HOW NOT TO ARGUE FOR THE INDETERMINISM OF EVOLUTION: A LOOK AT TWO RECENT ATTEMPTS TO SETTLE THE ISSUE

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1. Introduction

Recently, philosophers of biology have engaged in a lively debate over the status of the evolutionary process: is evolution deterministic or indeterministic? The thread of the current discussion seems to have begun in 1984 with Elliott Sober 1984 arguing that macro-level evolutionary phenomena were not safe from micro-level indeterminism. In response, Alex Rosenberg (1988, 1994) and Barbara Horan (1994) argued against Sober and for the determinism of evolution. However, it is with the publishing of Robert Brandon and Scott Carson's full-fledged defense of evolutionary indeterminism that the debate seems to have kicked into high gear (Brandon and Carson 1996; hereafter BC). BC's paper inspired Leslie Graves, Horan, and Rosenberg to join forces and write a response defending "asymptotic" determinism (Graves, Horan, and Rosenberg 1999; hereafter GHR). Most recently, evolutionary indeterminists have struck back with a pair of papers by David Stamos (2001) and Bruce Glymour (2001). These two essays have persuaded one of the determinists, Rosenberg, to concede that the evolutionary process is "indeterministic in at least some of its most important fundamental processes" (2001, 536).[1] So, is the debate over? Have the indeterminists won? Not quite yet, I will argue.

Before beginning, I should come clean with my own views on this debate. I am neither an evolutionary determinist nor an evolutionary indeterminist, having argued previously that the appropriate position to take on this issue is one of agnosticism (Millstein 2000). Moreover, unlike Rosenberg, I have not been persuaded by Stamos and Glymour to change my position. However, agnosticism will not be my specific argument in this essay, although I will address

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[1] Contrast this quote with Rosenberg's earlier remarks: "…it appears to be reasonable to conclude that, like mechanical phenomena, evolutionary phenomena are after all deterministic, or at least as deterministic as underlying quantum indeterminism will allow" (1994, 82). However, he has not conceded the related claim, made by BC, that

some of the same issues that I have before. Instead, I want to illustrate some of the difficulties in finding a resolution to this debate, and to show why Stamos and Glymour do not settle the issue. Then briefly, at the end of my essay, I will suggest how the debate might be resolved, if it is to be resolved.

Although ultimately my focus is on the arguments by Stamos and Glymour, I will spend some time discussing BC and GHR. I think the case can be made that it is primarily these two papers that have set the terms of the current debate. In order to characterize this debate, we will need some rough definitions. Let us take *determinism* to be the view that given the *complete* state of the world at one point in time, the state of the world at every future point in time is uniquely determined. We will take, *indeterminism*, on the other hand, to be the view that, given the complete state of the world at one point in time, the state of the world at every future point in time is *not* uniquely determined; for a given point of time in the future, more than one state is possible.[2] Now let us take a *scientific realist* to be someone (a philosopher, scientist, or layperson) who accepts the probable truth of theories that are well-supported by evidence, and a *scientific realist about quantum mechanics* (QM) to be someone who accepts the claim that QM implies indeterminism at the micro-level.

Given these definitions, the question that emerges from BC and GHR is: if you are a scientific realist, and a scientific realist about QM in particular, should you believe that the evolutionary process is indeterministic?[3] The "if" clause here is crucial – the entire debate

the probabilities of evolutionary *theory* are fundamental propensities, a claim that I discuss elsewhere (Millstein, forthcoming).

[2] I am not particularly wedded to the wording here; the key point is that the definitions of determinism and indeterminism are ontological rather than epistemological.

[3] Parts of BC's arguments do not rely explicitly on a realist stance towards QM, as will be discussed further below; however, Brandon states these arguments do "not commit us the truth, or even the sensibility, of the following counterfactual: If quantum mechanics were deterministic then the evolutionary process would still be indeterministic. I for one have no idea how to evaluate the truth of that statement" (ms.).

assumes scientific realism and the reality of quantum probabilities. As BC emphasize, the arguments would not be convincing to someone who was a scientific instrumentalist in general, for reasons outside of this debate. BC's answer to the above question is "yes"; GHR answer "no".

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.[4] 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.

Although these authors treat the two issues in concert, in this essay I will consider only the former question of the determinism or indeterminism of the evolutionary process in order to give due weight to the answer. The question of the proper interpretation of probability for evolutionary theory is interesting and important,[5] but it is partially dependent on the determinism/indeterminism question. Specifically, if the evolutionary process is deterministic, then some interpretations of probability are ruled out, most notably fundamental propensity (although there are propensity interpretations that are consistent with determinism). On the other hand, if the evolutionary process is indeterministic, almost any interpretation of probability would be appropriate, but fundamental propensity will play at least some role. So, answering the determinism/indeterminism question is a necessary step in answering the interpretation-of-probability question.

Furthermore, the determinism/indeterminism question is interesting and important in its own right, and not only because the answer tells us something about the nature of our reality.

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[4] Rosenberg has since recanted this view (2001).

For example, Gould has argued that if we could "replay the tape of life" that humans might not evolve a second time (Gould 1989). But what does this claim mean? Does it mean only that, given different initial conditions, humans might not have evolved, or does it mean something stronger – that even given the same conditions, humans might not have evolved? (Gould seems to have intended the former, weaker claim). If the evolutionary process is indeterministic, the stronger claim is warranted; if deterministic, only, perhaps, the weaker. Another example is the perennial issue of human "free will". To the extent that indeterminism is relevant to this issue (not unproblematically, as has been amply discussed elsewhere), indeterminism of the evolutionary process is going to be more relevant than indeterminism of quantum phenomena alone.

So, given the terms of the debate as I have described them above, there are two basic strategies by which one might seek to argue for indeterminism, as Stamos and Glymour do. The first is to focus on the implications of QM for evolution, and to argue that the indeterministic micro-level percolates up to the macro-level. This is the strategy that Stamos uses. The second strategy largely ignores the implications of QM, and argues for the indeterminism of evolution on independent (autonomous) grounds. This is primarily (although not solely) the strategy that Glymour uses. I will examine each of these strategies in turn.

2. The Percolation Strategy

Again, both sides of the debate assume that micro-level phenomena are fundamentally indeterministic. However, the question remains as to whether the quantum mechanical

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[5] See Millstein (forthcoming) for discussion of this issue.

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

> 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 (1984, 121; italics in original).

It is important to recognize that the percolation argument is not denied outright by determinists. GHR admit "it is not in principle impossible that quantum indeterminacy might occasionally alter a biological outcome" (145). Thus, the determinists concede that it is *possible* that micro-level indeterminacy can percolate up to the evolutionary macro-level. However, they argue that it is extraordinarily unlikely; by the time we reach the macro-level of evolutionary processes, the world is essentially deterministic. I will refer to this as the "asymptotic determinism" argument.

Rosenberg has recently offered a useful clarification of the asymptotic determinism argument:

For example, Newtonian mechanics suitably restricted to the description of the behavior

of macroscopic objects over finite periods of time only, will be instantiated by the

histories of many possible worlds–including the actual world–in which (indeterministic)

quantum mechanics obtains, but in which the probabilities of the violation of Newton's

laws by macroscopic objects are so low that there is not a single *actual* violation in the

amount of time taken up by the whole history of the possible world in question. That the

actual world is such a world is what GHR had in mind in claiming that quantum

phenomena *asymptotically* approach determinism in almost all biologically significant

processes (2001, 537-8; italics in original).

Stuart Glennan offers an everyday example of asymptotic determinism in his observation that in

spite of micro-level indeterminism, "pushing the button on a Coke machine deterministically

produces a Coke" (1997, 498). Glennan points out that even when pushing the button *doesn't*

produce a Coke, this is due to a failure of the mechanism of the Coke machine and is not an

indeterministic phenomenon (1997, 515, n. 3).

    In one sense, these two opposing arguments–indeterministic percolation versus asymptotic

determinism–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, there *is* a certain amount of conflict between the two arguments, and this seems

to have to do with the *frequency* of percolations. The asymptotic determinism argument implies

that percolation almost never occurs, whereas the percolation argument implies that percolation

occurs, if not frequently, then somewhat more frequently than "almost never." Or perhaps "almost never" is stating the asymptotic determinism position too strongly – GHR are willing to concede "that quantum indeterminism might *occasionally* alter a biological outcome" (145; emphasis added). On the other hand, when BC argue for the indeterminism of evolution, they are not talking about a phenomenon that occurs rarely, or even occasionally, but rather, a phenomenon as common as two cloned plants that are raised in identical conditions, yet have different numbers of flowers (329).

Thus, the terms of the debate (via the percolation strategy), as set by BC on the one hand, and Graves, Horan and Rosenberg on the other, are fairly vague. It is unclear how many "percolations" would have to occur before we would consider our world to be fundamentally indeterministic. I think that the debate has been cast in such vague terms because the logically possible extreme positions–complete determinism or complete indeterminism–are implausible. On the other hand, what is interesting to us is the degree to which evolution is indeterministic. And there *is* a substantive difference between an evolutionary process that is rarely indeterministic and an evolutionary process that is frequently or commonly indeterministic. However, casting the debate in these vague terms means that it will be difficult, although perhaps not impossible, to settle.

What *would* make the debate impossible to settle is if were kept it in these purely abstract terms – if one simply asked how frequently micro-level indeterminism percolates up to the macro-level. To answer "frequently" or "very rarely" is simply to trade one philosophical intuition for another. The observable world contains numerous phenomena that *appear* deterministic and numerous phenomena that *appear* indeterministic. Without the demonstration of a concrete example, there is no good reason to prefer one intuition over the other. (I will refer

to this abstract trading of intuitions over percolation as the "percolation fallacy"). Perhaps in recognition of this point, BC present a concrete example where a quantum mutation would have a population-level effect – a percolation to the macro-level.

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 does not 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 to know when it has been settled).

It should be noted that BC do not intend the percolation argument to be their primary argument for the indeterminism of the evolutionary process (and this will be discussed further below). 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. However, I think the question of whether evolution is non-autonomously indeterministic is interesting and important in its own right. 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.

GHR offer additional criticisms of BC's example, but rather than considering those criticisms directly, I will address Stamos's response to them. Stamos (2001) identifies five reasons that GHR give for thinking that BC's example is highly improbable: 1) BC need to defend their assertion that the processes creating point mutations are indeterministic, 2) the number of bases of relatively small genes (let alone large genes) is too large for spontaneous mutations to have anything but a negligible effect, 3) the genetic code is redundant, 4) many amino acid substitutions in proteins result in little if any difference in function, and 5) a point mutation could be canceled out by additional random events.

Of these five reasons, only four are really directed at the claim that BC's example is improbable (namely, reasons 2, 3, 4, and 5 above). Stamos provides counterarguments to these four reasons. These counterarguments are quite persuasive, although they are not conclusive, because even if Stamos were to succeed in defending BC's example, the debate will still not be settled, for the reasons that I just mentioned–it is only one example, and it is an unrealistic example.[6] Thus, I will not give any further discussion of these four counterarguments.

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[6] In addition, he does not address GHR's general reasons for endorsing asymptotic determinism.

In any case, the primary focus of Stamos's arguments is not against GHR's claim that BC's example is improbable. Instead, Stamos focuses in particular on GHR's suggestion, made in a footnote, that point mutations may not be indeterministic (reason 1 above). GHR state:

> Of course, saying that point mutations are caused by quantum activity is not the same thing as saying that point mutations instantiate quantum indeterminacy. Consider the shape and complexity of an [adenine] molecule. The changes required to mutate this molecule into a guanine molecule will be quite considerable, clearly involving a substantial aggregation of micro-processes. Because the outcome of micro-events aggregating to this extent is asymptotically deterministic at even the level of macro-physical processes, BC's assertion that the processes creating point mutation are indeterministic is an assumption very much in need of defense (144, n. 7).

So Stamos defends the assumption–and defends it well. After numerous papers in which philosophers have glossed over the indeterminacy of point mutations, Stamos gives us a detailed, biologically sophisticated explanation of why point mutations are indeterministic. I cannot do justice to it here, but in short, he examines several candidate mechanisms for point mutations (discussing controversies over different proposed mechanisms), and illustrates how the different mechanisms "hook up" to quantum mechanical processes (Stamos 2001). It is a compelling and convincing argument, and it is a genuine contribution to the philosophical literature, filling in a yawning gap. However, it does not settle the question of whether evolution is largely deterministic or indeterministic. Or so I will argue.

As I noted already, most of Stamos's essay is a response to GHR's footnote. Now it is true that some philosophers (who I will decline to name) are notorious for sneaking their most important points into the footnotes, but I don't believe that is what GHR have done. It is clear from the rest of their essay that they largely grant the indeterminism of point mutations (and the earlier essays by Horan and Rosenberg support this interpretation as well). That is not what's at stake between the two sides. What is at stake is whether (and to what extent) point mutations produce an effect at the level of evolutionary processes. BC's example addresses this issue directly, by describing a scenario where a point mutation has an immediate evolutionary effect. But for the most part, Stamos does not discuss the implications of point mutation for evolution; instead, he focuses on demonstrating that point mutations are indeterministic. This fills in an essential point in BC's percolation argument, but again, BC's percolation argument does not settle the debate (nor was it intended to, as discussed above).

My response to Stamos may seem odd here. After all, no one doubts that mutation plays an important role in evolution. So, if point mutations are indeterministic, then evolution must be indeterministic also, right? This seems to be Stamos's implicit argument. Yet I do not think the implicit argument succeeds.

To see why not, we must consider the role that mutation, in general, plays in the evolutionary process. Mutation can be viewed from two different perspectives in evolution. First, it can be seen as an evolutionary process, alongside other evolutionary processes such as natural selection, random drift, and migration. That is, when there is a mutation in a population, there is a change in the genetic composition of the population–in other words, there is evolution, at least according to some definitions of it. However, when you consider mutation in this light, its effects are very weak as compared to natural selection, random drift, and migration. So, any

indeterministic contribution would be small. Viewed from the second perspective, mutation is a source of variation that the other processes (natural selection, random drift, and migration) act on and amplify. Here one sees that the role of mutation in evolution is a key one, although mutation is not the only source of phenotypic variation; recombination is also a source of a considerable amount of phenotypic variation in a population. More importantly, not all variations due to mutation result in a long-term evolutionary effect; many are immediately lost to selection or drift.

Furthermore, we must consider that not all mutations are point mutations (i.e., base substitutions). Other kinds of mutations include deletions, insertions, inversions, translocations, and gene conversions (Hartl and Clark 1989, 97-103). This is not to say that Stamos makes the mistake of saying that all mutations are point mutations, but just to clarify that the previous points made about mutation concern phenomena of which point mutations are just a subset. We would need an additional paper (or papers!) akin to Stamos's, to convince us that all of these forms of mutation are indeterministic.

When you consider all these points together: that mutation as an evolutionary process has weak effects, that mutation as a source of variation is often not perpetuated, and that not all mutations are point mutations, the right conclusion to be drawn seems to be that point mutations do contribute a certain amount of indeterminism to the evolutionary process, but they do not make it indeterministic to any great degree. Or, at least, it is not obvious that they do; more argument would be needed to demonstrate this claim. Now of course, all of this is quite vague; but once again, the terms of the debate, as set by BC and GHR, are quite vague. Consequently, the questions become: can the indeterminist show that evolution is largely indeterministic, or can the determinist show that evolution is not largely indeterministic? Stamos has not addressed

these questions. Therefore, although Stamos's demonstration that point mutations are indeterministic supports a key point of BC's, it does not settle the debate in favor of the percolation argument for indeterminism.

3. The Autonomous Strategy

The second strategy by which one might argue for the indeterminism of evolution does not look to QM to undergird the argument, but instead argues directly for the indeterminism of higher level evolutionary processes. In this strategy, QM plays a role that is at best peripheral, perhaps opening the door for the possibility of indeterminism. However, since no explicit connection is made, it can be said that evolution is treated autonomously.

BC use this strategy in their example 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. BC argue that these kinds of experiments are evidence for the indeterminism of evolution.

Glymour's (2001) arguments primarily follow the same autonomous strategy of arguing from a higher level process, i.e., an evolutionary process, although he uses the percolation strategy as well. For the moment, I will focus on his use of the autonomous strategy. Like Stamos's essay, Glymour's essay is biologically sophisticated, philosophically interesting, and a

genuine contribution to the literature. And as with Stamos's essay, my summary of Glymour will not do it justice; I will be simplifying his discussion considerably. Glymour examines the phenomenon of random foraging behavior, as it has been observed in bluegill sunfish and the parasitoid wasp. Glymour demonstrates that when foraging, these organisms use a random search strategy, as opposed to a systematic strategy; their behavior can also be described as a random walk.

Moreover, if I understand Glymour correctly, he is suggesting that not only are the paths random (rather than systematic), but that they are generated randomly; that is, the organisms do not follow the same nonsystematic path each time, but a different nonsystematic path is generated each time. Glymour seems to be suggesting that this random generation of random patterns is evidence for the indeterminism of the foraging process. Glymour then makes the case that foraging search paths affect both energy expended and the number of prey captured, and are thus subject to natural selection (presumably, on the assumption that search path behavior is heritable). Therefore, he concludes, there is evidence that evolution is at least sometimes indeterministic. (Glymour thus succeeds in tying his argument to evolution, whereas as we saw above, Stamos fails to do this).

So, to put the point simply, in both BC's and Glymour's arguments, a random pattern is identified at the evolutionary level, and a case is made that indeterminism is the best explanation for the random pattern. Both essays acknowledge the obvious counterargument, that the patterns could be the result of determinism in conjunction with hidden variables. In BC's example, a determinist can maintain that either the organisms were not truly identical (a mistake occurred

during the cloning process),[7] or the environments were not truly identical (a mistake was made in constructing the environmental settings). Now, in Glymour's example, we would need to know more about the causal factors that give rise to foraging behavior in order to decide which hidden variables are plausible (if any). Again, however, deterministic hidden variables are at least in principle possible.

"Random" functions in computer programs provide a useful analogy here.[8] Most computer languages contain a random function, often called "rand". Suppose a program is run that calls the function "rand" ten times, producing a sequence of ten numbers. Each time the program is run, the same sequence of ten numbers would be repeated – which is not terribly random! However, "rand" functions can be fed what is called a "seed." Different "seeds" produce different sequences. Let us now suppose that each time we run our program, the "seed" used is the time on the computer's clock (or better yet, the duration between keystrokes on a keyboard, which is difficult to reproduce). Now each time the program is run, a different sequence of ten numbers is produced. By all appearances, the pattern of sequences is random. Of course, it is not really random; the program is deterministic, and, more specifically, the sequence is specified by the seed. But that is exactly the point. A pattern can look random and yet be the result of a completely deterministic process. Indeed, random foraging patterns can be generated by deterministic computer programs. An observed random pattern is not sufficient evidence for indeterminism.

Thus, the basic problem with the autonomous strategy is that when you have random data patterns (even random data patterns generated randomly), it is possible to account for the data in

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7 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).

two ways. The random data patterns may be the result of an indeterministic process, or the random data patterns may be the result of a deterministic process in conjunction with one or more hidden variables. The challenge then becomes how to argue for one position without begging the question against the other position.

BC's response to this challenge is to assert that one should only posit a hidden deterministic variable if it aids in the development of the theory; however, "the positing of deterministic hidden variables in evolutionary theory serves no theoretical purpose at all" (331). GHR attempt to counter this claim by suggesting that it is the positing of deterministic hidden variables that provides the impetus to seek out additional causes, and that "to do otherwise is to abdicate the scientist's self-appointed tasks" (153). However, as both Stamos and Glymour point out, an indeterminist can still seek out hidden variables for some of the apparent randomness, with the understanding that some randomness will remain due to fundamental indeterminism.

What this exchange between the determinists and the indeterminists illustrates is that there are really two issues here. The first issue is whether it is ever useful to think that there may be hidden factors producing a random pattern. Here, both indeterminists and determinists seem to grant that sometimes it is useful to think there are deterministic hidden variables – but when? When it aids in the development of the theory, as BC suggest? But how could we know that in advance? It seems to me that (at best) it is only in retrospect that we can say whether a search for particular hidden variables has been a waste of time. The second issue is the more general one of whether there is some sort of methodological advantage to being a determinist or an indeterminist. However, it is hard to see what that would be. Furthermore, it is not at all clear that biologists (now or in the past) have ontological conceptions of determinism or

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[8] These are often called "pseudo-random" functions, for reasons that will become clear.

indeterminism in mind when they develop evolutionary theory. I do not think methodology will tell us whether to prefer a deterministic or an indeterministic account of a random pattern.

Glymour, on the other hand, addresses the issue of accounting for a random pattern deterministically by stating that, "[w]e would have reason to take this possibility seriously, that is, to doubt that random search behavior is genuinely random, if it were simply impossible for organisms to exploit indeterministic features of their own physiology or environment in generating search behavior" (2001, 526). He then proceeds to motivate the claim that indeterministic behavioral mechanisms are possible. However, this response is not persuasive, largely because it stacks the deck against the determinist and in favor of the indeterminist. The determinist should not have to show that it is *impossible* for the behavior to be indeterministic; the determinist only needs to show that the behavior is not *in fact* indeterministic. The indeterminist, on the other hand, needs to do more than show that indeterminism is possible. (Both sides grant that indeterminism is possible, in any case). At the risk of stating the obvious, many things are possible without being actual. Thus, the indeterminist needs to show positive evidence in favor of indeterminism.

4. A Return to Percolation?

Perhaps in recognition of this point, Glymour shifts from an autonomous strategy to a percolation strategy, appealing to the indeterminism of the underlying processes. He asserts that "[w]hatever procedure is used to generate random behavioral patterns and random searches in particular, at some level this procedure must commonly involve the operation of cells" (2001, 526-7). Furthermore, there is evidence, Glymour claims, that ion channels "behave

stochastically" and that "this stochasticity appears to account for variations in cellular behavior";
more specifically, there is evidence that the stochastic behavior of neural cells has some effect on
learning behavior (2001, 527).

However, the studies that Glymour cites simply raise more questions. What reason do we
have to think that ion channels behave *indeterministically* – which is more than saying that they
behave *stochastically* ("stochastic behavior" being merely an observation of statistical
outcomes)? Namely, what is the underlying quantum mechanical process? If the ion channels
are indeterministic, how does this indeterminism percolate to the level of cellular behavior, for
neural cells in particular? Moreover, if neural cells do behave indeterministically, then how does
this indeterminism percolate to the level of foraging behavior and search patterns?

In short, Glymour neither defends his claims for indeterministic cellular behavior nor
demonstrates the connection between the cellular behavior and the foraging behavior. What is
needed is a discussion of the specific processes that produce foraging behavior, not just an
assertion that there must be some connection. As Glennan (1997) points out, there are some
macro-level events that are deterministic (in spite of micro-level indeterminism) – such as
pushing a button on a Coke machine – and there are others where the indeterministic micro-level
clearly does percolate up to produce an indeterministic macro-level phenomenon – such as the
action of a Geiger counter. So, is foraging behavior like a Coke machine or like a Geiger
counter? (Or somewhere in between?) Glennan's suggested method of answering questions such
as this is the similar to the one that I am urging, namely to require a detailed understanding of the
underlying causal processes. Unless an explanation of the how the percolation from the micro-
level to the macro-level is occurring can be given, it is just the indeterminist's intuition versus the

determinist's intuition, with no clear reason to prefer one intuition over the other. In other words, Glymour has committed the "percolation fallacy" that was discussed above.

Even if I am correct that Glymour does not make a positive case for indeterminism, however, the following challenge of Glymour's remains. He asks, if there were indeterminism at the micro-level (a point that the determinist's position grants), what would prevent its percolation up to the macro-level? If percolation to the evolutionary level of behavior patterns could happen, but never happens, it seems as though we have a mystery on our hands: "were it a fact that no [indeterministic] behavioral patterns exist, it would be, as far as we can now say, simply an accidental fact: the generalization is backed by no conceptual reason, nor by any putative laws of nature, but rather depends for its truth on a happy sequence of evolutionary changes in which on every occasion where an indeterministic mechanism is possible and would be advantageous, no such mechanism ever arises" (2001, 527). Part of Glymour's argument here makes the point that if an indeterministic mechanism were to arise at the "level of macro-physiology or organismal behavior", that such a mechanism could very well be advantageous and therefore subject to natural selection (2001, 527). This point seems well taken, and Glymour's own example supports it; if random foraging behavior were an indeterministic mechanism, it would be preserved through natural selection (just as it would be preserved if it were a deterministic mechanism). Sometimes randomness is advantageous, and it should not matter if it is randomness via a deterministic mechanism or an indeterministic one.

That said, however, it remains to be seen whether the assumption underlying Glymour's argument is true: whether indeterministic mechanisms arise at the level of macro-physiology or organismal behavior (in other words, do they arise in the first place for natural selection to act upon)? Glymour seems to be suggesting that since indeterministic percolation is possible, if

there were no such indeterministic mechanisms it would purely be by accident. In other words, if there is no law to prevent them, it seems as though somewhere, sometime, they ought to occur. If percolation is possible, then the world is not safe from indeterminism.

Here I worry that Glymour is again teetering on the edge of the percolation fallacy. But putting aside that worry, suppose that he is right – suppose that somewhere in the world, indeterministic mechanisms have arisen at the level of macro-physiology or organismal behavior, and that these mechanisms have been preserved by natural selection. This, of course, would not show that *foraging behavior* was indeterministic, nor would it show that evolution is *substantially* indeterministic. Perhaps indeterministic events at the evolutionary level are uncommon, as with BC's example. Or perhaps not. My point here is that this argument will not settle the debate either way. A better strategy would be a more thorough analysis of the studies on neural cells, as discussed above.

In sum, Glymour is forced to shift from the autonomous strategy to the percolation strategy, but since he does not follow through completely on the percolation strategy, he does not argue persuasively for the indeterminism of evolution.

5. Conclusion

So, what is the current status of the debate? Where have we gone right, and where have we gone wrong? Where do we go from here? I would argue that the debate is still unresolved, but that the discussion has proved illuminating as to what is, and what is not, a good strategy for settling it. The autonomous strategy is appealing if one is worried about biology being reduced to physics, or biology becoming the handmaiden of physics, or the like. However, I think those

worries are misplaced, first of all, because biology is a well-established science (currently garnering more than its share of grant money, I might add), and second, because the issue here is not a global reduction of evolution to QM. The issue is rather a more limited one of whether we need to appeal to QM to settle the question of whether evolution is indeterministic. The problem is that if we don't appeal to the indeterminism of QM – that is, if we don't appeal to a point that is granted by both sides – we end up begging the question against each other. We end up arguing over whether there are, or are not, hidden variables; we end up arguing over whether indeterminism is, or is not, a better methodological assumption. These arguments are destined to go nowhere. The autonomous strategy is a nonstarter.

That leaves the percolation strategy. Here I think we can draw from what is best about the existing arguments for indeterminism. What is best about Stamos's argument is that he does not gloss over the connection between QM and point mutation; he demonstrates it. What is best about Glymour's argument is that he does not gloss over the connection between foraging strategy and evolution; he demonstrates it. Stamos succeeds at the lower level, whereas Glymour succeeds at the higher level. In short, if we want to argue for the percolation of QM to evolution, we need to demonstrate direct causal connections between the indeterministic phenomena at the lower level and the statistical behavior at the next highest level, at all the pertinent levels between the micro-level and the evolutionary level (as discussed above in the context of foraging behavior). What is compelling about BC's example is that it provides a direct connection between point mutation and evolution; but again, it is such an unusual example that it doesn't demonstrate that evolution is largely indeterministic. That's the final caveat; whatever phenomenon we choose to examine, it ought to be a common one, keeping in mind that the debate is not over the occasional percolation, but over whether evolution is significantly

indeterministic. Candidate phenomena for demonstrating indeterminism include Stamos's and Glymour's own examples of point mutation and foraging behavior, respectively, as well as the phenomenon of developmental noise (Millstein 2000).

Perhaps the most important lesson that we can take away from an examination of this debate is that it should not be about whether evolution is completely deterministic or completely indeterministic. Rather, the crucial question is: which aspects or processes of evolution are deterministic, which are indeterministic, and to what extent? Once we have answered this question (no small undertaking!), we will have advanced considerably in our understanding of the nature of evolution.

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