Journ@l Electronique d'Histoire des Probabilités et de la Statistique

Electronic Journ@l for History of Probability and Statistics

Vol 5, n°1; Juin/June 2009

www.jehps.net

Letter from Joseph L. Doob, dated June 6, 1985

In 1985, Pierre Crépel consulted with Paul-André Meyer about how to ask Joseph L. Doob about the genesis of his work on martingales. Meyer advised Crépel that if he wanted a response, he should keep his inquiry short, leaving it to Doob to tell whatever he wanted to tell.

We would like to thank Crépel for allowing us to reproduce the letter Doob wrote in response.

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June 6, 1985

Dear Dr. Crepel:

Meyer's advice was sound.

My 1932 thesis was in analytic functions but involved measure theory and Fatou's theorem. I then spent two complex variable years at Columbia University, on National Research Council fellowhips. In 1934 I had no job prospects but Koopman told me that there was money in probability and statistics, and that I should talk to Hotelling, a Columbia statistics professor. Hotelling got me a grant for a year, during which I studied probability. In 1939 (?) I reviewed Ville's book, which led me to martingale theory. My first probability work was dominated by the idea of putting intuitive ideas into rigorous mathematics. See for example my 1936 NOTE ON PROBABILITY, which led me to optional times and suggested a result in what would now be called martingale differences. Halmos proved this result in his 1938 thesis, written under me.

As you see, the desire to eat led me from analytic functions into probability. It was seeing Kakutani's 1944 work connecting Brownian motion with harmonic functions that led me back. I had read Rado's book on subharmonic functions and the analogy made submartingales natural, but without crossing inequalities for submartingales I needed the discrete parameter Meyer decomposition to get submartingale convergence theorems. I proposed to my student Snell that he prove submartingale crossing inequalities and he did so in his thesis. I was challenging probabilists to prove the Meyer decomposition theorem for several years (of course I did not have the precise formulation) and he finally came along and proved it.

Meyer has been properly sarcastic about me and the strong Markov property. The fact is that I did not see how to prove it. However I refereed Blumenthal's strong Markov property paper and had him change his original formulation to put it into the context of sigma algebra filtrations - with no change of proof.

My stochastic process book was started during World War II, in about 1944, but the martingale work in it was derived after 1947, if my unreliable memory is correct. I did not get serious potential theory applications until my 1954 paper. It was only then that I realized that there was more than a formal relation between potential theory and ~~probability~~ martingale theory. In my recent book I show that some proofs are valid in both contexts.

I learned a lot from Lévy's 1937 book, but my martingale theory work was not influenced by him.

I considered the orthogonal series work by Marcinkiewicz and others as amusing but special results and did not see any connections with martingale theory.

L. J. Savage once wrote me that Bachelier had done some martingale theory in one of his books, but I lost Savage's letter and never verified his statement.

Do you know Bernstein's martingale difference work? (See Doklady 1937 p. 275 and later work. See also the commentary on this in Vol, 4 of Bernstein's collected works.) I discovered this work a few years ago.

This is all I can think of.

Sincerely,

J L Doob