Mathematical Conversations

Larry Shepp: From Putnam to CAT Scan

Larry Shepp

Interview of Larry Shepp by Y.K. Leong

Lawrence Shepp is world-renown for pioneering and fundamental contributions to computed tomography and for extensive work on applications of probability, statistics and mathematics to physics, engineering, communications, mathematical finance and genetics. His work in tomography

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financial mathematics has contributed substantially to a better methodological understanding of the fundamentals of modern finance, and also the role of mathematics in the current financial crisis. He also touched on the consequences that will be drawn with respect to teaching and research from this development - but not only in (financial) mathematics.

Crisis management – learning about the dos and don'ts

has a great influence on biomedical imaging which has important applications in medical X-ray and nuclear magnetic resonance (NMR) technology.

He was a winner of the William Lowell Putnam Intercollegiate Mathematics Competition in 1958 and obtained his PhD from Princeton University in 1961. From 1962 to 1996, he was a Distinguished Member of Technical Staff at Bell Laboratories, and concurrently held joint appointments at Columbia Presbyterian Hospital, Columbia University and Stanford University. From 1996 onwards, he returned to academia full-time, first at Columbia University and then at Rutgers University. He is Board of Governor's Professor at the Statistics Department, Rutgers University since 2004. From June 2010, he will be emeritus professor at Rutgers University and professor of statistics at Wharton School, University of Pennsylvania.

His work in stochastic processes and computer tomography has earned him numerous honors and awards, among them membership of the National Academy of Science, Institute of Medicine, Academy of Arts and Science, fellowship of the American Institute for Medical and Biological Engineering, Paul Lévy Prize and the IEEE Distinguished Scientist Award.

He has been invited for visiting positions by many countries throughout the world. After returning to academia, he continues to offer his services to the medical and engineering industries. He also serves on the editorial boards of leading journals in probability, imaging sciences and computer tomography.

Shepp was invited by the Institute to give a public lecture on *Data mining with modeling: Managing diabetes* on 24 April 2008. On the same day, he was interviewed by Y.K. Leong on behalf of *Imprints*. The following is an edited and enhanced version of the interview, in which Shepp traces a distinguished and colorful career from his first success in mathematics in the Putnam Mathematics Competition to the deep impact and influence that his work on computed tomography has exerted in the medical sciences. Brimming with the energy and passion of an avid problem solver, he also gives us a glimpse of a halcyon period of multidisciplinary research in Bell Laboratories.

Imprints: You were a winner of the Putnam Mathematics Competition in 1958. Could you tell us something about it?

Larry Shepp: The remarkable thing about that was that I was trained by my mentors, Don Newman and Murray Klamkin in the problems of the exam. I am sure that I would not have won without their help. We went over the old exams very

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thoroughly and this helped me enormously. I would never win the Putnam today. The problems are hard, but after awhile they are very much like crossword puzzles. At that time I was at a very small engineering school, Brooklyn Poly [Polytechnic], and it was amazing that we beat Harvard. In the next 5 years, Brooklyn Poly continued to dominate because I trained them. They were smart people but it was the training that did it.

I: Did you get a scholarship because of that?

S: No, the scholarship came before that. I was not that strong a student in high school because it was not only mathematics and I was not good at anything except mathematics. I got a scholarship with Brooklyn Poly. They paid 300 dollars out of 600 dollars tuition. Then I won the Putnam and I got a scholarship to go to Harvard. That's what the Putnam winners get. But I turned that down. I didn't go to Harvard. I went to Princeton because of [William] Feller. He came to give a talk at Brooklyn Poly and I couldn't understand a word he said. For one thing, he spoke English with a very heavy accent. The mathematics was way above my head even though I had won the Putnam, but I knew I wanted to work with him. That was the time of Sputnik and the money was flowing from the government. So I went to Princeton instead and I never regretted it. Princeton had a wonderful impact on me. I was helped by Feller and other people. He was much older than me. I had the ideas of a young man and he had the ideas of somebody who had been around for a long time and went through many difficulties. He was from Yugoslavia and may have had some Jewishness in his past, but he never thought of himself as Jewish. He decided to leave [Yugoslavia] because he could see what was coming at that time. He and I were not on the same wavelength politically because I was not so interested in politics at that time, but we talked about mathematics and he was very helpful to me.

I: After your PhD you were at the University of California at Berkeley for only a short period and then you joined Bell Laboratories for quite a long period of time. What made you join Bell Laboratories?

S: My first job was at Berkeley and Feller helped me get that job. I was there for only a year. What happened there was interesting. Several things happened. One was that my father became ill. I knew I would have to go back to New York in the east. I wanted to get a tenure position at Berkeley, but I thought I would go back to the east for a year and then I would go back to Berkeley, but it didn't work out well. I was very busy studying the Russian language for my own interest. They gave me two very big courses to teach, you know, hundreds of people. It was very hard for me. And then, Jerzy Neyman asked me to run a seminar. It was a lot more than I thought I could handle, but he said, "You don't have to do it if you don't want to do it." So I said, "Well, in that case, I don't want to do it because I'm too busy writing papers, teaching so much and I have all these responsibilities and learning Russian." And then he said, "But the youngest person in the department runs the seminar. That's the tradition." By this time, I was fed up with him. I didn't respect him that much anyway, and I said, "The tradition is over." He just left and I didn't know he was angry. Then a few years later, I applied to be an exchange scholar in Russia and I lived in Russia for 6 months. But when I got the letter, I saw the level of anti-Semitism in Russia and I began to work against the Russian regime in any way I could. There were some laws that I broke while I was there in 1966. As a result, the KGB [state and intelligence agency of the Soviet Union] threatened me with 15 years in prison if I didn't become a Soviet agent. In January of 1967, I lived in the US embassy for a week and made an application for an exit permit.

I: It sounds like fiction.

S: It's all true. I can prove it. You can check the *New York Times*. What happened was that they gave me an exit visa. I had anticipated that they would. They would not risk the exchange program which was very beneficial to the Soviets 'cause they could send spies to America while we were sending naïve people. I wasn't naïve and they threw me out. When I got back to the United States, the FBI [Federal Bureau of Investigation] thought that because they threw me out, I was a Soviet agent and that they threw me out as a cover. So they would not let me go back to Bell Laboratories. That was the only place I could go to at that time, I could have gone to University of Kansas or Bell Laboratories. Bell Laboratories was like manna from heaven. I had a wonderful time there.

I: Did you do any classified work at Bell Labs?

S: I never did any classified work directly though I often spoke with engineers who did classified work, but they always hid details from me. The FBI thought that having a KGB agent inside the Bell Laboratories would not be so good and they refused to let me return to my position. But Bill Baker, who was Vice-President of Bell Labs (I didn't know him), came to me and asked me in a roundabout way whether I was a spy. I said, "No". I worked against the Soviets; the FBI was safe in the United States and they were accusing me of being a traitor. I told him, "I am a loyal American and I hate communism." He believed me and he told me later on that he argued with the FBI in Washington

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for a whole day and they finally let me back into Bell Labs. Many years later, I made a freedom of information act request – we have this wonderful freedom of information act in the United States. As a citizen, you can get access into government files about you. The law enabled me to get what Neyman said about me. Neyman said, "Never put this person in a position of trust." The FBI went to interview everybody who knew me and Neyman was in that category. Everybody else said, "No, he's fine. He's a loyal American." But Neyman said, "Don't trust him." You know why? Because I wouldn't run his seminars. But I like Berkeley and I go back to Berkeley a lot, and they made me a Miller Fellow.

I: But you were at Bell Labs for a long time.

S: Only for 34 years! In 1996, Bell Laboratories broke up – the mathematicians went one way and the engineers went the other. I was always working with both of them, and I didn't really want to make that choice. I thought that 34 years was enough, so I went to academia at that point – one very nice year at Columbia and then I went to Rutgers.

I: You also went to Stanford?

S: I went to Stanford over the years frequently. They made me an adjunct professor in statistics for about 15 years. Then they decided that they didn't want to have any more adjunct professors, they wanted me to go there full-time. I couldn't do it because of my children and my wife and my son. Bell Labs is on the east coast and Stanford is on the west coast. I have a very long-standing relationship with Stanford but it was never full-time

I: Could you tell us how you came to work in computed tomography?

S: I happened to be in Columbia Presbyterian Hospital when the great English engineer Godfrey Hounsfield [Nobel laureate in Physiology/ Medicine 1979] was trying to sell Columbia Presbyterian Hospital a CAT scanner in 1976. I happened to be there that day. I saw his demonstration, I asked him, "Are you using a formula based algorithm or are you doing it by some iterative procedure?" He said, "We're doing an iterative procedure, which we believe is optimal". I didn't believe that was optimal. So I switched from probability to engineering for a while, and that was perhaps the smartest thing I ever did. The most important, interesting and motivating thing that I have ever done was in experimental tomography.

I: Did you get into the experimental part?

S: I did. The key ideas in the design were due to my colleagues, not me, but I played a big role in the electronic design of the 4th generation CAT scanner. I was in pretty much every aspect. That was in 1972 through the 1980s. Now the CAT scan is dominated by magnetic resonance imaging; technology moves very fast.

I: What were the mathematical aspects?

S: The mathematical aspects from the mathematician's point of view are very clear. Radon's theorem had to be involved and that was very exciting. If it had not been for Radon's theorem, I never would have got into it because it gave me the feeling that I could make some contribution. I suspected that Hounsfield didn't know Radon's theorem and I did, and that was a good thing. The contribution that I made was not so much in the algorithmic development (although everybody talks about the Shepp-Logan algorithm) but it was more in the understanding of how to judge and how to read rather than the development of the algorithm. I did very well on that but that was largely pretty well understood from the work of all the people like [Ronald] Bracewell and [A.V.] Lakshminarayanan and [G.N.] Ramachandran and other people. The probabilists, [Harald] Cramér and [Herman] Wold wrote a paper in 1927 – they were not aware of Radon's theorem but they re-derived it via the Fourier transform. All that stuff was pretty well understood by the time I came along. I made a contribution to the numerical aspects of the algorithm. I made a very clever step that speeds up the algorithm and that was an important thing.

I: That wasn't really probability, wasn't it?

S: I like to think it's probability, a little bit. Computed tomography is based on reconstructing a function from its marginal distributions, and a marginal is certainly a basic concept in probability. I do not think of CAT scanning as all that far from probability because of this. Even more so, emission tomography is driven by statistics in an even stronger way since the major limitation is statistical noise due to low counts.

I: There seems to be two types of tomography, one is continuous and the other is discrete, isn't it?

S: Yes. There is a discrete tomography and there are many types of tomography, i.e. many inverse problems that can be called tomography. What is usually called discrete tomography is a very special one and is still very much in a research mode. It refers to trying to find a fault in the crystals used in integrated circuitry. Most other problems of inverse type are, as you say, continuous, including emission tomography.

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I: Are the problems solved by a combination of algorithmic and statistical methods?

S: The problems in computed tomography – projection, Radon inversion – that's Fourier transform. Emission tomography seeks a maximum likelihood estimate which has a statistical basis. NMR, magnetic resonance imaging, is tomography also; that's pure Fourier transform based.

I: Have advances in computed tomography contributed to non-invasive methods of detection and treatment in medical science?

S: Yes, extremely so. Nobody is going to get to old age anymore without being screened by CAT, MRI scanners and maybe emission scanners (PET [positron emission tomography] or SPECT [single photon emission computed tomography]) as well.

I: Are advances in the hardware comparable with the advances in the theoretical aspects?

S: Yes. Every one of these tomographies depends very much on the advances in hardware as well as software. But I would say that in the CAT scanner (Hounsfield's invention) the main role is played by mathematics. In emission tomography, the main role is played by statistics. The algorithm in each case can be set up by using ray processes and things like that. This is a secondary issue. NMR is very strongly driven by Fourier transform and the use of gradient field. The advances in a rapidly growing field depend a lot on the hardware, but you really cannot separate the two.

I: Do you see yourself as a problem solver?

S: Yes. It's really funny that you've pointed this out in the interview where you go back to the Putnam. It's true that I see myself as a problem solver. I really urge people to think of themselves as problem solvers and apply whatever methods that they can dream up to solve them. I learned that from Don Newman who was a mentor of mine. Rather than being driven by trying to develop a theory, you are driven by problems you are working on. This is a very important philosophy. Sometimes people forget this and fall back on the methods they know to solve the problem that just appeared. This rarely works well.

I: After all, mathematics is about solving problems.

S: Absolutely. I think it was Hermann Weyl who said the mathematics walks on the feet of little problems.

I: Which piece or pieces of work in probability and statistics give you the fondest memories?

S: I'm most happy with my work on probabilistic problems on random covering and zeros of random polynomials. It is amusing that probabilists do not seem to be as interested in solving problems any more. Rick Durrett and I have written a polemic on this in which I point out that a recent paper of mine that solved a problem I worked on for 40 years and finally solved was rejected because the referee did not like the method that was used.

I: When did you become interested in problems in mathematical finance?

S: In 1983 or 1984, ATT [American Telephone & Telegraph] found that they did not have enough expertise in economics to argue effectively for their monopoly position. A new economics department was formed within the math department under Ed Zajac to provide theory for dealing with the FTC (Federal Trade Commission). Ed hired 40 economists and since they were going to be put into the math department, they had better be good mathematicians. He hired some very smart people. That was when I began to get interested, but I didn't get completely interested in it until a little later with when my friend [Albert] Shiryaev and I began to work on problems in Russian options. I had done a lot of stochastic optimization problems before getting into finance per se, but he wanted to work in finance. He helped me bridge that gap. But since I have gone to academia and found that many of our students want to become rich on Wall Street and want somebody to teach them the mathematics behind Wall Street, which is basically Ito calculus, I am more involved in mathematical finance. (This may change with the 2008 stock market disaster.)

I: You collaborated quite a bit with Shiryaev, isn't it?

S: Yes, we have many joint papers and I'm expecting him to visit me in a couple of weeks in New Jersey. In fact, even when I was in Russia at the time when the KGB clamped down on me, I always entertained the idea that he might have been part of it.

I: Although you left industry to join academia full-time in 1978, you have continued to maintain an almost continuous connection with industry. What is the motivating force behind this?

S: That is a very good question. I guess part of the reason was problem-oriented in a sense. Academics often go off the deep end but they lose track of why they are doing it

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because it is so pure. I like to play a role in the world like all applied mathematicians. I like to solve problems and so I do a lot of consulting for industry like electronics, engineering, semiconductors and lots of other companies. I like to stay close to the real problems; it's my training from Bell Laboratories.

I: I think you were once quoted for something about rediscovery.

S: I can tell you what that was about. Gina Kolata [of *New York Times*]called me up one day. She said she lost the quote that I made when somebody said that he knew something I claimed to have discovered and that in fact I simply rediscovered it. I replied, "When I discovered it, it stayed discovered." Now, in fact, I stole that quote. It was said by somebody else, maybe even by many people, but I heard [John] Tukey say it. Tukey in fact said, "When I discovered the fast Fourier transform, it stayed discovered". I believe that I gave him the "credit" for this clever defense when I stole it. Maybe I neglected to do it, but, in any case, I said to Gina, "Don't ascribe the quote to me, ascribe it to Tukey because it's NOT mine." She said, "Okay, I promise to do that." Sure enough she did not do it; I called her to complain that she had promised to do it and did not keep her promise. Her excuse was that the editor cut it out because the article was too long. Aargh!

I: But it's a great problem. There's so much information and knowledge scattered all over the place. It's hard to locate them unless somebody writes a book

S: I'm not going to write a book. I tried that but it's very hard to write a book. There is a big role to play if somebody wants to do that. There is a good book [*Mathematics of medical imaging*] by Charles Epstein on tomography from Larry Shepp's point of view. I am grateful to Charles for doing something I cannot do. Mark Kac was great at writing books too.

I: I notice you are also interested in problems in genetics.

S: Yes. Genetics attracted my interest because it's such an important thing. I tried to contribute and we wrote a paper on entropy and information theory in genetics. You really want to understand the correspondence between the gene and the part of the genome and the function you observe. We made a feeble attempt by trying to use entropy. We did find an interesting conclusion of negative type. We showed that introns have lower entropy (less randomness) than protein producing exons. This contradicted the conventional wisdom that the role of introns is a placekeeper for large scale genetic changes. It is tempting to form this hypothesis because to assume that gradual changes in genes can occur it is necessary that each change has survival advantages but this seems very unlikely say in the development of an entirely new function such as hearing or sight. Placekeeping changes may well take place in the so-called junk DNA, but if introns were placekeepers then they ought to have higher entropy but they certainly do not. Therefore they must have survival functionality. (See M. Farach, M. Noordewier, S. Savari, L. Shepp, A. Wyner and J. Ziv, ``On the entropy of DNA: Algorithms and measurements based on memory and rapid convergence'' *Proceedings of the Sixth Annual ACM-SIAM Symposium on Discrete Algorithms,* (1995).)

I: Do you have any graduate students?

S: I have at the present moment only one or two, but I've been carrying about six along at the same time. It's a lot of fun. I do feel the need to pass on to others what I have learned from Feller, Newman, Klamkin and Slepian, who helped me as mentors.

I: Is Slepian a mathematician?

S: Slepian was trained in physics and he was a great mathematician. He died recently. [David Slepian (1923 – 2007) was head of the Mathematical Studies Department of Bell Labs.]

I: Do you believe one should try hard problems?

S: I never worked on the Riemann Hypothesis or the four-color problem because neither of those problems turned me on and I knew that both were not in the area that I am good at. Those problems did not grab me but tomography grabbed me so much that it was the only thing that I wanted to do. I asked Dick Hamming, "Dick, would you use iterative methods or would you use a formula?"

I: Is it Hamming of "Hamming codes"?

S: Yes, he was a very good engineer, mathematician and computer scientist. He was at Bell Labs. Dick said he would just use iterative solutions. He made wild statements like "The Lebesgue integral is of no practical value in the sense that if the design of a plane required the Lebesgue rather than the Riemann integral then I refuse to fly in it". He is, of course, right in this assertion, but for one who is trained like I was trained to love Lebesgue integrals, one wants to believe that knowing Fourier-Lebesgue integration has utility. That was a big motive for me. I wanted to show Hounsfield and Hamming and myself that one could do

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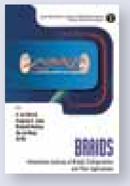
something with Lebesgue's mathematics. At the end of the day, the mathematics that I and Ben Logan created was not exactly Lebesgue integration. The truth seems to be quite on Hamming's side. But, nevertheless, I think there are insights that were obtained from pure mathematics. I'm pleased that I could show that in some way, and even more so that I could I leave it to Charles Epstein to write the book.

I: Any advice for students?

S: I would urge them to, when they see a problem that really turns them on, throw everything at it, don't waver, work hard, don't give up and stick with it until somebody, preferably you, solve it. But you've got to choose the right problem. It's so hard to decide when you've got the right problem. Sometimes you've got to put it on the back burner. You don't want to get hung up on one problem forever. I learned at Bell Labs to work on several problems at the same time. It's a hedge.



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