

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.

ELLIOTT SOBER is Hans Reichenbach Professor and William Vilas Research Professor in the Department of Philosophy, University of Wisconsin-Madison. His many publications include *Philosophy of Biology, 2nd Edition* (1999) and *Unto Others: The Evolution and Psychology of Unselfish Behavior* (1998) which he co-authored with David Sloan Wilson.

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The logic behind the science

ELLIOTT SOBER



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

Contents

<i>List of figures</i>	<i>page</i> ix
<i>Preface</i>	xv
<i>Acknowledgements</i>	xix
1 Evidence	1
1.1 Royall’s three questions	3
1.2 The ABCs of Bayesianism	8
1.3 Likelihoodism	32
1.4 Frequentism I: Significance tests and probabilistic <i>modus tollens</i>	48
1.5 Frequentism II: Neyman–Pearson hypothesis testing	58
1.6 A test case: Stopping rules	72
1.7 Frequentism III: Model-selection theory	78
1.8 A second test case: Reasoning about coincidences	104
1.9 Concluding comments	107
2 Intelligent design	109
2.1 Darwin and intelligent design	109
2.2 Design arguments and the birth of probability theory	113
2.3 William Paley: The stone, the watch, and the eye	118
2.4 From probabilities to likelihoods	120
2.5 Epicureanism and Darwin’s theory	122
2.6 Three reactions to Paley’s design argument	125
2.7 The no-designer-worth-his-salt objection to the hypothesis of intelligent design	126
2.8 Popper’s criterion of falsifiability	129
2.9 Sharpening the likelihood argument	131
2.10 The principle of total evidence	136
2.11 Some strengths of the likelihood formulation of the design argument	139

2.12	The Achilles heel of the likelihood argument	141
2.13	Paley's stone	147
2.14	Testability	148
2.15	The relationship of the organismic design argument to Darwinism	154
2.16	The relationship of Paley's design argument to contemporary intelligent-design theory	154
2.17	The relationship of the design argument to the argument from evil	164
2.18	The design argument as an inductive sampling argument	167
2.19	Model selection and intelligent design	177
2.20	The politics and legal status of the intelligent-design hypothesis	184
2.21	Darwinism, theism, and religion	186
2.22	A prediction	188
3	Natural selection	189
3.1	Selection plus drift (SPD) versus pure drift (PD)	192
3.2	Comparing the likelihoods of the SPD and PD hypotheses	199
3.3	Filling in the blanks	201
3.4	What if the fitness function of the SPD hypothesis contains a valley?	212
3.5	Selection versus drift for a dichotomous character	215
3.6	A breath of fresh air: Change the <i>explanandum</i>	219
3.7	Model selection and unification	226
3.8	Reichenbach's principle of the common cause	230
3.9	Testing selection against drift with molecular data	235
3.10	Selection versus phylogenetic inertia	243
3.11	The chronological test	253
3.12	Concluding comments	261
4	Common ancestry	264
4.1	<i>Modus Darwin</i>	265
4.2	What the common ancestry hypothesis asserts	268
4.3	A Bayesian decomposition	275
4.4	A single character: Species matching and species mismatching	277
4.5	More than one character	294
4.6	Concluding comments on the evidential significance of similarity	310
4.7	Evidence other than similarity	314
4.8	Phylogenetic inference: The contest between likelihood and cladistic parsimony	332
	<i>Conclusion</i>	353
	<i>Bibliography</i>	368
	<i>Index</i>	385

Figures

1.1	Present evidence and its downstream consequences.	page 4
1.2	Three possible distributions of longevities.	20
1.3	A flat prior density distribution for p and the non-flat posterior density occasioned by observing one head in four tosses.	22
1.4	When the coin lands heads in five of twenty tosses, the maximum likelihood estimate of $p = Pr(\text{the coin lands heads} \mid \text{the coin is tossed})$ is $p = \frac{1}{4}$.	23
1.5	When two independent and reliable witnesses each report on whether proposition p is true, two yeses provide stronger evidence for p than one, and one yes provides stronger evidence than zero.	43
1.6	Smith and Jones differ in their inclinations to place different orders for breakfast.	44
1.7	A new set of breakfast inclinations for Smith and Jones.	44
1.8	S either has tuberculosis or does not, and you, the physician, must decide whether to accept or reject the hypothesis H that S has tuberculosis.	58
1.9	If $p = \frac{1}{4}$ is the null hypothesis and $p = \frac{3}{4}$ is the alternative to the null, and $\alpha = 0.05$ is chosen, the Neyman–Pearson theory says that the null hypothesis should be rejected if and only if twelve or more heads occur in thirty tosses of the coin.	61
1.10	Each of the observations can be represented by a data point. $L(\text{LIN})$ is the straight line that fits the data best; $L(\text{PAR})$ is the parabola that fits best.	67
1.11	$L(\text{LIN})$ is the straight line that is closest to the data; the LIN model postulates an error distribution around this line.	68
1.12	If a coin lands tails on the first two tosses and heads on the third, this outcome might be the result of two different experiments.	73

1.13	A fixed-length experiment in which a coin is tossed twenty times and a flexible-length experiment in which a coin is tossed until six heads occur.	74
1.14	The prediction problem that Akaike considered.	83
2.1	Two theistic hypotheses.	110
2.2	Creationism and theistic evolutionism.	112
2.3	If A individuals have a fitness of 0.6 and B individuals have a fitness of 0.2, no other evolutionary forces impinge, and the population is infinitely large, trait A must evolve to 100-percent representation.	157
2.4	A trait that evolves from a value of 10 to a value of 20 by the process of Darwinian gradualism in an infinite population must have a fitness function that monotonically increases from 10 to 20.	158
2.5	If there are n parts to an eye, how fit are organisms that have 0, or 1, or 2, $\dots$, or $(n - 1)$, or all n ?	159
2.6	An arch surmounted by a keystone satisfies the definition of irreducible complexity.	161
2.7	Hypothetical example of epistatic fitness relationships.	163
2.8	If we accept the bridge principle $q \approx p$, we can estimate the value of p by observing the frequency f .	173
2.9	The (One) model unifies the 20 million observations; the (20 Million) model treats each toss of each coin as a separate problem and is therefore more disunified.	183
2.10	Evolutionary biology proposes a unified model of the features that organisms have. Intelligent-design theory proposes a disunified model.	183
3.1	The pure-drift (PD) hypothesis can be thought of as a random walk on a line. The selection-plus-drift (SPD) hypothesis can be represented as a biased walk, influenced by a probabilistic attractor, the optimal phenotype.	193
3.2	Three fitness functions that have the same optimum ($\theta = 12$).	196
3.3	According to the SPD hypothesis, a population that has a given trait value at t_0 can be expected to move in the direction of O , the optimal trait value.	197
3.4	According to the PD hypothesis, a population that has a given trait value at time t_0 has that initial state as its expected value at all subsequent times, though the uncertainty surrounding that expected value increases.	198

List of figures xi

3.5	The likelihoods of the SPD and the PD hypotheses.	199
3.6	The population must evolve from its ancestral state A to its present state P .	200
3.7	The body size of ancestors of current polar bears (S) can be (a) observed, or inferred from (b) fossilized relatives (FR_1 and FR_2), or from (c) extant relatives (ER_1 and ER_2).	204
3.8	The solid curve represents Cook and Cockrell's (1978) estimate of how the amount of food (f) a ladybird obtains from eating an aphid depends on the amount of time (t) spent feeding on it.	206
3.9	Given the trait values of present-day polar bears and their relatives, the principle of parsimony provides estimates of the character states of the ancestors A_1 and A_2 .	208
3.10	If $P = a$ is the present trait value and the lineage has experienced pure drift, the maximum likelihood estimate of the trait value of the ancestor is $A = a$.	209
3.11	If $P = a$ is the present trait value and selection has been pushing the lineage towards the optimal value O , the maximum likelihood estimate of the trait value of the ancestor is not $A = a$.	210
3.12	A fitness function for the camera, cup, and compound eye that has a valley.	214
3.13	The SPD and PD hypotheses differ in the probabilities they specify for a lineage's ending in the state $P = 1$.	217
3.14	The observed fur lengths for different bear species show a downward trend and are closely clustered around an independently motivated optimality line.	220
3.15	Two scenarios in which selection causes bear lineages to evolve in the direction of an optimality line.	220
3.16	The ancestors A_1 and A_2 both have optimal trait values and their environments get colder.	221
3.17	If two descendant lineages stem from a common ancestor A and then evolve in the direction of an optimality line that has a negative slope, the expectation is that a line through D_1 and D_2 will also have a negative slope, if the trait's heritability is approximately the same in the two lineages.	222
3.18	If two descendant lineages stem from a common ancestor A and then evolve by drift, the expectation is that a line through D_1 and D_2 will have zero slope.	223

3.19	If two descendant lineages stem from a common ancestor A and then overshoot the optimality line postulated by the adaptive hypothesis, does this count as evidence favoring drift over selection?	224
3.20	Survival ratios and male care of offspring in anthropoid primates.	228
3.21	Possible explanations of patterns of variation, all for hypothetical data.	230
3.22	Although the principle of the common cause is sometimes described as saying that an “observed correlation” entails a causal connection, it is better to divide the inference into two steps.	232
3.23	Given the phylogeny, the neutral theory entails that the expected difference between 1 and 3 equals the expected difference between 2 and 3.	238
3.24	The number of nonsynonymous and synonymous differences that exist within and between three <i>Drosophila</i> species at the <i>Adh</i> locus.	241
3.25	The relative rate test and the McDonald–Kreitman test focus on different events in this tree.	242
3.26	Selection for character state 1 raises the probability that the descendant D will exhibit that character state.	247
3.27	If smoking causally contributes to lung cancer, smoking should raise the probability of lung cancer for people who have the same degree of asbestos exposure.	248
3.28	To test for phylogenetic inertia, lineages alike in their selective regimes must be compared.	249
3.29	The fact that species have common ancestors permits phylogenetic inertia and selection to each be tested by means of controlled comparisons without estimating ancestral trait values.	251
3.30	When the principle of parsimony is used to reconstruct the character states of ancestors in this phylogenetic tree, the conclusion is that trait T and trait W each evolved once.	254
3.31	The probability of the data (the trait values of tip species) is affected by the character states assigned to ancestors A_1 , A_2 , and A_3 .	256
3.32	Two reconstructions of ancestral character states.	256

List of figures

xiii

3.33	The two reconstructions of ancestral character states depicted in Figure 3.32 assign different events to branches <i>a–e</i> .	257
3.34	Two hypotheses about events in the lineage leading to land vertebrates that make different predictions about the trait combinations that land vertebrates and their relatives should exhibit.	259
4.1	Two competing genealogical hypotheses about the phylogeny of human beings (<i>H</i>), chimpanzees (<i>C</i>), and gorillas (<i>G</i>).	265
4.2	If you are a diploid organism with one chromosome pair, two of your four grandparents must have failed to make any genetic contribution to your genome.	270
4.3	Hypothesis (a), that there was a LUCA, is denied by both (b) and (c), which disagree as to how much relatedness there is among the <i>n</i> organisms and fossils (<i>S</i> ₁ , ..., <i>S</i> _{<i>n</i>}) that exist now.	271
4.4	A CA ₁ and a CA ₃ genealogy for Bacteria (B), Archaea (A), and Eukaryotes (E), both of which involve rampant lateral gene transfer in early life.	273
4.5	Three scenarios under which organisms <i>X</i> and <i>Y</i> share a trait because it was transmitted to them from an earlier organism <i>O</i> .	275
4.6	The common-ancestry and separate-ancestry hypotheses.	279
4.7	Two possible transformation series for a trait <i>T</i> that has <i>n</i> states.	285
4.8	When <i>X</i> and <i>Y</i> are scored for whether they match on a dichotomous trait <i>T</i> , there are two possible observations.	293
4.9	Three fitness functions: (a) frequency independent selection for trait <i>A</i> ; (b) drift; (c) frequency dependent selection for the majority trait.	299
4.10	Four likelihood ratios, two of which depend on the amount of time between ancestor and descendants.	304
4.11	Two character distributions for the two species <i>X</i> and <i>Y</i> .	308
4.12	Two alternatives to the hypotheses that all the traits of the taxa <i>W</i> , <i>X</i> , <i>Y</i> , and <i>Z</i> stem from a single common ancestor.	317
4.13	If the evolutionary process is gradual, the CA hypothesis predicts the existence of ancestors that had intermediate forms, regardless of the character state of the common ancestor <i>Z</i> .	319

- 4.14 Either X and Y have a common ancestor or they do not (SA). Cells represent probabilities of the form $Pr(\pm \text{intermediate} \mid \pm \text{CA})$. Gradualism is assumed. 320
- 4.15 Either X and Y have a common ancestor or they do not (SA). Cells represent the probability that we have observed an intermediate, or that we have not, conditional on CA and conditional on SA. 321
- 4.16 Observing an intermediate fossil favors CA over SA, and failing to so observe favors SA over CA, if $a > 0$ and $q < 1$. 322
- 4.17 These dated fossils form an intermediate series between the extant species X and Y . 323
- 4.18 H , C and G are each temporally extended lineages; time slices drawn at random from H and from C can be expected to be temporally more proximate to each other than time slices drawn at random from H and from G (or from C and from G). 327
- 4.19 The genealogy of X , Y , and Z is $(XY)Z$. 328
- 4.20 Each of the dichotomous traits A and B can experience two changes and each kind of change can occur on each of the two branches. 335
- 4.21 Models are more complex the larger the number of adjustable parameters they contain. 336
- 4.22 Two sites in two aligned sequences that come from different branches of a phylogenetic tree. 337
- 4.23 Four models of molecular evolution and their logical relationships. 338
- 4.24 Conjunctions of the form “tree topology & process model” containing adjustable parameters; these are nuisance parameters in the context of making inferences about topologies. 340
- 4.25 The example described in Felsenstein (1978) in which parsimony can converge on the incorrect tree as more and more data are consulted. 347
- 4.26 The tree in Figure 4.25 is in the “Felsenstein zone” when $p \gg q$. 348

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 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.

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