Two Outbreaks of Lawlessness in Recent Philosophy of Biology

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John Beatty (1995) and Alexander Rosenberg (1994) have argued against the claim that there are laws in biology. Beatty's main reason is that evolution is a process full of contingency, but he also takes the existence of relative significance controversies in biology and the popularity of pluralistic approaches to a variety of evolutionary questions to be evidence for biology's lawlessness. Rosenberg's main argument appeals to the idea that biological properties supervene on large numbers of physical properties, but he also develops case studies of biological controversies to defend his thesis that biology is best understood as an instrumental discipline. The present paper assesses their arguments.

1. Introduction. Are there laws in biology? John Beatty (1995) says there are none and Alexander Rosenberg (1994) says there is just one. Have they got their numbers wrong? That's a question I will want to address. However, my first concern is the arguments they give. Do the considerations they adduce support the lawlessness they advocate?

Beatty and Rosenberg rely on a standard logical empiricist conception of law. Laws are true generalizations that are "purely qualitative," meaning that they do not refer to any place, time, or individual. They have counterfactual force. And finally, Beatty and Rosenberg require that laws be empirical. My main disagreement with this traditional picture is that I want to leave open whether a law is empirical or *a priori*. I have argued elsewhere that the process of evolution is governed by models that can be known to be true *a priori* (Sober 1984, 1993).

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For example, Fisher's (1930) fundamental theorem of natural selection says that the rate of increase in fitness in a population at a time equals the additive genetic variance in fitness at that time. When appropriately spelled out, it turns out to be a mathematical truth—in populations of a certain sort, fitness increases at the rate that Fisher identified. Fisher's theorem governs the trajectories of populations just as Newton's laws govern the trajectories of particles. Fisher's theorem, and statements like it, are purely qualitative, support counterfactuals, and describe causal and explanatory relations. Because evolutionary processes are governed by such propositions, I want to say that evolution is lawful. How we are able to *know* the laws of evolution is a separate question. Whether a natural process is lawful is not an epistemological issue (Dretske 1977).

This revised notion of law does not entail that every *a priori* statement is a law. The concept of a *process law* allows us to avoid this result. A process law is a counterfactual-supporting, qualitative generalization, which describes how systems of specified type develop through time. Typically, such laws are time-translationally invariant (Sober 1994); given a system that occupies a particular state at one time, a process law describes the probability distribution of the different states the system may occupy some fixed amount of time later; the date of the starting time is irrelevant. "Bachelors are unmarried," is not a process law, but not because it is *a priori.*

When I claim that there are laws of evolution, and Beatty and Rosenberg demur, there is no disagreement. My use of the term "law" leaves open whether a law is empirical; Beatty's and Rosenberg's does not. Beatty and I agree on the bottom line—there are no *empirical* laws of evolution; Rosenberg and I also agree, save for the one exception he has in mind. So what is there to argue about? It is their *reasons* for denying the existence of empirical laws. This is the bone I want to pick.

2. The Evolutionary Contingency Thesis. Beatty (pp. 46–47) articulates his claim about lawlessness by describing what he calls *the evolutionary contingency thesis* (the ECT): "All generalizations about the living world are just mathematical, physical, or chemical generalizations (or deductive consequences of mathematical, physical, or chemical generalizations plus initial conditions), or are distinctively biological, in which case they describe contingent outcomes of evolution." Beatty agrees that organisms obey the laws of physics. However, there is no additional layer of autonomous biological law that living things also obey.

Beatty illustrates what he means by the evolutionary origins of biological regularities by discussing several examples. One of them is Mendel's first "law," which says that diploid sexual organisms form haploid gametes by a "fair" 50/50 meiotic division. Beatty cites two reasons for thinking that this is not a law. First, it is sometimes false; genes that cause segregation distortion are counterexamples. And more importantly, fair meiosis, when it exists, is a contingent outcome of evolutionary processes; another set of initial conditions would have produced a different segregation ratio.

Below is a schematic version of the ECT. A set (I) of contingent initial conditions obtains at one time (t_0); this causes a generalization to hold true during some later temporal period (from t_1 to t_2):

$$\frac{\text{I} \quad \rightarrow \quad [\text{if P then Q}]}{t_o \qquad\qquad\qquad t_1 \quad t_2}$$

Since the generalization is true only because I obtained, we may conclude that the generalization is contingent. However, there is *another* generalization that this scenario suggests, and it is far from clear that *this* generalization is contingent. This generalization will have the following logical form:

(L) If I obtains at one time, then the generalization [if P then Q] will hold thereafter.

The fact that the generalization [if P then Q] is contingent on I does not show that proposition (L) is contingent on anything. This point also holds if (L) is given a probabilistic formulation.

Is proposition (L) contingent? Recall that the ECT is a claim about *causality*; as applied to Mendel's first law, it claims that the evolutionary process *caused* segregation ratios to take the values they did. If causality requires the existence of laws, then there *must* be laws in the background—the evolutionary contingency of [if P then Q] *demands* the existence of laws. Anscombe (1975) argues that causal claims about token events do not entail the existence of general laws. Her point concerns the meaning of the word "cause," and she may be right. Still, it is part of the practice of science to expect noncontingent generalizations when one event causes another, and her observations do nothing to discredit this expectation.

If (L) is noncontingent, is it "distinctively biological?" In one sense, it is. The generalization that helps explain a given segregation ratio describes the variation found in ancestral populations, the fitnesses that attached to those variants, the background biology present in the population, etc. The generalization is biological because of its distinctive vocabulary. However, there is another way to interpret "distinctively biological," which has the opposite result. If a distinctively biological proposition cannot be *a priori*, then (L) is not distinctively biological if it is a mathematical truth. On this interpretation, the fact that (L) is not contingent is no threat to the ECT. This proposal has the curious result that biologists are not doing biology when they construct mathematical models of biological processes; rather, they are doing mathematics. There probably is no point in disputing how the phrase "distinctively biological" should be understood. The idea I want to emphasize is that the contingency of Mendel's "laws" on a set of prior evolutionary events should lead us to expect that there are *other* general propositions that are *not* contingent on that set.

3. Relative Significance Controversies and Theoretical Pluralism. Beatty has two further arguments for the ECT. The fact that biologists engage in "relative significance controversies" and find attractive "the explanatory ideals manifested in 'theoretical pluralism'" are said to "support" the ECT (p. 76). The question may be asked as to what the behavior of *scientists* shows about the existence of *laws*. Beatty's idea seems to be that pluralism and an interest in relative significance controversies are responses to lawlessness; biologists comport themselves as they do because they see that there are no laws. To defend this argument, Beatty must show, not just that biologists act this way, but that this fails to be true in sciences where laws are thought to exist.

Carrier (1995, 88) puts his finger on what is wrong with this claim when he considers the (derived) law of free fall in physics; this says that a body near the earth's surface falls with constant acceleration, provided that no force other than gravity acts upon it. Carrier points out that "every parachutist constitutes an exception" to this law, not because parachutists show that the statement is false, but because they violate the condition specified in the law's antecedent. A bowling ball and a feather exhibit different trajectories when released above the earth's surface because air resistance is an important influence on one, but not on the other. Physicists and biologists both investigate which forces are significant influences on what happens (Sober 1996). And when it comes to feathers, physics teaches us to be pluralists—to see both gravity and air resistance as important influences on the resulting trajectory. When scientists entertain questions about relative significance, and when they claim that a phenomenon has a plurality of causes, this does not show that their subject lacks laws.

This point becomes clearer when one examines what relative significance controversies in biology are about. One example that Beatty mentions—neutrality versus selection as a theory of molecular evolution—is representative. The issue here is whether $Ns << 1$ (Kimura 1983). If the product of the effective population size and the selection coefficient attaching to a gene is much less than unity, the gene is said to be "effectively neutral." This question concerns the contingent values that parameters happen to have. The problem is not which general model is true. Judged as a set of if/then statements, Kimura's model of neutral evolution is not in dispute. The model's truth does not depend on any evolutionary contingency.

Beatty thinks that theoretical pluralism in biology is strongly at odds with what he calls "the Newtonian tradition" (p. 68), whose guiding ideas are summarized in Newton's rules of reasoning in philosophy. Newtonians believe the maxim "to the same natural effects we must, as far as possible, assign the same causes." Pluralists, on the other hand, maintain that effects frequently have many causes. My reaction to this point is that pluralists can be good Newtonians and that this contrast does not represent a methodological rift between biology and physics. In both sciences, a defeasible preference for monism is perfectly compatible with a *de facto* embracing of pluralism. Newton said we should, *as far as possible*, prefer more monistic theories over more pluralistic ones. Pluralism in biology involves no rejection of this advice. We prefer monistic theories unless the data force us to embrace pluralism. But if the data *do* have this character, we *should* be pluralists (Forster and Sober 1994, Sober 1996).

One example of Newtonianism in biology may be found in the use of parsimony as a criterion in phylogenetic inference (Sober 1988). Why do the mammalian species we presently observe have hair? It is conceivable that hair evolved independently in every extant species, but this would be dreadfully unparsimonious. It is far more plausible to see the trait as a homology—an inheritance from a common ancestor (Nelson and Platnick 1981, 39). This does not mean that all similarities must be explained in this way. Rather, we should try to interpret similarities as homologies *as far as possible*. When we cannot, we embrace the hypothesis that some traits originated more than once. It is a mistake to think that parsimony is relevant to the search for laws, whereas pluralism is appropriate when one inquires into the character of historical particulars. In both types of science, parsimony is desirable, but defeasible.

4. Supervenience. Rosenberg's brief for lawlessness rests on an entirely different set of arguments than Beatty's. Rosenberg (1994) uses the idea of supervenience to argue that, with one exception, there are no laws in biology. The one genuine law is what Rosenberg calls "the theory of natural selection," by which he means Mary Williams' (1970) axiomatization. Rosenberg represents this axiomatization as saying that (i) there is an upper bound on the number of organisms in a generation,

(ii) each organism has a fitness value, (iii) fitter traits increase in frequency and less fit traits decline, and (iv) populations show variation in fitness unless they are on the brink of extinction (p. 106).

I want to raise two questions about this axiomatization. Proposition (iv) is probably true, but I do not see why the existence of variation in fitness should be regarded as a law. Statement (iii) is false if fitness means expected number of offspring; and if fitness means actual number of offspring, it also is false, since traits with higher reproductive outputs can fail to increase if they are not heritable or if there is a counterbalancing mutation or migration pressure. Williams and Rosenberg do not spell out what they mean by "fitness" because they think that philosophical problems (e.g., what to do about the claim that the theory of evolution is tautologous) can be solved by regarding "fitness" as an undefined primitive. However, it needs to be said that interpretive problems about the fitness concept are not solved by refusing to say what the term means. If the term is primitive in an axiomatization, then it is not a defined term *in that system*; this does not remove the need to clarify what the term means in the mouths of biologists (Mills and Beatty 1979).

Anyway, Rosenberg's argument about the rest of biology is the main subject I want to discuss. Rosenberg argues that the supervenience of biology on physics shows that there are no biological laws (aside from the law he thinks is captured in Williams' axiomatization), or that we will never be able to discover any laws, should they exist. Consider the accompanying Figure, adapted from Fodor 1975. Suppose P and Q are predicates in a higher-level science, such as biology or psychology; P supervenes on properties $A_1, A_2, \ldots, A_n$, while Q supervenes on properties $B_1, B_2, \ldots, B_n$. The A and B properties are studied in some lower-level science, physics perhaps. Roughly speaking, supervenience means determination; if one of the A_i's is present, then so is P, and if

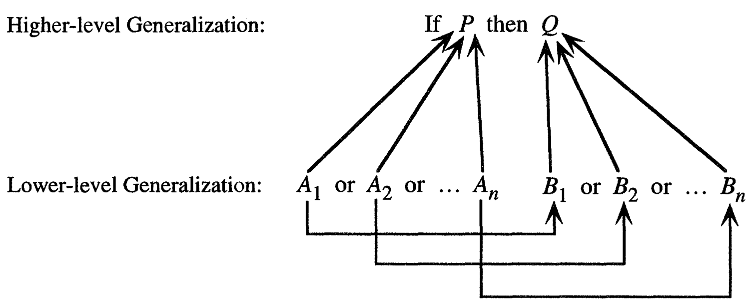

Figure 1.

one of the B_i's is present, so is Q. P and Q are said to be "multiply realizable," two objects may both have P and still be different from the point of view of the lower-level theory, in that one has A_i while the other has A_j $(i \neq j)$. The higher-level predicate describes what these objects have in common, something the lower level-theory cannot do.

This diagram suggests an argument for the lawfulness of [if P then Q]. If each A_i necessitates its counterpart B_i, and if P entails that one of the A_i's must be present, then P necessitates Q. Higher-level generalizations are laws in virtue of the lawfulness of the lower-level generalizations on which they supervene. This does not show that we will be able to discover that [if P then Q] is true and lawful. Rather, the argument suggests that the law exists. My point in describing this argument is not to endorse it, but to raise the question of how Rosenberg manages to use supervenience in defense of biological lawlessness. Rosenberg thinks that chemistry supervenes on physics, but that chemical laws exist and can be discovered. Why does he think that biology is different?

Rosenberg's answer is that the process of natural selection has made the world especially complicated. There are many, many physical structures that perform the same function. Since natural selection selects for traits that perform a given function, and is indifferent as to which structure evolves to do the job, we should expect an immense proliferation of supervenience bases in biology. Selection has led prey organisms to be able to evade their predators; however, the physical properties that permit prey organisms to do this are enormously varied. The evolutionary process has made life so complicated that biology will never be able to arrive at laws. As a result, biology is and will remain an "instrumental" discipline.

One gap in Rosenberg's argument is that he does not tell us *how* complicated the living world is, or *how* complicated it has to be to elude our search for laws. I am not asking for a precise measure of complexity, but for a reason to think that the complexity of nature puts biological laws beyond our ken. Consider, for example, what we know about fitness. Fitness is the supervenient biological property *par excellens*. What do a fit zebra, a fit dandelion, and a fit bacterium have in common? Presumably, nothing much at the level of their physical properties. However, this has not prevented evolutionists from theorizing about fitness. I have already mentioned Fisher's theorem and there are lots of other lawful generalizations that describe the sources and consequences of fitness differences (Sober 1984). It might be objected that these generalizations are *a priori*, and so are not laws, properly speaking. This raises the question of whether laws must be empirical, but let us put that issue aside. If the multiple realizability of a property makes it "complicated," then fitness is complicated. And if the complexity of a property makes it impossible for us to discover qualitative, counterfactual supporting, and explanatory generalizations about the property, then we should have none available about fitness. But we do, as Rosenberg concedes. The human mind does not slam shut in the face of radical multiple realizability. Understanding the sources and consequences of fitness differences is not rendered impossible by the fact that fitness is multiply realizable. It is therefore puzzling why the multiple realizability of other biological properties should mean that we will never know any laws about them.

The above diagram suggests a diagnosis of why Rosenberg thinks that multiple realizability makes supervenient laws unknowable. Perhaps Rosenberg assumes that a supervenient law can be known only by knowing the laws on which it supervenes. If there are 10,000 lower-level generalizations of the form [if A_i then B_i], then there is a lot to know, perhaps more than our frail minds can absorb. However, this argument involves a misinterpretation of the diagram. The diagram does not depict what one must do to discover that [if P then Q] is true and lawful. Rather, it represents the metaphysics of how higher-level and lower-level generalizations might be related. It seems to me that higher-level facts can be known without exhaustively examining their lower-level bases. If so, Rosenberg's "argument from supervenience" fails.

5. Three Biological Examples. Rosenberg has another argument for lawlessness in biology. He examines three biological areas and in each case defends an instrumentalist interpretation. The areas are classical genetics, the theory of neutral evolution, and the units of selection problem. It turns out that Rosenberg uses the term "instrumentalism" ambiguously. In discussing classical genetics, he claims that Mendel's "laws" are *false*, and so are not laws at all. However, Rosenberg does not similarly argue that Kimura's theory of neutral evolution is false. Rather, he claims that the theory's use of probability concepts reflects its *observer-relativity*. The reason that probability is used to describe drift is not that this process is objectively chancy; rather, we talk of chance only because we are ignorant of physical details.

Rosenberg's observer-relativity argument for instrumentalism confuses semantics and pragmatics. What a statement means should not be confused with how and why it is used. Perhaps we use a probability statement to make a prediction only because we are ignorant of finer-grained details. However, this does not mean that the statement is observer-relative in what it says. Consider, for example, the interpretation of probability that equates probability with actual frequency.

The actual frequency of an event in a containing population is not observer-dependent, whatever our reasons may be for using such probabilities to make predictions. This undercuts Rosenberg's argument for an instrumental interpretation of the theory of neutral evolution.

Rosenberg's last example concerns the units of selection controversy. He uses a strengthened version of an idea about causality that Sober and Lewontin (1982) defended. This is the idea that C is a positive causal factor for bringing about E precisely when C raises the probability of E in at least one background context, and does not lower it in any. For example, smoking is said to be a positive causal factor for lung cancer, if smoking increases some people's chances of getting cancer and does not lower anyone else's. Lewontin and I intended the range of background contexts to be the ones that are actually exemplified in the population. However, Rosenberg expands this set to include background contexts that are merely conceivable. It is no surprise that causal claims that seem to be true turn out to be false under his strengthened criterion. Just imagine a science fiction circumstance in which smoking actually reduces the chance of lung cancer, e.g., by causing physicians to supply a preventative drug.

This leads Rosenberg to conclude that organisms and groups are never units of selection, but that "properties of the genetic material required for gene expression and replication stand a chance of satisfying [the criterion for being a unit of selection]" (p. 99). Rosenberg then recognizes that biologists do not in fact impose the stringent criterion he describes. Rather, they evaluate claims about units of selection by "identifying the particular factors of the local environment that make the trait conducive to survival of the organism and its reproduction" (p. 101). Rosenberg concludes that scientists adopt a weakened criterion because it suits their instrumental goals and finite cognitive abilities. However, another diagnosis is possible. If a strong criterion never judges organisms or groups to be units of selection, perhaps this is because the criterion is wrong. Alternative conceptions of the units of selection problem exist (Sober and Wilson 1994); rarely do they have the effect of making the subject conform to Rosenberg's picture of instrumental biology.

6. Conclusion. The supervenience of biological properties—even the radical level of multiple realizability wrought by natural selection—does not show that biology is lawless or that laws cannot be known. Moreover, when biologists engage in relative significance controversies and sometimes embrace theoretical pluralism, this is not evidence that biology lacks laws. And the fact that the biological generalizations that hold at one time trace back to earlier evolutionary contingencies does not show that there are no laws of evolution. These negative remarks hold true, regardless of whether one adopts the logical empiricist notion of law or the modified idea of process law that I have suggested.

Still, the oddity remains that when one tries to state an evolutionary law precisely, the result seems always to be an *a priori* model in mathematical biology. Why has biology developed in this way, whereas physical processes seem to obey laws that are empirical? Beatty and Rosenberg try to explain this peculiar state of affairs by describing properties of the evolutionary process. Perhaps it is time to investigate the possibility that biology has no empirical laws of evolution because of the strategies of model building that biologists have adopted.

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