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Theory of probability, Volume I, Bruno de Finetti, John Wiley & Sons, New York, 1974, xix + 300 pp., $22.50.

Theory of probability, Volume II, Bruno de Finetti, John Wiley & Sons, New York, 1975, xviii + 375 pp., $29.50.

In a foreword to this pair of volumes, Lindley says "I believe that it is ... destined ultimately to be recognized as one of the great books of the world". I think this is more likely to apply to the original version in Italian, for the English translation is much less lucid than most of the chapters in de Finetti's *Probability, induction and statistics,* which is a collection of articles. In this review I shall refer to Volumes I and II as (I) and (II) and to this other book as (0).

de Finetti is one of the pioneers in the development of subjective probability, and of the Bayesian or, more accurately, the neo-Bayesian school of statistics. At first, his writings appeared in Italian and French, beginning in the 30's, and especially in 1937, and were not at first influential in English-speaking countries until he was "discovered" by L. J. Savage who edited one of de Finetti's articles for publication in 1951.

The language barrier acted in both directions, for de Finetti arrived at his basic position without knowing of the somewhat similar work by F. P. Ramsey which was published in England in 1931.[1] Both de Finetti and Ramsey argued convincingly that a system of precise subjective (personal) probability judgments must satisfy the familiar axioms, and that rational actions should maximize expected utility. de Finetti's position is, however, more "radical" (to use his own epithet), for he claims that "Probability does not exist" (I, p. x). By this he means that it does not exist in an objective sense, in other words he denies the existence of physical probability. Although I agree that physical probability cannot be measured without using subjective probability, I feel that to deny its existence is too extreme. It could have been consistently maintained that the probabilities underlying classical statistical mechanics are necessarily subjective, and arise because of our ignorance of the precise initial conditions, but the probabilities of quantum mechanics might well be an irreducible feature of the interaction between a physical system and a piece of physical apparatus. Even in classical mechanics, the notion that the initial conditions could "exist" to an accuracy of millions of decimal places seems far-fetched; yet Laplace's demi-urge would have urgently required such accuracy because a detailed prediction to a time t into the future, of specified accuracy, would require a number of decimal places proportional to t!

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[1] de Finetti had an important direct influence on Savage; whereas my main sources were Ramsey, Keynes and Harold Jeffreys. My 1950 book was reviewed by both Savage and Lindley, the latter when he was still a frequentist, so the Cambridge school might have had an early indirect influence on both these prominent Bayesians. It was Savage's book of 1954 that completed Lindley's conversion to the Bayesian camp. The entire network of influences is of course very complex and may depend more on oral than on written communication.

After constructing a utility scale, de Finetti introduces probability via expectation, which he calls *prevision*. The prevision of a random variable X, "according to your opinion, is the value $\bar{x}$ which You would choose" if "You are commited to accepting any bet whatsoever with gain $c(X - \bar{x})$ where c is arbitrary (positive or negative) at the choice of an opponent" (I, p. 87). An event E is regarded as a special case of a random variable, taking the value 0 or 1 depending on whether E is (verifiably) false or true, and its prevision $P(E)$ is also called its (subjective) probability. de Finetti shows that an equivalent definition of the prevision $P(X)$ is obtained by assuming a squared loss, proportional to $(X - \bar{x})^2$, and he produces a very ingenious but elementary geometrical argument, based on squared loss, to prove the product law of probabilities (I, p. 137; and 0, p. 15). It is one of the features of de Finetti's work to use illuminating geometrical arguments, as may be an Italian tradition. He also likes to give intuitive reasons for theorems, their "wherefore" (II, p. 218), which he rightly regards as more important than their formal proofs. It is a pity that this opinion is not universally taken for granted.

Another assumed property of $P(X)$ is that You should be indifferent to an exchange of X for $P(X)$ so that $P(X)$ can also be called the *price* of X. This leads to an additive property for P. It also implies that the definition in terms of gain is self-consistent since $c(X + Y - (\bar{x} + \bar{y})) = c(X - \bar{x}) + c(Y - \bar{y})$, though I am not sure whether de Finetti mentions this obvious requirement explicitly.

After completing the arguments, based on coherence of betting behavior, in favor of the classical axioms of probability and rationality, de Finetti argues in considerable detail against the adoption of Kolomogorov's axiom, the axiom of countable additivity. He regards this axiom as irrelevant for practical purposes and unjustifiable on theoretical and conceptual grounds. In particular, he mentions (I, p. 124) that limit-frequency probabilities cannot satisfy Kolmogorov's axiom. This is clear because event E_i could occur just at time i, where $i = 1, 2, 3, \ldots$, *ad inf.* so that $P(E_i) = 0$ for all i, yet $P(E_1 \vee E_2 \vee E_3 \vee \cdots) = 1$.

There is an unstated but interesting philosophical implication in the adoption only of finite additivity. For it would permit, while not necessarily supporting, Karl Popper's claim that every fundamental law of physics, expressible in finite terms, has zero logical probability, while at the same time it would permit their logical disjunction to have positive probability. It is ironical that de Finetti should unwittingly give a loophole to Popper who is an arch-enemy of the use of subjective probability in science, and especially of the theories of de Finetti and Savage.[2] My refutation of Popper's thesis that laws have zero probabilities (*Synthese* **30** (1975), p. 49) depended on Kolmogorov's axiom and is therefore not decisive if only finite additivity is assumed. (I still do not agree with Popper, but the *relative* probabilities of theories are in any case usually more important than their absolute probabilities.)

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[2] Popper's attack is made in *Dialectica* **11** (1957), 354–357. He sent me a preprint for comments, and when I pointed out that his attack did not apply to my position he simply deleted my name from his text!

In my opinion de Finetti's most important single contribution to the foundations of probability is his representation theorem. It shows, in brief, that a self-consistent set of subjective probability judgements concerning the outcomes of an infinite sequence of events, when the prior is exchangeable (*permutable* in the earlier and preferable terminology of W. E. Johnson, 1924) can be represented *as if* physical probabilities exist and are unique. It is possible to interpret this mathematical theorem as indicating the impossibility of refuting solipsism. Of course this does not by any means imply that de Finetti is a solipsist, in fact he says (I, p. 218) that he does not support any "isms", not even subjectivism, but that "the sole concrete fact which is beyond dispute is that someone . . . feels himself in a state of uncertainty . . . all the rest is . . . something of an extra, which, at best, serves to help fix one's ideas." If de Finetti adopts any ism it would I think be operationalism.

After dealing with the foundations of probability in the first five chapters, de Finetti discusses the mathematical theory of probability (distributions, characteristic functions, and stochastic processes) in Chapters 6 to 10. Even in this familiar territory he shows originality. It is like Picasso showing he can paint well in a traditional style. In the final two chapters of the "text" he deals with inductive reasoning, statistical inference, and mathematical statistics. Some impression of his position can be gleaned from the following extracts: "If probabilities and probability distributions are not mentioned, any reference to an 'estimate' is nonsense" (II, p. 200). "Sometimes the use of the improper uniform prior distribution is interpreted as representing 'total ignorance'. This is nonsense" (II, p. 237). "The *likelihood* . . . is a point function, and 'equating' it to a density is a meaningless idea" [because it ignores the Jacobian] (II, p. 238). "Given . . . the Bayesian approach, . . . the 'likelihood principle' inevitably appears to be rather obvious, and certainly not worth getting excited about" (II, p. 240). " . . . they ignore one of the factors (the prior probability) altogether, and treat the other (the likelihood) as though it . . . meant something other than it actually does. This is the same mistake as is made by someone who has scruples about measuring the arms of a balance (having only a tape-measure at his disposal . . .), but is willing to assert that the heavier load will always tilt the balance (thereby implicitly assuming, although without admitting it, that the arms are of equal length!)" (II, p. 248). " . . . occasionally one hears that 'to accept an [sic] hypothesis' means 'to agree to behave as if it were certainly true'. This is nonsense" (II, p. 252). "Free at last from paradoxes and contradictions, we emerge from our sea of troubles" (II, p. 255).

The work restarts with an appendix of over a hundred pages which runs over some of the ground again in greater detail and deals especially with verifiability and with quantum mechanics. de Finetti appears sympathetic here to the possibility of philosophical indeterminism but he does not state that physical probabilities might exist. The final paragraph, referring to his attack on Kolmogorov's axiom, is "I may be wrong. My criticisms will not have been in vain, however, if . . . to refute them someone . . . explains and justifies . . . those things which, up till now, have merely been 'Adhockeries for mathematical convenience'."

The two volumes do not have a bibliography because this had been provided in (0). Since Koopman's work (1957) on probability in quantum mechanics is cited in (I, p. 15) and (II, p. 303), I mention that it was not a book, but a chapter in the Proceedings of a Symposium. Also Ramsey's initials were not FDR though he might have made a good philosopher king. On a point of terminology, "Bayesian estimation interval" would be better than "Bayesian confidence interval" (II, p. 244), which sounds too much like a square circle. When "Bayesian" is dropped, the confusion is apt to be further increased.

On p. 225 of (I) de Finetti discusses decimals with missing digits, or with digits having the "wrong" frequencies, and he seeks a bibliographical reference. One such is *Proc. Cambridge Philos. Soc.* **37** (1941), p. 200, where the reviewer conjectured a relationship between entropy and the Hausdorff-Besicovitch dimensionality of such sets, a relationship that was proved by Eggleston in 1949. Hausdorff-Besicovitch dimensionality could be used to enrich de Finetti's theoretical discussion of "levels" of zero probability (I, §3.11).

In summary, these volumes make important writings of this pioneer available to the English-reading world, and will encourage some probabilists, statisticians, and philosophers of science to learn Italian.

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Presentation of groups, by D. L. Johnson, London Mathematical Society Lecture Note Series, no. 22, Cambridge Univ. Press, New York and London, 1976, v + 204 pp., $11.95.

Given a set X there exists a free group F having X as a basis; the elements of F are all *words* in X, that is, all formal products $x_1^{e_1} \cdot \ldots \cdot x_n^{e_n}$, where $x_i \in X$ and $e_i = \pm 1$. The set X is called a basis of F because it behaves very much as a basis of a vector space does: given any function $\varphi: X \to G$, where G is an arbitrary group, there is a unique homomorphism $\tilde{\varphi}: F \to G$ extending φ. An immediate consequence of the existence of free groups is the theorem that every group G is a quotient group of a free group. If X is the underlying set of G and $\varphi: X \to G$ is the identity, then $\tilde{\varphi}$ is a homomorphism of F onto G, where F is free with basis X; if R is the kernel of $\tilde{\varphi}$, then $F/R \cong G$. One knows that every subgroup of a free group is itself free, so that R is free on some basis Y' comprised of certain words in X, and, obviously, Y' generates R. Since R is a normal subgroup of F, however, one may describe R by a smaller set of words than Y', namely, a set Y that generates R as a normal subgroup of F (in building R from Y, one may not only form words in Y, he may also form words in conjugates fyf^{-1} for $f \in F$). These two sets of words X and Y describe F and R completely, hence describe $G = F/R$. $\langle X | Y \rangle$ is