An interview with
PHILIP J. DAVIS

Conducted by
William Kolata
and Gail Corbett

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ABSTRACT

Philip J. Davis grew up in Lawrence, Massachusetts about 30 miles from Cambridge. He recalls being drawn to mathematics in high school and getting a book on interpolation and approximation from the MIT Library. After high school he went to Harvard on scholarship, where he majored in mathematics. Upon graduation from Harvard, in 1943, he became a member of the Air Force Reserves and worked on problems in aerodynamics at Langley Field in Virginia. He returned to Harvard after the War and began work on a PhD in mathematics. He wrote a thesis on entire functions and infinite interpolation under the supervision of Ralph Boas. After receiving his PhD, in 1950, he did some post-doctoral work with Stefan Bergman and Joseph Walsh at Harvard. He joined the Numerical Analysis Section of the National Bureau of Standards, in 1954. Davis describes the environment and people that passed through NBS at this time when it was one of the few places in the world studying numerical methods and computing. He discusses his work on the SEAC with Phillip Rabinowitz, with whom he later wrote a book on numerical integration. He also describes his work on the highly cited handbook of mathematical functions, Abramowitz and Stegun. He became a founding editor of the Journal of the Society for Industrial and Applied Mathematics. In 1959, Davis became the Head of the Numerical Analysis Section. In 1963, lead by his desire to write, he left NBS for Brown University where he established a career as a professor of applied mathematics and a writer of technical and non-technical books and articles on mathematics, numerical methods and computing.

This is an interview with Philip J. Davis, for the SIAM History of Numerical Analysis Project, held on Thursday, April 15, 2004, at SIAM. The interviewers are William Kolata, SIAM Technical Director and Gail Corbett, editor of SIAM News.

KOLATA

Phil let us begin. We'll be talking a little bit about, first about your career. What got you interested in mathematics, and what drew you to Harvard?

DAVIS

There was a slender book on interpolation and approximate integration came out of a laboratory in Edinburgh* and a copy discarded from the MIT Library caught my attention when I was in high school. And the beginning of the book says how everybody that comes into this seriously has to provide himself with a pad of squared paper and a copy of Barlow's tables of squares. But, anyway, it had formulas in it which I did not understand, of course, but I got interested in the question of interpolation and how you represent data by polynomials and so on, and I worked out for myself, this was when I was in high school, I worked out quite for myself what subsequently are previously as known as Newton's interpolation formulas, I had that. So this was a start, actually, that takes me up to high school.

When I went to college I was not particularly interested in computation or interpolation or any of those things, I was (eventually) mixed up with, and I majored in mathematics.

*(Sir Edmund Whittaker ran a computation laboratory at the University of Edinburgh from 1913 to 1923. The book referenced above came out of the lab; David Gibb, Course in Interpolation and Numerical Integration for the Mathematical Laboratory, G. Bell & Sons, London, 1915)

KOLATA

This was at Harvard?

DAVIS

Yeah, at Harvard. Why did I go there? It was a neighborhood school. I grew up in Lawrence, Massachusetts, which was about thirty miles from Cambridge, and that was the neighborhood school, my sister had gone to Radcliff and my brother had gone to MIT, and so on, so it was a natural thing. Also Harvard gave me a scholarship.

KOLATA

When you were at Harvard it was in the early 40s, correct?

DAVIS

Yeah, I was a freshman in 1939, and I got out as a senior in 1943.

KOLATA

At that time it was just prior to World War II and also prior to the advent of the computer. Was there any sense in the mathematics department, particularly among the younger people, students, that something like that was developing or was it still too early?

DAVIS

You mean something like interest in computation?

KOLATA

The development of the first computers, was there any awareness of that present at that time?

DAVIS

I would say definitely not, maybe there was some vague rumors floating around that some people were, you know, (working on this). We knew what Vannevar Bush was doing with his Intergraph* and things of that sort. We'd heard about those things, but the thrust in mathematics in those days was quite pure. The mathematics department never even had a mechanical computer, for example, a Marchant, one of these mechanical things that did adding, subtracting, multiplying. If you got a good one it did dividing, no, in those days the only department that had that kind of equipment was the astronomy department because those guys were figuring out comet orbits and things of that sort. The mathematics department wouldn't be caught dead with, in those days, with (doing) computation.

*(In 1927, Vannevar Bush, along with colleagues at MIT, designed a differential analyzer - an analog computer that could solve simple equations. This device was dubbed the Product Intergraph)

<u>KOLATA</u>

So you went through what would, I guess, be the traditional and pure mathematics curriculum in that day?

<u>DAVIS</u>

Yeah, pretty much, pretty much, right. And I came out with a BSC, Bachelor of Science Degree.

<u>KOLATA</u>

What do you think were the strengths of the tools that were provided by that curriculum that helped you in the next step in your career?

<u>DAVIS</u>

Well, I would say that, you know, I knew analysis, real analysis, I knew complex variables quite well, and so I think that that was a help. But then what happened was the War came on and suddenly computation becomes necessary in order to figure out this and that. In all kinds of areas, aerodynamics, radar, operations research and cryptography and Lord knows what more, there were a half dozen areas that involved mathematics quite seriously. And I had had abstract algebra, so I would say that I was as a senior, just a more recent bachelor, I'd say that I was quite equipped to go into some of these areas. It was called the War effort in those days.

KOLATA

And so when you graduated you went into the Air Force Reserves?

DAVIS

When I graduated, there were special programs (at Harvard) in those days for upgrading the knowledge of soldiers and sailors. After a couple of months of teaching soldiers and sailors at Harvard, the Department of Mathematics suggested that NACA*, Langley Memorial Aeronautical Laboratory, Langley Field, Virginia wanted people, and that was the place to go. So I went down there, this was in '44, in the spring of '44. I was eventually inducted into the Air Force Reserves and then placed in inactive, set back in inactive duty at NACA in Langley Field. And I got into a bunch of people, young people often, who were real hotshots in mathematics. I thought I was (one of them), but there were others too. People like Joe Diaz was there, and other people, and the old timers were quite adept at mathematics so it worked, mathematically speaking or physically speaking, it was a good environment. My civil service title was not mathematician, but it was aerodynamicist, and Bill (Kolata) has found on the Web or some place, the first, this will interest you Gail (Corbett), found the first paper that I've ever written**. Here it is, it's in aerodynamics, and that's to do with the problem that Bill says has come up recently in terms of an airplane failure, but this had to do with an airplane failure due to rudder overload. This paper actually appeared in a printed version in the bound copies of, what were called, the TRs down there, TR (technical report) or something, but this is a preliminary version.

*(NACA: National Advisory Committee on Aeronautics; in 1958, it became NASA. See also, Philip J. Davis, Numerical Life at the Dawn of the Digital Computer Era, http://www.siam.org/siamnews/general/dawn.htm)

**(NACA Report No. 838, John Boshar and Philip Davis, Consideration of Dynamic Loads on the Vertical Tail by the Theory of Flat Yawing Maneuvers, 1946.)

KOLATA

In that particular paper, what interests me is you mention within the paper computation, you're computing loads on the tail, tails of the aircraft. Now what I'm interested in is how did you compute that, there weren't, as far as I know, computers at Langley Field at that time?

DAVIS

Well that's an interesting question. First of all what we were dealing with is today extremely simple, a second order linear differential equation with a right hand side that was graphical. In other words it was a nonconstant, nonzero right hand side, but it was a graphical thing. In those days what did we have? Well, we had the Marchant computers, these were electric computers that did adding, subtracting, multiplying and dividing, we had slide rules, and we had planimeters. A planimeter is a mechanical device which if you put a graph down and you traced a stylus around the graph you can view the area and even the moment. I can tell you a story about those planimeters, and we also had nomograms. Nomograms were a way of arranging data by burning rollers over different scales and so on; you computed functions often in many variables. And I think that, with the exception of the nomograms, I think I brought in every type of computation that was around, you see. Now today what I did here is done in nanoseconds, but it took quite a number of weeks to work out the computation there. I think a lot of us kids, and we were kids at that time, I was what probably 21, 22 at the time, we were doing a fair amount of computing and there were a whole bunch of young women there and they were doing a bunch of computing. They were thought to be better than the men because they were more accurate, this was thought to be, and whether they were I don't know. And in those days "a computer" was not a piece of equipment, "a computer" was a person, and generally speaking a computer was a girl, or a young woman. Then it changed, the War had changed it.

CORBETT

Do you think that you got to understand the problem better by having used all these different computing (devices)?

DAVIS

Yeah, to some extent, but you understand the problems of accuracy, you understand better the problems of round-off, and so on. You realize that the stuff that you're working with is accurate only to (a limited extent). The right hand side of the differential equation comes from Pitot* tubes for air pressures, and so on, things of that sort. You understand that there's limited accuracy in the thing so you worry about the net accuracy. A lot of these coefficients that appear in this paper here, there are a whole bunch of aerodynamic coefficients, and a lot of them come out of just recording devices of one sort or another, and a lot of them come out of doing certain elementary computations, like getting areas under pressure curves, and the standard way in those days was to take a planimeter and go over the curve, you see. The planimeters all came from Germany and these were things that had to be very beautifully built, to be accurate pieces of equipment. You had to put them back into a velvet box, you know, and keep the dust from interfering with the things.

*(The Pitot static tube produces a direct measurement of dynamic pressure allowing calculation of the gas velocity in ducts, pipes, and wind tunnels)

KOLATA

So their accuracy depended on the mechanical efficiency?

DAVIS

Mechanical efficiency? They needed more and more of these things, and Germany was cut off. At NACA they had a beautiful shop, and, of course, manufactured things. They took one of these planimeters apart and merely duplicated it. The cost, I was informed, was five thousand dollars to make one of these things. Five thousand dollars in those days was two years of my salary, which I thought was generous. It was enough to live on and to have a nice apartment and so on … shows what inflation does. Then it occurred to me, of course, that there's no real reason to use a planimeter on the data. I mean you can just do it with approximate integration formulas, but that wasn't done and I realized this at the time, and my little book that came from the laboratory in University of Edinburgh, which I read in high school, indicated (methods) along those lines. But, of course, I couldn't tell my boss that he was using planimeters at five thousand dollars a piece, and that he didn't need these - just do it by approximate integration. That I said in a book* that I wrote later on approximate integration.

*(Davis, P.J. and Rabinowitz, P., Methods of Numerical Integration., New York: Academic Press, 1975; 2nd ed. 1984)

KOLATA

And that was sort of tradition that was soon to be changed.

DAVIS

It was tradition and –

GAIL CORBETT

Well would your answer have been worse?

DAVIS

No, it would have been the same order of accuracy –

CORBETT

As accurate?

DAVIS

Oh yes, absolutely.

CORBETT

But nobody dared to tell this guy that?

DAVIS

Well, it was a tradition and it saved time actually, it was easier to check the pointer, or instrument, and draw it along like this. You have to be careful and so on, and then you would read out on a dial the number. It was much more convenient to do it that way than to do a whole bunch of additions and multiplications and so on.

KOLATA

So, at the end of this period at Langley Field you were prepared to go back to Harvard, you still wanted to get your (Ph.D.) degree in mathematics?

DAVIS

Well it was an option. You know, I could have stayed on, a number of people did stay on, and become a pioneer in aerodynamics and various aspects of it, and materials and flight problems and things of that kind. But I wanted a degree, and I went back after the War was over. I went back in 1946, in the fall of 1946, and then I took courses and so on.

KOLATA

Do you think that your experience at Langley Field influenced what courses you took in any way?

DAVIS

I don't think so. But what did influence me was the spirit of the math department in those days that I think was all around the country. It was that the only subsequent career for a mathematician was an academic one. I had spent two or three years with the Government and I realized that this thinking is wrong, what they're saying is wrong. The Government has many problems that are worthy of solution and difficult. The Government employs mathematicians who are extremely talented, extremely brilliant and so on, so there are other careers that are available than to go back into university.

KOLATA

So you had a flavor of what else is out there?

DAVIS

I had an early flavor of what else was out there.

KOLATA

When you went back to Harvard after the War, did you feel that the notions of computing had any influence by that time?

DAVIS

Yes. Because right adjacent there was, what's his name, was doing these relay computers, he was in the engineering department, now what was his name –

KOLATA

Aiken?

DAVIS

Aiken, Howard Aiken, and this was just across the lawn from where I was working, as a TA (teaching assistant) or pre-doctoral candidate or something of that sort. So this (influence) was coming in, coming in slowly. Some people were affected by this, and also by their war experience. I think one of the principal people in the math department, in the pure math department in those days, was Garrett Birkhoff*. Garrett Birkhoff changed his career from abstract algebra, and so on, and went into applications including computation. Among his early Ph.D. (students) in those days was David Young, and Dave Young's thesis was certainly on computation and linear algebra. He had other Ph.D.s at a later age, later period, and so on, so he was very much influenced by them. There were people also in the engineering department who were very oriented towards a mixture of both theory and computation. One was Stefan Bergman, I had courses with Bergman, and, I say, he was interested in computation. He was also interested in visual models. He made four-dimensional models out of solid materials, I mean this sounds so primitive these days, where you do these things with the beautiful computer graphics that's available to everybody. But there's some fun in thinking about the old days when we had to go across the country in Conestoga wagons and so on [Laughter].

*(See also, Philip J. Davis, From Where I Sat: Garrett Birkhoff (1911-1996) http://www.siam.org/siamnews/obits/birkhoff.htm)

KOLATA

Your course work at Harvard, for your Ph.D., that was in pure mathematics?

DAVIS

It was in pure mathematics but it had some relationship to applications. My thesis was done under the guidance of Ralph Boaz. He was my supervisor. Boaz wasn't on the faculty at Harvard, but he was running the Math Reviews at the time. But he lived in Cambridge, and he used to come to all the colloquia. He was interested in problems of, what I called, entire functions of exponential type; this is in complex variable theory. My thesis was on the topic of infinite interpolation, that is to say, you're interpolating not only in six points and seven points and eight points and so on, but you're interpolating simultaneously an infinity of points and the question is how do you do this and if you do it in a certain way is the answer unique and so on. So I was back into interpolation again, but from a rather elevated point of view.

KOLATA

Very generalized theory?

DAVIS

Yeah, very generalized theory. And now it's also true that when I was at Harvard, both as an undergraduate and as a graduate student, there was another side of me. The other side of me was writing, and I had, as an undergraduate, I had courses in writing and I knew a bunch of kids who subsequently became famous novelists and so on, and there was another man by the name of Philipp Frank, he was a refugee from Prague, and he was a philosopher, a Positivist of Vienna school variety, and I had some courses with him. When I got back to graduate school I had courses with him both in mathematics and mechanics and relativity, but also in the philosophy of mathematics, and so these were strains that were dormant for many years. When I took a job at The National Bureau of Standards, in Washington, one of the reasons that I left there was this (desire to write). I was perfectly happy in Washington, but the Government (had restrictions on writing).

CORBETT

That was the first after being a (PhD) student?

DAVIS

No, it wasn't, I had a couple of jobs in between that I didn't like, they were also governmental jobs. I worked for the underwater sound laboratory in New London for a while, I didn't care for that job particularly, but the one in Washington was a very good job. But looking ahead just a little bit in terms of career, it became pretty clear in those days that the Government was not the right place to write books. There was every pressure against it, and so that's one of the reasons I, after eleven or twelve years, I left Washington and went up to Brown (University) because I'd heard that the university environment was much more friendly towards writing books.

CORBETT

Not math books?

DAVIS

Oh, any kind of books –

CORBETT

Any kind?

DAVIS

Any kind of book, right.

KOLATA

Let's back up a little. You finished your Ph.D., you did some post- doctoral work, I think, and you mentioned Stefan Bergman, well known in applied mathematics.

DAVIS

I worked with him, I never wrote a joint paper with him. I worked with Professor Joe Walsh, and I wrote a couple of joint papers with him.

CORBETT

Ed's* advisor?

*(I. Edward Block, one of the founders of the Society for Industrial and Applied Mathematics, and later its Managing Director)

DAVIS

Yeah, he was Ed's advisor that's right. I worked with him; he was a great authority on interpolation and approximation particularly in the complex plane. And through Boaz and through Walsh I fell in love with the complex variable thing and did a number of papers along that line.

KOLATA

Joe Walsh is the same Walsh of the Walsh functions?

DAVIS

Yes, the same Walsh. And he was one of the early writers on splines. Besides Walsh, there was Al Byrd, and there was another, Neilsen, Ed Nielsen. These guys were working for the aircraft company in Hartford*. So these questions in numerical analysis were, at that time, (being) developed. The whole spline thing was coming in at that time.

*(Pratt & Whitney Aircraft Company, East Hartford, Connecticut)

KOLATA

So, when you finished your work at Harvard what did people feel about going to NBS? Was it still the attitude that you mentioned previously that –?

DAVIS

Oh yes, I lost status. I'm sure that I lost status. You know there was a system at Harvard. For each undergraduate majoring in a subject, there was a tutor. This was like the British system. The tutor was a person that didn't bone you up on the first year of calculus or anything like that, but he worked on advanced topics with you. You saw him once a week, twice every other week, or something like that. I was George Mackey's first student, and Mackey was a pretty abstract guy. I read group theory with him, and so on, as a sophomore. Later on I had other tutors, but I heard later on that Mackey said of me after I took my job at the National Bureau of Standards, he has left us, he has become a traitor.

CORBETT

Mackey said that of you?

DAVIS

Yeah, something like that.

GAIL CORBETT

Okay.

DAVIS

Don’t quote me I mean, because that’s a rephrasing of what he said, or a paraphrasing of what he said and what he thought. Now actually in later years my relations with him were perfectly okay. He relaxed a little bit. [Laughter]

<u>KOLATA</u>

Let’s move on to your career at NBS.

<u>DAVIS</u>

Career at NBS.

<u>KOLATA</u>

The computing environment had changed by then?

<u>DAVIS</u>

The computing environment had changed considerably by then. The first generation of digital computers were there, and there was one called the SEAC*, S-E-A-C, and that was, now we’re talking 1952, which is when I went there, and that was the hotshot computer in its day, and there was a great group of people there. The group at NACA, sorry, the group at The National Bureau of Standards was an inheritance from a thing called the Math Tables Project of the WPA (Work Projects Administration). It was in New York. They computed mathematical tables, and this existed through the War. They computed tables for the exponential function, sines, cosines and Bessel functions and a few of these things and put them in huge volumes. This was simultaneous with what was going on in England with the Admiralty and so on.

*(National Bureau of Standards Electronic Automatic Computer (SEAC) was the first electronic computer with an internally stored program in the United States Government. It was the first of three computers built at NBS. It was designed, built, and operated at NBS by engineers, scientists, and mathematicians.)

KOLATA

They didn’t use any computers other than the electrical-mechanical computers?

DAVIS

Yeah in those days, at the beginning, those were the only things that were available. They were a great group of people, some of them from England, John Todd* and Olga Taussky were there (at NBS), and other people were there. There were a whole bunch of people from Switzerland, (Peter) Henrici was there, and (Walter) Gautschi was there. These were at different times. It was the, in those days, through the1950s, I would say. Maybe even ’48 or ’49, 1950, all the way up to let’s say ’57 or ’58; The National Bureau of Standards became the leading outfit in the exploration and development of methods for scientific computation using the newly developed electronic computers. And then, of course, as the computer spread, and so on, the knowledge and technique spread and the primacy of the group (at NBS) was diffused a lot, and I now couldn’t say it is the leading one in the country or the world, but it still prominent. In those days, one of the nice things about it was that we had very much freedom to develop what we could. I was never given a specific task really, I might have taken one, but I was never given a specific task. What I thought that I would do was to develop a certain methods in partial differential equations the solutions of which I knew how to get, and see whether it worked.

*(See also, Philip J. Davis, Congrats, Jack! Reminiscences and Appreciation, http://www.siam.org/siamnews/07-01/todd.htm)

KOLATA

You mean whether the numerical technique you were using worked?

DAVIS

Whether the numerical technique worked, and was the right thing, was expeditious, was competitive with others. Every problem has twenty different methods at least, and I tried a number of things that were published. One of them was, this came from Bergman, getting into a partial differential equation from fluids by going through a complex theory of functions of two complex variables. I tried this out, and (even though)it was published, it was not so good. It was not competitive; there were better ways of doing things. I tried out things using certain complex, orthogonal functions in 2D, which had been also promoted by Bergman but also by a very prominent group in Rome from a computational lab (directed by) Gaetano Fichera. He was a big name in those days, and this was also published. It turned out not to be so competitive or useful. In those days it was, and it still is I suppose, an important part of the work in numerical analysis to try out different methods and to see whether they work and whether they're competitive, and what their plusses and what their minuses are. And so, you can think, for example, that the only thing that should be recorded is success but you have to point out that the success is based on twenty-eight different failures, you see, you try this out and then you discard it if it is an evil for something else, and that's part of developmental work. But the people, the effort, considerable effort that went in and didn't lead to success is often thought to be a failure, but it isn't because it leads to what is proper, you see, and what is better, and that's true in any development area, could be true in pharmaceuticals, for example, they try out a hundred different things and you try them out and you see what the failures are.

KOLATA

At NBS, you met Phil Rabinowitz?

DAVIS

I met Phil Rabinowtiz there –

KOLATA

And later did some work and wrote a book with him? How did that happen?

DAVIS

Phil was a crackerjack programmer. I had only programmed one thing in my life on that old system which has what was called a four address system, this is ancient history of course, he was crackerjack at that, so we worked together and I would work out the theory, lay out the algorithms, Phil would do the programming. And that worked out very beautifully. Now in a number of these things that turned out not be too hotsy-totsy, we had to do a lot of approximate integration, and so I got interested in doing approximate integration in a more sophisticated way. And that led to the book. We did a fair amount of work on Gaussian integration, which was a very hard thing to do in the pre computer days.

KOLATA

You have a fascinating story in your book, "Mathematical Encounters"*, that led to you and Phil being called "Heroes of the SEAC". That story is worth retelling.

*(Mathematical Encounters of the 2nd Kind, Philip J. Davis, Birkhauser, 1997)

DAVIS

Well, the story is that, it still is true, that when you write something, when you write a piece of code, you write a program or something in code, it has bugs in it almost inevitably, so you run it, We had laid out a program for doing the Gaussian integration, it was tricky, I didn't know whether it works, and then debug it, you may have to debug it a half a dozen times, but I would know if it would work or not. I was following some leads that came from some advanced stuff in orthogonal functions, on the zeroes of orthogonal functions, and I brought that advanced stuff in, it came from a book, on orthogonal polynomials* that Gábor Szegö had written in 1935, 1939, something like that, which I was familiar with. I have no notion under the sun whether or not this scheme was going to work, so I laid it out for Phil and then I did some of the programming, but I said I can't do the double precision stuff, you do the double precision stuff because I figured we had to go to double precision which is twenty figures otherwise we'd get junk. He laid that out, I still have the program sheets back home. I didn't want to throw them out. [Laughter]. And then we put it on the SEAC, and the way it worked was you had to type it out in those days, and put it on wire, and then you shoved the wire in and you push the button and then you waited. Lo and behold it started outputting numbers, which seemed to be good, it was a miracle, absolute miracle, and a miracle from two points of view. A miracle because first of all the programming was correct, which was a miracle. And the second that the algorithm was working, and so this being a rare case, they slapped on "Hero of the SEAC". In those days, that was a takeoff on "Hero of the Soviet Union", because when one heard about heroes of the Soviet Union they were highly regarded people and they got prizes and so on in Moscow, so this was a takeoff on that and for a couple of days we went around with a scarf [Laughter] –

*(Orthogonal Polynomials, Gábor Szegö, Colloquium Publications, vol 23, American Mathematical Society, Rhode Island, 1939)

KOLATA

Some of the –

DAVIS

Anyway, talking about the Gaussian stuff, the Gaussian stuff has been pushed to a fair thee well by Walter Gautschi who's done all kinds of stuff that we couldn't do in those days, not just Gaussian but all kinds of Gaussian type integration formulas with arbitrary weight functions, weight functions with strange weights, or sometimes even graphical weights and so on. The methods that we used have been pretty much replaced by methods that were developed by Gene, out there in Stanford, Gene –

KOLATA

Gene Golub?

DAVIS

Gene Golub, right, and some of his students. And the subject is still alive developmentally, although I've lost interest in it.

KOLATA

At NBS, you worked a little bit on the famous Abramowitz and Stegun results*, which I actually used later on. You wrote the sample chapter that everybody copied?

*(Abramowitz and Stegun: Handbook of Mathematical Functions, first published by the National Bureau of Standards in 1964. It remains a technical best-seller and is among the most widely cited of all math reference compendia.)

DAVIS

I think I did, yeah, my chapter came first. Milton (Abramowitz) wanted to interview me about getting into the thing, and I said sure I'd be glad to get interviewed. What would you like to do, because this was a thing on special functions, I said well I know something about the gamma function, I would like to do the gamma function. Why did I know something about the gamma function? Well as a senior, I think, as a senior undergraduate my tutor was a man by the name of Dave Widder, W-I-D-D-E-R, he was a full professor (at Harvard) whose specialty was the Laplace transform, he was "Mr. Laplace transform" in those days, which was an important thing for the theory of linear differential equations. He later worked with one of his graduate students who was a classmate of mine by the name of I. I. Hirschman, and they wrote a book on the

Weierstrauss transform a few years later. But, anyway, we had to do what was called a reading period assignment, those kids who were hotshots had to do a reading period assignment on their own, and he suggested that I do a reading period assignment on the theory of the gamma function. The theory gets interesting only really in the complex plane, but I had had complex variables already so I was equipped for that. As a result, I knew something about the gamma function, so I suggested to Milton that I would do the gamma function and the various functions that derived from it. He said fine, go ahead, and I worked on that and I laid out a kind of a scheme for representing, for organizing the material. It could be formulas and so on, and they also had to do a few more computations, because in those days it was still important to build tables into the final product. We realized when we were doing this, everybody realized that even as we were doing the book that it would be out of date as soon as it was printed. We knew this, but we cannot jump somehow, you cannot jump to the future. When you jump to the future, it becomes the present. But we can think about the future. Anyway, it took NIST fifty years before they were even thinking of updating that book, it was still used around in its primitive form, and has an enormous number of references to it. The number of references to Abramowitz and Stegun,, that's Irene Stegun, who was one of the advanced computers in those days, the number of references to Abramowitz and Stegun is enormous, absolutely enormous. If you go into citation indexes it's one of the references way up there, you know, more than anything.

<u>CORBETT</u>

Does it still get cited today?

<u>DAVIS</u>

Yeah, (it does) get cited, nothing is higher than Abramowitz and Stegun.

<u>KOLATA</u>

I can tell you when I worked in the 80s, I used that book. Although I didn't use the gamma function, I used the error function –

DAVIS

The error function and so on –

KOLATA

You mentioned previously that there were a lot of women involved, and I noticed that with Abramowitz and Stegun, when you look at the references there are a lot of women, Emilie Haynsworth, Lucy Slater, and Gertrude Blanch.

DAVIS

Gertrude Blanch? Well some of these women were in two categories. There were women that were just computers in the old sense, but there were also women that were well versed in mathematics and had higher degrees in mathematics, Gertrude Blanch, Lucy Slater and Emilie did, and there were some others. And I got to know Emilie, and that was a wonderful friendship. Emilie left The Bureau of Standards to go take an academic job at Auburn University in Alabama. I'm always in touch with Emilie, she was a matrix person, and I learned a lot of matrix theory from Emilie. She would invite me down to Auburn to give a course. I went down there for quarters, and taught some courses. I even gave a talk, on her say so, to the Kiwanis Club of Auburn, Alabama, that was quite a thing for which I was paid a chicken dinner.

KOLATA

In the development of the tables you mentioned computing. So you were using the SEAC in various spots?

DAVIS

Using the SEAC but also it was a mixed bag, also using still the mechanical computers in those days. Yeah, it was a mixture.

KOLATA

I see. So you hadn't quite –

DAVIS

Hadn't quite divorced –

KOLATA

The technology hadn't quite matured?

DAVIS

Not quite, no, no. I would say probably by 1959, or something like that, it had pretty much moved away from those things, but they were being used. I left The Bureau of Standards in '63, after John Todd left and Olga Taussky left, I took their job, my job was given to me, and I was the head of the numerical analysis section there and my job then, I think, was largely to promote numerical analysis in various ways. And I rehired Frank Olver from England, he had visited there briefly, we rehired him, he's retired from NIST, but he's still there at NIST and the University of Maryland over in College Park. Still doing excellent work on special function theory and modes of computation and so on.

KOLATA

But you were ready to move on by 60s?

DAVIS

I was ready to move on because I was itching to write some stuff and actually I wrote a book on interpolation of approximation*, I wrote that at The Bureau of Standards, and I had to assign royalties to the Government, I assigned all my royalties to the Government because I had worked on it, although by the time it came out I was up at Brown. And this annoyed me very much that I had to assign the royalties to the Government. In those days, and this is ancient history, ancient political history, President and General Dwight D. Eisenhower had written a book on his experience, apparently he had written it on Government time, well certainly because he was President, and he was a General that's Government pay, and since there was this law that he wrote on Government pay you had to assign the royalties to the Government, to the Treasury. Oh well. But Congress passed a special law for Dwight D. Eisenhower assigning the royalties to him and his heirs in perpetuity and so on, but it wouldn't pass a special law for Philip J. Davis.

[Laughter]

*Philip J. Davis, Interpolation and Approximation, Blaisdell, New York, 1964.

GAIL CORBETT

Was that the book with Rabinowitz(?)?

DAVIS

No, that was a book I'd written at Brown.

KOLATA

When you made the transition to Brown, you weren't looking to go back into academia. How did that happen? How did that job offer come?

DAVIS

The job offer came through a guy working for the ONR, whose name I have forgotten. He was alert to all the job possibilities, and in those days there were jobs galore, there were jobs everywhere in mathematics and in computation and so on and so on. So, it wasn't a surprise that there was a job opening at Brown. And Brown was in my native New England. I realized that it would be an opportunity to teach and to research and also to write books, so I grabbed on to it. This was in '63.

KOLATA

I have a question that's related to the time that you made that transition. In the introduction to the first volume of the SIAM Journal of Numerical Analysis, Cornelius Lanczos, and I'm paraphrasing him here, said this is an opportune time for a journal on numerical analysis because, in paraphrasing him, there are all these routines out there, all this software out there, and people are storing it and including stuff on computers without analyzing the problems before hand. That was his point of view. Do you agree with that? Do you think that it's gotten to that point already?

DAVIS

Absolutely. Now we're talking about '52, '53, -

KOLATA

The first issue –

DAVIS

When was the first issue?

KOLATA

The first issue of the SIAM Journal on Numerical Analysis was in ’64.

DAVIS

Yeah, but there was an earlier one, the SIAM Journal *--

*(The Journal of the Society for Industrial and Applied Mathematics, first published in September, 1953; became the SIAM Journal on Applied Mathematics in January 1966.)

KOLATA

Oh, yes, yes –

DAVIS

The SIAM Journal also had papers that related to computation –

KOLATA

Yes –

DAVIS

And I had a very tiny finger in that. Ed (Block) was setting that up and he brought me into this to look it over and do some editorial work, I don’t know if my name every got on the front cover, that was irrelevant.

CORBETT

You were a founding editor?

DAVIS

Yeah, that was it. So I worked a little bit on that and Dave Young was working, I remember a meeting, Dave was at the University of Maryland in those days so it was convenient. There were two schools of thought, one was that if you had to do theoretical work before you put stuff on, and there was a lot of theoretical work that had piled up in the ancient days that was ready for testing out, like I explained earlier. But there was a lot of stuff that you could just try out and you didn't need theoretical work, and I will say that engineers and technologists went on with theoretical work, sorry, did experimental work, they just wrote programs and so on, and sometimes it worked and sometimes it didn't, and the problems of round off, and so on were serious, not in round off but buildup of error, and so on, and these things weren't well known in those days so it was very good to have theoretical work that did error analysis, and I think that Lanczos probably would be referring to that kind of theory not to the error analysis, a lot of the work on the computer is iteration. You try it once, you feed it back into itself, and it's stable or it isn't stable, if it isn't table then you get junk, and if it is stable that's fine and you can go, on and so on, have confidence in it and if you have a theory of stability then it's even better. And then there is a little work on this, a famous paper by Courant and Friedrichs, and so on, on the stability in doing -

KOLATA

So it was definitely a time for mathematics and computing to interact with each other?

DAVIS

Absolutely, a strong interaction. And you have stuff with Peter Lax on stability, and some early work with Peter Lax, and Lax and Wendroff and so on in stability, and it was time for all kinds of stuff to enter into the Journal. Lanczos was right.

KOLATA

So when you moved on to Brown did your interests change?

DAVIS

Yeah.

KOLATA

You worked in a slightly different field?

DAVIS

My interest changed, I didn't work so much in numerical methods. But I did some. We put out three editions of the book on Approximate Integration.

KOLATA

This is the book with Phil Rabinowitz?

DAVIS

Yes, the book with Phil Rabinowitz. But what I found was that my interest was changing toward more philosophical questions. Particularly the relationship between mathematics and society, that was my interest, it still is. And I gave a talk, I got prize in, I forget when, '61, '62, something like that, I got a prize, it was called the Chauvenet prize*

*(1963 Chauvenet Prize: Philip J. Davis, "Leonhard Euler's Integral: An Historical Profile of the Gamma Function," Amer. Math. Monthly 66 (1959), 849-869. The Chauvenet Prize is awarded for an out standing expository article.)

KOLATA

That's from the Mathematical Association of America (MAA)?

DAVIS

Yeah, the MAA. And I had to give an acceptance talk when I received that, this was out at Berkeley, at the winter meeting. I gave an acceptance talk in which I pointed out that the relationship between mathematics and society was becoming more and more omnipresent and more and more severe and complicated, and so on, and one had to look at some of the humanistic problems that were implicit in this. And it was a short talk, and one of the persons in the audience I believe was Rueben Hersh. Rueben heard what I said and he said, ah, gee, I've been thinking along these lines maybe I ought to collaborate with this guy. And that worked out very well. So he got in touch with me and I saw him, and his philosophy was sympathetic to mine and we wrote two books together. The first of which was a very influential, very influential indeed, called "The Mathematical Experience*, and not only got into problems of society but also questions of why mathematics is true, why it's applied and so on, why it's useful, and questions, very influential over the years.

*(Philip J. Davis and Rueben Hersh, The Mathematical Experience, Birkhauser, Boston, 1981.)

KOLATA

And did your mathematical work change some? I know that you started to know people in computer- aided and geometric design. How did that come about?

DAVIS

Well it came about through approximation theory, approximation theory was how you represent curves in surfaces by convenient formulas. And I knew a lot about approximation theory, and in the book that I had written I had said something about Bernstein polynomials, and so on. They had a future in this business, and this was picked up, I think, I don't know if the chronology is correct, but it was picked up by Bob Barnhill. Bob Barnhill wanted me to come to Brown and explore this a little bit, he was ready to change his field, which was in topology, I think. I think he got his degree from Wisconsin, and he came to Brown, and I put him on computer-aided geometrical design. I think that's his term by the way, I think he invented that term, and he ran for a touchdown in that field. Over the next twenty-five years he took right back to Utah and he ran for a real touchdown, and he had many students all of whom worked on some aspect of that sort of computation. And I used to send him graduate students, graduate students started off with me and I saw that they were interested in this, I said well the person that knows much more about it is Bob Barnhill and he'll take you on in Utah if you go out there. So we set it up and the person may have gotten a degree from Brown but really worked with Bob, maybe they got a degree from Utah, I'm not certain. So that was a very fine collaboration. I consider him one of my protégés, Bob, not a student in any formal sense but certainly a protégé. He had me out to Utah on a number of occasions. I spent, all in all, perhaps a whole year out in Utah giving courses in numerical analysis. Using that term numerical analysis, I have to tell you going back to the age, the Conestoga wagon age, that there were very few books on this, on computation, very few. One was by Collatz* in German, and one was by Whittaker in Edinburgh. (End of side)

*(Lothar Collatz, Professor at University of Hanover, Germany, author of several books, including the Numerical Treatment of Differential Equations, Springer-Verlag, 1964)

<u>KOLATA</u>

(New side)

So you were mentioning (a book by) Collatz.

<u>DAVIS</u>

Yeah, (another) one I say is Whittaker and Robinson* was a book that was around, there were a couple other books that were around, but not many, and they related to the technology of slide rules and this earlier technology of adding machines, and so on. I think the person that invented the term "numerical analysis" was George Forsythe, I've heard that said. I don't know if it's so, but I'm willing to give the late George Forsythe the credit for inventing that term. It took off and stuck and now numerical analysis is, well you see it everywhere in courses and magazines and careers and so on.

*(Whittaker, E. T. and Robinson, G., The Calculus of Observations: a Treatise on Numerical Mathematics, Blackie & Son, 1924.)

KOLATA

That brings up an interesting question. When did universities start teaching courses in numerical analysis?

DAVIS

Well, I think it probably was around in the late 50s or early 60s, something like that. There was initial resistance of course and some of these courses were in the engineering department and the astronomy department, and so on. In those days the computer departments were splitting off, initially they were either in electrical engineering or they were in mathematics, and then eventually they split off from math and engineering and became something other. Then, of course, they became far, far larger than mathematics and then they overwhelmed it and became elephantine in size and in terms of their coverage, very different.

KOLATA

Is there anybody that you had close interactions with in the field of numerical analysis or computing that we haven't really mentioned yet?

DAVIS

Let me think. We developed a number of people at The Bureau of Standards like John Rice and Marvin Marcus. Marvin Marcus, I used to call him "Mr. Matrix". When he left The Bureau of Standards, he went to the University of California, Santa Barbara, and he developed this huge database on matrix methods. I don't know if it still exists now that Google is around and Get

Smart, and some of these other things. But for a while, it was a big database which was useful. I got him to write a summary of matrix theory that would be useful for computation, and he did this when he was at The Bureau of Standards. It came out as a little pamphlet for The Bureau of Standards. When he got out of government he increased this book, it's called "Marcus and Minc"*, M-I-N-C, I think. Minc was with him in Santa Barbara. It gave a large summary of matrix theory, and I got interested in matrix theory. This comes from my experience with Todd, and with Olga Taussky, and with Minc, and there was another guy down there whose name, one forgets names as one gets older. Maybe it will come back to me, and Emilie, Emilie was very important –

*(Marcus, Marvin and Minc, Henryk. *Survey of Matrix Theory and Matrix Inequalities* Boston, MA: Allyn and Bacon, 1964.)

<u>KOLATA</u>

Emilie?

<u>DAVIS</u>

Emilie Haynsworth was very important. I got interested, and I did a book on the theory of circulant matrices, a book* that came out, I don't know sometime in the 80s, something like that. It was a book that took a special topic in matrix theory and ran that for a touchdown, and that's been quite useful to some people. But I also got into philosophy and other topics, and into fiction, science fiction, and fantasies of one sort or another, Thomas Gray –

*(Philip Davis, Circulant Matrices, Wiley, New York, 1979.)

<u>CORBETT</u>

Fiction reading, not writing?

DAVIS

Yeah, fiction, both, fiction reading always –

CORBETT

Well, Thomas Gray –

DAVIS

Writing and biography, and autobiography, then eventually I met Gail Corbett [Laughter], and that started another career doing reviews for SIAM news, you see, which I considered very important.

KOLATA

Well one of the things that, you've had this interview, but one of the things I've noticed is in your books, in SIAM News, the articles you've written for Gail, there's a lot of your personal interactions with people, you wrote something about Garrett Birkhoff for SIAM News, and you've written other things that have a lot of interesting anecdotes that are in fact applicable to this very project. It's useful to have somebody who has been involved in it and has written, and writes so well.

DAVIS

Well, I like to bring in myself and bring in other people, I think the human portion of the story is very important.

CORBETT

How is that?

DAVIS

I mean in mathematics we go from person to person, really, and we all stand on everybody else's shoulders, and that is true, but there's a human aspect there, these people are personalities in one way or another and I think that my whole ideal for biography was having read at an early age, in college or someplace, Eminent Victorians*, by Lytton Strachey, now that is an outstanding piece of satirical biography, and I have it right in here as a matter of fact, I came down by train to save a few bucks instead of going by plane, and –

I'm going to reread Lytton Strachey's "Eminent Victorians", and it's wonderful from the point of view of style, it's wonderful from the point of view of satire, it's quite non-objective, it's been criticized by later biographers as, well this is his subject and his scandalous view on some of these people including Florence Nightingale and all these eminent Victorians –

*(Lytton Strachey, Eminent Victorians: Cardinal Manning, Florence Nightingale, Dr. Arnold, General Gordon, G.P. Putnam's Sons, New York, 1918)

CORBETT

Scientists?

DAVIS

No, I don't think he did a scientist, he did Cardinal Manning, and he did General what's his name, in Africa that lost the battle against the Mahdi.

CORBETT

Keane?

DAVIS

No, he didn't do Keane, he would have been contemporary with Keane.

KOLATA

Let me interject myself here. Thank you for the interview, you do have a train to catch Phil, it was a wonderful interview and I'd like to thank you.

(End of Interview)