
Carl de Boor: On Wings of Splines >>>

Interview of Carl de Boor by Y.K. Leong

Carl de Boor made fundamental contributions to the theory of splines and numerous applications of splines that range from highly efficient and reliable numerical algorithms to complete software packages. Some of these applications are in computer-aided design and manufacturing (of cars and airplanes, in particular), production of typesets in printing, automated cartography, computer graphics (movie animation, for example) and signal and image processing.

He has given numerous invited talks at scientific meetings throughout the world. He has served as editor of leading mathematics journals. He has received numerous honors and awards, among them the Humboldt Research Prize and John von Neumann Prize. He is a member of the U.S. National Academy of Sciences, the National Academy of Engineering, the Academia Leopoldina (Deutsche Akademie der Naturforscher) and the Polish Academy of Sciences, and a fellow of the American Academy of Arts and Sciences. He was Professor of Mathematics in the Departments of Mathematics and Computer Science at University of Wisconsin-Madison from 1972 until his planned retirement in 2003. He remains there as Emeritus Professor and continues to be active in research as a member of the Wavelet IDR Center.

Continued on page 17

Continued from page 16



Carl de Boer

He was interviewed by the Editor of *Imprints* on 16 August 2004 when he was at the Institute to give a plenary talk at the international conference on wavelet theory and its applications. The following is a vetted and enhanced version of the transcript of excerpts of the interview which gives a frank account of the serendipitous path from Hamburg to Harvard, followed by first contact with splines (in a research laboratory in Michigan) that soon took off into an exciting world of path-breaking discoveries and immediate applications.

Imprints: Your early university education was at Hamburg and your PhD was from Michigan. Could you tell us something about the way you went from Hamburg to Michigan?

Carl de Boer: It's definitely a story of accidents. I moved to Hamburg in 1955, and I met there (as the result of a further sequence of accidents) an American girl to whom I got engaged in '58. Her father was a professor of political science at Harvard, and he arranged for me to come over to Harvard for a year, in '59. My future father-in-law had known the Birkhoffs, both the older and the younger, well and had proposed that Garrett Birkhoff might give me a research assistantship and had mentioned that I had worked as an "assistant" to Collatz who was at that time a main figure in numerical mathematics, at least in Germany. Now, I had indeed worked as a teaching assistant for Collatz and even done some calculations for him. But Birkhoff misunderstood "assistant" and thought that I had been an "Assistant". In the German system, an "Assistant" is someone close to a PhD. So, Birkhoff was very happy to give me this job. But it became clear very quickly that I was not at all qualified for it. Birkhoff was a very kind person. He did not kick me out, but he gave me a good problem, and I have never worked so hard in my life, trying to produce at least something.

During that year, I decided that American university life was freer than German university life at that time. So I decided to stay, and I got married. However, I couldn't support my wife on this research assistantship. Birkhoff was then consultant to General Motors Research in Warren, Michigan. He persuaded them to give me a job. So I worked at General Motors Research and even ended up writing some papers there. But my colleagues with PhDs were much freer in the choice of problems and were much better paid, of course. So I decided I should get a PhD too, and the nearest good university was the University of Michigan at Ann Arbor.

I: Was Garrett Birkhoff at any time interested in numerical mathematics?

dB: Birkhoff was, in fact, interested in many aspects of mathematics. He was trained as an algebraist. He did various things in algebra, universal algebra, and he wrote a book on lattice theory – he practically invented lattice theory. He was also interested in applied mathematics and numerical mathematics. He wrote, for example, a book on the numerical solution of elliptic PDEs (with R. E. Lynch). Maybe his interest in numerical mathematics started in the war years – as it did for many mathematicians then.

I: He gave you some problems to work on?

dB: Yes. He was at that time working on problems in fluids. He had written a book (with Zarantonello) on "Jets, Wakes and Cavities". He was looking at specific problems. I had to work out two-dimensional flows over or under an obstacle, like flow under a sluice gate. I produced some numerical results which ran counter to the perceived wisdom at the time.

I: Was your PhD connected with that work?

dB: Not at all. My PhD was totally different. When I came to General Motors, they had just started to use computers in order to represent car surfaces mathematically. The notion was that once you had a mathematical description of a car surface, then you could use computers to generate cutting paths for numerically controlled milling machines to cut the dice needed for forming or stamping that surface in sheet metal. Computers had just become powerful enough for this to be feasible. When I came to General Motors, they had started using splines to represent curves and surfaces. My thesis was something that came to me as I was thinking about improving on what I found there.

I: Who was your PhD advisor?

Continued from page 17

dB: Robert Bartels. He was the only numerical analyst at the University of Michigan at that time. He was also running the Computing Center there. But, in a way, I learned perhaps more from Birkhoff because of the many interactions with him at General Motors, and from Bob Lynch, a Birkhoff student there, and from John Rice - I wrote papers with each of them there. My thesis, the topic and the writing, did not have any real input from anybody else. But I'm sure that Collatz also had some influence on me, because I had learned from him when I was in Hamburg. I did not have an advisor in the sense that I went to him every week and he would say, "That's okay" and "What's the next step?"

I: Was your PhD work crucial in shaping your future interests?

dB: Yes and no, in the following way. My thesis has to do with the use of splines in solving ordinary differential equations, and it was really looking at what is now called the projection method. You project the equation onto a finite-dimensional space and in this way get a finite-dimensional problem which you then solve. What was new in this thesis was that I pushed this point of view of projection. Second, I used not the standard functions that people used to use (people like Galerkin or Ritz used polynomials) but I used splines, especially B-splines. Unfortunately for me, I finished the thesis in 1966, and in that very year there appeared an English translation of a book by Kantorovich and Akilov which also dealt with projection methods, and in 1966 there also appeared the paper by Schoenberg and Curry concerning B-splines. So I felt scooped, and I did not publish my thesis. But it shaped my thinking because in the thesis I realized that B-splines (these are splines of minimal support) really are the right tools for understanding and working with splines. Of course, this was understood by other people before that - Schoenberg, who invented them, understood that. It became clear to me then and it colored what I did for the next twenty years.

I: What does the "B" stand for?

dB: "B" stands for "basic" or "basis". Schoenberg called them "B-splines". If you take any space of piecewise polynomials with a certain number of continuous derivatives across junction points, any such space has a basis consisting of these basic splines (splines of minimal support).

I: How much was your theoretical work motivated by problems in other disciplines in science and technology?

dB: I like to have what I do used by others. I like that very much. But what really turns me on is when in this mess, this complicated situation, I can see something simple, that it all actually comes down to something very simple. Initially

a problem might have come to me because I was interested in some applications or because I like to look at problems with some applications. But once I get intrigued by it, it doesn't matter any more where it came from although I'm very pleased that people use what I do.

I: Do you seek out problems in other fields?

dB: I never have been a person to look around for problems. There are always more problems than I can do. You listen to a talk and there is a problem. I don't actively go and talk to physicists and say, "Please give me a problem." No, but I do listen more carefully to a problem that I see has some uses. If it has no use, it has to be very intriguing.

I: Did the computer play any significant role in your discovery of theorems or proofs?

dB: Well, first of all, without computers, there would be no full spline theory today. Spline theory really developed in the sixties because only then could the computer make use of it. There was then some pressure to understand better these piecewise polynomials. So, in that sense, most of what I have done has been motivated and used by computers in a central way. But these days, I also use a computer simply because it is a wonderful tool. I work on very practical things, like representing functions or solving functional equations. For anything that I wish to prove or try to understand, the computer readily provides examples. It's an integral part of my research work. I travel with a laptop and use it all the time.

I: Is there any particular discovery or result of yours that gave you the greatest satisfaction in your research career?

dB: I have had this wonderful feeling of sudden insight only a few times in my life, but I remember every one of these moments. I can taste them even now. For example, finding the dual functionals for B-splines, realizing that the recurrence relations for B-splines, which I had come across earlier, could actually be used for the stable evaluation of splines, seeing the final step in a proof that Allan Pinkus and I made up for conjectures of Bernstein and Erdős, seeing the mathematical reason for the superconvergence numerically observed by Blair Swartz, seeing the Courant hat function as the shadow of a cube, i.e., as a box spline, etc. You suddenly see, and every time I think about these moments of insight, I'm pleased all over again.

I: Do you know whether any of your discoveries have been directly used in industry?

dB: I have to smile at that question. Three weeks ago, I was at a meeting, the annual meeting of SIAM (Society of Industrial and Applied Mathematics) in Oregon, and one of

Continued from page 18

the invited speakers, Thomas Grandine, gave a talk entitled "One day in the life of splines at Boeing". In this talk, he made the point that the B-spline recurrence relations that I mentioned were used at Boeing, by his estimate, five hundred million times a day.

The Fortran programs I wrote at General Motors in the early days were still in use there in the late eighties, as I discovered when I was back there as a consultant then, and may still be in use there buried in some big code today.

I: Could you briefly explain to a non-specialist the difference between approximation theory and numerical analysis?

dB: I know I mentioned these two terms in my CV. Maybe I can explain them along these lines. What I really do is work with piecewise polynomial functions or splines. You can use these functions to represent information, say to represent some function, curve or surface. To the extent that you then worry about how well you can approximate a particular function, or class of functions, by those splines, you are doing approximation theory. But how you approximate a function depends also on what you know about the function. If the function is given to you only implicitly, as the solution of a differential or integral equation, then you are solving functional equations numerically. When you develop and analyse those numerical procedures, you are doing numerical analysis.

I: I believe that approximation theory has a rather long history.

dB: Both have a long history. You might say that both started with Newton, with polynomial interpolation. Approximation theory proper maybe started in the 19th century, with Chebyshev, with his characterization of a best uniform approximation. Then there is Weierstrass who showed that any continuous function can be approximated arbitrarily well by polynomials. But it is Bernstein, in the early 20th century, who really developed the theory, characterizing the rate at which a function can be approximated by polynomials. On the other hand, numerical analysts think of Gauss as an early contributor (think of Gauss elimination, least-squares, and Gauss quadrature) and, by the beginning of the 20th century, people were finding numerical solutions to partial differential equations in systematic ways.

I: Numerical analysis started even before computers came in?

dB: Yes, definitely. Scientists have to find solutions to the models that they make of the world. They have no choice but to compute and they had to be very clever in this when they could only use pencil and paper.

I: How has applied mathematics changed since the early years of your research career?

dB: I don't know that much about applied mathematics. Some people have a global vision, they see their field in some more general context. I'm very much of an "opportunist". I see something interesting, I go for it. I don't have long-range plans. I don't worry about what's going to happen ten years down the road. I follow what I am intrigued by. So, how has applied mathematics changed? The computer for sure has totally changed it. Before, you had to worry very much about formulating models in such a way that a good approximate solution could be hoped for. These days you are much freer to formulate a model. You can have a very complicated model and still hope to compute good approximate solutions.

I: Do you think that the computer has, in some sense, not encouraged conceptual development?

dB: Well, it is true that even some pure mathematicians these days behave more like physicists in the sense that they can explore problems experimentally, by computations. Certainly, numerical analysts now work on complicated problems without being able to prove that the methods they are using are appropriate or effective. They have to come to terms with the fact that they may not be able to prove their results in a rigorous sense.

I: Do you consider this to be a positive development?

dB: Very much so. The more freedom there is to find out something, the better off we are. Of course, we are mathematicians, so ultimately, we do try very hard to prove that what we see experimentally is actually so.

I: Mathematicians also like to create theories. If you have a lot of information being churned out by the computer, ...

dB: ... then mathematicians are all the happier. I think mathematicians are always trying to make order out of chaos, trying to see what is really going on and what makes it go. With the computer generating all this experimental evidence, I think mathematicians are in their element. I think having the computer is very enriching.

I: Do you have any predictions or expectations of the directions in which approximation theory and numerical mathematics will be moving in the next ten years?

dB: I might guess that approximation theory will concentrate on the efficient representation of information, but I really have no idea, nor do I feel badly about that. As an example, in 1985, certainly nobody in my area would have predicted

Continued from page 19

the onset and influence of wavelets. There were, at that point, some experts who knew about them, who knew a lot about them. There were even people in numerical methods who knew about the idea of multiresolution. Still, when wavelets hit approximation theory and numerical mathematics, it was a real surprise. So, for all I know, another such fundamental change is just around the corner.

I: What sort of advice would you give to a graduate student in applied mathematics who wishes to get started in research?

dB: First, get a good teacher. If a student does not know enough to choose a good teacher, there is no hope. Also, it doesn't matter so much what the student does or chooses to do - the teacher is there to help - but the student must feel passionate about it, must really want to do it. And then the rest, assuming that the student has talent, will not be a problem.

