The conflation of “chance” in evolution

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Abstract. Discussions of “chance” and related concepts (such as “stochasticity,” “randomness,” “indeterminism,” etc.) are found throughout philosophical work on evolutionary theory. By drawing attention to three very commonly-recognized distinctions, I separate four independent concepts falling under the broad heading of “chance”: randomness (as a property of sequences), epistemic unpredictability, causal indeterminism, and probabilistic causal processes. Far from a merely semantic distinction, however, it is demonstrated that conflation of these obviously distinct notions has an important bearing on debates at the core of evolutionary theory, particularly the debate over the interpretation of fitness, natural selection, and genetic drift.

Keywords: chance, fitness, genetic drift, natural selection, propensity, randomness

1. Introduction

Our discussions of evolutionary theory are saturated with references to the concept of “chance.” Though many instances can be found, including macroevolution (Conway Morris, 2009; Desjardins, 2011), mutation (Stamos, 2001; Dietrich, 2006; Merlin, 2010), and even topics as diverse as foraging theory (Glymour, 2001), perhaps the most hotly debated comes from the literature surrounding the appropriate interpretation of natural selection, fitness, and genetic drift. In recent years, two main positions on the nature of selection and drift have crystallized. One approach, deriving from early literature on the propensity interpretation of fitness (Brandon, 1978; Mills and Beatty, 1979), considers selection and drift to be probabilistic causal processes (Stephens, 2004; Ramsey, 2006; Abrams, 2009; Otsuka et al., 2011). Another approach, which has come to be known as the “statisticalist” interpretation of evolutionary theory (Matthen and Ariew, 2002; Walsh et al., 2002; Ariew and Lewontin, 2004; Krimbas, 2004; Walsh, 2007; Ariew and Ernst, 2009; Walsh, 2010), claims that, rather than causal processes, selection and drift are merely statistical trends, abstracted from real events in the lives and deaths of individual organisms.

It is clear that many concepts falling under the broad heading of “chance” are present within this debate. Fitness itself is considered probabilistically because different organisms in different environments will have, as Darwin noted, a differing “chance of leaving offspring” (Darwin, 1859, p. 88). Genetic drift is described as “random,” often on the basis of our inability to predict in advance, for any given population, the precise impact of genetic drift on its evolution – Brandon calls drift a “prediction without a direction" (2006, p. 325). And the choice between the statisticalist and causalist approaches seems to be one between objective and subjective notions of "chance."

Given this tight relationship, one would expect that there would be an extensive literature discussing the relationship between these various notions of chance and the interpretation of evolutionary theory. Surprisingly enough, however, such a literature is almost entirely absent, and I hope to take the first step toward providing this analysis here.

I will begin by laying out a set of relatively innocuous distinctions which we may use to tease out the specific meaning of "chance" at work in a given argument. For our purposes, I will separate four distinct "chance" concepts. Far from being a merely semantic exercise, however, I will then turn to two examples: one drawn from the causal-interpretation camp, and one from the statisticalists. Both of these arguments, I claim, are dramatically undercut by a thoroughgoing conflation of these various notions of "chance." While the distinctions I draw, then, are well-known, it seems that they are not well-understood and not utilized with sufficient caution.

2. Some Concepts of "Chance"

Before I turn to clarifying some of the concepts that fall under the heading of "chance" in the philosophy of biology literature, I should pause for several precautionary remarks. First, I have singled out the term "chance" for no particular reason other than its breadth. Chance is just one of a family of concepts, all often (and unfortunately, often interchangeably) invoked in the philosophical literature. Equally well could we frame the discussion in terms of "randomness" in evolution, "stochasticity" in evolution, or "indeterminism" in evolution, to name only a few examples. This trouble with meaning is most certainly part of the problem – it is, in fact, quite difficult to speak with clarity on the issue, as this entire array of terms lacks current, widely-accepted meanings.

Second, I do not intend to offer here anything like an exhaustive "taxonomy" of the concept of chance (and, again, these related terms), as used in the biological sciences. Indeed, such an exhaustive description may well be impossible, and there is certainly no *a priori* reason to think that "chance" would be the appropriate "root" for such a taxonomy. Instead, I will separate out four notions of "chance" using three distinctions, all of which are, I believe, widely appreciated and utilized. A mere recognition of these four senses and the differences between them will suffice for the arguments in the remainder of the paper.

2.1. Three Distinctions

2.1.1. *Chance versus Randomness*

The first distinction separates chance from *randomness.* While agreement on the matter is not universal, as a general rule we may say that "randomness" is a property of a sequence of *outcomes,* while "chance" is, in some sense, a property of the process, device, causal system, or what-have-you, which produces these sequences.[1] This parallels usage in mathematics – much of the mathematical literature on randomness has focused on criteria by which a sequence may be demonstrated (as far as it is possible to do so) to be random (Martin-Löf, 1966; Martin-Löf, 1969; Kolmogorov and Uspenskii, 1987).[2]

2.1.2. *Subjective versus Objective Chance*

On the "process" side of the prior divide, we may next separate subjective from objective chance. Drawing from the literature on the interpretation of probability theory, this distinction, too, has been fairly cleanly drawn in the philosophical literature.

For our purposes, I intend to bring out only one clearly subjective notion of chance: chance as *unpredictability.* The biological literature is replete with chance in this sense, as it is the one which most directly impinges upon the biologist's ability to execute experiments. In particular, the population genetics literature makes extensive use of unpredictability (Roughgarden, 1979).

2.1.3. *Causal Indeterminism versus Probabilistic Causal Processes*

The last distinction we require, between two different possible varieties of objective chance, is a bit more esoteric. To begin, many authors in the debate – especially those supporting the propensity interpretation of fitness – have argued that the outcome-randomness we see at the biological level requires philosophical explanation in terms of *probabilistic causal processes.* If individual fitness, for example, is a probabilistic cause which governs the reproductive output of a given organism, then it is obvious that the reproductive outputs of an ensemble of identically prepared organisms will constitute a (biased) random sequence.

On the other hand, we might think that this outcome-level randomness is due not to probabilistic causal processes, but to genuine *causal indeterminism* manifest at the macro-level. Stamos (2001), for example, has argued that quantum-mechanical effects (which, we may assume for the sake of argument, are genuinely indeterministic in this sense) have direct effects at the population-genetics level.[3] Both Brandon and Carson (1996) and Glymour (2001) have argued that in addition, the same kinds of concerns which cause us to ascribe causal indeterminism to quantum mechanical processes should lead us to believe that the same sort of indeterminism is manifested at the evolutionary level.

There are therefore two different concepts of objective chance here. But are they necessarily independent? That is, might we just argue that probabilistic causal processes must be undergirded by genuine causal indeterminism, declare these two notions of objective chance to be equivalent, and move on?[4] Drawing on work by Sober (2010), I believe this claim would be mistaken. Recall the way in which Popper originally offers support for the propensity interpretation of probability. Probabilities, in his terms, are grounded in "relational properties of the experimental arrangement" (Popper, 1959, p. 37). Now consider the example of a coin-flipping device in an entirely deterministic universe. Surely, if this device flips coins with some range of initial velocity and angular momentum parameters, then there exist relational properties of the experimental arrangement that will suffice to ground the truth of some claim like "the probability of this coin-flipping device producing a heads-up coin is one half." Of course, for any individual coin, we could (if we so chose) determine with precision (probability one or zero) whether that coin would land heads or tails. But the relational properties of the coin-flipping experiment as a whole still stand – and they stand, it seems, even if only one coin (or no coins at all!) is flipped by the device.

Further, there seems to be a sense in which we are justified in saying that the device *causes* the coin to wind up either heads or tails. But it does not cause one outcome or the other: rather it causes heads at a certain probability, and tails at another probability. Nor does it need to violate physical law to do so. Rather, these macro-level probabilistic facts *just are* a way of summarizing the micro-level physical facts. Such a process would be an objective feature of the world – this would remain a valid, causal description of the device even if there were no observers around to describe it – and it does not depend at all on whether or not the underlying laws governing the box are deterministic or indeterministic. Hopefully, then, we have a minimal notion of probabilistic causal processes which both can be instantiated in an entirely deterministic universe and makes reference to nothing particularly metaphysically outlandish. Causal indeterminism is therefore independent of the possible existence of probabilistic causal processes.

2.2. Four Notions of Chance

We are therefore left with four quite independent, yet related, concepts that fall under the broad umbrella of "chance" (see figure 1). We have randomness as applied to sequences, unpredictability (as a variety of subjective process-chance), and causal indeterminism and probabilistic causal processes (as two varieties of objective process-chance).

Again, it is worth reiterating that I do not intend this to be (anywhere close to) a complete partitioning of all possible concepts which might fall under the broad scope of "chance" (or "randomness," or "stochasticity," or

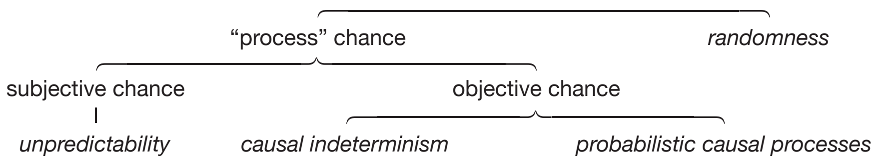

Figure 1. Drawing three relatively common distinctions within the broad category of "chance" leaves us four concepts which we will use in the following (italicized).

"indeterminism," etc.). I rather believe that it is a fairly inoffensive set of four obviously distinct and independent notions at play in work on chance and randomness, four notions which we can agree we ought to keep separate in our philosophical work on "chancy" phenomena.

It is precisely this caution, then, that I hope to show has been absent from much of the philosophy of biology literature. I will provide two examples – one debate from the propensity interpretation of fitness literature, and one from the statisticalist literature – which are both rendered highly problematic due to a persistent conflation of these four distinct concepts.

3. The Propensity Interpretation of Fitness

Other than the often-repeated claim that fitness must be interpreted as a propensity in order to prevent the "survival of the fittest" from coming out tautologous, authors rarely discuss what exactly it is that is supposed to make fitness and selection "chancy." One attempt, however, to provide an answer to this question is provided by Brandon and Carson (1996).[5] They ascribe this chanciness to the influence of genetic drift, so we should begin with their characterization of drift.

Brandon and Carson (henceforth BC) are defenders of what might be called the "sampling error" school of genetic drift: "we suggest," they write, "that genetic drift be characterized as any transgenerational (evolutionary) change in gene or genotype frequencies due to sampling error" (1996, p. 321). For BC there exists, that is, an expected outcome – the outcome we would predict were a given scenario governed merely by natural selection, with offspring populations produced in proportion to fitness values. Any deviation from this expected outcome due to what they call "random sampling effects" (sampling of gametes to form offspring, of survivors of a population bottleneck, etc.) is defined as genetic drift.

Turning to selection and fitness, then, BC argue that "natural selection is indeterministic at the population level because (in real life as opposed to certain formal models) it is inextricably connected with drift" (1996, p. 324).

The chance present in drift, that is, will be used to ground a very robust sense of "indeterminacy" in the evolutionary process. It seems that they intend this use of "indeterminacy" to refer to some sort of objective chance – they argue that "if one is a realist...then one should conclude that [evolutionary theory] is fundamentally indeterministic" (1996, p. 336).

Their discussion of genetic drift, however, does little to provide support for the quite strong role they expect it to play. First, they claim that "drift clearly is a stochastic or probabilistic or indeterministic phenomenon" (1996, p. 324). As we have already noted, however, this is an obvious conflation: "stochastic" generally refers to sequence-level randomness, "probabilistic" might refer to any sense of objective chance, and "indeterministic" usually applies only to the particular sense of causal indeterminism. What sense of chance, then, do they consider drift to exemplify?

Deciphering a response to this question in their arguments is quite difficult. Most problematically, many of their assertions about drift are phrased in terms of "the inferences we can make" (p. 322), or what genetic drift "can predict" or "cannot predict" (p. 323). But turning again to our distinctions from the last section, unpredictability is a merely epistemic notion of chance, and while it might offer us some indication that a particular phenomenon in nature is objectively "chancy," there is no determinate relationship between objective and subjective chance. We may have both objectively chancy and highly predictable systems (such as collapse interpretations of quantum mechanics), or we may have non-chancy, yet highly unpredictable systems (such as in the case of deterministic chaos (Werndl, 2009) or systems like Norton's dome (Norton, 2003; Malament, 2008)).

If, therefore, BC intend their argument regarding drift to support the stronger proposition that genetic drift exhibits not merely unpredictability, but some sort of objective chance, it has missed the mark.

They return briefly to this issue near the end of their paper, arguing that even if events at the level of individual organisms are in fact "purely deterministic, the population-level generalizations are probabilistic" (p. 335) because these generalizations are "autonomous" statistical laws, in the sense of Hacking (1990). This argument is similar to the one I used in the last section to separate probabilistic causal processes from causal indeterminism. Even though population-level generalizations are grounded in events in the lives of individual organisms, there may remain a sense in which we can say that these higher-level generalizations are objectively probabilistic, even if these individual-level events are not.

Two responses are particularly salient. In order to ground this inference to the objectivity of macro-level probability ascriptions, BC appeal to Hacking's criterion of "autonomy." This notion of autonomy should certainly not, I argue, be used in this manner. Hacking defines autonomous statistical laws as those which "could be used not only for the prediction of phenomena but also for their explanation" (Hacking, 1990, p. 182). But what grounds the ability of these laws to make such predictions is not described in Hacking's work. The criterion as it stands is under-specified, if not circular: autonomous statistical laws are those that can explain, and the laws that can explain are, perhaps, the autonomous ones.

It is important to note that this causes no problems for Hacking's own analysis. Hacking's project is a historical one: he is searching through history in order to locate the first time that an experimenter *used* a statistical law *as though it were autonomous,* i.e., as though it supported explanations. Francis Galton, Hacking argues, was the first to do so. The grounding problem is not, therefore, Hacking's – he is not attempting to tell us *why* Galton believed that these statistical laws were able to explain, merely that he did so. It follows that Hacking's criterion of autonomy cannot, therefore, be used as an independent method of sorting statistical laws into two kinds: autonomous and non-autonomous.

Secondly, BC focus once again on prediction and explanation, which does nothing to clarify our confusion about what sort of objective chance it really is that BC are searching for. On Hacking's notion of autonomy, the ability to ground explanations clearly adds *something* over and above prediction, so we have arrived at a notion of chance slightly more robust than the merely subjective unpredictability we saw above. What exactly it is that has been added, however, is left entirely unstated.

Let's turn, then, to a response to BC published by Graves, Horan, and Rosenberg (1999, henceforth GHR). As I have laid out BC's argument, it would seem that GHR should have much to critique, and indeed they do. They begin by agreeing with (or, at least, accepting for the sake of argument) BC's characterization of drift as sampling error. They then go further, noting that some (citing Hull (1974) and Beatty (1984)) have claimed "that the theory of evolution is statistical precisely because of this fact" about drift.

For GHR, however, "the following question remains: Are the probabilities employed in the theory epistemic or not?" (1999, p. 146). That is, they reiterate the question which we have pursued here: since it seems that BC want to establish drift as providing some sense of chance more robust than mere epistemic unpredictability, what exactly *is* this sense of chance? As I have noted, BC provide no definition of what this might consist of, nor do they give this position any real grounding. GHR therefore conclude that BC's argument "*presupposes* objective chanciness" (1999, p. 150, original emphasis), rather than arguing for it. Begging the question is, GHR claim, the only manner in which BC's conclusion could follow from their premises.

This seems to be fairly close to a knockdown point in favor of GHR. BC assert that genetic drift exhibits some sort of robust "indeterminacy," then argue not for this point but the unrelated claim that genetic drift is unpredictable. But is this really the most charitable reading that we can provide of

BC's argument? I argue that it is not. In particular, if we resolve the confusion present in BC's argument over the idea of chance, we can make their point stronger (and make their argument not, or not obviously, beg the question). I will hazard that they wish to argue that genetic drift is a theory of probabilistic causal processes.

We may support this reading of BC's argument in several ways. Most importantly, Brandon's other writings on drift are consonant with this view. His recent work describes drift as an element of the "Zero Force Evolutionary Law," which states that in all evolutionary systems (with variation and heredity) "there is a tendency for diversity and complexity to increase" (Brandon, 2010, p. 708). His toy model of this law consists of a particle diffusing away from an initial position with given transition probabilities (Brandon, 2010, p. 703). After a few iterations, we are able to specify the particle's state only probabilistically, by specifying the odds that the particle will be found in each of the possible end-states.

Importantly, however, there is for Brandon *a causal explanation* underlying this diffusion process. He notes that "the drift producing potential of the sampling processes that are constitutive of the evolutionary process is a fundamental part of the causal structure of our world," featuring in a special class of causal explanations he calls "default-causal" explanations (Brandon, 2006, p. 329).

In a section titled "Drift as a Causal Concept" in his book co-authored with Daniel McShea, they write that an important requirement in coming to understand scientific explanation is "to develop an adequate account of probabilistic causation that would ground such probabilistic explanations" (McShea and Brandon, 2010, p. 106). Indeed, they seem to endorse this way of understanding genetic drift directly when they write that

> Some might think of drift as the absence of cause. But, as we see it, the relevant causal understanding is the full set of objective probabilities that govern the entities to be sampled. ...causal understanding is achieved when we assemble the relevant probabilities governing the events in question. (McShea and Brandon, 2010, p. 107)

Returning to Brandon and Carson, my reading of BC's argument is also supported by their peculiar reference later in the paper to arguments from "hidden variables." They claim that the appropriate way to overturn their claim of indeterminacy would be to demonstrate that there were unequally distributed hidden variables governing the evolutionary process, and then that "assignments of relative fitnesses would be merely epistemic, merely useful instruments given our state of ignorance of the hidden variable[s]" (Brandon and Carson, 1996, p. 326). Initially, this argument seems quite perplexing. Assuming, for the sake of argument, that the world may be approximated reasonably well by Newtonian mechanics,[6] there exists a quite obvious set of hidden variables that would do the job: namely, the positions and momenta of all the particles in the universe. These would *clearly* predict future biological outcomes, and our assignments of relative fitnesses (just like our assignments of all quantities other than particle positions and momenta) would be the result of our ignorance of these hidden variables.

So what do BC intend this argument to show? If, consonant with the reading here, they are concerned with probabilistic causes as described by genetic drift, then we may make sense of their claims. For in this case, what would be required to prove their argument wrong would be not deterministic hidden variables within some other theory upon which evolution depends (molecular biology, chemistry, physics, etc.).[7] Rather, defeating BC here would require the existence of deterministic hidden variables *within the very causal processes* of natural selection and genetic drift, driving these evolutionary outcomes (hidden variables of which they are right to claim that we have no evidence). To put this another way, if we can manage to keep the issue of probabilistic causation separate from the question of causal determinism, as I have argued in the last section, we can see that BC's position on hidden variables is much more explicable.

It seems, therefore, that BC conflate at least three of the notions of chance we laid out above (unpredictability, causal determinism, and probabilistic causation), and that only on one reading – the existence of probabilistic causation in genetic drift – can we make sense of their argument. This is, I claim, the notion of "chance" or "indeterminacy" that BC hope to find in evolutionary theory.

We should then return to GHR's response. They claim that begging the question is the only way in which BC's conclusions might follow from their premises.[8] However, I have provided a reading on which BC's argument concerns macro-level probabilistic causal processes. Reconstructed in this way, their hidden-variables argument is at least plausible, and their overall position doesn't obviously beg the question.

To return to the main theme, the tone throughout GHR makes it clear that their primary concern is not with probabilistic causal processes, but rather with something more like causal indeterminism (combined with some worries regarding reduction or supervenience). They argue that "an omniscient being would not be using probabilities at all to guess at outcomes in situations like" genetic drift (1999, p. 151). Such an omniscient being would merely use information from underlying theories – perhaps biochemistry or physics – in order to predict exactly (and deterministically) what the final fate of a population undergoing genetic drift would be. As I have noted earlier, however, this is a non sequitur – these are not the right kind of "hidden variables" to disprove BC's argument.

GHR's next critique claims that BC's conclusions would be tantamount to proposing "ungrounded propensities of populations, causally inexplicable by the manifest properties of organisms on which they supervene" (1999, p.

155). But it is clear that this, too, need not follow. Probabilistic causation may, as I have already discussed, be present even given the existence of underlying determinism. GHR have clearly conflated unpredictability with causal indeterminism.

We are thus in the following position: BC have conflated (at least) three different notions of "chance" in their argument, and GHR have conflated (at least) two in theirs. As a result, it is no longer obvious that GHR's argument even genuinely engages with the paper it was intended to critique. This is, to put it mildly, a strong indication of a philosophical problem. Conflation of the various "chance" concepts I have identified above, therefore, is quite clearly responsible for problems in the arguments of otherwise quite careful philosophers.

4. The Statisticalist Position and "Chance"

In their initial elucidation of what has come to be known as the "statisticalist" interpretation of evolutionary theory, Matthen and Ariew (2002) claim that there exists no way, in principle (as opposed to in specific empirical examples), to distinguish the effects of natural selection and genetic drift, if both are considered probabilistically. One portion of this argument is phrased in terms of the role of "chance" in various evolutionary explanations. Let's unpack it in detail.

In general, Matthen and Ariew (henceforth MA) aim to demonstrate that believing, as proponents of the "causal process" notion of genetic drift do, that natural selection and genetic drift may be separated as distinct causal influences on the evolutionary process "violates sound probabilistic thinking," and that it does not make sense "to say that drift is a *force* or, more generally, a cause of change that acts independently of selection" (2002, pp. 62, 60). They begin with the following example:

> Consider this analogy. You toss a coin four times. What would explain the outcome *two heads?* Answer: the physical setup of the coin-tossing trials. What would explain the outcome *four heads?* Answer: the same thing. Although the second result is less probable, the same setup explains both outcomes. (Matthen and Ariew, 2002, pp. 60–61)

They go on to offer an almost identical instance of this argument as it applies to a biological population. Consider two heterozygous populations, one of which evolves to heterozygosity at each allele. One of these alleles, however, is more fit than the other in the given environment. "What explains this difference" in outcome, they ask? "The answer, just as in the case of the coin, might well be: *exactly the same thing,* the same positioning of the traits in the adaptive landscape" (Matthen and Ariew, 2002, p. 61).

The argument, therefore, seems to go something like this. We have a particular random sequence (be it the results of a coin-tossing experiment, or population-level evolutionary outcomes). Defenders of the causal interpretation of genetic drift argue that there are two, separate influences that determine outcomes in situations like these: genetic drift and natural selection. This amounts to the claim, however, that one could "partition the homogeneous reference class to which these trials belong by *improbability* or *chance,*" tagging "these things [selection and drift] as 'forces' that occasionally favor the less likely outcome" (Matthen and Ariew, 2002, p. 61). A bit later, they provide a more detailed version of this claim:

> Although it is six times more probable that two heads will turn up in a run of four tosses of a coin than that four will, chance does not play any more of a role in a particular run of four heads than in a particular run of two and two. Thus, one cannot in general differentiate between individual events on the basis of how much they are attributable to chance. (Matthen and Ariew, 2002, p. 64)

Causal theorists of genetic drift, they argue, are in effect asking us to take events in the lives and deaths of organisms, and divide them up into two piles: "more chancy" (the domain of genetic drift), and "less chancy" (the domain of natural selection). Such a division is impossible, MA argue, and given this, the causal interpretation of genetic drift fails.

There are several ways for a causal theorist to respond to this argument. One, taken by Pfeifer (2005), is to challenge MA on their understanding of the probabilities at work in evolution, causing their argument to fail on its own terms. While I think this is a worthwhile approach, it is not the one I will take here. Rather, our discussion thus far should make one feature in particular of this argument stand out: what is it that MA mean by "chance" when they claim that the causal theory is committed both to the partitioning of evolutionary events in this manner, and to the claim that "it makes sense" in these cases "to quantify the role of chance" (2002, p. 64)?

First, we may convincingly rule out two of our already-mentioned notions of "chance": unpredictability and causal indeterminism. MA state very explicitly that it is their intent to demonstrate that "there is, in general, no *objective, as opposed to epistemic,* apportioning of causal responsibility to selection as against drift in a concrete evolutionary history" (2002, p. 68, emphasis added). This statement confirms that MA are certainly not interested in merely epistemic notions of unpredictability. They also state explicitly that while one situation "in which the notion of probabilistic causation is invoked" is "in quantum mechanics, where it is claimed that the interaction of certain variables is irreducibly indeterministic...one would not be justified in claiming that the individual events above [in the examples already mentioned] contained elements of indeterminacy" (2002, pp. 62–63, fn.). They thus also disavow any interest in the issue of causal indeterminism.

What remain, then, as possible referents for MA's use of "chance"? I see two answers to this question, both of which indicate trouble for their argument. First, we might read them, as we did BC's argument earlier, as concerned with the issue of probabilistic causation in biological systems. This doesn't seem right, though. For nowhere is the causal theorist of genetic drift committed to the claim that the process of natural selection is "less chancy" and genetic drift "more chancy" in this sense. Most problematically, I am not certain how being a probabilistic causal process can come in degrees. Indeed, it is often claimed that non-probabilistic causation is merely a special case of probabilistic causation, where all probabilities are either one or zero.[9] If this is right, then the distinction between probabilistic and non-probabilistic causation is binary: either all probabilities are one or zero, or they are not. If they are not, there can be no sense in which one process is "more probabilistic" than another.

Further, even if there were a sense in which two causal processes could be compared on this axis, it is not obvious that the causal theorist of drift would be committed to the claim that drift was the "more chancy" of the two processes. Certainly, natural selection is a *biased* probabilistic process – biased, of course, by organismic fitness values (if one adheres to the propensity interpretation of fitness). But while drift lacks this precise sort of bias, it is biased by a host of other variables, including population size as well as a host of other environmental effects. There is no sense, then, in which drift is an "unbiased" causal process.

Interpreting MA's use of "chance" as probabilistic causation, despite several indications that this is how they intend their argument to be interpreted, fails. This leaves us, I claim, with only one notion of "chance" left: the randomness of the outcomes of the processes of drift and selection. If we interpret MA's use of chance in this way, we may at least understand their arguments: it is indeed impossible to take individual outcomes from a particular random evolutionary sequence and claim that some of these outcomes are "more random" or "less random" than others.

This leaves MA with a different, but equally devastating problem. For this argument does nothing whatsoever to attack the coherence of the causal-process notion of drift and selection. Just looking at the outcomes of a particular sequence, as Millstein (2002) effectively argued in response to Beatty (1984), tells us nothing about the processes generating those outcomes. On the most common uses of selection and drift, she notes that "although random drift and natural selection can be distinguished when they are conceived as processes, they cannot be distinguished fully when conceived as outcomes" (Millstein, 2002, p. 46).[10] MA's failure to distinguish selection and drift, therefore, comes as no surprise, given that the only way to make sense of their argument is to read it as attempting to distinguish the *outcomes* of selection and drift.

This points to the same philosophical trouble in which the BC/GHR debate found itself. MA claim to be arguing against the coherence of the entire causal view of selection and fitness, the token example of which they take to be Sober's force metaphor (2002, pp. 58*ff*). But given the fact that they have conflated chance in the sense of random outcomes with chance in the sense of probabilistic causation, their argument entirely fails to engage with the view that they claim to be opposing. Once again, conflation regarding concepts of "chance" has caused these central arguments in the debate over the nature of drift and selection to entirely fail to engage with one another.

5. Conclusion

I began by laying out three commonly appreciated distinctions between concepts which fall under the broad heading of "chance." This left us with four notions of "chance" which have relatively well-understood meanings: (1) randomness of outcomes, (2) subjective unpredictability, (3) causal indeterminism, and (4) probabilistic causal processes.

We have then seen two examples in which conflation of these four notions has an important bearing on the way in which we should understand the debate over the causal role of natural selection and genetic drift. In order for Brandon and Carson's argument to work, we must resolve a systematic conflation of three notions of chance, Graves, Horan, and Rosenberg themselves conflate two separate notions, and Matthen and Ariew conflate a different two distinct concepts. These widely cited arguments are undermined at least in part by this thoroughgoing conflation.

It is certainly the case that the proper interpretation of chance and randomness is a difficult philosophical problem with a long and storied history. The same is true for the closely allied problem of the interpretation of probability. But to detect this confusion in argumentation has not required that we resolve all the issues that come with these challenging problems. Indeed, as I have been at pains to note, the three distinctions which we drew in order to arrive at our four separate "chance" concepts are part of the agreed-upon presuppositions of these philosophical debates – they are in and of themselves relatively uncontroversial. We fail to pay attention to them, as I have shown, at our own peril.

Notes

[1] See Eagle (2011) for a discussion of both a process sense of randomness and a product sense of chance, and the problems entailed by both.

[2] For a review of this mathematical work, see Dasgupta (forthcoming).

[3] Rosenberg (2001) and Davies (2004) discuss the issue as well.

[4] There is an interesting analogy here to be drawn with epiphenomenalism in the philosophy of mind, see Shapiro and Sober (2007).

[5] I can find two other attempts to solve this problem: Ramsey (2006) offers another solution, in terms of the possible lives of individual organisms, and Pfeifer (2005) ascribes the chanciness (at least in part) to our choice to ignore certain environmental features.

[6] Or, just as well for our argument, one could consider what the evolutionary process would look like were it instantiated in a classical universe.

[7] I follow Brandon and Carson's lead in choosing the relatively innocuous "dependency" to describe the relationship between biology and "lower-level" scientific theories like chemistry and physics. In particular, it "is not meant to imply that biology is reducible to chemistry and physics" (Brandon and Carson, 1996, p. 319).

[8] Notably, there is more to BC's argument than the one claim I have pointed out here, and there is thus more to GHR's rebuttal than simply this. On some of these other points – for example, BC's closing example on botanical experiments – GHR's critiques are quite right, and it is by no means my intent to cast doubt on those. My reconstruction of BC's main point, however, does not rely on that example.

[9] This seems to be the position, for example, of Woodward's analysis, on which probabilistic causation is accommodated simply by extending the non-probabilistic account "to include invariant generalizations relating the values of variables to the probability of some outcome, or the probabilistic distribution or expectation of some variable" (2003, p. 6).

[10] I should note that I do not intend to claim that there is *never* any way in which to separate the empirical effects of natural selection and genetic drift – there is a large literature that would lead one to conclude otherwise (Richardson, 2006; Millstein, 2008; Brandon and Ramsey, 2008; Millstein et al., 2009). The claim here is merely that for some arbitrary sequence of evolutionary outcomes, it is impossible in general to claim that a given outcome is "due to selection" or "due to drift."

Acknowledgements

Thanks to the participants in the "Chance in Evolution" seminar at Notre Dame for sparking my thoughts on this subject. Many discussions with Grant Ramsey have proven invaluable. Thanks to Don Howard and Fritz Warfield for sharpening my thoughts on the "taxonomy" of chance.

References

Abrams, M.: 2009, 'Fitness "kinematics": biological function, altruism, and organism-environment development'. *Biology and Philosophy* **24**(4), 487–504.

Ariew, A. and Z. Ernst: 2009, 'What fitness can't be'. *Erkenntnis* **71**(3), 289–301.

Ariew, A. and R. C. Lewontin: 2004, 'The confusions of fitness'. *British Journal for the Philosophy of Science* **55**(2), 347–363.

Beatty, J. H.: 1984, 'Chance and natural selection'. *Philosophy of Science* **51**, 183–211.

Brandon, R. N.: 1978, 'Adaptation and evolutionary theory'. *Studies in History and Philosophy of Science* **9**(3), 181–206.

Brandon, R. N.: 2006, 'The principle of drift: biology's first law'. *Journal of Philosophy* **103**(7), 319–335.

Brandon, R. N.: 2010, 'A non-Newtonian model of evolution: the ZFEL view'. *Philosophy of Science* **77**(5), 702–715.

Brandon, R. N. and S. Carson: 1996, 'The indeterministic character of evolutionary theory: No "no hidden variables proof" but no room for determinism either'. *Philosophy of Science* **63**(3), 315–337.

Brandon, R. N. and G. Ramsey: 2008, 'What's wrong with the emergentist statistical interpretation of natural selection and random drift?'. In: D. L. Hull and M. Ruse (eds.): *The Cambridge companion to the philosophy of biology*. Cambridge: Cambridge University Press, Chapt. 4, pp. 66–84.

Conway Morris, S.: 2009, 'The predictability of evolution: glimpses into a post-Darwinian world'. *Die Naturwissenschaften* **96**(11), 1313–37.

Darwin, C.: 1859, *On the origin of species*. London: John Murray.

Dasgupta, A.: forthcoming, 'Mathematical foundations of randomness'. In: P. S. Bandyopadhyay and M. Forster (eds.): *Handbook of philosophy of science: philosophy of statistics*. North-Holland.

Davies, P. C. W.: 2004, 'Does quantum mechanics play a non-trivial role in life?'. *Bio Systems* **78**(1-3), 69–79.

Desjardins, E. C.: 2011, 'Historicity and experimental evolution'. *Biology and Philosophy* **26**, 339–364.

Dietrich, M. R.: 2006, 'Three perspectives on neutrality and drift in molecular evolution'. *Philosophy of Science* **73**(5), 666–677.

Eagle, A.: 2011, 'Chance versus randomness'. In: E. N. Zalta (ed.): *The Stanford Encyclopedia of Philosophy (Spring 2011 Edition)*. URL: <http://plato.stanford.edu/archives/spr2011/entries/chance-randomness/>.

Glymour, B.: 2001, 'Selection, indeterminism, and evolutionary theory'. *Philosophy of Science* **68**(4), 518–535.

Graves, L., B. L. Horan, and A. Rosenberg: 1999, 'Is indeterminism the source of the statistical character of evolutionary theory?'. *Philosophy of Science* **66**(1), 140–157.

Hacking, I.: 1990, *The taming of chance*. Cambridge: Cambridge University Press.

Hull, D. L.: 1974, *Philosophy of biological science*. Prentice Hall.

Kolmogorov, A. N. and V. A. Uspenskii: 1987, 'Algorithms and randomness'. *Theory of Probability and its Applications* **32**(3), 389–412.

Krimbas, C. B.: 2004, 'On fitness'. *Biology and Philosophy* **19**(2), 185–203.

Malament, D. B.: 2008, 'Norton's slippery slope'. *Philosophy of Science* **75**(5), 799–816.

Martin-Löf, P.: 1966, 'The definition of random sequences'. *Information and Control* **9**, 602–619.

Martin-Löf, P.: 1969, 'Algorithms and randomness'. *Review of the International Statistical Institute* **37**(3), 265–272.

Matthen, M. and A. Ariew: 2002, 'Two ways of thinking about fitness and natural selection'. *Journal of Philosophy* **99**(2), 55–83.

McShea, D. W. and R. N. Brandon: 2010, *Biology's first law: the tendency for diversity and complexity to increase in evolutionary systems*. Chicago and London: University of Chicago Press.

Merlin, F.: 2010, 'Evolutionary chance mutation: a defense of the Modern Synthesis' consensus view'. *Philosophy and Theory in Biology* **2**, e103.

Mills, S. K. and J. H. Beatty: 1979, 'The propensity interpretation of fitness'. *Philosophy of Science* **46**(2), 263–286.

Millstein, R. L.: 2002, 'Are random drift and natural selection conceptually distinct?'. *Biology and Philosophy* **17**, 33–53.

Millstein, R. L.: 2008, 'Distinguishing drift and selection empirically: "The Great Snail Debate" of the 1950s'. *Journal of the History of Biology* **41**(2), 339–367.

Millstein, R. L., R. A. Skipper, and M. R. Dietrich: 2009, '(Mis)interpreting mathematical models: drift as a physical process'. *Philosophy and Theory in Biology* **1**, e002.

Norton, J. D.: 2003, 'Causation as folk science'. *Philosophers' Imprint* **3**(4).

Otsuka, J., T. Turner, C. Allen, and E. A. Lloyd: 2011, 'Why the causal view of fitness survives'. *Philosophy of Science* **78**(2), 209–224.

Pfeifer, J.: 2005, 'Why selection and drift might be distinct'. *Philosophy of Science* **72**(5), 1135–1145.

Popper, K.: 1959, 'The propensity interpretation of probability'. *British Journal for the Philosophy of Science* **10**(37), 25– 42.

Ramsey, G.: 2006, 'Block fitness'. *Studies in History and Philosophy of Biological and Biomedical Sciences* **37**(3), 484–98.

Richardson, R. C.: 2006, 'Chance and the patterns of drift: a natural experiment'. *Philosophy of Science* **73**, 642–654.

Rosenberg, A.: 2001, 'Discussion note: indeterminism, probability, and randomness in evolutionary theory'. *Philosophy of Science* **68**(4), 536–544.

Roughgarden, J.: 1979, *Theory of population genetics and evolutionary ecology: an introduction*. New York: Macmillan and Co.

Shapiro, L. and E. Sober: 2007, 'Epiphenomenalism – the do's and the don'ts'. In: G. Wolters and P. Machamer (eds.): *Studies in causality: historical and contemporary*. Pittsburgh, PA: University of Pittsburgh Press, pp. 235–264.

Sober, E.: 2010, 'Evolutionary theory and the reality of macro probabilities'. In: E. Eells and J. Fetzer (eds.): *Probability in science*. Springer, pp. 133–162.

Stamos, D. N.: 2001, 'Quantum indeterminism and evolutionary biology'. *Philosophy of Science* **68**(2), 164–184.

Stephens, C.: 2004, 'Selection, drift, and the "forces" of evolution'. *Philosophy of Science* **71**(4), 550–570.

Walsh, D. M.: 2007, 'The pomp of superfluous causes: the interpretation of evolutionary theory'. *Philosophy of Science* **74**(3), 281–303.

Walsh, D. M.: 2010, 'Not a sure thing: fitness, probability, and causation'. *Philosophy of Science* **77**(2), 147–171.

Walsh, D. M., T. Lewens, and A. Ariew: 2002, 'The trials of life: natural selection and random drift'. *Philosophy of Science* **69**(3), 429–446.

Werndl, C.: 2009, 'What are the new implications of chaos for unpredictability?'. *British Journal for the Philosophy of Science* **60**(1), 195–220.

Woodward, J. and C. Hitchcock: 2003, 'Explanatory generalizations, part I: A counterfactual account'. *Noûs* **37**(1), 1–24.