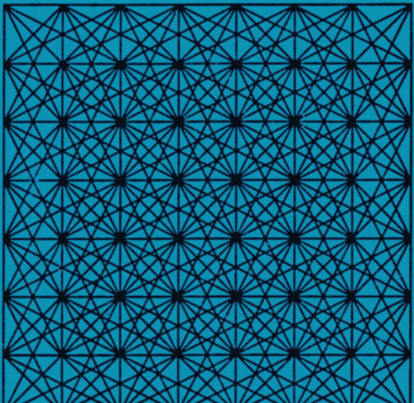
Good Thinking

The Foundations of Probability and Its Applications

I. J. Good



The University of Minnesota Press
gratefully acknowledges assistance provided
by the McKnight Foundation
for publication of this book.

Good Thinking

The Foundations of Probability and Its Applications

I. J. Good

UNIVERSITY OF MINNESOTA PRESS □ MINNEAPOLIS

Published by the University of Minnesota Press,
2037 University Avenue Southeast, Minneapolis, MN 55414
Printed in the United States of America.

Library of Congress Cataloging in Publication Data

Good, Irving John.
Good thinking.
Bibliography: p.
Includes index.
1. Probabilities. I. Title.
QA273.4.G66 519.2 81-24041
ISBN 0-8166-1141-6 AACR2
ISBN 0-8166-1142-4 (pbk.)

Contents

Acknowledgments

I am grateful to the original publishers, editors, and societies for their permission to reprint items from *Journal of the Royal Statistical Society*; *Uncertainty and Business Decisions* (Liverpool University Press); *Journal of the Institute of Actuaries*; *Science 129* (1959), 443-447; *British Journal for the Philosophy of Science*; *Logic, Methodology and Philosophy of Science* (Stanford University Press); *Journal of the American Statistical Association*; *Foundations of Statistical Inference* (Holt, Rinehart, and Winston of Canada, Ltd.); *The American Statistician* (American Statistical Association); (1) *PSA 1972*, (2) *Foundations of Probability Theory, Statistical Inference, and Statistical Theories of Science*, (3) *Philosophical Foundations of Economics*, (4) *Synthese* (respectively, © 1974, © 1976, © 1981, and © 1975, D. Reidel Publishing Company); *Machine Intelligence 8* (edited by Professor E. W. Elcock and Professor Donald Michie, published by Ellis Horwood Limited, Chichester); *Proceedings of the Royal Society*; *Trabajos de Estadística y de Investigación Operativa*; and *Journal of Statistical Computation and Simulation* (© Gordon and Breach); and *OMNI* for a photograph of the author. The diagram on the cover of the book shows all the possible moves on a chessboard; it appeared on the cover of *Theoria to Theory 6* (1972 July), in the *Scientific American* (June, 1979), and in *Personal Computing* (August, 1979). I am also beholden to Leslie Pendleton for her diligent typing, to Lindsay Waters and Marcia Bottoms at the University of Minnesota Press for their editorial perspicacity, to the National Institutes of Health for the partial financial support they gave me while I was writing some of the chapters, and to Donald Michie for suggesting the title *Good Thinking*.

This page intentionally left blank

Introduction

This is a book about applicable philosophy, and most of the articles contain all four of the ingredients philosophy, probability, statistics, and mathematics.

Some people believe that clear reasoning about many important practical and philosophical questions is impossible except in terms of probability. This belief has permeated my writings in many areas, such as rational decisions, statistics, randomness, operational research, induction, explanation, information, evidence, corroboration or weight of evidence, surprise, causality, measurement of knowledge, computation, mathematical discovery, artificial intelligence, chess, complexity, and the nature of probability itself. This book contains a selection of my more philosophical and less mathematical articles on most of these topics.

To produce a book of manageable size it has been necessary to exclude many of my babies and even to shrink a few of those included by replacing some of their parts with ellipses. (*Additions* are placed between pairs of brackets, and a few minor improvements in style have been made unobstrusively.) To compensate for the omissions, a few omitted articles will be mentioned in this introduction, and a bibliography of my publications has been included at the end of the book. (My work is cited by the numbers in this bibliography, for example, #26; a boldtype number is used when at least a part of an article is included in the present collection.)

About 85% of the book is based on invited lectures and represents my work on a variety of topics, but with a unified, rational approach. Because the approach is unified, there is inevitably some repetition. I have not deleted all of it because it seemed advisable to keep the articles fairly self-contained so that they can be read in any order. These articles, and some background information, will be briefly surveyed in this introduction, and other quick overviews can be obtained from the Contents and from the Index (which also lightly covers the bibliography of my publications).

Some readers interested in history might like to know what influences helped to form my views, so I will now mention some of my own background, leaving aside early childhood influences. My first introduction to probability, in high school, was from an enjoyable chapter in Hall and Knight's elementary textbook called *Higher Algebra.* I then found the writings on probability by J. M. Keynes and by F. P. Ramsey in the Hendon public library in North West London. I recall laboriously reading part of Keynes while in a queue for the Golders Green Hippodrome. My basic philosophy of probability is a compromise between the views of Keynes and Ramsey. In other words, I consider (i) that, even if physical probability exists, which I think is probable, it can be measured only with the aid of subjective (personal) probability and (ii) that it is not always possible to judge whether one subjective probability is greater than another, in other words, that subjective probabilities are only "partially ordered." This approach comes to the same as assuming "upper and lower" subjective probabilities, that is, assuming that a probability is interval valued. But it is often convenient to approximate the interval by a point.

I was later influenced by other writers, especially by Harold Jeffreys, though he regards sharp or numerical logical probabilities (credibilities) as primary. (See #1160, which is not included here, for more concerning Jeffreys's influence.) I did not know of the work of Bruno de Finetti until Jimmie Savage revealed it to speakers of English, which was after #13 was published. De Finetti's position is more radical than mine; in fact, he expresses his position paradoxically by saying that probability does not exist.

My conviction that rationality, rather than fashion, was a better approach to truth, and to many human affairs, was encouraged by some of the writings of Bertrand Russell and H. G. Wells. This conviction was unwittingly reinforced by Hitler, who at that time was dragging the world into war by making irrationality fashionable in Germany.

During Hitler's war, I was a cryptanalyst at the Government Code and Cypher School (GC & CS), also known as the Golf Club and Chess Society, in Bletchley, England, and I was the main statistical assistant in turn to Alan Turing (best known for the "Turing Machine"), Hugh Alexander (three times British chess champion), and Max Newman (later president of the London Mathematical Society). In this cryptanalysis the concepts of probability and weight of evidence, combined with electromagnetic and electronic machinery, helped greatly in work that eventually led to the destruction of Hitler.

Soon after the war I wrote *Probability and the Weighing of Evidence* (#13), though it was not published until January 1950. It discussed the theory of subjective or personal probability and the simple but powerful concept of Bayes factors and their logarithms or weights of evidence that I learned from Jeffreys and Turing. (For a definition, see weight of evidence in the Index of the present book.) I did not know until much later that the great philosopher of science C. S. Peirce had proposed the name "weight of evidence" in nearly the same technical sense in 1878, and I believe the discrepancy could be attributed to a mistake that Peirce made. (See #1382.) Turing, who called weight of evidence "decibannage," suggested in 1940 or 1941 that certain kinds of experiments could be evaluated by their expected weights of evidence (per observation) $\Sigma p_i \log(p_i/q_i)$, where p_i and q_i denote multinomial probabilities. Thus, he used weight of evidence as a quasiutility or epistemic utility. This was the first important practical use of expected weight of evidence in ordinary statistics, as far as I know, though Gibbs had used an algebraic expression of this form in statistical mechanics. (See equation 292 of his famous paper on the equilibrium of heterogeneous substances.) The extension to continuous distributions is obvious. Following Turing, I made good use of weight of evidence and its expectation during the war and have discussed it in about forty publications. The concept of weight of evidence completely captures that of the degree to which evidence corroborates a hypothesis. I think it is almost as much an intelligence amplifier as the concept of probability itself, and I hope it will soon be taught to all medical students, law students, and schoolchildren.

In his fundamental work on information theory in 1948, Shannon used the expression $-\Sigma p_i \log p_i$ and called it "entropy." His expression $\Sigma p_{ij} \log[p_{ij}/(p_{i.}p_{.j})]$ for mutual information is a special case of expected weight of evidence. Shannon's work made the words "entropy" and "information" especially fashionable, so that expected weight of evidence is now also called "discrimination information," or "the entropy of one distribution with respect to another," or "relative entropy," or "cross entropy," or "dinegentropy," or "dientropy," or even simply "entropy," though the last name is misleading. Among the many other statisticians who later emphasized this concept are S. Kullback, also an excryptanalyst, and Myron Tribus. The concept is used in several places in the following pages.

Statistical techniques and philosophical arguments that depend (explicitly) on subjective or logical probability—as, for example, in works of Laplace, Harold Jeffreys, Rudolf Carnap, Jimmy Savage, Bruno de Finetti, Richard Jeffrey, Roger Rosenkrantz, and myself in #13—are often called Bayesian or neo-Bayesian. The main controversy in the foundations of statistics is whether such techniques should be used. In 1950 the Bayesian approach was unpopular, largely because of Fisher's influence, and most statisticians were so far to my "right" that I was regarded as an extremist. Since then, a better description of my position would be "centrist" or "eclectic," or perhaps "left center" or advocating a Bayes/non-Bayes compromise. It seems to me that such a compromise is forced on anyone who regards nontautological probabilities as only interval valued because the degree of Bayesianity depends on the narrowness of the intervals. My basic position has not changed in the last thirty-five years, but the centroid of the statistical profession has moved closer to my position and many statisticians are now to my "left."

One of the themes of #13 was that the fundamental principles of probability, or of any scientific theory in almost finished form, can be categorized as "axioms," "rules," and "suggestions." The axioms are the basis of an abstract or mathematical theory; the rules show how the abstract theory can be applied; and suggestions, of which there can be an unlimited number, are devices for eliciting your own or others' judgments. Some examples of axioms, rules, and suggestions are also given in the present book.

#13 was less concerned with establishing the axiomatic systems for subjective probabilities by prior arguments than with describing the theory in the simplest possible terms, so that, while remaining realistic, it could be readily understood by any intelligent layperson. I wrote the book succinctly, perhaps overly so, in the hope that it would be read right through, although I knew that large books look more impressive.

With this somewhat perfunctory background let's rapidly survey the contents of the present book. It is divided into five closely related parts. (I had planned a sixth part consisting of nineteen of my book reviews, but the reviews are among the dropped babies. I shudder to think of readers spending hours in libraries hunting down these reviews, so, to save these keen readers some work, here are my gradings of these reviews (not of the books reviewed) on the scale 0 to 20. #761: 20/20; ##191, 294, 516, 697, 844, 956, 958, 1217, 1221, and 1235: 18/20; ##541A and 754: 17/20; #162: 16/20; ##115 and 875 with 948: 15/20; ##75 and 156: 14/20.)

The first part is about rationality, which can be briefly described as the maximization of the "mathematical expectation" of "utility" (value). #13 did not much emphasize utilities, but they were mentioned nine times. Article #26 (Chapter 1 of Part I of this volume) was first delivered at a 1951 weekend conference of the Royal Statistical Society in Cambridge. It shows how very easy it is to extend the basic philosophy of #13 so as to include utilities and decisions. It also suggests a way of rewarding consultants who estimate probabilities—such as racing and stock-market tipsters, forecasters of the weather, guessers of Zener cards by ESP, and examinees in multiple-choice examinations—so as to encourage them to make accurate estimates. The idea has popular appeal and was reported in the Cambridge *Daily News* on September 25, 1951. Many years later I found that much the same idea had been proposed independently a year earlier by Brier, a meteorologist, but with a different (quadratic) fee. My logarithmic fee is related to weight of evidence and its expectation, and, when there are more than two alternatives, it is the only fee that does not depend on the probability estimates of events that do not later occur. (This fact apparently was first observed by Andrew Gleason.) The theory is improved somewhat in #43 (Chapter 16). The topic was taken up by John McCarthy (1956) and by Jacob Marschak (1959), who said that the topic opened up an entirely new field of economics, the economics of information. There is now a literature of the order of a hundred articles on the topic, including my #690A, which was based on a lecture invited by Marschak for the second world conference on econometrics. #690A is somewhat technical and is not included here, but I think it deserves mention.

The logarithmic scoring system resembles the score in a time-guessing game that I invented at the age of thirteen when I was out walking with my father in Princes Risborough, Buckinghamshire. The score for an error of t minutes, rounded up to an exact multiple of a minute, is $-\log t$. When there are two players whose errors are t_1 and t_2, the "payment" is $\log(t_1/t_2)$. The logarithms of 1, 2, 3, . . . , 10, to base $10^{1/40}$, are conveniently close to whole numbers, which, when you think about it, is why there are twelve semitones in an octave. For probability estimators in our less sporting application, the "payment" is $\log(p/q)$, where p and q are the probability estimates of two consultants, rounded up to exact multiples of say 1/1000 so as not to punish too harshly people who carelessly estimate a nontautological probability as zero. If you hired only one consultant, you could define q as your *own* probability of the event that occurs. Then, the expected payoff is a "trientropy" of the form $\Sigma\pi_i\log(p_i/q_i)$, where the π_i is a "true" probability, and it pays the consultant in expectation to choose his or her p's equal to the π's. If the consultant agrees with your estimates, he or she earns nothing, except perhaps some constant fee, because $\log 1 = 0$.

One way to interpret the logarithmic payoff is that it rewards most highly the consultant who is the least surprised by the outcome, where surprise is also measured logarithmically. We return to this topic when reviewing Part IV.

Another subject discussed in #26 is that of hierarchies of probabilities. For example, the probability of a statement containing probabilities of type 1 is itself of type 2. This hierarchical Bayesian approach is developed in several of my statistical articles, and I believe it is an essential "psychological" technique in the application of neo-Bayesian methods to problems containing many parameters, although Savage (1954, p. 58) summarily dismisses it. It is one example of the influence of philosophy on statistics. Some history of the hierarchical Bayesian technique is surveyed in #1230 (Chapter 9 of this volume).

An article not included in this book is #290 because its ideas are mostly covered by later articles. In addition, it argues that the concept of Bayesian rationality should be understood by the managers of large firms and that this would greatly increase the value of operational research. Bayesian rationality is now widely taught in college courses in departments of economics, business, engineering, statistics, political science, and psychology, but this was not true twenty years ago. The article incidentally contains two natural measures of the precision of a probability judgment.

#679 (Chapter 2) is an attempt to collect together and to codify the basic foundations of rationality, as I understand it, into twenty-seven brief "priggish principles." Axiomatic systems for probability and rationality are esteemed by mathematicians because of their succinctness and sharpness, and I thought it useful to attempt a codification of my own views that goes beyond a formal axiomatic system. Note that "Type II rationality," in principle number 6, again leads naturally to a Bayes/non-Bayes compromise, or synthesis, as well as to other compromises, such as between subjectivism and credibilism. #765 (Chapter 3) is another attempt at classification, this time of the varieties of philosophies that have been called Bayesian. The number of categories turns out to exceed the number of members of the American Statistical Association.

#838 (Chapter 4) describes some of the Bayesian influence in statistics and also some of the tricks used in non-Bayesian techniques to cover up subjectivity. This article includes an elaboration of the codification **#679**.

Part II deals mainly with probability. **#85A** (Chapter 5), half of which is omitted, asks whether probability or statistics is historically or logically primary and comes up with an eggs-and-hens answer. It contains some early criticisms of some conventional statistical concepts. **#182** (Chapter 6) discusses kinds of probability, again with some historical background. **#230** (Chapter 7), originally a 1960 lecture, describes the theory of #13 as a "black box" theory and shows how it can be used to generate an axiomatic system for upper and lower subjective or logical probabilities. The resulting axiomatic system is somewhat related to work by B. O. Koopman (1940a, b). It also turned out to be related to C. A. B. Smith (1961), and this was not surprising since Smith's aim was to produce a prior justification for the philosophy of partially ordered subjective probabilities.

#815 (Chapter 8) is a discussion of randomness that was delivered in a symposium on randomness dedicated to the memory of Jimmie Savage. The main topic of the article is the place of randomization in statistics. An omitted page gives a clear intuitive reason, in terms of "generalized decimals," for believing in the mathematical "existence" of infinite random sequences, though no such sequence can be explicitly defined.

I have already mentioned **#1230**, which is included as Chapter 9.

In #13 it had been mentioned that, if we wish to talk about the probabilities of mathematical theorems, we need to make a small adjustment to the usual axioms of probability. The same point occurs when we say that the output of a computer gives information. The most standard notation, among mathematicians and statisticians, for the probability of A given B is $P(A|B)$, and a familiar beginner's error is to forget about B when discussing the probability of A. But even with A and B both mentioned, one's subjective probability $P(A|B)$ can vary in time just by thinking or by computing. This is especially clear in a game of "perfect information" such as chess. I have called such probabilities "evolving," "sliding," or "dynamic." Dynamic probability is the really fundamental variety of subjective probability. This may be true even in the limiting case of ordinary logic, a case briefly considered by Henri Poincaré. The statement "all statements by Cretans are false," itself asserted by a Cretan, oscillates rapidly between truth and falsehood as one considers its implications until one conjectures that the word "other" needs to be inserted into the statement. The concept of dynamic or evolving probability *must* be invoked, as in #599, to resolve an important paradox in the philosophy of science that had been brought to my attention by Joseph Agassi. I have reluctantly omitted #599, which is one of my favorites, because it is largely superseded by **#1000**, Chapter 23 in Part V, and because **#938**, Chapter 10 in Part II, deals in detail with dynamic probability.

For further surveys of degrees of belief and of axiomatic systems for probability, see my encyclopedia articles ##1300 and 1313, neither of which is included in this book. I wish there had been room for them.

Part III deals mainly with corroboration, hypothesis testing, and simplicity. **##518** and **600** (Chapter 11 and 12) resolve "Hempel's paradox of confirmation," in which "confirmation" means "corroboration" and can be best interpreted as weight of evidence, although this fact is not essential to the statement or to the resolution of the paradox. The somewhat surprising conclusion reached in these two short articles is that seeing a black crow does not necessarily confirm the hypothesis that all crows are black. Also an explanation is given for why this seems surprising. It is incidentally unfortunate that many philosophers have been confusing themselves by sometimes using "confirmation" to mean "probability." Bad choices of terminology often have bad effects, even though logically they shouldn't.

#603B (Chapter 13) was written in an attempt to decide whether the Titius-Bode "law" of planetary distances could be due to chance, an issue that many astronomers had decided by means of snap judgments but not always in the same direction. The Bayesian evaluation of scientific theories, in more than purely qualitative terms, has not yet been well developed, and I thought it would be interesting to begin with a fairly "numerological" law. The Titius-Bode law is not entirely numerological, and it receives some indirect support especially from a similar law for the moons of Saturn, which I called "Dabbler's law." I pointed out that Dabbler's law had some predictive value regarding the satellite Phoebe, but it "predicts" another moon (or ring?) at a distance of 4 million miles from Saturn, and this has not been observed.

The interesting question about both the Titius-Bode law and Dabbler's law is not whether they are "true," for obviously they are not, but whether they deserve an explanation. Most of **#603B** is omitted because it is long and technical and because it would, in fairness, be necessary to discuss later literature on the topic. (See Bode in the Index and Efron [1971], and Nieto [1972].) The editor had invited me to include a summary of my views on probability and induction and that is almost the only part of the article reprinted here. It is somewhat repetitive of material in other articles, but it is extremely succinct. The reader who likes some fun could refer to the original for the heated discussion.

#1234 (Chapter 14) discusses hypothesis testing using various possible tools: tail-area probabilities, Bayes factors or weights of evidence, surprise indexes, and a Bayes/non-Bayes compromise. Of course, the probability of an observation, described in complete detail, given a single hypothesis, is usually exceedingly small and *by itself* does not help us to evaluate the hypothesis, and that is why tail-area probabilities were invented.

The third part ends with **#846** (Chapter 15), which surveys the topic of corroboration. This article includes a few pages on my philosophy of probability and rationality as well as sections on complexity and on checkability. A section on "explicativity" is mostly omitted here because it is covered by #**1000**. Another omitted article that is relevant to Part III is #1330, which deals with the philosophy of the analysis of data. The reason qualitative Bayesian thinking is basic to this topic is that the analysis of data necessarily involves the implicit or explicit formulation of hypotheses, and these hypotheses can have various degrees of "kinkosity."

Part IV begins with some parts of #**43** (Chapter 16), much of what is omitted, having been covered by #**26**. Here Warren Weaver's surprise index is generalized and related to Shannon's entropy. The topic was continued in #82, which is not included. As mentioned in #82, "Perhaps the main biological function of surprise is to jar us into reconsidering the validity of some hypothesis that we had previously accepted." That article also points out the relationship between surprise and simplicity. (See also #755.) ##**43** and 82 anticipated measures of information often attributed to the eminent probabilist Rényi.

G. L. S. Shackle had proposed that business decisions were usually based on the concept of potential surprise and not on subjective probability. I argued that, since surprise can be measured in terms of subjective probability, feelings of potential surprise can help you to make probability judgments and vice versa, so that the two forms of judgment can be used to enrich one another. I did not persuade Shackle of this. I think that anyone who disagrees strongly with my view had his mind in shackles.

#77 deals with the terminology and notation of information theory, but it is not included here. In it I proposed, simultaneously with Lindley, that information in Shannon's sense could be used as a quasi-utility in the design of experiments. The idea had been suggested in 1953 by Cronbach in a technical report, but the idea of using expected weight of evidence for this purpose had been long anticipated by Turing, as I mentioned before, and comes to essentially the same thing. Fisher had even earlier used his own measure of information for the same purpose, though he never thought of it as a substitute for utility. For an account of Turing's statistical work during World War II, see #1201 which is not included in the present book.

Perhaps the term "quasiutility" should be reserved for a quantity whose expectation it is reasonable to maximize in the design of a statistical experiment. A necessary condition for a quasiutility is additivity for independent experiments. Examples of "quasiutilities" are (i) Fisher's measure of information concerning the value of a parameter, (ii) Shannon's mutual information, (iii) more generally weight of evidence, (iv) logarithmic surprise indexes, and (v) explicativity (see #**1000**).

There is a theorem which owes its existence to Abraham Wald, that a "least favorable" prior is minimax. (Readers interested in less technical subjects could skip the rest of this paragraph.) He intended "least favorable" to be interpreted in terms of utilities, but it was pointed out in ##618 and 622, which are omitted and are therefore denoted by timid type, that we can apply the theorem with the utilities replaced by quasiutilities and that this shows that the "principle of maximum entropy" and the "principle of minimum discriminability" for formulating hypotheses (Kullback, 1959; and very explicitly p. 913 of #322) and Jeffreys's "invariant prior," are all minimax procedures. This fact sheds light on both the advantages and the disadvantages of these techniques. (Maximum entropy was originally suggested by E. T. Jaynes for the generation of credibilistic priors.) A principle of minimum Fisherian information could also be tried for hypothesis formulation in the absence of a sample: it can be seen to lead, for example, to the hypothesis of independence for multivariate normal distributions, as does also the principle of maximum entropy. Similarly, the maximization of entropy leads to the hypothesis of independence for contingency tables and to that of the vanishing of interactions of various orders for multidimensional contingency tables (see #322). But, when the prior probabilities of the hypotheses are already known, or assumed, the minimax method is meaningless, so there is no principle of minimum explicativity unless the probabilities of the hypotheses are interval valued instead of being sharp.

#508 (Chapter 17) explains in detail, in terms of rationality, why it pays, at any rate in one's own judgment, to acquire new evidence when it is free, a question that was raised by A. J. Ayer at a conference of distinguished philosophers of science, none of whom had come up with the answer. But **#855** (Chapter 18) shows that, from the point of view of another person who knows more than you do, it can sometimes in expectation be to your disadvantage to acquire a small amount of new evidence.

#659 (Chapter 19) is a quick survey of my work on the topics of information, evidence, surprise, causality, explanation, and utility. Its appendix was **#679**, which constitues Chapter 2.

#814 (Chapter 20) points out that, contrary to an opinion expressed by Eddington, we should not be surprised that our galaxy is unusually large.

Part V deals with probabilistic causality and explanation. Most philosophical writings on causality interpret causality and explanation in a strictly deterministic sense, but probabilistic causality is of more practical importance (for example, when assigning blame and credit), and anyway strict causality is a special case. The only serious philosophical work on probabilistic causality that I know of, preceding my work, was by Hans Reichenbach and by Norbert Wiener. **#223B** (Chapter 21), which uses a desideratum-explicatum approach, is more ambitious than Reichenbach's work in the sense of suggesting a quantitative explicatum (in terms of weight of evidence) for the degree to which one event tends to cause another one. Wiener's work dealt with stochastic processes instead of with events. The writing of **#223B** kept me off the streets for a year of evenings and weekends. A relationship between Wiener's work and mine might well emerge from the application of my explication to regression theory, as in #1317, but this mathematical article is not included. **#1336** (Chapter 22) simplifies some of the argument of **#223B**. Although **#223B** is somewhat mathematical, I thought its omission would damage too much the picture of my work on foundations. Recently, there has been increasing interest in the topic of probabilistic causality. See, for example, Suppes (1970), Salmon (1980), and Humphreys (1980), reviewed or answered, respectively, in ##754, 1263, 1331, and 1333. See also Mayr (1961), Simon (1978), Sayre (1977), and Rosen (1978), and ##928 and 1157.

The final article is **#1000** (Chapter 23), which deals with "explicativity," a topic already mentioned in this introduction. Some of the ideas had already appeared in #599. Explicativity is a measure of the explanatory strength of a hypothesis or theory in relation to given observations, and its maximization can be regarded as a sharpening of the razor of Duns and Ockham. The explication proposed can be regarded as a compromise, or synthesis, between Bayesian and Popperian views. The article contains a brief discussion of "predictivity." A feature of **#1000** of interest in applied philosophy is that the explication for explicativity can be used in statistical problems of both estimation and significance testing. The explicatum, which again uses logarithms of probabilities, is thus supported not just by the strong prior desiderata but by the deduction of sensible results resembling those in classical statistical theory. Nevertheless, these statistical applications are somewhat technical and have been omitted from this book. The complete article was reprinted, as #1161, in the volume of essays in honor of Harold Jeffreys. That volume also contained #1160, which dealt with the contributions of Jeffreys to Bayesian statistics. Although the statistical part of **#1000** has been omitted, this article makes an appropriate conclusion to the present book because of its intimate blend of philosophy and statistics.

Part I. Bayesian Rationality