Biol Philos (2013) 28:833–851
DOI 10.1007/s10539-013-9386-y

Stability and lawlikeness

Jani Raerinne

Received: 14 September 2012 / Accepted: 28 May 2013 / Published online: 28 June 2013

Abstract There appear to be no biological regularities that have the properties traditionally associated with laws, such as an unlimited scope or holding in all or many possible background conditions. Mitchell, Lange, and others have therefore suggested redefining laws to redeem the lawlike status of biological regularities. These authors suggest that biological regularities are lawlike because they are pragmatically or paradigmatically similar to laws or stable regularities. I will review these re-definitions by arguing both that there are difficulties in applying their accounts to biology and difficulties in the accounts themselves, which suggests that the accounts are not adequate to redeem the lawlike status of biological regularities. Finally, I will suggest a new account of laws that also shows how non-laws might function in some of the roles of laws.

Keywords Biological laws · Evolutionary contingency · Invariance · Lawlikeness · Stability

Introduction

There appear to be no biological regularities that have the properties traditionally associated with laws. Beatty's (1995) evolutionary contingency thesis offers perhaps the best argument against the existence of such regularities.

For Beatty (1995: 53–58), there are two kinds of contingency to which his thesis refers. By s*trong* contingency, Beatty means, first, that biological regularities lack the necessity associated with laws. Even if biological regularities were true, there might be and quite likely would be background conditions in which the regularities

J. Raerinne (✉)
Department of Philosophy, History, Culture and Art Studies, University of Helsinki, P.O. Box 24, SF - 00014 Helsinki, Finland
e-mail: Jani.Raerinne@helsinki.fi

fail to hold. Thus, biological regularities are at most accidental truths that hold, owing to certain background conditions. Second, given strong contingency, evolution can lead from similar starting points to different outcomes, given the same or similar selection pressures. Thus, even with the same selection pressures, similar or identical adaptations need not follow, even given similar organisms in similar environments, because evolution is easily derailed or disturbed by small changes in its initial conditions.

Weak contingency means simply that biological regularities are riddled with exceptions; in other words, their scope is limited.

Taken together, weak and strong contingency pose a threat both to biological regularities' lawlikeness and to their truth. The strong contingency of biological regularities suggests that they are not lawlike, because they lack nomic necessity, whereas their weak contingency suggests that they are not representable even as accidentally true generalizations, owing to their exceptions.

Mitchell (1997, 2000, 2002), Lange (1999a, b, 2002, 2004, 2005a, b, c, 2008, 2009), and others (Cooper 1998; Mikkelson 2004) have suggested redefining laws—at least in part in response to Beatty's evolutionary contingency thesis—to redeem the lawlike status of biological regularities. These authors suggest that biological regularities are lawlike because they are pragmatically or paradigmatically similar to laws or stable regularities.

There are other responses to Beatty's evolutionary contingency thesis as well as other re-definitions of laws in addition to those discussed here. One example is that laws hold only *ceteris paribus* (Pietroski and Rey 1995; Weber 1999); another example is that laws are about dispositions (Woodward 1992; Lipton 1999; Cartwright 2002). Since there is already an extensive discussion of these two examples as re-definitions of laws, I will concentrate on the accounts by Mitchell (sections "Normative, pragmatic, and paradigmatic accounts of laws" and "Conclusions") and Lange (section "Collective stability"), which so far have remained undiscussed. In section "Normative and paradigmatic stability accounts of laws", I will discuss the idea of identifying lawlikeness with stability. I argue that the above re-definitions of laws are not adequate to redeem the lawlike status of biological regularities, owing to various difficulties in the accounts. In the final section, I will suggest a new account of laws that also shows how non-laws might function in some of the roles of laws.

Normative, pragmatic, and paradigmatic accounts of laws

Mitchell (1997, 2000) has distinguished three ways of defining laws, which she calls normative, paradigmatic, and pragmatic accounts.

A normative account of laws gives a prior definition of lawlikeness that established the criteria which regularities must fulfill in order to count as laws. Mitchell is convinced that because philosophers almost universally adopt some version of a normative account of laws, this—rather than something in the biological regularities—creates difficulties, which suggests that there are no biological laws. This is because normative accounts do not fit the examples of putative biological laws, such as having unlimited scope or holding in all or most of the background conditions (Mayr 1956; Murray 2000).

As an alternative, a paradigmatic account is a comparative account that takes some examples of regularities—presumably from physics—as examples of laws. It then compares them and their properties to other regularities, such as biological ones, to see how the latter live up to the standards suggested or described by the examples.

The problem with the paradigmatic account of laws is that there seems to be no agreement on what the examples or paradigmatic physical laws teach about how biological lawlike regularities should look. Yet such an agreement is needed to compare and evaluate whether biological regularities are similar in properties to the paradigmatic examples of laws and their lawlike-related properties.

The ideal gas law states that at constant temperature (T), the pressure (P) of gas is inversely proportional to its volume (V) according to the equation $PV = rT$, where r is a constant. What does this example teach us about laws? Is there an agreement on what its central lawlike or non-lawlike properties are? Is there consensus that the statement is a law? There are no lucid or clear-cut answers to any of these questions.

Some hold the ideal gas law or van der Waal's law to be a phenomenological regularity (Salmon 1984: 121, 136, 227), not a causal or explanatory one, whereas others hold the opposite (Woodward 2000: 207–209, 218–219). Although many authors think that the ideal gas law can be described as an empirical generalization, others have put forward the idea that it is an analytical statement or a definition (Smith 2002: 254–257). Some think that the law is true; others, that it is false (Earman and Roberts 1999: 461–462). Among those who think that the ideal gas law is a law, there are various ideas—even contrary or contradictory ideas—about what kind of a law it is: a *ceteris paribus* law (Pietroski and Rey 1995: 82–83, 89–91, 97–98), a coexistence law (Hempel 1965: 242, 352), a succession law (Salmon 1984: 135–136, 227), a law about disposition (Lipton 1999: 166; Cartwright 2002: 428, 430–433), and so on.

The point is that there are different interpretations of both the ideal gas law and what its central lawlike properties are. And this holds true, even if some of the interpretations mentioned are like comparing apples and oranges. Moreover, a comparable situation holds for other, more fundamental physical regularities, such as Newton's laws of motion and gravitation, for which there are also different interpretations as to what kinds of regularities they are and what their central lawlike properties are (Nagel 1961: 61–62; Hempel 1965: 301; Joseph 1980; Cartwright 1983: 56–67, 2002; Lange 1993a: 236–237, 245–247; Carrier 1995: 89, 93–96; Pietroski and Rey 1995: 86, 89, 104–106; Earman and Roberts 1999; Earman et al. 2002; Smith 2002).

The points outlined above suggest that the paradigmatic account is a non-starter, because there is no agreement as to what the central properties of physical lawlike regularities are that can then be compared to other regularities in order to see whether they live up to the standards suggested or described by the paradigms of laws. There is a further idea to which proponents of a paradigmatic account of laws might refer, namely, that stability is the central property of lawlike regularities, which all laws have some degree. Rather than discussing this idea here, I will take it up in the next section.

What has been said about physical regularities applies to biological regularities as well: for instance, there is no consensus or agreement among philosophers on how natural selection should be interpreted or what kind of a regularity it is (Scriven 1959; Williams 1970: 362; Ruse 1973: 38–41; Brandon 1978; Mills and Beatty 1979; Reed 1981; Rosenberg 1985: 126–129, 154–169, 211–225, 239–243; Rosenberg and Kaplan 2005).

If the situation is divided among philosophers, is it less so among scientists? No. For instance, ecologists have reached no consensus on the central properties of ecological lawlike regularities and/or what the paradigmatic examples of ecological laws are (Lawton 1999; Murray 2000; Berryman 2003; O'Hara 2005). In fact, ecologists hold ambiguous and non-committal ideas about laws. I assume that the situation is similar among physicists as well. As an example, Berryman (2003) suggests that laws are regular under "certain conditions," but not are necessarily regular outside of those conditions. The problem is that this account tells next to nothing about what ecological laws are. Berryman's account is consistent with different normative and paradigmatic accounts of laws, for instance, with laws as regularities that hold *ceteris paribus*. His account is also consistent with an account that says laws are about dispositions or tendencies that need not manifest in regularities. Furthermore, qualifying laws with protective clauses, such as laws holding under certain conditions, could lead to semantic, epistemic, and empirical problems in accounts of laws.

The above reasons militate against the paradigmatic account, and the situation seems to call for a normative or pragmatic account of laws.

The third alternative account of laws—one that Mitchell favors—is pragmatic. In a pragmatic account, the focus is neither on a prior definition nor on criteria of lawlikeness as in the normative definition. Nor is it on comparisons between regularities, as in the paradigmatic account. Instead, the focus is on the roles of laws, and especially on the question of whether different regularities satisfy the roles that laws are supposed to have, and if they do, how. If some regularities satisfy the roles of laws, then these regularities are laws, regardless of what their lawlike status is said to be according to normative or paradigmatic accounts.

Let us suppose that the central role of laws has to do with providing scientific explanations. That this is the central and even the proprietary role of laws is an idea shared by many, including Mitchell. Moreover, she refers to the stability of regularities as the relevant pragmatic dimension or property of laws:

> Thus the difference between the laws of physics, the laws of biology, and the so-called accidental generalizations is better rendered as degrees of stability of conditions upon which the relations described depend, and the practical upshot is a corresponding difference in the way in which evidence for their acceptance must be treated in their further application. (Mitchell 2002: 346.)

> At one end of the continuum are those regularities whose conditions are stable over all time and space. At the other end are the so-called accidental generalizations. And in the vast middle is where most scientific

> generalizations are found. It is my view that to reserve the title of "law" for just one extreme end is to do disservice to science by collapsing all the interesting variations within science into one category, non-laws. (Mitchell 2000: 254.)

If these passages were taken out of context, one might conclude that Mitchell is engaged in a normative project of defining lawlikeness via the stability of regularities. This is not her point. Rather Mitchell's point is that stability is a central property of regularities whose possession makes them explanatory. The difficulty with Mitchell's pragmatic account of laws is that stability is a wrong property of regularities insofar as their explanatory power is concerned. There are explanatory biological regularities that are invariant rather than stable (see sections "Normative and paradigmatic stability accounts of laws" and "Conclusions").

Normative and paradigmatic stability accounts of laws

Traditional accounts of laws conceptualize lawlikeness as a dichotomous thing that is contrary to the contingency or accidentally of regularities. There is one account that conceptualizes lawlikeness differently, namely, the stability account of laws. The stability account discussed here could be presented either as a normative or as a paradigmatic account.

Because Mitchell is an advocate of a pragmatic account of laws, it is not fair to attribute the normative or paradigmatic stability account of laws to her. Such an account can nevertheless be extracted from her ideas, and she has statements that, to the unwary, seem to identify stability with lawlikeness and replace the dichotomy of lawlikeness with a continuum of stability (Mitchell 1997: S470; 2000: 254–255; 2002: 346). Yet other philosophers have proposed that lawlikeness is similar to stability, if not in fact to be identified with lawlikeness, and moreover, that lawlikeness is a continuum (Woodward 1992; Lipton 1999; Cooper 1998: 571–578; Mikkelson 2004: 124–125).

Let me call a regularity stable if it holds in many possible background conditions. An alternative phrasing would be that a regularity is stable if it holds during numerous interferences. Unstable regularities break down under such conditions. Stability is a concept of degree as well. A regularity is more stable the more possible background conditions there are in which it holds. Stable regularities can be claimed to be context-insensitive and reliable in the sense that they continue to hold in many possible background conditions.

Stability has many right properties to redeem the lawlike status of biological regularities. Since stability is a degree, there might also be degrees of lawlikeness. Consequently, even if some biological regularities failed to achieve a maximal or a high degree of stability—as is typically attributed to physical laws –, these regularities could nevertheless count as lawlike if they had a high enough degree of stability. If stability is the right property to identify with lawlikeness and if it is a property with degrees, then the evolutionary contingency of biological regularities is not as serious a threat to their lawlike status as it would be if a dichotomous account of lawlikeness is held. Rather than thinking of contingency and nomic necessity as contrary and dichotomous, the stability account would view the two as opposing ends on a continuum of stability. Besides, since all regularities are more or less stable, the contingency of biological regularities is not a sufficient condition to deny their lawlike status. To make such criticism potent it has to be shown that biological regularities lack high degrees of stability in general and/or in contrast to the regularities of other sciences. Likewise, the continuum nature of lawlikeness might explain why it has been so difficult to distinguish between laws and accidental truths.

As persuasive as these properties are for the identification of lawlikeness with stability, I have qualms about whether stability *as the sole criterion* (see section "Conclusions") will be satisfactory in the given context. The stability account appears to count cases of spurious causation as laws when and if they exhibit a high degree of stability. However, cases of spurious causation are paradigms of accidentally true generalizations. This suggests that the stability account—as a normative or paradigmatic account—fails to provide an account of lawlikeness, since it fails to distinguish between laws and non-laws.

Moreover, there is not just one stability, but different stabilities to identify with lawlikeness, which I define as follows: *constancy*—lack of change in a system despite changes in its background conditions; *persistence*—the survival time of a system when faced with changes in its background conditions; *inertia*—the ability of a system to resist change in its background conditions; *elasticity*—the speed of return of a system after changes in its background conditions; *amplitude*—the area or extent of equilibrium of a system during changes in its background conditions; *cyclical stability*—the ability of a system to return to a cycle or an oscillation, despite changes in its background conditions; and *trajectory stability*—a property of a system whereby it could lead to the same or similar end results in its dynamics, despite differences in its initial background conditions.

What is common to these stabilities is that they describe what is needed from a system or a regularity in order to hold during changes in its background conditions or during interferences. Nevertheless, these stabilities are also different. Whereas constancy, persistence, and inertia deal with the *endurance* of systems or regularities during changes in the background conditions or interferences, elasticity, amplitude, and cyclic stability deal with *recovery* during changes in the background conditions or interferences. Trajectory stability could be understood as dealing with the *sensitivity* or *inevitability* of systems or regularities during changes in the background conditions or interferences. Thus, we have different, but related concepts of stability.

Ergodic systems exhibit most of the above forms of stability to some extent. I define ergodic systems as those capable of returning to their original or initial state or dynamic. Non-ergodic systems are non-stable in the sense of elasticity and amplitude. In addition to systems, the above forms of stability apply to regularities about the systems as well. Regularities, rather than generalizations, are stable in the sense that their holding is dependent on different background conditions. The holding of a regularity depends on whether the regularity that a system displays in its dynamics, behavior, or state holds in certain background conditions. The generalization describing the regularity will be true or false depending on whether the regularity in question holds in the background conditions. Constancy, that is remaining unchanged, is meant to apply to regularities or systems that do not exhibit dynamics, in contrast to inertia, which is meant to apply to regularities or systems that exhibit some dynamics or active behavior. In other words, the difference between constancy and inertia has to do with the difference between co-existence laws or non-change-relating regularities that are stable in the sense of constancy and succession laws or change-relating regularities that are stable in the sense of inertia.

Accordingly, there are many interpretations of the lawlikeness of regularities in terms of stability. There is no need to discuss all of them. It is not a plausible interpretation of lawlikeness to analyze it in terms of persistence or elasticity of regularities, since these properties concern temporal aspects of stability, without any implications about whether the relevant background conditions have changed. But with the remaining five concepts, we have more plausible candidates for lawlikeness, since these concepts have to do with more general aspects of endurance, recovery, or sensitivity. In other words, lawlikeness can be identified with constancy, inertia, amplitude, cyclical stability, and/or trajectory stability of regularities. Is it an advantage or a disadvantage of the stability account of laws that there are so many forms of stability identified with lawlikeness? On the one hand, it is conceivable that different sciences or disciplines favor different forms of stability of regularities as the most important ones in their domain. Consequently, it could be argued that there are different lawlikeness*es* in different sciences/disciplines. This is an intriguing suggestion, one that has remained unexamined. This suggests that the fact that there are many forms of stability to be identified with lawlikeness is an advantage of the stability account of laws. On the other hand, most philosophers think of lawlikeness as a common property of laws. Thus, they treat lawlikeness as a monolithic thing in contrast to what I have just suggested as being true of stability.[1] This suggests that the fact that there are many forms of stability to be identified with lawlikeness is a disadvantage of the stability account of laws.

The problem with the latter view—i.e., the view that lawlikeness is a monolithic thing—is that it is not clear which of the stabilities mentioned should be preferred as the lawlikeness of regularities. Moreover, biological and non-biological examples of putative lawlike regularities presented in the literature display many of the above-mentioned stabilities rather than some specific stability. The problem with the former view—i.e., the view that there are different lawlikeness*es* in different sciences—is that there are no systematic connections between stabilities of regularities and different sciences. Nor it is true that different sciences favor

[1] Some common trends emerge in the papers that discuss stability in the context of laws. First, although explicit definitions of stability are missing in these papers, it becomes clear once we look at their examples of stability that they have understood stability in the sense of constancy or inertia, which, second, are not distinguished from one another (Woodward 1992; Cooper 1998; Lipton 1999). Third, other stability concepts are neglected, with Mitchell (1997, 2000, 2002) being an exception to this and who also discusses trajectory stability. There does not seem to be anyone who discusses—in the context of laws—persistence, elasticity, amplitude, and cyclical stability, although insofar as manipulations and control of systems are concerned, these are important properties (Odenbaugh 2001; Justus 2008). Fourth, some authors confuse scope with stability and/or stability with invariance, even though the three are different and play different roles in scientific explanations (see section "Conclusions").

different stabilities of regularities as the most important in their domain. Finally, regularities exhibit the above stabilities with heterogeneous and diverse patterns in different sciences or disciplines. To establish the points just made, consider ecology and the stabilities that its regularities display.

Island biogeography investigates such putative examples of laws as area, distance, endemicity, and the species-area rule (see Figs. 1, 2). According to the area rule, an island's extinction or turnover rates depend on the island's size. When an area of an island is decreased (or increased), its species extinction rate is increased (or decreased). According to the distance rule, the immigration rates of islands depend on their distance from the continental source of the immigrant species. When the degree of isolation from the source region of an island is increased (or decreased), its immigration rate is reduced (or increased). According to the endemicity rule, the percentage of endemic species—that is, those species that are indigenous only in a specific area—increases with the size of an island, which follows from the area rule. Finally, according to the species-area rule, the number of species (on an island) varies with the area (of that island) and can be presented as a power equation, $S = cA^z$, where S is the number of species of a given taxonomic group, A is the area, and c and z are constants.

A famous experiment in island biogeography consisted of small islands—of different sizes and different distances from their faunal source area—that were defaunated (MacArthur and Wilson 1967; Wilson and Simberloff 1969). After the defaunation of the islands, re-colonization of the fauna on the islands was monitored.

The results indicated that the species number of an island is in a globally stable point of equilibrium determined by the variables the area or size of an island and its distance from the faunal source area. Although there was thus amplitude stability in the species number and trajectory stability or succession towards it, there was no constancy or inertia in the species composition, at least in the first years, nor was

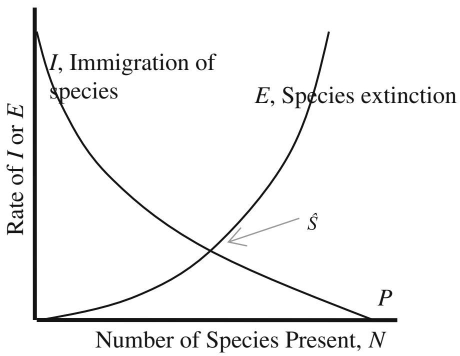

Fig. 1 An equilibrium model of an island. The equilibrium number of species, $\hat{S}$, is reached at the intersection point between the *curve* of the immigration of the new species and the *curve* of the extinction of the species from the island. P is the number of species in the species pool. The point at which immigration and extinction *curves* intersect is a globally stable equilibrium. Note that, although the *number* of species is at equilibrium in the intersection, the identity or composition of the species in it need not to be constant

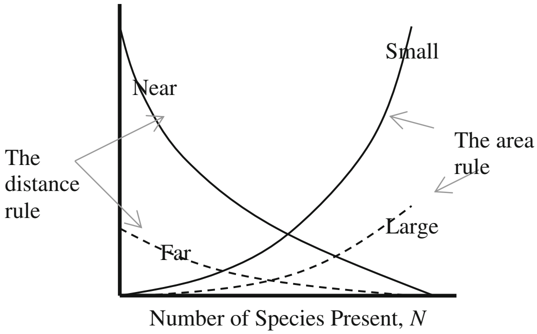

Fig. 2 An equilibrium model of islands at *varying distances* from the source area of immigrant species and of *varying size*. An increase in distance (from *near* to *far*) lowers the immigration *curve*, while an increase in island size (from *small* to *large*) lowers the extinction *curve*. Manipulation of size or distance of an island thus has an effect on what the equilibrium number of the species, $\hat{S}$, of that island will be

there any trajectory stability toward it. In addition, the speed of return (elasticity), inertia, and so on of the species number and some other factors were studied in this and later experiments on island biogeography.

As another example, May (1975) demonstrated that simple, traditional non-linear models of population growth, such as the logistic and exponential models, in their difference equation forms drastically change their dynamic behavior if a value of one of their parameters—*r*, the intrinsic growth rate of population—is set above a certain threshold. The results demonstrated that, above the threshold value of *r*, well-behaved classical models exhibit new kinds of stabilities (e.g., cyclical stability instead of point equilibrium) and that they likewise exhibit changes in their stability conditions (e.g., they displayed low trajectory stability characteristic of chaotic systems). These results were important for the research on population regulation, since the results suggested that observations of stochastic fluctuations in population densities or numbers do not necessary imply density-independence in population growth as was traditionally thought by ecologists and as was suggested by their models.

At the same time, many allometries and scaling laws, that is, regression equations in which body size or weight of taxa is treated as an independent variable of anatomical, physiological, morphological, behavioral, life historical, ecological, or paleobiological traits—exhibit only specific stability, such as inertia.

Thus, it is an open question as to whether there are systematic connections between the stabilities of regularities and different sciences. Instead, the examples given above suggest that regularities of some science(s) display all or many of the stabilities mentioned above. Nor are there reasons to think that some areas of science, such as ecology or evolutionary biology, deem only some of the stabilities mentioned in the context of laws to be important. The points just outlined suggest that the view that there are different lawlikeness*es* in different sciences or disciplines is implausible. Moreover, since the above regularities are often presented in the literature as examples of putative laws, this spells trouble for the view and for the idea that some specific stability should be identified with the lawlikeness of biological regularities.

There is another, more important concern about the stability account of laws. The main reason behind the stability account is that its proponents subscribe to a covering law model of scientific explanations (Cooper 1998). If it can be shown that stability is a wrong concept in the context of scientific explanations, then the general idea of identifying stability with lawlikeness loses much of its motivation.

Consider cases of spurious causation that, as regularities, often display high degrees of stability, especially inertia. There is a stable and positive correlation between readings on a barometer and the occurrence of storms, which is due to a common cause, namely, changes in atmospheric pressure. The stable correlation between readings on a barometer and the occurrence of storms allows for predicting the occurrence of storms. But readings on a barometer neither explain nor cause the storms. Nor does the stability of the correlation between joint effects of the common cause just mentioned have much to do with explanations. Rather, it is the common cause, that is, the changes in the atmospheric pressure, that explains the occurrence/non-occurrence of storms and accounts for why there is a correlation between the two.

What has just been said applies to many allometries and scaling laws. Although allometries and scaling laws as regularities could be stable, many of them are not explanatory, for instance, because they are cases of spurious causation (Raerinne 2013). There is a relation between the variables the maximum density of herbivorous mammals and body size that holds in many different background conditions. But although the relation is stable, it is a case of spurious causation or one, which does not remain invariant under its interventions. This suggests that many allometries and scaling laws do not represent causal or explanatory regularities, but rather accidentally true generalizations that hold owing to their common causes. This is also true of many putative examples of physical laws presented in the literature, such as "all regions of space exhibit a cosmic background radiation of 2.7 degrees of Kelvin."

Let me take stock. The first problem of identifying stability with lawlikeness is that there are different stabilities to identify with lawlikeness, which regularities and examples of putative lawlike regularities display by means of heterogeneous patterns. This is a problem especially for the *paradigmatic* account of laws if it is used here to suggest that paradigmatic laws are stable regularities. The second problem of the *normative and paradigmatic* stability accounts is that the accounts counts as laws cases of stable spurious causation. However, many of us think of these as paradigmatic cases of accidentally true generalizations.

The third problem of the stability account of laws is that stability does not furnish regularities with explanatory power, which is also the critical problem for Mitchell's *pragmatic* account of laws discussed in the previous section. This is a problem for the stability accounts of laws *in general*, because lawlikeness is commonly thought of as a central property of regularities in the context of scientific explanations.

The fourth problem is that biological regularities seem to lack maximal and high degrees of stability. Bergmann's rule is a geographical or latitudinal gradient in body size, according to which the members of a species of endothermic animals are larger in body size in colder regions or at higher latitudes than members of the same species in warmer regions or at lower latitudes. The uniformity or regularity described by Bergmann's rule has a low trajectory stability in the sense that its validity is dependent on certain initial or historical conditions. Endothermicity arose as an adaptation to fluctuating environmental conditions. Now, if the earth's past climates or environmental conditions had been constant rather than fluctuating, then it is possible that endothermicity would not have evolved. If endothermicity had not evolved, then the regularity described by Bergmann's rule would not hold, since the regularity applies to endothermic rather than to ectothermic animals. Past environmental or climate conditions can thus be seen as an initial condition that could have disturbed the validity of the regularity described by Bergmann's rule had these conditions been different.

The same holds for the regularities described by Mendel's rules and the Hardy–Weinberg rule. Had there not been mitosis or had there been some equivalently fit or fitter alternative, it is likely that meiosis would not have evolved. Therefore, Mendelian rules would not have evolved on this planet. Had Mendelian rules not evolved or had conditions changed so that these rules no longer held, then the Hardy–Weinberg rule might not hold, because it seems to be a consequence of Mendelian rules. In other words, the regularities just described are contingent or accidental products of history, whose evolution could have been derailed or disturbed had some of their initial or historical background conditions been different. They display moderate or low degrees of trajectory stability. The generalizations that describe the regularities are true or false, depending on whether the regularities hold.

Biological regularities seem to have low or moderate degrees of constancy and inertia as well. By contrast, a maximal or very high degree of constancy or inertia is typically attributed to physical regularities. For many biological regularities, it is true that had some of their background conditions been different, then they would not hold (cf. Utida 1953; Savile 1960; Dayton 1971; Crow 1979; Rabenold 1979). The *ceteris paribus* law account literature is also riddled with examples of this kind (Weber 1999). This suggests that biological regularities are accidents lacking nomic necessity that hold because of certain background conditions.

Collective stability

Lange (1999a: 254–260, 1999b: 630–634, 2002: 412–421, 2004: 96–101, 2005a: 396–402, 2005b, c: 280–285, 2008: 76–83, 2009: 322–333) has referred to stability in the context of laws. In lieu of focusing on stability conditions of individual and separate regularities as others have done (section "Normative and paradigmatic stability accounts of laws"), Lange makes the intriguing claim that laws are necessary, support counterfactuals, and/or are lawlike owing to their collective stability. Lange's point with his collective stability is to reinforce and supplement his former inference rule account of laws (Lange 1993a, b, 1995).

For Lange, collective stability means that laws remain true in every counterfactual scenario that is nomically possible, that is, in every counterfactual scenario consistent with *the logical consequences of other* laws that, together with the law in question, form a set of laws of that science or discipline. Collective stability gives the stability domain over which laws are supposed to hold.

Consider one of Lange's (2004: 103, 2005a: 400) examples. The Lorentz force law is a physical law that would not hold if charged bodies that travelled at a speed faster than the speed of light existed. But other physical laws and their logical consequences guarantee that a counterfactual scenario of such bodies travelling faster than the speed of light is forbidden. Hence, the Lorentz force law is a member of a stable set of integrated physical laws, and it is nomically necessary, owing to its stability within the collective stability domain, that the other physical laws restrict or limit for it. Accidental regularities do not hold or remain stable within counterfactual situations or suppositions given by laws and their logical consequences that are consistent with these accidents. There is nothing in the laws of physics or in their consequences that would make the construction of a gold cube larger than 1,000 kg impossible, whereas the logical consequences of laws of nuclear chain reactions forbid the construction of a lump of uranium-235 weighing over 1,000 kg, because this is greater than the critical mass of the isotope, which is around 50 kg. Thus, "all persisting lumps of pure uranium-235 have a mass less than 1,000 kg" is nomically necessary, whereas the similar gold regularity is not.

Lange (2004: 101–105, 2005a) uses examples of biogeographical regularities, such as the area, distance, endemicity, and species-area rule to show how ecological regularities form a set of integrated laws. Moreover, these laws in their collective stability could be independent from considerations that deal with physical counterfactual scenarios that would or could violate ecological laws. What makes ecological regularities nomically necessary or lawlike is that they remain stable within the domain of counterfactual scenarios dictated by other ecological laws in their integrated set of laws. If they break outside this domain, then they could be nomically necessary, because non-ecological or non-biological counterfactual scenarios need not be within the "purposes" of ecology or within its collective stability domain that is given by its laws. Ecological laws do not have to remain unviolated in physical counterfactual situations that remain within the possibilities of physical laws.

Consider a physically possible counterfactual scenario: had the Earth been struck in the past by a meteorite that destroyed life, then ecological laws would not have held on this planet. However, this scenario is not one that ecologists need be worry about, because it is outside the domain or purpose of ecology. Thus, there is no problem if ecological laws are violated in such counterfactual scenarios.

Although ecological regularities need not hold given every physically possible counterfactual scenario, if they are collectively stable within the possibilities restricted by a set of ecology's laws, then they are still nomically necessary regularities. Collective stability seems to be a clever solution to Beatty's (1995) strong contingency criticism of biological regularities.

Lange's treatment of and focus on collective stability leaves open what he means by the stability of individual or isolated regularities. Unfortunately, Lange does not give these general semantics or indicate how and under what conditions they acquire truth values over and above his insistence that individual lawlike regularities need only be "reliable" rules of inference.

Lange is aware that biological laws—and perhaps even the fundamental laws of physics—are not reliable rules of inference without provisos. Thus, Lange (1993a, 1995, 2002, 2005a) admits that biological laws are contingent. This shows that they are not strict laws, but *ceteris paribus* laws (Lange 1993a, 2002). The *ceteris paribus* account of laws raises empirical, epistemological, and semantic problems that are unresolved, at least given the main theories and proposals of such laws in the literature (Earman and Roberts 1999, 2002; Mitchell 2002; Smith 2002; Woodward 2002). In short, it has proven difficult to define what *ceteris paribus* clauses mean. If a more precise meaning or semantics is not provided, then we have "laws" that are empirically, epistemologically, and semantically vague. Since Lange has not given any semantics or truth conditions for *ceteris paribus* clauses of *ceteris paribus* laws, this suggests that his account of laws is incomplete.

Yet Lange (1993a: 239–245, 2002) has insisted that *ceteris paribus* laws might be pragmatically justified or that there exists "shared implicit and tacit scientific background knowledge" that is in need of no explication or exact determination. It is this background knowledge that furnishes us with reliable rules of inference.

It is common knowledge that the meaning of *ceteris paribus* clauses is discipline- or science-specific. For example, physicists are interested in the symmetry conditions of the regularities, whereas biologists are not. Economists are not that concerned about there being a drug that makes people act non-rationally, whereas psychologists might be very interested in such a drug and its effects. However, economists are interested in whether economic regularities hold under the changes of an agent's *information*. The point is that, although the existence of such local shared background knowledge is well-known, it is not an *explanans*, but an *explanandum* that calls for an explanation and an explication. Lange's appeal to background knowledge as determining the meaning of *ceteris paribus* clauses postpones the problem to the matter of defining in more general and philosophically enlightening terms what this background knowledge consists of.

I suspect that in the biological sciences, we have at our disposal sets of integrated laws that restrict one another's stability in the neat and systematic way suggested by Lange (2004, 2005a) and as required by his stability account. Putative examples of such sets of laws involve the theory of biogeography and the competition theory in community ecology. But there is more to ecology than these two theories.

To present just one example, consider synecology, the study of ecological interrelations. For decades, ecologists have sought a general, unified, and integrated theory of species interactions. Yet even today there is considerable disagreement, first about whether such a theory can be given and second, how such interactions between species, such as predation or mutualism, can be presented as parts of such a theory (but see Vellend 2010). The only part of the synecological theory that has any coherence is the competition theory. But even this theory is reticulate and loose when it comes to the connections of its regularities. Thus, it is far from clear that ecological regularities form sets of laws that restrict one another's stability in the sense that Lange postulates as being distinctive of laws. The above suggests that

Lange's stability account does not redeem the lawlike status of ecological regularities.

Besides, it seems that, as a solution to strong contingency, collective stability presupposes that the other laws which give the stability domain of some law cannot be weakly contingent, because Lange needs exceptionless laws to establish the collective stability domain laws. The question is how does Lange's collective stability cohere with his previous idea of the exception-ridden, yet reliable inference rule account of laws?

The problem is that exception-ridden regularities admit exceptions to stability conditions of laws whose stability domain the former determine. The admittance of exception-ridden laws to a set of laws implies that laws are not necessary or that they are strongly contingent after all, because laws do not necessarily hold even within their collective stability domain, since there are exceptions to laws. This seems to be a consequence of the fact that Lange treats stability as a feature with no degrees (Lange 1999a, 2005a: 397, 2005b: 425, 2009: 325), as the next passage illustrates:

> Possessing some variety of 'necessity' is supposed to be *qualitatively* different from merely being invariant under a wide range of counterfactual suppositions. Because the set of laws is maximally resilient – as resilient as it could logically possibly be – there is a species of necessity that all and only its members possess. No variety of necessity is possessed by an accident, even by one that would still have held under many counterfactual suppositions. (Lange 2008: 83.)

If there are exceptions to the law that "charged bodies cannot travel at a speed faster than the speed of light," then it follows that the Lorentz force law is not maximally resilient, since it *does not hold under all the counterfactual situations consistent with the logical consequences of other physical laws* that constrains the stability of the Lorentz force law in the manner discussed above. Owing to the exceptions of the former law, there is a nomically and physically possible counterfactual situation in which the Lorentz force law does not hold or remain stable, namely, the one in which charged bodies travel at a speed faster than the speed of light. Thus, the Lorentz force law is not maximally resilient, and consequently, it is no law within the set of physical laws, but an accidentally true generalization. Similarly, if there are exceptions to the laws of nuclear chain reactions, then the construction of a lump of uranium-235 weighing over 1,000 kilograms is nomically and physically possible—or at least the physical laws do not forbid the construction of such a lump of uranium every time and in every place, owing to the exceptions to the laws of nuclear chain reactions. In other words, the uranium-235 regularity is not maximally resilient within its collective stability domain, and consequently, it is not a law. Furthermore, the argument could be generalized to other examples of physical laws if it is true that all physical laws have exceptions, which suggests that there are no laws in physics. A similar argument can also be used to reason against the existence of biological laws, given that there are exceptions to biological regularities, which thus seems to suggest that biological regularities are not maximally resilient within the integrated set of biological laws.

Lange's (2002: 413–414, 416 and 2004: 100–101) claim that individual laws (as inference rules) need only be "reliable" is unenlightening and ad hoc in this context.

In the context, Lange might refer to his suggestion that there are meta-laws, which govern ordinary laws (Lange 1999a: 248–249, 252, 2005c: 280, 2007, 2009: 342–344). The idea would be that meta-laws are not riddled with exceptions, and thus they are capable of delimiting the range of ordinary laws' collective stability. The problem is that the idea of universal and exceptionless meta-laws begs the question. It remains to be shown or argued that there are universal and exceptionless meta-laws. Finally, Lange (1993a, 2002) admits that there are exceptions to *all* laws, including fundamental physical ones.

The points presented above suggest that there are difficulties, undeveloped points, and perhaps even inconsistencies in Lange's collectivity stability account, at least when the account is being applied to biology.

Conclusions

I have argued that some recent re-definitions of laws as pragmatically or paradigmatically similar to laws, or as stable regularities, as suggested by Mitchell, Lange, and others, have difficulties in redeeming the lawlike status of biological regularities. The main reason that so many recent philosophers suggest that a re-definition of laws is needed does not have so much to do with the lawlike status of biological regularities per se, but rather with the concern that without laws, there would be no autonomous biological explanations. The problem with this reason is that there are explanatory regularities that are invariant—rather than lawlike—in the sciences (Woodward 2000, 2001). Invariant regularities describe dependency relations that can be used to manipulate things. An invariant regularity describes what would happen to a value of a variable of a regularity or relation if a value of one or more of its other variables were changed by an intervention or manipulation. Invariant regularities can be quite dissimilar to laws, at least when compared to the ways in which philosophers and scientists think of laws. There is no requirement that invariant regularities or generalizations should contain only purely qualitative predicate terms, be universal, be maximally or highly stable, or belong to a systematic web or collectively stable set of other laws, as many have suggested about laws. A regularity can thus be invariant and explanatory regardless of its lawlike status.

Thus, although lawlikeness in biology might be an elusive or inapplicable concept, there is perhaps no reason to be concerned about biological laws at all. Lange's examples of biogeographical regularities mentioned above are explanatory if they are invariant under their interventions and regardless of whether they are lawlike and/or collectively stable regularities (Raerinne 2011b).

Of course, we can stipulate the word "law" to mean whatever we wish. It could be claimed that if invariant regularities are explanatory, then they are laws, because the term is functional in meaning, as in this statement: "Whatever has a proprietary role in scientific explanations is a law, regardless of what other properties the thing has." This would save the pragmatic account of laws in general, but at the same time it would run counter to Mitchell's version of the account, since it is invariance, not stability, that is the relevant pragmatic dimension of laws. At the same time, one could ask the pragmatists how they motivate their account. What is it in laws that makes them indispensable for scientific explanations? Why can non-laws not function in some of the roles of laws? It appears that the motivation for the pragmatic account is adherence to a covering law model of explanation. However, there are many counterexamples to and difficulties with the covering law model.

To show how non-laws might function in some of the roles of laws, let me suggest a new normative account of laws. Rather than thinking of lawlikeness as a monolithic and dichotomous thing, I suggest that laws are to be defined as having a maximal or very high degree of both stability (especially inertia; Raerinne 2011a: chapter 2) and invariance and as having very large or unlimited scopes (Raerinne 2011a: chapters 4 and 5). Although there might be a few such regularities in biology and even in the sciences in general, this might be of little pragmatic consequence. This is because the properties that jointly make up lawlikeness can be used to explicate how something less than a law is capable of functioning in some of the roles of laws.

Although it is the invariance—rather than the stability—of regularities that furnishes us with explanatory regularities, stability does have an important function, namely, it furnishes us with extrapolability and reliability of explanations. Stability is an important property of regularities insofar as extrapolability and reliability are concerned, because stable regularities furnish us with support for the counterfactuals, which have the form "had this-or-that background condition been different, a regularity would still have held." In general, the larger the stability domain of a regularity, the more (important) counterfactuals it supports, and accordingly, the more extrapolable and reliable the regularity it is, because it holds in many—and/or in more important—different possible background conditions rather than holding owing to some actual or incidental background conditions. Stable regularities can be claimed to be relatively context-insensitive, applicable to different background conditions, generalizable, predictable, and reliable in the sense that they continue to hold in many possible (important) background conditions. In this sense, stable regularities function as many believe only laws can function. Although stability is an important property of lawlike and explanatory regularities, it is not sufficient for explanatory regularities, in contrast to Mitchell's pragmatic account (section "Normative, pragmatic, and paradigmatic accounts of laws"), nor it is sufficient for lawlikeness, in contrast to normative and paradigmatic stability accounts (section "Normative and paradigmatic stability accounts of laws"). Stability is only a necessary component of both lawlike and explanatory regularities.

Scope has a different, but related function: it provides unification and systematization of knowledge. Scope deals with a generality that has do with the actual distribution or range of (dis)similar systems to which a regularity applies or has applied, whereas stability deals with a generality that has to do with the holding of a regularity in various possible background conditions. Scope captures the idea that biological regularities are distributed in their applicability to different taxa, places, epochs, and so on. Many biological regularities have narrow or limited scope: Mendel's rules have their scope as sexually-reproducing taxa; some of the members of the lineage of horses exhibited the pattern of Cope's rule in the Miocene; and currently, rain forests are almost exclusively located at low latitudes.

A choice that traditional law-centered views have forced upon us is the distinction between laws and case studies. The choice is made even by those who think that there is no (need for) biological laws. In a sense, I have presented the degrees between the two extremes. I have suggested that we have at our disposal biological explanatory regularities that are more or less extrapolable and that unify or systematize our knowledge. Invariant and stable regularities with varying scopes function in the above-described manner despite the fact that they need not be lawlike as regularities and despite the fact that they can be weakly and strongly contingent as regularities. Of course, the above does not make the concept of lawlikeness—as maximally stable and invariant regularities having unlimited scopes—futile. The concept might have other roles in the sciences, such as in analyzing induction, confirmation, and theory structure in physics.

Acknowledgments The work was supported financially by the Academy of Finland as a part of the project Causal and Mechanistic Explanations in the Environmental Sciences (Project No. 1258020). I am grateful to two anonymous referees for this journal, who provided helpful comments and suggestions on a previous draft of the paper. A version of the paper was presented at the Philosophy of Science Group/Trends and Tensions in Intellectual Integration seminar, on October 26, 2009, at the University of Helsinki, Finland. For their suggestions made then, the author would like to thank Till Grüne-Yanoff, Inkeri Koskinen, Jaakko Kuorikoski, Caterina Marchionni, Uskali Mäki, Kati Näätsaari, Tuomas Pärnu, Samuli Pöyhönen, Päivi Seppälä, and Petri Ylikoski. I am grateful also to Marc Lange who made helpful suggestions about a draft version of this paper.

References

Beatty J (1995) The evolutionary contingency thesis. In: Wolters G, Lennox JG (eds) Concepts, theories, and rationality in the biological sciences. Universitätsverlag Konstanz, Konstanz, pp 45–81
Berryman AA (2003) On principles, laws and theory in population ecology. Oikos 103:695–701
Brandon RN (1978) Adaptation and evolutionary theory. Stud Hist Philos Sci 9:181–206
Carrier M (1995) Evolutionary change and lawlikeness. Beatty of biological generalizations. In: Wolters G, Lennox JG (eds) Concepts, theories, and rationality in the biological sciences. Universitätsverlag Konstanz, Konstanz, pp 83–97
Cartwright N (1983) How the laws of physics lie. Oxford University Press, Oxford
Cartwright N (2002) In favor of laws that are not ceteris paribus after all. Erkenntnis 57:425–439
Cooper G (1998) Generalizations in ecology: a philosophical taxonomy. Biol Philos 13:555–586
Crow JF (1979) Genes that violate Mendel's rules. Sci Am 240:134–146
Dayton PK (1971) Competition, disturbance, and community organization: the provision and subsequent utilization of space in a rocky intertidal community. Ecol Monogr 41:351–389
Earman J, Roberts J (1999) Ceteris paribus, there is no problem of provisos. Synthese 118:439–478
Earman J, Roberts J, Smith S (2002) Ceteris paribus lost. Erkenntnis 57:281–301
Hempel CG (1965) Aspects of scientific explanation. Free Press, New York
Joseph G (1980) The many sciences and the one world. J Philos 77:773–791
Justus J (2008) Complexity, diversity, and stability. In: Sarkar S, Plutynski A (eds) A companion to the philosophy of biology. Blackwell, Malden, pp 321–350
Lange M (1993a) Natural laws and the problem of provisos. Erkenntnis 38:233–248
Lange M (1993b) Lawlikeness. Noûs 27:1–21
Lange M (1995) Are there natural laws concerning particular biological species? J Philos 92:430–451
Lange M (1999a) Laws, counterfactuals, stability, and degrees of lawhood. Philos Sci 66:243–267
Lange M (1999b) Why are the laws of nature so important to science? Philos Phenom Res LIX:625–652

Lange M (2002) Who's afraid of ceteris-paribus laws? Or: how I learned to stop worrying and love them. Erkenntnis 57:407–423
Lange M (2004) The autonomy of functional biology: a reply to Rosenberg. Biol Philos 19:93–109
Lange M (2005a) Ecological laws: what would they be and why would they matter? Oikos 110:394–403
Lange M (2005b) Laws and their stability. Synthese 144:415–432
Lange M (2005c) A counterfactual analysis of the concept of logical truth and necessity. Philos Stud 125:277–303
Lange M (2007) Laws and meta-laws of nature: conservation laws and symmetries. Stud Hist Philos Mod Phys 38:457–481
Lange M (2008) Could the laws of nature change? Philos Sci 75:69–92
Lange M (2009) Must the fundamental laws of physics be complete? Philos Phenom Res LXXVIII:312–345
Lawton JH (1999) Are there general laws in ecology. Oikos 84:145–147
Lipton P (1999) All else equal. Philosophy 74:155–168
MacArthur RH, Wilson EO (1967) The theory of island biogeography. Princeton university press, Princeton
May RM (1975) Stability in ecosystems: some comments. In: Dobben WH, Lowe-McConnell RH (eds) Unifying concepts in ecology. Centre for agricultural publishing and documentation, The Hague, pp 161–168
Mayr E (1956) Geographical character gradients and climatic adaptation. Evolution 10:105–108
Mikkelson GM (2004) Biological diversity, ecological stability, and downward causation. In: Oksanen M, Pietarinen J (eds) Philosophy and biodiversity. Cambridge university press, Cambridge, pp 119–129
Mills S, Beatty J (1979) The propensity interpretation of fitness. Philos Sci 46:263–286
Mitchell SD (1997) Pragmatic Laws. Philos Sci 64:S468–S479
Mitchell SD (2000) Dimensions of scientific law. Philos Sci 67:242–265
Mitchell SD (2002) Ceteris paribus: an inadequate representation for biological contingency. Erkenntnis 57:329–350
Murray BG (2000) Universal laws and predictive theory in ecology and evolution. Oikos 89:403–408
Nagel E (1961) The structure of science. Harcourt, London
O'Hara RB (2005) The anarchist's guide to ecological theory. Or, we don't need no stinkin' laws. Oikos 110:390–393
Odenbaugh J (2001) Ecological stability, model building, and environmental policy: a reply to some of the pessimism. Philos Sci 68:S493–S505
Pietroski P, Rey G (1995) When other things aren't equal: saving ceteris paribus laws from vacuity. Br J Philos Sci 46:81–110
Rabenold KN (1979) A reversed latitudinal diversity gradient in avian communities of eastern deciduous forests. Am Nat 114:275–286
Raerinne JP (2011a) Generalizations and models in ecology. Dissertation. University of Helsinki. http://urn.fi/URN:ISBN:978-952-10-6768-6
Raerinne JP (2011b) Causal and mechanistic explanations in ecology. Acta Biotheor 59:251–271
Raerinne JP (2013) Explanatory, predictive, and heuristic roles of allometries and scaling relationships. Bioscience 63:191–198
Reed ES (1981) The lawfulness of natural selection. Am Nat 118:61–71
Rosenberg A (1985) The structure of biological science. Cambridge University Press, Cambridge
Rosenberg A, Kaplan DM (2005) How to reconcile physicalism and antireductionism about biology. Philos Sci 72:43–68
Ruse ME (1973) Philosophy of biology. Hutchinson, London
Salmon WC (1984) Scientific explanation and the causal structure of the world. Princeton University Press, Princeton
Savile DBO (1960) Limitations of the competitive exclusion principle. Science 132:1761
Scriven M (1959) Explanation and prediction in evolutionary theory. Science 130:477–482
Smith S (2002) Violated laws, ceteris paribus clauses, and capacities. Synthese 130:235–264
Utida S (1953) Interspecific competition between two species of bean weevil. Ecology 34:301–307
Vellend M (2010) Conceptual synthesis in community ecology. Q Rev Biol 85:183–206
Weber M (1999) The aim and structure of ecological theory. Philos Sci 66:71–93
Williams MB (1970) Deducing the consequences of evolution: a mathematical model. J Theor Biol 29:343–385

Wilson EO, Simberloff DS (1969) Experimental zoogeography of islands: defaunation and monitoring techniques. Ecology 50:267–278
Woodward J (1992) Realism about laws. Erkenntnis 36:181–218
Woodward J (2000) Explanation and invariance in the special sciences. Br J Philos Sci 51:197–254
Woodward J (2001) Law and explanation in biology: invariance is the kind of stability that matters. Philos Sci 68:1–20
Woodward J (2002) There is no such thing as a ceteris paribus law. Erkenntnis 57:303–328