An interview with

Seymour Parter

Conducted by Philip Davis
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DAVIS: This is an interview with Professor Seymour Parter, conducted on April 20, 2004, in Phil Davis' office in the Department of Applied Mathematics at Brown University. Seymour are you still teaching?

PARTER: No, I'm not teaching.

DAVIS: But you're still into research and so on?

PARTER: I do some research, with a little luck I'll get something finished today.

DAVIS: Very good. And you've come to work with David Gottlieb?

PARTER: Right.

DAVIS: In the days when you were teaching and had graduate students, you had many graduate students I understand, was this a day when the Web was available to the students?

PARTER: No, not for most of them. I mean the Web was available but most of them didn't use it. My last student graduated perhaps in 96 or 97, and, no even earlier than that, I retired officially in 96 so maybe 93, they never used the Web.

DAVIS: They never used the Web? This is something that's on my mind as a matter of fact because I go to the new book shelf, I see so many new books in mathematical fields, I ask myself how is it possible to keep up with so much information, when a person has a doctoral student how do they narrow down the information these days?

PARTER: Well, first of all, there is always an, I shouldn't say always, in fact I may digress… One of the impressive things to me is if you go into any science library and look at the math journals prior to World War II, there are ten, twelve, maybe three that dealt with applied mathematics, and now never mind all the new books just think of all the journals.

DAVIS: All the journals?

PARTER: Yeah, there are at least three major journals in matrix theory kinds of things that I can think of, there's at least four or five major journals in numerical analysis per se, several journals in applied math per se, the problem has been there for certainly while my students were around, there were many many journals, yeah I can remember in 1955 or so there was just one SIAM journal, and then around 1960 they introduced the SIAM Review, and now SIAM itself has about ten journals.

DAVIS: About ten journals, that's right. I had the following experience I was going to work with a, I won't say he was a student of mine but he was a bit beyond that as a matter of fact, and this person was so in love with the Web that he kept looking up stuff and looking up stuff, looking up stuff more and more, getting more references, and not working on the thing that we had set up. In other words, the Web was a distraction to his work in some way.

PARTER: But I don't think that's new, I remember, unfortunately I've forgotten the name, but I was at a particular industrial laboratory kind of place and I got involved with working with a man and I was overwhelmed by the fact that he barely could move until he had looked up fifteen references, was scouting around to see what was done –

DAVIS: How did you cut it off?

PARTER: I don't think we ever finished the project.

DAVIS: That's the point.

PARTER: I think that that's a question for different people's personality, I think, as a matter of fact that's related to something I always say to people, mainly you know they say that mathematics is a young man's game, I think it's a young man's game only because the young are ignorant. They don't know too much so they come up with new ideas, I think that when you know too much or you are in the habit of looking to find out things too much you are overwhelmed with what's already been done and the problem you're looking at and you don't do much with it, and I believe that, I believe for myself. For example, I believe that a great deal of the research I've done in the last ten years of my really active period arose because of problems that I knew a lot about but had somehow bypassed and suddenly I got an idea, and in fact at that point nobody else cared about those problems but they bothered me still because I knew too much and perhaps I felt they were productive years, but I suspect that if someone would look at my vita they would say well in those years you wrote papers that are not in the mainstream, and perhaps they were and perhaps they weren't, but they were things of interest to me, but they interested me again because I knew too much, I knew a great deal and was probing hard problems at that point.

DAVIS: Yeah, I tend to agree with you that if you know too much it's a hindrance actually. Well, let's go back now a little bit. Where did you grow up Seymour?

PARTER: I grew up in Chicago.

DAVIS: And you went to a public high school in Chicago?

PARTER: I went from a public grade school into a public high school.

DAVIS: When did it occur to you that you were good at math, and that math might become your –

PARTER: Ninth grade. [It] didn't occur to me that it would become a profession but it certainly appeared to me that I was good at it as soon as I took my first algebra class.

DAVIS: Really good at it.

PARTER: I knew I was good at it.

DAVIS: And then where did you go to college?

PARTER: I went to college at the Illinois Institute of Technology in Chicago, I have a bachelor's degree and a master's degree from IIT.

DAVIS: [Did you] have great teachers there in math?

PARTER: I certainly did.

DAVIS: Who were some of them?

PARTER: Well, I wrote a master's thesis with a great man named Karl Menger.

DAVIS: Oh my goodness, he was at IIT?

PARTER: He was at IIT, and a topologist, and there is a very, I don't know if he has the same reputation in algebra as Menger had in topology, but there's a man named Gordon Pall who worked in number theory and algebraic kinds of problems. There was a man named Lester R. Ford who wrote the famous book on automorphic functions –

DAVIS: And difference equations I think.

PARTER: I don't think that's the Ford.

DAVIS: I remember studying automorphic functions in that book of Ford, as a matter of fact, I thought it was very clearly written, that's right, and I can't say as much for a lot of the stuff that he's written. A lot of the stuff is written, I don't know, to make an impression on somebody rather than to convey information clearly.

PARTER: Well, I don't know if it's that so much as it is the style of writing has changed, again, this is a sign of my old age, if you go to the library and you browse in the pre World War II math books, first of all you'd be overwhelmed at the people who are writing the papers, and you know they are still household names, there were fewer mathematicians writing, and I can only read in English, I can read German if I have to, I read French if I have to, but I read mostly in English when I did this browsing and it's written in sort of a nice informal style, you do this, you do that, and then you see somehow this is what we've proven to put it all together. And now, and I find myself doing it. I'm rewriting something now to get more, get the lemma out, get the word lemma in there, and write it down in a formal way as opposed to just discoursing and saying where we're going. And, actually, that isn't so bad because in this particular sense there was something that I was taking for granted, David was taking for granted, or it was obvious, I started to formalize it as a lemma –

DAVIS: This was some hypothesis of something?

PARTER: Just something we got, you know, we've gotten so far now you see you only have to prove this, well I'm not sure of that anymore, I might be sure by tomorrow but right now I'm nor sure of it. So I think that this urge to be formally correct and formally precise tends to make the writing harder and it tends to let you say less, also the editors don't let you talk, don't let you ramble, they complain about it. And I think rambling is really kind of important in telling where you're going and why you think you're going there and why you think the problem's important.

DAVIS: Yeah, the editors of the magazines always complain that, you know, they have a limited amount of space and so on and so on, but I tend to agree with you on this topic and I think maybe this is a symptom that we are of this certain generation.
PARTER: We are in fact. [Laughter]

DAVIS: We are in fact of that generation, no doubt of it. So after, what was your topic with Menger, by the way?

PARTER: Generalized metric spaces.

DAVIS: Oh, this was log linear spaces or even more general?

PARTER: Well, there is no linear structure in these metric spaces, we have distance, gave up the symmetry on it, we kept the triangle law [triangle inequality], we gave up the symmetry. On rare occasions we even allow the distance between points of the zero. Menger had already written several papers[1], and I'm not sure, at this point I don't remember whether I allowed zero distances or not, but certainly not symmetric distances.
DAVIS: Do you have any Menger stories to share? What sort of a character was he?

PARTER: Oh he was a great man, he's absolutely a great man.

DAVIS: He was from Vienna wasn't he?

PARTER: He was from Vienna. I urge anyone who finds the time, first read the AMS notices which has an obituary of him[2], written well after he died, just to get a sense of who he was. Maybe I got that sense because I already knew him personally. You read this book, which I think you reviewed on people who solved the Hilbert problems[3], you are amazed at how many of the Germans were helped along by Menger, attended the Menger seminars, etcetera, and participated. He was as a human being to talk to and to deal with he was extremely nice.

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[1] Karl Menger, Statistical metrics, Proc. Nat. Acad. Sci. U.S.A. vol 28 (1942), 535-537.
[2] Seymour Kass, Karl Menger, Notices of the AMS, 43, no.5, 1996, 558-561.
[3] Benjamin H. Yandell, The Honors Class: Hilbert's Problems and their Solvers, AK Peters, Natick, Massachusetts, 2002. Reviewed by Phil Davis in SIAM News, June, 2002.

DAVIS: Did he discuss philosophy at all, because he was a member of the Vienna Circle of positivist philosophers?

PARTER: I never discussed philosophy, per se. But, I'll tell you one or two of my favorite Menger stories. In the first one, he was talking about how people sometimes draw spirals by taking a compass, you mainly draw a semi-circle and then you make the distance between the radius smaller and you cut them up making another center, making another semi-circle –

DAVIS: Making tangents of something –

PARTER: And keeping- the well, yes. And he said those constructions always bother him and after a while he realized what it was, the second derivative wasn't continuous. Now whose eyes can see that the second derivative isn't continuous?

DAVIS: Menger's eye could see a derivative running continuous?

PARTER: Right. And the other story, you know Menger wrote two famous books[4] [5]and they were so famous and so complete, it's a sin to do something so completely that no one can ever follow you up. One was in dimension theory, and his development of dimension theory while important when he did it was superceded by other people's approach to dimension. He also wrote a book called Curve Theory, they're both in German so it's Kurventheorie, but, in fact I once asked him about writing a thesis in curve theory and he said it's done; there's nothing left to do. But he told this story how when he first got started with curve theory he was a young man, he was eighteen years old or so, and he was stopped by an older professor on the street and who said well Karlie what have you proven lately about curves? And Menger said, "Well, the last thing I proved was that a curve cannot consist entirely of end points", and the man went away just laughing his head off. So the next day Menger came to this man and handed him an example of a curve, which met Menger's definition of a curve, in which the end points were dense. [Laughter]

DAVIS: Menger had been, I believe, at Notre Dame for a while, and when he was at Notre Dame he wrote a little pamphlet –

PARTER: On algebra of analysis?

DAVIS: Well I don't if that was it, he wrote a little pamphlet on a certain take on calculus.[6]

PARTER: That he did while I was in IIT.

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[4] Karl Menger, Dimensiontheorie, Tuebner, 1928.
[5] Karl Menger, Kurventheorie, Tuebner, 1932
[6] Karl Menger, Calculus, a Modern Approach, Ginn, 1955.

DAVIS: And I read that when I was doing my undergraduate thesis and I thought well it might help me, but it didn't in fact and that whole development didn't take off and I understood that it was a big disappointment to him.

PARTER: That's true. When I was a student, working on my master's degree, I don't know if he did it at Notre Dame, but he certainly tried to do it at IIT and we tried to teach calculus on his notes, I was a T.A., and we didn't have any trouble with it but the students were dying and the engineers were going crazy, they just hated it.

DAVIS: They didn't like it.

PARTER: Well, they wanted those differentials in there and there were no differentials.

DAVIS: It was all algebraic?

PARTER: All algebraic? Well, the notation was algebraic; the analysis was still analysis.

DAVIS: Well it had to be so.

PARTER: But the notation was all very formal and, you know, there are certain things in there that people had adopted without realizing that he did it first, I mean maybe everybody had the same idea. For example, you see many people now who don't like writing F (x). They just want to write F.

DAVIS: F?

PARTER: You say F is evaluated at the point x, but F is the function. He had certainly had that idea, among other things that I can remember, that was a very very long time ago.

DAVIS: Well, the notation of functions has changed over the years. Now you have this business where it's F colon with an arrow and things of this sort. After Chicago where did you go for a Ph.D.?

PARTER: Well, come now, there's a small hiatus. Yeah I went to work at Los Alamos [National Laboratory], which is where I got involved in computing. With my master's degree I went to work at Los Alamos, and Los Alamos essentially sent me back to school.
DAVIS: Who were some of your colleagues in Los Alamos, do you remember? This was before the day when what's his name was there –

PARTER: It was after the day, after the day of Oppenheimer. I never knew Oppenheimer. It's my running joke, and you may as well hear it. I came to Los Alamos in March of 51, and in October of 51 they handed me an airplane ticket to Washington, D.C. and a SEAC manual, and I went off and joined the group that was computing on the SEAC.

DAVIS: Oh, you were there? I was there on the SEAC in 52.

PARTER: Well we left in May of 52.

DAVIS: Ah, then I missed you, cause I wasn't there until perhaps June or July of 52.

PARTER: No, no, we left on May 1 of 52. That's the point I'm to make as my joke. We stayed a very long time and one of the reasons I was selected to go, as I always say, is not because I was smart, everybody was obviously smart. It was just that I was a bachelor and nobody gave a damn whether I came back to Los Alamos or not. So I soon became one of the few people who were working on the big machines. Everybody else was working on what we called CPCs, which was a card programmable calculators by IBM. So I was very soon working for a person, Bob Richtmyer, who was the associate director of T-division, I was Bob's programmer, although he programmed –

DAVIS: That's at Los Alamos?

PARTER: At Los Alamos –

DAVIS: Right.

PARTER: He did a lot of programming on his own but I worked for Bob directly.

DAVIS: Well you were at SEAC in Washington you must have known John Curtiss[7] and John Todd and Milton –

PARTER: I knew John Todd and Olga[8], as a matter of fact when I –

DAVIS: Milton Abramowitz?

PARTER: I didn't know Milton, I knew Todd and Olga and actually that ties in with what we said before when I was going from Los Alamos to Washington, I naturally stopped in Chicago to visit my family, and I went over and I visited Menger and told him what I was up to, and he informed me that John Todd and Olga were at the Bureau and that Olga had been a student of his, in a class, not a student of his for a thesis.

DAVIS: She's from Vienna also.

PARTER: Yes, so she had been a student of his in Vienna and he knew her well and etcetera, so yeah, in fact John Todd, we used to see John Todd every morning, we worked the midnight to eight in the morning shift and then we closed up and we'd walk in, to see John Todd and use his office to call Los Alamos.

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[7] John Hamilton Curtiss was chief of the Applied Mathematics Division of the National Bureau of Standards from 1946 to 1953.

[8] Olga Taussky Todd, mathematician and John's wife. See Chandler Davis, Remembering Olga Taussky Todd, AWM Newsletter,January-February, 1996, Volume 26, #1.

DAVIS: Did you know some of the other people that were there, Alan Hoffman, Miles Newman?

PARTER: I barely knew Alan, I got to know him later, I didn't know, there's was a young man there named Karl Goldberg.

DAVIS: Oh, Karl Goldberg, yes –

PARTER: And actually, Karl Goldberg, there is a little nudge, in my guilty conscience in regard to Karl, I'm not sure I referenced him in one point where he should have been referenced. I think I did, but then he wrote a little thing. As you may know, very early in my career I wrote several papers that related graph theory to matrix theory.

DAVIS: Yes, I was going to ask you about that.

PARTER: And the basic idea of one of those, which is in fact related to what I'm doing right now, actually came from this little paper that Karl had written and put in the proceedings of the Bureau of Standards, or something, and no one has ever seen it, you know, and I always knew maybe I should have played it up more, the gem of the idea was in Karl's paper back then.

DAVIS: Unfortunately, Karl died young.

PARTER: I remember that.

DAVIS: Now when you talk about graph theory in terms of, it was, linear algebra wasn't it?

PARTER: Matrices, right.

DAVIS: Matrices. Did you use theorems from graph theory or combinatorial graph theory or is it just the diagrammatics?

PARTER: In theorems that I'm using for what I'm doing right now, if it works out, is that observations of data that I thought was right is really right. I learned about graphs, I learned about graphs from, I mean it's part of my education and so is my master's degree. So I knew about graphs theory from Menger and one of the first things I did in graph theory was merely the structure, I was dealing with trees, matrices associated with trees, and they came about, again in an applied way. I was dealing with people. I was visiting Brookhaven and there were people there doing tracer experiments, and they had these models of what the body was like and they would inject in one box and it would come another box and they had these matrices which described equation dX/dt = AX, X is the concentration of in turn pick the part of the body. At one point in the discussion, I argued with them about something, and they said well in the case of no recycling there is such and such. I said no recycling, ah ha, the graph's a tree, and so I wrote those papers in which the graph was a tree. And, but then in this paper, the one that came later, I used the topology of them and I used the following basic fact that with every graph there is a skeleton which is a tree, and you can get that skeleton in the following way, go to a cycle, remove one arc, now that's no longer a cycle, go find another cycle and remove one arc, keep doing that and in this way you'll construct a skeleton. And the other thing that I used was that the number of arcs that you remove in this way is an invariant, no matter how you choose the cycles, and that that number is the number of independent cycles, and I believe Menger called it the Betti number, although I'm not sure.

DAVIS: That's a topological concept.

PARTER**:** It's a topological concept, right. And I used that fact, and I use a rather strongly.

DAVIS: Let's move on out of Los Alamos. When were you getting a Ph.D.?

PARTER: In 53, in the spring of 53, I went to NYU. NYU had just gotten a great big Univac, and the team of Richtmyer.

DAVIS: Robert Richtmyer?

PARTER: Robert David Richtmyer, who's just died at age 92, I'm not sure maybe at the age of 93, passed not too long ago. Richtmyer and I and a man named Lester Barnhoff, and a man named Roger Lazarus, Paul Stein, that's it, we all went to New York to work on the new Univac computer at NYU, part of something called the AEC Computing Center. We were doing a problem for Los Alamos, and as a result of that I just stayed on at NYU as a student. The Lab found ways to keep sending me back to work on various projects after that one was finished. All the time I was a graduate student at NYU, I was an employee of Los Alamos, and I finally got my degree in 57.

DAVIS: That was a good deal when you came.

PARTER: Well, I worked hard. I was a full time student and working.

DAVIS: Now that's all right.

PARTER: It was a good deal.

DAVIS: Yeah, it didn't hurt you. And, so, whom did you work under?

PARTER: I worked under Lipman Bers.

DAVIS: Under Lipman Bers, ah yes.

PARTER: I wrote a thesis in the quasi-conformal maps of multi-connected domains.

DAVIS: Yeah, Bers had a partner up at MIT, and they both worked on the generalization of complex analytic functions.

PARTER: I think you mean Ahlfors –

DAVIS: No, not Ahlfors, no, there's somebody else, it will come back to me. Anyway, that's irrelevant –

PARTER: There was Gelbart.

DAVIS: Gelbart?

PARTER: He was at Syracuse not at MIT.

DAVIS: Abraham Gelbart, that's right. Anyway, that development also didn't seem to work out. Is that correct?

PARTER: Well Bers had two developments. One was that one, one was something called pseudo-analytic functions.

DAVIS: That's the one I'm talking about –

PARTER: That's the one he did with Gelbart and that didn't work out –

DAVIS: That didn't work out.

PARTER: But the other one was things that he did, not exactly together with him, I don't know if they ever wrote a joint paper, but sort of in parallel with Ahlfors and that was the quasi-conformal maps, and that did work out and he went from that into Riemann surfaces and there he did great things.

DAVIS: I had a, not a course on quasi-conformal maps with Ahlfors, but I listened to him, his lectures on that topic, and he had something called invariant distance, or invariant, something like that, and it played a role in this and I remember that Henry Pollak, who later went on to run mathematics at the Bell Labs, wrote a thesis on this topic.

PARTER: Oh yeah. That's interesting I didn't know that.

DAVIS: Henry Pollak was a student of Ahlfors.

PARTER: Well that's interesting, because Henry and I interacted a great deal at the Conference Board on Mathematical Sciences.

DAVIS: Yeah, Henry got interested in education.

PARTER: Right, and had a big educational project that he invited me to be part of. And I hadn't realized that we both had written our thesis on the same subject. Yes. Anyway, so all the time I was employed at, all the time I was working on my thesis I was employed at Los Alamos.

DAVIS: When did you get back into academia?

PARTER: Well, I got my degree and I left Los Alamos and NYU and I got, it's a complicated story which I probably won't go into too much, except that I went to MIT in September of 57, and spent one year. Los Alamos and I agreed, happily, because I felt greatly indebted to them, that if I really wanted to go try academia I should go do it. I was encouraged in that by [Stanislaw] Ulam and by Carson Mark.

DAVIS: Did you get to work with Ulam at all?

PARTER: I knew Ulam, I wouldn't say I worked with him, I knew him, I was quite friendly with him. He had something to do with my decision in fact to go into academia.

DAVIS: Was Gian-Carlo Rota there?

PARTER: Gian-Carlo, while I was a graduate student at NYU, Jack Schwartz and Gian-Carlo and Dunford[9], although I never saw Dunford, came to NYU for a year. Gian-Carlo was a student of Jack's, he hadn't finished his degree yet, and they came. Jack came visiting for a year and then never left, Gian-Carlo spent the year there, so I got to know Gian-Carlo quite well.

DAVIS: Coming back to NYU, did you get to know Courant or [Kurt] Friedrichs or [Wilhelm] Magnus? You know any stories that you can make public?

PARTER: Well I don't know if I can tell you in public, I know all of them. You see, again, because I really did have a rather strange career and I won't go into all the details. I just spent lunch hour telling David some of those stories –

DAVIS: You should have taped it.

PARTER: Yeah, but because of the fact that I was an employee of Los Alamos all the time I was there and I had come there with Bob Richtmyer, who later left Los Alamos to became a professor at NYU, my friends were the faculty. I had already known Peter Lax, Peter and I were then, and still are, very good friends. So I immediately became, got to know [Louis] Nirenberg, and we became very good friends, and so the faculty were my friends. I went to lunch with Fritz John on a regular basis, or with Louie on a regular basis. Kathleen [Morawetz], all of those people, were my friends, and I had friends among the students, but really much more so among the faculty. I didn't know Courant that well, I knew him well enough to talk to him, well enough to say hello to him, and Friedrichs and I certainly knew each other. I have a great Friedrichs story –

[9] Nelson Dunford, Yale University. Jack Schwartz's PhD advisor.

DAVIS: A Friedrichs story, let's hear it.

PARTER: Right. Kurt Otto Friedrichs, and that was always a joke to me because his initials are KOF, and Friedrichs was always coughing, {coughing}. He would give a lecture coughing. Anyway, after my degree was essentially done, I was still hanging around, actually NYU gave me, there was one year I was not working with Los Alamos, and NYU gave me a postdoc even though the degree hadn't been completed yet. So I spent this last year because I was essentially done, and I was sitting in on a class that Friedrichs was teaching. Oh, I don't know, a third of the way into the semester, quarter of the way into the semester, he caught me at tea time one day and he said I've noticed that you're sitting in on my class and I have a favor to ask. Would you ask questions? I said, Professor Friedrichs, I don't want to make it sound like I'm, you now, saying nice things about your lectures, but you know I'm a little more advanced than most of the students in the class and I don't really have any questions. And he said oh I know that, that's okay, but you have to ask me questions anyway. He said I'll tell you how it is, most lecturers lecture at a constant velocity, I lecture at a constant acceleration, and you have to stop me to ask questions. Stop me and ask me the time of day, but ask questions.

DAVIS: That's a wonderful story. Well, anyway, you did get back to academia.

PARTER: I did get back to academia, right. Well I had never been in academia, I'd been a student, but now I was part of the faculty.

DAVIS: Where was your first instructorship or professorship?

PARTER: I told you at MIT.

DAVIS: At MIT. And then after that?

PARTER: I went to Indiana for two years. In all these cases, I had a tie to a computer center. I had a joint appointment at MIT between the math department and the computer center. And at Indiana, I actually had the auspicious title of Associate Director in Charge of Research for the research computing center. I was there for two years, and then I went to Cornell for two years, and then I was a visitor at Stanford for one year, and then I went to Wisconsin, and I've been at Wisconsin ever since.

DAVIS: Since when?

PARTER: I came there in September of 1963.

DAVIS: 1963, so, that' when I came to Brown, so we have been –

PARTER: Both settled down, we put our roots down and we settled down.

DAVIS: That's right, it was good.

PARTER: It was good.

DAVIS: That's right, so why move?

PARTER: Exactly.

DAVIS: Although in those days there were jobs galore.

PARTER: In the early days there were jobs galore, yes, I had many opportunities, tested the waters several times, and turned out a few things that might have seemed at the time more attractive, but we were comfortable so we stayed.

DAVIS: Let me change the subject now from the vita sort of thing to, do you consider yourself a specialist or a generalist in numerical work?

PARTER: [Laughter] That's a very good question. I'll tell you why, actually it's funny question, because, as I told you, I started out on my master's thesis in topology and my Ph.D. thesis was, you know, partial differential equations with bounded measurable coefficients, because in the middle I got involved in computing and I was always interested in computing. That I worked with Bers on quasi-conformal maps was a mistake, not a mistake, an accident. I never went back to it. It was a lark. But when I tell people that I made a major change in my career by wanting to be a topologist to wanting to be a applied mathematician or numerical analyst, they think what the hell am I talking about, this is all within the framework of mathematics, and now you're asking me within even a narrower framework, within even a narrower framework of numerical analysis. Am I a generalist, I would say I'm more of a generalist in the following sense. Well, I had worked in a variety of areas. I did a variety of stuff which involved matrices and graph theory, I did an awful lot of work in iterative methods for elliptic equations, but I also did work in, again, iterative equations but now formulas and this new thing called first order least squares methods. I have a major, so-called major paper, in sparse matrix theory and so I worked in sparse matrix theory and iterative methods. I worked in finite element methods and I worked in finite difference methods. I've worked in spectral methods. I worked in various aspects of elliptic problems, namely, formulating methods and also solution methods.

DAVIS: Matrices are essential.

PARTER: Matrices have been the essential aspect of all of this, but so has been the analysis of equations. I was one of the, one of the first things that I did, which had to do again with iterative methods, but this is 1959, 1960, I wrote this series of papers on estimating the eigenvalues of various iterative methods that people use and a strong tool was the notion of a weak solution of an elliptic equation, which was just beginning to be talked about among numerical people at that time. Of course now it's a major part of finite element theory, but I had to use it. Incidentally that's one of the things that I had learned from writing the thesis with Bers and so I knew about weak solutions and their power and what you could do with them. But I've worked in a variety of things, matrices have certainly played a big role in it, and I've also switched out and done analytical things, I've done some bifurcation theory, I've done some singular perturbation theory, so in that sense I would say I'm more of a generalist.

DAVIS: I would agree with that. I noticed that one of your areas of interest was called semi-circulant preconditioners.

PARTER: Well I recently wrote a paper on that, no that's not correct, that's a matrix theory is what it turns out.

DAVIS: I wrote a little book on circulant matrices, and I wonder whether we're talking about the same thing?

PARTER: We are.

DAVIS: We are, so –

PARTER: By circulant matrix you mean one –

DAVIS: That pushes down.[10]

PARTER: Yeah.

DAVIS: Yeah, moves over one and wraps around –

PARTER: Right. The toughest matrix that wraps around. No, that certainly, these are not semi-circulant PDEs, these are semi-circulant preconditioners –

DAVIS: Ah, they're preconditioners –

PARTER: It's a preconditioner, the point being that people, I can even tell you the names of the people, but they are a group of people who have looked at elliptic equations particularly on a square and said, you know, how can we precondition. Preconditioning means introducing a new matrix A into the problem that looks very much like the one we started with but which you can handle. So there are a few fringes that are different, and so the "semi-circulant". You can make the semi-circulant for, say, for the normal Dirichlet problem. If you took a pure circulant you would be taking obviously, what's the word I'm looking for, periodic boundary conditions, and you would be looking at Laplace's equation let's say with periodic boundary conditions and attempting to use that operator which is easily invertible. Well you can't do Laplace's operator because that's singular, but if you took Laplace plus something then that matrix, if you took the circulant for that, that's easily invertible and then you would try to use that to solve the Dirichlet problem and people have done that. The papers that I got interested in had to do with semi-circulants. So it was a circulant in the x direction and Dirichlet in the y direction, and

[10] A circulant matrix on a list of length n is an nxn matrix whose rows are cyclically shifted versions of the list.

that's again not hard to solve because you solve the circulant part and separate variables essentially since you are on a square. But, again, that's an interesting problem in that particular paper, which has just appeared, there were two aspects of it which I think shows the generalist, broadness, if that's the word I'm going to use. I hadn't realized it the first time I looked at it, but there was another paper by somebody else, whom I can mention but it's not necessary, who had used the semi-circulant precondition. They had used it in the case of equations where there was a epsilon in front of the Laplacian term, and there were lower order terms. So the equation looked like Lu is equal to epsilon Laplacian plus some first order terms. And they had done the calculation, used a semi-circulant preconditioner, but their basic equation they had set up in such a way that the epsilon over delta-x squared term was a constant. So delta-x was chosen to be the square root of epsilon, sort of a strange thing to do but they had done this, and then they had used a preconditioner on this and they got results that didn't make sense to me because they got all bounded eigenvalues for the preconditioner. Tom Manteuffel and I in 1990 had proven that if you're going to get reasonable eigenvalue estimates on a preconditioner then the boundary conditions have got to be the same, not necessarily the same but wherever there were Dirichlet boundary conditions you had to have Dirichlet boundary conditions on the preconditioner. And the problem with what these other people had done is that they had Dirichlet boundary conditions everywhere when their preconditioner had periodic boundary say in the x direction and Dirichlet in the y direction, and yet they were getting beautifully bounded eigenvalues. So I didn't believe it at first. And then I realized that they had this extra condition, and so then the first thing that I proved was that the reason that there was no contradiction was that their limiting operator was not elliptic, their limiting operator was indeed a hyperbolic operator. And then I got a new way to get their preconditioning results and I got preconditioning results in other cases. Then I argued that even though they had the wrong operator their results were still pretty good because of the fact that we all knew that the elliptic problem converts to hyperbolic, anyway. So, all of these things are mixed in that paper[11]. There's the knowledge of hyperbolic and elliptic problems and then there's the preconditioning elliptic, the eigenvalue understanding.

DAVIS: Just to get on the tape for the record, could you say just a couple of sentences on what the function of the preconditioner is?

PARTER: Oh, well when you solve elliptic equations, let's say, simplest case solve Laplace's equation, the matrix which you're trying to invert has a one over delta x squared in the denominator, and delta x squared in the denominator which is to say the elements of the matrix are very large, the elements of the inverse matrix are very nice but that doesn't change it, that helps you, helps you a great deal. You see there are two problems in elliptic theory one is, okay suppose I get this large system of equations, now suppose I solve it, how good is my answer, that's one question. The other question is how in the hell do I solve it. The fact that the inverse is beautifully bounded means that once I get the answer it's perfect, it's good, it's very good. On the other hand, the fact that the original operator has this delta x squared in the denominator means that it gets

[11] Sang Dong Kim and Seymour V. Parter, Semicirculant Preconditioning of Elliptic Operators, *SIAM J. Numer. Anal.*, 41 (2003), pp. 767-795.

very hard to solve the problem and so what you do is you take the original problem which has a delta x squared in the denominator and multiply through by something, so you start out with an AX equals Y, multiply through by B, you have BAX equals Y equals BY and you hope that the new matrix that you have to invert, which is BA is better conditioned. That's a technical terms but what it means is that both it and its inverse behave themselves and if that's the case then it's much easier to solve the problem and all of the standard methods for solving that problem depend on that.

DAVIS: So it's part of the strategy to find an appropriate preconditioner.

PARTER: Part of the strategy is to find a way to solve the problem. So in preconditioning studies, [you] first of all invent the preconditioner and then show that it works.

DAVIS: Have you ever introspected on the question of where your ideas come from?

PARTER: Well, sometimes. I can certainly say that sometimes ideas come from a chance remark that somebody else made; sometimes I don't know where they come from. I mean, I think most ideas come, I've thought a lot about where ideas come from, and I think they come from just scribbling at the problem a great deal. You scribble, erase, you make an example here and you make an example there, and you think about.

DAVIS: Try this and try that?

PARTER: Yeah. I think that one of the things that is important though is to try to keep your eye on what else might be useful. In fact that's the problem with these people who want to study everything about a given problem. They get so focused on the literature on this kind of a problem that they frequently don't realize that information and ideas from other parts of mathematics might play a role. And I would almost say that my most successful work has been when that's what happened. I mean, for example, I wrote this little paper called the, you know, all my matrix theory and graph theory papers they came about naturally, I really got into that in a very natural way and was, as I say, someone was talking to me about the problem he had with tracer experiments and they said no recycling, and I suddenly realized there was a graph involved and it was worthwhile thinking about the graph as a tree, and because Menger had taught me about trees I was able to bring that to bear, and similarly there's the –

DAVIS: Certain fund of knowledge –

PARTER: But realizing that there's other things out there that may be brought to bear and I think that comes in, and then, for example, again, you know my paper which appeared in the Reviews, which was entitled "Use of Linear Graphs in Gaussian Elimination"[12], but people tell me that's a famous paper, and again, it was because I was suddenly thinking in terms of graphs associated with matrices, which indeed. Well I can go back and tell you what Karl [Goldberg] did. Karl made the observation that if you

[12] S. Parter, The Use of Linear Graphs in Gaussian Elimination, SIAM Review, vol 3, no 2, 119-130

made the graph, I don't know if he put it this way, but this is my memory of his observation and it was 1951 when I learned it, so my memory may not be so good. If you look at this graph and you look at the cycles in the graph there's a product associated with the cycles and those are the only things that count when you go to evaluate the products, because the determinant is a product of cyclical determinants -

<u>(TAPE ENDS ABRUPTLY ON SIDE A)</u>
<u>(SIDE B)</u>

PARTER: When is it that I remembered Karl Goldberg's observation that there were these cycles in this graph which were the only thing that mattered when you were computing the determinant and that played a role. Now where did the idea come from to suddenly remember that Karl Goldberg had done that? You know, I hadn't been studying his papers, I had read his paper, I thought it was cute, somehow it stuck in my mind, and it popped up when I needed it and it popped up in a different form, I looked at the cycles and I looked at other things and I don't remember right now whether Karl expressed it that way but I know that from reading his paper I got that idea. So how does that work? Who knows?

DAVIS: Well the more you know the better?

PARTER: I think knowing what you know too deeply is wrong, not wrong, I think –

DAVIS: Not productive?

PARTER: It's not productive, it leads you into looking at scratchy little problems which nobody cares about, and have been likely bypassed. But having a breath of knowledge is sometimes very very useful.

DAVIS: Just a side remark, his full name was Karl Marx Goldberg.

PARTER: Oh, okay. [Laughter]

DAVIS: What's your take on the trade off between teaching and research in the university environment?

PARTER: Well that's a very hard one because I think it's a very personal thing and it may even be time dependent with the person. Look, I used to say, there was never any danger, but I used to say that if someone offered me a permanent appointment to the Institute of Advanced Study I would turn it down, and I would turn it down simply because I could not imagine having nothing else to do but research. I would go out of my mind because there are days when nothing gets done and I don't want to go home depressed. You know, there are days you try, you try, you scribble at the blackboard all day and nothing works, and I don't want that, at least with the teaching you have an anchor, you go in and you have something you're sure of and something you think is worthwhile doing and you do it and you feel good about it. You taught somebody something and you did it right, if you were conscientious and prepared your lecture. So I think that for me I needed them both, now to balance how much teaching that's a different question and I think, I think that varies again with a lot of things which are like what are you teaching, what course levels are you teaching at, and how do you feel.

DAVIS: What sort of courses have you taught over your academic career?

PARTER: Well, again, it didn't come up, but at Wisconsin I had a joint appointment between mathematics and computer science and so I always taught numerical analysis courses in the computer science department. Actually one year I taught a basic programming course, but other than that I usually taught a graduate level course, junior or senior level or graduate level course in numerical methods of one kind or another, and towards the end, the end being the last twenty years perhaps, I regularly taught a course called numerical methods for ordinary differential equations which, although it used differential equations as a vehicle, certainly was broader than that in terms of computation and ideas about computation and things like that.

DAVIS: You had to cover the waterfront in that -

PARTER: Well I tried, I had only one semester, but I tried. And then in math I taught a lot of linear algebra, you know the interesting thing is I never taught calculus, the last time I taught calculus was in the spring of 1958 at MIT.

DAVIS: Out of Thomas'[13] book?

PARTER: No, Thomas' book didn't exist. I don't remember what book we used but it [Thomas' calculus] didn't exist yet. It was just beginning to be talked about. And I taught a lot of the pre-calculus algebra. I never taught the pre-calculus trig. I taught a course which involved both trig and algebra. I didn't like that because we taught it to 300 students. The algebra which we taught, the pre-calculus algebra which we taught, you know, to a reasonable size classes, and I sort of enjoyed that. It's sort of a pleasure to take a bunch of kids who are there because they failed the placement exam and therefore have to take this elementary stuff and try to turn them on. Sometimes you succeed, sometimes you don't, but it's worth trying. And, as I've said, taught a lot of linear algebra, I taught a lot at a junior senior level of differential equations, and in the math department I had taught function analysis and real variables.

DAVIS: Did you get to, at Wisconsin, did you by any chance did you get to know Izzie Schoenberg?

PARTER: Oh yes, very well.

DAVIS: Any Schoenberg stories? I knew him and liked him very much.

PARTER: Well we called here him Iso not Izzie.

[13] George B. Thomas, Elements of Calculus and Analytic Geometry, Addison Wesley, 1963, Reading MA

DAVIS: Iso, he used to sign his name, his letters I-E-S-S-O.
PARTER: Iso –

DAVIS: S-C-H-O-E-N-B-E-R-G.

PARTER: Yeah, the same as the musician.

DAVIS: He invented splines.

PARTER: He invented splines. I don't know if I have any Iso stories, but his wife is still alive, she's 90, still going strong.

DAVIS: I once visited him in his house and his wife baked a Linzer torte, I remember that very vividly.

PARTER: She's not German, she's Dutch.

DAVIS: She's Dutch, but she made a Linzer torte. He was from Romania.

PARTER: He was from Romania. Well his first wife died; she was [Edmund] Landau's daughter –

DAVIS: That's right.

PARTER: And Dolly, his second wife, is Dutch, and was much younger than him. He played the violin and he liked art-work that came out of mathematics. I don't know how to describe that, you have to see what's hanging on his walls to appreciate it.

DAVIS: Yeah, I have an idea what that is.

PARTER: No, I don't have any Iso stories.

DAVIS: Well let's get on to one final topic and then we can call it a day. What do you see as currently the hard problems for numerical computation? Unsolved problems let's say.

PARTER: You know I don't really know. Partly because I haven't been working hard for the last few years and so the things I've been working on are things that people will come and ask me questions about that will tickle my fancy and I'll work on it. But I have an opinion which is related to that, and that is, that again, what I'm afraid people are doing is digging deeper into areas where they shouldn't be. The simplest example I can give you is the Navier-Stokes equations. The Navier-Stokes equations hold a large number of questions analytically and numerically, etcetera, on the hold they know how to use them efficiently enough to build airplanes; they know how to use them efficiently enough to do almost anything they're trying to do. An enormous effort into improving

ways to solve the Navier-Stokes equations, unless you're using it for research to try to understand the analytic theory, I think is a misguided effort.
DAVIS: What does it mean when the Clay institute [Clay Mathematics Institute] offers one million dollars -

PARTER: That's for theoretical work, they want a proof –
DAVIS: For the Navier-Stokes –

PARTER: And they want a proof –

DAVIS: Existence proof ?

PARTER: An existence proof, and a long time existence proof. You have to know does a generic solution blow up in finite time or does a generic solution exist forever. And –

DAVIS: The stability is on.

PARTER: Yeah. And so –

DAVIS: But getting back to –

PARTER: Getting in to numerics, which is a different question. What concerns me is I don't see the community turning to even try to find out what are the real problems in biological questions. I think they should be, I think, well they're turning a little bit to the problems in finance and those are really challenging because they involve simultaneously learning something about finance which none of us know anything about that's why we're professors, and the other thing it involves is learning something about stochastic differential equations, which, again, most numerical analysts simply aren't ready for that. It hasn't been part of the standard curriculum to really understand probability at the level that you need to understand stochastic differential equations. And [Seymour reports that since this inetrview took place, he has returned to the study the work on stochastic differential equations and has changed his views significantly] I might say that in that area I think the experts are barking up the wrong tree, and I've sat in on several classes taught by my colleagues on stochastic differential equations and from my point of view they're asking the wrong questions, they're doing beautiful things from the probability point of view, but they're asking the wrong questions. I can tell you what the right question is. The right question is when all is said and done give me a deterministic problem to solve that will compute the things about the distribution that I want to know, the mean, standard deviation, etcetera. As opposed to telling me oh go do Monte Carlo simulation and then compute this, I can't do that. I want a single deterministic problem. Well maybe it can't be done, but maybe it can be and maybe that's where we ought to be going. Anyway, enough of that, but I think that those are the areas, stochastic differential equations in terms of the problems of physics, in other words if you look, go to the library and look at any book on stochastic differential equations they're proving things about, you know, about the Euler theorems, like y prime equals f(t) and y in an initial condition in a stochastic environment. I don't care about that. I want to see the wave equation with distribution on the parameters of that, I want to see the heat equation with the distribution on the parameters and some information about the solution. That's the kind of thing I want to see. I want to see the problems of physics with the parameters made stochastic and they don't look at that, but computationally we ought to be looking at it and we ought to be thinking about it, we can but we're not. That's one area where we're not going, we're also not going into –

DAVIS: After all every computation is deterministic.

PARTER: Every computation is deterministic so, look at the level that we don't know anything, where we haven't gotten that theorem I want, he says here's a theorem now do this deterministic calculation which will tell you the mean and its standard deviation and all that, do Monte Carlo simulation, we've got big computers go do it, it's an important problem, go see what you can find out, get some insight, we're not doing that, okay, we're just not doing that. We could but we're not. As I say, the biology problems I think we ought to be going into, and I think we ought to stop mining Laplace's equation; the last thing we ought to be doing is Laplace's equation on a rectangle.

DAVIS: Well thank you very much Seymour.

PARTER: Thank you.