An interview with FRANK STENGER

Conducted by Philip Davis
on
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Department of Applied Mathematics, Brown University

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ABSTRACT:

In this interview conducted by Phil Davis, Frank Stenger discussed his career in numerical methods, including his best known work on sinc methods, and his long tenure at the University of Utah. Stenger, who left his native Hungary as a youth and moved Canada, began by studying engineering physics at the undergraduate and master's level at the University of Alberta before switching to mathematics for his Ph.D., also at Alberta. Stenger then moved to the University of Michigan, where he spent three years before a rift in the mathematics department motivated him to accept a position at the University of Utah, where he has remained for over thirty years. Stenger also spent one year at the National Bureau of Standards, where he worked most closely with Frank Olver, over one year at the University of Montreal Research Center, one year at the University of British Columbia, and a total of over one year at the University of Tsukuba, and at the University of Aizu.

Stenger's work with sinc methods – a term coined in the 1950s by Harry Nyquist and Claude Shannon – began when he substantially revised a paper written by John McNamee and Lee Lorch on Whittaker's Cardinal Function. The work took off, as sinc methods turned out to be an excellent tool for making approximations. They can be used to solve problems in a wide range of areas, including in electrical and fluids problems, and is the primary wave tool use in wavelet applications. Stenger argues that sinc methods have not received as much exposure as they deserve, in part because he is not naturally inclined to promotion or marketing, and in part, because finite element methods were developed somewhat earlier, and thus became popular first.

In addition to numerous articles, Stenger has authored a 565-page text called *Numerical methods based on Sinc and analytic functions, a* jointly authored text on approximation called *Selected topics in approximation and computation* and has produced a software packages for quadrature, for solving ordinary differential equations, and most recently, a package, written in MATLAB, for utilizing sinc methods for solving nearly all problems stemming from applications, including partial differential and integral equations. Interestingly, during his time at Michigan, Stenger shared an office with MATLAB developer Cleve Moler. Also, a former student, John Lund and his colleague, Ken Bowers, wrote a 335-page text, *Sinc methods for quadrature and differential equations.*

Stenger has observed a recent downward trend in student interest in numerical methods, but notes that interest in particular fields is often cyclical. Stenger, who considers himself more of a generalist than a specialist because of his wide-ranging interests, believes that special functions can be extremely valuable, and suggests that their usefulness have been underestimated by some.

PHIL DAVIS: This is an interview with Professor Frank Stenger held on June 24, 2004 in the Applied Math building at Brown University. The interviewer is Phil Davis. The occasion of my meeting Professor Stenger is an international congress, which is being held here at Brown, on the topic of numerical methods for partial differential equations. Professor Stenger gave a lecture on the utility of Sinc functions in numerical work. So, now we can start with some of your reactions. Let me tell you what my principle reaction to this congress was. When I started in the business many, many years ago there were only a handful of people all over the world that were doing research in numerical methods. There were hardly a handful of textbooks, there were no courses in the thing. Now, here we have 200 people come to this meeting, and the meeting is only on one small but important subset of numerical methods. I mean, the growth is phenomenal. Do you have any reactions to this over your career?

FRANK STENGER: My only reaction to it is that initially there were many jobs in this area because of the lack of people. The numerical analysts were the people that solved problems in mathematical applications. They were all computationally oriented at that time – the jobs were there, people flocked to the area. Then, as often happens, the big universities got set up very quickly for this trend, and produced many, many students. And that's the way the growth occurred. I don't know if that's the reason you're looking for, but –

DAVIS: How is it at your university? Do you have many students in numerical methods?

STENGER: It's now a waning area. It's no longer so popular. For a while the areas of computing and visualization were popular, but you can already see the downtrend in these, too. The jobs are also filling up in companies. For example, I have a student in two very well known companies. One of these companies, which is a very good company in the U.S., has adopted a policy of firing five percent of its programmers every three months. They're becoming fussy, so students are no longer as interested in working for them. The growth has also slowed in computer science, in the graphics and visualization areas.

DAVIS: That's very interesting, that's the first time I've heard about this down growth in this field.

STENGER: Numerical analysis happened earlier. I mean, in the late 1990s it more or less stopped – the jobs sort of stopped then.

DAVIS: Is this something that's likely to pick up in the future?

STENGER: It's quite possible that it will. Some of the areas oscillate. I know, for example, that in metallurgy, undergraduate student enrollment at the University of Utah varies from forty to four, and that the typical cycle is four years.

DAVIS: You've had a number of Ph.D. students I take it –

STENGER: Yes, about twenty.

DAVIS: About twenty. And you've been at the University of Utah for thirty years or so?

STENGER: Yes, since 1969.

DAVIS: What sort of jobs have your Ph.D. students typically had over the years?

STENGER: Well, initially they all have university jobs. Then later on, towards the end of the 1980s, they turned to companies. And then that stopped very quickly also.

DAVIS: Are jobs in the university now saturated?

STENGER: Pretty much I think, yes.

DAVIS: You'd mentioned the downtrend in visualization. I've been hearing rather the reverse – I means in terms of technique anyway, not about jobs – all these models, potentialities for visualization.

STENGER: I think the area is still very important and it's very useful and companies still want people. But, again, it's one of those areas where the enrollment has grown so fast that we've saturated the market. The field is important, but I think we've reached a level at which students are hesitant about coming to universities to get a job for a company. What they're doing instead is deciding to stay around and get an advanced degree and then get a job at a university.

DAVIS: So what are they getting an advanced degree in?

STENGER: It's still the same area.

DAVIS: Same area, I see. The University of Utah, as I remember, was a pioneer in visualization; now I'm talking about what twenty-five years ago or so.

STENGER: Indeed, it's done very well in this respect. We've had some really good people in the area.

DAVIS: The University of Utah made a number of striking advances in terms of both software and in hardware, I think, because the companies located in Salt Lake. Let's talk briefly on a more personal level, a little bit of background. Where did you do your undergraduate degree, Frank?

STENGER: I did it at the University of Alberta. I have a very complicated background coming from Hungary, being shipped out of Hungary into East Germany, sneaking across the border to the West after one year, and then coming to Canada. Then I more or less acquired a *Canadian mind*, which most Canadians recognize as differing from a *US mind*.

DAVIS: What is the distinction between the Canadian mind and the U.S. mind?

STENGER: I think it's probably more tolerant.

DAVIS: Well, I think of the Canadians as being a little more relaxed than Americans, do you agree with that?

STENGER: Yes. Also little bit more caring for one another, I think. Anyway, I did an undergraduate degree in engineering at the University Alberta. I came over (partly) into mathematics later on after finishing that degree undergraduate degree (I simultaneously did a masters in both electrical engineering and mathematics). A former mentor advised me to continue my studies, and he said, "If you don't want to do it in engineering, come over and see me."

DAVIS: Who was this?

STENGER: His name was John McNamee. He discovered me in my second year and after that he sort of kept an eye on me. By my fourth year, although I had some really good job offers from companies, he arranged for me to spend a whole summer at the University of Oklahoma, which more-or-less eliminated engineering for future study.

DAVIS: This is in Norman?

STENGER: Yes.

DAVIS: Who was there at the time that was interested in numerical stuff?

STENGER: Oh, they – Richard Andre-- had a huge grant and a variety of visitors.

DAVIS: Of course, I knew McNamee. What was his background? I was never clear about his background.

STENGER: His also was an electrical engineer. As far as I know he only became interested in analytical functions after he got together with you. I still don't know why you got interested in it.

DAVIS: Well, I could tell you that, perhaps a little bit later. So, your undergraduate degree was at the University of Alberta, and then where did you do your –

STENGER: I did engineering physics there. I should probably add that because it was a small group of people that were allowed to take the same physics as honors physics, and the same math as honors math. And so I learned my math – in fact I learned undergraduate complex variables – from Lee Lorch.

DAVIS: Oh yes, I knew Lee Lorch.

STENGER: And later on it was Eoin Whitney who taught me advanced complex variables.

DAVIS: You got your Ph.D. at Alberta?

STENGER: Also at Alberta, but in a different department. I first got a Masters, officially my advisor was Whitney. My mentor, McNamee farmed me out to him. And then I also did a Ph.D. officially with Whitney, although I got my topic from Frank Olver at the National Bureau of Standards.

DAVIS: We have known one another for a long time. Can you remind me when we first met? Roughly?

STENGER: I know I tried to meet you in 1963. McNamee had arranged for me to study with you at the Bureau of Standards, but I never met you I think until a couple of years after that.

DAVIS: Well, that was the time that I came to Brown, so I was just leaving the Bureau of Standards. But you worked at the Bureau of Standards.

STENGER: Yes, I spent one year there.

DAVIS: Who were some of the people that you intersected at the Bureau of Standards?

STENGER: Of course, there was Morris Newman, Frank Olver, Seymour Haber, Karl Goldberg, Bill Pell.

DAVIS: Bill Pell who went on later to head up mathematics at the National Science Foundation. Morris Newman was great on algebras and matrix theory and so on. Seymour Haber was doing quadratures, Oxford quadratures, and Frank Olver was doing, as I remember, Bessel functions and other special functions and things of this sort –

STENGER: Asymptotics –

DAVIS: Asymptotics, yeah, particularly asymptotics. All of these people are involved one way or another in numerical methods, experimentations and so on. Which of these people did you work with mostly?

STENGER: Well, mostly I worked with Olver. It was again, at the suggestion of McNamee after he found out that you had left the NBS. I worked with Frank, and we wrote one joint paper while I was there.

DAVIS: Have you continued correspondence with Frank over the years?

STENGER: Less frequently, but we still communicate with one another occasionally.

DAVIS: So applied mathematics dominated back in the beginning because of your engineering training?

STENGER: Yes.

DAVIS: In your doing numerical work, do you think of yourself as a generalist or as a specialist in some particular topic?

STENGER: I think I'm more of a generalist. I do like diversity and I've always loved to learn about other areas. I love beautiful mathematics and I also love being able to apply it.

DAVIS: As a generalist, what kind of a range of topics have you covered?

STENGER: Perhaps I should continue with what happened after I got my Ph.D. at the University of Alberta. I spent one year in a computer science department there, and then I decided I needed to learn more mathematics. I applied to several places and I was accepted at the University of Michigan, which was excellent for me. Although I didn't write many papers while I was there, I attended numerous talks and seminars each week in different areas and so on.

DAVIS: The mathematics department of the University of Michigan in those days was fairly abstract, wasn't it?

STENGER: It was what modern pure mathematicians would call a classical pure math department, where they emphasized analysis and areas like complex variables.

DAVIS: Complex variables, Approximation theory, Functional analysis and Banach spaces, that sort of thing?

STENGER: And applied mathematics, and probability, and modern algebra and so on, right.

DAVIS: Now when did you get to the University of Utah.

STENGER: In my third year there, the department had a split, and I am one of those people who doesn't like to join sides. You know, if you were an assistant professor and if you were on one side you had just as a good chance to get hurt by the other as vice versa. But if you're not on any side, then there's a good chance that you'd get hurt by both.

DAVIS: The sides being on the one hand pure mathematics and applied mathematics or computer science on the other side?

STENGER: Yeah, there was sort of a split and every department has its own.

DAVIS: There's a split here at Brown.

STENGER: Sometimes, it kind of catches fire and becomes worse than others.

DAVIS: I remember that in those days there were a number of pure mathematicians for whom the idea of computation was poison.

STENGER: Oh yeah, sure. Before I came to Michigan, Bob Bartels was more or less forced out and then put into computer science because numerical analysis was not considered to be good mathematics then. I think something similar probably happened to [George] Forsythe at Stanford, that's why he formed a separate department.

DAVIS: It happened also at Princeton. It happened in many places, but I think the attitude has changed, but slowly –

STENGER: I accepted the first job I was offered -- Utah. Michigan made me an offer to stay, but for some reason …you know, once it gets in your blood to leave, you take off. So I left. Looking back, though, I think it would have been good for me to stay because I liked so many people over there.

DAVIS: This was at Michigan?

STENGER: Especially during my last year when I discovered the different areas where were stuck on the same problem.

DAVIS: But years later you don't have too many regrets about it considering what happened in Utah?

STENGER: Well, perhaps not. You know you always wonder what might have happened if – [laughter]

DAVIS: Oh, you know, "what if?" – the big unanswerable question of life. You have devoted a good fraction of your career to the exploration and popularization of sinc methods. Can you say something about how you got into that area?

STENGER: Oh sure. This was many years ago, when I was an undergrad probably. Of course, we should here begin with when McNamee visited Philip Davis. I'm not quite sure how he did this: perhaps he applied for a visiting position – that's what had happened with the National Bureau of Standards. He came back very excited about error bounds after working with analytic functions. So I fit right into that because I had taken courses first with Lorch and then with Whitney on analytic functions and I loved the area. After McNamee wrote a paper on errors on Gauss-Legendre and trapezoidal quadratures, he and Whitney collaborated on a paper called "Whittaker's Cardinal Function in

Retrospect." They submitted it to SIAM, and actually it might have been quite a relevant paper, looking back now after having become associate editor of about six journals. But it was turned down unfairly; they got a bad referee, and being very proud men, put it aside. Later on, when I was at Michigan, I looked over this paper. And after reading it over several times, I got excited about the beauty of the sinc function expansion, and I asked them, "Suppose I modify your paper, including the (only) two good comments that the referee made, and suppose I also add some other things like applications of the series to different equations and so on, would they allow me to become a co-author?" Their response was, "Sure, go ahead." So I did that and sent it off, this time to *Mathematics of Computation*, and it was accepted.[1] By learning about this paper, this beautiful function, I more or less became hooked on the subject, although I did put it aside then. But later on, I had what you might call a fortuitous intersection with the same subject. When I read a paper, which was just completed by Ron Douglas, who was at the University of Michigan, and Walter Rudin, who was at the University of Wisconsin, on the existence of approximations of arbitrary L-infinity functions on the unit circle, by a finite number of rations of functions called inner functions, which are unimodular analytic functions on the unit disc, I found a constructive proof of their result, via use of elliptic functions. I found that with just two rations of inner functions I could approximate any L-infinity function on the unit circle to arbitrary accuracy. But this novel elliptic function tool which enabled me to do this later on led me to the development of a refinement of this particular function that I used – an approximate characteristic function. I noticed that when using this characteristic function I could derive the whole gamut of what would later become Sinc quadrature schemes.

DAVIS: This characteristic function, if I remember correctly, is the Gaussian E to the minus X squared (E^{-X})?

STENGER: No, no, it's not.

DAVIS: What is it?

STENGER: For the interval [-1,1], it is an elliptic function, which is just slightly above the exact characteristic function of [-1,1] on all of R. It is a good approximation on all of the real line in both L-one and L-infinity, where the function is just above one between minus one and just above zero on the exterior of [-1,1].

DAVIS: What do you call the function?

STENGER: An approximate characteristic function of [-1, 1].

DAVIS: The characteristic function. Now what, for the sake of –

[1] J. McNamee, F. Stenger, and E.L. Whitney, "Whittaker's cardinal function in retrospect," *Mathematics of Computation*, vol. 25, pp.141-154, 1971.

STENGER: And it had poles. If you multiply this by a function analytic and bounded on the upper half plane, integrate the product over the real line, and evaluate the integral via use of residues at the poles, you get a very highly accurate quadrature series –

DAVIS: This is an elliptic modular function?

STENGER: No. It was constructed by means of elliptic functions, yes. It's an elliptic function and then, by use of conformal maps one can derive the trapezoidal rule with it. And not only trapezoidal rule: you could derive quadrature formulas over any arc, and these are all exponentially accurate. In fact, it's been shown by two people, Bourchard and Hollig, that the rate of convergence is close to optimal.

DAVIS: Again, for the sake of the tape, what is the Whittaker cardinal series?

STENGER: Well, the Whittaker cardinal function is called a sinc series. This is s-i-n-c.

DAVIS: S-i-n-c, not s-y-n-c. I made a mistake earlier.

STENGER: That's all right – it probably doesn't matter. It should probably be called s-y-n-c. But the term goes back to Harry Nyquist and [Claude] Shannon who wrote a paper in the 1950s. The function is originally due to Whittaker, perhaps it even dates back to Poisson. The research was revived again in the mid-1950s. I mean, this Whittaker –

DAVIS: Well, Whittaker's from the 1910s, 1920s, or something like that…1909.

STENGER: And then Whittaker's son wrote a booklet in the 1920s. But nothing was done after that until about 1950 when Harry Nyquist and Shannon illustrated the importance of this series of expansion with a real-line equispaced interpolation -- an infinite series sum of f(KH) times sinc(X/h minus K) (sinc(x) = sin(pi x)/(pi x)) .

DAVIS: And that's from minus infinity to plus infinity –

STENGER: In signal processing and communication theory – a very, very important area. In fact, it was not known then that…Shannon's paper, incidentally…he was not allowed to publish his result because this was –

DAVIS: During the war –

STENGER: During the war. And it so happened that there was a man named [V.A.] Kotelnikov, who actually did the work back in 1937 in Russia, who also was not allowed to publish his results. But Shannon's work was released before Kotelnikov's –

DAVIS: I don't know that name, the Russian name: K-O-T-E-L-N-I-K-O-V, Kotelnikov. Yeah, I've not heard that story. But the sinc function here, what it that?

STENGER: A sinc X is sine pi X over pi X.

DAVIS: Sine pi X over pi X.

STENGER: This was an engineering term. In fact, the sinc term came out of the papers of Harry Nyquist and Shannon. I don't know, I think it was Shannon who defined this term.[2]

DAVIS: Do you have any recollection of how Whittaker himself got into the cardinal expansion?

STENGER: No, I don't, but there he may have bee aware of Poisson's work. It's quite possible that he did that there –

DAVIS: So as I see it, you have run this idea for a touchdown. Can you say a little bit about that?

STENGER: For me, it's been a very, very lucky area in which to work. It's a very powerful way of doing approximations. It turns out that in the series, when X is replaced by the transformation of the interval to the real line then, you can get an approximation on the interval. On that interval you can treat singularities and still get exponential convergence, which polynomials won't do. But approximation power of these mapped Sinc series is roughly equivalent to the power of rational approximation, so they're better than polynomials. (Indeed, all polynomials are equivalent to polynomials in cosines, which are a subset of all trigonometric polymials, while trigonometric polynomials are equivalent to a subset of certain sinc expansions of periodic functions, and such since expansions of periodic functions are a subset of sinc expansions of all functions.) Sinc expansions have many beautiful properties: an orthogonal expansion; the bases are both discrete and continuous orthogonal; if you take the Fourier transform, you get the DFT [Discrete Fourier Transform] formula, so it's related to the FFT [Fast Fourier Transform] stuff; you can get this whole series, you get a gamut of approximations for every area – for every operational calculus, you get a very simple analysis –

DAVIS: What sort of matrices does this give rise to?

STENGER: Matrices are…for instance, when you proceed for using finite difference methods to solve a PDE or an ODE, the matrices are well conditioned, and on the whole real line, just as they are –

DAVIS: Positive definite?

STENGER: Definitely, they have almost the same properties as finite difference matrices. In fact, they are slightly better conditioned than finite difference matrices, for instance, for finite differences, the second derivative -- the minus-1 (-1), 2, 1, matrix: the eigenvalues are similar in value to those of the corresponding second derivative sinc matric, although the sinc matrix is full, not sparse as for finite differences. However, the

[2] Sinc is short for *sinus cardinalis*.

condition of a 2 N plus 1 by 2 N plus matrix is N-squared (N^2). But for the other intervals, the others they're that same –

DAVIS: That's not too bad, N-squared (N^2).

STENGER: No, that's the same as for finite difference. But for any other interval, however, that same matrix – where the real line sinc matrix is multiplied by a diagonal -- and if you factor out that diagonal and you get the same low condition number– if you include the diagonal, then you're going to get a very bad condition number. If you do the work properly, as a good numerical analyst should, things work out pretty good.

DAVIS: You have written a book on this, haven't you?

STENGER: I've written one book. In fact, I've also been a joint author with two Polish mathematicians: I wrote two chapters in a book called *Selected Topics in Approximations*.[3]

DAVIS: What was the name of the Polish joint authors?

STENGER: They were [Marek] Kowalski and [Krzysztof] Sikorski. It is a book on approximations.

DAVIS: Now you've produced disks and software, and so on.

STENGER: Yes. Now we have a new volume that is currently on a CD – about 370 pages. And then there are about 300 relatively short programs, which are written in MATLAB, for solving almost any kind of problem: every kind of PDE (partial differential equation) problem and so on, for all curvilinear regions, and in fact we also get separation of variables when solving PDEs.

DAVIS: Speaking of MATLAB, I seem to recall that many years ago you shared a room with Cleve Moler, who was the inventor of MATLAB.

STENGER: That's right. Moler and I shared an office in Ann Arbor, Michigan. It was a little bit crowded in those days, building-wise. As many big universities did in the past, they spent most of their budget on people rather than on buildings.

DAVIS: That's something that we would like to hear more about, as a matter of fact, today. Did you have any inkling that Moler was going to go on to make this great commercial success? Was he talking about that kind of thing?

STENGER: Moler is a very gregarious, hardworking person. He came through computer science. In those days, I think, there was a certain amount of feeling against

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[3] The book is: *Selected topics in approximation and computation*, 1995, Oxford University Press, Oxford.

mathematics. But he was also very gregarious and very open; he was a pretty energetic guy.

DAVIS: But you said that there was a feeling against mathematics, or a particular kind of mathematics?

STENGER: Well, I think coming from Stanford, some of his professors might not have gotten as good a treatment from other mathematicians as –

DAVIS: Moler was a student of John Todd, was he not?

STENGER: No, Forsythe.

DAVIS: He was a student of Forsythe. I thought he came from John Todd. I remember there was a Todd celebration about five years ago, when Todd turned ninety, and Moler was –

STENGER: I'm sorry I didn't make it to that. I'd have liked to come. I learned much of my numerical analysis from that book that Todd edited, in which you had a chapter, and Morris Newman had a chapter in, and [Walter] Gautschi had a chapter in, and so on.[4]

DAVIS: Moler…there was a joint publication with Forsythe, Moler and somebody else. Was it Richtmeyer?

STENGER: Just with Forsythe.

DAVIS: I see. What was the topic of that, do you remember?

STENGER: Forsythe and Moler had a nice little linear algebra text. But Henrici, Fox and Moler later wrote a paper on solving Laplace over an L-shaped region. And the fact that it was L-shaped meant that it had that reentrant corner, which even though the boundary conditions are a smooth function of R-squared restricted to the boundary, nevertheless the gradient of the solution blows up as you approach that reentrant corner, which causes problems with approximations. They resolved the problem by introducing a different basis for the corner. In fact, it was a Bessel function, with the right kind of singularity.

DAVIS: You know the famous book put out by the National Bureau of Standards on special functions.

STENGER: Oh yes, I was there when it was put out.

DAVIS: You were there when –

STENGER: I was there when it was –

4 J. Todd, ed., *Survey of Numerical Analysis*, McGraw-Hill, New York, 1962.

DAVIS: Did you have a finger in it?

STENGER: Not really. It was already finished when I got there. But I did nevertheless order, I think, sixteen copies to ship to friends.

DAVIS: Birthday presents, or something.

STENGER: It was so inexpensive, it cost less than one cent a page in those days.

DAVIS: Yeah, I know because the initial edition was put out by the Government Printing Office and it sold for a song. Well, in the interim, of course, the National Bureau of Standards [NBS] has changed into NIST [National Institute of Standards and Technology], and moved out of downtown Washington, D.C. and they are now working on an update of that book. You are familiar with the update in any way?

STENGER: Yes, I've heard about it. I'm not having anything to do with it. I guess the emphases are slightly different, although Frank [Olver] was very interested in my package –

DAVIS: Frank has contributed to this update –

STENGER: Yeah, Frank Olver, right?

DAVIS: Frank Olver.

STENGER: Yeah, he wanted to, but I more or less told them I think what I'd like to do instead of…initially it was talked about that he would consider adding a CD of my package to this book, but I felt, I told him I hadn't decided yet what to do. Maybe I wanted to try to sell it and make some money.

DAVIS: I see. So, that's still open, as a matter of fact, that possibility is still open. How important in your experience are special functions?

STENGER: Well, for me they have done a great deal because I studied them intensely, and they've given me considerable insight into numerical analysis and approximations and things like that. I have benefited greatly from knowing that. My NBS Handbook is pretty earmarked.

DAVIS: I suppose what will come out is a super-handbook, of course, with more special functions and better programs and online potentialities, and so on. Well, this is inevitable – it represents the progress that technology has been made in fifty years. There's another claim that has been made, particularly by people who work in difficult nonlinear partial differential equations, that special functions are rather less important there because in any case they're going to solve the differential equations numerically. Do you have any feelings about the role of special functions in such areas?

STENGER: For people working in nonlinear, I can see their bias, but even there I would like to think that special functions should often give them insights into what is happening. For example, the Korteweg-deVries equation: the two oddly behaving solutions of that were first discovered numerically, and then that led to the discovery of what was actually happening. Again, these things can also be explained through elliptic functions and so on. The behavior is already there but…I mean, it's easy to be critical. As I'm getting older I'm more and more not wanting to be critical of any approach.

DAVIS: So in general you are quite in favor of special functions and their study in perturbation and so on?

STENGER: Yes. I can see, though, that as a course they would not, perhaps, be so popular anymore.

DAVIS: Do any universities give courses in special functions?

STENGER: Not that I know of. There was a time when it was given. For instance, at the University of Wisconsin…Askey, Richard Askey, was interested in those.

DAVIS: Yes, well Richard Askey is one of the contributors of this update and that's been his lifetime field. What do you see as some of the unsolved problems in numerical work that are going to be around?

STENGER: Well, right now, I think from the point of view of the finite element areas, they're bogging down because of the size of the matrices, but I think these probably will be resolved as the computing power continues to grow as it has.

DAVIS: What size matrices do they configure with now?

STENGER: Well, on the order of a million by a million.

DAVIS: A million by a million.

STENGER: Yeah, for instance in these methods that I have here, I can beat the finite element methods by orders of magnitude, in part because…first of all, Sinc methods have more rapid convergence, i.e., they converge very quickly, when you solve a PDE in three or four places. But classical finite difference and finite element methods cannot do that because we don't have enough storage in the computer to store the large matrix they require. The methods that I have I only use one-dimensional matrices and so I can pretty much get arbitrary accuracy. You notice I'm giving you a sales job here for sinc methods.

DAVIS: Are sinc methods receiving sufficient publicity?

STENGER: No, I haven't done as much as I should here. You mentioned Moler before. One of his assets is that he's a very, very good salesman. Whereas I'm not as good a salesman – I need to spend more time selling the stuff. Years ago, when Cleve Moler first sold MATLAB, he used to spend every summer at Stanford, where he would give visiting numerical analysts free copies of his program package. And moving his company near MIT probably enabled him to further increase his sales, i.e., if the MIT scientists use it, then so must the scientists in the rest of the country. So that was a very excellent ploy.

DAVIS: Well, I think MATLAB is a very good package; I like it very much. Plus, his main office is not too far from here up in Massachusetts, about twenty miles west of Boston. You know he puts out these, what do you call it, tool kits, is there any possibility of getting sinc into a tool kit?

STENGER: In fact he once suggested that tool kit, yes.

DAVIS: But this MATLAB is a commercial operation, so if you were to put sinc into a tool kit then presumably you would get some sort of a payment for this, or royalty or something of that sort. Is that the way it works?

STENGER: Yeah, but I haven't really looked into that, that's something I have to do.

DAVIS: I should think that you would want to do this. Because as you say, he's a great salesman and the stuff that he puts out gets wide publicity. You know he has a newsletter, he does his own newsletter, and I get it, I get a copy of it practically twice a week. He's not only that but he's a whole institution, publishing house and –

STENGER: Incredible energy –

DAVIS: And so on, yeah. Where does the energy come from? I don't know. But let me ask you this question, which is a little bit philosophical: you work along and suddenly you get an idea that you ought to do this or that and see if it works. Where do these ideas that you get come from? Have you ever thought of that question?

STENGER: Yes. Well this is very difficult to explain, but I have my own notions on this. I think many of the directions in my research work have been governed by problems that were in the background. For instance, this Ann Arbor problem, I first solved this problem by another procedure, and then only later on it happened to fall apart using sinc convolution functions. I get many of my ideas in bathrooms, in bathtubs, and while tossing and turning restlessly in bed.

DAVIS: What was the Ann Arbor problem?

STENGER: This was the Wiener-Hopf problem, the so-called method of Wiener-Hopf for solving an integral equation on zero-infinity. It's a convolution-type equation of the second kind. For applied mathematicians, the way Wiener- and Hopf did it, there are only special instances when you could get an explicit factorization of (one minus) the Fourier transform of the convolution kernel into a function that's analytically in the upper half plane times a function that is analytic in the lower, but most of the time you can't do this. It's also very difficult to do this numerically. But mathematically the method is very, very beautiful, and touches on some very beautiful theorems like the Wiener --

DAVIS: Fragmented window of –

STENGER: Let's see. For instance the –

DAVIS: This factorization was not due to Wiener. This was known before by the complex analysts.

STENGER: Yeah, yes. But the Wiener-Lévy theorem is very beautiful. for instance, for L-one functions, and the Riemann-Lebesque lemma applies. Of course, the way I'm doing it now, with Sinc convolution, I'm eliminating all of the beautiful math – just being able to write down an approximation that's arbitrarily accurate and explicit.

DAVIS: Over the years you have had, as you say, many students, many graduate students, so you've done a lot of teaching and advising, and also research work. How about the trade-offs between teaching and research, how do you see them? Because teaching does interfere with research –

STENGER: Yes. And at a university there's never enough time to do the kind of good job you'd like to do in either one of those areas, so there's always a loss involved. For me, I like to do a good job opf both. When I first came to Michigan there was a gentleman, his name was Chuck Dolph, who was my mentor. He said, "Well, Stenger, you're a Canuck", he says, "right?" [laughter] And here (at Michigan), all of the professors were either "A" or "B" teachers, very good teachers. He said, "You're a Canuck, so that means you're a 'C' teacher, so you're going to have to take a course in how to teach." And so I took a course in how to teach, and I became an "A" teacher, at Michigan anyway.

DAVIS: Was the course useful?

STENGER: Yes.

DAVIS: It was useful.

STENGER: Yes, it was very good for me. He also said, "If you were from England you'd be a 'D' teacher."

DAVIS: A "D" teacher – and if you're a Canuck from Canada you're a "C" teacher. Well, in my experience there is rather little correlation between the quality of research work and the quality of teaching. I mean, I've had some famous teachers who were terrible as teachers and had international reputation in terms of research.

STENGER: I'll tell you story – off-the-record – later on that.

DAVIS: You don't want to put it on the tape?

STENGER: Alright, let's put it on the tape.

DAVIS: Let's put it on the tape.

STENGER: Alright. You know John was the kindest man – John McNamee. When he came from Ireland he had a very heavy accent. As a teacher, you couldn't understand him -- I couldn't understand him, and when he wrote on the blackboard I couldn't read his handwriting. But I managed to get my Ph.D. anyway, and when I later went to Chicago, to a SIAM meeting, in 1965, and got on this bus for a tour ride. Clenshaw, who was also there stepped into the bus first – I came next, between Clenshaw and Fox –

DAVIS: Charlie Clenshaw and Leslie Fox.

STENGER: And so I went by Clenshaw, to the back, thinking Clenshaw would prefer to have Fox sit by him, instead of me. So I sat farther back. And when Fox came in, he went by the empty seat where Clenshaw sat, and instead, sat by me. I said, "I thought you might want to sit with Clenshaw." He said, "Oh that man's pinned my ears back too often." [laughter] So then he looked at my tag and said, "Oh you're from Alberta. Isn't John McNamee in Alberta?" "Oh yeah, he's the main reason that I'm here," I told him.

[End of Tape 1, Side A – Start of Tape 1, Side B]

STENGER: Fox then added: By the way, is he still so hard of hearing?"

By the way, my advisor, Ian Whitney, also didn't spend any effort on teaching, although I understood what he did, and I loved his mathematics.

DAVIS: Clenshaw, as I recall, was a great advocate of Chebyshev methods for approximation. Do you know what the current status of that approach is?

STENGER: Well, I think the Chebyshev polynomials have caught on in the area of spectral methods for differential equations. This was in part…it's quite well known that it's very difficult to get a best approximation of a function that involves a matrix. It's perhaps not so difficult nowadays, but it still involves a lot of work and –

DAVIS: You mean Redman's algorithms, or things of that sort?

STENGER: Yeah, yeah, right. Whereas if you write down a polynomial for interpolation based on the Chebyshev polynomial you get one with an error that is not much bigger than the error of best approximation by a polynomial of the same degree for the function, i.e., by a factor of the log of the degree. So that's what people use (instead of the best).

And the Chebyshev polynomial has other great properties for quadratures and for explicit evaluation times and so on.

DAVIS: Does your work intersect wavelet theory in any way?

STENGER: Oh yes. Actually, the sinc series is the original wavelet and (I have read that) ninety-seven percent of all the wavelength applications use sinc function wavelets. I have never investigated this aspect – I haven't had time.

DAVIS: But since the wavelet thing is so hot these days, I would think that you might have gotten into it.

STENGER: Yeah, I'm tempted to do that, especially since those maps that I'm using – i.e., the conformal maps that give accuracy on arbitrary intervals and satisfy all the same laws as the wavelength methods and so on. So they're directly applicable to that whole host of algorithms based on wavelength methods.

DAVIS: Changing the subject back to education again. You refer to the fact that the demand for numerical courses is diminishing.

STENGER: Yes.

DAVIS: But if some student, young student, undergraduate came to you and said, "I'm vitally interested in these things," what sort of a curriculum would you suggest that such a student pursue?

STENGER: Well, there are still a lot of problems – I mean, you asked me about problems earlier – there are a number of difficult problems that others are interested in. For instance, recently, in writing this sinc package I was actually supported by the U.S. Air Force in solving one of these stress-strain problems using sinc methods. So what I would have a student do…there's quite a lot of interest in these problems involving fluids and solving electric problems, and for solving tomography -- problems along those lines, for industry. I would have them solve a problem by these methods that there was interest in from companies. I still believe that one is able to discover beautiful new mathematics and algorithms in the process of attempting to solve difficult problems.

DAVIS: You're thinking at the higher level, maybe a master's degree or a Ph.D.?

STENGER: Yeah.

DAVIS: But what about at the undergraduate level? What should an undergraduate come into this business knowing?

STENGER: Yeah, I would love to design a sinc course for undergraduates. By the way Moler has a book out, you know, in using MATLAB teaching numerical analysis, although he has his own interests. I think he probably picked the most popular areas of numerical work. But I'm more interested in the classic part because I believe that if a student understands the classical numerical aspects, he can also deal with the more modern things. I can learn from Moler's approach, however.

DAVIS: Well, let's bring this interview to an end. We've been at it for about an hour, which is just right from my point of view, and I'd like to thank you.

STENGER: I appreciate your asking me.

DAVIS: It's been a big pleasure for me, and I hope you've enjoyed it.

End of Interview