EVIDENCE AND EVOLUTION

How should the concept of evidence be understood? And how does the concept of evidence apply to the controversy about creationism as well as to work in evolutionary biology about natural selection and common ancestry? In this rich and wide-ranging book, Elliott Sober investigates general questions about probability and evidence and shows how the answers he develops to those questions apply to the specifics of evolutionary biology. Drawing on a set of fascinating examples, he analyzes whether claims about intelligent design are untestable; whether they are discredited by the fact that many adaptations are imperfect; how evidence bears on whether present species trace back to common ancestors; how hypotheses about natural selection can be tested, and many other issues. His book will interest all readers who want to understand philosophical questions about evidence and evolution, as they arise both in Darwin's work and in contemporary biological research.

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The logic behind the science

ELLIOTT SOBER

CAMBRIDGE
UNIVERSITY PRESS

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Shaftesbury Road, Cambridge CB2 8EA, United Kingdom

One Liberty Plaza, 20th Floor, New York, NY 10006, USA

477 Williamstown Road, Port Melbourne, VIC 3207, Australia

314–321, 3rd Floor, Plot 3, Splendor Forum, Jasola District Centre, New Delhi – 110025, India

103 Penang Road, #05–06/07, Visioncrest Commercial, Singapore 238467

Cambridge University Press is part of Cambridge University Press & Assessment,
a department of the University of Cambridge.

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education, learning and research at the highest international levels of excellence.

www.cambridge.org
Information on this title: www.cambridge.org/9780521871884

First published 2008

A catalogue record for this publication is available from the British Library

Library of Congress Cataloging-in-Publication data
Sober, Elliott.
Evidence and evolution : the logic behind the science / Elliott Sober.
p. cm.
Includes bibliographical references.
ISBN 978-0-521-87188-4 (hardback : alk. paper) – ISBN 978-0-521-69274-8 (pbk. : alk. paper)
1. Evolution (Biology)–Philosophy. 2. Natural selection–Philosophy. 3. Evidence.
4. Probabilities–Philosophy. I. Title.
QH360.5.S625 2008
576.8–dc22
2007051438

ISBN 978-0-521-87188-4 Hardback
ISBN 978-0-521-69274-8 Paperback

In memory of my friend Berent Enç (1938–2003)

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Preface

Biologists study living things, but what do philosophers of biology study? A cynic might say "their own navels," but I am no cynic. A better answer is that philosophers of biology, and philosophers of science generally, study science. Ours is a second-order, not a first-order, subject. In this respect, philosophy of science is similar to history and sociology of science. A difference may be found in the fact that historians and sociologists study science as it is, whereas philosophers of science study science as it ought to be. Philosophy of science is a *normative* discipline, its goal being to distinguish good science from bad, better scientific practices from worse. This evaluative endeavor may sound like the height of hubris. How dare we tell scientists what they ought to do! Science does not need philosopher kings or philosophical police. The problem with this dismissive comment is that it assumes that normative philosophy of science ignores the practice of science. In fact, philosophers of science recognize that ignoring science is a recipe for disaster. Science itself is a normative enterprise, full of directives concerning how nature ought to be studied. Biologists don't just describe living things; they constantly evaluate each other's work. Normative philosophy of science is continuous with the normative discourse that is ongoing within science itself. Discussions of these normative issues should be judged by their quality, not by the union cards that discussants happen to hold.

Pronouncements on "the scientific method" all too often give the impression that this venerable object is settled and fixed – that it is an Archimedean point from which the whole world of scientific knowledge can be levered forward. The fact of the matter is that a thorough grasp of scientific inference is a goal, not a given. Like our current understanding of nature, our present grasp of the nature of scientific inference is fragmentary and a work in progress. Scientists themselves disagree about the methods of inference that should be used, and so do statisticians and philosophers. For this reason, the first chapter of this book, on the concept of evidence, is not a report on a complacent consensus. The position I develop on what evidence means in science is controversial. It is an intervention in the long-standing disagreement between frequentists and Bayesians. I wrote this chapter for neophytes, not sophisticates. No prior understanding of probability is presupposed; I try to build from the ground up.

The methods of inference used in science take two forms. Some are entirely general, in the sense that they apply no matter what the subject matter is. These are the sorts of procedures described in texts on deductive logic and statistics. A method for estimating the average blood pressure in a population of robins is also supposed to apply to the problem of estimating the average weight in a pile of rocks. The different sciences also include methods that are narrower in scope; these methods are tailor-made to apply to a specific subject matter. For example, in evolutionary biology, a concept of parsimony has been developed that underwrites inferences about phylogenetic trees; this method is not general in its subject matter, it applies only to hypotheses about genealogies of a certain sort. The usefulness of this concept of parsimony has been controversial in evolutionary biology. When I consider the role of parsimony considerations in evolutionary biology in Chapters 3 and 4, I again will be intervening in a methodological dispute that is alive within science itself.

When scientists disagree about which of several competing inference methods they should use, it often is fairly obvious that there is a philosophical dimension to their dispute. But philosophical questions also can be raised when there is a thoroughgoing scientific consensus. No competent biologist now doubts that human beings and chimps have a common ancestor. The detailed similarities that unite these two species are overwhelming. It takes a philosopher to see a question in the background – why does detailed similarity provide evidence of common ancestry? Philosophers can ask this question without doubting the good judgment of the scientific community. They want to uncover the assumptions that need to be true for this inference from similarity to common ancestry to make sense. Analyzing inferences that seem to be obviously correct has long been a favorite project for philosophers.

Two grand ideas animate the Darwinian theory of evolution, both in the form that Darwin gave it and also in the form that modern Darwinians endorse. These are the ideas of common ancestry and natural selection. In each case, we can think of Darwinian ideas as competing with alternatives. The hypothesis that the species we now observe trace back to a common ancestor competes with the hypothesis that they originated separately and independently. The hypothesis that a trait in a species – say, the long fur that polar bears now have – evolved by natural selection competes with the hypothesis that it evolved by random genetic drift and with other hypotheses that describe other possible causes of character change and stasis. Most of Chapters 3 and 4 is devoted to understanding how the Darwinian position can be tested against its competitors. But I also spend time exploring how ideas about natural selection and common ancestry interact with each other. Biologists use information about common ancestry to test hypotheses about natural selection. And inferences about ancestry often rely on information about how various traits have evolved. The two parts of the Darwinian picture are *logically independent* of each other, but they are *methodologically* interdependent.

This book is aimed at philosophers of science and evolutionary biologists. Both tend to have little patience with creationism, so I want to explain why I devote Chapter 2 to its evaluation. I do not think that "intelligent design" is a substantive scientific theory, but I am not satisfied with the standard reasons that have been offered to explain why this is so. For example, Karl Popper's ideas on falsifiability are often used in this context, but philosophers of science have long realized that there are serious problems with Popper's solution to the demarcation problem – the problem of separating science from nonscience. In Chapter 2, I try to develop a better account of testability that clarifies what is wrong with the hypothesis of intelligent design. Another standard critique of creationism begins with the fact that many of the adaptations we find in nature are highly imperfect. It is claimed that an intelligent designer would never have produced such arrangements. I explain in Chapter 2 why I find this criticism of creationism problematic. Although it isn't true that every word of Chapter 2 matters to the material in Chapters 3 and 4, there nonetheless is a through-line from Chapter 1 to Chapters 3 and 4 that passes through Chapter 2. The Duhem–Quine thesis about scientific testing is introduced in Chapter 2 and so is the concept of a fitness function; both play important roles in what comes after.

Chapter 3 begins where Chapter 2 leaves off, by asking whether hypotheses about natural selection are in any better shape than hypotheses about intelligent design. It is no fair switching standards – setting the bar impossibly high when evaluating creationism, but lowering the bar when evolutionary hypotheses are assessed. I begin with the apparently simple problem of explaining why polar bears now have (let us assume) fur that is, on average, 10 centimeters long. Which is the more plausible explanation: that the trait evolved by natural selection or that it evolved by drift? In the first few sections of Chapter 3, I describe what needs to be known if one wishes to test these hypotheses against each other. The result is a catalog of difficulties. I then argue that the situation is transformed if we take up a different problem: Rather than trying to explain why polar bears have an average fur length of 10 centimeters, we might try to explain why bears in cold climates have longer fur than bears in warm ones. This new problem is easier to solve, and the fact that bears have a common ancestor plays a role in solving it. The rest of Chapter 3 discusses some of the methods that biologists have used to test hypotheses about natural selection; for example, they use DNA sequence data and they also infer the chronological order of the novelties that evolve in a phylogenetic tree.

Chapter 4 addresses a question I mentioned before: Why, or in what circumstances, is the similarity of two species evidence that they have a common ancestor? After developing an answer to this question that is based on the concept of evidence described in Chapter 1, I explore Darwin's idea that similarities that are useless to the organisms that have them provide stronger evidence for common ancestry than adaptive similarities do. Although Darwin's suggestion is right for a large class of adaptive similarities, it emerges that that there is a type of adaptive similarity for which the situation is precisely the reverse. I then consider how intermediate fossils and biogeographical distribution provide evidence concerning common ancestry. The chapter concludes with a discussion of two conflicting methods for inferring phylogenetic trees.

The title of this book may be a little misleading, but I hope that the subtitle corrects a misapprehension that the title may encourage. The title perhaps suggests that this is a book that describes the evidence *for* evolution. There are many good books that do this; they are works of *biology*. The book before you is not a member of that species; rather, it is a work of *philosophy*. My goal in what follows is not to pile up facts that support this or that proposition in evolutionary biology. Rather, I want to describe the tools that ought to be used to assess the evidence that bears on evolutionary ideas. Scientists, ever eager to draw conclusions about nature, reach for patterns of reasoning that seem sensible, but they rarely linger over why the procedures they use make sense. Although this book is not a work of science, I hope that scientists will find that some of the thoughts developed here are worth pondering. I also hope that the philosophers who read this book will be intrigued by the evolutionary setting of various epistemological problems.

Acknowledgements

I have been lucky in my collaborators, both philosophical and biological. Some of these coauthors will find that some of the ideas in this book are drawn from papers we have written together (citations indicate where the extractions and insertions occurred); others will find a connection to work we have done together that is less direct, but I hope they will see that it is tangible nonetheless. This book would be very different or would not exist at all (depending on how you define "the same book"), had it not been for my interactions with these talented people: Martin Barrett, Ellery Eells (whom I miss very much), Branden Fitelson, Malcolm Forster, Christopher Hitchcock, Christopher Lang, Richard Lewontin, Gregory Mougin, Steven Orzack, Larry Shapiro, Mike Steel, Christopher Stephens, Karen Strier, and David Sloan Wilson.

I also have been lucky that many philosophers and biologists read parts of this book and reacted with criticisms and suggestions. Some even read the whole thing. Let me mention first the dauntless souls who plowed through the entire manuscript and gave me valuable comments: Martin Barrett, Juan Comesaña, James Crow, Malcolm Forster, Thomas Hansen, Daniel Hausman, Steven Leeds, Richard Lewontin, Peter Vranas, and Nigel Yoccoz. They read, as far as I know, of their own free will. I'm not sure I can say the same of the students who took seminars with me in which the manuscript was discussed, but their comments have been no less helpful. My thanks to Craig Anderson, Mark Anderson, Matthew Barker, John Basl, Ed Ellesson, Joshua Filler, Patrick Forber, Michael Goldsby, Casey Helgeson, John Koolage, Matthew Kopec, Hallie Liberto, Deborah Mower, Peter Nichols, Angela Potochnik, Ken Riesman, Susanna Rinard, Michael Roche, Armin Schulz, Shannon Spaulding, Tod van Gunten, Joel Velasco, Jason Walker, and Brynn Welch. Matthew Barker and Casey Helgeson also helped me with the references, John Basl with the figures, and Joel Velasco with the corrections.

I next want to thank the people who read portions of the manuscript and sent me comments or who responded to questions that came up as I wrote; at times I felt I was being helped by an army of experts. For this I am grateful to Yuichi Amitani, Eric Bapteste, Gillian Barker, David Baum, John Beatty, Ken Burnham, David Christensen, Eric Cyr Desjardins, Ford Doolittle, John Earman, Anthony Edwards, Branden Fitelson, Steven Frank, Richard Healey, Jonathan Hodge, Dan Hartl, Edward Holmes, John Huelsenbeck, James Justus, Bret Larget, Paul Lewis, William Mann, Sandra Mitchell, John Norton, Ronald Numbers, Samir Okasha, Roderick Page, Bret Payseur, Will Provine, Alirio Rosales, Bruce Russell, Larry Shapiro, Mike Steel, Christopher Stephens, Scott Thurow, and Carl Woese.

I am deeply indebted to the Vilas Trust at the University of Wisconsin; were it not for the research support provided by my William Vilas Professorship, I would not have been able to work so long and hard on this project. I also am grateful to the Rockefeller Foundation for the month's stay I had during May–June 2006 at their research center, the Villa Serbeloni in Bellagio, Italy. This is where I wrote a draft of Chapter 1 in delightful circumstances that still make me smile each time I think of them. Finally, I want to thank Sandra Mitchell and John Norton at the University of Pittsburgh's Center for Philosophy of Science for organizing a workshop on my book manuscript that took place in March 2007; I learned a lot during this event and the book is better because of it.