Is the Evolutionary Process Deterministic or Indeterministic? An Argument for Agnosticism

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Presented at the Biennial Meeting of the Philosophy of Science Association
Vancouver, Canada, November 2000

Abstract: Recently, philosophers of biology have debated the status of the evolutionary process: is it deterministic or indeterministic? I argue that there is insufficient reason to favor one side of the debate over the other, and that a more philosophically defensible position argues neither for the determinacy nor for the indeterminacy of the evolutionary process. In other words, I maintain that the appropriate stand to take towards the question of the determinism of the evolutionary process is agnosticism. I then suggest that an examination of the phenomenon of developmental noise might yield a solution to the problem.

1. Introduction

The question of whether the universe is ultimately deterministic or indeterministic is a long-standing philosophical problem. Since the advent of quantum mechanics and the "Copenhagen interpretation", however, many scientists and philosophers feel that the issue has been settled in favor of indeterminism for phenomena at the micro-level. However, the question remains as to whether the macro-level is indeterministic as well.

In the philosophy of biology, a lively debate has ensued over the question of whether evolution, in particular, is a deterministic or an indeterministic phenomenon. In a number of recent essays, philosophers have taken opposing stands on this question. Alexander Rosenberg (1988, 1994) claims that an omniscient account of evolution would have no need for the concept of random drift – that all instances of random drift can be explained in terms of natural selection. Rosenberg uses this claim to argue that although evolutionary *theory* is statistical, the evolutionary *process* is a deterministic one. According to Rosenberg, evolutionary theory is statistical purely for instrumental reasons; random drift serves merely as a useful fiction. A similar claim for the determinism of the evolutionary process is made by Barbara Horan (1994). Contra Rosenberg, Roberta Millstein (1996) argues that any evolutionary theory, omniscient or otherwise, must take random drift into account – that random drift is not eliminable from evolutionary theory. Robert Brandon and Scott Carson (1996) [hereafter BC] further challenge Rosenberg's and Horan's claims; they maintain that a scientific realist should conclude that the evolutionary process is fundamentally indeterministic. Stuart Glennan (unpublished) argues that BC are correct in their claim that the evolutionary process is indeterministic, but incorrect in their claim that evolutionary models are realistic. Most recently, Horan and Rosenberg join with

Leslie Graves in an attempt to counter the arguments of BC (Graves, Horan, and Rosenberg 1999 [hereafter GHR]). In this essay, I will focus primarily on the disagreement between GHR and BC, although I will discuss other works where relevant.

What issues are at stake here? The most basic point of disagreement is over whether a scientific realist ought to believe that the evolutionary *process* is deterministic or indeterministic, with GHR taking the former view and BC taking the latter. However, that is not the only point of disagreement; there is also disagreement over the related question as to how to interpret the use of probabilities in evolutionary *theory*. According to GHR, the probabilities used in evolutionary theory are purely epistemic ones. According to BC, even if it turns out that evolution *is* a deterministic process, the probabilities used in the theory are not merely epistemic, but are explanatory of genuine phenomena.[1] Although these authors treat the two issues in concert, for the sake of simplicity I will consider only the former question of the determinism or indeterminism of the evolutionary process.

These points of disagreement, interesting and important in their own right, have implications for other issues in the philosophy of biology. If the probabilities used in evolutionary are purely epistemic, is biology largely an instrumental science, as Rosenberg (1988) argues? If the probabilities used in evolutionary theory are *not* purely epistemic, do they arise

1 There are really three positions being taken towards points of disagreement: 1) Evolution is deterministic, and therefore the probabilities used in the theory are purely epistemic (GHR); 2) Evolution is indeterministic, and therefore the probabilities used in the theory are fundamental (BC); 3) Whether evolution is deterministic or indeterministic, the probabilities used in the theory are fundamdental (BC). GHR (142, n. 3) seem to suggest that to hold 2) and 3) simultaneously is inconsistent, but it is not. The way out of the inconsistency is to claim that the probabalistic nature of the theory is due to the indeterminism of the process (if it is indeed indeterministic), and for other reasons *as well*.

from quantum mechanics or do they have an independent source? In other words, what is the status of evolutionary theory?

In order to begin to answer these questions, I review some of the arguments that have been given on both sides. BC's claims for the indeterminism of evolutionary biology rest primarily on two arguments: the "percolation" argument, and an argument from experiments on cloned organisms.[2] I examine each of these arguments, as well as GHR's responses to these arguments. I argue that neither side makes its case; furthermore, the debate in its present form has reached a philosophical impasse. Thus, at this point in time, a scientific realist should remain agnostic towards the question of the determinism or indeterminism of the evolutionary process.

I then discuss one possible source of indeterminism in evolution – the seemingly random phenomenon of developmental noise. The etiology of this phenomenon is currently unknown, but if the cause (or causes) turn out to be indeterministic at a micro-level, then evolution will be indeterministic as well. Thus, the empirical study of developmental noise is of considerable philosophical interest.

2. Percolation versus Asymptotic Determinism

As I mentioned above, the Copenhagen interpretation of quantum mechanics suggests that micro-level phenomena are fundamentally indeterministic. However, the question remains as to

[2] There is a third argument, in which BC argue that there are cases where random drift is "forced," i.e., has to occur. However, it is not clear why this should count against the definition of determinism which BC use (if only for the sake of argument): "…*mereological determinism holds of a system S if and only if the total micro-state description of S at t determines every micro-state and hence ever macro-property of S for every* $t + \Delta$" (329).

whether the quantum mechanical indeterminism of the micro-level can "percolate up" to the macro-level described by evolutionary biology. The percolation argument, endorsed by Sober (1984) and BC, concludes that the micro-level *can* percolate up to the level of evolutionary processes. As formulated by Sober, the percolation argument asserts:

> If *enough* elementary particles had behaved differently, the behavior of the macro-object (the organism, the population) that they compose would have also been different. And there is no deterministic guarantee that the ensemble of particles *must* have behaved the way it did. The most that the ensembles of particles we call organisms can do is exhibit an impressive degree of predictability. But, so long as they are made of particles that have an irreducible chance component in their behavior, they too must be indeterministic systems. If chance is real at the micro-level, it must be real at the macro-level as well (Sober 1984, 121; italics in original).[3]

The percolation argument is not denied outright by determinists. Rosenberg admits that the micro-level can "infect" (i.e., percolate up to) the macro-level (1994, 60). More strongly, Horan acknowledges that, "[m]utation is a likely exception to the claim that evolutionary forces are completely deterministic" (1994, 83, n. 1). Thus, the indeterminists concede that it is *possible* that micro-level indeterminacy can percolate up to the evolutionary macro-level.

However, Rosenberg denies the import of the percolation argument. Rosenberg maintains that: "[i]n general, the quantum probabilities are so small, and the asymptotic approach to

3 GHR (142, n. 3) imply that Sober is not committed to the indeterminism of the evolutionary process, but this quote suggests otherwise.

determinism of everything physical above the level of the chemical bond is so close, that the quantum mechanical probability could never explain the probabilistic character, if any, of either evolutionary phenomena or evolutionary theory" (Rosenberg 1994, 61; a similar argument appears in GHR). In other words, according to Rosenberg, it is *possible* that indeterminism could percolate up, but it is extraordinarily unlikely; by the time we reach the macro-level of evolutionary processes, we are essentially dealing with determinism. I will refer to this as the "asymptotic determinism" argument.

Thus, the indeterminists make the percolation argument; the determinists counter with the asymptotic determinism argument. Unfortunately, both of these arguments are solely "in principle" arguments. The percolation argument claims that in principle, micro-level indeterminism can percolate up to the macro-level, while the asymptotic determinism argument maintains that in principle, by the time we reach the macro-level we will have asymptotically approached determinism. In one sense, these arguments are not in conflict at all; it is possible to believe simultaneously that the micro-level *can* percolate up to the evolutionary macro-level, while claiming that this almost never happens (since we have asymptotically approached determinism). However, the proponents of each of these arguments do see a conflict, and this seems to have to do with the *frequency* of percolations. Those making the asymptotic determinism argument believe that percolation almost never occurs, whereas those making the percolation argument believe that percolation occurs, if not frequently, then more frequently than "almost never."

It should be evident that this disagreement over the frequency of percolations is extremely vague. Furthermore, the discussion up until this point is completely abstract; it is left unstated as to which sorts of processes might exhibit percolation and which sorts of processes might exhibit asymptotic determinism. Because the disagreement is both vague and abstract, it is, in its current terms, virtually impossible to settle. We are thus left at a philosophical impasse.

BC attempt to break down the impasse by presenting a concrete example where a quantum mutation would have a population-level effect. This is an admirable attempt to overcome the abstractness of the debate. In BC's example, there is a population consisting of two haploid genotypes, *A* and *a*. The population has an unstable equilibrium point when the population is composed of equal numbers of *A*'s and *a*'s; one mutation from *a* to *A* would cause the population to consist entirely of *A*'s, whereas a mutation from *A* to *a* would cause the population to consist entirely of *a*'s. Thus, if some mutations are quantum events, it is clear that this would be a case where quantum effects would percolate up to the level of evolutionary processes.

Unfortunately, this is only one example, and it leaves one wondering if similar arguments could be made with other kinds of examples. In addition, as BC admit, the example is unrealistic. In fact, the very thing that makes the example persuasive – the unstable equilibrium point that leads to the dramatic shift in the composition of the population – is what makes it unrealistic. As BC note, we would not expect to find such populations in nature because random drift would likely push the population from the point where the two genotypes were of equal frequency. Thus, because BC's example is only one example, and because it is an unrealistic example, it does not go very far in settling the question of how often percolations occur. It certainly doesn't seem to bump us very far from the "almost never" category and into the "more frequently" category (again, the terms of the debate are vague, so it is difficult, if not impossible, to know when it has been settled).[4]

BC raise the level of the debate by showing how the micro-level *can* percolate up to the macro-level, but without further evidence that this percolation occurs across different types of evolutionary scenarios, the most we can say is that evolution may be indeterministic to some small extent. The percolation argument, as extended by BC, fails to answer the larger question of whether evolutionary biology is generally indeterministic. Thus, we are still at a philosophical impasse concerning the question of the determinism or indeterminism of evolution.

It should be noted that BC do not intend the percolation argument to be their primary argument for the indeterminism of the evolutionary process. They are more concerned with *autonomous* indeterminism than they are with indeterminism *simpliciter*: "For ET to be autonomously indeterministic it must be indeterministic in a way that does not depend on QM" (BC, 320). It certainly would be an interesting result if evolution turned out to be indeterministic, completely independently of quantum theory. What I am somewhat skeptical of is whether such arguments can be made without trading one philosophical intuition for another. Furthermore, I think the question of whether evolution is non-autonomously indeterministic is interesting and important in its own right. For one thing, an indeterministic picture of the history of life on this planet looks radically different from a determinstic picture. In other words, in one sense, I take BC's percolation argument to be more important than they do; I think that the *kind* of argument that they give has the potential to settle the debate, even if I don't think that this particular argument succeeds.

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4 GHR offer additional criticism of this argument. Glennan (unpublished), on the other hand, argues that scenarios that are more realistic can be given, e.g., a point mutation introduces a novel allele, which confers a significant, frequency-independent, selective advantage to its bearer.

3. Argument from Experiments on Cloned Organisms

Another point of disagreement between determinists and indeterminists concerns the interpretation of experiments on cloned organisms. As BC note, experimental setups that use cloned organisms in controlled environmental settings are quite common in biology. The results are equally commonplace: organisms that are (purportedly) genetically identical and are raised in (purportedly) identical environments will differ physically, so that some will be more reproductively successful than others. For example, cloned plants grown in identical environments may have different heights and weights, or different numbers of flowers, leading to differential reproductive success. I take it that these sorts of results are not in dispute. What *is* in dispute is how to interpret the results. Do such results provide evidence for the indeterminism of the evolutionary process, or not?

BC claim that such results *are* evidence for the indeterminism of evolution. In other words, they claim that the reason that genetically identical plants raised in identical environments differ physically is that evolution is indeterministic, and that indeterminism is the best explanation for this phenomenon.

However, there is an obvious response that the determinist can make to BC. The determinist can maintain that either the *organisms* were not truly identical (a mistake occurred during the cloning process),[5] or the *environments* were not truly identical (a mistake was made in

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5 Lewontin states: “Given the known rates of mutation, the likelihood that two actually existing genomes are identical over their entireties is extremely low, even for those of identical twins or other clonally reproduced organisms” (1992, 139).

constructing the environmental settings). Note that the determinist can claim either that there is one variable that appears in some preparations, but not others (e.g., an undetected difference in soil among the preparations), or that there are *numerous* hidden variables which differ across the different preparations.

In response to such claims, BC suggest that the experimental *procedure* of using multiple copies of genetically identical organisms *itself* presupposes indeterminism. They state:

> …this experimental procedure assumes an indeterministic response. That is, it assumes that different copies of the same genotype in the same treatment will give different results; otherwise the experiment could be made much smaller with single copies of each genotype for each treatment (BC, 330).

In other words, according to BC the reason that biologists need to use multiple copies of each genotype is that evolution is indeterministic; if evolution were deterministic, multiple copies would not be needed.

However, there is a very natural *deterministic* interpretation of the practice of using multiple copies of a genotype: that of the randomized experiment. As R. A. Fisher notes: "…the uncontrolled causes which may influence the result are always strictly innumerable" (Fisher 1953, 55); to overcome this problem, we use the method of a randomized experiment. In a randomized experiment, every characteristic possessed by members of the population is, on average, matched in the different treatments (Giere 1991, 241). The determinist can argue that we use multiple preparations for each treatment so that if there *are* any hidden differences between preparations, those differences on average occur equally in the all the treatments. This prevents us from mistakenly attributing a particular effect to the difference in question when it is really due to a hidden difference between the treatments. If we used only *one* replicate for each treatment, a hidden variable (rather than the difference under study) might be the cause of the given result. By using *multiple* replicates for each treatment, the hidden variables are randomized across treatments. Thus, the determinist can argue that we use this kind of experimental design because we cannot control for every possible factor. GHR come close to making this point (that the experimental setup can be interpreted as a randomized experiment) when they suggest that "it is just because there are no truly identical clones, and no qualitatively identical experimental conditions in biological (even in molecular biological) experiments that biologists produce multiple clones and subject them to experiments" (152).

So, both the determinist and the indeterminist have a way to account for the experimental *results* and a way to interpret the experimental *procedure*. Yet BC claim that the results favor an indeterministic interpretation. The question is, can they make this argument without simply begging the question against the determinist? In other words, do they offer any additional reasons to favor an indeterministic interpretation over a deterministic one?

In fact, BC propose two criteria for determining when it is appropriate to posit a theoretical entity (in this context, a hidden deterministic variable). The first criterion is that "the positing of the entity aids the development of theory"; the second criterion is that "the available empirical evidence supports the posit" (BC, 331). According to BC, the indeterminst satisfies *both* of these criteria, while the determinist satifies *neither* of them:

> It is beyond doubt that the positing of genuinely probabilistic propensities governing the evolutionary fates of individual organisms has been an integral part of the impressive

> development of evolutionary population genetic theories in this century...and all the available empirical evidence supports this idea. In contrast, the positing of deterministic hidden variables serves no theoretical purpose at all, and, insofar as it is allowed to be addressed by data is contradicted by empirical data (BC, 331).

Consider the first of BC's two criteria. It is important to realize that their claim that the positing of deterministic hidden variables serves no theoretical purpose is not a view that is shared by all evolutionary biologists. Most notably, biologist Sewall Wright takes an opposing stance. He states:

> A certain danger for science must be squarely face[d]. The acceptance of statistical description as ultimate may lead sometimes to premature abandonment of analysis in cases in which analysis would be pushed farther by one who believes firmly that there is a deterministic mechanism to be found (Wright 1964, 288).

In other words, there is a danger to the indeterminist's outlook. The indeterminist might "give up" and stop looking for underlying causal factors, while a determinist will be motivated to keep looking. Thus, there is the possibility that the indeterminist will overlook causal factors that the determinist might find. Here it should be noted that Wright was no stranger to the role of statistical description in evolutionary theory – Wright was a strong proponent for the role of random drift, a thoroughly statistical concept. And the fact that Wright, one of the primary architects of population genetics, was *himself* sympathetic to a deterministic outlook is evidence against BC's suggestion that it is an *indeterministic* outlook that has led to the impressive development of population genetics.[6]

Here the indeterminist might concede that in field studies of evolution, where we cannot rigorously control conditions, there is an advantage to the determinist's hypothesis, while arguing that in laboratory studies (particularly controlled studies of the kind that BC describe) there is little or no such advantage. But to grant this point is to grant that the positing of hidden deterministic variables has been useful in evolutionary biology, for certainly the theory should account for natural as well as experimental contexts.

Let me clarify that my point here is not to argue that the determinist's hypothesis is theoretically superior to the indeterminist's. The indeterminist could counter the arguments of Wright by pointing out that the determinist may end up wasting time fruitlessly looking for possible hidden variables. As GHR note, their position is similar to that of the adaptationist, who assumes "that there are selective causes for features even when these cases are not obvious or not apparent to us" (153). This is the adaptationist strategy that Gould and Lewontin characterize in their critique of the adaptationist programme: "…if one adaptive argument fails, try another," or, more weakly, "if one adaptive argument fails, assume that another must exist" (1979, 586). As Gould and Lewontin note, such a strategy is unfalsifiable. One can continue to tell plausible stories (GHR might, for example, account for phenotypic differences by appealing to different molecules in the soil), but when does the telling of plausible stories end? At some point (the indeterminist counters) isn't the reasonable thing to do to *switch* strategies and assume that the apparent indeterminism is *genuine* indeterminism?

What I am arguing is that neither view is superior to the other; both the determinist's

6 GHR make similar points (153).

hypothesis and the indeterminist's hypothesis have their advantages and disadvantages. Furthermore, I think BC clearly go too far in claiming that the positing of deterministic hidden variables in evolutionary theory serves no theoretical purpose whatever. There *is* a purpose in the strategy of continuing to look for causal factors, just as there is reason not to continue clinging to a strategy in the hope that the strategy will eventually pan out. Which strategy is better? As Beatty (1987) argues, how we distribute the resources of the evolutionary community has to do with the questions we pose for ourselves. With regard to the question concerning the relative importance of selection versus drift, Beatty suggests that if this is a question we really want to answer, "then we really must give serious thought to distributing the resources of the evolutionary community between the pursuit of selection and drift hypotheses" (1987, 72). Analogously, if we want to know whether evolution is deterministic or indeterministic, then the community should pursue both strategies, although individual biologists might choose one or the other.

Let us turn now to BC's second claim, the claim that the determinist's hypothesis is contradicted by the empirical data insofar as it is allowed to be addressed by data. I am not entirely certain what BC mean by this claim; however, they seem to be suggesting the determinist's hypotheses are merely *ad hoc*, and that without such *ad hoc* hypotheses, the data do not fit. Indeed, BC say that the determinist's positing of hidden variables "is purely gratuitous" and is done "for no reason other to save the deterministic character of the theory" (333). However, as we saw above, there are additional theoretical reasons one might be a determinist. BC further point out that the deterministic interpretation is not the only way of accounting for the phenotypic differences between the clones – that one can be an indeterminist. They seem to be asking "Why be a determinist?" in an attempt to shift the burden of proof to the determinist.

In response, GHR explicitly try to shift it back: "…the default position here is determinism, and the burden of proof is on the shoulders of those who hold that the variation among cloned grasses results from indeterminism" (152). According to GHR, determinism is the default position because:

> …all of chemistry, organic chemistry, molecular biology, and cellular physiology that one would invoke to explain the actual character of each blade of grass is deterministic…Even quantum mechanics recognizes that at the level of the macromolecule, nature asymptotically approaches determinism (152).

However, this restatement of the asymptotic determinism argument is no more persuasive against the percolation argument than it was in the case of evolution (discussed above). It simply trades one philosophical intuition for another.

The essence of the question that BC raise is whether the data distinguish between the two hypotheses, and it seems clear that they do not. The indeterminist accounts for the phenotypic differences in the cloned organisms by claiming that the process is indeterministic. The determinist accounts for the data by claiming that there are hidden variables that account for the differences. Thus, the data do not settle the issue between the determinist and the indeterminist. Each side tries to shift the burden of proof, but what is needed is for someone to take up the burden. In the next section, I suggest one avenue for doing so.

4. Percolation in Cloned Experiments?

The percolation and asymptotic determinism arguments leave us at a philosophical impasse. Furthermore, both determinists and indeterminists can interpret the experiments on cloned organisms and can claim to have satisfied the criteria of theoretical fruitfulness and experimental confirmation. Given these considerations, agnosticism is the most defensible philosophical position. However, I would like to point in a possible direction of solution. Let us go back to the biological coffeepot, and take a closer look at the experiments on cloned organisms. Perhaps something new will percolate up.

In discussing the experiments on cloned organisms, GHR state that an "important source of deterministic differences [between the cloned organisms] is to be found in 'developmental noise'" (151). But what reasons do we have for thinking that developmental noise is a deterministic phenomenon? It seems to me that this is the question that we should be asking.[7]

It is generally agreed upon that the development of an organism is a function of a combination of genetics and environment. Unsurprisingly, then, it is genetics and environment that form the primary basis for the disagreement between GHR and BC over the interpretation of the experiments on cloned organisms; GHR think that there is reason to posit hidden genetic and environmental variables, whereas BC deny that there is any such reason. However, genetics and environment do not uniquely determine an organism; there is additional variation that neither genetics nor environment can account for, and this additional variation is what biologists refer to as "developmental noise" (also known as "developmental instability"). For example, in

[7] I don't mean to imply that an exploration of developmental noise is the *only* approach that can be taken to further address the question, but simply that it is one that ought to be explored. For example, Glennan (unpublished) provides an argument for the indeterminism of environmental evolutionary influences.

Drosophila, there are often different numbers of bristles on the left and right sides of the fly, although both sides are arguably genetically identical and have the same environmental history (Lewontin 1992, 141). The difference in bristle number (an example of what is referred to as "fluctuating asymmetry") is said to be the result of developmental noise. It is important to note that for some traits the contribution of developmental noise can be significant; it "may account for most of the phenotypic variance in inbred lines (Wright 1952; cf. Thoday 1956), in the wing pattern traits of butterflies (Mason et al. 1967) and *Drosophila* bristle characters (Latter 1964; Reeve and Robertson 1953)" (Soulé 1982, 755-6).

Thus, it is possible that developmental noise accounts for some of the variation in the cloned organisms that BC discuss. But what is the mechanism underlying developmental noise? Unfortunately, this area has yet to be fully explored, contrary to GHR's claim that the phenomenon is "well-known." In the *Proceedings* from the "International Conference on Developmental Instability: Its Origins and Evolutionary Implications", editor Therese Markow states that the process of developmental noise is "unknown," and that:

> Discussion at the conference revealed a number of issues requiring clarification in future studies. First priority should be given to the identification of the mechanisms underlying developmental instability (1994, 2).

Similarly, Bendikt Hallgrímsson claims that despite the interest in fluctuating asymmetry, "remarkably little effort has been directed toward understanding the etiology of developmental noise" (1993, 422). This lack of certainty about the causes of developmental noise is in itself reason for agnostism concerning the question of the indeterminism of the evolutionary process; if we don't know what developmental noise is, it would clearly be precipitous to make any pronouncements concerning its determinism or indeterminism.

Nonetheless, biologists do have some hypotheses regarding the origin of developmental noise. For example, a popular textbook states that phenotypic differences that occur in genotypically identical organisms raised in identical environments are,

> …partly dependent on the processs of cell division that turns the zygote into a multicellular organism. Cell division, in turn, is senstive to molecular events within the cell, and these may have a relatively large random component (Griffiths et al. 1996).

One researcher suggests that developmental noise is:

> …a suite of processes that tend to disrupt precise development, such as a) small random differences in rates of cell division, cell growth and cell shape change, b) effects of thermal noise on enzymatic processes, c) small random differences in rates of physiological processes among cells (Palmer 1994, 337).

Another researcher offers three possible mechanisms for developmental noise:

> First, it is possible that developmental noise derives not only from stochasticity in the cellular activity of growth and morphogenesis, but also from all cellular activity relevant to the form in question…Second, developmental noise may derive from "thermal" noise in the movements of molecules…Soulé (1982) has also suggested that developmental noise

> derives from random movement at a molecular level (Hallgrímsson 1993, 438-9).

Here is yet another suggestion:

> At the biochemical level, one can think of noise in probabilistic terms. Suppose some key biochemical has a particular atomic configuration 98% of the time, but 2% of the time, the atoms in the molecule flip into a different configuration (which may be energetically less stable, for example). A key developmental event happening during 2% of the time, could produced a different outcome than when the more stable molecular state mediates the stop in question (Fausto-Sterling 1997, 249).

None of these hypotheses has received extensive experimental confirmation.

Are these proposed mechanisms deterministic or indeterministic? In answering this question, we must resist running into the same philosophical impasse we ran into at the higher, organismic level. Take random differences in the rates of cell growth as an example. We should not simply assume that micro-level indeterminism does – or does not – percolate up to the level of cellular activity. To do either of these things at the cellular level would be to trade one philosophical intuition for the other, or to trade one set of theoretical benefits for the other, just as disputants do at the organismic level.

Instead, what we need to do, once one of the proposed mechanisms is experimentally confirmed, is to evaluate the mechanism itself: is it an indeterministic process according to our best theories of quantum mechanics? Since we have assumed the position of a scientific realist, this is an appropriate approach to take. For example, if developmental noise were due to a "change in the configuration of a key biochemical," this would imply that developmental noise is indeterministic; it is the same mechanism by which a point mutation is produced, a mechanism generally acknowledged to be indeterministic. Once we have done that, we will then be in a position to see whether quantum mechanics does indeed percolate up to the evolutionary level.

The question of the indeterminism of the evolutionary process thus becomes an empirical question, an empirical question that we are currently not in a position to answer because we do not yet know the mechanism that underlies developmental noise. The scientific realist should therefore remain agnostic on the question, pending further scientific study.

5. Conclusion

The issues on which BC and GHR differ (the percolation argument and the argument from experiments on cloned organisms) leave us at a philosophical impasse. Of course, it is possible that a philosophical argument will be proposed that can settle the question of the determinism or indeterminism of evolution. However, given the longstanding nature of this problem, and the ease of simply trading philosophical intuitions, such a solution is not likely to be forthcoming. A more defensible approach is to remain agnostic for now, and to see if the question can be decided empirically.

However, even if one is agnostic on the question of the determinism or the indeterminism of evolution, one need not give up all hope of providing an account of the probabilistic nature of evolution. Furthermore, one need not rely on quantum mechanics to provide such an account. That is to say, without settling the debate between the determinist and the indeterminist, we can still ask whether evolutionary theory is inherently and unavoidably probabilistic, or whether it is probabilistic solely for instrumental reasons – because we find probabilities more useful and tractable in evolutionary contexts. If the evolutionary process is *indeterministic*, then the answer to this question is clear; evolutionary theory is inherently and unavoidably probabilistic. On the other hand, if the evolutionary process is *deterministic*, evolutionary theory may still be inherently probabilistic (Sober 1984, BC, Millstein 1997, Glennan unpublished). These issues bear further exploration.

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