Evolution without Naturalism

Elliott Sober[1]
Philosophy Department
University of Wisconsin, Madison

> "… any confusion between the ideas *suggested* by science and science itself must be carefully avoided." – Jacques Monod, *Chance and Necessity*, p. xiii.

Does evolutionary theory have implications about the existence of supernatural entities? This question concerns the *logical relationships* that hold between the theory of evolution and different bits of metaphysics. There is a distinct question that I also want to address; it is *epistemological* in character. Does the evidence we have for evolutionary theory also provide evidence concerning the existence of supernatural entities?

An affirmative answer to the logical question would entail an affirmative answer to the epistemological question if the principle in confirmation theory that Hempel (1965, p. 31) called the special consequence condition were true:

> *The special consequence condition*: If an observation report confirms a hypothesis *H*, then it also confirms every consequence of *H*.

According to this principle, if evolutionary theory has metaphysical implications, then whatever confirms evolutionary theory also must confirm those metaphysical implications. But the special consequence is false. Here's a simple example that illustrates why. You are playing poker and would dearly like to know whether the card you are about to be dealt will be the Jack of Hearts. The dealer is a bit careless and so you catch a glimpse of the card on top of the deck before it is dealt to you. You see that it is red. The fact that it is red confirms the hypothesis that the card is the Jack of Hearts, and the hypothesis that it is the Jack of Hearts entails that the card will be a Jack. However, the fact that the card is red does not confirm the hypothesis that the card will be a Jack.[2] Bayesians gloss these facts by understanding confirmation in terms of probability raising:

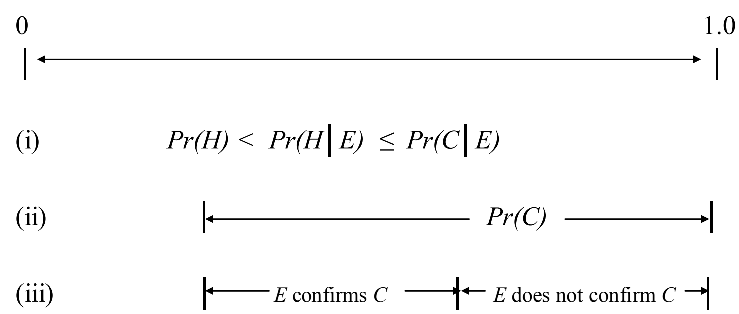
> *The Bayesian theory of confirmation*: *O* confirms *H* if and only if $Pr(H \mid O) > Pr(H)$.

[1] My thanks to Juan Comesaña, Mark Colyvan, James Crow, Hartry Field, Geoffrey Hellman, Richard Lewontin, Bence Nanay, Alexander Rosenberg, Michael Ruse, Carolina Sartorio, Larry Shapiro, Alan Sidelle, and Michael Titelbaum for useful comments.

[2] This illustrates Dretske's (1970) thesis that the evidence relation involves a non-penetrating epistemic operator.

The general reason why Bayesianism is incompatible with the special consequence condition is depicted in Figure 1.[3]

Figure 1. Why Bayesianism entails that the special consequence condition is false. If *E* confirms *H*, the inequality between the first and second terms in (i) holds. If *H* entails *C*, the inequality holds between the second and third terms in (i). Given just these two facts, the value of *Pr(C)* could be anywhere in the interval shown in (ii). Where *Pr(C)* is located determines whether *E* confirms *C*, as shown in (iii).



Although you don't have to be a Bayesian to see that the special consequence condition is mistaken, the Bayesian analysis of why it fails in the poker example suggests a larger pattern. The hypothesis of interest is a conjunction; it says that the next card is a Jack *and* that it is a heart. The observation that the card is red raises the probability of the second conjunct and also raises the probability of the whole conjunction; however, it does not raise the probability of the first conjunct. Perhaps evolutionary theory is a conjunction of metaphysical and empirical elements; observations may confirm the theory by confirming its empirical elements but without confirming what is metaphysical. I offer this as a thought to ponder, not as a thesis now to be embraced. Beware! There are reasons to hesitate. Maybe the theory, properly construed, has no metaphysical elements at all. Or maybe the metaphysical elements and the empirical elements can't be separated in this neat way. Let us see.

In what follows, my two main metaphysical topics are God and numbers. I'll begin with the familiar general point that the existence of God is compatible with evolutionary theory and then turn to the more specific question of what evolutionists mean when they say that mutations are unguided. Doesn't this entail that there is no God who guides the evolutionary process? I'll then consider whether theistic evolutionists must be deists. Must they hold that God starts the universe in motion and never intervenes in what happens after that? Although I will focus mainly on how theistic

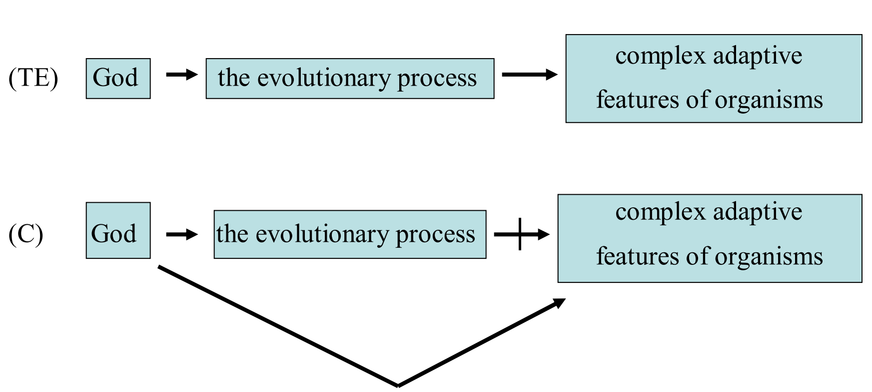
[3] I am indebted to Kotzen (2007) for this way of representing the point.

evolutionism should be formulated, I hope the points I'll make will also be relevant to how agnostics and atheists should think about scientific theories. The upshot is that evolutionary theory is neutral on one question about naturalism – the question of whether a supernatural deity exists. Numbers, my second main topic, raise a different set of issues, since mathematical evolutionary theory entails that numbers exist. If Platonism is the right philosophy of mathematics, then evolutionary theory (like other mathematized sciences) entails that naturalism is false. Returning to the inadequacy of the special consequence condition, I'll argue that the evidence we have for evolutionary theory isn't evidence for the existence of numbers. Here I'll criticize the Quine/Putnam indispensability argument and defend the anti-holistic thesis that theories typically have many parts and scientists are within their rights to hold that these parts sometimes differ in their epistemic status. My overall goal in this essay is to challenge the influential idea that the opposition between evolutionary theory and creationism is a clash of two philosophies – naturalism and supernaturalism. In one respect, evolutionary theory is uncommitted as to whether naturalism is true; in another respect, it is anti-naturalistic.

God

Creationists maintain that the theory of evolution entails that there is no God. If they are right, then the theory has metaphysical implications. Atheistic evolutionists (e.g., Dennett 1995 and Provine 1989) often agree with creationists on this point. The conditional "if evolutionary theory is true, then there is no God" is therefore common ground. Where creationists have their *modus tollens*, these evolutionists have their *modus ponens*.

Figure 2. Theistic evolutionism (TE) says that God made organisms and their complex adaptive features by setting the evolutionary process in motion. Creationism (C) says that the evolutionary process is incapable of producing the complex adaptive features we observe and that these were the result of God's direct intervention in nature.



Both sides are wrong. Theistic evolutionism is a logically consistent position (Ruse 2000; Sober 2008b). This is the idea that God uses the evolutionary process to make organisms. The difference between creationism and theistic evolutionism is depicted in Figure 2. Creationists don't limit themselves to the assertion that there is a God who is responsible for the living things that we observe. They additionally maintain that the evolutionary process is inherently incapable of producing the complex adaptations that organisms have and that these are due to God's *direct* intervention in nature. According to theistic evolutionism, God produces organisms *indirectly*, by setting the evolutionary process in motion.

In saying that theistic evolutionism is logically consistent, I am not saying that it is plausible or true. I'm merely saying that it isn't contradictory. Evolutionary theory is silent on the question of whether God exists. Even if you think the theory knocks the wind from the sails of the argument from design,[4] you still need to consider the fact that there are other arguments for the existence of God. And even if you think that none of these arguments is rationally persuasive, you still need to consider whether belief in God must be based on evidence. This is the old debate between William James' (1897) "Will to Believe" and W.K. Clifford's (1877) "The Ethics of Belief," which Alvin Plantinga (2000) has engaged via his thesis that belief in God is "properly basic." Evolutionary theory has no implications about the cosmological argument or the ontological argument nor does it say whether you are entitled to believe in God even if you can offer no good argument that such a being exists. Atheistic evolutionists may scoff at these other arguments and at those who believe in God while admitting that they can offer no compelling argument that there is such a being. But this is not the theory of evolution talking.

Although evolutionary theory is silent on the question of whether there is a God, it is not neutral with respect to logically stronger hypotheses about God. Consider, for example, the statement

> Life appeared on earth about 10,000 years ago due to divine intervention.

This statement *is* inconsistent with evolutionary theory. Does this show that the theory has metaphysical implications? After all, it entails that there is no God of the sort just described. Maybe so, but only in the trivial sense that every statement is thereby shown to have metaphysical implications. Consider the statement "my car is gray." This entails the falsehood of the statement

> A supernatural intelligent designer insured that everything in the physical world would fail to be gray.

Is the statement about my car thereby shown to have metaphysical implications? I am inclined to think that it is much more hum-drum. In any event, our inquiry is less likely to lapse into triviality if we ask about specific metaphysical propositions and whether evolutionary theory has anything to say about them. The question is not whether

[4] I argue in Chapter 2 of Sober (2008) that the argument from design is defective for reasons that are independent of whether the theory of evolution is true.

evolutionary theory has any implications about metaphysical matters, but whether it has implications, for example, about whether there is a God. And it does not.[5]

Undirected Mutation

What is this thing called "evolutionary theory," which theistic evolutionism is able to encompass consistently? It includes the origin of life from nonliving materials by physical processes, the branching genealogical process whose upshot is that current organisms are connected to each other by relations of common ancestry, the random origin of new mutations, and the processes that govern trait evolution within lineages, such as selection and drift. Among these several propositions, the idea that mutations are "random" may seem to be a sticking point. Random mutation suggests "blind chance," but what God causes does not occur by blind chance. Theistic evolutionism is often thought of as evolution under the guidance of God. How can this be true if mutations are unguided?

When evolutionists say that mutations occur "at random," they do not mean that mutations are uncaused. Rather, their point is that mutations do not occur *because* they would be useful to the organisms in which they occur. It is well known that there are physical events, like radiation, that influence the probability of mutations. Some of these causes raise mutation rates at many loci. Others are more specific, raising the probability of some mutations but not of others. And there are parts of the genome, so-called "hot spots," that are especially vulnerable to mutation. Suppose that an organism in a given environment would benefit from having mutation M occur. There are physical facts about the organism and its environment that make this mutation have whatever probability it has. But this beneficial mutation is not more apt to occur because it would be beneficial. The causation goes through the physical facts about the organism and its environment, period. As a first pass, let's consider expressing the idea that mutations are undirected by saying that the physical causes of mutation (which encompass both the physical make-up of the organism and physical facts about its environment) *screen off* the mutation's occurring from whether or not it is beneficial:

(SO) $$\Pr(\text{mutation } M \text{ occurs in } O \text{ at } t_2 \mid E \text{ at } t_1) = \Pr(\text{mutation } M \text{ occurs in } O \text{ at } t_2 \mid E \text{ at } t_1 \ \&\ M \text{ would be beneficial to } O \text{ at } t_2).$$

Here E is a complete description of the physical facts about the organism and its environment at the earlier time t_1.

The trouble with SO is that it doesn't capture the thesis that there is no *physical mechanism* (either inside organisms or outside of them) that detects which mutations would be beneficial and causes those mutations to occur. The SO equality merely expresses the thesis that the physical facts at t_1 are *causally complete* with respect to the

[5] It is not in dispute that evolutionary theory conflicts with certain forms of Biblical literalism; in this respect, the theory is no different from theories in other sciences.

occurrence of mutations at t_2.[6] And why isn't it a "physical fact" that a mutation would help an organism to survive and reproduce? Some hints about how this formulation can be improved may be found in Lenski and Mittler's (1993, p. 188) description of what it means for a mutation to be directed:

> We define as directed a mutation that occurs at a higher rate specifically when (and even because) it is advantageous to the organism, whereas comparable increases in rate do not occur either (i) in the same environment for similar mutations that are not advantageous or (ii) for the same mutation in similar environments where it is not advantageous.

The basic idea here can be understood in terms of a simple example. Consider an organism that might live in a green or a red environment and suppose that protective coloration would provide it with an adaptive advantage; being green in a green environment and being red in a red environment would be good, but being green in a red environment and being red in a green environment would be bad. This means that there are two fitness peaks and two valleys in the table shown in Figure 3.

Figure 3: Four fitness values and their relationships. If protective coloration is advantageous to an organism, there are two fitness peaks and two valleys.

		Environment is: Red		Environment is: Green
	Red	w_1	$>$	w_2
Organism is		$\vee$		$\wedge$
	Green	w_3	$<$	w_4

Now suppose that the organism is blue and we are considering two mutations that it might experience; one will cause its offspring to be green and the other will cause its offspring to be red (I assume that parent and offspring live in the same environment).

[6] In Sober (1999) I defend a screening-off formulation of the thesis that physics is causally complete and discuss its relationship to the thesis of supervenience and also to the thesis that mental properties are causally efficacious.

These two mutations are directed only if inequalities among mutation probabilities *mirror* inequalities among the fitnesses given in Figure 3. The pattern is shown in Figure 4; peaks and valleys in fitnesses match peaks and valleys in mutation probabilities. I've introduced two small changes in Lenski and Mittler's formulation. First, I use the comparative concept of one mutation's *being more beneficial* than another rather than the absolute concept of a mutation's *being beneficial*. I've also used probabilistic inequalities rather than rate inequalities; this is because I think of rates as observations that provide evidence about probabilities, and it is the latter that characterize the relevant mechanisms. I trust it is clear how the pattern of mutation probabilities mirroring fitnesses can be generalized to more than two environments and more than two mutations.[7]

Figure 4: Four mutation probabilities. If the mutations are directed, inequalities among mutation probabilities must mirror the inequalities among the fitness relationships shown in Figure 3.

		Environment is		
		Red		Green
	Red	p_1	$>$	p_2
Organism mutates to		$\vee$		$\wedge$
	Green	p_3	$<$	p_4

Notice that the concept defined in Figures 3 and 4 is specific to an organism, a pair of mutations, and a pair of environments. This leaves it open that mirroring might hold in some organisms, but not in others. It also allows that there might be mirroring for some of the mutations a given organism experiences but not for others; it also is possible that mirroring might hold in some environments, but not others. Although the screening-off thesis SO is totally general, questions about the mirroring relation must inevitably be addressed piecemeal. And whereas SO is settled by a general commitment to a

[7] Consider the simplest genetic case – a site in an organism's genome has nuceleotide *G*, and we are considering the three mutations it might experience – to *A*, to *C*, and to *T*. There are six possible environments we might consider that are distinguished from each other by the ordering of the fitnesses of these three mutations. To check for mirroring, we'd need to consider 18 fitnesses and 18 mutation probabilities. The concept of perfect mirroring is defined as it was before, but we'd like also to have a concept of less than perfect mirroring as well. I won't address the question of how this should be understood.

physicalistic metaphysics, questions about mirroring are not settled by philosophy, but need to be addressed via specific empirical (preferably experimental) results.

It would not be very surprising if an organism, a pair of environments, and a pair of mutations were discovered that exhibit this mirroring relationship. Surely we can cherry-pick from a large data set to find a few examples of this sort. Biologists would not only decline to conclude from this that mutations are *generally* directed; I think they'd also decline to conclude that *these* mirroring mutations are directed. Mirroring is a necessary condition for a set of mutations to be directed; it isn't sufficient. What is missing from the relationship of mirroring is the word "because." What needs to be added is that more beneficial mutations have higher probabilities than ones that are less beneficial *because* this asymmetry is beneficial.[8] It is here that the concept of natural selection makes its appearance.

Prima facie, it seems clear that a mechanism that increases the frequency of advantageous mutations and reduces the frequency of deleterious mutations would be favored by natural selection.[9] Unfortunately, counting mutation events in laboratory populations of micro-organisms like *E. coli* is difficult. What one observes, in the first instance, is the *traits* of organisms; whether an organism's trait is due to a new mutation or is part of the standing variation in the population is often difficult to discern. This discrimination is critical if one wants to determine whether additional mutations occurred *after* a new selective environment was imposed. The history of this subject is one in which biologists have devised careful experiments, but then other biologists uncover subtle flaws in the experiment design or think of new hypotheses that account for the results that do not require the hypothesis of directed mutation. It is to be expected that new and better experiments will be made. But, for now, the hypothesis that mutations are guided should be viewed with skepticism. True, the ability to direct mutations would be favored by natural selection if it were present in the population. But that does not mean that it ever was present as a variant. Selection can't cause a trait to increase in frequency unless it is exemplified in the first place.[10]

The thesis that mutations are undirected is sometimes presented as a rock-bottom "philosophical" commitment of evolutionary biology, akin to materialism and just as central to the scientific worldview. This is a mistake. The thesis is certainly central to

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[8] I'm here defining what it means for mutations to be directed towards what is good for organisms. One also could consider other, less benign, forms of directedness. For a more detailed discussion of what directed mutation means, see Jablonka and Lamb (2005) and Sarkar (2005).

[9] I say "prima facie," because it may be that there are negatives associated with such a mechanism (e.g., if it imposes a huge energetic cost on the organism) so that, on balance, it would not be selectively favored over alternatives.

[10] There are theoretical dimensions to the question of directed mutation that are worth considering. For example, many organisms are capable of *phenotypic plasticity* (Pigliucci 2001). An iguana can change its color without needing to mutate. In what circumstances will directed mutation provide advantages that phenotypic plasticity cannot? One difference might be that mutation in an adult organism often leaves its own phenotype unchanged will altering the phenotype of its offspring whereas phenotypic plasticity changes the adult's own phenotype. An iguana that lives in a highly changeable environment may do better with the latter than it would do with the former.

biology, but it is important to see that it has nothing to do with materialism or theism. It is like the thesis that an organism can't synthesize vitamin *D* from sunlight or can't regenerate lost limbs. It is an empirical question whether the mirroring relation obtains for a given organism with respect to a given set of mutations in a given range of environments. If it does, that is something that evolutionary biology needs to explain. Mutation rates vary among and within species and it is a good question why they do so. Explaining patterns of variation in mutation rates is a problem *within* evolutionary biology, not a *challenge to* evolutionary biology.

I conclude that the fact that mutations are undirected is not a problem for theistic evolutionism. Maybe God arranged for mutations to be undirected. And if some mutations do turn out to be directed in the sense described here, that is no threat to atheism. Atheism has no more of a stake in mutations being undirected than it has in organisms being unable to synthesize vitamin *D* from sunlight.

Ernst Mayr (1961) emphasized the importance of distinguishing proximate from ultimate causation. Mayr's point can be illustrated by considering the question of why sunflowers turn towards the sun. One answer to this question cites the proximate mechanism inside each sunflower that causes it to do so; the other points to the reason the trait evolved, presumably natural selection. There is no conflict between these two "levels" of explanation. The theistic evolutionist should embrace Mayr's pair of concepts and add a third, maintaining that there is, in addition, a still more distal level of divine causation. God can direct the evolutionary process in an ultimate sense, though mutations are undirected in a proximate sense. Biology says nothing about the former and theism says nothing about the latter.

Evolution and Hidden Variables

If theistic evolutionism is correct in its claim that the only pathway from God to the adaptations we observe passes through the evolutionary process, this means (if there is no action at a distance) that the evolutionary process *screens off* God from evolutionary outcomes:

$$\Pr(\text{adaptations we observe} \mid \text{evolutionary process}) = \Pr(\text{adaptations we observe} \mid \text{evolutionary process \& God's choices}).$$

This should not be taken to mean that evolutionary *theory* does the screening off. The theory is probabilistic; it does not say of itself that it is *causally complete*. The theory is consistent with there being *hidden variables*, natural *or* supernatural. For example, the theory is consistent with determinism at the micro-physical level (Sober 2008c).[11] This means that even if a probabilistic evolutionary model says that some possible outcome has a non-extreme probability, a complete physical model may say that the outcome's probability is 0 or 1. Evolutionary theory is silent on this question about physics.

[11] Compare Brandon and Carson (1996), who argue that evolutionary biology shows that there are no such hidden variables.

To get a feeling for the distinction between (a complete description of) the evolutionary process's screening off and evolutionary theory's doing so, consider the process of Mendelian inheritance. It is generally thought that this process is Markovian – that grandparents influence the genotypes of their grandchildren only by way of influencing their children. There is no action at a temporal distance in genetic transmission. However, this fact about the process does not mean that there can't be models in population genetics in which the state of generation 3 depends not just on the state of generation 2 but on that of generation 1 as well. For example, in the theory of inbreeding, when all matings are between sibs, the heterozygosity (H_t) in generation t depends on the heteroyzgosity in the previous two generations, t-1 and t-2 (Crow and Kimura 1970, p. 87):

$$H_t = (1/2)(H_{t-1}) + (1/4)(H_{t-2}).$$

The *process* is Markovian, but *models* of the process can fail to be; this just means that the models are causally incomplete.

It is consistent for theistic evolutionists to claim that there is more going on in the evolutionary process than is dreamt of in evolutionary biology, just as a friend of determinism can consistently maintain that there is more involved in the process of flipping a coin than is described in a simple probabilistic model:

$$\Pr(\text{coin lands heads at } t_2 \mid \text{the coin is tossed at } t_1) = 0.5.$$

This probability statement is entirely consistent with the truth of another:

$$\Pr(\text{the coin lands heads at } t_2 \mid \text{a complete description of the initial conditions at } t_1) = 0 \text{ or } 1.$$

That both can be true is no more puzzling than the fact that *X* doesn't entail *Z* while *X&Y* does. Asking what the true probability is of *Z* makes no sense. The question is incomplete; the proper question is -- what is the probability of *Z*, given this or that other proposition?

A probabilistic model of coin tossing is consistent with the thesis that the system is deterministic. If determinism is true, there are hidden variables, not represented in the probability model, which turn all the probabilities into 0's and 1's when their values are taken into account. In just the same way, a probabilistic model of the evolutionary process is consistent with the thesis that the process is deterministic. If determinism is true, there are hidden variables that affect the evolutionary process. Evolutionary theory says nothing about whether such hidden variables exist. It therefore says nothing about whether there are supernatural hidden variables.

Does this mean that evolutionists overstep what they know when they say that evolution is an undirected process? No more than experts on gambling devices overreach themselves when they say that a roulette wheel obeys a certain probability model. In both cases, there is evidence that favors the probability model in question over competitors -- *this* is what is behind the scientific pronouncement. Those who hold that everything that happens is due to the sustaining power of God can accept what experts say about gambling devices; they should take the same view of what evolutionists say about the evolutionary process.

Theistic evolutionists can of course be deists, holding that God starts the universe in motion and then forever after declines to intervene. But there is no contradiction in their embracing a more active God whose postCreation interventions fly under the radar of evolutionary biology. Divine intervention isn't part of science, but the theory of evolution does not entail that none occur.

Ockham's Razor and Theistic Evolutionism

Creationists have been hostile to theistic evolutionism. The same is true of those who prefer to call themselves Intelligent Design Theorists. Here is what William Dembski (1998, pp. 20-21) says about the position:

> … intelligent design is incompatible with what typically is meant by theistic evolution. Theistic evolution takes the Darwinian picture of the biological world and baptizes it, identifying this picture with the way God created life. When boiled down to its scientific content, however, theistic evolutionism is no different from atheistic evolutionism, treating only undirected natural processes in the origin and development of life…
>
> If theistic evolution finds no solace from intelligent design, neither does it find solace from the Darwinian establishment. For the Darwinian establishment the theism in theistic evolutionism is superfluous. For the hard-core naturalist, theistic evolution at best includes God as an unnecessary rider in an otherwise purely naturalistic account of life. Thus by Ockham's razor, since God is an unnecessary rider in our understanding of the natural world, theistic evolution ought to dispense with all talk of God outright and get rid of the useless adjective *theistic*. This is the received view within the Darwinian establishment.

This remark does not provide an *argument* that theistic evolutionism is false, but expresses a *motive* that Dembski and others have for wanting creationism to be the only game in town.

Dembski and other creationists are worried that the hypothesis that God exists will be rendered superfluous by science. If the God hypothesis isn't needed to explain the adaptive features of organisms, there is no biological reason to think that there is such a being. If the same thing happens in the other sciences, there will be no scientific reason to think that there is a God. By "reason," I mean evidence. Ockham's razor then seems to tell us to slice away the proposition "God exists" from our corpus of beliefs. This slicing away will result in agnosticism or atheism, depending on how the principle of parsimony is formulated.

There are two ways in which a theistic evolutionist might answer this argument. The first is to point out that all scientific explanations rely on a description of a system's initial conditions (Swinburne 2004). This is true in physics just as much as it is true in biology. If the universe isn't infinitely old, there is such a thing as the initial state of the universe, and no scientific explanation explains the character of that initial state. Does the God hypothesis thereby explain something that would otherwise be inexplicable? Atheists may complain that this initial state does not need to be explained, or that the God hypothesis in fact offers no genuine explanation, or that the hypothesis that God caused the initial state is untestable. All these options are worth considering. Evolutionary theory has nothing to say about them.

Theistic evolutionists can reply to the argument from Ockham's razor in a second way. They can point out that the principle of parsimony is part of the scientific method, but that they don't think that one needs to justify belief in God by appeal to the scientific method. This is the simple idea that belief in God is a matter of faith, not evidence. Atheists will probably regard this reply as an abdication, perhaps taking the side of W.D. Clifford in his disagreement with Williams James. The debate between these two philosophies is – no surprise – *philosophical*, not *biological*. The theory of evolution has nothing to say about this, either.

Numbers

The debate over evolutionary theory versus creationism is often assimilated to the conflict between two philosophies – naturalism and supernaturalism. Both are metaphysical doctrines. The former holds that the only objects that exist are objects *in* nature; the latter denies this, claiming that at least some objects exist *outside* of nature – they are *super*natural. Nature, for our purposes, can be understood as the totality of things located in space and time. We've just seen that when God is the supernatural entity in question, it is a mistake to think that evolutionary theory is committed to naturalism in the metaphysical sense just described. But there is a second issue about metaphysical naturalism; it comes from the philosophy of mathematics, not the philosophy of religion, and it raises different issues. Here a case can be made, not that evolutionary theory is *silent* on the issue of naturalism, but that it actually entails that metaphysical naturalism is *false*.

Mathematical Platonism says that mathematical statements describe objects that exist outside of space and time. These objects are said to exist independently of human thought and language. Even though there would be no *names* for numbers *or mental representations* of mathematical facts if there were no intelligent beings, the numbers themselves would still exist and mathematical propositions would still be true. Not only are mathematical truths independent of whether there are beings with mind and language;

they also are independent of whether there are any material objects. For example, the prime number theorem – that there exist an infinite number of prime numbers -- does not depend for its truth on how much mass there is in the universe or on how that mass is distributed. In view of the fact that mathematical propositions are true independent of what is going on *inside of* nature, it is natural to think that those propositions describe objects that exist *outside of* space and time. I don't want to give the impression that mathematical Platonism is *obviously* true. Some serious challenges have been raised. For example, Benacceraf (1973) has argued that if mathematical propositions are true or false in virtue of facts about such Platonic entities, that it would be impossible for human beings to have mathematical knowledge. But we clearly do have some mathematical knowledge. And Field (1980, 1989) has argued that some portions of mathematics can be reinterpreted in such a way that their subject matter consists entirely of things that exist inside of space and time. He also has suggested that the parts of mathematics that are irreducibly Platonistic in their commitments should be regarded as useful fictions. These challenges are serious and interesting, but they should not lead us to conclude that mathematical Platonism is obviously mistaken. If this metaphysics is false, it is not easy to refute, and the alternative positions have problems of their own.[12]

What has this question in the philosophy of mathematics to do with evolutionary theory and its metaphysical commitments? If mathematical Platonism is true as an account of *pure* mathematics, then it also is true as an account of *applied* mathematics. Evolutionary theory became a mathematical subject in the 20th century, thanks initially to work in population genetics. Mathematical evolutionary theory makes claims about genes, organisms, and populations, but it also makes claims about numbers. In this respect it is just like mathematical physics. Consider, for example, a simple one-locus two-allele model of natural selection in which heterozygote superiority entails that the population will evolve to an equilibrium gene frequency in which both alleles are maintained. The mathematical content and Platonistic commitments of this model become clearer when we formalize this implication of the model as follows:

> For any diploid population, and for any locus in that population in which there are two alleles (*A* and *a*), and for any frequency f ($0 < f < 1$) that the *A* allele has in the population at time t=0, and for any three fitness values p_1, p_2, and p_3 that the three genotypes (*AA*, *Aa*, and *aa*) might have, if $w(AA)=p_1 < w(Aa)=p_2 > w(aa)=p_3$, then there exists an equilibrium frequency e (where $0 < e < 1$) such that $f \rightarrow e$ as $t \rightarrow \infty$.

Here the fitness value of a genotype is the probability that an organism with that genotype has of surviving from egg to adult. This model quantifies over numbers -- probabilities and frequencies. If Platonism is the right philosophy of mathematics, evolutionary theory is incompatible with naturalism.

[12] See Papineau (1993) for discussion of this issue in the philosophy of mathematics within the larger context of naturalism.

Figure 6: The odds version of Bayes' Theorem.

$$\frac{\Pr(H_1 \mid E)}{\Pr(H_2 \mid E)} = \frac{Pr(E \mid H_1)}{Pr(E \mid H_2)} \times \frac{Pr(H_1)}{Pr(H_2)}.$$

ratio of posterior probabilities	likelihood ratio	ratio of prior probabilities

Although mathematized evolutionary theory entails that there are numbers, the empirical evidence for evolutionary hypotheses does not provide evidence that numbers exist. Here we are back to the earlier point about the falsity of Hempel's special consequence condition. One way to see what is going on here is to think about evidence in terms of the Law of Likelihood (Hacking 1965, Edward 1972, Royall 1997):

> *The Law of Likelihood.* Evidence E favors hypothesis H_1 over hypothesis H_2 precisely when $Pr(E \mid H_1) > Pr(E \mid H_2)$.[13]

The Law of Likelihood is central to Bayesianism; for the Bayesian, it describes the one and only avenue through which observational evidence can modify one's degrees of belief. The odds version of Bayes' Theorem (Figure 6) shows why. Notice that the ratio of the posterior probabilities can differ from the ratio of priors only if the likelihoods of the hypotheses are different. And the more the likelihood ratio deviates from unity, the more the ratio of posterior probabilities will differ from the ratio of priors. The likelihood ratio represents the power that the observations have to transform one's degrees of belief.[14]

The Law of Likelihood suggests the general thesis that testing in science is typically *contrastive.*[15] To test a hypothesis H, you must test H *against* some alternative.

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[13] I here omit the second clause in the Law of Likelihood, which says that degree of favoring is to be measured by the likelihood ratio. This additional claim won't be relevant in what follows.

[14] You don't have to be a Bayesian to find merit in the Law of Likelihood; likelihoodists hold that the principle describes how evidence should be interpreted even when you are unwilling to assign prior or posterior probabilities to the hypotheses under test.

[15] Dretske (1972) is the *locus classicus* for the argument that a number of philosophically interesting concepts are contrastive.

There are two possible exceptions to this pattern; each arises when a hypothesis and a set of observations are deductively related. If *H* entails *E* and you see that *E* is false, it follows that *H* is also false, there being no need to consider alternatives to *H* along the way. And if *E* entails that *H* is true, and you see that *E* is true, it follows that *H* is true: no need for contrasts here, either. Neither of these patterns has much purchase in the world of scientific testing. The uselessness of the second pattern is especially clear – rarely are the hypotheses that scientists want to test deducible from the observations. It may be thought that a point due to Duhem (1914) and Quine (1953) vitiates the interest of the first pattern. Their logical point is that, typically, you need auxiliary propositions to bring the hypothesis you wish to test in contact with observations. It isn't *H* that entails *E*, but the conjunction *H&A* that does so. The point is right, but it doesn't show that testing must be contrastive. If you know that *A* is true, and if the conjunction *H&A* entails *E*, and *E* turns out to be false, you can deduce that *H* is false; hypotheses alternatives to *H* play no role. The real reason that testing almost always has to be contrastive is that hypotheses, with independently supported auxiliary assumptions in place, almost always confer non-extreme probabilities on the evidence. *H* doesn't entail *E*, but neither does *H&A*.

Not only is testing almost always contrastive. In addition, testing in science typically pits different empirical theories against each other that make use of the same mathematical resources. This shared mathematical background isn't tested, but assumed. When evidence discriminates between two empirical theories H_1 and H_2 that use the same mathematical framework *M*, the usual pattern is that the evidence says nothing about whether *M* is true. This is because *M* has not been tested against an alternative mathematics *M**; in fact, the usual situation is that no such alternative mathematics has even been formulated.

Although the Law of Likelihood represents testing as a contrastive enterprise, this point about testing is not unique to likelihoodism or Bayesianism. NonBayesian inferential frameworks are used in science and they have the same character. For example, model selection theory is a part of statistics in which the hypotheses under evaluation contain adjustable parameters (this is what the word "model" means in this branch of statistics). Consider two evolutionary hypotheses concerning the two alleles *A* and *B* that exist at a genetic locus in a population that you are studying. One hypothesis says that the genes of interest differ in fitness and that their fitnesses are independent of their frequencies:

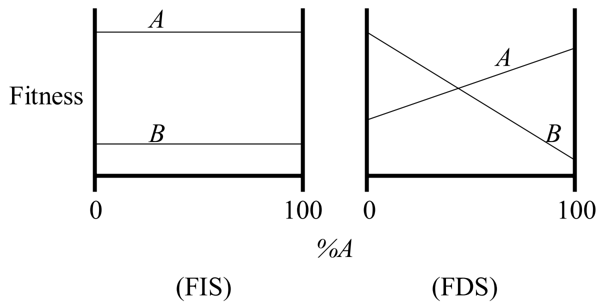
(FIS) There exist numbers w_1 and w_2 such that $w(A) = w_1$ and $w(B) = w_2$.

The other model says that each of the alleles has a frequency dependent fitness, where an allele's fitness value linearly depends on how common or rare it is:

(FDS) There exist numbers m_1, m_2, b_1 and b_2 such that for any frequency *f* of the *A* allele, $w(A) = m_1f + b_1$ and $w(B) = m_2f + b_2$.

Instances of the two models (with adjustable parameters replaced by constants) are depicted in Figure 7.

Figure 7. Instantiations of the frequency independent selection (FIS) and the frequency dependent selection (FDS) models.



One influential idea in model selection theory is called the Akaike Information Criterion (AIC). It scores a model by using data to obtain maximum likelihood estimates of its adjustable parameters. Models like FIS and FDS, in virtue of their existential quantifiers, are infinite disjunctions; each disjunct is a "member" of the model. By fitting a model *M* to the data, one obtains the proposition *L(M)*, which is the member of the model that makes the data most probable. AIC takes the log-likelihood of *L(M)* and then subtracts a penalty for complexity (= the number of adjustable parameters):

The AIC score of model $M = \log\{\Pr[\text{data} \mid L(M)]\} - k$.

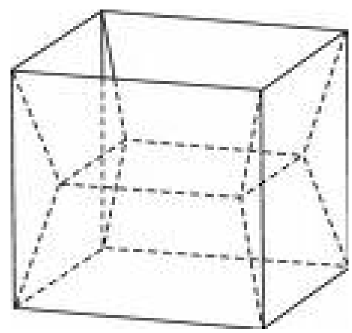
Notice that AIC does not compute the likelihood of the model *M*, but of the fitted model *L(M)*. And it doesn't use the likelihood of *L(M)* as the model's score, but instead subtracts *k*; models that differ in their complexity have different penalties imposed. What is of interest about AIC is not a model's *absolute* score, but *comparisons* of the scores of different models (Sakamoto *et al.* 1986, p. 84). AIC and the Law of Likelihood are different approaches to representing what the evidence is saying,[16] but both are contrastive.

In our example, the models FIS and FDS have different numbers of adjustable parameters. Data drawn from the population concerning the birth and death rates found in a sample of organisms allow us to decide which model has the better AIC score.

[16] AIC provides an estimate of a model's predictive accuracy, while the Law of Likelihood describes how one should decide whether the data favor the hypothesis that M_1 is true over the hypothesis that M_2 is true. Models known to be false can be good predictors; in fact, a model known to be false can sometimes be known to be a better predictor than a model known to be true. For further discussion, see Sober (2008).

However, the observations that allow us to assign AIC scores to FIS and FDS do not permit us to assign AIC scores to the propositions "numbers exist" and "numbers do not exist." These propositions are not "models" in the sense of that term used in statistics. According to this body of statistical theory, the data say nothing about whether numbers exist. AIC is a procedure for scoring *models*, but not every consequence of a model is a model.

Figure 8: One of Plateau's soap-bubble experiments provided evidence pertaining to the mathematical question of what the surface of least area is that can be inscribed inside a cube.



I am not here endorsing the false thesis that pure mathematics is never empirically tested. When Plateau dipped his wires into soap suds, he was performing an empirical test of various mathematical propositions (Courant and Robbins 1941, pp. 385-391). The mathematical questions he addressed have the form – what is the surface of least area that is bounded by a given closed curve? See the example depicted in Figure 8. The physics of how soap bubbles form in wire frames allowed Plateau to address his questions experimentally. The same pattern occurs now when mathematicians rely on computer assisted proofs (Tymoczlo 1979) and do computer simulations (Easwaran 2008); the output on the computer screen provides observational evidence as to whether a given mathematical conjecture is true. Perhaps the use of paper and pencil also should be regarded as an empirical source of information in mathematical reasoning; the same goes for the mathematical propositions we believe on the basis of testimony. Propositions in pure mathematics are sometimes empirically tested. However, the point remains that testing empirical hypotheses like FIS and FDS that have mathematical commitments typically don't test those mathematical commitments.

In the Jack of Hearts example I gave at the start of this paper, the hypothesis of interest is a conjunction that gets confirmed because one of its conjuncts is confirmed but the other is not. Is this a good model for separating the metaphysical and empirical elements in mathematical evolutionary theory, and, by implication, in other scientific theories? Here it is worth considering Friedman's (2001, pp. 35-36) contention that Newtonian physics should not be viewed as a conjunction of pure mathematics and empirical elements:

> … there are fundamental asymmetries in the way in which different elements of this Newtonian synthesis actually function. Consider, for example, the relationship between the mathematics of the calculus and the Newtonian formulation of the laws of mechanics. Newton's second law of motion (in only slightly anachronistic form) says that force equals mass times acceleration, where acceleration is the instantaneous rate of change of velocity (itself the instantaneous rate of change of position); so without the mathematics of the calculus … this second law of motion could not even be formulated or written down, let alone function to describe empirical phenomena. The combination of the calculus plus the laws of motion is not happily viewed, therefore, as a conjunction of elements symmetrically contributing to a single total result. For one element of a conjunction can always be dropped while the second remains with its meaning and truth-value intact. In this case, however, the mathematics of the calculus does not function simply as one more element in a larger conjunction, but rather as a necessary presupposition without which the rest of the putative conjunction has no meaning or truth-value at all. The mathematical part of Newton's theory therefore supplies elements of the language or conceptual framework, we might say, within which the rest of the theory is then formulated.

Notice that the conjunction that Friedman considers has all the mathematics in one conjunct. I agree that this isn't plausible, since expressing the empirical content of the theory requires mathematics, just as Friedman says. What I have in mind, rather, is another sort of division, in which ontological commitments to numbers and physical objects are segregated from the rest of the theory:

(*) Numbers and physical objects exist & if numbers and physical objects exist, then …

There will be mathematics in the second conjunct and talk of physical objects as well. If we think of the population genetic models FIS and FDS in this format, we can see that testing them against each other really involves testing the second conjunct of FIS against the second conjunct of FDS. I am not suggesting that the first conjunct of (*) exhausts the metaphysics of evolutionary theory; nor am I claiming that "metaphysical elements" always take the form of existence claims.[17] What I hope to have described is a heuristic for isolating some of a theory's metaphysical commitments. The onion can be peeled layer by layer.

[17] Consider, for example, the question of whether evolutionary theory shows that taxa don't have essences.

Some Objections

I have argued for the following: when an empirical theory makes indispensable use of a body of mathematics, it is a mistake to conclude that the empirical evidence for the theory must also provide empirical evidence for the math. This is a criticism of the indispensability argument for mathematical Platonism made famous by Quine (1953) and Putnam (1971).[18] My simple point is that the indispensability argument is wrong because the special consequence condition is wrong (Sober 1993b). The indispensability argument has been criticized on others grounds -- that it doesn't explain the obviousness of some mathematical propositions (Parsons 1980) and that it doesn't allow there to be reasoning internal to pure mathematics that helps one decide which mathematical propositions are true (Maddy 1992). Although these objections are telling if the indispensability argument purports to identify the *only* pathway by which we gain evidence for mathematical propositions, the argument need not be thought of in that way. These objections do not count against the thesis that indispensability in a well-confirmed empirical theory is *one* such pathway (Hellman 1999, p. 32). It is this more modest thesis that I have challenged.

I now want to consider a few criticisms that have been made of my criticism of the indispensability argument.

Hellman (1999, p. 30) concedes that the special consequence condition is often wrong, but maintains that the principle is right for certain key mathematical axioms (e.g., a weak form of the axiom of infinity that holds that an infinite totality exists or is logically possible). They have Bayesian priors that are less than unity, he says, and their probabilities are increased by the fact that they are used in well-confirmed empirical theories. We can return to Figure 1 to see what Hellman's thesis of probability boosting requires. If O confirms H and H entails C, for O to confirm C, it isn't enough that $Pr(C) < 1$. Of course, if $Pr(C) < Pr(H \mid O)$, that would suffice for O to confirm C (Kotzen 2007 discusses this sufficient condition). The problem is that it isn't clear why agents should arrange their probabilities in this fashion.

Hellman (p. 30) presents a second consideration, which has nothing to do with the Bayesian concept of confirmation as probability raising. Rather, the fact of interest is that probability obeys a rule of weak monotonicity: if H entails C, then $Pr(H \mid X) \leq Pr(C \mid X)$, no matter what X is. Hellman suggests that weak monotonicity vindicates the indispensability argument. I am skeptical. Notice first that weak monotonicity holds for *any* consequence that H has, whether or not it is mathematical and whether or not it is indispensable; H is related to *each* of its consequences in the way described. Furthermore, although weak monotonicity says that $Pr(H \mid X)$ is a lower bound for $Pr(C \mid X)$, it also says that $Pr(C \mid X)$ is an upper bound for $Pr(H \mid X)$. Hellman seems to be thinking of Bayesian agents who are very confident in the truth of a mathematized theory and extract from this a confidence in the embedded mathematics that is at least as great. But we also should consider Bayesian agents who are not at all confident that numbers exist and who therefore are at least as unconvinced of the mathematized theories that

[18] See Colyvan (2004) for a useful formulation of the argument and a survey of criticisms.

entail that there are such things. Montonicity says nothing about which thinkers have their heads screwed on better. Many of the scientists I know get impatient when they are asked how sure they are that numbers exist. They regard this as a philosophical question on which they needn't take a stand. When it is pointed out to them that the theories they embrace entail that numbers exist, they reply with surprise that they never thought about that part of the theory. Their confidence, such as it is, is focused on their theories' other parts.

Hellman (1999, p. 33) suggests a third reconstruction of the indispensability argument. Suppose a theory *T* successfully predicts some observations about which its competitors are silent. Suppose further that *T* makes use of a strong body of mathematics *M*, but that its competitors use only the weaker mathematics *M'*. Hellman thinks that in this case the observations not only confirm *T*; they also confirm *M*. The epistemological picture that Hellman is painting here is not in keeping with the law of likelihood, as he notes. In fact, it isn't in keeping with Bayesianism, either. For Bayesians, *O* confirms *H* only if $Pr(O \mid H) > Pr(O \mid notH)$; the idea that *O* confirms *H* even though no competitor says anything at all about *O* is *impossible*. Although Hellman's first two arguments are couched in a Bayesian framework, this third argument ventures beyond it. More needs to be said about this new epistemology before this proposal for recasting the indispensability argument can be evaluated.

Other critics of my criticism of the indispensability argument also have resisted seeing it through the lens of the Law of Likelihood. Colyvan (2001, p. 128) says that the epistemological framework provided by that principle is a "straightjacket" because it precludes the comparison of empirically equivalent theories on the basis of nonempirical criteria like simplicity and elegance. He is right that the epistemology described in Sober (1993a) has this consequence. The point I would make here is that invoking *nonempirical* criteria to help one evaluate empirically equivalent theories does not rescue the indispensability argument for mathematical Platonism. That argument maintains that the *empirical* evidence for a scientific theory also provides *empirical* evidence for the pure mathematics it uses. If there are *nonempirical* reasons for thinking that numbers exist, that is beside the point. I also see a difference between the Quine/Putnam indispensability argument and what Resnik (1997, pp. 46-47) calls "the pragmatic indispensability argument." Resnik's argument has nothing to do with a specific theory's empirical confirmation providing empirical confirmation of the mathematics it embeds. Rather, Resnik takes the fact that we need to use mathematics to do science to show that we are justified in thinking that various mathematical propositions are true. I have no quarrel with Resnik's thesis about pragmatic justification if the point is that scientists aren't being irrational when they assume a mathematical framework. But our being pragmatically justified does not mean that the empirical evidence we have for a theory also provides empirical evidence for the mathematics it embeds.

Philosophers sometimes think of the following argument when they consider the Quine/Putnam indispensability argument:

One of the theories T_1, T_2, …, T_n is true.
Each theory T_i entails M.

M is true.

If M is common ground among all the competing theories that scientists at a time take seriously, isn't that a good reason for them to think that M is true? After all, you can *deduce* that M is true if you are prepared to assert that the disjunction of these competing theories is true. This is often a big *if*. Scientists do their tests by considering a set of competing hypotheses, but those competitors often fail to be exhaustive. So the first premise of this argument will often be false. In addition, the valid argument displayed above makes no mention of empirical evidence. The fact that the special consequence condition is false applies here, as it did before, and the fact that probability is weakly monotonic is of no help, either.

Colyvan (2001, 2004), Hellman (1999), and Resnik (1995, 1997) each reject a symmetry principle that I recommended in my 1993 article. If a body of pure mathematics gets confirmed by its being used in empirically successful theories, why doesn't it get disconfirmed by its being used in scientific theories that are empirical failures? Think of all the empirically unsuccessful theories that embed the calculus! These three friends of indispensability reject this symmetry principle because they think that the mathematics used in science is confirmable but not disconfirmable. It is worth remembering that, according to Bayesianism, this is *impossible*; O confirms H if and only if *notO* would disconfirm H. Colyvan, Hellman, and Resnik endorse something like the mirror image of Popper's asymmetry thesis that scientific propositions are falsifiable but not verifiable. Even though Bayesianism should not be taken on faith, the question remains of how a plausible confirmation theory can avoid a symmetry of confirmation and disconfirmation.

Hellman, Resnik, and Colyvan answer by invoking Quine. If a mathematized empirical theory makes successful predictions, it is confirmed and so are its mathematical consequences. If the theory makes unsuccessful predictions, we know that something in the theory needs to be revised. Quine's principle of minimum mutilation then counsels that we revise the nonmathematical parts; we don't reject the math because the math is centrally located in our web of belief. This is not the place to develop a sustained evaluation of these ideas, but I will make a few comments. The first part of this picture needs to be spelled out so that it avoids a wholesale embracing of the special consequence condition. The second uses a dichotomous notion of acceptance, not a notion of evidence that is a matter of degree. And both see testing in terms of the black and white of predictive success and failure. Many have found this Quinean picture suggestive; it remains to be seen whether a plausible and precise epistemology can be developed along these lines.

Colyvan (2001, p. 134) defends the idea that evidence for a theory is evidence for all the theory's parts by way of an intriguing analogy. He suggests that a scientific theory is like a computer program. When a computer program "works" in a series of tests, this is evidence that all its parts are bug-free. Similarly, when a scientific theory "works," this is evidence that all its parts are true, including the mathematical parts. The analogy is questionable. Perhaps the parts of a typical computer program are related in a special way; maybe none of the parts are *idle*, in that each must play its role if inputs are to yield the right outputs. In contrast, it is well known that theories can be constructed that contain idle postulates. Just conjoin a routine scientific theory with the statement "undetectable angels exist." The predictive success of the conjunction is hardly evidence that the second conjunct is true. It also is well known that a theory can be empirically successful even when it is known to contain idealizations; its success in that case does not provide evidence that each of its parts is true. These points underscore the fact that Colyvan's analogy is only an analogy. It needs to be cashed out via a principle of confirmation. There is ample reason to grant that the special consequence condition is false, and the fact that the consequence of interest happens to be a proposition of pure mathematics does not automatically rescue the principle.

Three Kinds of Naturalism

I have described naturalism as a metaphysical doctrine, but an alternative formulation, methodological naturalism, has also been much discussed in connection with the conflict between evolutionary theory and creationism (Numbers 2003). I want to add a third version of naturalism to this familiar pair. Here are all three:

> *Metaphysical Naturalism*: The only entities that exist are in nature.
>
> *Methodological Naturalism*: The only entities that scientific theories should postulate are in nature.
>
> *Evidential Naturalism*: The only entities whose existence we have evidence for are in nature.

Evolutionists who criticize Creationism often emphasize that methodological naturalism doesn't entail metaphysical naturalism and that science is committed only to the former. I agree that the one doesn't entail the other, but I disbelieve them both. In fact, I think that all three naturalisms are flawed.

Methodological naturalism is often defended by appeal to the thesis that claims about supernatural entities cannot be tested (Pennock 1999). When testability is understood in the usual way, this is an overstatement. Consider, for example, the claim that

> A supernatural intelligent designer caused everything in nature to be purple.

This statement about a supernatural being *is* testable. It is refuted by an observation statement -- that my car isn't purple. A more promising way to develop the idea about testability is to think of testing contrastively. Consider, for example, the hypothesis that

(ID) A supernatural intelligent designer created the vertebrate eye.

Suppose we want to test this ID hypothesis against the hypothesis that

(NS) Natural selection created the vertebrate eye.

Both hypotheses entail that vertebrates have eyes, and so both are testable in the usual, noncontrastive, sense of that term. But notice that the fact that vertebrates have eyes does not *discriminate* between ID and NS. To solve this discrimination problem, we need a set of observations that renders the two hypotheses unequally likely. So let's consider the observation that the vertebrate eye has features *F*. Is this outcome more probable if ID is true, or if NS is true? The problem is that we can't assess what probability the ID hypothesis confers on this observation unless we have independently justified information about the goals and abilities the putative designer would have if there were such a being. And we have no such information (Sober 2008b, Chapter 2).

If ID can't be tested against NS, it is equally true that NS can't be tested against ID. What's poor evolutionary theory to do then, if creationism isn't able to put up its dukes? The answer is that evolutionary biology allows one to formulate multiple hypotheses about the vertebrate eye and to test these against each other. One alternative to natural selection is drift. Talk of testing evolutionary theory is a bit like talk of testing chemistry. Evolutionary biology is a *field*. There are many possible evolutionary explanations of a particular trait's presence in this or that group of organisms and biologists test these hypotheses against each other.

Just as methodological naturalism doesn't entail the metaphysical variety, it also is true that metaphysical naturalism does not entail the methodological version. This independence thesis seems to fly in the face of a simple argument. If there are no supernatural entities, then scientific theories that postulate such things are false. And scientific theories shouldn't include falsehoods. QED.

One flaw in this line of reasoning becomes clear when we reflect on the fact that scientists aren't making a mistake when they formulate and test theories that contain idealizations (Maddy 1992). Physicists construct models that postulate point particles, spherically symmetrical planets, and frictionless planes; evolutionary biologists construct models that postulate infinitely large populations. Why do scientists bother to do this if they know in advance that such theories are false? One justification comes from the fact that idealizations are often known to be *harmless*: they don't much affect the predictions that the theory makes (Sober 1993b). If *I* is a harmless idealization of a more realistic theory *T*, then you can see how accurately *T* predicts by seeing how accurately *I* predicts. And if you think that *T* is true, you might as well use *I* to make your predictions. None of this should give pause to scientific realists.[19]

[19] However, there is another reason to formulate and test hypotheses that are known to include idealizations – namely, that we want to find theories that make accurate predictions about observations and that all the

If metaphysical naturalism is true, then there are no numbers, at least not as Platonistically conceived. But this doesn't mean that scientists should abstain from formulating and testing hypotheses that entail that numbers exist. Science *needs* to postulate numbers, whether or not such things really exist. So even if we *knew* that metaphysical naturalism is true, that would not oblige us to play by the rules of methodological naturalism. This line of reasoning requires that we recognize that there is an ambiguity in talk about "the things that science needs to postulate." There are the things that scientists are entitled to *believe* exist. And there are the things that can be postulated in the models that scientists *use* without fully believing them. A useful fiction known to be such can't be postulated in the first sense, but it obviously can be in the second. So metaphysical naturalism does not entail methodological naturalism, where the latter prohibits formulating and testing theories that entail the existence of numbers, and using some of them to make predictions. And if a theory can be explanatory without being 100% true, then theories with false metaphysics can even be explanatory.

Although the legitimate use of useful fictions is enough to show that metaphysical naturalism doesn't entail methodological naturalism, it is important not to exaggerate the similarity between idealizations in science and science's use of mathematics. If Platonism is false, then frictionless planes and numbers have in common their nonexistence. But still, there is a world of difference. For one thing, numbers aren't just *useful*; they are *indispensable*. As Friedman notes, we use mathematical language in describing our observations, and theories that hope to do justice to those observations must quantify over numbers. In contrast, most idealizations in the sciences are, in an important sense, dispensable. Physicists can write models in which balls experience friction when they roll down incline planes and geneticists can write models in which finite population size is taken into account. These models are more complicated, but they are not beyond our ken. Maddy (1992) points out that idealized models are often more mathematically tractable, but that does not mean that we are hopelessly in the dark without these idealizations. Another difference between frictionless planes and numbers is that we have empirical evidence that planes aren't perfectly frictionless. However, we have no empirical evidence that there are no numbers (Hellman 1999, p. 31).

There are two senses of indispensability. Electrons are indispensable in the electron theory if they aren't *idle postulates* within that theory – if, in using the theory to predict and explain, we need to use the hypothesis that electrons exist and have the various properties that the theory describes. However, electrons are not indispensable *in theories that compete with the electron theory*. This is why we can *test* whether electrons exist. Numbers are different. They are indispensable in the electron theory, and they also are indispensable in theories that compete with the electron theory by making different quantitative predictions. Electrons are indispensable *within* a theory, but numbers are indispensable *across* theories. This is why evidence for the electron theory can provide evidence for the existence of electrons, but not for the existence of numbers.

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theories we are able to assess include idealizations. Considering this instrumentalist suggestion would take us too far afield, however.

Indispensability across theories is just what *ruins* our chances of providing empirical evidence in favor of a postulate.

What about the third naturalism described, evidential naturalism? Is this the kernel of truth that remains after we recognize that methodological naturalism is an overstatement? The problem here concerns the fact, noted earlier, that there can be empirical tests of statements in pure mathematics, including existence claims. Benacceraf's question is – how can what we see *in* nature be evidence for the existence of things that are *outside* of nature? He thinks the answer to this question is that natural observations can't have this evidential import. But why not? How can what we see *in New Jersey* be evidence for the existence of things that are outside of New Jersey? Of course they can be. I grant that the puzzle about Platonism is more difficult than the puzzle about New Jersey. But there is a useful parallelism; observing one thing can be evidence for the existence of another if the two are "connected." This need not involve a causal relationship, although that is one possibility. Maybe propositions in the calculus of variations aren't *causally* connected to the soap bubbles that Plateau observed, but, evidently, they are connected.[20]

Even if evidential naturalism is false, there is a more limited evidential thesis that is correct. This is exemplified by the models FIS and FDS. Though each entails the existence of numbers, each is an empirical hypothesis and they can be tested against each other. The Platonistic commitments are what they have in common; testing them against each other does not test that shared presupposition. When scientific tests are organized in this way, the observations do not provide evidence for or against the existence of supernatural entities. However, the same can be said about the irrelevance of scientific tests to the question of whether physical objects exist. The contrastive thesis is right, but it isn't distinctively *naturalistic*.

Concluding Comments

I started this paper by quoting Jacques Monod. His dictum is relevant to atheistic evolutionists and to creationists alike. Evolutionary theory apparently "suggests" to many people that there is no God, but the theory has no such implication. As Monod says, "any confusion between the ideas *suggested* by science and science itself must be carefully avoided." There is another confusion that needs to be avoided as well. We must be careful not to confuse the implications of a well confirmed theory for which we have empirical evidence and the implications for which we have none. If Hempel's special consequence condition were true, there would be no such distinction. But the special consequence condition is wrong.

Although evolutionary theory is silent on the question of whether there is a God, it has implications about supernatural entities of another kind. The theory quantifies over numbers just as much as it quantifies over genes, organisms, and populations. If numbers

[20] Colyvan (2001, Chapter 3) and Lyon and Colyvan (2008) argue that pure mathematics sometimes bears an explanatory, though not a causal, relation to physical phenomena.

are Platonistic entities, evolutionary theory violates the requirements of methodological naturalism. This does not mean that evolutionary biologists are obliged to be mathematical Platonists. They have the option of adopting a Carnapian (1956) attitude, granting that their theory uses the framework of numbers while asserting that they adopt this framework for its convenience, not because they can produce a good argument for its truth. The theory of evolution, like other empirical theories, is a whole with many parts and its parts differ in their standing. Some are well attested empirically, others are known to be useful fictions, and still others are indispensable, not just useful. Duhem and Quine defended epistemological holism, arguing that it is whole theories that are tested, not their parts.[21] But the fact of the matter is that theories can often be pulled apart and their parts tested separately (Sober 2004). Scientists don't take the same attitude to all the parts of their theories, nor are they obliged to do so (Maddy 1992). Putnam (1971, p. 57) says that anti-realists are guilty of intellectual dishonesty, since they daily presuppose the existence of the entities that they say they forswear. My response is that there is nothing dishonest about saying that you are using a model that contains assumptions you are unable to test, nor is it dishonest to admit that some of the entities postulated by the model are useful fictions. What is dishonest is pretending that you have evidence for all the propositions on a list when you know that you have evidence only for some.

The two questions about metaphysics that I have discussed concern supernatural beings – God and numbers. But the antiholistic view of evidence that I have described also applies to metaphysical questions about nature itself. Evolutionary theory entails that there are physical objects (genes, organisms, populations) that are external to my mind; it therefore entails that solipsism is false. However, this does not mean that the evidence we have for evolutionary theory is evidence against solipsism. The models that evolutionary biologists test against each other all *assume* that solipsism is false. This proposition is assumed, not tested, just like the framework of numbers.[22]

Although evidence is the epistemological concept on which I have concentrated, it is worth considering how a nonholistic view of evidence bears on the role of "acceptance" in the scientific enterprise. Does rational acceptance obey a closure principle? If you (rationally) accept a hypothesis H and you know that H entails C, must you also (rationally) accept C? My claims about evidence do not require one to deny this principle. I think that evolutionary biology provides lots of evidence for evolutionary theory but none for the existence of numbers. If the theory entails that numbers exist, are biologists being irrational if they endorse the theory while being agnostic about the Platonism? Of course, most scientists have not considered the fact that mathematical theories entail that numbers exist. But let us bring some of them into a room and make them listen. There is no reason why biologists should not avail themselves of the simple response that they take no stance on M&E, but wish to affirm merely that "if M, then E."

[21] In Sober (1993a) I distinguish two forms of holism. Nondistributive holism holds that the confirmation of a whole theory never means that any of its parts have been confirmed. Distributive holism says that the confirmation of a theory entails that each of the parts is confirmed. The latter view is wrong if Hempel's special consequence condition is wrong.

[22] My arguments against the special consequence condition do not mean that we have no empirical evidence that solipsism is false. Reichenbach (1938) argued that we do; I consider his argument in Sober (2008).

Epistemologists can have their closure principle and biologists can have the parts of their theory that they care about.[23]

There have been many versions of "the theory of evolution;" some have been more metaphysical while others have been more austere. The present inquiry does not require that one version must be the "real" theory while all the others are inflated or deflated pretenders. Hull (1988) argues that theories are historical entities, like biological species; theories are not defined by their including this or that essential proposition, but by their historical relationships. Hull's main example is "Darwinism. For Hull, what makes people Darwinians is their relationship to others, not their assent to a list of required beliefs. I have argued that "evolutionary theory" is silent on the question of whether there is a God, but that it entails the existence of numbers. This pair of claims does not require an essentialist view of what evolutionary theory is. Whether atheistic evolutionism is the "real" theory of evolution does not matter. The point is that evolutionary biology provides no evidence that there is no God. The same point holds of theistic evolutionism; whether or not this is the "real" evolutionary theory, it includes an element for which evolutionary biology provides no evidence. Similar remarks pertain to the question of whether numbers exist. Mathematical evolutionary theory entails that there are numbers, but nonmathematical propositions about evolution (e.g., that humans and chimps have a common ancestor) do not. Without having to decide which of these is the "real" theory of evolution, we can simply note that there is no biological evidence that numbers exist. So what is the difference between God and numbers? You need numbers to do science. But that isn't evidence that numbers exist and that God does not.

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[23] See Everett (2006) for discussion of "antiskeptical conditionals" in connection with skepticism about knowledge.

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