# BOOK REVIEWS

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*Measure theory*, by J. L. Doob. Graduate Texts in Mathematics, vol. 143, Springer-Verlag, New York, 1994, xii+210 pp., $49.00. ISBN 0-387-94055-3

This, the third book by J. L. Doob, follows two previous works. The first, *Stochastic processes*, had a major influence on twentieth century probability. The second, *Potential theory and its probabilistic counterpart*, was the exposition of two separate but related theories, to which Doob himself made crucial contributions. What about this one? It is simply called *Measure theory* and is rather thin (200 pages) but not strikingly so (it cannot compare with the famous—and long out of print—first edition of Bauer's *Maßtheorie* as a *Sammlung Göschen*, which could be kept in the inside pocket of one's jacket with the credit cards). Its most visible feature upon opening it is the remark that it is not typeset with TEX, with some loss of typographical quality but with the refreshing impression of finding an apple or a tomato on the market that does not look exactly like all apples or tomatoes. Then upon reading the introduction we discover that "this book was planned originally not as a work to be published but as an excuse to buy a computer... ," and then we may wonder, "If *I* bought a computer, would I write a book on measure theory?" The implication is that the author *likes* measure theory—an elderly lady that once had a notorious affair with N. Bourbaki but later came back to a settled life. A book written by someone that enjoys the subject is likely to be attractive. Many students are afraid of measure theory; in fact, it is often a turning point in their mathematical education, as a rather austere concentrate of epsilons and etas. If one really finds it boring, then it may be a good idea to turn to some kind of "geometry".

An old lady indeed! She was already mature in the thirties, when one got rid of the particular features of intervals and cubes: the second edition of S. Saks's *Theory of the integral* (1937) does not differ substantially from the books I perused to write this review. All recent books on measure theory are "good" books, well planned for students, and they are very much alike. I must say our library has stopped buying them. It will buy this one. Why?

If we look again at the introduction, three claims to originality are stated. The first one is "the use of pseudometric spaces". The book claims to use true functions everywhere, instead of classes of functions, which are too often handled sloppily. It is true that we usually think in this way, and it is good to see someone saying it frankly instead of the usual excuses about using the language of functions while dealing with classes. But still classes must be swallowed (and you will find them on page 115) in order to turn $L^2$ into an honest Hilbert space. Thus the difference is more psychological than mathematical.

Another claim to originality is the importance given to convergence of measures. It is very good that the author included the Vitali-Hahn-Saks theorem and studied weak convergence of measures on metric compact and locally compact spaces. The more recent (Prokhorov) theory of tightness and weak convergence on metric spaces is not included—possibly a reasonable choice for an elementary book.

There are several levels of measure theory (which in history were successively branded as "empty abstraction"). The first level is Lebesgue integration on the line and on euclidean space. The second one is the standard general measure theory, either in its abstract form or in its topological form (Radon measures). Doob's book stops at this level or, more precisely, includes measure theory on compact metric spaces but not on Polish spaces. The third level requires analytic sets and capacities and is still considered hard mathematics—which it really is not; they are not even mentioned in the book, though Doob himself was among the first to use capacities in probability theory. The fourth level (still under active research) involves a lot of descriptive set theory and possibly refined axioms like the continuum axiom or Martin's axiom or the existence of large cardinals. Again Doob's choice seems very reasonable.

Let me now discuss the main original feature of this book with which I most heartily agree. As the introduction says, "Probability concepts are introduced in their appropriate place, not consigned to a ghetto." That is, a student using this book (after all, it belongs to the Springer collection of "Graduate Texts" and is meant to be used by students) will learn probability without even noticing it, as M. Jourdain was unwittingly talking prose. Most illustrations given in this book are taken from probability theory, though a few important ones (the $L^2$ theory of Fourier series, the Fourier-Plancherel theorem, and the existence of radial limits of harmonic functions in the disk) are borrowed from "pure" analysis. Even in 1994 saying that probability theory is not only *a part of* measure theory but *is the same as* measure theory is a bold point of view. I think it corresponds to the truth, but it is worthwhile to insist a little on the reasons why it is not very popular.

First of all, probability theory has deeper roots in the "real world" than most of mathematics. Historically it arose from gambling, which belongs to the "real" world but is a human activity ("The Lord does not play dice"). Then it invaded other forms of human activity (insurance, economy, warfare), then biology (heredity), and much more recently parts of physics; the problem of quantum mechanics must be left aside, since it involves a different kind of probability. Mathematicians have a strong tendency to believe in the "reality" of the objects they are studying, whether a triangle or a Lie group; otherwise, would they devote so much time and thought to them? Gamblers also have a strong tendency to believe in chance. Thus, probabilists have a philosophy that differs from that of "pure" mathematicians. It is an historical fact that Kolmogorov's annexation of probability theory to measure theory was violently rejected by many probabilists as useless abstraction which did not respect the basic intuitions of probability. When I was a student, Paul Lévy used to tell us that he could not accept the idea of choosing once and for all one single $\omega$, while it was clear that chance was acting all along time. Actually it was Doob, who, being one of the first to take seriously Kolmogorov's approach, found the way to reintroduce time into measure-theoretic language, as the index set for a *family of* $\sigma$-*fields*. This is the subject of the last chapter of the book on martingale theory.

On the side of nonprobabilists, there were symmetrical objections, not to the mathematical model, of course, (measure theory) but to the philosophy of probability. I found a typical example of this in a beautiful lecture of R. Thom on determinism and chaos. After discussing the collapse of classical determinism, he expressed his dislike of probabilistic models as being a dishonest escape from the problems of science. Five minutes later, he was quietly talking about the Liouville measure of a hamiltonian system, and ten minutes later he mentioned his interest in proving structural stability in "almost all" situations. Thus some parts of measure theory, or some classes of negligible sets, seem to be more honest than others.

Even probabilists may have doubts about the "honesty" of their domain. Paul Lévy also used to tell that he did not understand how an electron could be "free". The paradox of probability theory is that of being a precise mathematical model for something which is *unthinkable*, namely, an effect without a cause. The electron is not "free"; if it were, there would be a *reason* for its behaviour, while actually there is none. Choosing a number at random means that I have absolutely no reason for choosing this number rather than that one, and still I do it.[1] After all, there are other examples of mathematical models describing unthinkable processes—can one "think" within our mental categories the relativistic unification of space and time or a curved space-time? Still, we handle this in a perfectly clear and efficient symbolic language, which is better adapted to the "real world" of physics than the naive language. But randomness is also a somewhat insulting idea ("The referee wrote he would have accepted my paper if he had got a double six"), and besides that, the mathematical model itself consists of many universes (sample functions) of which we see only one. Thus, probability theory is held in suspicion by many scientific thinkers, who do not accept the claim that sometimes "no explanation" is the best explanation.

The book completely avoids this kind of philosophical feud. It is striking to see that Doob, one of the great probabilists of our century, does not claim any privilege for probability theory within measure theory. At least, he is careful not to give any hint about his own ideas on the subject—maybe he prefers not to think about the unthinkable. At several places, he makes a sharp distinction between "mathematical probability" and "real probability" in terse, ironical sentences. Personally, I consider this very healthy.

On page 80 it is mentioned that $n$ drunken *men* are trying to return home. The author should have added *or women.*

One may hope that, from now on and partly because of this book, it will be impossible to write a measure theory text without including probabilistic ideas.

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[1] This statement seems to apply to the uniform law only. However, applying a one-to-one mapping does not change causality, and all probability laws without atoms are isomorphic from a measure-theoretic point of view.