

PHILOSOPHY OF SCIENCE

*Debating Design: From Darwin to DNA*

Edited by WILLIAM A. DEMBSKI and MICHAEL RUSE

Cambridge University Press, 2004. xvi + 406 pp. £35.00

*Debating Design: From Darwin to DNA* is a collection of articles on the theory of Intelligent Design (henceforth, ID). The collection contains articles critical of ID as well as articles supportive of ID. The focus of the book is on whether ID is good science, and as such, social, legal, and policy questions are not addressed.

Although the book is divided into 4 parts, the main arguments for and against ID are to be found in Parts I and IV. Part I, which contains articles by Ayala, Miller, Sober, and Pennock, is devoted to arguments against ID. Part IV, which contains articles by Dembski, Bradley, Behe and Meyer, is devoted to arguments for ID. Parts II and III are miscellaneous in nature, and connect with the main debate less directly. (An exception is Weber and Depew's article in Part II, which really ought to be in Part I.)

I found the organization of the book to be a little haphazard; the reader will probably do well to read the arguments for ID in Part IV first, followed by the arguments against ID in Part I, leaving Parts II and III as optional extras.

There is probably little in this book that will be new to the expert. Nevertheless, Parts I and IV present a very useful and accessible summary of what have become the standard arguments for and against ID. I expect the book will function quite well in getting newcomers to the debate up to speed.

For the remainder of the review, I would like to give the reader a sense of how the debate proceeds by summarizing the main argument for ID that emerges from Part IV, and then discussing an especially important objection to this argument that pops up a number of times in Part I.

Let us begin with the argument for ID. The central idea of Darwinism is that a relatively simple biological entity can evolve into a significantly more complex biological entity by a sequence of small steps, each of which increases the fitness of the entity as a whole.

The main problem that ID raises, however, is that many biological entities are composed of parts in such a way that if any part is absent, or even slightly modified, then the entity ceases to function in any useful way at all. For instance, if any one of the approximately 40 proteins in the bacterial flagellum are removed or interfered with, the flagellum stops doing anything useful. It does not merely work *less well*—it ceases to perform *any* useful function *at all*. Similarly, if the clotting cascade is interfered with only slightly, one gets a mechanism that at best does nothing, and at worst, is tremendously harmful. (These examples and others are developed by Dembski and Behe in the text.) It is consequently very difficult to identify any obvious evolutionary predecessor for these biological entities.

Now, the proponent of ID is willing to admit that our search for evolutionary predecessors might be proceeding too naively here. The Darwinist, after all, need not be committed to the view that there was some sort of functioning, useful biological organism with 39 proteins in it, merely awaiting the addition

of a 40th protein in order to become a full-fledged bacterial flagellum. Perhaps, for instance, several proteins had to be absorbed at once, and another protein discarded. The problem, however, is that as the physical process required becomes more complicated, the probability of it actually occurring *plummets*. In Part IV, Bradley discusses various simple models (for different systems) in which the relevant probabilities are  $10^{-75}$  and  $10^{-63}$ . Meyer's article mentions a probability of  $10^{-65}$ . Even given the age and size of the universe, these probabilities are astonishingly low.

So, if the steps that evolution makes are small, then there should be obvious evolutionary predecessors to entities like the bacterium flagellum and the clotting cascade, which there are not. But if the steps that evolution takes are large, then one will require long sequences of *astonishingly improbable* events. Either way, the Darwinist is in trouble. Given that ID avoids these problems, it should be admitted into the scientific arena as a bona fide alternative account of the existence of life.

This is the standard argument for ID, and as far as I can tell, the worthiness of the ID enterprise rests on whether this argument is good or not. If this is a good argument, then the Darwinist is in trouble, and if it is a bad argument, then the ID enterprise is bankrupt.

In Part I, a number of distinct objections to the above argument are developed. There is, however, an objection that appears in some shape or form in *all* the contributions to Part I, that is nevertheless not totally clear, and which really ought to be made clearer. I will try to make this argument more explicit here.

Given the exceedingly complex nature of biological systems, we are forced to think of their behaviour as, in some sense, random. But there can be different ways of incorporating randomness into an analysis of a system, that can have enormously different effects on the subsequent theory and its predictions.

For instance, imagine watching someone drawing cards from a deck in a haphazard way. We would like to treat this as a random process. One possibility is that the person is selecting the cards in such a way that equal probabilities should always be assigned to any remaining card being selected next. Another possibility is that the person has a preference for certain cards with bent corners, but that his selection is otherwise unbiased. These distinct possibilities will lead to very different assessments of the likelihoods of certain outcomes. What looks like a miracle on one way of incorporating randomness into the analysis can look utterly expected on another way of incorporating randomness into the analysis.

More complicated cases of this can be found in the sciences. Imagine a thin layer of liquid caught between two glass plates, slowly being heated. It seems reasonable to expect that, as the liquid gets hotter, the individual molecules will move around with increasing randomness, in such a way that there will be no correlations between the positions of individual molecules over observable time scales. In fact, the exact opposite turns out to be true. We observe (to our amazement) that the liquid organizes itself into hexagonal convection cells. If one thought that there ought to be no correlations between positions of molecules over observable time scales, one would have to conclude that a

*miracle* happens every time a hexagonal convection cell forms. But of course, the good physicist realizes that although there is *some* sort of randomness in the system, it is not quite the *type* of randomness he naively expected. Once he rethinks the sense in which the system is random, what appeared miraculous turns out to be expected. Indeed, now that we have a theoretical understanding of the Rayleigh-Benard hydrodynamic instability, and understand more clearly the type of randomness compatible with it, the appearance of hexagonal cells in this context has moved from the realm of the miraculous to the realm of the expected.

This story is not unique. The same episode has occurred over and over in twentieth-century non-equilibrium statistical mechanics, as we have learned more and more about the physics of self-organizing systems. Naive assumptions about randomness make self-organizing behaviour seem like a miracle; modifying those assumptions, and doing some physics, makes this sort of behaviour utterly expected.

The main *scientific* problem with the argument for ID now becomes clear. A crucial part of the ID argument is devoted to showing that any sufficiently complex physical process in which several distinct parts of a biological system come together to form a larger system must be *wildly improbable*. As mentioned earlier, proponents of ID have actually performed calculations with simple models, and have come up with probabilities like  $10^{-65}$ .

These calculations, however, make very specific assumptions about the way in which the relevant process is random. In particular, it is assumed that the parts of the system float around *utterly independently* of each other, and must, against tremendous odds, find themselves all bumping into each other in exactly the right way at a single point in time, in order to create the larger biological system. (The relevant calculation is then purely *combinatorial*, and invokes essentially no physics.) Under this assumption, it is indeed true that the formation of the larger biological system would be miraculous. But this is not news to anyone. The problem is that it is an idealization to assume that the constituent parts float around utterly independently of each other. There are all sorts of subtle physical and chemical phenomena going on when, for instance, proteins float around and bump into each other. The question is whether all this additional physics and chemistry actually makes it *probable* (or at least, *not vastly improbable*) that the larger biological system will emerge.

By making a very simplistic assumption about randomness that *forces* us to ignore all this additional physics and chemistry, and that thereby implicitly *guarantees* the improbability of self-organizing behaviour, ID infers that evolution is highly improbable. But this, of course, is a *totally question begging* argument. By assuming that the constituent parts of the system behave in a radically unordered way, with *nothing to ever stop* them behaving in a radically unordered way, ID argues that the emergence of order is highly improbable. In this way, the ID argument really just assumes what it is trying to prove. To restate the point once more: ID merely argues that if you set things up in such a way that the emergence of large scale structures is improbable, then the emergence of large scale structures is improbable. But this proves nothing about the probability or improbability of evolution.

There is, of course, a large scientific literature dealing with the probability of the development of complex systems. A classic text is Simon's *The Sciences of the Artificial* (3rd edition, MIT Press, 1996.) Here Simon argues that the probability of assembly of complex systems increases dramatically if one allows for the existence of small, stable subsystems. (In particular, see Chapter 8, 'The Architecture of Complexity—Hierarchic Systems'). Another possibility is that some complex systems could have evolved as *subsystems* of larger, less structured, systems. Because the larger system has less overall structure, its evolution is more probable. The parts of the larger system irrelevant to the smaller, stronger subsystem then drop off. This sort of possibility is sometimes discussed in connection with the origin of the bicoid gene—see, for instance, Wimsatt and Schank's article 'Generative Entrenchment, Modularity, and Evolvability: When Genic Selection Meets the Whole Organism', included in Schlosser and Wagner's *Modularity in Development and Evolution* (University of Chicago Press, 2004), as well as Schmidt-Orr and Wimmer's article 'Starting the Segmentation Gene Cascade in Insects' in the same volume.

In summary, ID does *not* give us any reason to think that non-trivial physical processes in which complex biological entities are formed out of constituent parts are wildly improbable. Of course, my pointing this out does not thereby give us a reason for thinking that such processes are *probable* either (or even *not* wildly *improbable*). The hard work here has to be left to the scientists, who have actually already enjoyed significant successes—though many fascinating and difficult questions remain open. Recall, however, that the very ambitious goal of the ID argument was to show that scientists *must* eventually be doomed to failure here. In this respect, the ID argument is unpersuasive.

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## PHILOSOPHY OF HISTORY

*Our Knowledge of the Past: A Philosophy of Historiography*

By AVIEZER TUCKER

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This is a welcome attempt to revive the largely moribund field of post-analytic philosophy of history. Tucker wishes to make a clean break with previous debate concerning the essential *form* of historiography—in particular, whether historical explanation requires covering laws, singular causal claims, or narratives. Tucker's topic is rather the relation between present evidence and historiographical 'hypotheses'. He asks whether such hypotheses are determined, underdetermined, or indetermined by the evidence. He argues that a large part of post-Rankean historiography is determined by the evidence, and should therefore be regarded as scientific. This historiographical development should be recognised as a significant achievement, indeed as a "third scientific revolution" (p. 260) (following Galileo and Newton). Some contemporary historiography is, however, underdetermined: this portion, together with all pre-Rankean historiography, is 'traditionalist historiography'. It makes no