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# *Betting on theories*

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## Preface

Under what conditions does evidence confirm a scientific hypothesis? And why under those conditions only? There is an answer to these questions that is both precise and general, and which fits well with scientific practice. I allude to the Bayesian theory of confirmation. This theory represents scientists as having subjective probabilities for hypotheses, and it uses probability theory (notably Bayes' theorem) to explain when, and why, evidence confirms scientific theories.

I think Bayesian confirmation theory is correct as far as it goes and represents a great advance in the theory of confirmation. But its foundations have sometimes been seen as shaky. Can we really say that scientists have subjective probabilities for scientific theories – or even that rationality requires this? One purpose of the present book is to address this foundational issue. In Chapter 1, I defend an interpretation of subjective probability that is in the spirit of Frank Ramsey. On this interpretation, a person has subjective probabilities if the person has preferences satisfying certain conditions. In Chapters 2 and 3 I give reasons for thinking that these conditions are requirements of rationality. It follows that rational people have (not necessarily precise) subjective probabilities. In Chapter 4, I apply this general argument to science, to conclude that rational scientists have (not necessarily precise) subjective probabilities for scientific theories.

The presupposition of Bayesian confirmation theory that I have been discussing is a *synchronic* principle of rationality; it says that a rational scientist at a given time has subjective probabilities. But Bayesian confirmation theory also assumes principles about how these probabilities should be revised as new information is acquired; these are *diachronic* principles of rationality. Such principles are investigated in Chapter 5. Here

I show that the usual arguments for these diachronic principles are fallacious. Furthermore, if the arguments were right, they would legitimate these principles in contexts where they are really indefensible. I offer a more modest, and I hope more cogent, replacement for these flawed arguments.

Non-Bayesian philosophers of science have focused on the question of when it is rational to *accept* a scientific theory. Bayesians have tended to regard their notion of subjective probability as a replacement for the notion of acceptance and thus to regard talk of acceptance as a loose way of talking about subjective probabilities. Insofar as the notion of acceptance has been poorly articulated in non-Bayesian philosophy of science, this cavalier attitude is understandable. But there is a notion of acceptance that can be made clear and is not reducible to the concept of subjective probability. Confirmation theory does not provide a theory of rational acceptance in this sense. Instead, we need to employ decision theory. This is argued in Chapter 6.

In my experience, Bayesian philosophers of science tend to think that if acceptance is not reducible to subjective probability then it cannot be an important concept for philosophy of science. But this thought is mistaken. One reason is that Bayesian analyses of the history of science, which are used to argue for the correctness of Bayesian confirmation theory, themselves require a theory of acceptance. Another reason is the acknowledged role of alternative hypotheses in scientific development; accounting for this role is beyond the resources of confirmation theory and requires the theory of acceptance. A third reason is that we would like to explain why gathering evidence contributes to scientific goals, and acceptance theory provides a better account of this than is possible using confirmation theory alone. These claims are defended in Chapter 7.

A decision-theoretic account of rational acceptance raises foundational questions again. We now require not only that scientists have probabilities for scientific hypotheses but also that acceptance of hypotheses have consequences to which utilities can be assigned. Since acceptance is a cognitive act, the consequences in question can be called cognitive consequences, and their utilities can be called cognitive utilities. It needs to be shown that the required measures of cognitive utility can

meaningfully be assigned. According to the approach defended in Chapter 1, this requires showing that rational scientists have preferences among cognitive options that can be represented by a pair of probability and cognitive utility functions. Chapter 8 takes up this challenge, and answers it by providing a new representation theorem.

In the final chapter of the book, I try to show that my theory of acceptance provides a fruitful perspective on traditional questions about scientific values. Here I partially side with those who see science as aiming at truth. In fact, I show how it is possible to define a notion of verisimilitude (or closeness to the whole truth), and I argue that this is ultimately the only scientific value. But on the other hand, I differ from most realists in allowing that different scientists may, within limits, assess verisimilitude in different ways. The upshot is a position on scientific values that is sufficiently broad-minded to be consistent with van Fraassen's antirealism, but is strong enough to be inconsistent with Kuhn's conception of scientific values.

I began work on the acceptance theory in this book in the winter of 1980, when I was a graduate student at the University of Pittsburgh. That semester, seminars by both Carl Hempel and Teddy Seidenfeld introduced me to Hempel's and Levi's decision-theoretic models of rational acceptance. I later wrote my dissertation (Maher 1984) on this topic, proving the representation theorem described in Chapter 8 and developing the subjective theory of verisimilitude presented in Chapter 9. Wesley Salmon gently directed my dissertation research, other members of my dissertation committee (David Gauthier, Clark Glymour, Carl Hempel, and Nicholas Rescher) provided valuable feedback, and Teddy Seidenfeld helped enormously, especially on the representation theorem.

In 1987, I received a three-year fellowship from the Michigan Society of Fellows, a grant from the National Science Foundation, a summer stipend from the National Endowment for the Humanities, and a humanities released-time award from the University of Illinois – all to write this book. Eventually I decided that the focus of the book needed to be narrowed; hence much of the research supported by these awards has been excluded from this book, although most has been published

elsewhere. Still, without those awards, especially the Michigan fellowship, this book would never have been written.

I gave seminars based on drafts of this book at Michigan and Illinois; I thank the participants in those seminars for discussions that helped improve the book. Critiques by George Mavrodes and Wei-ming Wu are especially vivid in my mind. Brad Armendt read the book for Cambridge University Press, and his written comments were very useful to me. When I thought the book was basically finished, Allan Gibbard's (1990) book prompted me to change radically the conception of rationality I was working with; I thank my Illinois colleagues who read through Gibbard's book with me, and thank Gibbard for an illuminating e-mail correspondence. Fred Schmitt and Steven Wagner gave me stimulating written comments on the material in Chapter 6. Many others discussed aspects of the book with me, and I hope they will forgive me if I don't drag these acknowledgments out to an interminable length by attempting to list them all. I will, however, mention my wife, Janette, who not only provided domestic support but also read much of the book, talked about it with me on and off for years, and suggested improvements. I will also mention my trusty PCs and Leslie Lamport's (1986)  $\text{\LaTeX}$  software, which together turned countless drafts into beautiful pages.  $\text{\LaTeX}$  deserves to be more widely used by philosophers than it is, especially since it is free.

Chapter 5 is an improved version of my "Diachronic Rationality," *Philosophy of Science* 59 (1992). Section 7.3 is a refined version of an argument first published in "Why Scientists Gather Evidence," *British Journal for the Philosophy of Science* 41 (1990). Section 4.3, and parts of Chapter 6 and Section 9.6, appeared in "Acceptance Without Belief," *PSA 1990*, vol. 1. This material is reproduced here by permission of the publishers.