better off becoming systems programmers.

PHILIP: Was that your object?

ALAN HOFFMAN: To be better off? Oh, to get the satisfaction. No, my object wasn't to have an impact. My object was to have a good time.

IRV LUSTIG: Now I think you have.

ALAN HOFFMAN: I certainly have. The whole thing is more, or less incredible. And at this point, there's no way it can be turned off. I really had a good time and it's not really spoilable.