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Are Probabilities Necessary For Evolutionary Explanations?

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Abstract. Several philosophers of science have advanced an instrumentalist thesis about the use of probabilities in evolutionary biology. I investigate the consequences of instrumentalism on evolutionary explanations. I take issue with Barbara Horan's (1994) argument that probabilities are unnecessary to explain evolutionary change given the underlying deterministic character of evolutionary processes. First, I question Horan's deterministic assumption. Then, I attempt to undermine her Laplacian argument by demonstrating that whether probabilities are necessary depends upon the sort of questions one is asking.

Key words: evolutionary theory, LaPlacian determinism, parsimony, probabilistic explanations, statistics

Introduction

Concerning the role that probabilities play in evolutionary theory, an enticing "instrumentalist" thesis has been gaining popularity among prominent philosophers of science, including Barbara Horan (1994) and Alexander Rosenberg (1994). The instrumentalist asserts that while use of probabilities in evolutionary theory provides an investigator with a heuristic or means of investigating evolutionary forces, the probabilities that investigators invoke in their theories do not reflect evolutionary processes in the real-world. In other words, the instrumentalist believes that probabilistic characterizations reflect the computational and cognitive limitations of the investigator; it is misleading to think that the process of evolution is stochastic (Rosenberg, p. 57).

The question I wish to raise is this: what consequences does the instrumentalist thesis have for evolutionary *explanations*? Indeed the most accepted

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account of fitness among philosophers characterizes fitness in terms of probabilistic propensities (Mills and Beatty 1979), and genetic drift is often characterized as a stochastic process in evolution (Crow and Kimura 1970).

According to Barbara Horan, probabilities are "*theoretically unnecessary* given the underlying deterministic character of evolutionary processes" (1994, p. 78). The gist of Horan's argument is as follows: probabilities reflect either real indeterminacies in the world or human ignorance. Since evolutionary processes are fundamentally deterministic, probabilities reflect only human ignorance. Theoretically speaking, a Laplacian supercalculator in possession of a complete state description and knowledge of the laws could predict and explain evolutionary processes without the use of probabilistic statements. The task of this paper is to critically assess Horan's argument. In my view, whether probabilities are necessary in evolutionary explanations depends on the sort of questions evolutionists are investigating, the question of Laplacian determinism notwithstanding.

A question about subject

Right off, there are questions about Horan's targeted subject. First, Horan uses the term "statistics" rather than the term "probabilities" to describe the character of evolutionary biology. According to Horan, "a theory is a statistical theory if it employs terms referring to statistical properties" (p. 79). But this won't do. Horan fails to distinguish the characteristics of the data from the characteristics of the theory. In my view, evolutionary theory is probabilistic (not statistical) because it uses probabilities in concepts like fitness, drift, etc. The term "statistics" is used to characterize our observations that take the form of frequencies, averages, means, modes, medians, standard deviations, or variances. Statistics provide the data against which a theory might be tested. For example, a statistic might be the *de facto* frequency of blue tail-feathers in a population. The theory of evolution does not mention statistics such that blue tail-feathers have a frequency of 63% at a certain place and time. In light of the data/theory distinction, I view the issue for Horan to pertain to the *probabilistic* character, not the statistical character of evolutionary theory. Hereafter I will employ the relevant terminology accordingly.

Second, Horan takes her instrumentalist thesis to be contrary to what she calls "the received view" that evolutionary theory *is* a probabilistic theory. Surely, Horan is wrong to assert that the theory as it is actually written down and understood by scientists is not probabilistic; a quick check of the journals and textbooks suffices to debunk this claim. Appeals to Laplacian supercalculators are beside the point on the issue of what the theory of evolution *is*. I suspect that Horan has in mind a separate claim – that evolutionary theory *ought not* be formulated so that the reference to probabilities is eliminated. In this case the Laplacian supercalculator is an ideal theorist, one that all of us lesser intellects might aspire to approximate. According to this interpretation of Horan's argument, proponents of the received view are supposed to affirm that evolutionary theory ought to be formulated probabilistically. If that is a correct reading of Horan, then my reply is that many of the philosophers Horan cites as advocates of the received view may not necessarily be raising the same normative question. Their focus has been on what evolutionary theory *is* (a notoriously difficult question to answer), not on how it ought to be so formulated.

Determinism and the argument from necessity

In what follows I focus on two of Horan's central claims: (a) evolution is a deterministic process; (b) If evolution is a deterministic process, then from a Laplacian point of view the use of probabilities to explain evolutionary processes is unnecessary. I will comment on each in turn.

Horan provides a lengthy attack on the idea that the forces of evolutionary change, mainly selection and genetic drift, are indeterministic. However, Horan's position rests on the idea that the macro-sized objects upon which evolutionary processes operate are fundamentally deterministic. But why does Horan believe in macro-determinism? I find no argument in her lengthy discussion, although Horan seems to accept the common belief that indeterminism is a brute fact about the quantum-world (p. 81). To some, micro-determinism suggests (but does not necessarily prove) that indeterminism is also a brute fact about the macro-world. If macro-sized ensembles are ensembles of micro-particles, and these micro-objects behave indeterministically, then perhaps indeterminacy exists at the macro-level as well. On this view, even if indeterminacies existing at the quantum-level infrequently affect our predictions of the behavior of macro-sized systems, it still remains a brute-fact that indeterminacies exist at the macro-level.

To illustrate the point,[1] consider the following analogy: suppose the roulette wheels, poker tables, and crap games found in a gambling casino were indeterministic systems. Every play at the roulette wheel has an indeterministic outcome; where the ball ends up is random. Yet, at the end of a year, there is *almost* no chance that the casino will fail to show a profit. Now, because each game operates indeterministically, there is a real yet remarkably tiny chance that at the end of the year the casino actually loses money. The point is that the indeterminacies operating at the individual game level do percolate up to figure in the casino's chance at making a profit, although the indeterminancies are so small.

Getting back to physics, it is important to note that if *enough* quantum-level entities behaved atypically, a macro-sized object composed of the quantum-level particles would also. If this view of the macro-world is correct, the use of probabilities in evolutionary theory is necessary, even to the Laplacian, to reflect the physically indeterministic behavior of all macro-sized objects. In her paper, Horan gives us no reason to disbelieve this metaphysical view of the macro world.[2]

Turning now to (b): according to Horan, assuming determinism, probabilistic characterizations are unnecessary in an explanation of the behavior of an ensemble because "a Laplacian supercalculator in possession of a complete state description and knowledge of the laws could predict and explain the behavior of the ensemble in terms of the . . . behavior of its *individual* members" (p. 81, my emphasis). The key point here is that, on Horan's view, probabilities describe properties of populations *and not of individuals*: ". . . statistics is basically sophisticated counting . . . Statistical properties are therefore *numerical* properties. Statistical properties are also *abstract* properties. The average income of a family of four is not a property that is possessed by any family in the way that a particular income is possessed by the Jones family. To ask which family possesses the average income is to make a category mistake of the classic sort" (Horan: p. 80). Accordingly, Horan distinguishes between the *macro-level* explanations provided by a probabilistic theory and the *micro-level* explanation of the Laplacian: while a probabilistic explanation describes the population as a whole, Laplacian micro-level explanations address the behavior of the population's individual members.

Two remarks are worth making here. First, Horan's point glosses over the distinction between what we may call "actual frequencies" and "probabilities". Actual frequencies are what Horan calls "probabilistic properties", properties assigned to ensembles. But actual frequencies do not exhaust the possible interpretations of probability. Consider "single-case" probabilities that may be assigned not to ensembles but to token objects or events themselves. On this interpretation, to say that a coin has a 50% chance of landing heads is to assign a probability to the coin itself. The single-case probability assigned here is not a property of a series of coin tosses. If I'm right, Horan is wrong to imply that probabilistic properties apply only to populations. Probabilities also appear in micro-level explanations in which the behavior of each individual is explained in terms of the probability that it will act in a certain way. This is an important point against Horan, for it undermines her distinction between micro and macro-explanations, whereby micro-explanations ranging over each individual separately are supposedly (necessarily?) non-probabilistic. The point is that under some interpretations, probabilities may be assigned to individual events as well.

Second, consider the force of Horan's claim in (b). Horan is not arguing that probabilistic theories cannot be cited to explain the behavior of an ensemble; she admits they can (p. 79). Rather, she concludes that probabilities are *unnecessary* in evolutionary theory. But Horan is vulnerable to this obvious reply: since probabilistic theories also explain the behavior of an ensemble in a deterministic world, it is the Laplacian micro-level explanation that isn't always "necessary". The point is that, by itself, an argument from necessity lacks force. Horan needs to demonstrate why we ought to prefer Laplacian explanations to probabilistic ones.

Horan offers *one* positive argument in favor of non-probabilistic explanations which, while having nothing to do with Laplacian supercalculators, concerns what she takes to be a distinction between physics and biology. According to Horan's "argument from diversity", physicists describe the behavior of *identical* kinds of entities, e.g., molecules of a gas, atoms, elementary particles, in terms of *identical* kinds of properties, e.g., mass, velocity, and energy level. On the other hand, evolutionary change requires biological *diversity*: "the sort of identity assumptions that physicists like to make about physical particles would contravene the fundamental precept of evolutionary theory" (p. 82). This difference in the domains of the two disciplines suggests to Horan a reason why probabilistic methods ought to be desirable to the physicist and undesirable to the biologist. Because physics applies to a homogenous domain, probabilistic methods are appropriate. But since biology's domain is heterogeneous, Horan suggests, "one may at least raise the question whether the same approach will succeed in biology, given the variability of the phenomena biologists study and the resulting heterogeneity of the biological domain" (p. 82). Horan's view is sketchy and she promises further discussion in a forthcoming article. But clarification is required, as this is the only positive argument in favor of non-probabilistic explanation that Horan supplies.

As is, Horan's argument from diversity is flawed. In statistical mechanics, particles *differ* in their positions, momenta, etc. That is, contrary to what Horan says, the ensemble's constituents are *diverse* vis-à-vis the properties used to explain their behavior. From the statistician's point of view, the ensemble exhibits a probability distribution of those traits that determines its transition through state-space. Here, the trajectory of the ensemble is understood as the upshot of the differing properties of each particle in the ensemble. So while diversity exists in the ensemble, statistical mechanics provides a way for us to gloss over the diversity of parts to say something more determinate about the characteristic of the whole. How is this different from a probabilistic analysis of the selection process affecting a population of organisms? Whether we are using population genetic models or phenotypic models (as in evolutionary game theory) the result is the same: organisms differ in their fitness capacities, and the ensemble exhibits a distribution of fitnesses that determines its evolution. In short, I don't see a relevant difference in the domains of physics and evolutionary theory. Consider as a case in point: the motivation for RA Fisher's fundamental theorem is to align a theory of evolution with the 2nd law of thermodynamics (Fisher 1930, p. 38).

Why biologists should sometimes prefer probabilistic explanations

In the remainder of her paper, Horan criticizes possible reasons why biologists may prefer probabilistic explanations. First, I explain why I think biologists should prefer probabilistic explanations; then I'll defend my view against Horan's criticisms.

In my view, whether probabilities are necessary in evolutionary biology depends upon the *explanandum*. The possibility of Laplacian micro-level explanations is not reason enough to eschew probabilistic explanations in all cases. A micro-level *explanans* may miss important biological generalizations or patterns exhibited in the behavior of an ensemble that an investigator may be interested in explaining. Models of natural selection describe how a population changes in response to the variation in fitness it contains (Sober 1993, p. 76). This generalization subsumes evolution in a wide variety of cases: from the saguaros of the Sonoran desert to the turtles of the Galapagos to the Drosophila of Maynard Smith's lab. For each population we hope to discover that one variant has a higher expected survivability and/or expected reproductive success rate so that we can explain the changes in gene frequency we observe in terms of one variant-type having higher fitness than its conspecifics. In contrast, a Laplacian micro-level explanation consists in individualistic explanations of each trait type's physical effect on actual survivability and/or reproductive success for each population separately. Granted, compared to an explanation that assigns differing probabilities to various trait types, the physical details provide a deeper explanation of the *single evolutionary events*, e.g., the changes in gene frequency for Sonoran saguaros. But suppose what needs explaining is not the single evolutionary event but general patterns that subsume evolutionary events for all the populations under consideration: saguaros, turtles, Drosophila. Since these populations differ in countless physical ways the Laplacian would have to tell a different story about the evolution of each population (Sober, p. 77). If general patterns are part of the *explanandum* the preferred *explanans* describes properties shared by the various populations in probabilistic terms. In other words, probabilities are indispensible for certain types of explanations.

Horan thinks that proponents of the received view, those who believe evolutionary theory is or ought to be probabilistic, are implicitly motivated by one of the following: (i) “a precipitous application of Occam’s razor”, (ii) a circular argument, (iii) an appeal to population genetics, a discipline that, according to Horan, faces “insurmountable limitations”.

On (i), Horan points out that micro-level explanations of each individual member in an ensemble would differ from each other, as individuals differ in their activities. In the face of these individual differences, Horan charges, a probabilistic explanation of the same ensemble “requires that we neglect these differences in order to explain more with less”. This is, according to Horan, “a precipitous application of Occam’s razor”.

Now, parsimony arguments are typically invoked when the aim is to choose an “objectively better” theory, i.e., one that is more plausible, or closer to the truth. The view is that (for any number of reasons) the simpler theory – one that explains more with less – is the preferable one. But the issue before us is not which explanation, the micro-level or the probabilistic, is correct; both may be true of the same phenomenon. Rather, the issue is which explanation *better satisfies our inquiry*. As I said before, if one is concerned to understand the physical differences between individual Drosophila and turtles, then a micro-level explanation may be appropriate. However, if one is concerned to explain the relation of selection exhibited both by populations of Drosophila and populations of turtles, then macro-level probabilistic explanations are appropriate. Here’s the crucial distinction Horan neglects: to say that simpler hypotheses are more likely to be true is to apply Occam’s razor; but to say (as I do) that probabilistic explanations sometimes satisfy our interests better is *not to invoke a parsimony argument at all.*

In regard to (ii), population geneticists are interested in biological generalizations, hence many of their explanations are probabilistic. Horan replies: “To advance [this] as a reason for adopting a population genetics formulation of evolutionary theory is clearly circular since the generalizations are none other than the equations of population genetics itself” (p. 92). In my view, explanations ultimately answer why-questions. Given a well-formed why-question, explanations provide us an answer. I take it that population geneticists ask lots of why-questions concerning the biological patterns they observe just as physicists ask questions about micro-particles. (And, population geneticists are not the only ones who ask questions about biological patterns. That is, population genetics is not the whole of evolutionary theory.) Unless one’s why-question is poorly formed, I see no reason to suggest that a population geneticist’s – or any other evolutionary theorist’s – why-question does not warrant an answer, just as Horan sees no reason to suggest that a physicist’s why-question does not warrant an answer.

Finally, according to Horan, population geneticists cannot provide a causal explanation of evolution because, briefly, it is "difficult" to detect the actual causal factors that explain population transformations over time. For, "the evolutionary pathways for all but the simplest traits are very complex" (p. 92).[3] However, the difficulties Horan cites are products of human ignorance and not in-principle difficulties from a Laplacian point of view. For example, she cites difficulties in inferring the initial conditions of biological processes. Obviously, these inferences are fallible, but necessary, given *human ignorance*. Remember, Horan's thesis concerns the sort of explanations that are theoretically possible from a Laplacian point of view. So, to say that population geneticists face difficult practical problems is beside the point. I take it that the complexities of the real world that stump ignorant humans pose no problem for the population geneticist's supercalculator. If Horan's theoretical argument is to work here, she must adopt the standard of the Laplacian supercalculator's abilities and not advert to the *de facto* limitations of mortal population geneticists.

Conclusion

Despite Horan's presentation, I see no reason why proponents of the received view should change their minds about the probabilistic character of evolutionary theory. Most obviously, evolutionary theory as written is undeniably a probabilistic theory. And, as far as evolutionary explanations go, I see no reason why one *ought always* to adopt Laplacian micro-level explanations over probabilistic explanations ranging over the ensemble as a whole (supposing for the sake of argument that such micro-explanations in fact exist). First, Horan fails to address the possibility that biological evolution is fundamentally an *in*deterministic process; second, Horan fails to appreciate that one's explanatory preferences ought to depend on the sort of questions one wants to answer.

Notes

[1] I owe the example to Elliott Sober (pers. com.).
[2] It is worth noting that Rosenberg (1994) makes no attempt to address this possibility either.
[3] Horan uses Lewontin (1985) to support her claims.

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