Theory of Probability

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Theory of Probability

A Critical Introductory Treatment

Bruno de Finetti

Translated by Antonio Machí and Adrian Smith

WILEY

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This work is dedicated to my colleague Beniamino Segre who about twenty years ago pressed me to write it as a necessary document for clarifying one point of view in its entirety

[1970]

Contents

Foreword

It is an honour to be asked to write a foreword to this book, for I believe that it is a book destined ultimately to be recognized as one of the great books of the world.

The subject of probability is over two hundred years old and for the whole period of its existence there has been dispute about its meaning. At one time these arguments mattered little outside academia, but as the use of probability ideas has spread to so many human activities, and as probabilists have produced more and more sophisticated results, so the arguments have increased in practical importance. Nowhere is this more noticeable than in statistics, where the basic practices of the subject are being revised as a result of disputes about the meaning of probability. When a question has proved to be difficult to answer, one possibility may be that the question itself was wrongly posed and, consequently, unanswerable. This is de Finetti's way out of the impasse. Probability does not exist.

Does not exist, that is, outside of a person: does not exist, objectively. Probability is a description of your (the reader of these words) uncertainty about the world. So this book is about uncertainty, about a feature of life that is so essential to life that we cannot imagine life without it. This book is about life: about a way of thinking that embraces all human activities.

So, in a sense, this book is for everyone; but necessarily it will be of immediate appeal to restricted classes of readers.

Philosophers have recently increased their interest in probability and will therefore appreciate the challenging ideas that the author puts forward. For example, those of the relationships between possibility and tautology. They will notice the continual concern with reality, with the use of the ideas in practical situations. This is a philosophy intended to be operational and to express the individual's appreciation of the external world.

Psychologists are much concerned with the manner of this appreciation, and experiments have been performed which show that individuals do not reason about uncertainty in the way described in these volumes. The experiments provide a descriptive view of man's attitudes: de Finetti's approach is normative. To spend too much time on description is unwise when a normative approach exists, for it is like asking people's opinion of $2+2$, obtaining an average of $4 \cdot 31$ and announcing this to be the sum. It would be better to teach them arithmetic. I hope that this book will divert psychologists' attentions away from descriptions to the important problem, ably discussed in this book, of how to teach people to assess probabilities.

Mathematicians will find much of interest. (Let me hasten to add that some people may approach the book with fear because of the amount of mathematics it contains. They need not worry. Much of the material is accessible with no mathematical skill: yet more needs only a sympathetic appreciation of notation. Even the more mathematical passages use mathematics in a sparse and yet highly efficient way. Mathematics is always the servant – never the master (see Section 1.9.1).) Nevertheless, the mathematician will appreciate the power and elegance of the notation and, in particular, the discussion of finite additivity. He will be challenged by the observation that 'mathematics is an instrument which should conform itself strictly to the exigencies of the field in which it is to be applied'. He will enjoy the new light shed on the calculus of probabilities.

Physicists have long used probabilistic notions in their understanding of the world, especially at the basic, elementary-particle level. Here we have a serious attempt to connect their use of uncertainty with the idea as used outside physics.

Statisticians are the group I can speak about with greatest confidence. They have tended to adopt a view of probability which is based on frequency considerations and is too narrow for many applications. They have therefore been compelled to introduce artificial ideas, like confidence intervals, to describe the uncertainties they need to use. The so-called Bayesian approach has recently made some significant impression, but de Finetti's ideas go further still in replacing frequency concepts entirely – using his notion of exchangeability – and presenting an integrated view of statistics based on a single concept of uncertainty. A consequence of this is that the range of possible applications of statistics is enormously widened so that we can deal with phenomena other than those of a repeatable nature.

There are many other groups of people one would like to see reading these volumes. Operational research workers are continually trying to express ideas to management that involve uncertainty: they should do it using the concepts contained therein. One would like (is it a vain hope?) to see politicians with a sensible approach to uncertainty – what a blessing it would be if they could appreciate the difference between prediction and prevision (p. 60).

The book should therefore be of interest to many people. As the author says (p. 12) 'it is … an attempt to view, in a unified fashion, a group of topics which are in general considered separately, each by specialists in a single field, paying little or no attention to what is being done in other fields.'

The book is not a text on probability in the ordinary sense and would probably not be useful as a basis for a course of lectures. It would, however, be suitable for a graduate seminar wherein sections of it were discussed and analysed. Which sections were used would depend on the type of graduates, but with the continuing emphasis on unity, it would be valuable in bringing different disciplines together. No university should ignore the book.

It would be presumptuous of *me* to say how *you* should read the two volumes but a few words may help your appreciation. Firstly, do not approach it with preconceived ideas about probability. I address this remark particularly to statisticians, who can so easily interpret a formula or a phrase in a way that they have been used to, when de Finetti means something different. Let the author speak for himself. Secondly, the book does not yield to a superficial reading. The author has words of wisdom to say about many things and the wisdom often only appears after reflection. Rather, dip into parts of the book and read those carefully. Hopefully you will be stimulated to read the whole.

Thirdly, the style is refreshing – the translators have cleverly used the phrase 'a whimsical fashion' (Section 1.3.3) – so that every now and again delightful ideas spring to view; the idea that we shall all be Bayesian by 2020, or how-to play the football pools. But, as I said, this is a book about life.

November 1973

University College London,
D.V. Lindley

Preface by Adrian Smith

I became a postgraduate student of statistics at University College London in 1968, soon after Dennis Lindley had moved there to become the head of the department. He was, at that time, one of the very few academic statisticians committed to the so-called Bayesian approach to the subject. While I was a postgraduate, Lindley several times mentioned to me that his American colleague and fellow Bayesian, L.J. Savage, had encouraged him, and indeed anyone interested in the subjectivist approach to Bayesian statistics, to read the works of the Italian probabilist, actuary and philosopher, Bruno de Finetti.

But there was a problem for most of us at that time. Very little of his work had been translated into English and his 1970 magnum opus, the two-volume *Teoria Delle Probabilitá*, was only available in Italian. The thought of struggling through several hundred pages of dense and difficult writing with the aid of a dictionary was simply too daunting.

In 1971, I left University College London to take up an academic post at the Mathematics Institute in the University of Oxford. Early in 1972, an Italian group theorist called Antonio Machí came to spend a year at the Institute. We became friends and at some stage I mentioned my interest in de Finetti and the frustrations of trying to get to grips with the *Teoria Delle Probabilita*. Antonio immediately suggested that we work together on translating the two-volume work into English. Two years later, after many exchanges between Oxford and Rome, the first Wiley English edition appeared, with a Foreword by Dennis Lindley, with whom I subsequently gave a series of lectures in London to draw the attention of the wider statistics community to the importance of de Finetti's ideas.

There was growing interest in Bayesian ideas throughout the 1970s, but it was still very much a minority view among academic statisticians. The first attempt by some of us to organize a specifically Bayesian international conference in 1978, the first of what were to become the four-yearly Valencia Conferences, attracted around eighty participants. However, by the time we reached the ninth such meeting in 2011, the attendance had grown tenfold and Bayesian thinking had become a significant and influential feature of the statistical landscape.

De Finetti predicts in these volumes that we shall all be Bayesians by 2020. There is still some way to go, but if it proves to be so it will be due in no small measure to the influence of these wonderful volumes.

Adrian Smith

Preface by Bruno de Finetti

Is it possible that in just a few lines I can achieve what I failed to achieve in my many books and articles? Surely not. Nevertheless, this preface affords me the opportunity, and I shall make the attempt. It may be that misunderstandings which persist in the face of refutations dispersed or scattered over some hundreds of pages can be resolved once and for all if all the arguments are pre-emptively piled up against them.

My thesis, paradoxically, and a little provocatively, but nonetheless genuinely, is simply this:

PROBABILITY DOES NOT EXIST

The abandonment of superstitious beliefs about the existence of Phlogiston, the Cosmic Ether, Absolute Space and Time, …, or Fairies and Witches, was an essential step along the road to scientific thinking. Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading misconception, an illusory attempt to exteriorize or materialize our true probabilistic beliefs.

In investigating the reasonableness of our own modes of thought and behaviour under uncertainty, all we require, and all that we are reasonably entitled to, is consistency among these beliefs, and their reasonable relation to any kind of relevant objective data ('relevant' in as much as subjectively deemed to be so). This is Probability Theory. In its mathematical formulation we have the Calculus of Probability, with all its important off-shoots and related theories like Statistics, Decision Theory, Games Theory, Operations Research and so on.

This point of view is not bound up with any particular philosophical position, nor is it incompatible with any such. It is strictly *reductionist* in a methodological sense, in order to avoid becoming embroiled in philosophical controversy.

Probabilistic reasoning—always to be understood as subjective—merely stems from our being uncertain about something. It makes no difference whether the uncertainty relates to an unforseeable future, or to an unnoticed past, or to a past doubtfully reported or forgotten; it may even relate to something more or less knowable (by means of a computation, a logical deduction, etc.) but for which we are not willing or able to make the effort; and so on.

Moreover, probabilistic reasoning is completely unrelated to general philosophical controversies, such as Determinism versus Indeterminism, Realism versus Solipsism—including the question of whether the world 'exists', or is simply the scenery of 'my' solipsistic dream. As far as Determinism and Indeterminism are concerned, we note

that, in the context of gas theory or heat diffusion and transmission, whether one interprets the underlying process as being random or strictly deterministic makes no difference to one's probabilistic opinion. A similar situation would arise if one were faced with forecasting the digits in a table of numbers; it makes no difference whether the numbers are random, or are some segment—for example, the 2001st to the 3000th digits—of the decimal expansion of π (which is not 'random' at all, but certain; possibly available in tables and, in principle, computable by you).

The only relevant thing is uncertainty—the extent of our own knowledge and ignorance. The actual fact of whether or not the events considered are in some sense *determined*, or known by other people, and so on, is of no consequence.

The numerous, different, opposed attempts to put forward particular points of view which, in the opinion of their supporters, would endow Probability Theory with a 'nobler' status, or a 'more scientific' character, or 'firmer' philosophical or logical foundations, have only served to generate confusion and obscurity, and to provoke well-known polemics and disagreements—even between supporters of essentially the same framework.

The main points of view that have been put forward are as follows.

The *classical* view, based on physical considerations of symmetry, in which one should be *obliged* to give the same probability to such 'symmetric' cases. But which symmetry? And, in any case, why? The original sentence becomes meaningful if reversed: the symmetry is probabilistically significant, in someone's opinion, if it leads him to assign the same probabilities to such events.

The *logical* view is similar, but much more superficial and irresponsible inasmuch as it is based on similarities or symmetries which no longer derive from the facts and their actual properties, but merely from the sentences which describe them, and from their formal structure or language.

The *frequentist* (or *statistical*) view presupposes that one accepts the classical view, in that it considers an *event* as a class of *individual events*, the latter being 'trials' of the former. The individual events not only have to be 'equally probable', but also 'stochastically independent' … (these notions when applied to individual events are virtually impossible to define or explain in terms of the frequentist interpretation). In this case, also, it is straightforward, by means of the subjective approach, to obtain, under the appropriate conditions, in a perfectly valid manner, the result aimed at (but unattainable) in the statistical formulation. It suffices to make use of the notion of exchangeability. The result, which acts as a bridge connecting this new approach with the old, has been referred to by the objectivists as 'de Finetti's representation theorem'.

It follows that all the three proposed definitions of 'objective' probability, although useless *per se*, turn out to be useful and good as valid auxiliary devices when included as such in the subjectivistic theory.

The above-mentioned 'representation theorem', together with every other more or less original result in my conception of probability theory, should not be considered as a discovery (in the sense of being the outcome of advanced research). Everything is essentially the fruit of a thorough examination of the subject matter, carried out in an unprejudiced manner, with the aim of rooting out nonsense.

And probably there is nothing new; apart, perhaps, from the systematic and constant concentration on the unity of the whole, avoiding piecemeal tinkering about, which is inconsistent with the whole; this yields, in itself, something new.

Something that may strike the reader as new is the radical nature of certain of my theses, and of the form in which they are presented. This does not stem from any deliberate attempt at radicalism, but is a natural consequence of my abandoning the reverential awe which sometimes survives in people who at one time embraced the objectivistic theories prior to their conversion (which hardly ever leaves them free of some residual).

It would be impossible, even if space permitted, to trace back the possible development of my ideas, and their relationships with more or less similar positions held by other authors, both past and present. A brief survey is better than nothing, however (even though there is an inevitable arbitrariness in the selection of names to be mentioned).

I am convinced that my basic ideas go back to the years of High School as a result of my preference for the British philosophers Locke, Berkeley and, above all, Hume! I do not know to what extent the Italian school textbooks and my own interpretations were valid: I believe that my work based on exchangeability corresponds to Hume's ideas, but some other scholars do not agree. I was also favourably impressed, a few years later, by the ideas of Pragmatism, and the related notions of operational definitions in Physics. I particularly liked the Pragmatism of Giovanni Vailati—who somehow 'Italianized' James and Peirce—and, as for operationalism, I was very much struck by Einstein's relativity of 'simultaneity', and by Mach and (later) Bridgman.

As far as Probability is concerned, the first book I encountered was that of Czuber. (Before 1950—my first visit to the USA—I did not know any English, but only German and French.) For two or three years (before and after the 'Laurea' in Mathematics, and some application of probability to research on Mendelian heredity), I attempted to find valid foundations for all the theories mentioned, and I reached the conclusion that the classical and frequentist theories admitted no sensible foundation, whereas the subjectivistic one was fully justified on a normative–behaviouristic basis. I had some indirect knowledge of De Morgan, and found that some of Keynes' ideas were in partial agreement with mine; some years later I was informed of the similar approach that had been adopted by F. P. Ramsey.

Independent ideas, which were more or less similar, were put forward later by Harold Jeffreys, B. O. Koopman, and I. J. Good (with some beautiful new discussion which illustrated the totally illusory nature of the so-called *objective* definitions of probability). I could add to this list the name of Rudolf Carnap, but this would be not altogether proper in the light of his (to me strange) superposition of the idea of a logical framework onto his own vivid, subjective behaviouristic interpretation. (Richard Jeffreys, in publishing Carnap's posthumous works, seems convinced of his underlying subjectivism.) A singular position is occupied by Robert Schlaifer, who arrived at the subjectivistic approach directly and with impressive freshness and originality, with little knowledge of previous work in the field. A similar thing, although in a different sense, may be said of George Pólya, who discussed *plausible reasoning* in mathematics in the sense of the probability (subjective, of course) of a supposed theorem being true, given the state of mind of the mathematician, and its (Bayesian) modification when new information or ideas appear. The following statement of his is most remarkable: 'It seems to me more philosophical to consider the general idea of *plausible reasoning* instead of its isolated particular cases' like *inductive* (and analogical) *reasoning*. (There have been so many vain attempts to build a theory of induction without beliefs—like a theory of elasticity without matter.)

A very special mention must be reserved, however, for Leonard J. Savage and Dennis V. Lindley, who escaped from the objectivistic school, after having grown up in it, by a gradual discovery of its inconsistencies, and through a comparison of its ambiguities with the clarity of the subjectivistic theory, and the latter's suitability for every kind of practical or theoretical problem. I have often had the opportunity of profitable exchanges of ideas with them, and, in the case of Savage, of actual collaboration, I wrote briefly of Savage's invaluable contributions as a dedication to my book *Probability, Induction and Statistics*, which appeared a few months after his sudden and premature death.

One should note, however, that, even with such close colleagues, agreement ought not to be absolute, on every detail. For example, not all agree with the rejection of countable-additivity.

Finally, having mentioned several of the authors who are more or less connected with the subjectivistic (and Bayesian) point of view, I feel an obligation to recall three great men—the first two, unfortunately, no longer with us—who, although they all shared an opposed view about our common subject, were always willing to discuss, and were extraordinarily friendly and helpful on every occasion. I refer to Guido Castelnuovo, Maurice Fréchet and Jerzy Neyman.

Rome, 16 July 1973 *Bruno de Finetti*

Translators' Preface

In preparing this English translation, we were concerned to achieve two things: first of all, and most importantly, to translate as accurately as possible the closely argued *content* of the book; secondly, to convey something of the flavour of the author's idiosyncratic *style*; the sense of the painstaking struggle for understanding that runs through the Italian original.

Certain of Professor de Finetti's works have already appeared in English, the principal references being Kyburg and Smokler's *Studies in Subjective Probability* (Wiley, 1964), and the author's *Probability, Induction and Statistics* (Wiley, 1972). For the purpose of comparison—and to avoid any possible confusion—we include the following preliminary notes on the terminological and notational usage that we have adopted.

In common with the above-mentioned translations, we use the word *coherent* when referring to degrees of belief which satisfy certain 'consistency' conditions, *random quantity* in place of the more usual 'random variable', and *exchangeable*, rather than 'equivalent' or 'symmetric'.

We part company with previous translations, however, in our treatment of the concept corresponding to what is usually called 'mathematical expectation'. In Kyburg's translation of de Finetti's monograph 'La Prévision: ses lois logiques, ses sources subjectives' (see Kyburg and Smokler, pp. 93–158), the corresponding word becomes 'foresight'. We shall use the word *prevision*. A discussion of the reasons for this choice is given more fully at the appropriate place in the text (Chapter 1, 10.3) but let us note straightaway that the symbol **P** now very conveniently represents both *probability* and *prevision*, and greatly facilitates their unified treatment as linear operators.

Readers who are familiar with the Italian original will realize that on occasions we have opted for a rather free style of translation; we did so, in fact, whenever we felt this to be the best way of achieving our stated aims. Throughout, however, we have been mindful of the 'misunderstandings' referred to by the author in his Preface, and we can but hope that our translation does nothing to add to these.

Finally, we should like to express our gratitude to Professor de Finetti, who read through our translation and made many helpful suggestions; to the editor at John Wiley & Sons for getting the project under way; and to Mrs Jennifer Etheridge for her care in typing our manuscript.

A. Machí
A. F. M. Smith