Pragmatic Laws

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Beatty, Brandon, and Sober agree that biological generalizations, when contingent, do not qualify as laws. Their conclusion follows from a normative definition of law inherited from the Logical Empiricists. I suggest two additional approaches: paradigmatic and pragmatic. Only the pragmatic represents varying kinds and degrees of contingency and exposes the multiple relationships found among scientific generalizations. It emphasizes the function of laws in grounding expectation and promotes the evaluation of generalizations along continua of ontological and representational parameters. Stability of conditions and strength of determination in nature govern projectibility. Accuracy, ontological level, simplicity, and manageability provide additional measures of usefulness.

1. Introduction. In a recent paper, Beatty (1995) argued for what he calls the *evolutionary contingency thesis* (*ECT*). This is the claim that generalizations about the living world are either just mathematical, physical, or chemical laws, or are distinctively biological in that they describe contingent outcomes of evolution. Beatty takes this to imply that there are no genuine biological laws because "whatever 'laws' are, they are supposed to be more than just contingently true" (p. 46). Sober (1997) and Brandon (1997) endorse the conclusion that insofar as the generalizations of biology are contingent, they fail to be laws, and Beatty (1997) explores further support for *ECT* from scientific disputes. I agree with the substantive claims of Beatty and Brandon concerning the use of biological generalizations in scientific practice and find no logical error in Sober's formal representation of these generalizations. Nevertheless, I will argue that these papers, like most discussions of biology's failure to produce genuine scientific laws, are limited by their shared normative approach to the question. After demonstrating these

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limitations, I will sketch an alternative strategy—a pragmatic approach to laws—and indicate how it provides a more adequate representational framework in which to characterize two important features of scientific practice:

- the variability of types of generalization within the empirical sciences, and
- the *nature* and *degree* of contingency characteristic of biological generalizations.

My paper is an attempt to direct the entire discussion away from the question of what we should call a law towards an understanding of how scientific generalizations of various types function in inferences to satisfy the pragmatic goals of science.

How do we decide whether or not biology has laws? There are three strategies for pursuing this question: a normative, a paradigmatic, and a pragmatic approach. The normative approach is the most familiar. To proceed, one begins with a norm or *definition* of lawfulness and then each candidate generalization in biology is reviewed to see if the specified conditions are met. If yes, then there are laws in biology, if no, then there are not laws in biology. The paradigmatic approach begins with a set of *exemplars* of laws (characteristically in physics) and compares these to the generalizations of biology. Again, if a match is found, then biology is deemed lawful. The pragmatic approach focuses on the *role* of laws in science, and queries biological generalizations to see whether and to what degree they function in that role.

2. The Normative Strategy. Beatty (1997), Sober (1997), and Brandon (1997) all acknowledge the legacy of logical empiricism in their questioning the existence of biological laws. In that tradition, laws were initially characterized syntactically as universal generalizations in first order predicate calculus. The problem of ruling out the merely accidentally true generalizations which share this form forced attempts to further restrict the definition. These intuitions are familiar. That the diameter of a sphere of enriched uranium never exceeds 100 meters instantiates a law of nature. That the diameter of a sphere of gold never exceeds the same length, is true, but accidentally so. What is required to draw this distinction is a way to isolate the necessitation of the consequent condition upon the action of the antecedent in order to cover only those generalizations that *could* never, not just *had* never, failed to be true. This feature—what has been called natural necessity—explains why laws, and not accidentally true generalizations, support counterfactual conditionals, can be confirmed by a small number of positive instances, and are projectible. Like much in the logical empiricist tradition, this notion of natural necessity was fashioned from the cloth of logic. Some of the problems with this approach are artifacts of that history. I will suggest that insofar as natural necessity closely mirrors logical necessity, it will fail to adequately characterize the empirical relations investigated by science.

Logical necessity is an all-or-nothing affair. Either a statement's truth follows necessarily from the truth of a set of statements, or in virtue of its form, without exception and all times, or it does not. The security of expectation warranted by logical necessity may well be comforting, but that security does not get carried along when expropriating the notion of necessity to the natural world. The truth of a statement that is not logically necessary is contingent on other things. All naturally necessary relations represented in lawlike statements in science are contingent in this logical sense. Hence the distinction between naturally necessary and merely accidental generalizations simply cannot be drawn on the presence or absence of contingency *per se*. The dichotomous character of the distinction (logically necessary/contingent, naturally necessary/accidental) must be abandoned. It is the *nature* and the *degree* of contingency, and not the fact of contingency, that separates the lawful from the accidental. Making this explicit forces a move out of the dichotomous space inherited from logical definitions into a continuous domain of kinds and degrees of contingency that may be exhibited by scientific generalizations.

A limitation of the normative approach can be seen in the disagreement between Sober and Beatty. While Sober agrees with Beatty that biological generalizations, if contingent, would fail to be laws, he suggests first, that there are *a priori* non-contingent biological laws, and second, that Beatty's account of evolutionary contingency entails that for every contingent biological law discovered, there must be a non-contingent law in which it can be embedded. Sober's argument depends on articulating the implicit *ceteris paribus* conditions antecedent in all scientific laws. The necessitation relation described by a law holds only when the assumed boundary conditions are also met. I will argue that this way to represent the problem obscures just those features of contingency that Beatty ascribes to the characteristically biological.

Beatty claims that distinctively biological generalizations, while true, are contingent on a particular historical pathway traversed as a result of evolutionary dynamics. Mendel's law of the 50:50 ratio of gamete segregation is true only because the genes determining that ratio had been selected for in a particular episode in the evolutionary history of life on this planet. If we had, in Gould's words, "run the tape again," it is likely that different genes would have been available through mutation, different traits would have evolved, and hence different generalizations would then be true. Indeed, those historical conditions on which the truth of a generalization is contingent (e.g., those determining the selective advantage of the 50:50 segregation gene) may change in the future, rendering the generalization no longer capable of truly describing the state of nature. Beatty calls this feature 'weak contingency'. In addition, by 'strong contingency' Beatty denotes the fact that from the same set of conditions with the same selection pressures operating, variant functionally equivalent outcomes may be generated. "To say that biological generalizations are evolutionarily contingent is to say that they are not laws of nature—they do not express any natural necessity; they may be true, but nothing in nature necessitates their truth" (Beatty 1995, 52).

Sober represents Beatty's thesis using the following logical formulation:

$$\frac{\mathbf{I} \quad \rightarrow \quad [\textbf{if P then Q}]}{\mathbf{t_0} \qquad\qquad \mathbf{t_1} \quad \mathbf{t_2}}$$

Here **[if P then Q]** is an evolutionarily contingent generalization and **I** represents the historical conditions upon which it depends. Sober argues that in using this representation we can easily see that there really is a non-contingent biological law being invoked—**(L): I → [if P then Q]**. Sober claims that here are two ways a law of the form **L** can escape contingency, either by being *analytic* or by taking seriously Beatty's causal claim that there was a particular set of conditions in evolutionary history responsible for **P → Q** being true. That the mathematical laws used in biology (like Hardy-Weinberg equilibrium) are logical truths is neither controversial nor particularly pertinent. That Beatty's very argument for contingency is self-refuting is more serious.

Notice what must be presumed to allow Sober the second interpretation. The assumed complex web of present and absent environmental conditions that rendered the 50:50 gene more fit than other variants, that did not trade off those consequences via other selective pressures, that prevented chance and mutation and migration from overriding the advantage, and so on, is represented simply by **I**. In addition, the complicated causal process which gave rise to the relation described by **[if P then Q]** is abstracted to the arrow of the material conditional. In what way does the new law, **L**, describe a naturally necessary relation between the antecedent, **I**, and the consequent, **[if P then Q]**? One could argue that if all the conditions which cause the rule to be true have been identified, then their occurrence must unconditionally necessitate the truth of the rule. If not, then one could counter that the complete causal story was not described. That is, insofar as Beatty's claim that evolutionary conditions and processes caused the generalization to be true, then a non-contingent causal generalization, namely **L**, should be capable of describing that. But is it really non-contingent? Suppose **L** is a physical, rather than a biological law. Fourier's law of heat conduction, for example, describes necessary causal relations on the presumption that our universe is in a state of thermal disequilibrium (Carrier 1995). As far as we know this condition was fixed by the distribution of particles in the primordial atom, nevermore to change unless, that is, the universe were to suffer heat death. Indeed, one expects the relation described by Fourier's law to be the only physically possible true relation of conduction because it depends on a very stable and enduring set of **I** conditions. Since the presumption is that thermal disequilibrium is a standing condition, articulating it explicitly in **I** is a mere formality. Sober transforms the explicitly contingent biological generalization not into a non-contingent law, but rather into an implicitly contingent physical law. This logical sleight-of-hand obscures, rather than illuminates, the similarities and differences between the evolutionary and physical relations that causally structure our world.

Two forms of complexity are hidden in Sober's representation. First, the set of conditions, **I**, in the biological story consists in a complex and unstable conjunction of conditions. In addition the causal story that lurks in the material conditional is also complex in the sense of being non-linear. That means that very minor variations in the complex set **I** could lead to very dramatic differences in the consequent represented by **[if P then Q]**. Even granting that the conditions specified by **I** cause the relation described by the consequent rule does not preclude variant outcomes from being determined. The simplicity of **I** → **[if P then Q]** conceals what is distinctively biological—namely both the complexity of the conditions upon which the law is contingent and the complexity of the nature of the dependence. In contrast, while a physical law, like Fourier's law, can also be represented as **[if P then Q]** and the historically contingent arrangement of the primordial atom be identified as **I**, the similarity ends there. The **I** conditions are stable, in that arguably they were fixed in the first 3 minutes of the birth of the universe and are extremely unlikely to change. In addition, in the absence of thermal equilibrium, Fourier's law arguably describes the unique relation of conduction true for our universe.

To summarize, Sober is correct that one may reformulate the weakly and strongly contingent biological generalizations that Beatty documents in a simple, logical representation that looks like a physical law. But what is lost is just the characteristic complexity of biological conditions and causes. It is not *that* biological generalizations are contingent, but rather *how* they are contingent that is significant. Importantly, practices of biologists differ from those of physical scientists in ways that correspond to these differences.

With respect to this latter point Beatty attempts to read the nature of biological generalizations backwards in an abductive, or transcendental, inference from the form of the relative significance debates in which biologists are engaged. What, we may ask, must the biological world be like to make sense of such a practice? I think this strategy is powerful, and I have engaged in a similar type of argument to explain what I have called the 'fact of pluralism' of explanatory models in biology (Mitchell 1997). The conclusion Beatty draws is that laws in biology are not universal and exceptionless, since the debates presuppose generalizations of limited scope. Nevertheless, Beatty's analysis of relative significance debates suffers from two ambiguities.

First, the debate structure of biologists' discourse is surely a function, not just of the ontological conditions of the biological world, but also of the biologists' understanding of scientific laws. If they have been properly schooled with the Popperians, as many of them have, then they should be looking for bold, universal, exceptionless laws, and their failure to find them will look like a failure to find laws at all. There still may be laws in biology, but they will not be recognizable as such by those blinded by a limited normative definition. So, while Beatty's approach has the potential to identify a mismatch between a particular *view* of scientific laws and the practices of biologists, it does not yet address whether or not there might be "laws" otherwise construed in biology.

Second, there is a conflation of two distinct problems in Beatty's claim that "The relative importance or significance of a theory within its intended domain is roughly the proportion of phenomena within the domain that the theory correctly describes" (1997). Biological explanations invoke multiple models for two reasons:

- a multiplicity of causal factors interact in generating complex phenomena, and
- different causal factors are restricted to only partially overlapping spatiotemporal domains.

Consider first Beatty's discussion of the lac operon theory. Though it was originally promoted to explain gene regulation in all organisms, it was later discovered not to be adequate to the task. Additional mechanisms (negative repression, positive induction and repression, and attenuation theory) were needed to describe the spatiotemporal diversity of systems from colon bacillus to the elephant. No one mechanism nor one combination of mechanisms was universal. In contrast, the debate between selectionist and neutralist theories of microevolution is not always about whether just selection or just neutralism operates most frequently in determining evolutionary outcomes across the domain, but also concerns the relative contribution of each casual factor in generating a specific complex outcome. I have argued elsewhere for the theoretical pluralism of idealized models and the necessary integration of explanation required to account for multi-factor complexity (see Mitchell 1992, 1997; Mitchell et al. 1997). Here, I just want to point out that these ways of failing to be exceptionless laws are different. While both support theoretical pluralism they entail different scientific practices in generating acceptable explanations. Within the confines of the normative approach, these problems are not *prima facie* distinguishable. Rather they are classified identically as failures to be universal, exceptionless laws.

The normative approach's set of necessary and sufficient conditions which must be met by generalizations to qualify as laws provides a limited conceptual space in which to explore the important differences among biological generalizations and between them and those of physics. A law is necessary, or it is not. Sober's logical representation also fails in this regard. It is like trying to describe the differences between Beatty, Sober, and Brandon by first representing them as stick figures. More must be done to enrich the contingency/non-contingency distinction in order to adequately describe the varied types of generalizations explored in the many sciences. That biological generalizations, **[if P then Q]**, are contingent on conditions **I**, does not distinguish them from physics or chemistry, for these too will reference some more basic conditions which are true of our world, are not logically necessary, and upon which the truth of the laws within those sciences also depends. It is only by attending to the *nature* and *degree* of contingency that a proper understanding of scientific generalizations can be developed.

3. The Paradigmatic Strategy. Let me turn briefly to a paradigmatic approach to answering the question of whether or not the generalizations of biology are laws. Here one engages in a primarily descriptive project which begins with identifying exemplars of laws in physical science, like Newton's laws of motion or the ideal gas law, and proceeds to examine the candidate generalizations in biology to see if they are similar to the paradigmatic laws. This is the strategy Carrier (1995) adopted in his critique of Beatty's *ECT*. Carrier claims that scientific laws should support counterfactuals but, *pace* Beatty, he concludes that biological generalizations are lawlike. What he provides is a series of arguments by analogy, detailing the similarity between biological generalizations and their analogs in physics. Thus, insofar as the paradigmatic physical laws are laws, and biological generalizations are like them, then biology, too, has laws. He shows that all physical laws invoke boundary conditions, and some physical laws, like those for high- and low-level superconductivity, have restricted domains of applicability. In addition he appeals to idealized formulations, like Newton's first law of motion, as evidence of the exception prone character of some members of the exemplar set.

Rather than explore this strategy in greater detail, I wish only to point out two problems in pursuing it. The first is a feature of lumping together all the exemplar laws of physics undifferentiated with the status of law. Specifically, Carrier seems at times to confuse the dependence of the truth of the consequent of the law upon the conditions of the antecedent being true (as in Newton's law of inertia) with the relations described by the law themselves being contingent on historically specific events (as in the superconductivity laws). This confuses the contingent relation described in the law (**Q** is contingent on **P**) with the dependence of that relation on other conditions **(I → [if P then Q])**. Second, and more importantly, while taking physical laws as paradigmatic and comparing them with biological generalizations is a useful enterprise, it leaves open the philosophical question of what a law of nature is. Biology on this account is no worse off than physics or chemistry, and as long as our intuitions about what counts as a law are secure in the exemplar domain, then our evaluation of biological generalizations will follow. However, it fails to address the underlying question of what it is about the cases we identify as exemplar laws that makes them laws in the first place. Carrier is certainly aware of this problem, suggesting that given his arguments, in the end, we could just as well say that there are no laws anywhere in the sciences. Indeed, as I have suggested, there are significant differences both within the set of laws of a given science, like physics or biology, and between them. As this paper argues, a richer conceptual framework allows a detailed account of these similarities and differences to be explored.

4. The Pragmatic Strategy. Taking a pragmatic approach to scientific laws replaces a definitional norm and multiple exemplars with an account of the *use* of scientific laws. How do they function in experiment, in explanation, in education or in engineering? The features of generalizations which perform in these roles can be determined, and one proceeds to see whether and how the generalizations in biology function as laws. The result is a framework for representing the multiple types of generalization found in the various sciences. Notice that rather than a dichotomous space defined by the normative approach, or the unsystematized space of the paradigmatic approach, this view supports a multidimensional frame in which to view these varied qualities of scientific generalizations. This enhances the investigation of multiple lines of relationship among generalizations both within a scientific domain like physics or biology and between these domains. Brandon's (1997) discussion of types of experimentation can be seen as a contribution to this philosophical enterprise.

An important insight in Brandon's discussion is the recognition that the multiplicity of practices in biology are driven by both the ontology of the biological world and the special interests of the scientific community. He shows that the reason biology, compared to physics, engages in less manipulative and more parameter setting experimental practices is a function of both the non-projectible, contingent relations investigated and the history of the epistemic community. Nevertheless, Brandon (1997) holds to the normative definition that to be laws generalizations:

1. have nomic or natural necessity;
2. are used essentially in scientific explanation; and
3. receive confirmation from (a small number of) their positive instances.

Brandon evokes the tension between the acknowledged explanatory character of biological generalizations and their failure to meet the stringent conditions of exceptionless universality. In his words: "the contingent regularities of biology have (a limited range of) nomic necessity and have (a limited range of) explanatory power, but lack . . . unlimited projectibility" (Brandon 1997). Brandon defends the ability of biological generalizations to explain phenomena and thereby to function to fulfill one of the clear goals of scientific practice, while recognizing their failure to satisfy the specified norm of lawfulness. For him, the tension is resolved by surrendering their lawlike status. Biological generalizations are projectible and ground explanations but only within a less than global range. How limited the range is varies and must be discovered empirically.

While I agree with Brandon's description of the way biological generalizations are used in explanation, I disagree that the best way to acknowledge the special character of biology is to remain loyal to the limited dichotomous conceptual framework of the normative approach to laws. By doing so, one privileges a form of generalization which occurs only rarely, if at all, even in physics. Brandon is led to this conclusion by rejecting what he takes to be the only alternative, namely merely extending law status by global edict to broader categories of generalizations in order to cover the evolutionarily contingent ones. I also reject this possibility, as it, too, would fail to provide resources for identifying differences in complexity evidenced by multiple component causes, non-linear causal relations and unstable conditionalizations. The pragmatic approach offers an alternative.

The function of scientific generalizations is to provide reliable expectations of the occurrence of events and patterns of properties. The tools we design and use for this are true generalizations that describe the actual structures that persist in the natural world. The ideal situation would be, of course, if we could always detach the generalizations gleaned from specific investigations from their supporting evidence, carry these laws to all regions of spacetime, and be ensured of their applicability. Such generalizations would be universal and exceptionless. But some causal structures—in particular those studied by biology—are not global. Thus the generalizations describing them cannot be completely detached from their supporting evidence. Nevertheless, we can and do develop appropriate expectations without the aid of general-purpose tools—laws that govern all time and space without exception or failure. To know when to rely on a generalization we need to know when it will apply, and this can be decided only from knowing under what specific conditions it has applied before. To use Sober's representation, the conditions **I** upon which **[if P then Q]** is contingent may be located on a continuum of stability. In addition, the nature of the dependence relation, →, reflects a continuum of strength including probabilities and multiple determinant outcomes. Life, it turns out is not as simple as we might have hoped. Our representations of it will be correspondingly complex.

In addition to the ontological parameters there are other pragmatic aims for which we use generalizations. Scientific representations can be evaluated for their usefulness in virtue of:

- **degree of accuracy** attuned to specified goals of intervention. The eradication of insect pests may require assessing a relatively crude lawlike relation between a chemical, say, and the death of the insect, while increasing the fecundity of other insects, like bees, may require describing more detailed mechanisms and relations.
- **level of ontology**. Generalizations about populations may describe structural relations between trait-groups (like large and small size on calling frequencies in crickets) or functional groups (like predators and prey). Some relations described appear only at a given level and not above or below.
- **simplicity**. We use generalizations ranging from rules of thumb like Ptolemaic astronomical "laws" to navigate, to ideal gas laws that yield approximations within engineering tolerances.
- **cognitive manageability**. Prior to the development of high-speed computation, mathematical equations were restricted to solvable linear formulations.

The contingency of generalizations in biology or other sciences does not preclude their functioning as "laws"—generalizations that ground and inform expectations in a variety of contexts. When we are entitled to have a particular expectation (the scope of domains to which we can export an empirically discovered relation) and the degree of strength of that expectation (in terms of probability or complexity) are dimensions that can be used to compare generalizations within physics or biology, as well as between them. In the multidimensional space defined by the multiple aims of scientific practice including the ontological parameters as well as accuracy, simplicity, ontological specificity, and manageability, it may well turn out that all or most of the generalizations of physics occupy a region distinct from the region occupied by generalizations of biology. The conditions upon which physical laws are contingent may be more stable through space and time than the contingent relations described in biological laws. The strength of the determination can also vary from low probability relations to full-fledged determinism, from unique to multiple outcomes. Indeed the causal contribution of particular features may vary in their sensitivity to environmental conditions including the presence or absence of other causal factors. While I have only sketched the parameters by which generalizations may be compared, it is clear that such a conceptual framework has the resources to display the multiple relationships that exist among and between generalizations in the sciences.

Rather than bemoan the failure of biological generalizations to live up to the normative definition of exceptionless universality, the pragmatic approach suggests a different philosophical project. To understand the multiple relations among scientific generalizations one must first explore the parameters which make generalizations useful in grounding expectation in a variety of contexts.

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