Margaret-Wright Interview by Irv Lustig, New York, NY August 12, 2019

Early Life

IRV LUSTIG: So I'm here today with Margaret Wright, a Professor in the Computer Science Department of the Courant Institute at New York University. Margaret has had a distinguished career in the area of optimization. I've known her since I was a graduate student at Stanford and Margaret was a research professor in the Systems Optimization Laboratory at Stanford back in the mid '80s. So we're going to hear today about Margaret's career in optimization, how she got there. So Margaret, I want to thank you for taking the time today for doing this interview.

MARGARET WRIGHT: It's a pleasure.

IRV LUSTIG: All right, so let's start off with your early life. Where and when were you born?

MARGARET WRIGHT: I was born in February of 1944. I was born in San Francisco, California but only lived there three days. My mother was a medical doctor, and she wanted me to be born in a certain hospital in San Francisco. And when I was old enough to travel, we went back to Hanford, California, which is in the Central Valley.

IRV LUSTIG: And that's where you--

MARGARET WRIGHT: That's where I grew up till I was 10.

IRV LUSTIG: To 10? And then where did you go?

MARGARET WRIGHT: To Tucson, Arizona. I went to junior high and high school in Tucson. And then my family moved to Southern California, but I went to Stanford.

IRV LUSTIG: So you were-- you graduated high school in Tucson.

MARGARET WRIGHT: In Tucson, right.

IRV LUSTIG: And what about your parents? Did they have any math backgrounds? You mentioned your mother was a medical doctor.

MARGARET WRIGHT: Right, both my parents were medical doctors. My father was a type of doctor that I think doesn't exist anymore, which is General Practice. So he did surgery. He did everything. I think that is no longer done. Everyone specializes.

My mother was in Eye, Ear, Nose and Throat, as it was called then, which again, they don't have now. It's completely divided up into specialties. My mom claimed, at different points in her life, that she'd been very good at math. But I never saw any evidence of that.

IRV LUSTIG: So in Tucson, did you start doing math then, when you were-- at what age did you become interested in math?

MARGARET WRIGHT: Well, I was always interested in math even in elementary school. But in Tucson, when we got into more interesting things, I was very interested in math. And I was always the best in the class, OK, which really sounds obnoxious. But it's factual. And I loved it.

So we used to have things where this one teacher had something where you would start at one end of the room. One student would be sitting down and the other one would be standing up. And he'd ask you a problem, OK? And then whoever got the right answer quicker moved on to the next person. And so the idea was to get as far around the room as you could. And again, obnoxiously, I will say, whenever I started out, I always ended up at the end of the room faster than anybody else.

IRV LUSTIG: Now. What year did you graduate high school?

MARGARET WRIGHT: 1960.

IRV LUSTIG: So 1960--

MARGARET WRIGHT: I was 16.

IRV LUSTIG: You were 16. So I guess you got a little bit ahead of the curve at that point.

MARGARET WRIGHT: Right, in Hanford, California, they skipped me a grade when I was in-- I never did fourth grade. Also, by an accident of birth, I was born in February. And the cutoff date for starting school was March 1st. So I was always at the young end.

IRV LUSTIG: I see, OK.

MARGARET WRIGHT: Now, being two years younger was kind of a big deal then. It certainly isn't true anymore.

IRV LUSTIG: But now, what was it like being, at that time and place, being a young woman in a high school in Arizona that was good at math, when there were stereotypes back then, I imagine, were you don't belong here.

MARGARET WRIGHT: Absolutely, well, that's something I have thought about occasionally. I had a counselor in high school. I told her I was interested in medicine. Both my parents were doctors so that wasn't so surprising. And she said, “maybe you could be a nurse.” And I said, “actually, I'd like to be a doctor.” And she said, “oh, well, are you sure you want to do that? Girls don't usually become doctors.” And I said, “my mom's a doctor.” So there was that. But apart from her, she was a counselor. She should have known better.

IRV LUSTIG: For sure.

MARGARET WRIGHT: But it was, you know, the late '50s. Gradually over time, I became less interested in being a doctor and more interested in math.

IRV LUSTIG: So what made you choose Stanford to go to school?

MARGARET WRIGHT: Both my parents had gone to University of California at Berkeley. So there was a certain amount of family history that they said, well you could go to Berkeley. And even though I was living in Arizona, UC had a rule that if you were the child of an alum, you could go in for in-state tuition, which was very cheap. So to be difficult, I said, I'd like to go to Stanford.

One of my teachers in high school had gone to Mount Holyoke. Another one had gone to Wellesley. And they were women's colleges. You know, women could not go to Harvard, Princeton, Yale, et cetera, et cetera. So I wanted to go to a famous school, a good school, that admitted women. Stanford was one of the few. So I decided to apply to Stanford and I got into various schools. But I decided I'll go to Stanford.

IRV LUSTIG: And what-- I imagine you must have also applied to Berkeley, as well.

MARGARET WRIGHT: Of course.

IRV LUSTIG: So what made you pick-- I mean, I know there's always the Stanford-Cal rivalries, right. So what made you pick Stanford over Cal?

MARGARET WRIGHT: I think part of it could have been a desire not to do just what my parents wanted. You know, they kept saying oh, you should go to Berkeley. I said, well, I don't know. Now, Stanford was considered expensive at the time.

IRV LUSTIG: Right, it's a private school.

MARGARET WRIGHT: Tuition was $1,000 for a year, but my parents could afford it. So I thought fine. I'll go to Stanford. It wasn't a well-thought-out, careful decision based on an analysis of the quality of the education.

College at Stanford

IRV LUSTIG: I see. And so when you got to Stanford, did you enroll immediately in math. Or what area were you in?

MARGARET WRIGHT: At Stanford I had the experience, which as you know from your own experience, many smart kids have, which is they're the best in their high school. They get to Stanford or wherever and suddenly everybody is just like them, was number one in their class. And it's a bit of a shock.

But I absolutely wanted to take math. And I started off my freshman year with calculus, which is what you had to do then. I hadn't had calculus in high school. This is before they started teaching calculus to high school students, so yes.

But what I also liked was French. And I also liked history and literature. But at Stanford, at that time, you didn't have to declare a major till your junior year. So I just thought I'll take the classes that I want and decide later. So that's what I did. But I always took enough math classes so I could finish a math major.

IRV LUSTIG: And that became your major as an undergraduate.

MARGARET WRIGHT: Well when I was, I think, just before I had to declare my major, I was talking to some older person, probably in his 20s (!)., OK. And I said, “I don't know. What should I major in? Should it be math? Should it be history? Should it be something else?”

And he said, you know, if you plan to have a job, which I did because my mom had always worked, math is really better. And I said, “they'll pay you to do math”? And he said, “yes, they will, much more than history or English or whatever.” So I said, OK. So I majored in math.

IRV LUSTIG: Now this was at the time they were forming the Operations Research Department at Stanford, while you were an undergraduate--

MARGARET WRIGHT: And the Computer Science Department.

IRV LUSTIG: As well.

MARGARET WRIGHT: Yes.

IRV LUSTIG: So did you have interactions with those departments as an undergraduate?

MARGARET WRIGHT: I did because they were starting to teach computer science courses. And of course, now it's amazing when you think how long ago this was. It was '64, '65. And they talked about computer science. And I took a computer science course as an undergraduate and I loved it.

I thought, this is the greatest thing I've ever done. I can put these-- now, these were decks of cards that you submitted to a key punch, OK. And I thought, this is so great. I could make the machine do what I want. So I loved computer science and took two computer science undergraduate courses, which they were just starting to teach. And then, the next year, the very next year, '65, was the first year that the computer science department existed. So I got a Master's in Computer Science in '65.

IRV LUSTIG: So that was in your fifth year, then--

MARGARET WRIGHT: It was a one-year master's.

IRV LUSTIG: So that was like-- so you must've been one of the first computer science graduates?

MARGARET WRIGHT: Yes.

IRV LUSTIG: I'm guessing first female computer science graduate?

MARGARET WRIGHT: No, there were a couple of other women, Barbara Liskov and Susan Graham, who were PhD students.

IRV LUSTIG: I see, OK.

MARGARET WRIGHT: They were PhD students. I was a master's student. And then, as now, a masters was a terminal degree. You know, you weren't going to then go on to get a PhD.

IRV LUSTIG: Which you then did.

MARGARET WRIGHT: Which I did, but a few years later.

IRV LUSTIG: So there was something in between the master's degree and getting the PhD. So what happened next?

First Job

MARGARET WRIGHT: Right, so here's what happened. I got a job. And there was a funny experience. So I was interviewing. You know, I had this master's, and all A's, and all that stuff. And I was interviewing at some company, I've forgotten which, in the Bay Area because I wanted to stay there. And the person said, well, do you know how to program in Fortran?

Well, in Stanford Computer Science at that time, they taught us Algol. And I said, no, I don't know Fortran. But I have been taught that, if you know one programming language, you can quickly pick up others.

And this man said, well, I don't know. We require a knowledge of Fortran in order to get this job. And I just thought, well, OK. But another person, who became my boss, believed that I could learn Fortran quickly, which I did. If you've programmed in Fortran, you know that it's pretty easy to pick it up after you know Algol. So I got a job at GTE Sylvania doing scientific programming.

IRV LUSTIG: And what kind of problems were you solving?

MARGARET WRIGHT: Because I didn't have a PhD, I was just the lowest of the low. I worked on things people assigned me. So GTE, at that time, was doing a lot of work for the military, mainly not for weapons but simulations, you know, like ship recognition and various things like

that, where they had simulations with numerical computing. And I wrote the codes that did these calculations.

So the engineers thought about the models. And then they would talk to me, and I would write the code. OK, so that's how it worked. And funnily enough, there was a guy in my department who said, there is this new thing in optimization by these two people called Fletcher and Powell. This was in '63 that they published their paper. But it had trickled down to Sylvania.

And he said, I bet you could use this to solve this problem that you have. And I said, oh, OK. So I read their paper and implemented, if you want to call it that, the Fletcher-Powell algorithm. Another person I worked with was using linear programming to do ship photo recognition.

I hate to think, now, what they were doing with those pictures of ships. He had figured out a way to use LP. Now, I had never learned linear programming as a math major. And in computer science, they did not teach optimization. So I had no idea that George Dantzig was at Stanford. I didn't know what linear programming was. But I had to learn it to write this code.

Another funny thing, which may come up later, is that it was legal, at that time, to discriminate against women in employment. In the newspapers, the classified ads would say jobs female, jobs male. The female jobs were often things like secretary, personal assistant, whatever. The male jobs were things like engineer, et cetera. And that was perfectly legal. That would never be allowed today, right. So but then it was legal.

So I discovered in some circuitous way, I've forgotten how, that these two guys in my department, who had degrees from junior colleges and they didn't really know any math. And they were kind of jerks, actually. And I found out that one of them earned 40% more than I did.

I wasn't very happy about this. So I went to the-- they called it personnel then, HR. And I said, look, I earn 40% less than this guy. And she said, “you're a woman. And we know you're not going to have a long career. And this guy has a family and has to support them. So you're going to get paid less.”

I wasn't too happy about that. But a few years later, the law changed. At that point, and also because I was getting a little tired of having other people tell me what to do, I decided to go back to Stanford and get a PhD.

IRV LUSTIG: So how many years were you at Sylvania?

MARGARET WRIGHT: I was at Sylvania-- I went to work there in '65. And I went back to graduate school in '71, so six years.

Back at Stanford for a PhD

IRV LUSTIG: Six years, OK. And so you went back. And you applied to the computer science department.

MARGARET WRIGHT: Well, again, this is one of those things where I was lucky. I called the computer science department. I talked to the person who was deputy chair or whatever. And I said, I have a master's from your department. But now I'd like to come back and be a PhD student. He said, fine. That's fine.

Now, you see, this would never happen today, right. The PhD admissions, it's a whole different thing. It's extremely-- they'd never say, oh, fine. You have a master's degree. You can be a PhD student. But that's what he said. So I said, oh, great. So I'll go back and be a PhD student. So I did. I didn't have to do anything. I never had to take the GREs. I never had to do anything.

IRV LUSTIG: And was it then too, when they would be allowing you to become a PhD student with financial support?

MARGARET WRIGHT: No, because I mean, I could have paid my own tuition. But as it turned out, Gene Golub of the computer science department, heard that I was interested in optimization. And he said, well, I have money. You can have an assistantship. So Gene actually supported me.

IRV LUSTIG: Now, did you know-- did you know Gene while you were an undergraduate student?

MARGARET WRIGHT: No, but somehow, I think I went to a talk at Stanford, and he always was very sociable and had little gatherings after the talk. And he said, well, you know, who are you, kind of. And I introduced myself and explained what I did. And I said I was working on Fletcher-Powell's method and he said, oh, well, how are you updating the inverse?

And of course, in the paper they talk about just updating the inverse explicitly, which of course, now we say, no, no, no. You have to update a factorization. Anyway, and he said, oh, maybe you'd like to come and give a talk in our department about what you're doing. And we were getting great results, so I thought, why not? So I went and gave a little talk in the computer science department.

IRV LUSTIG: So now, it's interesting that there was by that point, the Operations Research Department, and I mean, I know Dantzig, Cottle was already there. I think Curtis Eaves was probably there. I'm trying to think of who else, Hillier, Lieberman. I mean, so there was this established group. And yet, you ended up and Gene saying, oh, well, here, you can do optimization over here in computer science. So it was-- what was the nature of the relationships back then between work in optimization and computer science and whatever was happening over in the OR Department?

MARGARET WRIGHT: Well, I actually don't know. Because later, when I was a PhD student, of course, I had to take a course in optimization. And I learned about linear programming. And people said, the father of linear programming is here at Stanford. But before that, I had not heard about it. To get a master's in computer science, I only had to take courses in computer science, like systems

IRV LUSTIG: Sure, but as a PhD student, now, so you come back in '71, how was the relationship at that point in time?

MARGARET WRIGHT: Well, as we have now, here and many other places do, first year PhD students have to get sort of a qualifying system out of the way. So I had to take a special course that was sort of creative thinking for computer science, taught by Bob Floyd. I had to pass an exam on systems, I had to pass an exam on compilers, I had to pass an exam on theory.

And I had to pass an exam on AI, none of which were offered in the OR department because it was a computer science degree. And I had to do all that to get admitted to candidacy, as they called it. So it was only later, my second year as a PhD student, that someone said, you know, you can take these classes in the OR department. And Curtis Eaves taught the linear programming when George was on leave that year, I think. So I met Curtis. So I didn't actually meet George for a couple of years.

IRV LUSTIG: I see, OK. And so you're in computer science. You studied under Gene Golub.

MARGARET WRIGHT: I did.

IRV LUSTIG: And your thesis was on--

MARGARET WRIGHT: Well, this is another set of coincidences. Philip Gill, who worked at the National Physical Laboratory in England, had come to Stanford as a visitor, I think in '73. And he was a great guy. You know him. Then Gene invited Walter, who was Philip's colleague and also worked at the NPL, to come to Stanford for a whole quarter and teach a class. And I was his teaching assistant.

So I was Walter's teaching assistant in, basically, numerical analysis, elementary numerical analysis, OK. And Walter was deeply shocked by Stanford because, in England, undergraduates are majoring in a specific topic and they're at a pretty advanced level even when they start. So he was used to teaching UK undergraduates who already knew plenty of math.

The students in elementary numerical analysis did not know plenty of math. Many of them barely knew what a derivative was. They didn't know linear algebra. So Walter would give these lectures. and the students would say, I don't know what that means. So I was the TA and I was much more sympathetic to the students, I think it's fair to say.

So they would come in and talk to me. I'd try to explain what things were. So it was probably good that I did this TA-ing because I got to know Walter. And then, when I finished, he was going back to England. And Gene said, do you think you might want to work with Walter on your thesis? And he said, if you think so, I'm going to just support you when you're in England working at the NPL. So I went to England, worked at the NPL, worked with Philip and Walter.

IRV LUSTIG: In the--

MARGARET WRIGHT: On nonlinear optimization.

IRV LUSTIG: Right, and your thesis topic was?

MARGARET WRIGHT: Well, it was called *Numerical methods for Nonlinearly Constrained Optimization*. So backing up a little bit, Walter, in his thesis, had written about what today we would call the penalty trajectory method. Martin Beale was his advisor.

Although Martin once told me that he and Walter were like orthogonal when they talked about anything, which you can sort of imagine knowing them, yes. OK, so Walter had always wanted to have someone who would work on the equivalent of the penalty trajectory method for barrier functions. So that's what my thesis was about. It was about using barrier methods for nonlinear optimization. And not for linear programming, but for nonlinear optimization.

IRV LUSTIG: And so, how much time did you spend in the UK, working on your thesis?

MARGARET WRIGHT: I was in the UK for six months-- six months. Then I came back to Stanford and finished.

The Beginning of Stanford's Systems Optimization Lab

And George Dantzig comes into the picture now because Michael Saunders had finished his PhD in computer science in '72.

IRV LUSTIG: He was also one of Gene's students.

MARGARET WRIGHT: Right, so when I arrived in '71, Michael was just finishing. So I got to know Michael and about his work in linear programming, and the linear algebra associated with linear programming. And he was forced, because of the terms of his fellowship, which had been from the New Zealand government, he had to go back to New Zealand.

He had promised, after he got his PhD, he would go back to New Zealand and work at, I forget what it was called, the government organization. And so he was spending a lot of time in the OR department, as was John Tomlin, who knew Michael. OK, and SOL, Systems Optimization Lab, decided that they wanted to hire people-- basically I'm laughing because it was kind of a cross-purpose thing. So I wanted a job where I could do research and optimization.

In retrospect, I see that what the OR faculty wanted was someone to write programs based on other people's work, which is what I had done at Sylvania, yes. So I had an interview. And I remember, one of the faculty members who would say, so we'll tell you about these methods. And then you can write the code.

And I said, well, that's really not what I have in mind. But George, bless his heart, said, this seems fine. It's fine with me. She can work on nonlinear things. It's OK. So they hired me as a Senior Research Associate, not a faculty member, just a soft money.

IRV LUSTIG: So now I know the SOL, I mean, it was the Gang of Four, as we-- we always refer to Gang--

MARGARET WRIGHT: It became that.

IRV LUSTIG: It became that. So when you started, who else was there. Or were you the first person hired?

MARGARET WRIGHT: I believe-- and see I wasn't in the OR department at that point. I believe it was John Tomlin. And an article about George, which is written by Philip, Walter, John, Michael, and me, included a statement by John, I'm pretty sure, that the Systems Optimization Lab consisted of a room with a sign on the door that said, Systems Optimization Lab, and a few decks of IBM cards, and John. And that was it.

George liked that name Systems Optimization Lab. He wanted to have a lab where people worked on big problems. I think Michael worked there for a little bit, either before or just after he came back from New Zealand. And John worked there. And there had been another person before that, but I've forgotten his name.

IRV LUSTIG: And then, soon after, then Philip and Walter then came and joined in.

MARGARET WRIGHT: Philip and Walter had become increasingly unhappy with the NPL. Previously the NPL had been a place where you could just do the research you wanted. It was part of the government. It didn't have to make a product. It didn't have to make money.

And as has happened to many other organizations, the bosses changed the view. They said, no, no, the people that work here have to make contributions to British industry. And they have to find business customers and this kind of thing. Well, they didn't want to do that.

You could say, too bad for them. But they said, we don't like it. We don't want to be here. And George had already expressed a strong interest in having a bigger group. He knew Michael would come back from New Zealand as soon as he finished his obligation. And Philip and Walter agreed that they would come to Stanford.

The four of us started working together. In the meantime, John had left. He went to Ketron another company. So anyway, that was when the so-called Gang of Four started.

Computing in PhD Research

IRV LUSTIG: So I want to actually rewind a little bit. In your thesis, which was on numerical methods for--

MARGARET WRIGHT: Nonlinear--

IRV LUSTIG: Nonlinearly constrained optimization, I imagine you did not only the math, but you also did some implementation at the time.

MARGARET WRIGHT: Oh, yes, I had some very nasty nonlinear problems. And I, in effect, had what we would now call a primal-dual method. So I updated the multipliers and the x's. It was just a different way to solve them.

IRV LUSTIG: Now, what kind of computing environments were available to you back then?

MARGARET WRIGHT: IBM 360. At SLAC.

IRV LUSTIG: All punch card types of stuff.

MARGARET WRIGHT: No, no, no. I had a terminal. I had terminals. We had a network. Yeah, I mean, for the time, it was fairly advanced.

IRV LUSTIG: Right, and through the computer sciences-- through the computer science department.

Right. There was a terminal, one terminal I think, in the office building where the numerical analysis students were. And you could log onto SLAC, the linear accelerator. And you could run things there. So that's what we did.

Collaborations at SOL

IRV LUSTIG: So your early collaborations were focused on nonlinear optimization as a group.

MARGARET WRIGHT: That's right. We decided that we would always have all four names on what we did and that they would be in alphabetical order. Philip liked that a lot because it would always be Gill et al. And I would sometimes say, what, if I changed my name to Aardvark. They'd all laugh. But I was always last, OK.

But we decided that there was no way we could work closely together and have arguments about who gets the credit, whose name gets to go first. Because we all knew of instances of that where it led to a very bad feeling. So we had all four people be on it all. But yes, we worked on NPSOL. We worked on, I don't know, some linear algebra things, lots of stuff like that.

We did a lot of really interesting and fun work, the four of us. We had talked for quite a while about writing a book. Michael, basically, said, I don't want to be a co-author. I don't want to do this. It's going to be away from the research that I want to do. I just don't want to do it. He just said, I'll be happy to look at what you've done. But I don't want to be a co-author. So the three of us started working on this book, *Practical Optimization*, which was-- I discovered just the other day because I was looking this up-- the second book to be published using TeX.

IRV LUSTIG: Yes, I saw it because I was looking at it before I came here. It was published 1981. And so that was TeX in its very early infancy.

MARGARET WRIGHT: It was. And if you read some of the things Knuth wrote at the time, he talks about discovering these pages of beautiful mathematics because there was a high quality

printer called the Alpha Type, which did I don't know, 5,000 DPI or something. Which we could use for the final pages, not in general. And that was us. That was our book. So that was really great to be able to do that.

IRV LUSTIG: Well, it's a great story in the history of TeX, which amazingly now, so we're talking almost 40 years later, is still the basis of the standards for people writing in all forms of mathematics.

MARGARET WRIGHT: That's right. That's right. It's fascinating. And I had taken a class from Knuth when I was a master's student. So I actually knew him, in a sense. But the TeX thing was just-- I mean, we heard about it. And we thought, OK, let's do this. The publisher, which was Academic Press at the time. We talked to them. And they said, if you'll give us the camera ready copy, we'll take it.

IRV LUSTIG: And there it was.

MARGARET WRIGHT: And there it was. Yeah, exactly, that's right.

Connecting Karmarkar's Algorithm and Barrier Methods

IRV LUSTIG: That's right. So, while at Stanford, and while I was there as a graduate student, the Karmarkar thing happened, which brought back barrier methods, which is obviously became the connection to your work that was done when you were a PhD student. So why don't you tell our audience a little bit of the stories of how that discovery came about, where you made the connection between what Karmarkar had done and barrier methods.

MARGARET WRIGHT: OK, so I'll go back before that to Khachiyan. That was '79. So, Khachiyan, you know, there had been this open problem. Is there a polynomial-time algorithm for linear programming? People knew about the Klee-Minty example. So the simplex method could be exponential time in the worst case. So people were really spending time on finding a polynomial-time algorithm for linear programming. Khachiyan's method made the front page of the New York Times saying, Soviet mathematician solves important problem.

IRV LUSTIG: That was right in the middle of the Cold War.

MARGARET WRIGHT: Right, Russia was our enemy. The Soviet Union was our enemy. When I teach about linear programming, I give a copy of that article because it's full of mistakes. It says that this will allow the Russians to break our codes.

Reporters were calling us. And they wanted to call George. But he, of course, did not want to talk to them. I remember a conversation with a reporter, where the person. And he said, OK, I have to write this story. Can you just tell me what polynomial-time means?

And I said, do you know what a polynomial is? He said, no. I said, OK, x squared. And I started. He said, I can't understand this. I can't understand this. Doesn't it just mean really, really fast? And I said, no, it does not just mean really, really fast.

So huge publicity, so I think we and other groups, I'm not positive who did this, decide to test Khachiyan's method. So we'd take some linear programs. We would run Khachiyan's method. And it was always, always, in our examples, way slower than the simplex method. I mean, way slower, it was not even a contender.

So we didn't ever write a paper about this. In retrospect, I wish we had because we had lots of numerical results about it. But basically, there was huge excitement about Khachiyan's method. And then people just said, well, it's not fast, meaning really fast. So that was '79.

And then in '84 Narendra Karmarkar, who worked at Bell Labs at the time, announced that he had found a polynomial-time method that was 50 times faster than the simplex method on every problem. Well, you could say those were fighting words. I mean, people just went, oh, come on. We went through this with Khachiyan.

But what Karmarkar had appeared to be different, OK. And everyone was excited about it, including George. He was very excited. I mean, even though you could say he would be upset if it was better than the simplex method, he wasn't. He just loved the idea of linear programming. And he invited Karmarkar to come to Stanford to give a talk. So he did.

Karmarkar came to Stanford. And he gave a talk. And I don't know if you would remember this kind of thing, but he was very, very, very cagey. He wrote down just-- he just wrote down a matrix at a right-hand side. People kept saying, well, what do you do, exactly? And he didn't tell them because he said it was AT&T proprietary and that he wasn't allowed to talk about the details.

But he did write down an equation which had the form of a diagonal matrix with 1 over the square of the constraints, OK. Now, we've talked about this, the group. We were sitting around listening to Karmarkar.

And from that conversation emerged the “this looks like the equations in a barrier method.” Now, I have asserted that since my thesis was about barrier methods, I'm the one that said it. Walter says, yeah, but I was your advisor, you know. I think I'm se-- Michael said, no, no, I knew about it. Philip knew about it.

So what was interesting was, if Karmarkar's thing had happened a few years later, I'm not sure there would have been anybody who really knew about barrier methods, except maybe Fiacco and McCormick. But we all did. So then we thought, well, maybe Karmarkar's method has some connection with a barrier method. And you look at it for a while and it does. So that was kind of a big event.

IRV LUSTIG: Right, I know that I was there. And there was all this excitement happening. And then, there was a paper that the-- it was the Gang of Four plus one, we remember calling it--

MARGARET WRIGHT: Right, John Tomlin, because he, of course, was very interested in this too.

IRV LUSTIG: And I know then you wrote a paper saying, look, this seems to be a connection and that there does seem to be something here. Because if I recall correctly, the numerical results that you obtained said, hey, sometimes it is better than simplex, and sometimes it's not.

And that led then, we were talking prior to the cameras rolling, in 1985, I know it was my first time going to a conference at the Math Programming Symposium in Boston, you and Karmarkar were back-to-back as keynote speakers. And so can you describe the environment and what was happening then, as people were reacting to all of this?

MARGARET WRIGHT: Well, of course, many people who were used to the simplex method could not conceive that anything would be faster than the simplex method, especially given what had happened with the Khachiyan, where it turned out not to be faster. OK, Karmarkar had been a student at Berkeley. Dick Karp was his advisor, I think. And he knew about algorithms. So he was absolutely plausible, as a person, to think of this. And he gave the problem in a different format. It wasn't in a standard form for LP.

So what had happened with us, in the meantime, was to put it into a standard form, and then try to figure out what it was doing, OK. So there was going to be this math programming meeting in 1985. And one of the big things that was happening was, is there a polynomial-time algorithm that's faster than the simplex method? Is it Karmarkar's method?

OK, so originally Walter was asked to give that talk and then, at the last minute, I've even forgotten what happened, but he didn't go. So I got chosen to give the talk.

So the group of us sat around and worried, what should we say. And we knew it was going to be very competitive. People were already saying they had found a better way to do this, blah, blah, blah, blah, blah. Anyway, I gave that talk. It seemed to me, I gave the theorem showing there was equivalence. You had to choose the barrier parameter in a certain way. But the steps were identical.

And then I gave our numerical results. I'd say half the audience was interested in the theory, half was interested in the numerical results, OK. So we did both. And after my talk where there was kind of an uproar, I would say, this pretty well-known person who is a friend of mine, came up to me and said, this is terrible. This is terrible.

And I said, what do you mean it's terrible? And he said, this can't be right. This can't be right. And I said, it's a theorem. You saw the proof. It's a theorem. We're mathematicians. It's a theorem. Theorems are true. He said, it's terrible. It's terrible. I can't stand it. It can't be right.

So people got really worked up about it, really worked up about it. I think people who had thought this was a fundamental breakthrough never seen before on the planet, were upset because if you say this is a barrier method from the late '60s. Look at the Fiacco and McCormick book, it kind of shatters that idea.

IRV LUSTIG: Right, right. And I know there's stories that I could tell about all of that happened when AT&T-- and I think in Dave Shanno's interview we talked about some of the stuff that

AT&T was doing-- and when Dave said, but it was SUMT. And there's a code. And it works. And if nobody's used it to solve linear programs, but if you did, it would be a barrier method.

MARGARET WRIGHT: Right. That was one of the things. My thesis was about barrier methods. But no one would have ever thought of using a barrier method for linear programming. It was a simplex mindset, which I think we all have to keep in mind that something can come along and overturn conventional wisdom, right. Maybe we should try this for this crazy thing and see if it works. Sometimes it won't work, of course. Sometimes it might work.

IRV LUSTIG: Well, and I know there is a paper, that you wrote around 2004 or something, that talked about the interior-point revolution and how it sometimes changes the pedagogy. Because now, we can't just teach linear programming about using the simplex method. We have to think about interior methods at the same time, which brings a whole different mindset to the problem.

MARGARET WRIGHT: It makes it much harder. I mean, speaking now, because I remember then-- remember I didn't teach-- and people would say, oh, now we have to teach them about Newton's method. And we have to teach the system. And I said, well, they should know it, really righteously. Now that I'm teaching about it, I think, I have to teach them about-- You have to teach about Newton's method. You have to teach about derivatives or you have to have matrices. So I have much more sympathy with that. But it's true, you can't just teach the simplex method anymore.

Leaving Stanford for Bell Labs

IRV LUSTIG: So shortly after that time, I think around 1988, you left Stanford and went to Bell Labs. And what caused that transition to occur?

MARGARET WRIGHT: Well, I mean, I said that the four of us worked together and we wanted all our names to be on every paper. And that's absolutely true. But I was getting slightly fed up with the fact that, in the OR department there was no possibility that I would ever get to be a research faculty member.

And I agonized about it. And I thought, what can I do? So I just started to mention, in the mid '80s, to people that I knew, that I saw at conferences, that I might be interested in leaving. And some of them followed up on it, in particular Norm Schryer of Bell Labs, called me up. And he said, would you like to come to Bell Labs? You know, you could work in the Computer Science Center. And there were a few universities that were also interested. And even though I hated to do it because we had had such a long run and worked so closely together, but I just thought, in 20 years am I still going to be here as a research associate? And you were talking earlier about control. And I think, you want to have some control over your career. It was way before departments might have thought, oh, this is a woman. Maybe we should give her some thought, no, no.

IRV LUSTIG: That's right, because I think all the faculty in the OR department, at that time, was all male.

MARGARET WRIGHT: Yes, definitely. I could have gone to the University of Wisconsin and Bell Labs was my other choice. I visited both places. That was a terrible time for me. You know, I felt I was betraying my friends by leaving, that I didn't know whether I would like Bell Labs. I didn't know whether I would like being a professor. But I ended up choosing Bell Labs. And what a good decision that was.

IRV LUSTIG: And why do you say that?

MARGARET WRIGHT: Because I think Bell Labs is unique in the history of science which, of course, I didn't know then. And its like probably won't be seen again, a big company setting up a research lab, where they just say do good work and that's it. You don't have to work on products. You don't have to work on making money. You just do good work.

IRV LUSTIG: Right, and the structure at Bell Labs at that time was, it doesn't have to be work that's connected with the business.

MARGARET WRIGHT: Absolutely, you could do whatever you wanted. When I talked to Norm, who was my boss, he said, what we care about is that you do good work. Now, he said, it's judged by academic standards. Do you publish papers? Do you give talks? Do you do that? But it doesn't have to be connected with the company. We like it if what you do is connected with the company. If someone comes by and asks you about something, you help them. But he said, you don't have to do that.

IRV LUSTIG: So were there people that you were collaborating with at the time, that were at Bell Labs? Or were your collaborations still happening with--

MARGARET WRIGHT: Well, because we wrote a second book, Philip, Walter, and I, a name which we regret, *Numerical Linear Algebra and Optimization Volume 1*--

IRV LUSTIG: Right, I was going to ask you about that.

MARGARET WRIGHT: Yes, because as we worked on it, it became very clear that I would be leaving to go to Bell Labs before the whole thing was finished. And so I don't remember which of us thought of this. Someone said, let's just put Volume 1, and then we can finish later. Well, of course, we didn't.

IRV LUSTIG: The Volume 2 never came out.

MARGARET WRIGHT: Well, it is sad. But Bell Labs let me wait for almost two years from my first interview to finally going there.

IRV LUSTIG: So when you went to Bell Labs, then, were there people at Bell Labs that you were collaborating with, that were at the lab?

MARGARET WRIGHT: David Gay was one. He was right across the hall from me, you know, author of AMPL, right. And Linda Kaufman, who was another person who had been a PhD student at Stanford. And I met Bob Vanderbei. But there was not much in optimization.

IRV LUSTIG: And how did it feel for you to be now doing things on your own?

MARGARET WRIGHT: Wonderful.

IRV LUSTIG: Wonderful.

MARGARET WRIGHT: Well, no, I mean, I have written that it was paradise to be at Bell Labs. At Stanford we were on soft money. You've been on that path. Every time you wanted to buy a pad of paper, you had to say which grant was paying for it. Which grant is paying for these phone calls?

So when I got to Bell Labs I said, OK, who's going to pay for when I go to trips, I give talks. And Norm said, we pay for you, your salary. We pay for whatever you need. So go ahead take trips, have visitors. It's fine. I thought, wow. And there were so many smart people at Bell Labs. I mean Ken Thompson was there, inventors of Unix. I mean, Dennis Ritchie was there, Brian Kernighan.

IRV LUSTIG: Because you were in--

MARGARET WRIGHT: --computer science. Well, plenty of famous mathematicians there, but remember my PhD is in computer science. And if I had to choose what field I would be in, I think it would be computer science. Although this is an INFORMS discussion we're having, I discovered after the fact, that OR includes math, it includes computer science, it includes economics, it includes statistics. So it's kind of an interesting accident of history, which I don't understand why they're all separate.

IRV LUSTIG: Well, I mean, I know the joke that we used to tell when I was a student at Stanford in the '80s was, well Stanford has four OR departments. Because there was the OR Department, there was Industrial Engineering, there was Engineering Economics and Management, and then there was a group over in the business school. Now the three that are in the School of Engineering finally became only one department since we both left. But it's evolved. And you know, I do want to come back to the topic of OR and how you see it a little bit later on.

MARGARET WRIGHT: But Bell Labs, when AT&T was a monopoly, they had an economic center. They mainly studied the economics of monopolies. But they did other things too, demand and supply, and traditional economics.

IRV LUSTIG: Well, and I think if I recall correctly, the group that was at Murray Hill had that research freedom.

MARGARET WRIGHT: Correct. I was incredibly lucky to be in the group that, in a sense, I mean, it amazes me now, that you could just do whatever you want.

IRV LUSTIG: It was an industrial research lab.

MARGARET WRIGHT: Right, but it didn't have any ties to the business, except I worked on some things that were interesting connected to the business. But that's because they were interesting in terms of optimization.

Wireless System Engineering at Bell Labs

IRV LUSTIG: Right, so is there something from that period at Bell Labs that you particularly have good memories of?

MARGARET WRIGHT: (Wise) wireless system engineering for wireless systems. Now, this is a long time ago. But it was something they were interested in. And I worked with Brian Kernighan, who did the user interface. David Gay and I did the optimization, Steve Fortune did the computational geometry, because it modeled rays in buildings, you know, reflecting and moving around the building.

And the wireless engineering people who did the engineering work. We wrote a paper. We had a code that they were going to use to sell wireless systems. It was great. And I learned about wireless engineering, which I never would have done before. And we had a lot of fun, a lot of fun.

IRV LUSTIG: Do you think that any of that work that you did then has had an influence on wireless technology, today?

MARGARET WRIGHT: I believe it has. They've, of course, gone way beyond it. They went and did something that was outdoor, for outdoor wireless systems. And the company people said, we have to have a cute name for this. We have to have a cute name for this product.

So one guy said, I'm going to think of cute animals. So he came up with Ocelot. It's called Ocelot. And people would say, what does it stand for? Like optimal-- he said, it doesn't stand for anything. It's just a cute animal. And he had a cute picture of an ocelot. You know, so that was awesome.

Leaving Bell Labs

IRV LUSTIG: So now, in 2001, you came here to Courant.

MARGARET WRIGHT: Correct.

IRV LUSTIG: And what initiated that transition?

MARGARET WRIGHT: This is like, This Is Your Life, Margaret Wright. So the perfection at Bell Labs began to disintegrate. First of all, I mean, AT&T was broken up. Then they split it into AT&T, Lucent, and NCR, I think. So there was that split, which meant now, AT&T and Lucent, which were two separate parts of the former AT&T, had competition.

So there was an agonizing time, I will never forget these conversations, where we were supposed to choose whether we wanted to go to AT&T or Lucent. They were going to split research. And everybody in my center, computer science, we had this incredible camaraderie and feeling of we all wanted to be together.

And some people said, well then, you should really be together. Others said, no, no, what you do is more like math. So in the end, I mean, it was way above my pay grade, but they decided to put basically all the CS people in Lucent and everybody else in AT&T. So it was Lucent Bell Labs.

IRV LUSTIG: And I think in one of the-- and Lucent took over the facility in Murray Hill.

MARGARET WRIGHT: Correct.

IRV LUSTIG: And I think, and then AT&T took the facility in Holmdel.

MARGARET WRIGHT: Right, and one in Florham Park, which was new. OK, so we were all going to stay together. But it was just agonizing, really agonizing. And more important, probably, was the tenor of the place seemed to change.

It was, you have to work on things connected with business, you know. We're going to count how much vacation do you get a year. I mean, when do you come in. There's nothing wrong with a corporate environment if that's what you want. But if you've been in a free-wheeling environment and you change to a corporate environment, it's different.

Plus, and this again may sound obnoxious, but pretty much everybody could get other jobs. Universities were eager to hire people. So the universities in this geographic area got a lot of Bell Labs people who wanted to stay in the area, but they didn't want to be in the new environment. So there was just this agonizing period of about a year, I would say, where people didn't know what to do.

And finally, there was a moment when Brian Kernighan, one of the great figures in computer science, said, I'm going to be a faculty member at Princeton. And it was sort of like that was-- everybody said, OK, we'd better leave too. You know, because Brian was a foundation of the old Computer Science Center. And, just at that moment, the director of the Courant Institute phoned me up. I was in touch with Michael, of course.

IRV LUSTIG: Michael was here, then.

MARGARET WRIGHT: Michael Overton.

IRV LUSTIG: Michael Overton was already here, then.

MARGARET WRIGHT: He had been here since '81, I think. So he knew I had been agonizing about this and what to do. And the director of the Courant Institute phoned me and said, we're looking for a chair of the computer science department. Are you interested?

Well, I wasn't really interested in being chair of the computer science department. I'd been a department head at Bell Labs for three years. And management is not my favorite thing. But I thought, I should go and see what they have to say. I mean, Courant Institute is a famous place. I know people there. So, I did. And here I am.

IRV LUSTIG: So there was not a matter of you-- like when you went from Stanford to Bell Labs, and you said you visited there, you visited Wisconsin-- that here you said, you got the call from Courant. You came over and you said, hey, this looks--

MARGARET WRIGHT: I interviewed at a few other universities. I figured, if I'm going to leave Bell Labs, which is a torture for me to do, and go somewhere, is Courant the best place? So there were a number of other options that I had.

And again, the usual agonizing-- you know what I mean. And people always say, it's a tough choice. But I tend to think, if a choice is tough, it's got to have these ingredients. You're really torn. You know, if it were obvious which one was optimum, you'd just do it.

Transition to Courant

IRV LUSTIG: Now, how is it that you also were moving from being-- at Stanford, being a research associate not teaching. You weren't teaching when at Bell Labs. And now, you were coming into a teaching environment, how were you thinking about that part of the transition.

MARGARET WRIGHT: So when I was at Stanford in the OR department, I had taught a few advanced optimization classes, actually. The very first time, right after I finished my PhD, I taught the elementary numerical analysis course because the computer science department needed someone to do that.

When I was at Bell Labs, I taught twice, I guess, at Princeton. So I had some experience with teaching. And I knew that it wasn't always fun. But on the other hand, you get to see the friendly, eager faces of all the students that are really interested in what you want to teach, ha, ha, ha.

But I was going to be chair of the department here. And that was the part that I think, in retrospect, worried me the most. Because I had heard stories about people from research labs going to academic environments in administrative positions, and having a lot of trouble. So a very well-known guy at Bell Labs had taken an academic job in New York as chair of the department.

At Bell Labs, if the director had a meeting, you would go. I mean, Bell Labs had a lot of people who were kind of wild and didn't obey orders. If the director said, I'm having this meeting, you'd go to the meeting. If the director made a speech during the meeting, you'd listen, OK.

So this guy went to a university, not here, and called a meeting. And I guess half the people didn't come. Half the people were late. And they would pay no attention to him. And he was used to this other environment. And the first thing I went to at NYU, a welcome to new faculty or something, we were all standing-- it was a big meeting,.

It wasn't just in Courant. It was all the new faculty. We were all standing around. And the president of the university was going to make a speech. Half the people just kept right on talking. And I thought, gosh, you know, if the president of Bell Labs had been giving a talk at Bell Labs, you would be quiet and pay attention. So I thought, OK, it's a little different environment. There's much less hierarchy.

Service to the Profession as a Woman

IRV LUSTIG: So I know, also, you spent a lot of time giving service to the profession at large. I mean, I know probably the majority of it was SIAM, I think. So can you speak about some of the things that you've done-- I know, I think, you were president of SIAM, before a bit-- That you're proud of accomplishments in that role.

MARGARET WRIGHT: Well, it's hard to talk about this, in a sense objectively, because some people, and you know this very well, are really good on committees. And others are terrible. Sometimes you meet someone and you think, if I'm on a committee with you, I want to be against whatever you're proposing.

And also, we have to be frank about this, I think more and more and more, people that make up committees are thinking, we'd better get a woman. We'd better have a woman, you know, especially in math, computer science, OR, whatever, all right. I mean, I know that that's true. I think that it's good that they're paying attention to that. Because once, there was a SIAM meeting on optimization, and I got a phone call from a really great guy.

And he said, hi, we would like you to be on the organizing committee of this meeting. And I thought, oh, this is nice. And then he said, I have to tell you, we've already chosen all the invited speakers. We've already defined the themes of the meeting. And we've already organized all the mini-symposia.

And I said, well, what exactly would I do? And he said, well, we'd want to hear your opinion. I said, but you've made all the decisions, haven't you? And he said, right. I said, no thanks. And I thought, hmm, OK. So I think that was gradually changing.

And I was on the SIAM council. And when I ran for the SIAM council, a very well-known guy was talking to me about why I should do this. And he said, well, you know that in SIAM we always try to have two candidates that are both strong, so there won't be a sort of setup and then someone no one's ever heard of. He said, but unfortunately, in this case, you're going to be the sacrificial lamb. Because there was a pretty well-known person running.

And I thought about it and I said, fine, it's OK. Well, I won. You know, I thought, ha, sacrificial lamb, eh.

I advise young women who are facing this issue where they go to a university. They're the only woman in their department. And they want them to be on every committee. You know, they'll say, will you be on the university committee for this? Will you be-- because they want to then say, look, we have a woman.

So I say, you should only do it if it's going to benefit you. Do not do it just to be a good person because, which has happened many times, then that young woman comes up for tenure. And people say, well, she didn't write any papers. She was just on all the--

IRV LUSTIG: She went to all the committees.

MARGARET WRIGHT: She was on all those committees. So I say, if you don't think you're going to get anything out of it, don't do it. And don't feel bad about it. You know, there's a stereotype, which is often true, that women feel they have to be the goody two-shoes. And you know, your chair says oh, please, please, please, won't you be on this committee? And you have to be a little heartless and just say no.

Women in STEM

IRV LUSTIG: You are one of the early women in computer science getting a degree from Stanford in the mid '60s. And now we're almost 50 years later. Scary, isn't it, right. What changes do you think you've seen in the course-- I mean, you mentioned earlier, obviously, the equal pay aspect. But that was early on. But what changes have you seen in terms of how women are being treated in, I'll say, STEM in general, right, not just in computer science or math or what have you, over the course of your career?

MARGARET WRIGHT: I think there's a lot of talk about it. I think there is much less action than there should be. I think that there are numerous examples-- I won't list them-- where there's, say a big important conference in optimization. And you look at the list of speakers. And there is maybe one woman and 20 men.

And you look at the organizing committee. And there are 40 men and one woman. I mean, you know, I think this is 2019. Think of the optics of this. You know, this is a big field. I can name plenty of people. But I think it's hard to change that. I think you have to deliberately think about it and not just say, let's pick the best people in this area who all are the people I happen to know and are my buddies.

You know, it's hard to change-- I understand it's hard to change that. But I think if you don't start saying, are there any women in this field? Would they be any good? Maybe they could talk about a different topic than the one we've got in mind. And I think there's less of that than there should be. Things are way better.

I was just at the ICIAM, International Congress for Industrial and Applied Math in Valencia, Spain. And the two women that are running that organization, are women. And they had plenty of women speakers. And I thought, OK, this is, you know, it's an example. So it's getting better. But it's still not perfect.

I think, you've read this a million times, overt sexism is much less. People no longer say, as they said to me when I went to Sylvania, do you know what a cosine is. I thought, cosine. They don't do that. But they think things like that. Women aren't as good. They don't have the right style. They won't fit in.

IRV LUSTIG: So are there things that you've done here to try to encourage more women being in computer science or in Courant?

MARGARET WRIGHT: There is an organization called WinC, Women in Computing, which is just an organization in our department. They've collaborated with, it's a group at Princeton, I forget, SEES, Women in Science and Engineering. SWEE, maybe, I forget. So I started that when I was chair. I said, OK, we have ACM, many of whom were stereotypical males who were awkward socially and didn't like having women around.

And we started WinC. And it's thriving now. I mean, it's really good. And we get together every year with the group from Princeton and have a meeting for undergraduate women. We've had things for high school students in New York, who come with their teachers. And we're very careful to have, see this is ageism in a way, but we have young women, students preferably, give talks about the research they're doing and how interested they are in it.

The high school girls, I'm using the term girl appropriately, relate to that much more than if I talk about how excited I am about my research.

Operations Research and Related Disciplines

IRV LUSTIG: So I want to return back to a topic that we touched on a little bit earlier, which was the OR, math, computer science, et cetera, and how do you view the world today, as it sits. You know, OR now has for the past 10 years or so, INFORMS has been using analytics. Now we hear a lot about data science, et cetera. How do you view these areas as being separate, together, different, et cetera?

MARGARET WRIGHT: Well, I happened to be looking at a website this morning that said that operations research is a field of applied mathematics. And I thought, I wonder how INFORMS would feel about that definition. I think, ideally, and it's kind of I'm an example of someone who's worked in what could be called math, computer science, OR, and maybe statistics.

IRV LUSTIG: When I was in academia, which is now 30 years ago, you know, we were publishing all over the place. There were INFORMS journals. There was Math Programming. I think the key paper that I wrote with Dave Shanno and Roy Marsten was in *Linear Algebra and its Applications* because they did a special issue about interior point methods.

MARGARET WRIGHT: So that's good---but I think that the field is becoming more diverse but also more fragmented.

Retrospective

IRV LUSTIG: Yeah, I agree. So what would you view as your main contributions to the field? I could say OR, optimization, is probably the right place to place you, computer science. I'm not sure. But I think optimization is probably the right place.

MARGARET WRIGHT: Right, so I would say the work on barrier methods. I mean, I actually liked my thesis a lot; the stuff about Karmarkar's method and what we showed, the equivalence, and all that. And then, because of the Wise work, I got interested in optimization without derivatives.

IRV LUSTIG: Anyway, well, I want to thank you for your time again, Margaret. This has been a fascinating discussion. And so thank you for your time in doing this.

MARGARET WRIGHT: Well, thank you for talking to me.