Intelligent Design and Probability Reasoning

Elliott Sober[1]

Department of Philosophy

University of Wisconsin, Madison

Abstract: This paper defends two theses about probabilistic reasoning. First, although *modus ponens* has a probabilistic analog, *modus tollens* does not – the fact that a hypothesis says that an observation is very improbable does not entail that the hypothesis is improbable. Second, the evidence relation is essentially *comparative*; with respect to hypotheses that confer probabilities on observation statements but do not entail them, an observation O may favor one hypothesis H_1 over another hypothesis H_2 , but O cannot be said to confirm or disconfirm H1 without such relativization. These points have serious consequences for the Intelligent Design movement. Even if evolutionary theory entailed that various complex adaptations are very improbable, that would neither disconfirm the theory nor support the hypothesis of intelligent design. For either of these conclusions to follow, an additional question must be answered: *With respect to the adaptive features that evolutionary theory allegedly says are very improbable, what is their probability of arising if they were produced by intelligent design?* This crucial question has not been addressed by the ID movement.

Philosophers schooled in the rules of deductive logic often feel that they can find their way when reasoning about probabilities by using the idea that probability arguments are approximations of deductively valid arguments. In a deductively valid argument, the premisses necessitate the conclusion; in a strong probability argument, the premisses confer a high probability on the conclusion. As a probability argument is strengthened, the probability of the conclusion, conditional on the premisses, increases; in the limit, the premisses confer a probability of unity on the conclusion. Deductive validity thus seems to be the limit case of strong probability arguments.

There is nothing wrong with this idea, though it does require refinement.[2] However, there is a distinct though closely related thought that can lead one very much astray. This is the idea that for each deductively valid form of argument, there exists a strong probabilistic argument that has roughly the same form. Granted, this principle is vague as stated, but nonetheless I think it plays a heuristic role for many philosophers (and nonphilosophers also). I want to explain why there are fundamental reasons why this heuristic is not to be trusted.

I'll begin with an example in which the principle does no harm. *Modus ponens* has the following logical form:

(MP) If X then Y
X

Y

A probabilistic analog of *modus ponens* can be constructed as follows:

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  Pr(Y*X) is high
  X
p[===========          (where p is high)
  Y
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"Pr(Y*X)" represents conditional probability (the probability of Y given X) and is standardly defined as Pr(Y&X)/Pr(X). The double line separating premisses and conclusion is meant to indicate that the argument is not deductively valid. The letter "p" that labels this line denotes the probability that the premisses confer on the conclusion.

With some tinkering this pattern of reasoning can be turned into a respectable form of argumentation. My preference is to turn it into a deductively valid argument in which a claim about the probability of Y is deduced. A first step in that direction might be the following:

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Pr(Y*X) is high
X
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Pr(Y) is high
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However, this is unsatisfactory as it stands. It is perfectly possible for Y to have a high probability conditional on X, but a low probability unconditionally; Even though it is very probable that the roulette wheel ball landed double-zero on the last spin, given that your honest and visually acute friend told you that this is what happened, it is still unconditionally improbable that the ball landed double-zero. The way forward is to time-index the probability functions:

(Prob-MP) $Pr_{t1}(Y{*}X)$ is high
X

$Pr_{t2}(Y)$ is high

We are to imagine that an agent at time t_1 assigns a high value to $Pr(Y{*}X)$. The agent then learns that X is true; this means that the probability assignment needs to be updated. If X is the total evidence that the agent acquires about Y in the temporal interval separating t_1 and t_2 , then he or she should assign Y a high probability at time t_2. This is nothing other than the Principle of Conditionalization[3] applied so as to respect the Principle of Total Evidence. I didn't mention either of these in my formulation of (Prob-MP), so a fuller statement of this form of argument should go as follows:

$Pr_{t1}(Y{*}X)$ is high
X is the total evidence that the agent acquires between t_1 and t_2
Updating proceeds by conditionalization

$Pr_{t2}(Y)$ is high

If (Prob-MP) is ok, what is wrong with the heuristic idea that deductively valid arguments have analogs that are probabilistically strong? We need look no farther than *modus tollens*:

(MT) If X then Y
not-Y

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not-X

If we construct a probabilistic analog of (MT), and assume both the Principle of Conditionalization and the Principle of Total Evidence, we obtain:

(Prob-MT) $Pr_{t1}(Y^{*}X)$ is high
not-Y

Pr_{t2}(not-X) is high

In other words, if a theory X says that Y is very probable, and we learn that Y fails to obtain, then we should conclude that the theory is probably false. Here is an equivalent formulation:

Pr_{t1}(not-$Y^{*}X$) is low
not-Y

Pr_{t2}(not-X) is high

If a theory X says that something probably won't occur, but it does, then the theory is probably false.

It is easy to find counterexamples to this principle. You draw from a deck of cards. You know that if the deck is normal and the draw occurs at random, then the probability is only 1/52 that you'll obtain the seven of hearts. Suppose you *do* draw this card. You can't conclude just from this that it is improbable that the deck is normal and the draw was at random.

This example makes it seem obvious that there is no probabilistic analog of *modus tollens*. However, this feeling of obviousness can fade when we look at other examples in which the relevant probability is far less than 1/52. Consider the following argument proposed by the biologist Richard Dawkins.[4] He is considering what a respectable theory of the origin of life on earth is permitted to say was the probability that life would evolve from nonliving materials:

> ... there are some levels of sheer luck, not only too great for puny human imaginations, but too great to be allowed in our hard-headed calculations about the origin of life. But ... how great a level of luck, how much of a miracle, *are* we allowed to postulate? ... The answer to our question ... depends upon whether our planet is the only one that has life, or whether life abounds all around the universe.
>
> ... the maximum amount of luck that we are allowed to assume, before we reject a particular theory of the origin of life, has odds of one in N, where N is the number of suitable planets in the universe. There is a lot hidden in that word 'suitable' but let us put an upper limit of 1 in 100 billion billion for the maximum amount of luck that this argument entitles us to assume.

Since there are approximately 100 billion billion planets in the universe, Dawkins thinks that we can reject any theory of the origin of life on earth that says that the probability of that event was less than 1/100 billion billion:

Pr(life evolved on earth * theory T) < 1/100 billion billion
Life evolved on earth

Theory T is false

One curious feature of this argument is Dawkins' choice of a lower bound. Why is the number of planets relevant? Perhaps Dawkins is thinking that if " is the *frequency* of life-bearing planets among "suitable" planets (i.e., planets on which it is possible for life to evolve), then the true *probability* of life's evolving on earth must also be ". There is a mistake here, which we can identify by examining how actual frequency and probability are related. With small sample size, it is perfectly possible for these quantities to have very different values; consider a fair coin that is tossed three times and then destroyed. However, Dawkins is obviously thinking that the sample size is very large, and here he is right that the actual frequency provides a good estimate of the probability. It is interesting that Dawkins tells us to reject a theory if the probability it assigns is too *low*, but why doesn't he also say that we should reject it if the probability it assigns is too *high*? The reason, presumably, is that we cannot rule out the possibility that our planet was not just suitable but *highly conducive* to the evolution of life. However, this point cuts both ways. Although " is the *average* probability that a suitable planet will have life evolve, different suitable planets still might have different probabilities; some planets may have values that are greater than " while others may have values that are lower. Dawkins' lower bound assumes *a priori* that the earth was above average; this is a mistake that might be termed the "Lake Woebegone Fallacy."[5]

There's a general reason why no probabilistic version of *modus tollens* is to be had. Theories that make good probabilistic predictions about lots of events will typically say that the *conjunction* of those events has a very low probability. Even if $Pr(E_1 * T)$, $Pr(E_2 * T)$, ..., $Pr(E_n * T)$ are each high (but less than unity), $Pr(E_1 \& E_2 \& ... \& E_n * T)$ will be very low, if the E_i's are sufficiently numerous and are probabilistically independent of each other, conditional on T. Consider a roulette wheel in which we distinguish only double-zero and not-double-zero as possible outcomes. A perfectly satisfactory theory of this device might say that the probability of double-zero is 1/38 and the probability of not-double-zero is 37/38 on each spin. Suppose we spin the wheel 3800 times and obtain a sequence of outcomes in which there are 100 double zero's. The probability of this exact sequence of outcomes is $(1/38)^{100}(37/38)^{3700}$, which is a tiny number. The fact that the theory assigns this outcome a very low probability hardly suffices to reject the theory.[6]

The accompanying table depicts the asymmetry between *modus ponens* and *modus tollens* for which I have argued. I assume that the riders concerning the Principle of Conditionalization and the Principle of Total Evidence are in place. There is a "smooth transition" between probabilistic and deductive *modus ponens*; the minor premiss ("X") either ensures that Y is true, or makes Y very probable, depending on how the major premiss is formulated. In contrast, there is a radical discontinuity between probabilistic and deductive *modus tollens*. The minor premiss ("not-Y") guarantees that X is false in the one case, but has no implications whatever about the probability of X in the other.

Table

Given that probabilistic *modus tollens* is invalid, there is a fallback position that we should consider. Perhaps if a theory says that an event is very improbable, but the event happens anyway, then the event counts as *evidence against* the theory. The event doesn't allow you to conclude that the theory is false, nor even that it has a low probability, but maybe the event lowers whatever probability you had assigned the theory before:

$Pr_{t1}(Y^*X)$ is low
Y

$Pr_{t2}(X)$ is lower than $Pr_{t1}(X)$

Whereas (Prob-MT) allows you to draw a conclusion about the *absolute value* of X's probability (it is *low*), the present proposal is that your conclusion should merely be *comparative* (X's probability is *lower* than it was before). This principle also is wrong, as a nice example from the statistician Richard Royall[7] illustrates: Suppose I send my valet to bring me one of my urns. I want to test the hypothesis that the urn he returns with contains 2% white balls. I draw a ball and find that it is white. Is this evidence against the hypothesis? It may not be. Suppose I have only two urns – one of them is as described, while the other contains 0.0001% white balls. In this instance, drawing a white ball is evidence *in favor* of the hypothesis, not evidence *against*.

Royall's example brings out an important feature of the concept of evidence. To say whether an observation is evidence for or against a hypothesis, we have to know what the other hypotheses are that we should consider. The evidence relation is to be understood in terms of the idea of *discrimination*.[8] E is evidence for or against the hypothesis H_1 only relative to an alternative hypothesis H_2. For evidence to be evidence, it must discriminate between the competing hypotheses. Another way to put this point is by saying that the evidence relation is ternary, not binary. The right concept to consider is "E favors H_1 over H_2 ," not "E is evidence for (or against) H." It needs to be understood that this thesis is restricted to hypotheses that do not deductively entail observations; if H entails E, and E fails to obtain, then E rules out (and hence disconfirms) H.

The idea that evidence is essentially a comparative concept is often associated with the *Law of Likelihood*:[9]

> Evidence E favors hypothesis H_1 over hypothesis H_2 if and only if
>
> $Pr(E * H_1) > Pr(E * H_2)$.

Notice that the absolute values of $Pr(E* H_1)$ and $Pr(E* H_2)$ don't matter here; all that matters is how they compare. The Likelihood Principle does not tell you what to believe nor does it even indicate which hypothesis has the higher probability of being true. It merely assesses the weight of the evidence at hand.

I say that the thesis that evidence is comparative is often "associated with" the Likelihood Principle, not that the two are essentially connected. There are two reasons for this. The first is that the Likelihood Principle, taken at its word, does not rule out the possibility that one can talk about the evidence for or against a given hypothesis without reference to alternative hypotheses. True, advocates of "likelihoodism"[10] have endorsed the Likelihood Principle and have also insisted that evidence is essentially comparative, but this just shows that likelihoodism goes beyond the letter of the Law of Likelihood. The second reason is that there are theories of evidence that depart from the dictates of likelihoodism but nonetheless agree that the evidence relation is ternary rather than binary. For example, standard Neyman-Pearson statistical theory (interpreted evidentially) tells one how to deal with the possibility of both "type 1" and "type 2" errors, and this entails that *two* hypotheses are being assessed,[11] not just *one*.

I don't want to give the impression that the comparative conception of evidence is universally endorsed in science. Unfortunately, there is a statistical methodology that is sometimes used that purports to assess how evidence bears on a single hypothesis. This is the theory, due to R.A. Fisher, of significance testing. The story of the valet and the two urns already suggests what is wrong with this approach, but let me add another example to help flesh out the picture a bit. Consider the hypothesis that a coin is fair. If the coin is tossed a large number of times (say, 1000 times), there will be 2^{1000} possible sequences of heads and tails that might occur. If the hypothesis that the coin is fair is true, then each of these exact sequences has the same tiny probability (namely $(½)^{1000}$) of occurring. Yet, it seems utterly wrong to say that each outcome would count as evidence against the hypothesis.[12] To make sense of what it means to test this hypothesis about the coin, we need to say what the alternative hypotheses are; if the alternative hypothesis one wishes to consider is that the coin is strongly biased in favor of heads, then sequences in which there are large numbers of tails count as evidence *in favor* of the hypothesis that the coin is fair; but if the alternative one wishes to consider is that the coin is strongly *biased against* heads, precisely the opposite interpretation of that observation would be correct. Fisher remarked that when a theory says that what one has observed is very improbable, that one's conclusion should take the form of a "... simple disjunction. Either an exceptionally rare chance has occurred, or the theory ... is not true."[13] There is nothing wrong with this point; the mistake is to think that this disjunction entails that one has obtained evidence against the theory.

This is not the place to present a systematic critique of Fisherian significance testing (under the interpretation of the method that equates the improbability of E if H is true with the strength of the evidence against H). That critique has been developed in several places already,[14] and no adequate response has been provided. So let us take stock. The first conclusion is that there is no probabilistic analog of *modus tollens*. This should be uncontroversial. Separate from this thesis about arguments that draw conclusions about the *probabilities* of hypotheses is the thesis I have defended about *evidence*: *Assessing whether an observation counts as evidence for or against a hypothesis must consider alternative hypotheses and what they predict about the observation.* As noted above, this comparative thesis is restricted to hypotheses that don't deductively entail observational claims, but merely confer probabilities on them. In discussing the Law of Likelihood, I mentioned that this principle does not tell you which hypotheses to accept or reject. However, if the acceptance or rejection of hypotheses requires the accumulation of evidence *pro* and *con*, then the comparative principle just stated provides a simple but important lesson about acceptance.[15]

These points about probability reasoning allow us to identify the central deficiency in the Intelligent Design (ID) movement. "Intelligent design" is the label that Michael Behe, William Dembski, and Philip Johnson prefer so that their position will not be confused with old-fashioned creationism.[16] The term "creationism" suggests the idea of *special* creation -- a denial of the claim that all life on earth is genealogically related; ID theorists don't endorse the idea of special creation – that each species (or "basic kind of organism") was separately created by an intelligent designer. Rather, their beef with evolutionary theory concerns the power of natural selection to produce complex adaptations. Behe is the pointman here, arguing that traits that exhibit "irreducible complexity" pose an in-principle difficulty for evolutionary theory and, indeed, for any theory that limits itself to mindless natural processes. The vertebrate eye, for example, exhibits irreducible complexity because all of its many parts must be arranged *just so* if the eye is to perform the function of allowing the organism to see. For this reason, Behe's argument isn't different in form from Paley's.[17] The novelty in Behe's presentation consists in his choice of examples. Behe thinks that basic features of biochemistry, such as the machinery that drives the bacterial flagellum and the mechanisms that get blood to coagulate, are irreducibly complex. Just as earlier creationists complained that an organism would gain no benefit from having 10% of an eye or wing, Behe argues that having 10% of the clotting process would be useless.

There are a number of philosophical and scientific objections that might be considered in connection with Behe's argument. Before I move on to my main complaint, I need to mention the fact that Behe equivocates between the process of gradual natural selection, taken on its own, and "evolutionary processes" construed more broadly. "Darwinian gradualism," taken in its strict sense, requires the steady accumulation of modifications, each conferring a small benefit. This means that 10% of a wing has to represent an advantage compared with 9%, if this type of selection is to transform a population from one in which all individuals have no wings at all to one in which all have 100% of a wing.[18] On the other hand, it is important to recognize that evolutionary theory countenances many processes additional to that of pure Darwinian gradualism. For example, since the theory is probabilistic, it is perfectly possible for a population to move from each individual's having 9% of a wing to each individual's possessing 10%, even if the latter state represents no selective advantage. This is called "random genetic drift." My point here is not that this transition is *probable*, but that it is *possible*, according to the theory. Behe is correct that the pure process of Darwinian gradualism cannot lead a wing to evolve if the fitnesses are those described in lines 2 or 3 in the accompanying figure, and that the monotonic increase depicted in line 1 is required. However, he concludes from this that "evolutionary theory" cannot explain the emergence of traits whose fitnesses conform to lines 2 and 3; this does not follow and it is not correct.

FIGURE

The objection to Behe's argument that I want to focus on here concerns the type of reasoning he employs against evolutionary theory and in favor of the hypothesis of intelligent design. Behe repeatedly vacillates between using a deductive and a probabilistic *modus tollens* against evolutionary theory. The vacillation sometimes occurs on the same page. Consider the following passage:

> ... I have shown why many biochemical systems cannot be built up by natural selection working on mutations: no direct, gradual route exists to these irreducibly complex systems ... There is no magic point of irreducible complexity at which Darwinism is logically impossible. But the hurdles for gradualism become higher and higher as structures are more complex, more interdependent (p. 203).

Behe's first sentence says that irreducible complexity *cannot arise* by Darwinian processes; however, the next two assert, more modestly, that irreducibly complex features are *improbable* on the Darwinian model and that they become more improbable the more complex they are. I hope it is clear from what I've said earlier why this shift is important. If evolutionary theory really did have the deductive consequence that organisms *cannot* have features that are irreducibly complex, then that theory would have to be false, if such features exist. But what if the theory merely entailed that irreducibly complex features are very improbable? Would the existence of such features show that the theory is improbable? Would it follow that the theory is disconfirmed by those observations? Would it follow that these features provide evidence in favor of intelligent design? The answers to all these questions are the same – *no*. There is no probabilistic analog of *modus tollens*.

In addition to rejecting evolutionary explanations, Behe advances the positive thesis that the biochemical systems he describes in loving detail "were designed by an intelligent agent" (p. 204). However, for these details to favor intelligent design over mindless evolution, we must know how probable those details are under each hypothesis. This is the point of the Law of Likelihood. Behe asserts that these details are very improbable according to evolutionary theory, but how probable are they according to the hypothesis of intelligent design? It is here that we encounter a great silence. Behe and other ID theorists spend a great deal of time criticizing evolutionary theory, but they don't take even the first steps towards formulating an alternative theory of their own that confers probabilities on what we observe. If an intelligent designer built the vertebrate eye ,or the bacterial flagellum, or the biochemical cascade that causes blood to clot, what is the probability that these devices would have the features we observe? The answer is simple – *we do not know*. We lack knowledge of what this putative designer's intentions would be if he set his mind to constructing structures that perform these functions.

The sad fact about ID theory is that there is no such theory. Behe argues that evolutionary theory entails that adaptive complexity is very improbable, Johnson rails against the dogmatism of scientists who rule out *a priori* the possibility of supernatural explanation, and Dembski tries to construct an epistemology in which it is possible to gain evidence for the hypothesis of design without ever having to know what, if anything, that hypothesis predicts. A lot goes wrong in each of these efforts,[19] but notice what is not even on the list.

Intelligent design theorists may feel that they have already stated their theory. If the *existence* of the vertebrate eye is what one wishes to explain, their hypothesis is that an intelligent designer constructed the vertebrate eye. If it is the *characteristics* of the vertebrate eye (the fact that it has features F_1, F_2, ..., F_n), rather than its mere existence, that one wants to explain, their hypothesis is that an intelligent designer constructed the vertebrate eye with the intention that it have features F_1, F_2, ..., F_n and that this designer had the ability to bring his plan to fruition. Notice that both of these formulations of the hypothesis of intelligent design simply build into that hypothesis the observations whose explanation we seek. The problem with this strategy is that the same game can be played by the other side. If the evolutionary hypothesis is formulated by saying "evolution by natural selection produced the vertebrate eye" or by saying that "evolution by natural selection endowed the eye with features F_1, F_2, ..., F_n ," then it too entails the observations.

To avoid trivializing the problem in this way, we should formulate the observations so that they are *not* built into the hypotheses we want to test. This can be achieved by organizing the problem as follows:

(O) The vertebrate eye has features F_1, F_2, ..., F_n.

(ID) The vertebrate eye was created by an intelligent designer.

(ENS) The vertebrate eye was the result of evolution by natural selection.

Behe claims that (O) has a low probability according to the (ENS) hypothesis. My complaint is that we do not know what the probability of (O) is according to (ID). If an intelligent designer made the eye, perhaps he would have been loathe to give it the features we observe. Or perhaps he would have aimed at producing those very characteristics.[20] The single sentence stated in (ID) does not a theory make. This problem is not solved by simply *inventing* assumptions about the putative designer's goals and abilities; what is needed is information about the putative designer(s) that is independently attested. Without that information, the theory makes no predictions about the eye or about the other examples of "irreducible complexity" that Behe discusses. And without those predictions, the intelligent design movement can provide no evidence against the evolutionary hypothesis.

After concluding that evolutionary theory cannot explain adaptations that are irreducibly complex, Behe briefly broaches the subject of whether some "as-yet-undiscovered natural process" might be the explanation. Here is his analysis:

> No one would be foolish enough to categorically deny the possibility ... [however] if there is such a process, no one has a clue how it would work. Further, it would go against all human experience, like postulating that a natural process might explain computers ... In the face of the massive evidence we do have for biochemical design, ignoring that evidence in the name of a phantom process would be to play the role of the detectives who ignore an elephant (pp. 203-204).

Notice that Behe claims that there is "massive evidence for biochemical design," but what *is* that evidence? It seems to consist of two facts, or alleged facts – that evolutionary theory says that irreducibly complex adaptations have low probabilities and that no one has yet formulated any other theory restricted to mindless natural processes that could be the explanation. However, if the comparative principle about evidence stated earlier is correct, this "evidence" is no evidence at all.

After evolutionary theory and "as-yet-undiscovered natural process[es]" are swept from the field, Behe immediately concludes that the biological mechanisms whose details he has described

> ... were designed by an intelligent agent. We can be as confident of our conclusion for these cases as we are of the conclusions that a mousetrap was designed, or that Mt. Rushmore or an Elvis poster were designed ... Our ability to be confident of the design of the cilium or intracellular transport rests on the same principles as our ability to be confident of the design of anything: the ordering of separate components to achieve an identifiable function that depends sharply on those the components (p. 204).

Behe is right that the nonbiological examples he cites favor hypotheses of intelligent design over hypotheses that postulate strictly mindless natural processes, but he is wrong about the reason and wrong to think that biochemical adaptations can be assimilated to the same pattern. In the case of mousetraps, Mount Rushmore, and Elvis posters, we are confident about intelligent design because we have strong evidence for *human* intelligent design. We know that all of these objects are just the sorts of things that human beings are apt to make. The probability of their having the features we observe, on the hypothesis that they were made by intelligent human designers, is fairly large, whereas the probability of their having those features, if they originated by chance, is low. The likelihood inference is unproblematic. But the probability that the bacterial flagellum would have the features we observe, or that the mechanism for blood clotting would have its observed features, if *human beings* somehow made those devices, is very very low. ID theorists therefore are led to consider possible *non*human designers – indeed, possible designers who are *supernatural.* Some of these *possibilia* would, if they existed, have goals and abilities that would make it highly probable that these devices have the features we observe; others would not. Averaging over all these possibilities, what is the probability that the device will have the features we observe if it was made by some possible intelligent designer or other? We do not know, even approximately.

Behe would like to be able to identify an observable feature of natural objects that *could not exist* if those objects were produced by strictly mindless processes and that therefore *must* be due to intelligent design (natural or supernatural). There is no such property. It is not *impossible* for irreducibly complex functional features to arise by the evolutionary process of natural selection, which is *not* a random process.[21] Indeed, it isn't even impossible for them to arise by a purely random chance process. This is the simple point made vivid by thinking about monkeys and typewriters and of particles whirling in the void. The next step is to think about the properties that an object *probably will* have if it is made by an intelligent designer and *probably won't* have if it isn't. The problem here is that there are many kinds of possible intelligent designers, and many kinds of possible mindless processes. Is there a property that a natural object probably will have, no matter what sort of possible intelligent designer made it? I am confident that the answer to this question is *no*. Is there a property that it probably won't have, no matter what sort of possible mindless process made it? As for this second question, here I am in agreement with Behe – *we really don't know*. But ignorance does not constitute a reason to reject the possibility that what we observe is due to mindless natural processes that we have not yet considered and conclude that what we observe must be due to intelligent design.

My critique of the intelligent design movement has been based on the comparative principle I stated about evidence – *to say whether an observation counts as evidence against evolutionary theory and in favor of the hypothesis of intelligent design, one must know what each predicts about the observation*. I have challenged intelligent design theorists to produce a theory that has implications about the detailed examples of "irreducible complexity" that Behe describes. However, there is another response that intelligent design theorists might contemplate. This is to deny the comparative principle itself. Dembski has seized this horn of the dilemma.[22] If he succeeds in developing an epistemology of this sort (so far he has not), the way will be paved for an unprecedented result in the history of science – the rejection of a logically consistent theory that confers probabilities on observations, but does not entail them, and its replacement by another, without its needing to be said what the replacing theory predicts.

Address for correspondence: Elliott Sober, Philosophy Department, University of Wisconsin, Madison, WI 53706.

Phone: 608 263 3700; Fax: 608 265 3701; E-mail: ersober@facstaff.wisc.edu

Figure Caption: What are the fitness consequences of having *n*% of a wing or eye, as opposed to having (*n*-1)%? According to line 1, each small increase represents an increase in fitness. According to line 2, having more and more of the trait makes no difference in fitness until a threshold (*t*) is crossed. Line 3 also depicts a threshold effect, but here having more of the

wing or eye is deleterious, not neutral, until the threshold is crossed. Evolution via the pure process of Darwinian gradualism requires the monotonic increase that line 1 exhibits, and cannot occur if the fitnesses are those represented by lines 2 or 3. However, evolutionary theory countenances processes additional to that of "pure Darwinian gradualism," so, in fact, the theory say that it *is* possible for the trait to evolve under all three scenarios.

Table Caption: Although *modus ponens* has a probabilistic analog, *modus tollens* does not.

Table

############# #	**Deductive**	**Probabilistic**
Modus Ponens	If X then Y X ------------- **VALID** Y	Pr_{t1}(Y*X) is high X -------------------- **VALID** Pr_{t2}(Y) is high
Modus Tollens	If X then Y not-Y ------------- **VALID** not-X	Pr_{t1}(Y*X) is high not-Y ------------------ **INVALID** Pr_{t2}(X) is low

Notes

1. My thanks to Branden Fitelson, Alan Hajek, and Terry Sullivan for helpful discussion.

2. One needed refinement is that the number of mutually exclusive and collectively exhaustive propositions be finite. When this fails, a probability of unity is not the same as necessity, and a probability of zero is not the same as impossibility. If I randomly choose a fraction that is between 0 and 1, the probability that I'll choose 13/345 is zero, but it isn't impossible that I'll choose that number.

3. The principle of conditionalization assumes that acquiring evidence involves becoming certain that various propositions are true. If we are never entitled to be certain about the truth values of observation reports, then a new rule for updating is needed. This is supplied by the idea of Jeffrey-

conditionalization; see Richard Jeffreys, *The Logic of Decision* (Chicago: University of Chicago Press, 1983). This point does not invalidate the claim that (Prob-MP) is correct; it merely points to a limit on its applicability.

4.See Richard Dawkins, *The Blind Watchmaker* (New York: Norton, 1986), pp. 144-146.

5. See Elliott Sober, "The Design Argument." In W. Mann (ed.), *Blackwell Companion to the Philosophy of Religion* (Oxford: Blackwell, 2003). Also available at the following URL: http://philosophy.wisc.edu/sober.

6. This conclusion cannot be evaded by saying that the theory entails that obtaining approximately 100 double zero's in 3800 spins is highly probable. The Principle of Total Evidence says that we have to use *all* of the evidence available in evaluating theories, not just part. Theories also entail that tautologies will be true, and tautologies are part of every data set, but this is no reason to set aside the total evidence and focus just on the tautologies that the evidence entails.

7.See Richard Royall, *Statistical Evidence – A Likelihood Paradigm* (Boca Raton, FL: Chapman and Hall, 1997), p. 67.

8. Elliott Sober, "Testability," *Proceedings and Addresses of the APA* 73 (1999): 47-76. Also available at the following URL: http://philosophy.wisc.edu/sober.

9.See Ian Hacking, *The Logic of Statistical Inference* (Cambridge: Cambridge University Press, 1965), Anthony Edwards, *Likelihood* (Cambridge: Cambridge University Press, 1972), and Richard Royall, *op cit*.

10. Anthony Edwards, *op cit*, Richard Royall, *op cit*, and Elliott Sober, *op cit*.

11. See Richard Royall, *op cit*, chapter 2 for discussion.

12. See Ian Hacking, *op cit,* p. 85. For some alternative formulations of the Fisherian idea, and

objections, see Richard Royall, *op cit*, chapter 3.

13. R.A. Fisher, R., *Statistical Methods and Scientific Inference* (New York: Hafner. 1959, 2nd edition), p. 39.

14. See Ian Hacking, *op cit*, Anthony Edwards, *op cit*, and Richard Royall, *op cit.*

15. The present point about the comparative character of evidence can be connected with the earlier argument about probabilistic *modus tollens* by considering the fact that every deductively invalid argument can be turned into a valid argument by adding premisses. How can this be done in the case of (Prob-MT)? Bayes' Theorem says that Pr(H*O) = Pr(O* H)Pr(H)/Pr(O). In consequence, the following argument is valid (assuming, as before, that O is the total evidence and that updating proceeds by conditionalization):

> Pr_{t1}(O* H) is low
> O
> Pr_{t1}(H) # Pr_{t1}(O)
> ------------------
> Pr_{t2}(H) is low

The new third premiss is equivalent to

> Pr_{t1}(H)[1 - Pr_{t1}(O* H)] # Pr_{t1}(O* not-H)Pr_{t1}(not-H).

Notice that the prior probability and the likelihood of the *alternative* hypothesis (not-H) enters into this formula. The lesson, again, is this: *if you want to argue that H is improbable, based on the fact that H says that what you observe is very improbable, you must have additional information about how probable the observations would be if H were false.*

16.Michael Behe, *Darwin's Black Box* (New York: Free Press, 1996); William Dembski, *The Design Inference* (Cambridge: Cambridge University Press, 1998); Philip Johnson, P., *Darwin on Trial*, (Downers Grove, IL: Intervarsity, 1991).

17. William Paley, *Natural Theology, or, Evidences of the Existence and Attributes of the Deity, Collected from the Appearances of Nature* (London: Rivington, 1802).

18. This problem is apparently more pressing when it comes to wings than it is with respect to eyes. Eyes come in various forms, some far more rudimentary than the vertebrate eye, and it is not at all difficult to see how all these various forms might provide an adaptive advantage; see Richard Dawkins, *Climbing Mount Improbable* (New York: W.W. Norton, 1996), chapter 5. The wing is more puzzling, since 5% of a wing provides no lift at all; being able to fly is a threshold effect. One part of the solution to this problem is to see that the rudimentary beginnings of wings can serve other functions, and that once wing evolution is under way, wings can continue to evolve because they facilitate flight. This is more than just speculation; J. Kingsolver and M. Koehl (in "Aerodynamics, Thermoregulation, and the Evolution of Insect Wings -- Differential Scaling and Evolutionary Change." *Evolution* 39 (1985) 488-504) provide empirical evidence for the claim that insect wings began evolving as devices for regulating temperature and then continued to evolve as devices for flying.

19. For a critical evaluation of Dembski's epistemology, see Branden Fitelson, Elliot Sober, and Christopher Stephens, "How Not to Detect Design -- Critical Notice of W. Dembski's *The Design Inference*." *Philosophy of Science* 66 (1999) 472-488, reprinted in R. Pennock (ed), *Intelligent Design Creationism and its Critics* (Cambridge, MA: MIT Press, 2001), pp. 597-616. Although Dembski tries to build on the (flawed) foundation of Fisherian significance testing, his proposals go far beyond Fisher's. For one thing, Fisher's method applies to the problem of testing a *specific* chance hypothesis, whereas Dembski thinks that he can "sweep from the field" the entire class

of *all* chance hypotheses. Another novelty in Dembski's approach is his use of ideas from complexity theory.

20. In fact, if the ID hypothesis says that *some* intelligent designer produced the effects cited, then one must consider different possible intelligent designers, weigh the probability that they were the ones involved, and assess the probability of the outcome if they were doing the work.

21. A random process is one that has a large number of equiprobable outcomes. The whole point of natural selection is that some outcomes are vastly more probable than others. Selection is a probabilistic process, but not all probabilistic processes are random.

22. William Dembski, *op cit*. Since Behe (pp. 285-286) praises Dembski's epistemological insights, he presumably would embrace this response.