Evidence-Based Darwinism

Evidence and Evolution: The Logic Behind the Science
Elliott Sober
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In Elliott Sober’s classic The Nature of Selection (1984) there is not even an index entry for “evidence.” Back then the big themes included fitness, chance, and group selection. None of these gets much attention in Evidence and Evolution, which is both broader and narrower than that title indicates. A more descriptive title might be “The Nature of Support.” It is about how to arrange our epistemic affairs so that, in and out of evolutionary biology, we can best judge how well our hypotheses stack up against the evidence. Two major and, it turns out, intimately connected classes of evolutionary hypothesis—on the processes that bring about evolutionary change and the genealogical patterns that result—illustrate the more general recommendations. So too does a discussion of design theory from Paley to Behe. Unsurprisingly, design theory comes off badly; but so does a standard objection to it.

The general recommendations are the starting point. Not that Sober simply makes them and runs. On the contrary, he develops and defends them in painstaking and sometimes forbiddingly technical detail over two long chapters, one directly on evidence, the other mainly devoted to design theory. The discussion takes in Bayesianism, the law of likelihood, and a great many other topics in formal epistemology. But standing back we get something like the following. First, support should be reckoned in relative terms. When asking about probabilities, we should ask not: how probable is this hypothesis?, but rather, is this hypothesis more probable than that one? “Support is essentially contrastive,” stresses Sober. Second, support should be earned the hard way. Theories that derive successful predictions from more basic premises deserve our backing. Theories that simply pack such predictions in willy-nilly deserve our scorn. Third, support should be the privilege of hypotheses with the right kind of intellectual scaffolding. Sober takes from Pierre Duhem (1861–1916) the notion that testable predictions can be extracted from theories only when these are augmented with “auxiliary” assumptions. For Sober, the evaluative question to pose of these assumptions is not whether they are present, or whether they have been made explicit, but whether their soundness has been independently attested. If not, any apparent support is disqualified.

The evidence from animals and plants, living and fossil, renders a hypothesis of evolutionary origins by natural law more probable than a hypothesis of separate creations by intelligent design. Just look at all the imperfect designs in the living world. Surely no Designer would have botched the job so badly! The argument goes back to Darwin himself. It is still an evolutionist favorite. Yet, says Sober (agreeing with others, notably Paul Nelson), notice how it depends on an assumption whose truth is anybody’s guess—namely, that the Designer’s goals in organism design would match human goals. In their eagerness to show evolution as more probable than design, in line with Sober’s first recommendation, evolutionists have tacitly embraced an uncheckable assumption and so fallen foul of his third recommendation. A better reason for rejecting design theory lies with his second recommendation. Darwin himself complained of design theory’s vacuity, and Sober agrees. The problem with it, he writes, is “not that it makes inaccurate predictions but that it doesn’t predict much of anything.” Whatever biologists’ findings are, that is what design theory predicts. It never works in reverse, with a prediction flowing from the theory and then being checked. Darwinians have a reputation, not entirely undeserved, for evidence-free “just so” stories; but their theory can and does deliver potentially unsuccessful predictions. And that, for Sober, makes all the difference.

I have scarcely scratched the surface of these initial chapters. Each deserves a whole review in its own right. Yet they are
really just a warm-up for the two equally long chapters that follow, on hypothesis testing in evolutionary biology. These constitute most of the remainder of the book. More so than anyone before, Sober brings out the mutual dependence in some testing situations of what might be called process hypotheses and phylogeny hypotheses. I will come back to these in a moment. But it will help first of all to recall an especially fine early treatment of evolutionary theory’s testability, as we can then better appreciate Sober’s contribution. In Mary B. Williams’ 1982 paper “The Importance of Prediction Testing in Evolutionary Biology,” published in *Erkenntnis*, she took on the small but then-influential crowd of commentators (including, briefly, Karl Popper) who held that Darwinian science makes no predictions at all. She showed that, far from being absent, predictive hypotheses are central. Typically, however, they do not concern a single organism or taxon in isolation, as the critics seemed to expect they would, but multiple taxa in comparison. She gave the example of an investigation of fur length, as reported in a leading professional journal. Is length of fur adaptive, with natural selection making fur longer or shorter depending on survival needs? To answer, the investigator reasoned hypothetically. But the hypothesis did not take the form: If natural selection has determined fur length, then, for this isolated animal or group of animals, living under these conditions, the actual length will match a theoretical optimum value. Instead it took the following form: If natural selection has been operating, then animals in the colder places should have longer fur than animals in the warmer places. The prediction, in other words, bore on a pattern among several related but distinct groups, living under varied conditions.

Although he does not discuss Williams’ paper, Sober makes exactly this point, down to the furry example. His distinctive interest in the logic of empirical support makes for a couple of notable departures, however. For one thing, where Williams considered a real data set, Sober invents one—the better, no doubt, to bring out the conceptual issues that engage him, though some readers will regret the missed opportunity for contact with real science. (Incidentally, where Williams wrote about the furry coverings on bees, Sober returns us to that philosopher’s staple, polar bears.) For another, Sober brings out far more vividly than Williams did the significance of the compared taxa being related. It is the sharing of a common ancestor that makes the patterns that emerge meaningful, since, wherever the taxa wound up in morphospace, they all started from the same position. When, in the example, colder climate taxa are found to have longer fur than warmer climate taxa, natural selection is a better bet than genetic drift or some other non-selectional cause, since the pattern is probable under natural selection but improbable otherwise. However, when there is no such correlation between trait values and ambient temperature—when, say, there is a range of furiness but temperature is constant—natural selection is the less-favored hypothesis. Common ancestry in effect turns a set of taxa into a selection detector. Without common ancestry, patterns in morphospace are close to meaningless, since, from their different starting points, individual taxa may have drifted into their current states or got there under natural selection’s influence. There is no telling.

Thus does common ancestry—the common ancestry, that is, of a particular set of compared taxa—function as the predictive leaven in the lump of Darwinian theory. Here is the magical auxiliary assumption, the source of the theory’s testability, the premise out of which come predictions that discriminate between competing hypotheses. By going taxacomparative in assessing the past action of natural selection, evolutionary biologists at the same time go hypothesis-comparative, in line with Sober’s first general recommendation. However, recall the third recommendation’s requirement that auxiliaries be checked. How can we know whether the compared taxa are truly related? It is tempting to invoke what Sober refers to as “modus Darwin: similarity, ergo common ancestry,” but there are notorious difficulties with that inference. Indeed, as Sober shows, the situation is even trickier than commonly understood. It is not merely that convergent adaptive evolution can produce similarity where there is no relatedness—the real reason, Darwin thought, why good taxonomists tended to avoid adaptive characters in classifying species. Similar nonadaptive characters are not always reliable signs of relatedness, and similar adaptive characters sometimes are (and to that extent, natural selection is here relevant to an inference to common ancestry). With these and other caveats duly noted, Sober provides a magisterially thorough inventory and examination of the conditions under which the inference from similarity to common ancestry is favored inference. As he emphasizes, it is an empirical matter whether, in any particular case, the conditions—he indentifies nine—hold.

Granted Sober’s way of setting up the problem of understanding how evidence and evolution go together, his answers are convincing and pretty well exhaustive. And yet, one comes away with the feeling that there is more to be said. Some readers will be vexed by the lack of coverage of evolutionary options beyond those sanctioned by the Modern Synthesis. It is left as an exercise for the reader to work out how the marshalling of evidence in the EvoDevo laboratory fits Sober’s scheme. Even those who have no quarrel with the synthesis may wonder about the exclusive concern with the sort of studies that Sober dwells upon. Undoubtedly, a lot of empirical work on natural selection takes a taxa-comparing form. Furthermore, as Sober shows, big problems lie in wait for anyone who tries to distinguish between the effects of natural selection and genetic drift without going comparative. However, there are other ways of joining adaptive evolution and prediction testing. To take a famous example, consider Dorothy Cheney,
Robert Seyfarth, and Peter Marler’s use of experimental playback in the 1970s to assess the meaning of the alarm calls of the vervet monkeys of Amboseli National Park in Kenya. The only monkeys in the frame were the vervets in that park, yet a number of predictions were put to the test, all to do with how alarm-responding vervets should behave if different alarms convey information about different predators. Behind the predictions lay the view that, in a Darwinian world, “semantics” should be found in different degrees in nonhuman communication systems, according to the animals’ survival needs. Natural selection figures centrally here; genetic drift, if it figures at all, is far offstage.

Endless questions of interpretation have been posed about the vervet playback results. But so it is with results in evolutionary biology generally, and indeed across the sciences. The scope for disagreement—about what a theory predicts, whether a prediction is successful, and the reasons for success or failure—goes unexplored here. No book can do it all, of course; but this absence is the more striking given Sober’s gestures toward Duhem, who famously showed how consequential such flexibilities can be. One of the subheadings in his *The Aim and Structure of Physical Theory* (1914) declared: “A ‘crucial experiment’ is impossible in physics.” That was because, in Duhem’s view, it is never just one idea that gets put to the test, but a whole system. He highlighted auxiliaries in order to underscore how much room for maneuver there is when the world disappoints expectations. After an unsuccessful prediction, one can give up on one’s theory; alternatively, one can keep the theory and adjust an auxiliary—notably, in physics, about the workings of an experimental setup. Such slippery behavior is nowhere in focus in Sober’s pages. On the contrary, evolutionary theory, as he represents it, yields crucial experiment after crucial experiment. Auxiliary assumptions, far from being the means of escape from empirical accountability, turn out to guarantee that accountability. The underdetermination of theory by data—the slogan that, after Quine, attached itself to a generalized version of Duhem’s analysis—is a non-issue.

It is possible, then, to contemplate a more authentically Duhemian work than *Evidence and Evolution*. The book is nevertheless a very considerable achievement. It is remarkably free of the automatism that characterizes so much philosophy of biology—the circling back to the same old puzzles, the ringing of minor changes on familiar solutions. Sober builds on his past work; his fans will recognize a number of pet themes in my précis, including the selective Duhemianism. (For Sober’s critique of epistemic holism, see Sober 2004.) Nevertheless, he takes them in directions that have less to do with trends in the specialist literature than with his own sense of where the interesting questions lie and, connectedly, what are the proper concerns of philosophy of science. Nothing could be less fashionable than his ambition to restore testability—still so much associated with Popperian doctrines and their well-known difficulties—as not just a respectable problem but also an outstanding one, maybe even the outstanding one. Yet where Sober goes, others tend to follow. A quarter century later, *The Nature of Selection* is still a sounding board for young philosophers of biology. Let us hope that the new book will begin to play that role, stimulating the creative and big-picture responses that Sober’s example should inspire.

### References


