Int J Philos Relig (2007) 61:69–81
DOI 10.1007/s11153-007-9112-2

ORIGINAL PAPER

What *is* wrong with intelligent design?

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Received: 19 September 2006 / Accepted: 19 November 2006 / Published online: 28 March 2007

Abstract While a great deal of abuse has been directed at intelligent design theory (ID), its starting point is a fact about biological organisms that cries out for explanation, namely "specified complexity" (SC). Advocates of ID deploy three kind of argument from specified complexity to the existence of a designer: an eliminative argument, an inductive argument, and an inference to the best explanation. Only the first of these merits the abuse directed at it; the other two arguments are worthy of respect. If they fail, it is only because we have a better explanation of SC, namely Darwin's theory of evolution by natural selection.

Keywords Intelligent design · Design arguments · Teleological arguments

1 Introduction

In recent years, a great deal of abuse has been directed at the movement known as intelligent design theory (ID). Daniel Greenberg (2005), for instance, writing in the *London Review of Books*, speaks of "the crackpot concept of intelligent design, a pseudo-scientific fabrication more marketable than its crude kin, creationism." Jerry Coyne (2005) speaks of ID as "the latest pseudoscientific incarnation of religious creationism, cleverly crafted by a new group of enthusiasts to circumvent recent legal restrictions." Ronald Pine (2005) goes further, suggesting that ID is "not even a pseudoscience"; it is "simply a scam and a fraud," whose goal is "to increase the hold of Evangelical Christianity on all our institutions and our way of life." William Saletan (2005) speaks of what he calls ID's "sophomoric emptiness," comparing ID advocate Michael Behe's testimony at the Dover, PA trial to the Monty Python "brontosaurus" skit. And Alan Gishlick and his fellow authors (Gishlick, Matze, & Elsberry, 2004) describe at least one defence of ID as a "rhetorical edifice" constructed out

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of the "omission of relevant facts, selective quoting, bad analogies, knocking down strawmen, and tendentious interpretations."

This level of vituperation may well be warranted, but such sweeping condemnations fail to distinguish the various kinds of arguments that ID advocates deploy. Even the more measured judgements offered by some philosophers—one thinks of Kenneth Himma's recent article (Himma, 2005) or Elliot Sober's discussions (Sober, 1999, 2002, 2003)—typically focus on just one of the argumentative strategies employed by ID advocates. If we wish to assess the ID project in its entirely, our discussion must have a wider scope. It must seek to examine the full range of arguments deployed by ID advocates. The present paper is a first step in that direction. My own judgment—like that of Himma, Sober, and others—is that, at the end of the day, none of these arguments is convincing. But some of the arguments deployed are stronger than many opponents of ID seem willing to concede. Intelligent design theory is an attempt to explain a fact that requires explanation, and at least some of the arguments offered deserve to be taken seriously. If at the end of the day they fail to convince, it is simply because we have a better explanation available, one that makes no appeal to a mysterious cosmic designer.

I shall take as representative of the intelligent design movement the work of Michael Behe, William Dembski, and Stephen Meyer. Behe is perhaps the best known ID advocate, by way of his accessible and widely read *Darwin's Black Box* (1999). But Dembski is the leading intellectual of the movement, with three major studies—*The Design Inference* (1998), *Intelligent Design* (1999), and *No Free Lunch* (2002)—outlining his version of ID. The third thinker, Stephen Meyer, is not as well known nor has he published as extensively, but he is director of the Center for Science and Culture which is part of the Discovery Institute in Seattle, the headquarters of the ID movement.

2 ID and the defeat of Darwin

A striking feature of much intelligent design literature is that it is directed to the defeat of Darwin's theory rather than the demonstration of design. To this extent, its arguments are largely negative. In *The Origin of Species*, Darwin made it clear what *would* defeat his theory. "If it could be demonstrated," he writes, "that any complex organ existed, which could not possibly have been formed by numerous, successive, slight modifications, my theory would absolutely break down"(Darwin, 1968). It is the claim that there exist biological structures that meet this test—that cannot be explained as the products of natural selection—which has lent some support to the case for ID. But the assumption here seems to be that ID could win by default. If Darwin loses, then design wins. But is this true? Could ID win by default?

Any argument to design would win by default only if two conditions were met. The first is that these two hypotheses—Darwin's and that of intelligent design—would need to be *the only possible* (or at least *the only available*) explanations of the facts in question. This would allow for an eliminative argument to design, of the kind which ID advocate William Dembski attempts with his "explanatory filter." (I shall examine this strategy in a moment.) The problem here is that there may exist other natural (i.e., non-design) processes, perhaps supplementing rather than replacing natural selection, which have contributed to the formation of the natural world. Stuart Kaufmann (1993), for instance, suggests that natural selection builds on certain "self-organizing"

properties of complex systems. Dembski (2002) tries to forestall such complementary explanations with his alleged "law of the conservation of information" (LCI), which states that *no* natural cause could generate the "specified complexity" (see below) which is characteristic of many biological systems. I shall not be examining this proposed law here. Suffice to say that its existence is hotly contested by philosophers, scientists, and Dembski's fellow mathematicians. (See, for instance, Elsberry & Shallit, 2003; Stenger, 2003.)

But even if Dembski were right—even if there did *not* exist other naturalistic hypotheses which could explain the complexity of the natural world—what would follow? Presumably that some non-natural cause was responsible for the complexity of living organisms. If one could spell out what is meant by a "non-natural cause," this would certainly be a conclusion of some significance. But it does not take us very far. Unless it can be given more content, it scarcely represents an alternative scientific theory. For we normally expect a scientific theory to have empirical content(Popper, 2002). It should not be entirely ad hoc, but should enable us to make a range of testable predictions. But the proposition that "some non-natural cause is responsible" lacks empirical content, a point to which I shall return in the final section of this paper.

3 Specified complexity as *explanandum*

As we shall see shortly, advocates of intelligent design offer a range of arguments in support of their conclusion. What unites these various arguments? It is the claim that at least some living organisms exhibit a feature or property that constitutes a reliable indicator of design (Himma, 2005, p. 1). The feature in question is variously named, but it has to do with the complexity of living organisms. Michael Behe speaks of the "irreducible complexity" of at least some biological structures, while William Dembski speaks of the "specified complexity" of at least some living organisms. Since Dembski's account is the more detailed, I shall take it as my starting point. What is "specified complexity"?

Complexity, for Dembski, is a measure of the likelihood that a particular structure should arise by chance, while specification has to do with its conformity to a pattern. A biological structure exhibits complex specification if the pattern it instantiates can be independently identified and if it is highly unlikely to have arisen by chance. Let me begin with the first of these criterion, that of specification. What Dembski means by a specified pattern is most easily approached by way of an illustration, that of an archer who shoots arrows at a wall. After the event, Dembski argues, she could make herself appear to be a skilled archer by simply painting bull's-eyes around whatever places on the wall an arrow falls. But the pattern thus created would not be a *specification*; it would be a *fabrication*. If the bull's-eye already exists, on the other hand, and she sets out to hit it and succeeds, it represents a specification. The specification is made prior to the event, or (as Dembski prefers to say) it is "detachable" from the event. What Dembski calls the "basic intuition" behind this notion can be simply described.

> Given an event, would we be able to formulate a pattern describing it if we had no knowledge which event occurred? Here is the idea. An event has occurred. A pattern describing the event is given. The event is one from a range of possible events. If all we knew was the range of possible events without any specifics about

> which event actually occurred, could we still formulate the pattern describing the event? If so, the pattern is detachable from the event (Dembski, 1998, p. 15).

Now one might argue that this seems too broad a criterion. It opens the door to all kinds of bogus connections, of the sort much loved by conspiracy theorists. This danger is highlighted by Dembski's claim that we can never *exclude* the possibility that a pattern is specified. It all depends on one's background knowledge. A pattern that seems unspecified at first may turn out to be specified later. I may see no detachable pattern in a collection of stones lying on a country road, but an astronomer may see that it "precisely matches some highly complex constellation"(Dembski, 1998, p. 17). Or, perhaps, I am tempted to say, that it forms the image of Elvis.[1]

But let's leave that aside. For what Dembski is trying to describe is surely a fact about the natural world which cries out for explanation, namely the existence of complex structures that seem highly unlikely to have occurred by chance. What "specified complexity" means could, I believe, be given a more helpful characterization, perhaps in terms of biological function. That this should be acceptable, even to Dembski's critics, is suggested by the fact that no less an opponent than Richard Dawkins makes a similar point. For Dawkins (1986, p. 9), too, what needs to be explained about living organisms is that they "have some quality, specifiable in advance, that is highly unlikely to have been acquired by random chance alone." In a passage cited by Dembski himself, Dawkins (1998, p. 40) illustrates his idea by referring to the probability of opening a bank safe by simply spinning the mechanism of the combination lock.

> Hitting upon the lucky number that opens the bank's safe is the equivalent... of hurling scrap metal around at random and happening to assemble a Boeing 747. Of all the millions of unique and, with hindsight, equally improbable, positions of the combination lock, only one opens the lock. Similarly, of all the millions of unique and, with hindsight, equally improbable, arrangement of a heap of junk, only one (or very few) will fly. The uniqueness of the arrangement that flies, or that opens the safe, is nothing to do with hindsight. It is specified in advance (Dawkins, 1986, p. 8).

The intuition here is that phenomena of this sort require more of an explanation than "Well, it just happened that way." For the chances of their "just happening that way" seem vanishingly small.

But just how small? What level of improbability is required if we are to reject the idea that such a structure could have occurred by chance? Key to Dembski's argument is the idea of a "universal probability bound," which he sets at 1 in 10^{150}. He calculates this figure generously by multiplying an estimate of the number of elementary particles in the universe (10^{80}) by the maximum number of changes which could occur each second (10^{45}) by the age of the universe in seconds, which he multiplies by a billion just to be on the safe side (to make 10^{25}). If the chance hypothesis assigns to the *explanandum* a likelihood lower than 1 in 10^{150}, then the chance hypothesis should be rejected (Dembski, 1998, p. 209). According to Dembski, what remains when specification is detected and chance is eliminated is specified complexity.

If specified complexity were merely the name of the *explanandum*, this would be a relatively uncontroversial proposal. One might argue with the details of Dembski's

[1] This is not an unfair illustration, since it is one used by Michael Behe, albeit in a qualified way; see Behe (1996).

probability bound. And (as we shall see in a moment) one might question the idea that we should reject a hypothesis merely because it renders what we observe highly unlikely. But as Dawkins's words remind us, even the atheist can agree that living organisms exhibit specified complexity and that this is a fact requiring explanation. The question is: what explanation do we offer? What Dembski and Behe claim that is the presence of specified (or irreducible) complexity is a reliable indicator that there exists a designer. To establish this, they need to produce an inference from specified complexity to design. There are three such alleged inferences that can be found in the work of ID advocates.

4 Dembski's explanatory filter

In *The Design Inference*, Dembski offers the first of his arguments, which he refers to as his "explanatory filter." This is a species of eliminative argument, which arrives at the conclusion that a structure exhibits specified complexity (and thus design) by a process of elimination. More precisely, the filter is an attempt to rule out, by statistical means, the possibility of a non-design origin. Branden Fitelson and his fellow authors set out the steps involved in Dembski's filter as follows. For any event E,

(1) There are three possible explanations of E—Regularity, Chance, and Design…
(2) The Regularity hypothesis is more parsimonious than Chance, and Chance is more parsimonious than Design. To evaluate these alternatives, begin with the most parsimonious possibility and move down the list until you reach an explanation you can accept.
(3) If E has a high probability, you should accept Regularity; otherwise, reject Regularity and move down the list.
(4) If the Chance hypothesis assigns E a sufficiently low probability and E is "specified," then reject Chance and move down the list; otherwise, accept Chance.
(5) If you have rejected Regularity and Chance, then you should accept Design as the explanation of E (Fitelson, Stephens, & Sober, 1999, p. 473).

In other words, "design" is what is left when the event in question—the formation of a biological structure—is of sufficiently low probability and the structure itself is "specified."

Let me concede, for the moment, that "regularity, chance, and design" exhaust the alternatives, although this is not immediately clear.[2] (After all, the mechanism posited by Darwin involves a *combination* of chance and regularity, for which Dembski's filter apparently leaves no scope.) A key question here is: What does Dembski mean by "design"? And as Howard van Till points out, it is not easy to find a clear answer. (For discussion, See van Till, 1999.) But let's take "design" to mean "the work of some intentional agent acting purposefully." What Dembski appears to be saying is that there would be no specified complexity were it not for the intervention of such an agent. Could Dembski's filter show this to be true? Does it allow us to eliminate all the alternative explanations, particularly Darwin's?

Well, perhaps it would, if we granted two further assumptions. The first is that we should reject a hypothesis if the likelihood of what we observe, on that hypothesis, is

[2] There are various ambiguities, if not outright confusions, in the way Dembski sets up his filter, only some of which I have chosen to explore here. For some others, see Fitelson et al. (1996, pp. 475–479).

sufficiently low. The second is that any natural—in the sense of non-design—hypothesis is equivalent to "chance," in our everyday sense of that word. The first assumption is perhaps defensible; the second is simply false.

4.1 The low likelihood criterion

Let's begin with the first of Dembski's assumptions. Should any hypothesis be rejected merely because the likelihood of the *explanandum*, on that hypothesis, is very low? Once again, we find Dawkins and Dembski to be unlikely bedfellows. For Dawkins also argues that "we can accept a certain amount of luck in our explanations, but not too much" (Dawkins, 1986, p. 139). What is too much? Interestingly enough, the "ball-park figure" at which Dawkins arrives is more demanding than Dembski's, namely 1 in 10^{19}, based on an estimate of the number of planets in the universe. We can admit at least this amount of luck, he argues, without our explanation being discredited. Note that what Dawkins is talking about here is the origin of *life*, namely the origin of self-replicating molecules (RNA and DNA). And he is assuming that such molecules must have arisen by chance recombination within a suitable environment. Dawkins differs from Dembski in arguing that once "cumulative selection" of the type posited by Darwin gets underway, very little luck is required to arrive at very complex creatures. I shall return to this point shortly. For the moment, I wish only to note that in disallowing explanations on the likelihood grounds, Dembski and Dawkins are at one.

Against this assumption one could offer an argument put forward by Elliot Sober. Assuming that the theories with which we are dealing make only probable predictions, Sober notes that "there is no probabilistic equivalent of *modus tollens*" (Sober, 1999, p. 58, 2002, p. 67, 2003, p. 34). There is no valid argument of the following form.

> If H were true, O would be highly improbable.
> But O.
> Therefore H is not true.

It is easy to see why this is so, for adopt an argument of this form would lead to absurd consequences. If you win the national lottery, this is a highly unlikely outcome, given that the lottery was fair and that you bought just one ticket. But it does not cast doubt on the hypothesis that the lottery was fair. And if we have a line which is 1000 miles long, onto which we drop a pin at random, the chance that it will fall just where it does is very small. But that does not cast doubt on the hypothesis that it was dropped at random. It follows, Sober writes, that there is no "Law of Improbability that begins with the premise that Pr(O|H) is very low and concludes that H should be rejected" (Sober, 2003, p. 34).

The moral which Sober draws from this lesson is that theories which make merely probable predictions must be tested comparatively. To reject a hypothesis, we need to have in mind an alternative, which would render the observed outcome more likely. So those who claim that some piece of evidence supports an hypothesis of intelligent design need to show that their hypothesis is more successful. They need to show that the specified complexity of the natural world is more likely on the design hypothesis than on any non-design alternative. Could this be done? Sober thinks not, for reasons to which I shall return shortly. My question here has to do with Sober's initial argument. Is it true that we cannot reject a theory merely because it renders what we observe highly unlikely? If it is, then Dembski's eliminative inference to design is

discredited. One cannot reject a non-design explanation—whether that be a matter of what Dembski calls "regularity" or "chance"—on merely statistical grounds.

Let me explore Sober's argument with the aid of some further reflections on the nature of explanation. To do so, I shall use the probability reasoning that he himself employs. Let's say that a hypothesis (H) is a *potential explanation* of an observed fact (O) if H increases the likelihood that we would observe O. More precisely, H is a potential explanation of O if

$$\Pr(O|H) > \Pr(O|\neg H).$$

Now if H increases the likelihood of O, then there may be circumstances in which it would be reasonable to accept H, even if the likelihood of O given H remains low. For instance, it might be reasonable to accept H, if only provisionally, if H were both a satisfactory explanation—one that is, for instance, simple, economical, and informative—and the only one on offer. And while the likelihood of O given H may be low, but it may still be higher than the likelihood of O given any alternative hypothesis. In these circumstances, too, it may be reasonable to accept H. It follows that Sober is correct. We are not entitled to reject a hypothesis merely because it renders what we observe highly unlikely. But neither are we obliged whole-heartedly to embrace it. We might accept H, for the time being, but only in some provisional sense, while we search for an alternative, one that would increase the likelihood of O (Sober, 1999, p. 73(16)).

4.2 Dembski's likelihood calculations

So perhaps we can concede this much to Dembski, namely that a sufficiently low likelihood calculation—one below his "universal probability bound"—might lead us to seek an alternative theory. And if "design" were really the only alternative, then so be it. The key question then is: How do we perform these likelihood calculations? What is striking is what Dembski does *not* do. He does not attempt to calculate the likelihood of the emergence of specified complexity given Darwin's theory. What he calculates is the likelihood that specified complexity should emerge by chance. But the result of his calculation would serve his purpose only if all non-design processes—Darwin's theory included—were equivalent to chance.

This assumption appears superficially plausible in the context of Dembski's argument, since he equivocates in his use of the term "chance." In his initial description of his explanatory filter, "chance" is contrasted with "regularity," so that the reader naturally understands "chance" to refer to a uniform probability distribution, one in which all possible outcomes are equally likely. This is the basis on which Dembski makes his probability calculations (Dembski, 2002b Section 4), which form the basis of his inference to design. Take, for instance, Dembski's favourite example of specified complexity, namely the bacterial flagellum. To calculate the likelihood of its originating in a non-designed way, Dembski calculates the probability of its formation as a "discrete combinatorial object" (d.c.o.). Dembski argues the probability of such an object arising (P_{dco}) can be calculated by multiplying three partial probabilities, namely P_{local} (the probability of a suitable collection of proteins assembling), P_{config} (the probability that this collection of proteins will form the object, if arranged at random), and P_{orig} (the probability of the proteins themselves assembling at random from amino acids) (Dembski, 2002a, p. 291). Each probability figure falls below Dembski's universal bound (1 in 10^{150}), thus eliminating chance as an explanation.

The problem with this procedure should be clear. We could infer design (simply) by eliminating chance only if *all* the alternative explanations involved "chance" in our everyday sense, that is to say, if they involved processes whose possible outcomes were all equally likely. But of course the Darwinian account involves much more than "chance" (in this sense). As van Till writes,

> *no biologist has ever taken the bacterial flagellum* [for example] *to be a discrete combinatorial object that self-assembled in the manner described by Dembski*. Dembski has not defeated any actual biological proposition. He has slain nothing more than an imaginary dragon—a fictitious adversary that Dembski himself has fabricated from a tall stack of rhetorical straw (van Till, 2002).

What makes Dembski's filter *look* like a sufficient argument for design (at first sight) is that on occasions he uses the word "chance" in a broader sense. As he writes on these occasions, "chance"

> includes anything that can be captured mathematically by a stochastic process. It thus includes deterministic processes whose probabilities all collapse to zero and one. It also includes nondeterministic processes, like evolutionary processes that combine random variation and natural selection. Indeed, chance so construed characterizes all material mechanisms (Dembski, 2002b, Sect. 3).

This twofold use of the term "chance"—I am tempted to say "this shameless equivocation"—can lead the reader into supposing that in eliminating chance on probabilistic grounds, Dembski has defeated his chief rival, namely Darwinism. Indeed given his broad definition of "chance," he seems, at first sight, to have eliminated all naturalistic alternatives to design. But he has done nothing of the sort.

5 An inductive argument to design

So at least this eliminative argument to design is a poor one. Can we stop there? No, because not all the arguments from specified complexity to design are eliminative. Both Dembski and Behe offer a second argument, an *inductive* inference to design. It is true that neither Dembski nor Behe present their inductive inference as a distinct argument—they present it as an argument which supports their eliminative inferences (van Till, 1999, p. 668)—but it seems to me that it must be regarded as such. For to employ the method of inductive inference is to look on design as another kind of regularity. In this case, we infer design not merely because the outcome is highly unlikely to occur by chance (design being apparently the only alternative), but because we know that such outcomes occur—regularly!—when intelligent agents are at work.

Let me begin with Michael Behe's inductive argument. In discussing inductive arguments to design, Behe concedes that they may not have looked particularly persuasive in David Hume's day, but he argues that Hume's objections have been "destroyed by the advance of science" (Behe, 1996, p. 219). Why? Because "modern biochemistry routinely designs biochemical systems, which are now known to be the basis of life. Therefore we do have experience in observing the intelligent design of components of life" (Behe, 1996). William Dembski offers a more general argument in support of his conclusion. The justification for regarding SC as a reliable criterion of design is, Dembski writes,

> a straightforward inductive argument: In every instance where the complexity-specification criterion attributes design and where the underlying causal story is known (i.e., where we are not just dealing with circumstantial evidence, but where, as it were, the video camera is running and any putative designer would be caught red-handed), it turns out design actually is present (Dembski, 1998, p. 25).

In fact, Kenneth Himma takes inductive generalization to be the central feature of Dembski's argument, while also pointing out what he considers to be its fatal weakness.

What is the weakness of this inductive argument, according to Himma? If it is to justify his conclusion, Dembski's inference needs to have the following structure.

1. Every *observed instance* of specified complexity is explained by the acts of some intelligent agent who brought it about.
2. Therefore, every *instance* of specified complexity is explained by the acts of some intelligent agent who brought it about (Himma, 2005, p. 12).

But, Himma argues, this is too broad a claim. First, the agents which are responsible for the examples of specified complexity to which Dembski refers are *human* agents. Second, there are examples of specified complexity (in the natural world) for which human agents could *not* be responsible. So the only inductive inference that could be justified is the following:

1. Every observed instance of specified complexity that *could be explained by the acts of some human being* who brought it about has, in fact, been explained by the acts of some human being who brought it about.
2. Therefore, every instance of specified complexity that could be explained by the acts of some human being who brought it about *is* explained by the acts of some human being who brought it about (Himma, 2005, p. 12).

And since no human could have brought about the specified complexity of the natural world, this argument will not support Dembski's conclusions.

I would be tempted to accept Himma's objection, if Dembski did not have John Stuart Mill on his side. In his *Three Essays on Religion* (1874), Mill notes that the famous design argument offered by William Paley is an argument from analogy. We know that Paley's watch was designed because we know that watches generally are designed. But to argue as Paley does that living organisms are designed is to argue that they bear some analogy to watches. Mill notes that this is a relatively weak form of argument. However, Mill argues that the design argument can be reformulated in a stronger way. It can be taken to demonstrate that man-made devices and living organisms are not merely analogous, but that they share a common feature. What they share is the arrangement of their parts in order to achieve some goal, or—in Mill's words—"the fact of conspiring to an end" (Mill, 1969, p. 447). And this is a feature

> which experience shows to have a real connection with an intelligent origin... The argument therefore is not one of mere analogy. As mere analogy, it has its weight, but it is more than analogy. It surpasses analogy exactly as induction surpasses it. It is an inductive argument (Mill, 1969).

So in Mill's view, at least, this is a sound inductive argument.

I shall not try to adjudicate this difference of opinion here. Instead, I will simply ask what would follow if Mill is right. Would it mean a victory for design? Not necessarily. For as Mill himself noted, there now exists an alternative explanation of precisely this feature of living organisms, which he describes, in an allusion to Darwin, as "the principle of 'the survival of the fittest" (Mill, 1969, p. 449). As it happens, Mill was not altogether convinced that Darwin's theory could do what was being asked of it. But he was right to note that it was the major rival to the design hypothesis. And since an inductive argument can, at best, make its conclusion more probable, even a sound inductive argument to design cannot be said to settle the question. For there may be a better explanation available, one that renders specified complexity more likely or that exhibits explanatory virtues the design hypothesis lacks. So the question this raises is: Can "design" be defended as a better explanation of specified complexity than that offered by Darwin?

6 Design as the best explanation

This leads us to a third argument for design, one that presents the design hypothesis as the best explanation of specified complexity, a form of argument commonly known as "inference to the best explanation" (IBE). Intelligent design advocates are divided about the wisdom of adopting this route. Dembski, for instance, rejects IBE as a way of arguing for design (Dembski, 2002a, pp. 101–110), while Stephen Meyer defends it (Meyer, 1999, 2004). It is perhaps understandable that Dembski is reluctant to embrace an IBE design argument. For to argue that design is the best explanation of specified complexity could be taken to imply that there are other explanations available. More precisely, it could be taken to imply that Darwin's theory is a *potential explanation* of the complexity of biological organisms[3]—one that would explain such complexity if it were true. And this is something that ID advocates are reluctant to concede. They want to argue that Darwin's theory cannot do what it promises, not merely that their theory is preferable.[4] But since some ID advocates offer an IBE argument, and since it seems at first sight a promising strategy, let's pursue this option for a moment.

There is much debate about what is involved in "inference to the best explanation." In particular, there is a clear divide between what I would call "justificationists" and "explanationists."[5] The justificationist argues that we are warranted in accepting a potential explanation if its overall probability exceeds 0.5 and it has no more probable rival. The explanationist argues that it can be reasonable to accept a potential explanation even if its probability has not been shown to exceed 0.5. What we should look for are the presence of certain explanatory *desiderata*. We should choose that potential explanation which has the most explanatory power, has been most extensively cor-

[3] On "potential explanations," see Lipton (1991). As I am using the phrase here, a potential explanation is simply a proposition or set of propositions, the truth of which would render the explanandum "what we would expect" in these circumstances. Inference to the best explanation, on this view, represents a choice of what is taken to be the actual explanation from among a field of competing potential explanations.

[4] There would be nothing to prevent an ID advocate from conceding that Darwin offered a potential explanation of specified complexity, while arguing that design is a better potential explanation and therefore worthy of acceptance. Interestingly, I know of no ID advocate who adopts such a view.

[5] I have drawn this distinction from the work of Alan Musgrave ("Scientific Realism and the Miracle Argument" unpublished paper, University of Otago, 2005), but it is prefigured in Bartley (1984). For a thoroughgoing defence of an explanationist view, see Lycan (1988) 112 et passim.

roborated by independent tests, and is the most simple, economical, and informative. (Other lists of explanatory virtues are possible, but these will do for the moment.)

I am inclined to prefer the explanationist position, for two reasons. The first is that the alternative, justificationist view requires us to make probability estimates. It requires us, first of all, to judge the comparative likelihood of what we observe, given competing theories, and then to assess the posterior probability of each theory, perhaps using some form of Bayes's theorem. But in cases such as this, it is hard to see how we could make such judgements, at least with any degree of precision. Richard Swinburne (2004), for instance, who adopts a justificationist approach, admits that his own probability estimates are to some degree "arbitrary." It would be good to avoid such arbitrariness if we can.

My second reason for preferring the explanationist view is that it seems to reflect best scientific practice. At the time of its introduction in 1859, could the probability of Darwin's theory have been shown to exceed 0.5? John Earman (1992), at least, thinks not. He argues that given the difficulties facing the theory at the time of its introduction, it had a low posterior probability. So if Darwin's theory was worthy of acceptance in 1859, it was not so much because of its overall probability given the evidence, but because it displayed some highly desirable features. It had a significant degree of explanatory power, much higher than any competitor, over a range of hitherto puzzling phenomena. It posited a mechanism (natural selection) for which there existed a familiar analogy (artificial selection). And it was potentially fruitful, suggesting new lines of research. In Philip Kitcher's felicitous phrase, Darwin's theory "gave a structure to our ignorance" (Kitcher, 1996).

So let me adopt an explanationist view, for the sake of the argument. After all, it apparently makes a lesser demand of the ID advocate. He does not have to show that there probably exists a designer; merely that the existence of a designer is the best available explanation of specified complexity. But could he show this? Could he show that "design" represents a better explanation of specified complexity than Darwin's theory? Does it have a higher degree of explanatory power? Is it better corroborated, and more simple, economical, and informative? The problem here is that "design" is a theory that is practically without content. What it affirms is that "an unknown intelligent designer did something, somewhere, somehow,"[6] for some unspecified reason in order to produce what we see before us. If this is an explanation, it is a highly uninformative one. Could this "explanation" be corroborated? It could be, in principle, if it were independently testable, if we could use it to predict facts other than the fact to be explained. But precisely because the advocates of ID leave the identify of their designer unspecified, this seems to be impossible.

Let me spell out that last point. If the posited designer belonged to a familiar class of intelligent designers, such as human agents, then there might be things we could predict, based on our previous experience of such agents. This would be merely another form of inductive argument. But the one thing we can say about the designer posited by ID advocates is that he (she? it?) is not human, or even significantly human-like. It appears, for instance, that he is an *unembodied* designer (Dembski, 2002a, pp. 333–334, 359). And if their designer is supposed to belong to some other familiar class of intelligent designers, we are not told what that is. So we cannot draw on our experience of some class of designers to predict how he might act. What about the rationality principle that underlies so many of our intentional explanations—the idea

[6] Gishlick et al. (2004), Summary of Paper.

that what we expect of intentional agents is what it would be rational for them to do, given certain beliefs and desires. Sadly, this will not do the job, either. For until we have identified the designer, we have no idea what beliefs and desires to posit, in order to deduce what actions are likely to follow. So we cannot make predictions by invoking the rationality principle.

It is worth noting that Dembski does respond to this objection. On the one hand, he rejects the demand that ID make testable predictions. The demand for testability, he argues, is misplaced, since designers are often innovators; we cannot predict what result they will come up with (Dembski, 2002a, p. 209). Despite this, he goes on to argue that ID does make predictions. What it predicts is "that nature should be chock-full of specified complexity and therefore should contain numerous pointers to design" (Dembski, 2002a, p. 362). Dembski's first point, if it were true, would actually lend support to my argument, since if we cannot predict what will follow from the ID hypothesis, then it is not testable. His second argument overlooks the fact that specified complexity is the original *explanandum*. Merely finding more examples does not constitute an independent test.

7 Conclusion

What follows from this all-too-brief overview of the ID literature? Well, ID advocates have identified a fact that is in need of explanation. They are not, of course, the first to do so. The origins of what William Dembski calls the "specified complexity" of biological organisms has long puzzled scientists and lay persons alike. But ID advocates are right to insist that it requires explanation and that "it happened by chance" would not be a good explanation. But if chance is eliminated, does it follow that it occurred by design, as Dembski's explanatory filter suggests? Not at all, although it might *appear* to do so if you equivocate, as he does, regarding the meaning of "chance."

But is there a second strategy? Could we offer an inductive argument to design, of the kind offered by both Dembski and Michael Behe? Well, perhaps we could. John Stuart Mill certainly thought so. But the question then becomes: Okay, but is "design" the *best* available explanation of biological complexity? Stephen Meyer affirms that it is, but he takes into account neither the challenge of Darwin's theory or the emptiness of his own. If what we are faced with is a choice between Darwin's theory—which has been corroborated by more than 140 years of research—and a vague appeal to design, there seems little doubt which would win. And even if you could *eliminate* Darwin's theory as a potential explanation of biological complexity, the appeal to the unidentified actions of an unknown and altogether mysterious cosmic designer can hardly be regarded as a serious alternative.

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