Review of Dean L. Overman (1997) A Case Against Accident and Self–Organisation New York: Rowman & Littlefield

To judge from the dust–jacket, this book has received a considerable amount of praise—and not just from the usual suspects. In particular, the publishers seem keen to promulgate the view that there is widespread support for the claim that Overman makes a clear, compelling, and well–argued case for the conclusions which he wishes to defend. However, it seems to me that those cited on the dust–jacket—Pannenberg ("lucid and sobering arguments"), Polkinghorne ("scrupulously argued"), Nicholi ("compelling logic and carefully reasoned argument"), Kaita ("cogent and lucid"), Gingerich ("interesting and convincing"), Behe ("compelling case"), and McGrath ("clear and informed arguments")—cannot have been commenting on the book which I am currently in the process of reviewing. True enough, the book is well–organised and mostly easy to read; moreover, the book clearly demonstrates that Overman is thoroughly acquainted with popular presentations of recent work in a variety of scientific fields. But the crucial question is whether it makes a *clear*, *compelling*, and *well–argued* case for the conclusions which Overman wishes to defend. I shall claim in this review that the book fails on all three counts.

One important question to ask at the beginning is about the ultimate purpose of the book. Overman ends, at p.197, with the claim that 'if life transcends the laws of physics and chemistry, then a rational conclusion is that a Person, not chance and the laws of physics and chemistry, caused and is causing life'. Moreover, there are other places in the book where Overman at least hints at other arguments for the existence of this Person—in particular, in the detailed argument from contingency which is set out at pp.164–5, in the moral argument which is given at pp.177–9, and in some of the examples which are given in the chapter on logic and reasoning. Even if it were not independently overwhelmingly plausible, this evidence makes it clear that Overman's project is intended to make a contribution to the cause of defending the reasonableness—if not the rational requiredness—of belief in God. (Consider also the claim by McGrath, featured prominently on the inner sleeve of the dust–jacket, that Overman's book 'reopens the case for divine design'.)

However, it isn't clear that the kinds of arguments which Overman gives are really well–suited to this ultimate purpose. Consider, for example, his defence of the claim that there is no good current scientific theory of the origins of life on earth. Even if he is right about the state of current scientific theorising about the origins of life on earth, it is just a mistake to suppose that, in itself, this lends any credence at all to theism. After all, it can't just be taken for granted that there are good theistic theories of the origins of life on earth. Perhaps the smart money should rather be on the claim that there is some perfectly naturalistic account of the origins of life on earth which we have not yet been able to discover. To the extent that Overman's arguments show just that we don't currently have

good scientific theories about certain phenomena, there is no reason at all to think that they are qualified to 'reopen the case for divine design'—or, at any rate, if there is such reason, it remains to be produced. Of course, if Overman's arguments showed that there are phenomena which require explanation, but which cannot be given a scientific explanation—or if they showed that there are phenomena which require explanation, but which can be given a much better theistic explanation than can be provided by any kind of scientific theory—then matters would stand rather differently. But there is nothing in the arguments which Overman produces which supports either of these claims.

Enough of these preliminary observations, and on with the review. The structure of the book is as follows:

(There is also some front matter, twenty–five pages of notes, and a selected bibliography. Overman's preface and some of the notes are worth attention; I shall mention them where it is appropriate to do so.)

At p.1, Overman announces that he will address the following three questions:

(1) Is it mathematically possible that accidental or chance processes caused the formation of the first form of living matter from non–living matter?

(2) Are current self–organisation scenarios for the formation of the first living matter plausible?

(3) Is it mathematically possible that accidental or chance processes caused the formation of a universe compossible with life?

Part 3 is the discussion of question (1); Part 4 is the discussion of question (2); and Part 5 is the discussion of question (3). Part 2 is a very curious discussion of logic and reasoning; Part 6 is a breathtaking discussion of alleged ethical implications of atheism; Part 7 is more or less what it purports to be. I shall discuss Parts 2, 3, 4, 5, and 6 separately, and in some detail. But, before I do this, there are some general observations to be made.

On the face of it, the answers to (1) and (3) seem easy: of course it is *mathematically* possible that accidental or chance processes caused the formation of a universe compossible with life and the formation of the first form of living matter from non–living matter. Although the notion of mathematical possibility is not an easy one, a minimum requirement is surely going to be possession of probability zero: if something fails to have probability zero, then there is a mathematical chance that it will occur—and hence it can hardly be said to be a mathematical impossibility. Of course, it may be that possession of probability zero is not a sufficient condition for mathematical impossibility—that depends upon the details of the measure theory which is used to define the probabilities. However—and bizarrely—Overman claims, repeatedly, that mathematicians normally regard anything with a probability of less than one in 10^{50} as a mathematical impossibility (1, 55, 181). Imagine a lottery with 10^{60} tickets: no mathematician is going to claim that it is *mathematically* impossible for there to be a winner in such a lottery! Rather than continue with this uncharitable belabouring of the obvious, we should reinterpret the questions which Overman is asking:

(1) Is the probability that accidental or chance processes caused the formation of the first form of living matter from non–living matter less than one in 10^{50}?
(3) Is the probability that accidental or chance proceses caused the formation of a universe compossible with life less than one in 10^{50}?

Despite this exercise in charity, I can't resist pointing out that, at p.183, Overman claims that a probability of one in 2×10^{44} is a mathematical impossibility. Perhaps we should revise (1) and (3) further in the light of this devaluation of the crucial probability! Or perhaps we should replace the numerical value with words: 'vanishingly small', or the like. After all, it hardly seems that it would matter if the figure were 10^{25}, or 10^{12}, or 10^{6}, or (perhaps even) 10.

There is a serious point here. Probability talk is notoriously difficult to interpret, especially in the context of discussions of what it is reasonable to believe. On the one hand, probability talk might be talk about objective chances—e.g. what is the probability that this radioactive particle will decay in the next five seconds—where these are taken to be agent–independent properties of the universe. On the other hand, probability talk might be talk about subjective probabilities—e.g. what is the probability that this coin will come up heads when it is tossed—where these are taken to be agent–dependent assessments. Since it is controversial whether there are objective chances—and since it seems clear that, even if there are objective chances, these are not the kinds of probabilities which Overman is considering—we shall henceforth restrict our attention to subjective probabilities.

Given that we are considering subjective probabilities, the next question to consider is whether we are talking about conditional or absolute probabilities. Some judgements of probability are absolute: we assign unconditional probability values to propositions. ('The probability that this radioactive particle decays in the next thirty seconds is 0.456.) Other judgements of probability are conditional: we only assign conditional probability values to propositions. ('The probability that this coin comes up heads, given that it is

fair, is 0.5.') Often, it is hard to tell whether probability claims are unconditional; the condition can be tacit or disguised. However, it does seem plausible to think that we make some unconditional judgements of probability. ('This lottery is fair. The probability that any given ticket is chosen is one in a million.')

There is a serious question about whether Overman is talking about conditional or unconditional probabilities. On the one hand, there is some plausibility to the claim that you do not believe a proposition unless you assign to it an unconditional probability which is greater than a half. But, in that case, it seems that the interesting version of question (1) would be whether the probability that accidental or chance processes caused the formation of the first form of living matter from non–living matter is less than one half. And then it is hard to see how Overman's discussion of extremely small probabilities is in any way relevant. However, if Overman is talking about conditional probabilities, then there is a question about the tacit or disguised conditions upon which the apparently unconditional judgements are based.

There are many more questions to ask—about prior probabilities, and coherence constraints, and the evolution of probability judgements under the impact of new evidence, and so forth—but I think that we have probably already gone far enough to see that the questions with which Overman begins his enquiry are actually far from clear. Given that Overman tells us so little about how to interpret his probability talk, it is impossible to assign a precise interpretation to the questions with which he begins. Since Overman's claims to have special expertise in matters of 'logic and the validity of premises, inferences and conclusions as they relate to an examination of evidence', this lack of clarity in the questions which govern the ensuing enquiry is worrying to say the least.

In the face of these difficulties, perhaps the most charitable thing to do is to reinterpret the questions again, leaving out any mention of probability values. I think that it is pretty clear that Overman would be happy to have the three questions framed as follows:

(1) Is it plausible to suppose that accidental or chance processes caused the formation of the first form of living matter from non–living matter?

(2) Are current self–organisation scenarios for the formation of the first living matter plausible?

(3) Is it plausible to suppose that accidental or chance proceses caused the formation of a universe compossible with life?

Overman claims—or, at any rate, is most charitably interpreted as being committed to the claim—that there are probabilistic calculations which support negative answers to each of (1), (2) and (3). I shall examine this claim of Overman's to critical scrutiny, beginning with (1). (I postpone discussion of Part 2 of the book until later. Despite Overman's claims, it is essentially irrelevant to the development of his arguments.)

Part 3: Case Against Accident from Mathematical Probabilities in Molecular Biology

The core of this chapter is some familiar calculations of the likelihood of the emergence of complicated structures given random permutations of the building blocks which make up those complicated structures (e.g. Hoyle and Wickramasinghe on the direct development of a bacterium in a prebiotic soup; Yockey on the direct development of a single iso–1–cytochrome c protein in a prebiotic soup; Bradley and Thaxton on the direct development of proteins from amino acids in a prebiotic soup; Morowitz on the direct development of a bacterium in an equilibrium ensemble; Kuppers on the direct development of the nucleotide sequence of a bacterium in a prebiotic soup; and so forth). Overman claims—at least on my reconstrual of his argument—that these calculations demonstrate that it is not plausible to suppose that accidental or chance processes caused the formation of the first form of living matter from non–living matter.

Whether this claim of Overman's is right or not depends a little on how the initial question is understood. If the claim is just that the first protein—or bacterium, or RNA molecule, or whatever—did not arise directly from random permutations and combinations in a prebiotic soup of the amino acids which make up these structures, then this seems to be a claim with considerable plausibility. However, if the claim is meant to be there is no sequence of accidental or chance processes which could lead to the development of the first protein—or bacterium, or RNA molecule, or whatever—in a prebiotic soup, then it is less clear that what Overman says is right. If there were a large number intermediate stages between the amino acids and the first protein—or bacterium, or RNA molecule, or whatever—then there might be some kind of evolutionary story which could be told. The mere mathemtical calculations by themselves do not rule out this possibility.

Perhaps it would be most charitable to suppose that Overman does intend the claim to be understood in the strict sense which only rules out direct production through random permutations. However, while the point of this argument stands quite independently of the arena in which the direct production is supposed to occur—it wouldn't matter for the purposes of the mathematical argument if the equilibrium system were nothing much like the Oparin prebiotic soup—Overman also wants to argue that it is not plausible to suppose that there was any kind of development of life in a prebiotic soup. Thus, for example, he spends some pages discussing the question whether there was oxygen in the atmosphere of the early earth, a problem raised by the presence of right–handed amino acids in the prebiotic soup, some reasons for doubting that there ever was a prebiotic soup, and so forth. While these issues have nothing to do with the mathematical argument, they are clearly relevant to the question whether accidental or chance processes caused the formation of the first form of living matter from non–living matter. However, since one might suppose that accidental or chance processes caused the formation of the first form of living matter from non–living matter, without supposing

that accidental or chance processes caused the formation of the first form of living matter from non–living matter *in a prebiotic soup*, it seems clear that there considerations really belong in a separate part of the book.

The point here is one about the clarity and cogency of Overman's argument. The 'standard' Oparin model—as outlined at pp.38–9—is not obviously one which falls prey to the mathematical argument. In his telling of the story, Overman has it that 'heterotrophs increased in complexity, and nucleic acids were formed which gave them the ability to reproduce'. I can't see how one could argue that this story is shown to be false by the kinds of mathematical calculations which Overman describes. So, in order to make the logical status of his case clear, Overman ought to distinguish between those theories which fall victim to the mathematical argument and those which do not—and he ought also to note that there may be versions of Oparin's theory which do not fall victim to the mathematical argument.

Of course, Overman doesn't think that there are plausible versions of the Oparin model which do not fall to the mathematical argument. One argument which he gives at this point is that 'natural selection does not exist in prebiological molecules'. However, it is hard to see what the argument is here. True enough, one might think that it is more or less analytic that natural selection does not occur in prebiological molecules. However, the crucial question is whether there are precursors to the familiar RNA and DNA in which natural selection does occur. If there are simpler molecules which could have stood at one end of an evolutionary chain which lead eventually to the familiar RNA and DNA, then there could be versions of the Oparin model which survive the mathematical argument. Whether we should call those simpler molecules 'biological'—because they are subject to natural selection—or 'prebiological'—because they are prior to the RNA and DNA which we currently take to be the simplest biological molecules—is neither here nor there.

Even if I am right about the argument discussed in the previous paragraph, it seems to me to be plausible to think that there are doubts about the viability of any model of the kind proposed by Oparin. The difficulties which Overman mentions—and the fact that there is a time window of only about 130 million years between the appearance of conditions suitable for the sustenance of living things and the appearance of life itself—suggest that it is unlikely that life began in a prebiotic soup. However, in the absence of more obviously plausible competing hypotheses, I am not convinced that this family of models can be simply discarded. At present, it seems unlikely that any model of this kind is correct; but who knows how things will look when we have more evidence to hand. (Note, by the way, that we shouldn't give any credence to the claim that the inability of current researchers to form living organisms from amino acids shows that there could have been no such development in the natural world. Overman seems to think—cf. pp.48–9—that if researchers have been unable to create life under artificial conditions, then life could hardly have developed in the natural world without intelligent guidance. But, on the one hand, that researchers have hitherto been unable to create life under artificial conditions may be very weak evidence that they shall not succeed in doing so in the future; and, on the other hand, if researchers do succeed in creating life under

carefully controlled laboratory conditions, we may be able to develop from those experiments a plausible account of how life arose in the natural world. To suppose that, in principle, the fact that researchers use their intelligence in designing and conducting experiments *rules out* any role for those experiments in the understanding of the development of life without intelligent guidance is absurd; one might as well argue that the fact that researchers use their intelligence in designing and carrying out experiments on subatomic particles rules out any role for those experiments in the understanding of the behaviour of those particles in circumstances in which they are not subject to intelligent guidance.)

There are some other things in Part 3 which should not be allowed to pass without comment. For instance, the chapter concludes with a discussion of the problems raised by the complexity of the human brain for 'the proponents of accident'. (Overman quotes approvingly from Denton at this point.) However, the topic of Part 3 is whether accidental or chance processes caused the formation of the first form of living matter from non–living matter, not whether evolution can bring about increases in the complexity of organisms. Nobody thinks that the first human brain arose as a random assemblage from component cells, or proteins, or amino acids, or whatever. So it is utterly bizarre that Overman includes this discussion at this point in the book.

There is also a theoretical section on mathematical probability—3.6 Calculating mathematical probabilities of accidental or chance events—which contains some curious material. At one point, Overman appears to confuse Bayes' Theorem with the standard defintion of conditional probability. (See p.53.) At another—and this is a more serious point—he approvingly cites du Nouy's claim that 'the probability of an event is equal to the numberof outcomes favourable to the event divided by the total number of possibilities with all possible outcomes considered to be equally probable'. (Again, see p.53.) Of course, the problem here is with the last clause, i.e. the claim that all possibilities should be considered to be equally probable. As Bertrand taught us, there is no sense to be made of this suggestion. Finally—and this point will take a bit more developing—Overman provides a critique of some writings by Emiliani and Dawkins which cries out for discussion.

Here is the passage from Emiliani:

> To show you how efficient natural selection can be, imagine that you want to have the entire Bible typed by a wild monkey. What are the chances that such a monkey, typing at random, will come up with the Bible neatly typed without a single error? The English Bible (King James version) contains about 6 million letters. The chances of success are therefore about one in $26^{6,000,000}$. .. I wouldn't exactly wait around. Suppose, however, that I introduce a control (the environment) that wipes out any wrong letter that the monkey may type. Typing away at one letter per second, and assuming an average of 13 errors per letter (half of 26), the monkey will produce the Bible in 13 x 6,000,000 seconds = 2.5 years. ... This is precisely what the environment does. It knows what kind of organism would best fit and if the wrong one appears it rejects it as you reject a wrong letter. All the environment does is to effectively

> eliminate all the random changes that are in the wrong direction. Given the chemical and environmental conditions of the primitive earth, the appearance of life was a foregone conclusion.

This passage does exhibit some worrying features. Three points to note. *First*, there is a sense in which the monkey 'types the entire Bible while typing at random' : the choice of keys which the monkey strikes is random, and the end result is a perfect copy of the Bible. However, it must be noted that the choice of which keystrokes to retain and which to delete is determined entirely by the activities of the control—a large majority of the keystrokes which are made by the monkey are simply ignored. Moreover, *second*, the analogy between the role of the control in this story and 'the environment' in processes of natural selection is not very close: it is simply not true that 'the environment' acts as a kind of filter to successively select from amongst random changes those changes which are required in order to produce a desired end product. Because the role of 'the environment' is more subtle that the 'effective elimination of random changes that are in the wrong direction', there is no support at all for the conclusion—if indeed it is intended to be a conclusion—that, given the chemical and environmental conditions of the primitive earth, the evolution of life was a foregone conclusion. Even worse, *third*, the last sentence of this passage talks about the *appearance* of life on earth. This suggests that Emiliani may have intended to construct an analogy for naturalistic accounts of the appearance of life on earth solely as the result of random processes. But, while his analogy might support the claim that random processes had a role to play, it could hardly support the claim that *nothing but* random processes had a role to play: for that all depends upon how 'the environment' plays the role which is assigned to it.

Perhaps one could tell a story of this kind which provides a better—though still imperfect—analogy to familiar naturalistic accounts of the evolution of life on earth. One could have a team of monkeys, one producing words, another producing sentences, another producing paragraphs, another producing chapters, and so on—each by random arrangement of the products of the previous monkey. The first monkey hits the keys of the typewriter at random. Each time he hits the space key, the product of his typing is sent to a store. If it is a string of English, it is retained; otherwise it is discarded. The next monkey takes the words from this first store, and assembles them at random; each time he adds a stop—all of the punctuation marks are saved in the store along with the more familiar words of English—the product is sent to the next store. If it is a sentence of English, it is retained; otherwise it is discarded. And so on, for successive monkeys and successive stores. This process WILL greatly increase the chances of success in the project at hand. Of course, we could increase the chances even more by including further intermediate steps between letter and word, word and sentence, and so forth; but the intuitive idea should be clear enough. Told this way, there is a much better analogy to familiar naturalistic accounts of the appearance of life on earth; though, of course, the selection of which words, or sentences, or paragraphs to maintain is still something which is not very similar to the process of natural selection. In order to get an even better analogy to familiar naturalistic accounts of the appearance of life on earth, we should like to replace the role of external intelligence entirely; but, in the case at hand, this would require the introduction of some sense of 'competition' between relevant strings—of

letters, or words, or sentences, or whatever—and that is harder, though perhaps not impossible, to do. (Perhaps one could try the suggestion that items which 'make sense' have greater survival value than those which do not. And perhaps one could introduce some kind of duplication mechanism in the stores: rather than have an external intelligence remove the nonsensical strings, one could have the 'sensible strings' being duplicated at a much faster rate, so that the monkey at the next stage is far more likely to be working with sensible strings. Finally, one might try the introduction of a random removal of strings from the stores as well. With all of this, one would still greatly increase the likelihood of getting out the works of Shakespeare in a reasonable amount of time, using only a team of monkeys which perform various random tasks and the resources of random activities in the stores.)

Now, Overman agrees that Emiliani's argument is questionable. Here is what he says about it:

> The unwarranted and unproven assumptions in this analysis are remarkable. An invalid assumption is that the 'environment wipes out any wrong letter', because this is the very assumption which must be proved to show that random processes can produce the Bible. Note that, without any evidence, the term 'environment' is endowed with characteristics including powers of intelligence to 'know' and 'reject' wrong letters. This is an example of circulus in probando; the answer is assumed in the premise, and the 'environment' will 'know what kind of organism is best and reject wrong letters or sequences'. The assumptions are made without any rationale and ignore a fundamental principle in science: natural selection does not exist in prebiological molecules. It is generally agreed that natural selection can only act on systems capable of replication. Natural selection alone is not sufficient to explain the origin of life. The relevant analogy is to the origin, not to the replication or mutation of life. The analogy fails because it does not relate comparable terms in a consistent context. The fallacy in this circularity is compounded by the arbitrary selection of the number 13 with an assumption that only one half of the Roman alphabet will be needed to produce the correct letter. What is the empirical evidence or rationale giving any validity to the assumption of 13 errors per letter. If a golfer assumes that she will birdie nine out of eighteen holes in golf and arbitrarily assigns a score of one under par for the last nine holes and plays only the first nine holes with a bogey on each hole, she will have assigned herself a score for the eighteen holes equal to par. But the reasonableness of that score is based on an assumption which must have some relationship to her abilities and to the probability of her scoring nine consecutive birdies. The empirical results of her previous scores provide a rational basis for testing the validity of her assumptions. The analysis also uses the term 'environment' as the entity responsible for appointing the precise desirable values or conditions required to achieve a particular purpose. When the term 'environment' is used with characteristics similar to a conscious mind, the question arises concerning the distinction between the term and the word 'intelligence' or 'Superior Intelligence'. If one uses the term in a manner implying intelligence, one is no longer discussing random, chance or accidental processes. The monkey is not producing a document by chance under the conditions given in the quotation.

Remember that Overman's claim to expertise lies in 'logic and the validity of premises, inferences and conclusions as they relate to the examination of evidence', and that we most concerned to assess whether or not the case which he presents is clear, compelling and well–argued. So how does he do here?

Let's begin with the second paragraph. Overman claims that it is arbitrary to suppose that there will be 13 errors per letter *on average*. However, while I suspect that the assumption that there will be 13 errors per letter on average is mistaken, I am sure that it is not arbitrary—and I am also sure that the reasons which Overman gives are quite beside the point. The monkey is supposed to types letters at random, and to move on to the next letter each time he gets a designated letter. So, in effect, the monkey is performing repeated trials of choosing one item from 26, with replacement, until it obtains a designated item. What we want to know is the first value of n for which $(25/26)^n$ is less than a half. This happens when n is a little bit less than 18. (And so the final answer which Emiliani should have computed is about 3.5 years, rather than about 2.5 years.) Emiliani's choice of 13 relies on the mistaken assumption that we have choices without replacement; but this mistake is irrelvant to the point which he is trying to make. Moreover, it should have been obvious to Overman that there is some fairly small number which is correct, even if that number is not 13: there is no way that 'empirical evidence' can have anything to do with what is clearly a fairly straightforward *a priori* mathematical calculation.

What about the other two paragraphs? Well, in order to assess these, we need to ask what the point of Emiliani's analogy is supposed to be; and we need to ask what Emiliani is trying to do in the passage in question, and what he could reasonably hope to do with the analogy which he develops.

Suppose that the idea behind the analogy is to show how random processes can be involved in the production of highly ordered results. You might have thought that having a monkey typing randomly at a keyboard could not plausibly be involved in the production of the works of Shakespeare. However, as the story makes clear, it all depends upon the other resources which are available… couple your monkey to suitable assistance, and you can get out the works of Shakespeare in a short period of time. This much seems unquestionably right. Moreover, there may be a sense in which this is 'precisely what the environment does in natural selection': it provides resources which, when coupled to random processes, allow the production of more highly organised entities. Against this way of interpreting what Emiliani says, the objections which Overman makes are clearly not to the point.

However, as I noted above, this interpretation is perhaps rather charitable to Emiliani. If we suppose instead—as Overman does—that Emiliani's analogy is supposed to show how life could have developed *solely* as the result of chance processes, then it is clear that Overman has some good points to make against him. When Emiliani writes: '... the environment knows what kind of organism would best fit and if the wrong one appears it rejects it as you reject a wrong letter; all the environment does is to effectively eliminate

all the random changes that are in the wrong direction ...', Overman is quite right to point out that 'the environment' does not literally know anything, and that it is not to be supposed that there is 'right direction' which 'the environment' is aiming towards.

How should Emiliani's passage be interpreted? In the absence of the larger context in which the paragraph is embedded, it's hard to say. Certainly, it seems implausible to me to suppose that Emiliani intended to be constructing an argument by analogy for the final sentence of the quoted paragraph: as I noted earlier, it is simply obvious that such an argument is very weak. If this weak argument is what Emiliani intended, then Overman is right to criticise him; but it is worth pointing out that this is an argument which almost no defenders of naturalistic accounts of the appearance of life on earth would—and no defenders of naturalistic accounts of the appearance of life on earth should—wish to endorse. Moreover, if this is the right interpretation of Emiliani, then Overman's criticisms are strangely formulated: the point isn't that the argument is is circular, or that terms are wrongly defined, or that it relies upon unsubstantiated assumptions; rather, the point is that the analogy which is constructed simply fails to support the conclusion which is allegedly drawn from it.

Since I haven't been able to track down a copy of Emiliani's book, I can't take this discussion any further at this point. So, let's move on to a more important point. After offering his criticism of Emiliani, Overman goes on to make the following observations:

> Richard Dawkins constructs a similar failed analogy in his book *The Blind Watchmaker*. Dawkins understands the odds against chance as the sole cause of life and presupposes that the process of natural selection determines the 'correct' letters which the monkey preserves. However, for the monkey to preserve the correct letters in the sequence requires an assumed intelligence apart from and greater than the intelligence of the monkey. This intelligence must have knowledge of the letters which construct a meaningful sentence. Without such an intelligence, no principle exists for deciding which letters should be preserved. Natural selection does not qualify as such an intelligence, because it is a process, not something like an intelligent mind which knows the alphabet and the structure of a meaningful sentence. Dawkins cannot have it both ways. He cannot logically assert that a process without the characteristics of a mind has the characteristics of a mind and the knowledge required to 'know' which letters to preserve. Such an assertion fails because it assumes a self–contradiction.

As critcism of Dawkins, this is simply absurd. Dawkins does make use of analogies involving monkeys typing randomly on typewriters in *The Blind Watchmaker*, but he certainly doesn't think that the argument which Overman attributes to Emiliani is any good, and he nowhere relies on an analogy of this problematic kind. It is noteworthy that Overman gives no page number reference to Dawkins' book: so much for Polkinghorne's claim that Overman brief is argued 'scrupulously'! At this point, at least, Overman displays the worst characteristics of the family of anti–evolutionary literature to which his book belongs: instead of a reasoned assessment of Dawkins' carefully argued book, we get spurious implication of guilt by association.

In sum: Overman's 'case against accident and chance from mathematical probabilities in molecular biology' is flawed in various ways. Most importantly, there is a persistent unclarity in the use of the expression 'accident and chance' which needs to be emphasised. On the one hand, if 'accident and chance' means that highly complex structures are formed solely by chance rearrangements of their micro–components, then it seems to me that there is a plausible case against accident and chance which derives from calculations of mathematical probabilities. However, careful evolutionary theorists—and Dawkins is a case in point—do not suppose that the appearance of life on earth was an accident in this sense. On the other hand, if 'accident and chance' means that highly complex structures formed from less complicated constituents without the intervention of intelligent design, then there is not even the beginning of a case against accident and chance from the calculations of mathematical probabilities which Overman gives. Of course, in saying this, I am taking it for granted that careful evolutionary theorists suppose that there is some kind of evolutionary story to be told about the development of RNA, DNA, and so forth—but, *contra* Overman's repeated assertions, this claim is not inconsistent with the 'fundamental principle of science that natural selection does not occur in prebiological molecules'. Moreover, it is not necessarily any comfort at all to theists that the details of this evolutionary story are currently unavailable to us: when the competing theistic theories are placed on the table, it may well become clear that it is far more reasonable to suppose that there is some naturalistically acceptable theory which remains to be discovered that it is to suppose that one of the theistic theories is correct.

PART 4: The Problem of Complexity—The Generation of Sufficient Information Content

The core of this chapter is a couple of very unclear arguments which are supposed to show that what Overman calls 'self–organisation' theories of the appearance of life on earth are implausible. Examples of these theories include (i) theories which suppose that RNA might have acted as a catalyst in a prebiotic soup; (ii) theories which rely on the observation that order can spontaneously appear in systems which are far from equilibrium; (iii) Cairns–Smith's proposal that clays may have formed the first self–replicating structures; (iv) Corliss' proposal that life began in deep sea hydrothermal vents; (v) Morowitz's proposal that metabolism recapitulates biogenesis; (vi) Kauffman's proposal for the origin of complexity on systems 'on the edge of chaos'; (vii) panspemia hypotheses, including, especially, the proposal that life arose on Mars and then migrated to the earth. Most of these theories are highly speculative, and subject to various kinds of difficulties. However, Overman seems to want to argue that there are *in principle* difficulties which face any theory of this kind.

The concluding sentence of the previous paragraph is a little incautious: it is not very clear exactly what Overman wants to argue with respect to 'self–organisation' theories. One claim which he makes is that we don't currently have a clearly acceptable theory of

this kind. True, no doubt. But, in itself, this is not even weak evidence that no theory of this kind is right. Another claim which he makes is that it is a matter of 'metaphysical faith' to believe in naturalistic accounts of the appearance of life on earth: however, it is quite unclear how objections to current 'self–organisation' theories could support this view. A third claim, which is suggested by the kinds of objections which he lodges against the 'self–organisation' theories mentioned above, is that no theory of this kind could be made to work. Overman is careful not to make this claim; but the major objections which he makes to 'self organisation' theories, if successful, would support that much stronger claim. Or so it seems to me.

Overman's has two principal complaints against 'self–organisation' theories. On the one hand, he claims that they typically confuse 'order' with 'complexity'. And, on the other hand, he claims that they are typically unable to explain the origins of the informational content which is to be found in the genetic code. If either of these objections can be sustained, then it looks as thought we might have weapons which can rule out any naturalistic account of the origins of life on earth. So we should take a look.

Overman makes a big point of the distinction between 'order' and 'complexity', and of the fact that he uses the word 'complexity' in the way in which it is used in information theory. However, he nowhere gives a precise explanation of the information–theoretic concept of complexity. The closest he comes is at p.75, where he says, roughly, that the complexity of a structure is given by—or depends upon—the minimum length of complete descriptions of that structure, i.e. by what he calls the 'information content' of a structure. Read straight, Overman's explanation would seem to have the consequence that a glass of water is far more complex than the genetic code: after all, it would require far more to precisely describe the state of the glass of water—the position and momentum of each of the constituent particles of the glass and the water—than it would to describe the state of a DNA molecule. Perhaps it might be said that Overman clearly doesn't intend to say that systems in thermal equilibrium are complex; however, the point is that it is far from clear what he does intend to say.

I suspect that what Overman really wants to say is that biological molecules exhibit a certain kind of combination of 'order' and 'complexity' which is unparalleled elsewhere in the universe. On the one hand, these molecules have a 'regular, predictable, pattern': the underlying double helical structure. On the other hand, these molecules involve irregular and aperiodic sequences of the nitrogen bases—and hence nucleotides—which they contain. It is the combination of organised structure and irregular aperiodicity which Overman wishes to characterise with the title 'complexity'. However, it is important to bear in mind that this kind of complexity has two components—the organised structure and the irregular periodicity—which may admit of different kinds of explanations. In particular, it should be noted that there is 'order' in biological molecules; and this 'order' might be explained by the kinds of 'self–organisation' theories which Overman examines, even if the 'complexity' which is found in these molecules has to be given a different kind of explanation (perhaps largely in terms of natural selection). This speculation of mine is rather vague and ill–defined; however, the target is even less

clearly defined, so it is hard to do better. Instead, I shall now turn to the second—and more important—of Overman's main complaints against 'self–organisation' theories.

Overman's main argument against 'self–organisation' theories—i.e. against naturalistic theories of the origins of life which do not fall foul of the argument from mathematical probability which is developed in Part 3 of the book—is that 'they do not present a plausible method of generating sufficient information content in the time available' (74). In the most developed version of the argument which he presents, it runs as follows:

> The paradigm for the emergence of life contains algorithms which must have at least as much information content as the genetic messages they claim to generate. The method for such generation is not clear. Because the information content or complexity in the laws of physics is much less than the content in the genome, the gap in content must be explained. (85)

Much about this passage is unclear. The first sentence suggests that Overman supposes that it is *a priori* that more complex things cannot arise from simpler things: the 'algorithm' which governs the development of life must have more information content than the genetic messages which are the outcome of that 'algorithm'. But what reason is there to suppose that naturalistic theories should be based on the assumption that there are 'algorithms' which govern the development of life? As Daniel Dennett argues in this recent book, the core of Darwin's dangerous idea is precisely the suggestion that more complex things can arise from simpler things without the oversight of other more complex things. The 'information content' in the genome is the result—the end product—of a long process of natural selection: random mutation and competition for scarce resources. If naturalistic theories are correct, then this process is not overseen by anything—though it does all occur in conformity to the laws of the relevant physical sciences—but that is no barrier to the explanation of how the genome comes to have the information content which it in fact has. Moreover—as we noted earlier—it is no objection at this point to insist that there is no natural selection in prebiological molecules: the question is whether there can be naturalistic accounts of the development of the building blocks for current life as a result of random mutation and competition for scarce resources.

The third sentence of the above passage also calls for comment. Allegedly following Yockey, Overman supposes that there is some difficulty in the explanation of the complexity of the genome which follows from the lack of complexity of the laws of physics and chemistry. It is very hard to find a cogent worry here. After all, it is very rare for explanations to be couched solely in terms of laws: typically there are also boundary conditions which play a role in explanation. Moreover, in the case of the genome, it is very plausible that boundary conditions are going to play a big role in the explanation of the complexity which it exhibits: we have good reason for thinking that the origin of life would not have occurred if conditions on the early earth were different in various kinds of ways.

Even setting aside these considerations, there are problems about the characterisation of complexity which remain. Consider, for example, the Mandlebrot set. This set is generated by a single, very simple mathematical equation. However, if we try to describe a graphical representation of the Mandlebrot set in terms of the points which are occupied, then we shall end up with a very long description indeed: there is a great deal of apparently aperiodic behaviour which is encoded in the single mathematical equation. Now, shall we say that the Mandlebrot set is simple or not? Well, it seems that that must depend upon the level of description which is chosen. Seen from one point of view, the set is extremely simple: but seen from other points of view, it is extraordinarily complicated. Can we rule out the suggestion that similar considerations apply in the case of the genome, and its relation to underlying law? Until we are given much more precision in the use of the terms 'complexity', 'information content', and so forth, it seems to me to be pretty much impossible to say.

In sum: the case which Overman makes against theories which are not ruled out by the consideration of mathematical probabilities is quite weak, mainly because it is so unclear how the main arguments are supposed to go. Perhaps Overman is right to claim, for example, that Cairns–Smith's hypothesis that clays formed the first self–replicating structures can't serve as an element in an adequate explanation of the origins of life. However, it is unclear that Overman's worries about the regularity of the structure of crystals is sufficient to undermine Cairns–Smith's suggestions. After all, at least as the hypothesis is presented by Overman, the claim which Cairns–Smith makes is just that clays formed the first self–replicating structures: at that leaves room for plenty of intermediate steps before we arrive as DNA. So Overman's insistence that 'the information density in a crystallite is not at all similar to the information content in DNA' may be correct but irrelevant.

In general, this chapter is very poorly organised and very poorly argued. One example of the poverty of the argument will have to suffice. Consider the following, from pp.91–2:

> Kauffman and some other Santa Fe complexologists base their positions on the following syllogism:
>
> > There are simple sets of mathematical rules that when followed by a computer give rise to extremely complicated patterns. The world also contains many extremely complicated patterns. Conclusion: Simple rules underlie many extremely complicated phenomena in the world. With the help of powerful computers, scientists can root out these rules.
>
> This reasoning was discredited by Naomi Oreskes .. who argued that the verification and validation of numerical models of natural systems was impossible, because natural systems are never closed. Oreskes argued that it is impossible to demonstrate the truth of any proposition except in a closed system based on pure mathematics and logic. Her argument may be summarised in part by the following example:

> "If it rains tomorrow, I will stay home and revise this paper." The next day it rains, but you find that I am not at home. Your verification has failed. You conclude that my original statement was false. But, in fact, it was my intention to stay home and work on my paper. The formulation was a true statement of my intent. Later, you find that I left the house because my mother died, and you realise that my original formulation was not false, but incomplete. It did not allow for the possibility of extenuiating circumstances. Your attempt at verification failed because the system was not closed.

Despite Orekes' and others challenges, Kaufmann concludes ....

Overman clearly supposes that Oreskes has a good objection to Kaufmann's "syllogism". However, if Kaufmann's argument is interpreted as a piece of deductive reasoning, then it is clearly invalid: just because *some* extremely complicated patterns are generated by simple sets of rules, it does not follow that extremely complicated patterns in the world are governed by simple sets of rules (the extremly complicated patterns in the world might be amongst those extremely complicated patterns which are not governed by simple sets of rules). Moreover, it is hard to see how Kaufmann's "syllogism" can be more sympathetically interpretted: the most that follows from his premise is that it *may* be that there are simple sets of rules which underlie extremely complicated patterns in the world.

Overman also supposes that Oreskes manages to successfully discredit Kaufmann's reasoning. He tells us that Oreskes claims that 'it is impossible to demonstrate the truth of any proposition except in a closed system'. But how is this relevant to Kaufmann's claim? To the extent that Kaufmann's "syllogism" is a demonstration, it is a derivation of a conclusion from a premise. If the premise were true, and the derivation were correct, then it is obvious that the conclusion would also be true; moreover, there would be a good sense in which we would thereby have a demonstration of the truth of a proposition. Of course, it might be objected—even waiving the worry about the possibility of logical error—that there is no guarantee that the premise is true. Well, yes ... but that's always true, even in the case of mathematics and logic. Since we aren't told what 'closed systems' are, it is not clear whether or not Oreskes is entitled to the claim that 'closed systems' are the province solely of logic and mathematics: however, it is worth pointing out that there can be deductively closed systems dealing with any subject matter you like.

Now, it might be thought that Oreskes' example is meant to show, precisely, that there cannot be deductively closed systems in areas other than mathematics and logic. But, quite apart from the absurdity of this claim, is it obvious that her example is not up to the task. Consider the original statement: "If it rains tomorrow, I will stay home and revise this paper." If, tomorrow, it rains but I do not stay home and revise my paper, then what I have said is simply false. Of course, there is another statement: "If it rains tomorrow, and nothing out of the ordinary happens, then I will stay home and revise my paper" which is true in the circumstances which Oreskes envisages. But it would be a gross error to conflate these two claims: they mean very different things, and they are true in quite different circumstances. Perhaps, when I utter the words "If it rains tomorrow, I will stay

home and revise my paper", I shall often be most charitably interpretted to be committing myself only to the proposition that if it rains tomorrow, and nothing out of the ordinary happens, then I will stay home and revise my paper—but, in that case, learning that it is raining but that I am not at home is simply insufficient to establish that what I *said* was false. Unless Oreskes wants to deny that we can ever make precise claims—i.e. that we can ever say anything which can be assessed for truth and falsity—there is no way of making sense of her objection to Kaufmann: for, once we fix on an interpretation of what I said when I uttered the words in question, we can then go on to ask whether what I said was true given the way things turned out.

The example which I have discussed here is but one of many; there seems little point in considering any of the other examples here.

PART 5: Case Against Accident From Precision of Values in Particle Astrophysics Required for the Formation of Life

This chapter of the book has four main parts. It begins with a discussion of a standard inflationary big bang model for the universe, and of projected completions of this theory which involve string theory, supergravity, and the like. It continues with a discussion of the alleged evidence for cosmological fine–tuning which may be located within the framework of the standard cosmological model. Next, it turns to a discussion of non–standard cosmological theories which seek either to avoid the initial singularity of the standard model, or to provide a naturalistically acceptable account of the apparent fine–tuning in the standard model, or both. Finally, it concludes with some discussion of anthropic principles, and their role in the debate about fine–tuning. I shall make some comments on each part in turn.

Overman's presentation of 'standard physical theory' is not too bad in its broad outlines, though it contains lots of problematic particular claims. For instance, he claims that Hubble *observed* the expansion of the galaxies in 1929 (105); that Christian Doppler discovered the Doppler effect for *light* (105); that the most common form of the electromagnetic *force* is light (112); that quarks and leptons have *infinite* density (114); and so forth. At p.109, he gets into a huge tangle trying to describe the initial singularity: 'only after the Big Bang at Planck time—10^{-43} of the first second—do space and time exists as we understand those terms. (Why isn't the Planck time t=0, if there is no time before it?) However, to insist on these infelicities is perhaps to quibble: Overman does a pretty fair job of summarising the popular scientific literature on these matters. One point to note is that Overman is very strongly committed to the truth of these theories: even his acknowledgement (124) that string theory is highly speculative and lacking in empirical foundation is carefully hedged. We shall return to this point later.

Much of Overman's presentation of the alleged evidence for fine–tuning is also a reasonably accurate portrayal of the current state of play. He cites the usual numbers, but

pays no attention to the question of the probability that the theories to which these numbers belong are false. Moreover, he also fails to note that the standard fine–tuning arguments only consider one dimension at a time: they pay no attention to the question of what would happen if several fundamental values were to vary simultaneously. Finally, he has a section on the applicability of mathematics to the physical world which contains a number of questionable claims. According to Overman, it is hard to see how one could give a naturalistic account of our ability to do physical science: what survival value is there in developing brains which are well–suited to this task? (144) But, of course, there is a straightforward answer to this question: there is obvious survival value in having big brains; and it is a fortunate consequence of having a big brain that one is able to do mathematical physics. Even more bizarrely, Overman claims that, while there is 'survival value' in the development of Newtonian physics—'its concepts were derived from observation of the world'—there is no corresponding 'survival value' in the development of Einsteinian physics (144–8). Setting aside the point that our ability to do mathematical physics might be a side–effect of natural selection, the obvious point to make here is that there is no difference between Newtonian physics and Einsteinian physics in point of the connection to natural selection: Newtonian physics is no less a recent 'mathematical abstraction' than is Einsteinian physics. Overall, as I said in connection with the previous section, it would be quibbling to insist too much on the infelicities: there are genuine problems in this area to which friends of naturalism should attend.

Overman's discussion of 'non–standard' cosmological theories is somewhat perfunctory: the only view which gets serious consideration is the Hartle–Hawking 'no boundary' cosmology. Moreover, it contains some questionable moves. We have already noted that Overman finds it hard to give a consistent account of the Planck time; these difficulties rearise with a vengeance when he turns to discuss accounts of what happened 'before the Planck time'. (152–4) Granted, these difficulties are not entirely his fault: much of the popular and scientific literature on this topic is shot through with conceptual confusion. Further, there are various points at which Overman disparages the views under consideration on the grounds that they are 'highly speculative'. But, of course, much of the material which appeared in the first part of this chapter is also highly speculative: we have precious little in the way of direct evidence for strings, compressed dimensions, inflationary expansion, and the rest. Finally, this chapter contains a very curious discussion of the role of a creator in the Hartle–Hawking cosmology. On Overman's account, even if there is no need for a creator to set things rolling, there is still a need for a creator to conserve the universe in existence from one moment to the next. Moreover, there is a kind of argument from contingency which proves that there must be a God who conserves the universe in existence. (164–5) But, on the one hand, if this argument from contingency were any good, then surely we wouldn't need to bother with the cosmological considerations about origins—we could just appeal directly to it in order to establish the existence of God. And, on the other hand, if we can't just appeal directly to this argument to short–cicuit the debate about origins, then it is hard to see what role it could play in making theists feel better about the Hartle–Hawking cosmology. Perhaps the idea is that cosmological considerations about origins are likely to have more pull against naturalistically inclined non–theists than do standard considerations about contingency; however, I can't see any reason to suppose that this must be so.

There is supposed to be an argument against ‘chance and accident’ in this chapter; I guess that it comes in the last section, on anthropic principles. In particular, Overman mentions the “Strong Anthropic Principle”: ‘multiple universes and infinite multiple universes’ (174). Overman claims, absurdly and without argument, that the theory ‘commits the logical fallacy of *petitio principii* if there are only a finite number of universes’ (175). He then claims, even more absurdly, that if there is an infinite array of universes, then, for any person, ‘somewhere he or she plays basketball better than Michael Jordan and golf better than Tiger Woods’ (175). The former claim is absurd on three counts: *first*, because theories do not commit argumentative fallacies; *second*, because it is hard to see what circularity could be involved in the claim that there are only finitely many universes; and *third*, because the fine–tuning calculations might be taken to be evidence of a lower bound on how many universes there are. The latter claim is absurd because it conflates two different conceptions of alternative universes: one which takes alternative universes to be different ways that our world could be; and another which takes alternative universes to be entirely independent and self–standing domains. In the sense which is relevant to the many worlds hypothesis response to fine–tuning arguments, alternative universes are entirely independent and self–standing domains: no object belongs to more than one of these domains. However, the sense in which there is a location at which I play basketball better than Michael Jordan and golf better than Tiger Woods is the other sense: alternative universes are different ways that the actual world could be, and they are peopled by more or less the same inhabitants as the actual world. True enough, some philosophers have defended the claim that we should interpret modal talk—i.e. talk about what is possible, what might have been—in terms of entirely independent and self–standing domains, and hence that we should conflate the two senses which I have distinguished. But, if we follow these philosophers, there is no absurdity in the claim that there is somewhere where I play basketball bettter than Michael Jordan and golf better than Tiger Woods, since this is just a disguised version of the claim that it is logically possible for me to do these things.

Of course, it shouldn’t be too quickly assumed that the ‘many universes’ response is the only response which can be given to the fine–tuning arguments. However, since Overman doesn’t consider any other possible responses, it will suffice to note that, for the one response which he does consider, the criticisms which he makes are very weak indeed. Perhaps one might go even further. There is an enormous amount of ostensibly factual material assembled in Chapter 5, but none of it plays any role in the ‘argument’ against ‘accident and chance’ which is developed therein. As in the previous two chapters, Overman seems so determined to establish that his arguments are founded in recent scientific activity that he neglects to put any care into the development of the arguments. Cynics may conclude that this is because there are no arguments to develop: the so–called ‘case’ which Overman develops is merely a not very successful attempt at ‘blinding with science’. I have some sympathy for this cynical response: certainly, I would need a lot of convincing that there are any decent arguments buried in the scientific rubble.

PART 2: Verbal and Mathematical Logic Relating to the Questions Presented

As I mentioned earlier, this part of the book is quite strange. It has five major parts, the most important of which are a discussion of some errors in reasoning which 'appear from time to time in the relevant literature involving molecular biology and particle astrophysics'. (5), and a discussion of the 'limits on logic' which are imposed by quantum mechanics and limitative theorems in metalogic. I shall turn to these matters after I have considered two sections of the chapter which have much lesser importance.

In section 2.1, Overman asserts that complete objectivity in science is an illusion: analyses always depend upon 'metaphysical assumptions'. This sound innocuous enough, though one might wonder what Overman means by 'metaphysical'. After all, it seems trivially correct to claim that analyses always depend upon assumptions—there can be no categorical reasoning without premises—and yet there is no obvious reason why the assumptions with which one begins in a given domain should be called 'metaphysical'. Moreover, it is also worth noting that, just because a given piece of analysis begins from certain assumptions, it does not follow that there cannot be good reasons for rejecting or accepting those assumptions. Overman will want to claim that theism and naturalism are both metaphysical stances—and that, in the end, it is a matter of faith which one accepts. (See, especially, pp.166–7.) But this is to rush very quickly over some very difficult questions which deserve far more serious and sustained treatment than Overman affords.

In section 2.2, Overman argues that if the universe and life are the products of accidents, then our thought is the product of accidents, and then wonders whether thought can be trustworthy if it is accidental. This argument involves a disastrous equivocation on the word 'accident'. To say that thought is the product of 'accidents' is just to say that there is no external intelligence which is responsible for its occurrence in the universe. However, to wonder whether thought can be trustworthy if it is 'accidental' is to emphasise the need for reliable connections between thoughts and that which is thought about. There are various naturalistic stories which one might tell about the reliability of thoughts; none of these stories involve the activities of supernatural intelligences. Rather than belabour the point, we should just set this argument gently aside.

In section 2.3, Overman offers a curious pot–pourri of 'errors of reasoning'. While some of the errors which he notes really are errors—over–extrapolation from small samples, circular reasoning, confusion of sequence with cause, failure to consider alternative hypotheses, changing the subject, dividing by zero in mathematical reasoning, equivocation, logical error—there is no theoretical or logical unity to the cases which Overman discusses. Rather, this section is just a pretext to drag out some bad arguments for conclusions which Overman does not like, and then to say that they fall victim to the error which is currently under consideration. This section of the book is really quite disreputable: it is clearly intended to create the impression that 'naturalists' are prone to the errors of reasoning which Overman diagnoses while theistic arguments are typically

formulated in ways which avoid these errors. However, quite apart from the fact that most of the bad naturalistic arguments are Overman's own inventions, it is simply obvious that this is a ludicrous way to try to discredit naturalism. And, even worse, the diagnoses which Overman makes of alleged errors of reasoning are unsystematic and often seriously in error. (I shall discuss some examples in detail in a couple of chapters time.)

In section 2.4, Overman discusses 'logic's limitations'. He considers: (i) Hempel's paradox of confirmation; (ii) liar paradoxes; and (iii) Godel's incompleteness result. (Overman says that Godel showed that in any consistent deductive system there are valid statements which are not provable by the rules of the system. (28) Of course, this isn't right: for example, the propositional calculus is both complete (with respect to the usual class of models) and decidable. There has to be a further constraint on the expressive resources of the system: if, for example, a theory has the resources to express its own metatheory, then it will contain true statements which are not provable.) Again, this is a curious mix. On the one hand, one might well take the view that Hempel's 'paradox' shows that there is something wrong with the conception of 'confirmation' with which Hempel worked. And, on the other hand, one might well suppose that we can be pretty confident in our ability to identify situations in which liar paradoxes and incompleteness results are genuine worries. Moreover, there is really nothing here which should motivate new modesty about the powers of human reason. We knew already that there are all kinds of claims which we shall never be able to understand, and arguments whose validity we shall never be able to assess: there are limits to the complexity of computations which we can perform. These limits are far more pressing and urgent than the limits which are imposed by liar paradoxes and incompleteness results; the crucial point about the Godel result is that it holds in principle, after merely contingent limitations are abstracted away. However, if we are interested in practical limitations, then the Godel result has no particular significance.

In section 2.5, Overman suggests that Heisenberg's uncertainty principle demonstrates limitations to our knowledge about the physical world. He then goes on to claim that, despite the limits imposed by Godel's limitative logical theorems and Heisenberg's limitative physical principle—i.e. despite the imperfections in human reasoning and human observation—'the principles of mathematics and logic work extremely well in the physical world'. (29) There is much which one might say here. On most interpretations of quantum mechanics, the uncertainty principle is read in a purely ontological way: it imposes no limits at all on our knowledge, but rather places constraints on the state of the world. Even if the uncertainty principle is read in an epistemological way, it is important to see that it has few direct consequences for human observation: what it does is to place limits on our ability to obtain precise simultaneous measurements of appropriately related quantities in quantum mechanical systems. Moreover—and now we get to an important point—it is a familiar fact from classical physics that "noise" almost always imposes a practical constraint on our ability to obtain precise measurements of quantities. If we are interested in "imperfections" in human observation, then there are purely classical considerations which are of far greater significance than any quantum considerations. Again, though—as in the case of our discussion of Godel's results—it is important to

note that the limitations imposed by the quantum uncertainty relations will persist even after we abstract away from considerations of noise. Moreover—and now we get to an even more important point—it is hard to see what relevance any of these constraints has to the use of 'logical thought, observation, and mathematical analysis ... in reviewing evidence': there is no reason at all to think that we need to worry about noise and quantum uncertainty when we ask ourselves whether a given argument is valid or whether given evidence supports a particular claim. Cynical readers might well draw the conclusion that the intended role of sections 2.4 and 2.5 is merely to dazzle those unacquainted with these topics.

To conclude this discussion, let me return to a couple of examples from section 2.3. My favourite is the section called 'modification of question presented'. Overman tells us that:

> Logical thinking requires disciplined focus on the issue under consideration. One must examine evidence to be certain that it relates to the question presented and not to a slightly different question. (22)

He then takes Voltaire to task for ridiculing Leibniz' contention that this is the best of all possible worlds which God could have made. ("As if everything in this world were as good as it could be!") According to Overman, Leibniz did not say that everything in this world is as good as it could be, 'but only that this world is better than *some* other worlds which God could have created'. (My italics) Oops! Who is modifying the material presented here? Leibniz definitely does say that this world is better than *all* other worlds which God could have created; the weaker claim would hardly have provided Voltaire with any material for parody. Moreover, the claim which is made against Leibniz—"as if everything in this world is as good as it could be"—can be read simply as a denial of the claim which Leibniz actually makes, and not as a slide into denial of the much stronger claim that *each* thing in this world is as good as it could possibly be. Voltaire's point is that it seems quite obvious that there are many things in the world which could be improved without making anything else worse—think, for example, of a forest animal which, unbeknownst to anyone, dies horribly in a forest fire—and, as recent discussions of the problem of evil show, it is by no means easy to show that Voltaire is wrong on this point.

In the section on circular reasoning (17–20), Overman claims that the argument of the following passage—from Shapiro's *Origins: A Sceptic's Guide to the Creation of Life on Earth*—is circular:

> The primitive atmosphere must have contained reducing equivalence in some form to yield amino acids, since no biomolecules or their precursors are formed when a mixture of carbon dioxide, water and nitrogen is sparked.

Despite Overman's confident assurance that this reasoning is circular—'the conclusion is assumed in the premise'—it is hard to see why he says this. As far as I can tell, the argument here must be something like the following:

1. No biomolecules or their precursors are formed when a mixture of carbon dioxide, water and nitrogen is sparked.
2. Biomolecules or their precursors were formed when the primitive atmosphere was sparked.
3. (Hence) The primitive atmosphere did not consist just of a mixture of carbon dioxide, water and nitrogen.

This argument is not circular. To think otherwise is presumably to make the mistake of supposing that any deductively valid argument is circular. Of course, (the suppressed) premise 2 is not one which Overman will accept—but then his objection ought to be that the argument involves a false premise, not that it is circular. If Oparin's theory were right—and, in particular, if amino acids were formed when the primitive atmosphere sparked—then the above argument could be an unexceptionable scientific inference from true premises to a true conclusion. This point alone is surely sufficient to establish that the argument is not circular. (It is probably worth pointing out that it is a highly contentious question how best to characterise the fallacies of begging the question and arguing in a circle. There is no extant widely accepted answer to this question.)

One last example. In the section on 'inconsistencies within the context of terms', Overman considers the following argument (in his presentation, it contains an extra but redundant premise):

1. Order can occur spontaneously in a system far from equilibrium.
2. (Hence) Life—i.e. complex living matter—can occur spontaneously in a system far from equilibrium.

Overman claims that this argument is vitiated by a 'fallacy of equivocation': order and complexity differ in important ways. However, it seems to me that this is the wrong objection for Overman to make. What he ought to say is that, even though this argument is sound—and even though it is true that life *can* occur spontaneously in a system far from eqiulibrium—it is massively improbably that living matter appeared spontaneously on the early earth. That is, the problem here is not with the argument itself, but with the suggestion that it has any importance for the debate about origins. (Here, we return to our initial worries about the unclarity which besets Overman's formulation of the entire debate. Given his confusions about possibility, probability, mathematical probability, and the like, it is understandable why he is driven to assess this argument in the way that he does. But, nonetheless, his assessment of the argument is just mistaken; and the confusions which beset his discussion ought to be expunged.)

Although there is much which is wrong with this part of the book—there are many other examples where Overman gets at least some of the details wrong—it should also be said that a fair amount of what he says is unexceptionable: the things which he calls 'errors of reasoning' really are errors of reasoning, and they are errors to which all parties to debates about origins are liable. However, as I mentioned earlier, it is so unsystematic, and so lacking in theoretical foundation, that it is unlikely to be of use to anyone. (Critics of 'practical logic' textbooks might suggest that Overman is far from alone in failing to

provide useful guidance to good reasoning. However, I shall keep away from this contentious issue here.)

Let me end with one last example from this chapter. At p.18, Overman writes:

> Assumed contradictions are often hidden in premises in arguments. For example, the question, 'If a designer designed the universe, who designed the designer?', assumes the contradiction by asserting that the designer was designed. Such an assertion is an assumed contradiction hidden in the question. This is similar to asking the question: who or what made triangles circular?

So, at last, a proof from Overman that God does NOT exist! How so? Well, I once designed a series of questionnaires for a large corporation. This makes me a designer. Now consider the question, 'If a designer designed those questionaires, who designed the designer?' On Overman's own admission, this question involves a hidden contradiction: designers cannot be designed! And so, since we are designers, and since, if there is a God, then God designed us, we have it immediately that there is no God. QED.

I leave it to the reader to figure out what exactly is wrong with Overman's diagnosis in the quoted paragraph.

PART 6: Ethical Implications of Chance or Impersonal Beginning

In this brief section, Overman argues that there are serious ethical implications for any view which denies that the formation of the universe and the formation of life were not personally overseen. Much of the section is taken up with a discussion of Nietzsche: the point of this discussion seems to be that Nietzsche 'accepted an impersonal beginning' and also 'rejected the concepts of universal right and wrong'. However, Overman himself also claims that Nietzsche 'rejected any universal truth' and 'believed in an absurd world'—so even Overman ought to have wondered whether the allegedly bad consequences of Nietzsche's views flowed from his 'acceptance of an impersonal beginning' alone. Moreover, even if Nietzsche were a more orthodox thinker in other respects, it would still be worth asking what conclusions could be supported by the observation of a single case of the conjunction of the 'acceptance of impersonal beginning' and 'rejection of universal right and wrong'. And, since Nietzsche is so difficult to interpret, it is also worth asking whether Overman gets him right; whether, for example, it is true that Nietzsche rejects any entitlement to talk about what is right and wrong.

Apart from the remarks about Nietzsche, this section contains repeated assertion of the claim that 'if one holds the view of an impersonal beginning, one cannot really talk about what is right or wrong'. (177) This is an enormously contentious claim, and one which

many adherents of ‘an impersonal beginning’ would certainly reject. The only argument which Overman advances in defence of this claim is this: ‘if the universe were an accident, then there are no absolutes; and, if there are no absolutes, then morals do not exist’. Neither of these conditional claims is in the least bit obvious, not least because it is unclear what Overman supposes ‘absolutes’ to be. Given the brevity of this section of the book—just over two pages—and the vast literature which already exists on this topic, I shall not here try to pursue these matters further.

ENOUGH! Let me bring this review to a close. I think that I have vindicated my initial claim that Overman fails to make a case for his three central claims which is either clear, or compelling, or well–argued. Given the evidence, I can only conclude that the enthusiastic endorsements which are cited on the dust–jacket of this book are the result of a kind of blindness to the many flaws of the book which is induced by the energetic piling up of scientific facts and theories in which the author engages. It is a curious—or perhaps not so curious—fact that many opponents of naturalism are now promoting themselves as friends of science, and, in particular, as friends of molecular biology and cosmology. However, it is important to ask whether the arguments which people like Overman produce are supported by the science on which they allegedly draw. In Overman’s own case, I think that the answer is quite obviously negative.