Jani Raerinne

GENERALIZATIONS AND MODELS IN ECOLOGY:
LAWLIKENESS, INVARIANCE, STABILITY, AND ROBUSTNESS

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ABSTRACT

The question at issue in this dissertation is the epistemic role played by ecological generalizations and models. I investigate and analyze such properties of generalizations as lawlikeness, invariance, and stability, and I ask which of these properties are relevant in the context of scientific explanations. I will claim that there are generalizable and reliable causal explanations in ecology by generalizations, which are invariant and stable. An invariant generalization continues to hold or be valid under a special change – called an intervention – that changes the value of its variables. Whether a generalization remains invariant during its interventions is the criterion that determines whether it is explanatory. A generalization can be invariant and explanatory regardless of its lawlike status. Stability deals with a generality that has to do with holding of a generalization in possible background conditions. The more stable a generalization, the less dependent it is on background conditions to remain true. Although it is invariance – rather than stability – of generalizations that furnishes us with explanatory generalizations, there is an important function that stability has in this context of explanations, namely, stability furnishes us with extrapolability and reliability of scientific explanations. I also discuss non-empirical investigations of models that I call robustness and sensitivity analyses. I call sensitivity analyses investigations in which one model is studied with regard to its stability conditions by making changes and variations to the values of the model's parameters. As a general definition of robustness analyses I propose investigations of variations in modeling assumptions of different models of the same phenomenon in which the focus is on whether they produce similar or convergent results or not. Robustness and sensitivity analyses are powerful tools for studying the conditions and assumptions where models break down – and they are especially powerful in pointing out reasons as to why they do this. They show which conditions or assumptions the results of models depend on.

Key words: ecology, generalizations, invariance, lawlikeness, philosophy of science, robustness, explanation, models, stability

CONTENTS

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The dissertation is dedicated to my wife and sons.

INTRODUCTION

The question at issue in this dissertation is the epistemic role played by ecological generalizations and models. I investigate and analyze such properties of generalizations as lawlikeness, invariance, and stability, and I ask which of these properties are relevant in the context of scientific explanations. I will claim that there are generalizable and reliable causal explanations in ecology by non-lawlike and "contingent" generalizations, which are invariant and stable. I also discuss non-empirical investigations of models that I call robustness and sensitivity analyses. Robustness and sensitivity analyses are claimed to be powerful tools for studying the conditions and assumptions where models break down – and they are especially powerful in pointing out reasons as to why they do this. They show which conditions or assumptions the results of models depend on. The dissertation contains six chapters, outlined below.

For some time there have been debates among philosophers of biology and biologists about whether the biological sciences have laws of their own. Recently, the debate surfaced among ecologists. Traditionally, laws have been defined by philosophers as *true* and *lawlike* regularities expressed in the sciences as generalizations. Most ecologists participating in the debate have not defined laws. Nevertheless, there are some definitions in the ecological literature.

In chapter 1, I review these definitions along with ecologists' reasons or justifications for finding the debate interesting. I argue that ecologists' definitions of laws and their justifications for the laws debate are insufficient. A practically relevant, historically influential, and strong justification for this debate is then suggested, namely, the idea that laws are necessary for scientific explanations. The idea of the so-called covering law account of scientific explanation is that phenomena are explained when they are shown to happen in accordance with the laws, and therefore these laws should be mentioned in explaining the phenomena. In this sense laws are essential, indispensable, or necessary to scientific explanations.[1] Thus, two central issues have been treated carelessly in the ecological literature on the laws debate: accounts of ecological laws and justifications for them. This is unfortunate, since such negligence effectively prevents a resolution of the debate.

In fact, there do not seem to be many, if any, laws in ecology – or in biology in general, and there are even many arguments purporting to show the absence of biological laws. In chapter 2, I analyze nine such arguments, which take advantage of various putative distinctive features of biological generalizations or phenomena, such as their reference to particulars, their conceptual imprecision, their exceptions, history, uniqueness, complexity, openness, multiple realizability, and contingency, which are held to be responsible for the absence of biological laws. I will claim that most of the arguments against biological laws are inappropriate or inconclusive for different reasons. Yet I also claim that there are two arguments that present important challenges to the existence of biological laws, namely, the argument according to which biological generalizations are riddled with exceptions, which is called their *weak contingency*, and the argument that biological generalizations lack the stability of laws, which is called their *strong contingency*. If my analysis is correct, then it follows that there are no biological laws owing to these contingencies. Accordingly, we have non-lawlike biological generalizations riddled with exceptions that hold only on limited spatio-temporal domains. Thus, there appear to be no biological generalizations that have the properties traditionally associated with laws.

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[1] The covering law account provides a strong justification for the laws debate. I do not claim that this justification is good; in fact, there are serious problems with it. My claim is that, given the apparent paucity of ecological laws, it is difficult to understand why ecologists should be engaged in the laws debate if no strong and practically relevant justification for the role of laws is given to begin with. Chapter 2 shows how this justification turns into a problem, because there appear to be no distinctively ecological (or biological) laws.

In chapter 3, I analyze (non-traditional) accounts of laws that tolerate exceptions. The accounts hold that biological generalizations could be lawlike despite their weak contingency or exceptions, because these generalizations are pragmatically or paradigmatically similar to laws, inference tickets, probabilistic laws, or *ceteris paribus* laws. I argue that the accounts that tolerate exceptions are not sufficient to salvage the lawlike status of biological generalizations, owing to various difficulties in the accounts. Another topic in chapter 3 is the distinction between two kinds of exceptions.

A widely held belief – one that I do not criticize – is that if an exception to a generalization can be explained, then it is not a disconfirming or falsifying *genuine* exception, but rather a harmless *apparent* exception. Another common belief is that the explanations for exceptions to generalizations come typically or exclusively from the lower levels of mechanisms and/or are stated in the vocabulary of the lower-level sciences. I argue that ecologists use concepts, causes, and mechanisms of the same level to explain away the exceptions to ecological generalizations. In other words, ecological generalizations can be corrected and made more precise, and their exceptions evidently can be explained within their own conceptual domain.

How are scientific explanations possible in ecology, given that there do not appear to be many, if any, ecological (or biological) laws? To answer this question in chapter 4, I present and defend an account of scientific explanation in which generalizations are explanatory if they are *invariant*. An invariant generalization continues to hold or be valid under a special change – called an intervention – that changes the value of its variables. According to this interventionist account of scientific explanation, causes are difference-makers that can be intervened upon to manipulate or control their effects. A generalization can be invariant and explanatory regardless of its lawlike status. There is no requirement that invariant generalizations should contain only purely qualitative predicate terms, be universal, maximally or highly stable, or belong to a systematic web of other generalizations, as many philosophers have suggested about laws

The interventionist account is a counterfactual account of scientific explanation that shows what would happen to variables of generalizations during interventions: how the value (or probability distribution) of Y would change during intervention in the value of variable X. What is needed for explanations are *active* counterfactuals in the form, "if the value of the variable X of a generalization $Y_i = f(X_i)$ were changed by an intervention from x_1 to x_2, then the value of the variable Y would be changed from y_1 to y_2 in accordance with the relation $Y_i = f(X_i)$." The "stability" of a generalization under interventions in its variables is what matters in explanations. The ability to remain true under active – rather than passive – counterfactuals distinguishes explanatory generalizations from non-explanatory ones. Explanations describe and exhibit how the phenomena described in *explananda* systematically depend upon the changes of *explanantia*. Invariant generalizations with their support of active counterfactuals provide answers to "what-if-things-had-been-different" questions that provide us with understanding and explanatory information about how the *explanandum* would have changed had we changed the initial or boundary conditions of an invariant generalization to "such-and-such."

I apply the interventionist account to ecological generalizations to show that invariance under interventions as a criterion of explanatory relevance yields interesting interpretations of the explanatory status of many ecological generalizations, such as allometries, scaling laws, and the **species–area** rule (see appendix). Ecologists should find this interesting, because some of them rely only on (unreliable and implicit) intuitions, when deciding by what criteria the explanatory status of ecological generalizations should be evaluated. Chapter 4 also reaches the conclusion that the debate about ecological laws is a red herring, not because lawlikeness as a concept is elusive or does not apply to biological generalizations, but because the justification for the debate can be questioned.

In chapter 5, I analyze such properties of generalizations as scope and stability. Although stability and scope seem to deal with the generality of generalizations, I argue that they are different. Scope deals with a generality that has to do with the *actual distribution or range* of (dis)similar systems to which a generalization applies or has applied. Stability deals with a generality that has to do with holding of a generalization in *possible background conditions*. For example, both of

Mendel's rules or laws (see appendix) have (nearly) all sexually reproducing taxa as their scope. Conditions, such as the evolution of mitosis and meiosis on which Mendel's rules depend for their validity, are the background conditions in the stability sense. Biological generalizations typically generalize about different taxa, features, entities, and so on in different times and/or places. Scope captures the idea that biological generalizations are distributed in their applicability to different taxa, places, epochs, and so on. Many biological generalizations evidently have narrow or limited scope, whereas laws are often understood to have unlimited scope. The same goes for stability: laws are typically thought of as maximally stable generalizations. The important point is that a generalization with a narrow scope could have a high degree of stability within this scope. The converse could also be true: a generalization that has a broad scope could have a low degree of stability within its scope.

In contrast to what previous authors have said, I suggest that stability is not a monolithic property of generalizations, but that there are several different, yet related concepts or meanings for stability. I then use this as a reason to criticize the idea that stability should be identified with lawlikeness. Likewise, I argue that stability differs from invariance. There are stable generalizations that are non-invariant, such as the many examples of correlations between joint effects of a common cause, which can be highly stable in the sense that they hold in many different background conditions. Nevertheless, these examples are non-invariant as generalizations.

In chapter 5, I also discuss the different functions of stability and scope in the context of "generalizing" scientific explanations. Stability is an important property of generalizations insofar as their extrapolability and reliability are concerned, because stable generalizations furnish us with support of *passive* counterfactuals that have the form "had this-or-that background condition been different, a generalization would still have held." Let us call the stability domain of a generalization the range or extent of background conditions or interferences over which it supports passive counterfactuals. In general, the larger the stability domain of a generalization, the more passive counterfactuals it supports, and accordingly, the more extrapolable and reliable the generalization 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. In this sense stable generalizations function as many philosophers believe only laws function. Scope has a different, yet related function: it unifies and systematizes (causal) knowledge. Although it could be claimed that unification is not what makes explanations explanatory, unification can nevertheless be viewed as a serendipitous feature of causal explanations. Unification, for instance, permits economical ways of presenting explanations.

Consequently, I argue that we have ecological generalizations that are generalizable or extrapolable in explanations and that unify or systematize our knowledge. Invariant and stable generalizations with varying scopes function in the manner described above despite the fact that they need not be lawlike generalizations and despite the fact that they are weakly and strongly contingent as generalizations.

While the earlier chapters deal with generalizations, chapter 6 takes up scientific models and their non-empirical investigations.

Recent philosophical discussion centers on Richard Levins' ideas of scientific modeling in biology and robustness analysis, which date back to the 1960s. Chapter 6 contributes to this discussion by providing a sense of robustness analysis – which I call *sufficient parameter* robustness – that has so far been neglected in the literature. Another sense of robustness analysis – which is called *derivational* robustness – has been the focus of previous authors. Moreover, a third non-empirical form of investigation of models – which I call *sensitivity* analysis – is distinguished from the two robustness analyses. I call sensitivity analyses investigations in which one model is studied with regard to its stability conditions by making changes and variations to the values of the model's parameters. As a general definition of robustness analyses, I propose investigations of variations in modeling assumptions of different models of the same phenomenon in which the focus is on whether the models produce similar or convergent results.

Robustness and sensitivity analyses are powerful tools for analyzing which conditions or assumptions the results of models depend on. They also have different functions. The purpose of sensitivity analyses is to show something about the stability conditions of *a* model. The purpose of derivational robustness analysis is to investigate whether the results of similar models depend on certain modeling assumptions about the models. The purpose of sufficient parameter robustness analysis is to serve as a defense of abstract, unifying, and simple models. Although robustness and sensitivity analyses are non-empirical investigations of models, they could help in testing and confirming models, since they have the potential to show which stability conditions and/or modeling assumptions the results of models depend on. In this sense, robustness and sensitivity analyses allow one to move toward truer or realistic ecological models and theories.

In the special sciences the strategy to use a *diverse* set of simple, robust, and abstract models to study complex phenomena is widespread. Ecological theories can be seen as collections of such models. Theories of competition, niche, diversity, and abundance of species and populations form a more or less coupled set of simple and abstract theoretical ecological models that are developed and elaborated when new models for phenomena get proposed and when the models and their results are studied via robustness and sensitivity analyses.

In these pages I sometimes speak of regularities and generalizations as interchangeable. Strictly speaking, this is inaccurate, since generalizations are statements or expressions of regularities that are (claimed to be) objective uniformities in nature.

In the appendix, a list of ecological and paleobiological generalizations and models to which I refer to in the text are briefly described along with some references. These generalizations and models are arranged in alphabetical order according to the portion of their terms in bold.

1

ECOLOGICAL LAWS

1.1 The Ecological Laws Debate

Recently, ecologists have begun to raise the question of whether there are distinctive ecological laws (see Loehle 1990; Peters 1991; Lawton 1999; Murray 1999, 2000, 2001, 2004; Turchin 2001; Bednekoff 2003; Berryman 2003; Colyvan & Ginzburg 2003; O'Hara 2005; Owen-Smith 2005). Yet the question is not a recent one. Many views, examples, and arguments for and against the existence of biological laws have been presented in the philosophical and biological literature since the 1950s. For instance, Bernhard Rensch (1959, 1960) believed that evolution is governed by laws. He enumerated some fifty of "the most important" evolutionary laws. Some of his examples were regression equations, for example, concerning the ontogenetic development of different body parts and organs in different taxa. Still other examples were evolutionary and ecological rules, such as the rules proposed by **Allen**, **Bergmann**, **Cope**, **Dollo**, **Gloger**, and **Williston** (see appendix), as well as other evolutionary, developmental, and genetic rules, such as the rule of **natural** selection, generalizations concerning the tempo and the intensity of natural selection, **Mendel's** rules, and so on.

In the literature on the laws debate, the trend – at least among biologists – has been to present examples of generalizations as laws. Ecologists have made no exceptions to this trend (cf. section 2.3). Yet such a trend perhaps reverses the more fruitful order of things: rather than presenting generalizations as examples of laws, one should first give *accounts or definitions* of laws, otherwise it is difficult to compare and/or evaluate the generalizations with regard to their lawlike status.

Although the laws debate has recently become popular in ecology, there seems to be few shared reasons or justifications among ecologists as to why they should engage in this debate. Moreover, the debate over the existence of laws is perhaps not that interesting when taken as an abstract question, at least for scientists who are tackling "practical" scientific issues and problems – in contrast to more "philosophical" ones. In other words, some role has to be singled out as the central role for laws in the present context. Without this, the debate has no interesting or relevant justification.

Thus, I suggest that two important issues *prior* to the ecological laws debate have been treated carelessly: the definitions of laws and the justifications for them. This is unfortunate, since such negligence effectively prevents a resolution of the debate.

I proceed as follows. The next section distinguishes laws from "accidentally true" generalizations, where I argue that the central property of laws is their lawlikeness rather than their truth. Section 1.3 presents some traditional accounts of laws defining lawlikeness more accurately. In section 1.4, I offer a critical review of the accounts of laws that have been given by ecologists. A common misconception is that ecologists identify the truth with lawlikeness. Another difficulty is that their accounts fail to commit themselves to any particular view or account of laws. Section 1.5 reviews the justifications of ecologists in the laws debate, which I found inadequate or secondary in importance. In section 1.6, I suggest that there is one practically important, influential, and strong justification for the laws debate, namely, the idea that laws are necessary for scientific explanations. The idea of the so-called covering law account of scientific explanation is that phenomena are explained when they are shown to happen in accordance with the laws, and therefore these laws should be mentioned in explaining the phenomena. In this sense laws are essential, indispensable, or necessary to scientific explanations. Section 1.7 briefly discussed the main problems of the covering law account. Section 1.8 concludes.

1.2 Laws versus Accidentally True Generalizations

According to a general traditional philosophical account, laws are *lawlike and true regularities or uniformities expressed as generalizations* in the sciences.

A regularity is often in this context represented by a statement in a universal form, as in "All *F*s are *G*s," which can be represented in formal and conditional form as $\forall x(Fx \rightarrow Gx)$, where $\forall x$ is a universal quantifier (read "For all *x*"), *F* and *G* are predicate terms, and → is a logical connective for "if, then." Generalizations in this form express the idea of uniformity: "For all *x*, if a thing *x* is an *F*, then *x* is also a *G*." In other words, all objects with the property *F* also have the property *G*. For example, "All ravens (*F* predicate) are black (*G* predicate)." Since laws are universal, they brook no exceptions: all *F*s are *G*s in every place and in every time. If all *F*s are not *G*s, then the generalization is falsified or disconfirmed.[2]

Laws are not only universally and conditionally expressed true generalizations, because this syntactic or formal requirement is a necessary condition of laws, not a sufficient one. In other words, there are true, but non-lawlike generalizations that can be expressed as statements in the above form. Let us call these *accidentally true* generalizations in contrast to lawlike and true generalizations, that is, laws.

Lawlikeness is the distinctive and proprietary property of laws. Note that the truth and the lawlikeness of a generalization are not the same things. Carl G. Hempel and Paul Oppenheim (1948: 153) put this as follows:

> Apart from being true, a law will have to satisfy a number of additional conditions. These can be studied independently of the factual requirement of truth, for they refer, as it were, to all logically possible laws, no matter whether factually true or false. Adopting a convenient term proposed by Goodman…, we will say that a sentence is lawlike if it has all the

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[2] In this chapter I discuss accounts of laws according to which these are – or from which follow – universal and exceptionless regularities. Other accounts of laws are dealt with subsequently.

> characteristics of a general law, with the possible exception of truth.

So laws are expressed as true generalizations, but not every true generalization is a law. The central issue of any account of laws is to give a clear demarcation between lawlike generalizations and accidentally true generalizations.

Consider two true, conditional, and formally universal generalizations. The first is, "all persisting lumps of pure gold have a mass less than 1,000 kg," which I presume is true, but is not a law. Contrast this with a putative law: "all persisting lumps of pure uranium-235 have a mass less than 1,000 kg." It is a coincidence that nobody has amassed gold into a lump weighing over 1,000 kg, although nothing precludes that such an event should not be true. Yet physical laws, namely, the laws of nuclear chain-reactions, bar of existing a lump of uranium isotope 235 with a mass of 1,000 kg, because this is much greater than the critical mass of the isotope, which is around 50 kg, depending on the shape and density of the lump. So the uranium-235 generalization seems to be true because of nomic necessity, that is, a law, whereas the gold generalization is true only by an accident.

Lawlikeness is not only the distinctive property of laws; it is also the property that is thought to give laws their support in counterfactuals or subjunctives. Counterfactuals are contrary-to-fact conditional statements of a form, "if this *a* were an *F*, then *a* would have been a *G*." Laws underwrite counterfactuals: if it is a law that all ravens are black, then "if this Blue-Headed Pionus had been a raven, it would have been black" is true or underwritten by that law.

Counterfactual support of laws tells how laws govern not just what actually happens, but what would have happened under certain background conditions had they happened. In other words, laws do not just describe what happens in the world, but they also "dictate" or tell what would happen had the conditions been such-and-such or this-and-that. That laws support counterfactuals is what gives laws explanatory and predictive power, modal force, and necessity – or least it illuminates how they get these qualities. Accidentally true generalizations do not have the above abilities. At most, they tell or describe what actually happens or happened.

Although laws are typically defined as lawlike and true generalizations, lawlikeness is a surrogate for a concept that needs to be defined. In the next section I present some traditional accounts of laws that furnish us with definitions of lawlikeness.

1.3 The Regularity and Universal Accounts of Laws

According to the so-called regularity account, laws are true regularities + lawlikeness. According to the "naïve" regularity account, lawlike and accidentally true generalizations can be distinguished, because the former contains essentially universal predicate terms (also called purely qualitative predicate terms or natural kind terms). In other words, lawlike generalizations make no essential reference to any particular, that is, to individual place, time, spatio-temporal location, proper name, object, or event (see Hempel & Oppenheim 1948: 152-157, Nagel 1961: 47-78, and Hempel 1965: 264-270, 291-293). In effect, every accidentally true generalization includes – at least implicitly – some spatial reference (as in "this generalization holds true only on the earth"), temporal reference (as in "this generalization ceased to hold after the Eocene, but was true before it"), includes some other restriction, or refers essentially to some particular individual, proper name, thing, place, or object in predicate terms. Acceptable predicate terms for laws concern such things as mass, predator, prey, competition, electric charge, acceleration, cold climates, money, metal, and body size.

The above criterion is intended to apply to fundamental laws, whereas derivative laws are permitted to contain predicate terms referring to particulars. Although Kepler's laws of planetary motions and Galileo's law of free falling refer to particulars, such as the moon, the earth, and the sun, they could count as derivative laws according to the above definition of the naïve regularity account. Newton's laws of motion and gravitation are the fundamental laws from which both Kepler's and Galileo's laws are derived from.

The naïve regularity account seems nicely to capture the idea of laws being general and universal uniformities. However, the account is in trouble. In fact the definition of lawlikeness based on differences in predicate terms does not distinguish between the gold and uranium generalizations given above. According to this naïve account both generalizations evidently represent (fundamental) laws because they are true and contain no forbidden predicate terms of the kind given above. This suggests that the naïve regularity account fails to provide an account of lawlikeness, since it fails to distinguish between laws and non-laws.

There are other accounts of lawlikeness given by other regularity theorists. For instance, a more "sophisticated" version of the regularity account – the so-called Mill–Ramsey–Lewis account (see Lewis 1973: 72-77) – holds that laws are axioms or theorems in the best(s) deductive system(s) of the knowledge of the world. Laws are generalizations belonging to the deductive system of propositions all of whose members are true, where such a system achieves the best balance between strength (or information content) and simplicity.

On this account, lawlikeness is not something a generalization possesses in isolation, but something that it possesses as a member of a system of other generalizations. The Mill–Ramsey–Lewis account has a better chance of incorporating the difference between the gold and uranium generalizations than does the naive account: It could be argued that the gold generalization does not belong to any deductive systematization of knowledge that balances simplicity and strength. The uranium generalization could belong to such a system that includes, say, quantum physics, nuclear physics, and other such areas of physics. This difference makes the latter a law and the former not.

Do biological theories and their generalizations have the theoretical structure required by the Mill–Ramsey–Lewis account? I would argue that they do not. The problem is especially pronounced in ecology, where evidently there is only one theoretical structure of the above kind, namely, the competition theory. Unfortunately, it is not even clear that the competition theory has the required structure. Rather, it is a structure whose generalizations have loose, unsystematic, vertical, and horizontal connections, that is, a theory whose structure resembles real webs rather than the fantasy of a logician (cf. also Schaffner 1980). Moreover, there is concern that systematic connections have nothing to do with lawlikeness, but there could be isolated laws (see also Psillos 2002: 148-149 for a similar criticism). Thus, it could be argued that systematization or adding strength or simplicity to a theoretical system is neither necessary nor sufficient for lawlikeness. Systematization of laws seems to be a contingent fact that should not be confused with the criterion of lawlikeness. The Mill–Ramsey–Lewis account has faced still other problems questioning its status as a coherent account of laws, which I shall not discuss (see, for instance, Armstrong 1983: 66-73, van Fraassen 1989: 40-64, Carroll 1994: 44-55, Psillos 2002: 148-158).

Still other regularity theorists refer to the evidential status of a law, to epistemic things, roles, attitudes, or even to pragmatic, subjective, and psychological things as distinctive of laws (cf. Braithwaite 1927, Goodman 1954, Spohn 2002). These are not dealt with here, since I am looking for objective criteria. As suggestions, they seem to be too relativistic. Some of them moreover confuse the roles or justifications for laws with the criterion of lawlikeness.

There is another account, which is neither a regularity account nor an empiricist account, but a necessitarian account. According to the regularity account, laws are special uniformities or regularities that describe the world accurately and truthfully throughout time and space. Lawlike generalizations or regularities are not in any strict sense necessary. Necessitarians, however, insist on the contrary.

According to one popular necessitarian account – the so-called universal account –, laws are *nomic necessity relations holding between universals* (see Dretske 1977, Tooley 1977, Armstrong 1983). Necessity here is not logical, but physical, nomical, or even metaphysical. In other words, universals necessitate other universals in laws: if it is a lawlike regularity or generalization that "All *F*s are *G*s," then universal *F* (e.g., "being uranium of isotope 235") necessitates universal *G* (e.g., "being a lump not exceeding the critical mass of about 50 kg"). The account also has it that laws *entail* exceptionless and universal regularities, but that laws themselves are not regularities. Accidentally true generalizations do not follow from relations between universals, but are just expressions of mere regularities.

Universals are thought to be unifying, general, and explanatory properties, such as having mass, being a proton, or a predator, which can have many spatiotemporal instances and which are connected via nomic relations, namely, laws.

The universal account of laws, I assume, would distinguish between the gold and uranium generalizations by simply claiming that as a matter of fact there is no necessity relation between the properties "being a lump of gold" and "being less in mass than 1,000 kg," whereas in the case of the uranium generalization, there is such a relation (which has been shown or found empirically). Nevertheless, one wants something more illuminating than such a fiat from a definition of lawlikeness. The problem is that there are no noticeable differences between the gold and uranium generalizations, but both seemingly refer to universals. Or at least I am not able to see any difference between them in this respect. So the universal account has to offer a better account of what distinguishes between universals and things that are not universals that figure in non-laws, because this grounds the distinction between laws and non-laws (see also Psillos 2002: 171-172 for a similar criticism).

The lesson to be drawn from the above short survey of different accounts of laws is that lawlikeness is elusive as a concept. In fact, at present there is no satisfactory account of laws, although the subject has kept philosophers busy for decades. However, the point of this section was not to defend any specific account of laws, but to show that there are different accounts of laws in the literature.

The regularity account is often treated as the received account of laws. For the sake of argument, let us suppose that the (naïve) regularity account of laws got the definition of laws right. I adopt the regularity account of laws for the following reasons. Although there are difficulties with the different versions of the regularity account (see, for instance, Armstrong 1983: 11-73 and Psillos 2002: 137-158), other accounts have their own, at least equally serious, difficulties. Moreover, the universality account discussed above has difficulties in dealing with probabilistic laws (van Fraassen 1987), whereas the regularity account does not. An account that allows for probabilistic laws could salvage the lawlike status of generalizations that are riddled with exceptions, whereas an account that has difficulties in dealing with such laws is more likely to run into problems with generalizations riddled with exceptions. This speaks for the regularity account, owing to the fact that biological generalizations are riddled with exceptions (see section 2.3). Moreover, that I adopt the regularity account here has no important consequences given that I focus first and foremost in this book on the epistemic function of laws rather than on different accounts of them. The fact is that different accounts of laws view their function similarly, namely, that laws furnish us with reliable and generalizable explanations. My criticism in the next section of the accounts of laws given by ecologists is likewise not affected by the issue of what is the correct account of laws. In other words, I argue that the problem with the accounts of laws by ecologists is not that the authors adopt the wrong account of laws, but that ecologists' accounts are not accounts of laws to begin with, because they are too non-committal and/or they identify truth with lawlikeness. In other words, I adopt the regularity account of laws at least partly for convenience rather than because it is the correct account.

1.4 Ecologists on Laws

In lieu of defining laws, most ecologists just present generalizations as examples of laws without giving much attention to laws' defining features. Fortunately, there are a few accounts of laws in the ecological literature.

Although the necessatarian and regularity accounts differ, what is common to both is either that laws are exceptionless and universal regularities or that from laws follow such regularities. As it happens, this is something that evidently all ecologists reject in their accounts. This is not the crucial problem of their accounts, since there are accounts of laws that tolerate exceptions (see chapter 3).[3] A common misconception in ecologists' accounts of laws is that they identify

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[3] On the other hand, traditional biological literature on laws admits that laws are universal and then laments that biological generalizations lack this feature (see Mayr 1956; see also Murray 2000, 2001, 2004).

truth with lawlikeness. At the same time their accounts are conflations of unrelated ideas or accounts of laws and/or too non-committal to be successful as accounts of laws.

For example, John N. Lawton (1999: 177) writes:

> Of course there are general laws in ecology. However, our science has rather few *universal* laws. My dictionary gives several different definitions of the word law. The most appropriate is: "Generalized formulation based on a series of events or processes observed to recur regularly under certain conditions; a widely observable tendency". Notice that there is nothing in this definition to say that a law has to be universally true; only that laws are usually true.

As an account, the above manages to be a conflation of unrelated ideas or accounts of laws. On the one hand, the account suggests that laws are tendencies (the second part of the account). On the other hand, it suggests that laws are regularities (the first part of the account), which is a different thing. Moreover, Lawton's account evidently misidentifies lawlikeness with truth, as the last sentence of the quoted passage suggests.

A. A. Berryman (2003: 695) trusts also partly in a dictionary definition of laws:

> Webster's New World Dictionary defines... a *law of nature* as "a sequence of events… that has been observed to occur with unvarying uniformity under the same set of conditions", or "an exact formulation of the principle operating" in an observed regularity… It is important to realize that laws usually have conditional constraints, in the sense that they are obeyed *under a certain set of conditions*. Thus, laws should not be expected to hold under all possible circumstances, something that is not always appreciated…

Berryman suggests that laws are regular – and perhaps even regularities – under "certain conditions," but are not necessary regular outside of those conditions. The problem is that this account is so non-committal that it tells next to nothing about what laws are. In other words, Berryman's account is consistent with very different accounts of laws, for instance, with laws being regularities that hold "*ceteris paribus*." His account is also consistent with an account that laws are not about regularities at all, but about dispositions or tendencies of things that need not manifest in (observed) regularities if the conditions for manifestation are not present. Furthermore, qualifying laws with protective clauses, such as that the laws hold only under certain conditions, leads to semantic, epistemic, and empirical problems in accounts of laws that are unresolved (see section 3.5).

The philosopher of science, Marc Lange (2005a: 395), has criticized Berryman's and Lawton's accounts on the grounds that both make any *true* generalization a law, whether it is lawlike or not. Lange also seems to propose that Berryman and Lawton think of laws as being regularities. Lange's points seem apt, because at least Lawton and perhaps even Berryman seem mistakenly to equate lawlikeness with the truth. Consequently, their accounts fail to demarcate between accidentally true generalizations and laws; for instance, Berryman's and Lawton's accounts evidently wrongly treat the gold and uranium generalizations presented above as the same, namely, as laws. At the same time Lange's points seem wrong. I do not find anything substantial in the above passages to propose that laws are regularities. Quite the contrary. Lawton's and Berryman's accounts are non-committal on what laws are. As accounts, they are useless, since they fail to provide meaningful criteria for lawlikeness.

R. B. O'Hara (2005: 391) has likewise come up with a definition of laws:

> They [i.e., philosophers] typically declare that a statement needs to overcome two hurdles before it can be considered a Law... Firstly, it has to have a natural necessity – i.e. it has to be true because of the way the world is. This means that it has to have some empirical content: a mathematical theorem is not a Law of Nature because it is true by virtue of logic. Secondly, the law has to be essential scientifically, i.e. it has to be used.

I would disagree with both of O'Hara's "hurdles." The first seems to confuse lawlikeness with truth. Or possibly it refers to some unanalyzed notion of necessity making up lawlikeness. However, it is not the first hurdle I am worried about.

It is the second hurdle, which has problematic consequences owing to its relativistic and unorthodox characterization of lawlikeness. O'Hara is evidently suggesting that the lawlike status of, say, **Gause's** rule (see appendix) or Kepler's laws depends on the scientific use. However, it is not clear that the use of laws has anything to do with their lawlikeness. Moreover, O'Hara does not give any clue about what he means by using laws. Does he mean that laws are used to manipulate or control nature? Or does using laws mean successful (theoretical) application? O'Hara's ideas about lawlikeness – if taken literally – also lead to unacceptable relativistic, contingent, or unstable ideas about what laws are. For instance, suppose that Kepler's laws were "used" in their heyday. Consequently, O'Hara's characterization of laws imply that Kepler's laws were physical or astronomical laws at the time, but not anymore, because they are not used in theoretical physics today if more than an approximation is needed. Alternatively, it implies that nowadays Kepler's laws are laws of engineering, instead of laws of physics or astronomy, since they are used (more) in those disciplines.

If O'Hara's conditions are not individually sufficient for laws, could they be both individually necessary and jointly sufficient? The answer to this is in the negative. There appear to be generalizations for controlling, manipulating, predicting, and explaining phenomena. These are stable and invariant generalizations that need not be lawlike generalizations (see chapters 4 and 5).

1.5 Ecologists on the Justifications for the Laws Debate

Although the laws debate has recently become popular in ecology, there seems to be few shared reasons or justifications among ecologists as to why they should engage in this debate. To my knowledge, the present section is the first explicit attempt to examine the justifications of ecologists for the laws debate as well.

For some ecologists, the debate seems to be only about terminology or is of little consequence (see Bednekoff 2003 and Colyvan & Ginzburg 2003: 651, 652). My view is that there has to be some justification for carrying on this debate. Otherwise, it is difficult to understand why ecologists argue for or against laws given that it is not clear that there are ecological laws, at least if these are understood as universal and exceptionless regularities (see also chapter 2).

Some think that once we have identified a small body of general laws, it becomes possible to formulate an axiomatic and deductively systematized scientific theory, which is consider a significant project and important for explanations (see Berryman 2003: 695, 696, 700). It is not clear exactly why this would be an important reason or justification for the laws debate, at least where practical matters of ecology and ecologists are concerned. Also – as was argued above – whether this project can implemented in ecology can be questioned. Some ecologists refer to related, but more obscure ideas, such as the notion that laws bring forth ecology's "coherence," "consistency," "theoretical connections," or "resemblance" to physical and/or other biological sciences (see Turchin 2001: 18-19, 24, Colyvan & Ginzburg 2003: 649, Owen-Smith 2005: 611).

Others highlight the importance of conservation and management problems and their solutions when it comes to ecological laws (see Lawton 1999 and Owen-Smith 2005). This justification is problematic on two scores. First, it needs to be clarified how and why *laws* – rather than something else – help with these problems (see, for instance, Shrader-Frechette & McCoy 1994), and, second, it needs to be shown that *ecological* laws help, because there are doubts about whether ecology can contribute solutions to these problems (cf. Sagoff 1985).

For some, the justification is connected with the scientific status of ecology and/or the demarcation between the sciences and pseudo-sciences (see Murray 1999, 2001 and Colyvan & Ginzburg 2003: 649). Another, more interesting justification is that biology is not just an application of physics or chemistry, but is an autonomous explanatory endeavor with scientifically respectable, empirically testable, and explanatory laws of its own. This justification has been put forward mostly by *philosophers* (see Smart 1963: 50-63, Rosenberg 1985: 126, and Lange 2005a: 400-402). Although it is an important justification, ecologists do not usually and explicitly refer to it in papers.[4]

Robert Henry Peters (1991) and Bertram G. Murray (1999, 2000, 2001, 2004) have highlighted the importance of predictive and rigorously tested ecology in the context of the ecological laws debate. What differentiates these authors from other ecologists is that they articulate their justification for the debate by explicitly referring to an account of scientific explanation (see the next section). As I will argue below, there is an important and (historically) influential justification for the laws debate in ecology having to do with the role of laws in scientific explanations, a justification also stressed by Peters (*ibid.*) and Murray (*ibid.*).

The justifications of some other ecologists alluded to above are often of secondary importance in the sense that by explicating what scientific explanations consist of and what is needed to have these, these latter justifications are analyzed or even eliminated. For instance, with the right account of scientific explanation, we should be able to obtain answers to the questions of what is the value of unification in explanations, how are autonomous biological explanations possible, how do scientific explanations differ from non-scientific or pseudo-scientific explanations, how is it possible to control and manipulate nature, and so on.

1.6 The Covering Law Account of Scientific Explanation

According to the so-called covering law account of scientific explanation, a phenomenon is explained by showing that some law covers it as a special case. In a word, the phenomenon is explained by

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[4] It is true that nearly every participant in the ecological laws debate reminds us that ecology should supply accurate predictions. For instance, John H. Lawton (1999: 178, 179) recognizes that laws and generalizations are useful and important for understanding nature and that these also matter for predictions. Unfortunately, hardly any of them – with the two exceptions to be mentioned below – say anything more substantial about this or about the role of laws in the context.

subsuming it under laws. These ideas apply not only to explanations of particular phenomena or events, but also to explanations of generalizations and laws: the latter are explained or subsumed by other more general laws.

The idea of covering law account is that phenomena are explained when they are shown to happen in accordance with the laws, and therefore these laws should be mentioned in explaining the phenomena. In this sense laws are essential, indispensable, or necessary to scientific explanations. Carl G. Hempel's (1965; see also Hempel and Oppenheim 1948) account of scientific explanation is the received or at least the most famous version of covering law accounts.

With the term the *explanandum* (pl. *explananda*) I am referring to *the object of explanation*, whether the object is a law, a generalization, a particular phenomenon, or something else. With the term *explanans* (pl. *explanantia*) I am referring to *the thing that does the explaining*. The *explanandum* and the *explanans* are general terms describing the things that explanations consist of: an explanation is an *explanandum* + an *explanans*.

To Hempel, explanations are arguments in which the conclusion follows either deductively or inductively from the premises. Since explanations are arguments, *explanantia* work as premises for *explananda*, which are the conclusions of the arguments. The *explanans* consists of two kinds of sentences: one contains statements of laws and the other describes antecedent (or boundary) conditions. The *explanandum* is a sentence describing the phenomenon to be explained.

Antecedent conditions are the particular and contextual explanatory conditions needed to be taken into account in order for the law to be applied to a situation. Consider Newton's law of gravity. The law states that the gravitational force between two bodies with masses m_1 and m_2, separated by a distance, r, is $F = gm_1m_2/r^2$, where g is the gravitational constant. An explanation citing this law specifies as the antecedent conditions that the masses of two bodies are "such-and-such" and that their distance is "this-or-that." Antecedent conditions thus concern auxiliary information that is context-specific in nature and is represented in singular statements, such as “this specific body has a mass of 1,230 kg.” These conditions should not include irrelevant explanatory details, such as the two bodies are “yellow in color” and that one of them is “a statue of Gary Gygax.”

For Hempel, the *explanandum* is explained and made intelligible by showing that it was to be expected given the *explanans*: one could infer from the premises (the *explanans*) that the *explanandum* follows deductively or that the *explanans* gives a high probability to the occurrence of the *explanandum*. Explanations function to remove any initial surprise and replace it with nomic expectations about the *explanandum*.

Hempel (1965: 334-347, 376-393) distinguished three specific accounts of scientific explanation, namely, the deductive-nomological account (the DN account, for short), the deductive-statistical account (the DS account), and the inductive-statistical account (the IS account). The idea of an explanation or argument of a DN account kind is to show that an *explanandum* follows deductively from laws and antecedent conditions adduced in its *explanans*. The conclusion follows deductively from the premise, because the laws in a DN explanation are universal and allow no exceptions. The DS account is a special case of the DN account. In a DS explanation, a statistical or probabilistic regularity or law is deduced from another law, which is broader in scope. Thus, both DN and DS explanations are deductive arguments. The IS is an account for statistical or inductive explanations of particular phenomena. In an IS explanation, laws that are doing the explaining of an *explanandum* are not universal, but statistical or probabilistic.

The DN account has the following logical form as an argument:

$L_1, L_2, \ldots, L_k$
$C_1, C_2, \ldots, C_j$

E

The sentence “$C_1, C_2, \ldots, C_j$” refers to antecedent conditions. The sentence “$L_1, L_2, \ldots, L_k$” describes law(s) needed to explain the *explanandum* sentence, “E.”

As an example, imagine that the statement “all ravens are black” is a universal law having the form $\forall x(Fx \rightarrow Gx)$. Suppose that we find that a particular thing, *a*, is a raven (*a* is an *F*, where the predicate term *F* is represented in its instantiated form, *Fa*). If this *a* is a raven, then we can deductively explain that *a* is also black (*a* is also a *G*, *Ga*) in an argument that has the logical form of a DN account:

$$\begin{array}{l} \forall x(Fx \rightarrow Gx) \\ Fa \\ \text{----------------} \\ Ga \end{array}$$

The IS account has the following logical form as an argument:

$$\begin{array}{l} L_1, L_2, \ldots, L_k \\ C_1, C_2, \ldots, C_j \\ \text{----------------} \\ \text{----------------}\ [r] \\ E \end{array}$$

The double line indicates that we have an inductive argument in which the conclusion follows from the premises with a probability, $[r]$. IS explanations differ from deductive arguments of the DN and DS kind in that their *explanantia* contain at least one statistical or probabilistic law, $Pr(Gx/Fx) = r$, from which the conclusion follows with a high probability rather than with a deductive certainty.

Hempel (1965: 301-302, 381-390) maintained that IS explanations should make their *explananda* highly probable. This is natural, given that Hempel endeavored to give an account with his IS that furnishes good or valid inductive arguments that predict their *explananda* to be “expected.” The high probability requirement is a natural analog to the deductive certainty of the DN and DS account arguments as well.

Hempel (1965: 234, 249, 364-376, 406-410) believed in the symmetry thesis of explanations and predictions. According to this thesis, scientific explanations and predictions have the same logical form. Consequently, every legitimate scientific explanation is a potential scientific prediction; the reverse is likewise true. The thing that separates the two is a contingent difference in presentation. We have an explanation when the *explanandum* is known in advance. We have a prediction when the *explanandum* is unknown in advance.

The reason I have dwelled on Hempel's account is that it suggests an important justification for the ecological laws debate, namely, that the proprietary function of laws is to furnish us with scientific explanations. According to the covering law account, laws are an essential or indispensable part of *explanantia*. Thus, there is one (historically) influential justification for the laws debate, which is also strong enough, given the apparent paucity of ecological laws, to justify such a debate. This justification has practical relevance for ecologists as well.

1.7 Problems with the Covering Law Account

The problems with Hempel's covering law account have been known for decades and are discussed here (for a more detailed presentation of these problems, see Salmon 1984: 28-32, 1989: 46-60, 68-80; Kitcher 1989: 411-414; Ruben 1990: 181-208; Psillos 2002: 215-262; and Woodward 2003a: 152-186). However, Hempel's account has been retained owing to the lack of an adequate rival.

The main problems with Hempel's inductive-statistical (IS) account: Hempel admitted that the high probability requirement is an arbitrary condition of adequacy for IS explanations, since a definite numerical probability value cannot be assigned as to when the *explanans* confers a high enough probability to its *explanandum*. Rather than giving up this requirement, Hempel evidently retained it as a general and non-definite condition of adequacy for IS explanations. Moreover, as a condition, the high probability requirement seems to be counterintuitive. In other words, it is evidently possible to explain and understand phenomena with low probabilities. Consider the development of paresis – a condition that is associated with muscle weakness and paralysis – from untreated syphilis. Let us suppose that the latter is the (only) cause of the former. It then seems to follow that the latter is the cause that explains the presence of the former as well, although the presence of untreated syphilis makes the development of paresis unlikely. According to some estimates, only about 25 % of syphilitics develop paresis. Thus, we seem to have a satisfactory explanation for the development of paresis despite the fact that it does not satisfy the high probability requirement.

Here Hempel faced a dilemma. The high probability requirement should perhaps be dispensed with, but if the requirement is discarded, then Hempel's central idea of scientific explanations in the case of IS explanations goes with it, namely, the idea that the function of scientific explanations is to remove the initial surprise and replace it with nomic expectations about the *explanandum*.

The IS account is also plagued by the ambiguity of inductive-statistical explanations (see Hempel 1965: 53-67, 394-403), which is a special problem of distinguishing between explanatory relevant and irrelevant information that also plagues Hempel's deductive accounts. Valid deductive arguments of the DN and DS kind are monotonic: if a conclusion of a deductive argument follows from a set of true premises, then the same conclusion follows from a set of premises to which other (irrelevant) premises are added. This is not true of inductive arguments. Inductive arguments can lead to contrary and even contradictory conclusions from different, but true sets of premises. As an example, consider a patient who is suffering from a bacterial infection. If the patient is administered penicillin, then it is likely that she will recover from the infection. Moreover, the recovery of the patient is explainable given the probabilistic law that "people who are suffering from a bacterial infection and are given penicillin are quite likely to recover from that infection." However, if the *explanans* is changed to include the premise that "the patient's infection is the result of a stock of bacteria resistant to the penicillin administrated," then it becomes likely that the patient will not recover from the infection and may even die of it.

Let F denote a potentially statistically relevant assembly of facts for the *explanandum* G. The IS account required that $Pr(Gx/Fx) = r$ should be high in r value if the law in question is to function as an *explanans*. In the expression $Pr(Gx/Fx)$, G is the attribute class and F the reference class. The ambiguity of IS explanations has to do with how to partition the reference class so that the probability it gives to the attribute class and to the *explanandum* is homogenous and thus unvarying between partitions. If the probability value, *r*, that a reference class assigns to the attribute class varies between different partitions of *F* (with a partition F_1 we have a probability r_1 that *G* happens and with F_2 we have a different probability r_2 for *G*), then we have the situation depicted above with Hempel's ambiguity of IS, whereby even contradictory conclusions can be achieved by using different partitions of the reference class in the premises.

Hempel never satisfactorily resolved the problem of the ambiguity of IS explanations. Rather, he simply restated the problem in his "requirement of maximal specificity," according to which we should not include explanatory irrelevant information in the premises of IS explanations. In other words, Hempel's solution was not to search for objective homogenous partitions of reference classes in IS explanations, but to relativize IS explanations to the epistemic contexts, in which they are given. This "solution" has the possible effect that there are no objective and true IS explanations at all in contrast to DN and IS explanations (cf. Hempel 1965: 402-430 and Salmon 1984: 48-55).

The main problems with Hempel's deductive-nomological (DN) account: We can explain why the length of a *shadow* of a flagpole is what it is by deducing the information from the height of the flagpole, the elevation of the sun, the laws of the propagation of light, and by using elementary trigonometry. This is a satisfactory deductive-nomological (DN) explanation. The problem is that, according to the account, one can also explain why the height of the *flagpole* is what it is by deducing this from information that concerns the length of its shadow along with the elevation of the sun, the laws of the propagation of light, and so on. This explanation satisfies Hempel's requirements for a DN explanation, but it is an explanation in which the asymmetry of a causal explanation is denied: here the effects explain their causes rather than vice versa. The problem can be generalized to other cases. This suggests that the DN account fails to be sensitive to the asymmetric nature of (causal) explanations.

It is also possible to devise DN (and IS) explanations in which there is a covering law that includes irrelevant explanatory information. Consider such putative examples of covering laws as "all salt dissolves into hexed water" and "all males who regularly take oral contraceptives fail to get pregnant." The problem is that lawlike generalizations with irrelevant explanatory information appear to furnish us with valid DN explanations according to Hempel's account. Consider a husband who has regularly taken his wife's birth control pills. Using the law "all males who regularly take oral contraceptives fail to get pregnant" it can be explained and deduced that the husband does not get – or is not – pregnant. This is a satisfactory DN explanation that is a poor explanation. Trivially, it does not make any difference to their non-pregnancy whether or not men take birth control pills. Similarly, a covering law that "all salt dissolves into hexed water" can be used in a DN explanation to explain the dissolving of a lump of a salt into water, but again the fact that water is hexed is irrelevant to whether the salt dissolves.

Another way of expressing the force and content of the above problems is to paraphrase them as claims that Hempel's covering laws – or the generalizations' lawlikeness – do not give us the right relation of explanatory relevance. The central task of any account of scientific explanation is to describe what the relation of explanatory relevance is. Failing to do this amounts to an account that should be relinquished or seriously revised.

The DN account is typically used in the context of explanations of singular phenomena or events, but it was meant to cover explanations of regularities and laws as well. However, Hempel and Oppenheim (1948) have already noted that there is a problem when the DN account it used to explain a law with another one. The problem is that a law can be deduced from a conjunction of that law with another law that is irrelevant:

> The precise rational reconstruction of explanation as applied to general regularities presents peculiar problems for which we can offer no solution at present. The core of the difficulty can be indicated briefly by reference to an example: Kepler's laws, *K*, may be conjoined with Boyle's law, *B*, to a stronger law *K.B*; but derivation of *K* from the latter would not be considered as an explanation of the regularities stated in Kepler's law; rather, it would be viewed as representing, in effect, a pointless "explanation" of Kepler's laws by themselves. The derivation of Kepler's laws from Newton's laws of motion and of

> gravitation, on the other hand, would be recognized as a genuine explanation in terms of more comprehensive regularities, or so-called higher-level laws. The problem therefore arises of setting up clear-cut criteria for the distinction of levels of explanation or for a comparison of generalized sentences as to their comprehensiveness. The establishment of adequate criteria for this purpose is as yet an open problem. (Hempel & Oppenheim 1948: 159.)

This suggests that the scope of the covering law account should perhaps be restricted to explanations of singular phenomena or events rather than to laws.

The problems with Hempel's symmetry thesis: We can successfully *predict* the occurrence of storms by reading a barometer. Let us suppose that there is a deductive (or inductive) law that connects the correlation of factors "the occurrence of storms" and "readings on a barometer" that will hold in many different background conditions. In this case, the above prediction of occurrence of storms seems to amount to a DN (or IS) prediction. Given Hempel's symmetry thesis of explanation and prediction, we are forced to claim that the occurrence of storms can be similarly *explained*. This is false. The two correlating factors are joint effects of a common cause – namely, changes in the atmospheric pressure – that explain the occurrence or non-occurrence of storms and which is the cause of the joint effects correlating. The paresis case suggests that the reverse is not true: supposing that untreated syphilis is the cause and the explanation of paresis, it is nevertheless not possible to predict the occurrence of paresis from the presence of untreated syphilis, since few who suffer from untreated syphilis develop paresis.

The problems of Hempel's covering law account of failing to distinguishing between explanatory irrelevant and relevant information and being insensitive to asymmetries in explanations seem to result from to the fact that Hempel denied a central place to causation in scientific explanations of singular phenomena (see Hempel 1965: 233, 250, 300-301, 347-354).

1.8 Conclusions

I have argued that the laws debate in ecology needs some justification. The justification alluded to above, namely, the covering law account, gave the laws debate in ecology a strong justification. I do not claim that this justification for the debate is good. My claim is rather that given the apparent paucity of ecological laws it is difficult to understand why ecologists should be engaged in the laws debate if no strong justification for the role of laws is given. And the given justification was not only sufficiently strong in contrast to those justifications usually alluded to by ecologists (cf. section 1.5), but also has practical relevance and importance for ecologists. The next chapter shows how this justification for the laws debate turns into a problem in ecology. This is because the appearance seems to be that no distinctively ecological (or biological) laws exist.

Some think that the laws debate in biology is an illusion created by the adoption of the syntactic view of scientific theories.[5] Rather than adopting this view, these same people claim that a semantic view of scientific theories should be embraced. In the semantic view, theories are not viewed as collections of laws, but as collections of idealized, abstract models that have no empirical interpretation on their own, but for which empirical content is given independently via "theoretical hypotheses."According to this view, laws describe models – in a sense, laws can be treated as definitions of models. Laws are thus tools for constructing models and laws are true of or in models. There is another version of the semantic view, but what has just been said suffices for the present purposes. In the context of the biological laws debates, proponents of the semantic

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[5] According to the syntactic view, a scientific theory can be represented as a deductively organized, axiomatic, and hierarchical collection of a small body of laws (axioms or fundamental laws) working together to explain a large number of other less general laws (theorems or derived laws), regularities, and phenomena. This view originated from studies focusing on physics and its theoretical structure (see Nagel 1961: 79-105 and Hempel 1965: 177-185, 338-343). Nevertheless, the view found friends among philosophers of biology and biologists (see Williams 1970, 1973b, 1981; Ruse 1973).

view often stress that there is no need for laws as traditionally defined, and that this speaks for their view, since appearances suggest that there are no biological laws (cf. Beatty 1980, 1981, 1987; Lloyd 1988; Teller 2001; Giere 2004; but see Sloep & van der Steen 1987a, 1987b, and Ereshefsky 1991, who disagree).

As a solution to the laws debate, the above appears to be of secondary importance, given that my focus here is on the epistemic roles of laws. By itself, the semantic view of theories does not illuminate how and by what means scientists furnish us with scientific explanations. Although I wish to remain neutral on the issue of the correct view of theories, many of my ideas are in accordance with the semantic view.

There are two influential alternative accounts to Hempel's covering law account in the literature. The causal-mechanical account of Wesley C. Salmon (1984, 1989, 1994, 1997) and the unification account of Philip Kitcher (1989) are critical responses to Hempel's account. Both are likewise non-covering law accounts according to which laws are not indispensable to scientific explanations.

Salmon's ideas about explanations first call for performing statistical analyses to make the *explanandum* sharper and then searching for the mechanisms for its *explanans*; which evidently describes correctly the practice of many ecologists as well. However, Salmon's account is not valid, because, among other things, it does not have the resources to discriminate between explanatory irrelevant and relevant information (Hitchcock 1995). James Woodward (2003a: 353-354) and Carl F. Craver (2007: 72-86) have expressed concerns about the applicability of Salmon's account to the biological sciences that have to do, among other things, with absences, omissions, and preventions that are sometimes treated as relevant explanatory things or causes in the biological sciences, but which as processes do not "transmit marks/conserved quantities" or "interact" as Salmon required from his explanatory causal processes and mechanisms. There are also difficulties in applying Salmon's account to complex and/or macroscopic explanations (see Woodward 2003a: 354-356).

There are equally serious problems with Kitcher's account (see, for instance, Barnes 1992, Psillos 2002: 276-279, Woodward 2003a: 361-367, and Craver 2007: 43-47). Moreover, Kitcher's account appears inapplicable to ecology, since ecological explanations are typically causal, mechanical, not unified, and work from the bottom to up (cf. chapters 4 and 5). Many ecological mechanisms evidently also operate in probabilistic or statistical ways, a fact that is difficult to reconcile with Kitcher's "deductive chauvinism."

Consequently, both of the non-covering law accounts should be given up in the context of ecology. This suggests that a covering law account should be adopted instead.

2

ARGUMENTS AGAINST BIOLOGICAL LAWS[6]

2.1 Distinctively Biological Laws

Since the 1950s, if not earlier, there have been debates concerning both the existence of biological laws and the nature of biological generalizations. At first, philosophers took physics as the model for the sciences. The idea was that if there were universal and exceptionless laws in physics, then this set the standard for laws in biology (see Williams 1970, 1973b, 1981; Ruse 1973). The problem was, and to a certain extent still is, that there are few if any biological generalizations that are true for every time and in every place.

There are many arguments in the literature believed to show the absence of biological laws. These arguments take advantage of various putative distinctive features of biological generalizations or phenomena – so the arguments usually go – that the physical sciences and their generalizations lack, where these features are held to be responsible for the absence of laws in biology. In this chapter I analyze the following nine arguments of the above kind:

Argument 1): Biological generalizations are about *species* and/or they make *reference to particulars*;
Argument 2): Biological generalizations are conceptually *imprecise*;
Argument 3): Biological generalizations are riddled with *exceptions;*
Argument 4): Biological phenomena are *historical*;
Argument 5): Biological phenomena are *unique*;

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[6] A version of this chapter was presented at the Philosophy of Science Group/Trends and Tensions in Intellectual Integration seminar 18 February 2008 at the University of Helsinki.

Argument 6): Biological phenomena are *complex*;
Argument 7): Biological phenomena are *open*;
Argument 8): Biological properties are *multiply realizable*; and
Argument 9): Biological generalizations are *contingent*.

Some of these arguments are marshaled to dispute the existence of *universal and exceptionless* biological laws, such as arguments 3 and 7, whereas most of the arguments are used to explain the absence of biological laws in general, such as arguments 1, 2, 4, 5, 6, 8, and 9. Sometimes the arguments are presented for the purpose of establishing that are no biological laws at all. Alternatively, we could understand these arguments as intended to establish that biology is a science deficient in laws.

In any event, the issue is not whether biology is a science that applies the laws of other sciences. Rather, the issue is whether there are *distinctive or proprietary* biological laws that are used by biologists in their domain to represent the laws of their subject matter. "Distinctively biological" could be understood to mean that such laws essentially make use of biological terminology and/or essentially refer to biological causal mechanisms, entities, things, and so on in addition to using logical, mathematical, and other subject-matter neutral terms. What has just been said is not intended to be a definition of the distinctively biological. Defining what this means is a delicate and elusive issue to which I offer no solution (see, however, Elgin 2006: 124-126, 130-132; see also Davidson 1970: 82-85 for related difficulties in defining what distinctively mental means).

I will argue that many of the above claims, such as arguments 2, 3, 4, 6, 7, and 8, are inappropriate or inconclusive for different reasons. Furthermore, some of the arguments, such as 1 and 5, have been successfully countered in the literature by other authors. Nonetheless, I maintain that some of the arguments, namely, 3 and 9, pose important challenges to the existence of biological laws. Thus, I claim that there are perhaps no biological laws, owing to the exception-ridden nature of biological generalizations and their "contingency."

Several philosophers have discussed the issue of biological laws (see Smart 1963: 50-63; Ruse 1970, 1973: 24-31; Hull 1974: 71-

100; Olding 1978; Rosenberg 1985, 1989; Ghiselin 1989; Steen & Kamminga 1991; Beatty 1995; Carrier 1995; Lange 1995, 2005a; Brandon 1997; Mitchell 1997, 2002; Sober 1997; Cooper 1998; Waters 1998; Weber 1999; Woodward 2001; Elgin 2003, 2006; Mikkelson 2003; Hamilton 2007). A shared feature among these philosophers is that their examples of biological generalizations are taken from evolutionary biology and (population) genetics. Exceptions to this trend are Gregory Cooper, Marcel Weber, Gregory M. Mikkelson, and Marc Lange, who at least occasionally, use ecology as their target of discussion as I do.

It might be argued that the narrow focus on evolutionary biology and (population) genetics of the above philosophers is biased insofar as the lawlikeness of biological generalizations is concerned. For instance, it might be argued that ecology is "more" lawlike than evolutionary biology. Alternatively, it might be argued that the scale of investigation is to be blamed for the appearance of lawlessness in biology, and, for instance, macro-ecology could furnish us with laws. In the following pages, I present generalizations from different scales of ecology and from paleobiology to suggest that the above bias does not matter. Evolutionary biology, genetics, (macro-)ecology, and paleobiology seem to lack laws. This is the main reason why I write here as if I were interested in the lawlikeness of biological generalizations in general rather than in ecology and its generalizations.

2.2 Reference to Particulars and Conceptual Imprecision

Argument 1): Biological generalizations are about *species* and/or they make *reference to particulars*.

J. J. C. Smart (1963: 50-63) was perhaps the first philosophers to use such examples as "albionotic mice always breed true,"[7] "all crows are black," and "all robins' eggs are greenish-blue" to argue that biology has no laws, because biological generalizations refer to particular species. Consequently, Smart maintained that biology as a

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[7] True breeding means that like parents produce only like offspring.

science is similar to engineering in that biologists are forced to borrow laws from other sciences when giving explanations and making predictions. Another, more general version of the argument claims that biological generalizations make in-eliminable references to all kinds of particulars (in addition to particular species), such as the earth and its history, the history of life on this planet, or phylogeny, some specific time or place, and so on. Again, Smart (1963: 50-63) was perhaps the first person to represent the latter version of the argument.

It is easy to see why the above should be damaging to the lawlike status of biological generalizations. The naive regularity account of laws (explained in section 1.3) held that the predicate terms of lawlike generalizations make no in-eliminable references to any particular. Instead, laws apply to all time and space, to all individuals, and they are expressible by general predicate terms. Indeed, it almost a universal demand of different law accounts that laws make no in-eliminable references to particulars (see, for instance, the discussion of quasi-universals in Armstrong 1983: 80, 100-101).

The problem with the above examples of generalizations – if they are interpreted to represent biological laws – is that they refer to species names, that is, to taxa, such as *Mus musculus* and *Corvus corax*, which appear to be examples of particulars. Particular species exist in specific spatio-temporal locations, they have a unique (phylogenetic) history, and so on. Moreover, if species as a *taxa* are individuals rather than classes "from a logical point of view," then the argument is apparently vindicated, because species do not refer to natural kinds or classes, but to things that function as proper names (see Ghiselin 1974, 1987a, 1987b; Hull 1974: 52-53, 1976: 256-260, 1978, 1987; Mayr 1976, 1987a, 1987b; Rosenberg 1985: 204-212, 1987; Williams 1987; Sober 1993: 146-159).

There are many difficulties with the above argument, however. For instance, it does not yet establish that generalizations concerning "particulars" cannot be derivative laws. The requirement that laws make no in-eliminable reference to particulars was meant to apply to fundamental laws. Besides, although the above considerations could establish that biological laws should not refer to particular species or taxa, provided that species are individuals, they do not show that there cannot be laws about species as a *category*. Moreover, biological generalizations typically refer to species as a category rather than as a taxon (see Hull 1976: 189, 1978: 640; Ghiselin 1987a: 129; van der Steen & Kamminga 1991: 453-456 for similar arguments). An example is **Mayr's** rule (see appendix), which refers to species as a category rather than as a taxon (cf. also the **founder** rule). In addition, the category of species does not refer to particulars. This is true even of the category of evolutionary species (see Ruse 1973: 138-139). The argument is silent on higher categories and taxa as well, which are the topic of many generalizations, especially in paleobiology that deals with genera, orders, and the like.[8]

Nor is it true that biological generalizations are necessarily or typically about "taxonomic things" or particulars. Ecological rules, such as **Allen's**, **Bergmann's**, **clutch** size, **Gloger's**, **hair**, and **litter** size rules (see appendix) are about *property gradients in space*. That is, they concern the variation of biological traits in space. Evolutionary rules, such as the rules of **Cope**, **Dacque**, **Dollo**, **Island**, **miniaturization**, and **unspecialized** are about *property gradients in time*. That is, they concern the variation of biological traits in time. Particular taxa are not in-eliminable parts of ecological or evolutionary rules, since they refer to property gradients that taxa exhibit in space or time. In fact, neither of the rules refers to species to begin with, but ecological rules refer to subspecies (or clines) and evolutionary rules refer to taxa above the species.

It might be suggested that the above rules nevertheless refer to specific spatial locations or temporal periods and thus to "particulars." For instance, ecological rules sometimes refer to latitudes. However, when generalizations, such as Bergmann's, Allen's, Gloger's, and the **latitudinal** diversity gradient, refer to latitudes, this is a surrogate for a property with which latitudes correlate. In the case of Bergmann's rule, the traditional interpretation is that latitudes correlate with "the mean temperature" during the coldest month(s), which appears to be a perfectly legitimate predicate

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[8] Although I am sympathetic to the species-as-individuals' thesis, I shall not discuss what the status of taxa above species is, e.g., Mammalia. Some of them appear as classes (see Ghiselin 1987a: 128-129).

term for lawlike generalizations. These properties are then understood as the causes for the property gradients (body size, diversity, length or size of protruding body parts, coloration, and so on) mentioned in ecological rules. What has been said about ecological rules applies to evolutionary rules, which refer to time.

As a conclusion, I suggest that the above argument fails to provide conclusive reasons against the existence of biological laws. Biological generalizations do not refer in-eliminably to particular species. When they refer to species, they refer to species as a category. Besides, many biological generalizations do not refer to "species" at all, but to other taxa and categories.[9] Moreover, biological generalizations are typically about biological traits and properties that taxa or categories exhibit in space or time.[10]

Argument 2): Biological generalizations are conceptually *imprecise*.

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[9] Of course, there are generalizations concerning particular species. However, no one takes them seriously as examples of laws, with the exception of Marc Lange (1995), who has argued that generalizations concerning particular species could be biological laws despite the fact that they refer to particulars. Lange's idea stands and falls with this account of laws, which will be discussed later (see sections 3.3 and 5.5). Trivially, generalizations concerning particular species can be advantageous. For instance, if one is going to gather mushrooms, it is useful to know that some fungi species, such as *Amanita virosa*, are deadly poisonous, or that *Coprinus atramentarius* has unfortunate side-effects if it is consumed with alcohol. However, the fact is that as *explanantia*, generalizations concerning particular species are shallow. Although these are not illuminating as *explanantia*, generalizations concerning particular species are interesting as *explananda*.

[10] See also Gregory M. Mikkelson (2003), who claims that ecological generalizations do not refer to particular species, specific taxa, or what he calls to "historical kinds." Instead they refer to and explain historical kinds with "functional kinds" and "structural kinds" of which historical kinds are instances. Examples of predicate terms of functional and structural kinds are predators, prey, competitors, resources, endothermic animals, clutch sizes, heterozygotes, autotrophics, and so on instead of Arthropoda, *Amanita virosa*, and so on, which are examples of historical kinds or taxa. Thus, Mikkelson concludes, as a science, ecology is more nomothetic-oriented than idiographic- or historically-oriented.

Although conceptual imprecision may not appear to have much to do with the existence of laws, there are at least two ways to reconstruct the argument. First, it is frequently held that laws should have a definite empirical meaning or truth value, if for no other reason than to make testing and (dis)confirming putative lawlike generalizations possible. This property might be lacking if the predicate terms of a generalization are imprecise. Second, one can construct the demand of conceptual *precision* as a criterion of lawlikeness. According to the naïve regularity account of laws, the predicate terms of lawlike generalizations give necessary and sufficient conditions for their application and/or refer to natural kinds. If the predicate terms of a generalization are imprecise, then these properties might be lacking, and thus the generalization is no law.

There are many biological concepts that could be charged with being imprecise.[11] There are different kinds of conceptual imprecision for which these concepts could be blamed as well. For instance, a concept could be charged with being a tautology, circular, incoherent, vague or ambiguous, non-committal, and so on. Since there are many concepts to be blamed and different kinds of conceptual imprecision, I shall concentrate on the most characteristic reproach directed at

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[11] For instance, the following biological concepts have been charged with being imprecise or incomplete: adaptation (Lewontin 1978, Gould & Vrba 1982, West-Eberhard 1992); biotope, habitat, and niche (Udvardy 1959, Schoener 1989); colonization, emigration, immigration, and the invasion of a species (Simberloff 1969, Shrader-Frechette 2001); community (Taylor 1992, Shrader-Frechette & McCoy 1993: 12-31, McCoy & Shrader-Frechette 1994); competition (Birch 1957, Cole 1960, Shrader-Frechette 1990a, Peters 1991: 256-273, McIntosh 1992); fitness (cf., for instance, Hull 1974: 65, 66-69; Peters 1976, 1978, 1991: 60-73; Ferguson 1976; Caplan 1977; Castrodeza 1977; Brandon 1978; Mills & Beatty 1979; Rosenberg 1982; Byerly 1983; Sober 1984: 61-85; Shrader-Frechette 1990; Murray 2000: 403-405); the species as a category (Ruse 1973: 126-139, 1988: 51-62; Rosenberg 1985: 191-203; Sober 1993: 153-159); and stability (Peters 1991: 92-96, McCoy & Shrader-Frechette 1992, Shrader-Frechette 1993: 32-60, Grimm & Wissel 1997, Mikkelson 1997, Odenbaugh 2001). The references given above refer to papers of which some are replies to charges of conceptual imprecision.

biological concepts, namely, the charge that a concept is *a conflation* of different meanings. This accusation is typically made about such ecological concepts as stability and community (see Peters 1991: 81-82, 92-96; McCoy and Shrader-Frechette 1992, 1994; Shrader-Frechette and McCoy 1993: 11-67, 114-119).[12]

Conflation concepts are imprecise, goes the criticism, because different *vague and/or unrelated* meanings are grouped or lumped under one concept. For instance, Alexander Rosenberg (1985: 191-203) complained that the species category is problematic, since there is no one universally valid and applicable meaning for the concept, but at least three or four different meanings, namely, the biological, the ecological, the evolutionary, and the morphological species concepts. Therefore, Rosenberg reasoned, the species is not a natural kind.

The problem with the conflation charges is that they are usually made in haste. From the fact that there are different meanings of one concept, it does not automatically follow that the concept is imprecise, incomplete, or somehow tainted. Elliot Sober (1993: 158) has put the point excellently in the context of species category:

> Indeed, the idea that there is a single species concept that should be used in all biological contexts is not something that we should assume dogmatically. Perhaps some form of *pluralism* is correct…. Pluralism should not be confused with *conventionalism*, according to which our choice of the species concept we adopt to describe a given biological situation is arbitrary. Pluralists maintain that we should use species concept *X* in some situations but concept *Y* in others.

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[12] In addition to 'stability' and 'community,' these authors blame many ecological concepts for conflation. However, strictly speaking, only Shrader-Frechette and McCoy (*ibid.*) make the specific and straightforward charge that conceptual imprecision is a reason for the absence of ecological laws. The other authors make charges that are more ambiguous insofar as the implications for the existence of ecological laws are concerned, although they clearly imply that conceptual imprecision has negative implications for (the existence of) biological laws. This is characteristic of conceptual imprecision charges in general, which makes it difficult to evaluate them as arguments.

> Conventionalists hold that whether we use concept *X* or *Y* in a given situation is arbitrary.

Although as I argue later on there are different meanings for stability in ecology, it does not automatically follow that stability as a concept is imprecise (cf. sections 2.6 and 5.3). Closer inspection of stability reveals that we have different, yet related concepts that have definite meanings, which are regretfully sometimes covered imprecisely by one concept.[13] The case with stability would be different and problematic if the different meanings had nothing in common and/or they were vague in meaning.

As a conclusion, I suggest that the conceptual imprecision argument is not convincing, at least in the form in which it is usually presented. Charges of imprecision have often confused the plurality of different, precise meanings of related concepts with situations in which a concept has a vague and/or unrelated meaning(s). The general moral is that the argument of conceptual imprecision is no silver bullet for shooting down the existence of biological laws: There are many kinds of imprecision for which a concept could be blamed. The implications of different kinds of conceptual imprecision for the existence of (biological) laws are usually open questions as well. For instance, it has remained unclear exactly what the implications are of the tautology charge of fitness for the lawlike status of the rule of **natural** selection; this charge is inappropriate for other reasons as well (see Sober 1984: 61-85 on both points). Finally, it is not an established fact that biology has a special status for conceptual imprecision in comparison to other sciences, simply because comparative studies of this kind are missing. This suggests that the conceptual imprecision charges do not give sufficient reason to deny the existence of *biological* laws.

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[13] Jay Odenbaugh (2001) has given a similar answer to the conflation charges against stability by Shrader-Frechette, McCoy, and others.

2.3 Exceptions

Argument 3): Biological generalizations are riddled with *exceptions*.

Along with the claim that biological generalizations make reference to particulars, J. J. C. Smart (1963: 50-63) added that it might be possible to express biological generalizations in a way that they make no such references by redefining their predicate terms. But, he continued, this would run into another problem, since when the predicate terms are thus redefined, it is likely that there would be exceptions to the generalizations. For example, if we define the North American robin (*Turdus migratorius*) as a species having "such-and-such" properties and we manage to do this without making any reference to its phylogeny, distribution, and so on, it is likely, so the argument goes, that if there is life on other planets, then there are exceptions to the generalization that "all life forms with such-and-such properties have greenish-blue eggs." In fact, it is possible that there are exceptions to the generalization on this planet, too, since there is variation within the species. The same considerations apply to genetic rules. Although it is possible to express **Mendel's** rule of segregation (see appendix) so that it makes no reference to "local" predicate terms of the forbidden kind, it is possible that life on other planets would be exceptions. Besides, there are exceptions, on this planet, to both of Mendel's rules, namely, meiotic drive, non-disjunction, and linkage.

According to Smart the reason why there are exceptions to biological generalizations, is that biological systems are complex and non-homogeneous in contrast to physical and chemical ones:

> My conclusion so far is that if the propositions of biology are made universal in scope, then such laws are very likely not universally true. If they are not falsified by some queer species or phenomenon on earth they are very likely falsified elsewhere in the universe. The laws of physics, by contrast, seem to be truly universal. Why is there this difference? Part of the answer seems to be this. The physicist, and to a lesser degree the chemist, talks about things which are relatively *simple* or else *homogenous*. (Smart 1963: 54-55.)

I shall not dwell further on Smart's arguments. There are already analyses and criticisms of these arguments by Michael E. Ruse (1970, 1973: 24-31) and C. Kenneth Waters (1998). In addition to Smart's complexity and non-homogeneity (see argument 6), there are other and more plausible reasons why biological generalizations are riddled with exceptions, which refer to their openness (argument 7), contingency (argument 9), natural selection, the arms races between and within species, biological variation, and so on.

I take it to be an established fact that there are exceptions to many – if not to all – *evolutionary* and *genetic* generalizations, as previous (philosophical) studies have shown. In addition to Mendel's rules, other examples of exception-ridden biological generalizations in the philosophical literature are the **central** dogma of molecular genetics, the rule of **natural** selection, the **biogenetic** rule, and examples of the "all crows are black" variety. For instance, given that fitness of an organism is not the same as its actual survival and/or reproduction success, but a propensity to survive and/or reproduce (Mills & Beatty 1979), it follows that there are exceptions to the rule of natural selection. Organisms fail to reproduce and die as a result of accidental causes and environmental factors that have nothing to do with their fitness or natural selection. What about other biological disciplines?

Consider ecological generalizations, for instance, **Gause's** rule. It is surprising that only one philosopher, namely, Marcel Weber (1999), has taken up Gause's rule as an example of a biological law, even though Gause's rule is well-known as an ecological generalization. However, as an example of a universal and exceptionless law, Gause's rule disappoints. There are many exceptions to it (see, for example, Lack 1945, 1946; Utida 1953; Ross 1957, 1958; Savage 1958; Cole 1960; Hutchinson 1961; Slobodkin 1964; Paine 1966; Leslie *et al.* 1968, Paine & Vadas 1969; Ayala 1972; Wiens 1977; Armstrong & McGehee 1980).[14] That there are exceptions to Gause's rule is also the reason why Weber (1999)

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[14] The exceptions to Gause's rule are discussed in detail later (see section 3.6 and chapter 6).

claims that Gause's rule is a *ceteris paribus* law rather than a universal and exceptionless one.

The ecologist Daniel S. Simberloff (1974) suggested that the **species–area** rule is a general law of ecology. John N. Lawton (1996, 1999), another ecologist, defended the rule as an example of a macro-ecological law that is more or less universal and less contingent than generalizations of traditional levels or scales of ecology. However, there are also exceptions to the species–area rule (see, for instance, Cook 1974 and Gilbert 1980). Consequently, the philosopher of science Marc Lange (2005a) has suggested that the species–area rule is a *ceteris paribus* law that tolerates exceptions.

John N. Lawton (1999) sees the **diversity–stability** rule as comparable to the species–area rule. However, there are striking exceptions to this rule too, such as the stable *mono*cultures of plants. Other macro-ecological generalizations mentioned by Lawton include the *interspecific* pattern of **abundance** and distribution, the body-size **frequency** distribution of a taxon, the **canonical** distribution of the abundances of species, the **latitudinal** diversity gradient, the **hollow** curve, and the relations between **range size** and **body size** (see appendix). The moral of the story should be clear: the examples just mentioned are riddled with exceptions.

There are examples of putative laws from paleobiology, such as **Dollo's** rule, which is often referred to as the most reliable of evolutionary rules. Since there are many exceptions to this rule as well, discussion has centered on whether the exceptions represent *genuine* or *apparent exceptions* (see, for example, Gregory 1936, Muller 1939, and Lee & Shine 1998; see also chapter 3). There are other candidates for paleobiological laws, such as **Cope's** rule, the **island** rule, the **miniaturization** rule, the relation between **specialization** and diversity, the rule of the **unspecialized**, Van **Valen's** rule of extinction, and **Williston's** rule (see appendix). In practice, paleobiologists agree on the lesson described above: the generalizations just mentioned do not represent universal and exceptionless laws, but rough trends in and of evolution.

There are philosophers and biologists who refer to allometries and scaling laws, such as the **energy equivalence** rule, **Kleiber's** rule, and the **thinning** rule, as biological laws (see Rensch 1960, Peters 1983, Mitchell 2000, Marquet 2000, Marquet *et al.* 2005, Elgin 2006). It suffices to say, first, that allometries and scaling laws are statistical trends in data, which have exceptions to them. This criticism might not strike as deep for those who believe that biological laws are probabilistic. However, there are other reasons to suggest that allometries and scaling laws do not represent lawlike generalizations (see sections 2.6, 3.4, and 4.4).

Do the examples of generalizations from ecology and paleobiology represent a different picture of the lawlikeness of biology than those traditional examples from the philosophy of biology literature that are drawn from evolutionary biology and (population) genetics? Evidently, the answer is no. There appears to be no universally true and exceptionless biological generalization. Moreover, biological generalizations apparently have an open-ended set of exceptions that defy systematic or simple treatment or characterization, which suggests that they have the appearance of *ceteris paribus* laws (see section 3.5). Thus, the argument of exceptions appears to be correct and true of biological generalizations in general. The above conclusion is not a hasty induction from a few cases, but the result of a search for an example in the biological literature: there are hundreds of generalizations that I have examined in this regard, but none has the appearance of a universal and exceptionless regularity.

2.4 History, Uniqueness, Complexity, and Openness

Argument 4): Biological phenomena are *historical.*

Although the idea is widespread among many (evolutionary) biologists that there cannot be laws about historical phenomena, there is a surprising paucity of explicit arguments for it. George Gaylord Simpson (1963) is perhaps the best example of an author who upheld this idea in the context of paleobiology. Another example is Stephen Jay Gould (1970: 208-210, 1980: 112-116). Simpson, for instance, did not deny that paleobiology uses and is governed by physical and chemical laws, only that owing to the historicity of paleobiological phenomena, there cannot be distinctive paleobiological laws. Unfortunately, his reasons for why the historical nature of paleobiological phenomena precludes there being laws about such phenomena are not well-articulated. The same goes for Gould.

Traditional *replies* to the history argument point out that history is not a dichotomy, but a continuum (see Nagel 1961: 547-551, Siever 1968: 74-76, Sober 1993: 14-18). Accordingly, all sciences are more or less historical and thus there are no qualitative differences between biology and physics, for instance, in this respect, although there could be quantitative differences. Moreover, what has just been said applies both between the sciences and within a single science. For example, in physics, there are disciplines that are more historical than others. Consider cosmology in contrast to particle physics: the former appears to be more historical than the latter. Nevertheless, in the former, laws are used together with historical data that concern the laws' boundary and initial conditions. In addition, there are physical phenomena – e.g., hysteresis – that are both historical and law-governed in the sense that their behavior is dependent on their past states, which appear to contradict the supposition that there cannot be laws concerning historical phenomena (see Hempel & Oppenheim 1948: 142 and Ruse 1973: 212).

One reason why scientist and some philosophers claim that evolutionary phenomena are historical – or so another traditional answer to the history argument goes – is that people confuse phylogeny with the theory or mechanism of evolution, whereby the former is a historical phenomenon that is explained by the latter, which is non-historical in nature (see Hempel 1965: 370 and Ruse 1973: 211-212). The scope of the history argument is likewise limited. There are scores of non-historical generalizations in biology, such as the **diversity–stability** and **species–area** rules (see appendix).

These traditional replies to the history argument drain the argument of much of its plausibility. Unfortunately, these replies miss an important issue insofar as the use of "history" in the arguments is concerned, namely, that history is not just a continuum, but a *problematic conflation* concept as well. In the history arguments, it is not history *per se* that authors argue is responsible for the absence of laws about historical phenomena. Rather, many other things are claimed to be responsible for the absence of laws, things that do not have much in common with history, but which nevertheless are mistakenly used as if they were synonymous with it. By history is sometimes meant the idea that phenomena are unique (argument 5), complex (argument 6), contingent (argument 9), or that phenomena refer to particulars (argument 1). In the history arguments by Simpson (*ibid.*) and Gould (*ibid.*), history is evidently used interchangeably with the things just mentioned. In fact, in many *replies* to the arguments from history, uniqueness, and complexity, these above things get confused with one another (cf. McIntyre 1997 and Steen & Kamminga 1991: 459-462). I consider it trivial that historical phenomena are not necessarily unique, complex, contingent, or refer to particulars. There is nothing in historical phenomena that makes them unique or vice versa. History denotes something about the tense of the occurrence of a phenomenon, namely, that it appeared in the past, whereas uniqueness refers to its frequency, namely, that it will not recur. I also consider it trivial that complexity and historicity are not necessarily connected. Moreover, historical phenomena need not be (strongly) contingent. Imagine a law that has all of its instantiations in the past. The instantiations have happened out of necessity, i.e., they are not contingent, because they are governed by the law in question.

As a conclusion, I suggest that the history argument fails. The argument has no *independent* force to establish much about the nomological character of the biological sciences, owing to the fact that history is a problematic conflation concept in the arguments. Traditional replies have provided many other reasons to doubt the argument as well.

Argument 5): Biological phenomena are *unique*.

Biological and especially evolutionary phenomena are said to be unique. Uniqueness has many meanings (see Ruse 1973: 90-91). However, the only sensible and independent meaning in the given context is that unique phenomena "cannot recur." There cannot be laws about unique phenomena, the argument goes, because regularities and generalizations presuppose repetion and recurrence of phenomena, which uniqueness precludes.

A uniqueness argument for biological phenomena is presented by Kristin Shrader-Frechette and Earl D. McCoy (1993: 116-119), namely, that the uniqueness of ecological phenomena is one reason why there are no general, empirical (and deterministic) ecological laws; or, as another, more moderate version of the argument goes, why the discovery and perhaps the existence of such laws is unlikely.

Already David L. Hull (1974: 97-100) showed why the uniqueness argument fails by pointing out that "events are not unique in and of themselves but only under certain descriptions." Consider a specific place, for instance, a bird feeder in the nature conservation area of Viikki (Helsinki, Finland) and a specific time, for instance, the 13th of January 2008 at 8 a.m. Suppose that the temperature on this morning is minus 27 degrees Celsius. In that place and time, there are two birds from different species of finches – *Fringilla montifringilla* and *Loxia curvirostra* – competing for the seeds of *Helianthus annuus*. The bird from the former species has its tibia and fibula broken, thanks to an encounter with a squirrel last night. Consequently, it hops slowly around on one leg and watches the individual of the bird from the other species consume the last seeds of *Helianthus annuus* in the bird feeder.

The above *is* a unique and singular description of the situation. But it is not the only description. Nor is the above the description we use in explanations of and generalizations about situations such as this. Alternatively, the situation can be describes as one in which two dietary specialist species (i.e., granivorous species) of a guild are engaged in interspecific exploitative competition for a common resource that is both in short in supply and critical for their survival in cold climates. This is not a unique description, but a generalizable phenomenon concerning species and guilds.

Argument 6): Biological phenomena are *complex*.

Various authors think of complexity as a major reason for the special nomological status of the biological sciences (see, for instance, Simpson 1963: 26-27, Smart 1963: 55-56, Shrader-Frechette & McCoy 1993: 119, Mayr 1996: 102). Arguments of complexity are likewise common in the social sciences (cf. Scriven 1956).

There are several different problems with the complexity argument. First, complexity is a *problematic conflation* concept. For instance, within a system, one can differentiate between two kinds of complexity. Structural complexity denotes something about the composition of a system. Process complexity denotes something about the dynamics of a system. The two are not necessarily connected. There are structurally (or formally) simple biological systems (or models) that can display complex dynamics, e.g., chaos (see May 1974, 1976). There are also many other meanings of complexity besides these two. By complexity could be meant that there are many different sufficient and/or necessary causes for biological phenomena. Complexity could refer to contingency (see argument 9). Complexity could denote that biological systems, traits, or phenomena are variable or non-homogenous. Complexity could refer to the thesis that in biology "everything is connected to everything else" (see argument 7). And by no means do these possibilities exhaust all the meanings of complexity.

Moreover and second, many of the different meanings unrelated. Third, it is obvious that many of these meanings are continua, as was the case with "history." Fourth, complexity is not an intrinsic property of phenomena, but depends on the way we describe the phenomena (see McIntyre 1993). If, as a result of scientific investigation, the way we describe phenomena changes, then phenomena that originally appeared complex might later on appear simpler; there are many such instances in biology (see Orzack 2005: 481-482).

Fifth, if a phenomenon appears complex or irregular at one level, then it may display considerable simplicity and regularity at some other level, for instance, at a higher level (see Brown 1995: 10-20, 226; Lawton 1996, 1999; Marquet *et al.* 2005). Sixth, we do not want the complexity of our laws to match the complexity of their targets. Laws and generalizations are not about all the characteristics of a thing, but rather about the characteristics a thing has in common with other similar relevant things. We thus generalize about and explain the *kinds* of things, even if the kinds in biology are not, strictly speaking, natural kinds (cf. Waters 1998: 9-13). Seventh, we are not told how complex the phenomena should be in order for there to be no laws about them. And eighth, comparative studies of complexity (or complexities) between biology and other sciences are lacking. Moreover, it is plausible that physical phenomena are as "complex" as biological ones.

As a conclusion, I suggest that the complexity argument is inconclusive at best. The argument has no independent force, since complexity is a conflation of different meanings and arguments. Many of the individual meanings of complexity are both continua and non-intrinsic properties of systems. Nor it is an established fact that biological phenomena are more "complex" than, for instance, physical ones.

Argument: 7): Biological phenomena are *open* (or interactionally complex).

The idea that the biological sciences deal with open phenomena is shared by many practioners in the field. The idea is likewise common in the social sciences. This openness is then contrasted with the physical and chemical sciences that deal with (more) "closed" phenomena.

Although the idea is commonly held that the openness of biological phenomena is responsible for the absence of biological laws, there are few explicit arguments for this. Fortunately, there is one argument from the openness of phenomena to the absence of *strict or universal* laws, by Nancy Cartwright (1983: 44-73).[15] Cartwright developed the argument with the physical sciences in mind, but the argument is more usefully applied to the biological sciences. Sometimes this argument is constructed as the complexity argument (as in Elgin 2006: 123-124). Since there are different meanings of complexity (see argument 6), I construct the argument from openness as independent of complexity. If one insists that it should be constructed as the complexity argument, then I suggest the use of a more precise term for the complexity involved, such as the *interactional complexity* (a term adapted from Wimsatt 1974).

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[15] Ray Pietroski and Georges Rey (1995: 89-94) have presented a similar argument.

The idea behind the argument is straightforward. Because biological phenomena are open, which means that there are many different outside factors coming from *other phenomena* that affect and interfere with the phenomenon we are investigating, generalizations and putative laws about the phenomenon in question are riddled with exceptions and do not hold, either universally or generally. This is because the other factors from other (not necessarily biological) phenomena interfere with the phenomenon we are investigating, and thus we get generalizations that hold only when the interfering factors are *ceteris paribus*. The presence of "noise" is a common theme in the sciences, but here we have something over and above this theme. The multiplicity of interfering factors from other phenomena is thought to be so great in number and/or so considerable in effect that these interfering factors do not just represent noise to generalizations, but show that generalizations' applicability is severely limited and/or that they are riddled with exceptions.

I have no doubt that biological phenomena are open in the way depicted – or that in biology "everything is connected with everything else." Both interspecific and intraspecific competitive systems in ecology, for instance, are affected by all kinds of external factors, which may affect the result of competition, e.g., through all kinds of accidents in an abiotic environment. In addition, ecological systems do not consist only of guilds species but also are parts of ecological communities and are therefore affected by trophic interactions both from "above" and from "below." Biological communities do not exist in isolation, but are also affected by all kinds of external factors and so on. Evidently, the conclusion of this argument is correct as well. In fact, we have here one plausible reason why argument 3 ("exceptions") should be true of biological generalizations.

But is openness a peculiar property only of biological (or social) phenomena? Consider an object falling toward the surface of the earth that is supposed to have a constant acceleration rate (9,81 m/s^2), owing to gravity. Gravity, however, is not the only force that might affect its acceleration. There are other physical forces, for example, air resistance and wind, that might cause deviations from the constant acceleration rate. Of course and importantly, there is already a *ceteris paribus* clause attached to Galileo's law of free fall, which states that an object has the above constant acceleration rate owing to gravity for all masses that are *freely* falling toward the *earth's* surface, that is, in the presence of a large enough mass, comparable to earth's, and in the absence of other physical forces other than gravity.

Thus, it appears that physical phenomena are open as well and affected by all kinds of forces and causes outside their robust boundaries (see also Joseph 1980, Cartwright 1983, Carrier 1995, Pietroski & Rey 1995). This suggests that openness is not a *sufficient* condition to explain why there are no biological laws, provided that there are physical laws. It is a separate issue as to whether openness has implications for the existence of (strict) laws *in general* in the sciences.

Yet it is not openness of phenomena that is the real problem. Nor it is the reason said to be responsible for the absence of laws in biology. Instead, the claim is that in biology – and in other non-physical sciences – generalizations are "heteronomic," whereas in the physical sciences they are "homonomic." Harold Kincaid observes the same thing in the context of the social sciences. The following is taken from a passage in which he notes that physical phenomena are as open as social ones:

> Perhaps the crucial issue concerns not the open or closed nature of actual systems but rather a theory's ability to handle those outside factors. A closed theory is complete: It can describe and explain in its own terms all the forces acting in its domain. So, the argument runs, forces affecting open physical systems can be fully handled within physics itself. In the social sciences, however, outside factors are not social in nature – and thus cannot be handled by social theory. Consequently, alleged social laws are bound to be incomplete and thus not laws. (Kincaid 1990: 60.)

The claim and the real argument behind the openness argument is that the biological sciences cannot deal systematically with exceptions and application conditions of their "laws" with their own, distinctive, and proprietary terminology. Rather they need to be complemented by other sciences, especially those involving lower level phenomena, in order to deal with their exceptions. Thus, even if there were a biological generalization that could be made exceptionless and whose conditions of applicability could be specified, it would not be *distinctively biological*, owing to the heteronomic character of biological generalizations. Rather, it would be a chemical or physical law, if it were a (strict) law in the first place.

I argue later on that the idea that systematic corrections to generalizations come from explanations that are "vertical" in character in the non-physical sciences, that is, from lower levels, can be questioned (section 3.6). There are "horizontal" explanations of exceptions in ecology that make ecology look homonomic in generalizations. In community and population ecology, ecologists explain away exceptions of generalizations by using community or population ecological concepts or causes of the same level rather than concepts or causes of a lower level. This suggests that the latter version of the argument fails as well at least in the context of ecology.

2.5 Multiple Realizations[16]

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 their body size in colder regions or at higher latitudes than members of the same species in warmer regions or at lower latitudes. The traditional explanation for Bergmann's rule is that the larger body size of an endothermic animal is an adaptation for "heat dissipation" in cold climates (see Kendeigh 1969 and McNab 1971). Lesser heat dissipation results from the fact that the ratio of "surface area to volume" is smaller in animals with larger body size than in animals with smaller body size, the reason is that when body size becomes larger, its surface area increases as the *square* of the mass, whereas its volume increases as the *cubic* of the mass. This allows larger-sized endothermic animals to conserve their metabolized heat

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[16] Alexander Rosenberg's argument presented and evaluated here is examined more deeply in a manuscript entitled "Multiple Realizability and Biological Laws," which I am co-authoring with Markus I. Eronen.

more effectively, because it is dissipated through the surface area in cold environments. Accordingly, if the temperature of an environment (during the cold season) is a critical factor for an organism's survival, then natural selection should generally and even universally favor and select for different body sizes – and this is seen in the pattern of Bergmann's rule, in which the members of a species of endothermic animals in cold climates have bigger body sizes than their relatives in warmer climates.

There are, however, other means to reduce the dissipation of heat in cold climates. For instance, thicker and heavier fur or plumage might have effects similar to heat dissipation as the increase in body size (see, for instance, Scholander 1955, 1956; Irving 1959). These, however, enhance an animal's *insulation* without affecting its surface area to volume ratio or body size. This shows that there are functionally equivalent, but physically different means to achieve the same level of fitness in cold climates. Thus, "dissipation of heat" is a multiply realizable property.

Alexander Rosenberg has presented an argument from multiple realizability of biological properties to there being no biological laws – at least in the "middle range" of biology (see Rosenberg 1985: 59-65, 1989: 247-255, 2001a: 205-209, 2001b: 138-140, 2001c: 737-738, 2001d: 138-140).[17]

Rosenberg (1989, 2001a, 2001b, 2001c) reminds us that natural selection is a process that selects for effects. Moreover, Rosenberg

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[17] Based on this argument, Rosenberg also makes different claims about the status of biology. On the one hand, Rosenberg (1985: 219-225, 1989) maintains that instrumentalism is true of the "middle range" of biology or that biology is capable of producing only "case-studies" because there are no covering biological laws. On the other hand, Rosenberg acknowledges that there are biological covering laws at very high and low levels of biological organization (cf. the theory of evolution by natural selection and biochemistry), because their properties are not as multiply realizable as are the middle range ones, and biological explanations are explanatory owing to these laws. Here, I am dealing with the general argument that there are no laws concerning multiply realizable properties, not the idea that there are differences between levels of biological organization as to how multiple realizable their properties are or how lawlike are their generalizations.

adds that processes which select for effects cannot discriminate between different realizations that have identical and perhaps even similar effects. In other words, if we have different realizations for a biological property with identical and perhaps even similar biological effects on fitness – as could be the case in the above example of the "dissipation of heat" –, then natural selection cannot discriminate between different realizations. This apparently results in a "diverse" supervenience base characteristic of functional kinds or biological properties, since, instead of selecting between different realizations with similar or identical effects, selection might preserve them all. This idea of selection is one of the central premises of Rosenberg's argument against biological laws, although not the one that is the main target of my criticism. In fact, as I argue below, Rosenberg needs other – more dubious or contentious – assumptions about the nature of natural selection, laws of nature, and multiple realizability in addition to the above premise in order to argue from the multiple realizability of biological properties to there being no biological laws.

There is no exact formulation of Rosenberg's argument from the multiple realizability of biological properties to the absence of biological laws. However, here is the best or the most succinct characterization of his argument (Rosenberg 2001c: 738, footnote 2; see also Rosenberg 1985: 59-65, 1989: 247-255, 2001a: 205-209, 2001b: 138-140, and 2001d: 138-140 for similar passages and ideas):

> To see why there can be no strict laws in biology, consider the form of a generalization about all *F*s, where *F* is a functional term, like gene, or wing, or belief, or clock, or prison, or money, or subsistence farming. The generalization will take the form (*x*)[*Fx*→*Gx*], a law about *F*s and *G*s. *Gx* will itself be either a structural predicate or a functional one. Either it will pick out *G*s by some physical attribute common to them, or *Gx* will pick out *G*s by descriptions of one of the causes or effects that everything in the extension of *Gx* possesses. But there is no physical feature common to all items in the extension of *Fx*: *Fx* is a physically heterogeneous class since its members have been selected for their effects. So *G* cannot be a structural predicate. Of course some structural feature may be shared by all of the members of *F*, but it will not be a

> biologically interesting one. Rather it will be a property shared with many other things — like mass, or electric resistance. These properties will have little or no explanatory role with respect to the behavior of members of the extension of Fx. For example, the generalization that 'all mammals are composed of confined quarks' does relate a structural property (quark confinement) to a functional one (mammality), and is exceptionlessly true. But it is not a law of biological interest. The existence of a functional property different from F that all items in the extension of the functional predicate Fx share is highly improbable. If Fx *i*s functional kind, then owing to the blindness of selection to structure, the members of the extension of Fx are physically diverse. As such, any two Fs have non-identical (and usually quite different) sets of effects. Without a further effect common to all Fs, selection for effects cannot produce another selected effect; it cannot uniformly select all members of F for some further adaption. Thus, there is no further function kind which all Fs share in common. Whether functional or structural, there will be no predicate Gs that is linked in a strict law to Fx.

From these points it supposedly follows that there are no laws involving multiply realizable biological properties. Let us consider a biological generalization that takes the form $\forall x(Fx \rightarrow Gx)$, where F stands for a biological (functional) property. Let us suppose that, given that natural selection selects for effects, it follows that F is multiply realizable; for instance, $(Px_1 \text{ v } \ldots \text{ v } Px_n)$ are realizations of F, where n is possibly large in number. Predicate G could stand for a structural or functional biological property. Rosenberg's argument then proceeds as follows: There cannot be a law linking F to another *structural* or physical property G, since *the realizers of F are structurally or physically heterogeneous*. In other words, there is nothing structurally or physically common or similar in the realizers of F which could then be linked via a biological law to a structural predicate G in the form of a generalization, such as all Fs are Gs. Of course, there are some things in common between different realizations of F. For instance, the realizations are all composed of atoms, have masses, obey physical laws, and so on. However, the point of Rosenberg's argument is that there is nothing in common between them that is *biologically interesting and/or explanatorily relevant* and that could be expressed in a biological generalization linking *F* and *G*. (In the discussion that follows, I shall call such realizations of biological properties "diverse and heterogeneous.") Furthermore, there cannot be a law if *G* is a functional property, since it is not possible – or at least, it is highly improbable – that there is *a functional property different from F* that is shared by all the realizers of *F* which could be linked via a law to realizations of *G*. Hence, if *F* is a biological (functional) property that is diversely or heterogeneously multiply realizable, then there cannot be laws of the kind $\forall x(Fx \rightarrow Gx)$, since there are no "links" between *F* and *G* that can be expressed in the form of a lawlike generalization due to multiple realizability of *F*.

One problem with the argument concerns its scope. In a tradition going back to logical positivism, philosophers took physics as the model for the sciences. The idea was that if there were universal and exceptionless laws in physics, then this should set the standard for laws in biology. Rosenberg might have originally constructed the argument with this account of laws in mind. At any rate, the argument evidently goes against such an account. For instance, it is not uniformly or invariably true that "all genes are composed of DNA," because some genes are composed of RNA. DNA and RNA are both composed of nucleic acids, but not identical nucleic acids, since DNA contains thymine, which is replaced in RNA by uracil. Furthermore, it is possible that alien life-forms (if they exist) have found alternative ways to transmit and code genetic information, whereby their realizations of "genes" are not composed of nucleic acids at all, but of something else.

The above considerations suggest that the argument from multiple realizability to there being no *universal and exceptionless* biological laws is true. Rosenberg might have thought that the argument applies to *other accounts of laws* as well. This is reflected in the fact that although Rosenberg sometimes presents the argument against the existence of strict biological laws (as in the passages quoted above), his intention is to apply the argument against *all* kinds of biological laws, whether strict or not (see especially Rosenberg 2001c).[18]

However, philosophers of biology and biologists today do not think of biological laws in terms of universal and exceptionless regularities. Moreover, there are several accounts of laws that tolerate exceptions (see chapter 3). Crucially, these accounts fit the biological sciences better than the accounts that take laws to be universal and exceptionless regularities. Furthermore, it is an open question of "how much" multiple realizability is needed to establish that exception-tolerating accounts of laws are troubled by Rosenberg's argument.

As discussed above, a central premise in Rosenberg's argument states that the realization bases of biological properties are diverse and heterogeneous. However, the relation between multiple realizability and natural selection is more complex than Rosenberg assumes. Specifically, the idea that biological properties are diversely and heterogeneously realized does not follow solely from Rosenberg's premise alluded to above, namely, the idea that selection selects for effects. The claim that the realization bases of biological properties are diverse and heterogeneous depends on several context-specific, empirical, and contingent assumptions about evolution, the form and mode of natural selection, the evolving populations in question, their selective environments, and so on. The problem is that these are assumptions that Rosenberg has not argued for, and in fact they are assumptions that cannot be argued for on a general basis.

The famous **Red** Queen rule or "law," for instance, could be used as a model of evolution from which it might follow that there are different realizations for biological properties. The Red Queen rule codifies the idea that the evolutionary progress of species (or taxa) is relative and temporary, owing to fact that biotic and abiotic selective environments of species are constantly changing and deteriorating. A classical example is the arms races between (and

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[18] By non-strict laws I mean laws that include *ceteris paribus* clauses, such as that laws hold only when "some of their (unknown) interfering background factors remain the same and/or absent."

within) species (cf. Dawkins & Krebs 1979). These arms races evidently provide one basic reason for Rosenberg to hold that there are no biological laws – that is, they seem to be one reason why he believes that biological properties have diverse and heterogeneous realization bases (see Rosenberg 2001c: 737-742 and 2009: 61-63).

For instance, an adaptation in predators, such as the evolution of a better catch or hunting apparatus, is felt by the prey as a selection pressure, which could lead to the evolution of (better) camouflage/mimicry, "quicker feet," changes in foraging behavior, and other changes in the prey. These changes in turn are felt by the predators as a selection pressure to change their traits, behavior, and so on. Thus, "catching the prey" appears to be a multiply realizable property, and selection that selects for functions could lead to a situation in which there are functionally equivalent and perhaps even identical, but structurally dissimilar trait-variants of the property in question in a predator population (or species), which are preserved and proliferated by selection owing to the arms race *between* species. This could lead to a diverse and heterogeneous realization base for the higher-level property in question, provided perhaps that selection *within* the species is frequency-dependent. The same goes for the higher-level property of prey, "predator avoidance," which apparently is a multiply realizable biological property as well.

The Red Queen rule is not a universally valid model of evolution, however. Moreover, in order for the rule to have above kinds of implications for the realization bases of biological properties, other contingent assumptions about natural selection, the evolving populations in question, and so on, have to be presumed to hold. For instance, the effective population sizes of prey and predator populations matter in a given context. In small populations, such as island populations, fluctuations of population size *reduce* genetic variability and effect homozygosis for their loci. *Ceteris paribus*, populations with many homozygous loci are capable of expressing less variable phenotypes and trait variants than populations with many heterozygous loci, owing to their lesser genotypic potential or variability.[19] This could be taken to suggest that populations with many homozygous loci are not diverse and heterogeneous regarding the realization bases of biological properties.

The argument evidently also presumes that selection for the traits in question is frequency-dependent. Although frequency-dependent selection is often considered the major evolutionary mechanism for polymorphisms of species and populations, it is also evident that it is not the only mode of selection that exists and its importance as an evolutionary mechanism varies with respect to different taxa, traits, selective environments, and the like. Consider, for instance, what would happen if the selection within the species in the above example were frequency-*in*dependent.[20] This would suggest that there might be no arms races within and perhaps even between species in the prey or predator populations with regard to the biological properties mentioned, namely, catching the prey and predator avoidance, since there could exist an *optimal* adaptation for the populations with regard to their adaptive challenges, such as evolution of the "very quick feet" or a mechanically-optimal catching apparatus. This in turn implies that the realization bases for the biological properties may not be diverse or heterogeneous, since one or a few biological properties and their realizations could be selected by frequency-independent (stabilizing or directional) selection.

The idea of the points just outlined is that without these kinds of empirical and contingent assumptions about evolving populations or species, their selective environments, the modes and forms of natural selection (such as frequency-independent or frequency-

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[19] A locus on a chromosome is said to be homozygous when its alleles are identical with one another. A locus with non-identical alleles is called heterozygous. If a locus is heterozygous, then this implies – in the case of a diploid cell – that it could express one of three kinds of genotypes, such as *AA*, *Aa*, or *aa*, whereas a homozygous locus is capable of expressing only one of two kinds, *AA* or *aa*.

[20] In a frequency-independent selection, the selection favors some trait(s) over other(s), regardless of what their frequencies are in the population. In a frequency-dependent selection, it is typically the trait(s) that is infrequent that has an advantage over the more frequent one(s).

dependent selection, stabilizing, directional, and disruptive selection), effective population sizes, auto- and syn-ecology of the species in question, and so on, it cannot be claimed or presumed (even given the Red Queen rule) that selection for effects in Rosenberg's sense leads to diverse and heterogeneous realization bases for biological properties. Equally problematic in this context is that the assumptions concern empirical and case-specific facts, which evidently *cannot be argued for generally*, but which have to be established on a case-by-case basis.

Besides, there are evolutionary factors or "forces," such as generative entrenchment, that work to counter the considerations to which Rosenberg alludes. Generative entrenchment means that there are traits that effectively are impossible to modify because other things depend upon their current form (cf. Wimsatt 2007: 133-145). For instance, although evolution has had a long time to "proliferate" different realizations for biological properties, there nevertheless exists only one way of coding genetic information that is widely shared among terrestrial organisms – the code based on nucleic acids. In other words, the ubiquity of this kind of genetic code is an accident whose evolution could easily have been "derailed" in the past. Nevertheless, this code has become so entrenched as to be nearly impossible to change because so many other things depend on it. In other words, it represents a frozen accident. Consequently, it is no surprise that there are striking exceptions to Rosenberg's contention that biological realization bases are diverse and heterogeneous, such as the ubiquity of our genetic code. Interestingly, there are many paleontologists who are amazed by the fact that the possible morphological space is so "unexplored" in both extinct and extant taxa. One possible reason for this is that generative entrenchment (or developmental constraints in general) is more pervasive as an evolutionary factor than we have so far acknowledged.

The point here is not to list and question all the assumptions, conditions, and rules concerning natural selection that could lead to realization bases of biological properties that are diverse and heterogeneous. Rather, the point is that the claim that the realization bases of biological properties are diverse and heterogeneous is based on substantial contextual, contingent, and empirical assumptions about natural selection. Whether these assumptions hold has to be determined on a case-by-case basis. The relation between natural selection and multiple realizability is complex, and it is not a general or conceptual truth that natural selection (for effects) leads to radically diverse and heterogeneous realizations of biological properties.

At the same time, there is an explanation of why the argument from multiple realizations appears to be so persuasive, namely, the argument is based on a concept that rests on unreliable intuitions about multiple realizations of previous philosophers. In contrast to the ambiguous uses of and intuitions about multiple realization, recent analyses of the concept undermine the plausibility of Rosenberg's argument (see Bechtel & Mundale 1999, Batterman 2000, and Shapiro 2000). The common theme in these papers is that when the cases of multiple realizations are scrutinized, it seems that the realizations of higher-level properties cannot be as heterogeneous as many have supposed. Differences that exist between realizations turn out to be largely unimportant, and/or explanatory important things are shared by realizations. Thus, it appears that in a closer analysis of multiple realizability, Rosenberg's claim that the realizers of biological properties are diverse or heterogeneous seems unlikely to be true.

Consider the corkscrew example presented by Lawrence A. Shapiro (2000). In order to count as *different* realizers of the "corkscrew," the realizations must differ in ways that are relevant to their function as corkscrews, that is, in ways that affect how the corks are pulled out. Functionally and mechanically equivalent corkscrews of different colors, although different, are similar realizations of the corkscrew. If we were to count cases such as this as multiple realizability, then the claims about multiple realizability would become trivial. Therefore, it is not the case that *any variation* among or difference in the realizers of a property automatically or unconditionally implies multiple realizability.

In the corkscrew example, there are in fact different ways of realizing a "corkscrew": there are waiters' corkscrews, two-lever corkscrews, and so on. They differ in the causal mechanism that gives the corksrew the capacity to remove corks. Yet many *biological* properties that have been considered prime examples of multiple realizability evidently fail to be multiply realizable in this analysis (Shapiro 2000). For instance, the octopus eye and the mammalian eye differ in molecular structure: the lenses are composed of different kinds of proteins and the retinas utilize different visual pigments. Hence, this seems to be an instance of multiple realization. However, despite these differences, both the octopus eye and the mammalian eye have the same basic mechanism for producing vision. In both cases, light is refracted by the cornea and the lens so that a focused and inverted image falls on the retina, where the image is tranformed into electrical signals that are transmitted to the brain via the optic nerve. Therefore, these instances should perhaps not be counted as different realizations of the "eye." If paradigmatic cases like this turn out not to be cases of multiple realization, then Rosenberg's assumption that the realization bases of biological properties are diverse and heterogeneous seems implausible.

Reconsider, for instance, heat dissipation. The claim in the context of Rosenberg's argument is that there should be different means for reducing heat dissipation in cold climates. As a matter of fact, this is not true. First, there are not so many different realizers or ways of reducing heat dissipation in cold climates. Yes, there is the idea that a larger body size results in smaller "surface area to volume ratio" and the idea that modifications of animals' fur or plumage results in better insulation. But there do not appear to be many other kinds of realizers for the dissipation of heat. Thus, "dissipation of heat" evidently is not diverse and heterogeneous in realizers insofar as different mechanisms for heat dissipation are concerned. Second, the idea that larger body size results in smaller "surface area to volume ratio" is not, strictly speaking, something that affects an animal's heat dissipation, for it does not have any effect on dissipation *per se*[21] in contrast to changes in insulation that *do* affect an

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[21] Rather the idea is that since the ratio of "surface area to volume" is smaller in animals with larger body size than in animals with smaller body size, it follows that a *relatively smaller amount of metabolized heat* is dissipated through the surface area of larger-sized animals than smaller-sized ones.

animal's dissipation of heat. The same goes for such "realizers" to reduce heat dissipation as changes in an animal's behavior, dwelling underground, modifications in blood circulation, and so on, which as adaptations may affect the *fitness* of an organism in cold climates, but which, strictly speaking, do not affect an animal's *heat dissipation*. Consequently, there appears to be only one or a few general mechanisms for heat dissipation, namely, the mechanism that affects insulation.

As a conclusion, I suggest that Rosenberg‘s argument is at best inconclusive and at worst false insofar as its implications for the existence of biological laws are concerned. My analysis of the argument reveals that the argument rests on dubious assumptions concerning the nature of natural selection, laws of nature, and multiple realizability. And finally, even if there were no biological laws, there might still be biological explanations, *contra* Rosenberg (see chapter 4). Thus, the argument does not provide substantial reasons for doubting the existence of biological laws or explanations.

2.6 Evolutionary Contingency

Argument 9): Biological generalizations are *contingent*:

Contingency is an argument that accuses biological generalizations of being "accidental" rather than necessary in the sense that laws are supposed to be. Although many have had ideas similar to the above (see, for instance, Smart 1963: 50-63, Schaffner 1980: 88-90, and Rosenberg 1985: 130-135), the best explication of this argument is John Beatty's (1995; see also his 1981: 405-409) evolutionary contingency thesis.[22]

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[22] Apparently, Beatty developed his thesis as an elaboration of Stephen J. Gould's ideas of contingency, which Gould has presented in several papers and books (see, for instance, Gould 1989). Since Gould's ideas are less systematic and more vague than Beatty's, I will concentrate on Beatty. In a recent article, Beatty (2006) tried to systematize Gould's ideas of contingency. Regretfully I found this attempt to be unenlightening about

> Beatty (1995: 46-47) formulated his thesis as follows:
> All generalizations about the living world:
>
> a) are just mathematical, physical, or chemical generalizations (or deductive consequences of mathematical, physical, or chemical generalizations plus initial conditions),
>
> or
>
> b) are distinctively biological, in which case they describe contingent outcomes of evolution.
>
> The first part of this claim is meant to acknowledge that there are generalizations about the living world whose truth values are not a matter of evolutionary history. Evolution has not and will not result in any forms of life that are not subject to the laws of probability, or to Newton's laws of motion. Nor will evolution result in any carbon based forms of life that are not subject to the principles of organic chemistry. But while these sorts of principles are true of the living world, we do not call them "biological" principles…

The evolutionary contingency thesis tries to establish that biological generalizations are random or accidental products of history and/or other background conditions that lack nomic necessity. The idea is that, say, had there not been mitosis or had there been some equivalently fit or fitter alternative, it is likely that meiosis would not have evolved (Cleveland 1947), and thus **Mendel's** rules (see appendix) would not have evolved on this planet. In fact, there are non-Mendelian mechanisms of inheritance even on our planet (Crow 1979). If the environment changes so that these become as fit as or fitter than Mendelian ones, then they might become as omnipresent as Mendelian mechanisms are at present. Moreover, had Mendelian rules not evolved or had the conditions changed so that they no longer hold, then the **Hardy–Weinberg** (see appendix) rule might not hold, because it seems to be a consequence of Mendelian rules. In

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either Gould's or Beatty's ideas of contingency (see also Turner 2010 and Powell 2010).

a word, biological generalizations are accidental consequences of still other accidental consequences.

To Beatty (1995: 53-58), there are two kinds of contingency to which his evolutionary contingency thesis refers. He called these weak and strong contingencies.

By s*trong* contingency Beatty means first and foremost that biological generalizations lack the necessity associated with laws. Even if biological generalizations were true, there might be and quite likely will be other times, background conditions, or places in the universe or this planet where these fail to hold. Thus, biological generalizations are at most *accidentally true* generalizations that hold because of certain background conditions. Accidentally true generalizations moreover do not support counterfactuals (or subjunctives) as laws are thought to do. For instance, the generalization "all persisting lumps of pure uranium-235 have a mass less than 1,000 kilograms" appears necessary in contrast and in comparison to a seemingly contingent generalization "all persisting lumps of pure gold have a mass less than 1,000 kilograms." This is because the former generalization is dependent on few pervasive background conditions in order to hold true, whereas the latter is dependent on many different and shallow conditions.

Given strong contingency, secondly and differently, evolution can lead to different outcomes from similar starting points given the same or similar selection pressures. In other words, evolution is easily "derailed" by small changes in its initial conditions. Thus, even from the same selection pressures, similar or identical adaptations need not, and probably do not, follow, even given similar organisms in similar environments. As I will argue below, this latter sense of strong contingency (i.e., lack of trajectory stability) is not differentiated well enough by Beatty (or others) from his previous sense of strong contingency.

Beatty gives reasons why biological generalizations are strongly contingent. First, natural selection often selects from a set of trait variants that are similar in fitness, but differ in realization. Second, there are other evolutionary forces besides selection, such as genetic drift, mutations and so on, that affect evolution and its consequences.

Weak contingency evidently means simply that there are actual exceptions to biological generalizations. This is a common criticism of biological generalizations (see the argument 3). Many of us moreover think of weak contingency or exceptions as the central problem in the context of biological generalizations' lawlikeness (as in Smart 1963: 56 and Ruse 1970). This is unfortunate as will be argued below, because it is strong contingency that we should be worried about in this context, not its weak counterpart.

There are many replies to the evolutionary contingency thesis. What most of these replies share is the claim that Beatty has confused situations in which generalizations fail to apply with situations in which the generalizations are false. When biological generalizations are properly stated, they are shown to be 1) (universally) *true* versus Beatty's weak contingency and/or 2) *necessary* versus Beatty's strong contingency.

An ordinary reply to weak contingency is that biological laws are *ceteris paribus* laws that are not, contrary to appearances, riddled with genuine exceptions (cf. Carrier 1995 and Weber 1999). A *ceteris paribus* law holds only under conditions specified in its *ceteris paribus* clause. A *ceteris paribus* clause is a protective, hedging, or qualifying proviso – such as that the law holds only "when some other (unknown) factors remain absent and/or the same" – that prescribes the domain of a law outside of which the law does not necessarily hold. When a *ceteris paribus* clause of a *ceteris paribus* law is satisfied (its *cetera* are *paria*), then the law applies to a given situation. But when it is not satisfied (its *cetera* are not *paria*), the law does not necessarily apply to a given situation.

The idea of *ceteris paribus* laws is to show that biological generalizations are true and exceptionless, that is, not weakly contingent, because their "exceptions" come from situations to which biological generalizations do not apply. In other words, the exceptions of biological laws come from situations in which the laws' *cetera* are not *paria*.

A reply to strong contingency by Elliot Sober (1997) is that Beatty has failed to recognize that laws do not hold outside their domain of application. When this domain is added to the statement of a law, the result is a statement that is not contingent, but necessary.

For instance, the Hardy–Weinberg rule is restricted to situations in which "there are no evolutionary forces present." Let "I" denote this applicability clause, which defines a domain of application. When there are evolutionary forces present, the Hardy–Weinberg rule does not apply. Let this rule be written as a statement of a universal form, $\forall x(Fx \rightarrow Gx)$; the probabilistic formulation would not affect the point being made here.

According to Sober, we get *a non-contingent statement* if we add the clause "I" as an antecedent to the statement of the Hardy–Weinberg rule. That is, the statement "$I \rightarrow (\forall x(Fx \rightarrow Gx))$" *as a whole* is not contingent, although both of its components "I" and "$\forall x(Fx \rightarrow Gx)$" could be. In this way the necessity of biological generalizations is redeemed.

Sober's reply to strong contingency is not the same as the *ceteris paribus* account of laws above, but the two are nonetheless similar. Both aim to show that "contingencies" can be avoided by adding a condition to the statements of laws. In both, the applicability of a law is restricted by some general protective or proviso clause that specifies in very general (and vague) terms those conditions under which the law in question is expected to apply to a situation.

The *ceteris paribus* account of laws raises several empirical, epistemological, and semantic problems that are yet unresolved, at least given the proposal of *ceteris paribus* laws in the literature (see section 3.5). In other words, it has proven to be difficult to define what *ceteris paribus* clauses mean. Moreover, these same problems of the *ceteris paribus* account of laws seem to apply to Sober's reply given above. Consequently, I do not think neither is a convincing respond to Beatty's evolutionary contingency thesis.[23]

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[23] In another reply, Gregory J. Morgan (2009) has suggested that the **Caspar–Klug** theory of virus structure (see appendix) is a distinctively biological law and a counterexample to Beatty's evolutionary contingency thesis. Thus, Beatty's thesis is false. However, there are two problems with this strategy of arguing against the evolutionary contingency thesis. First, even if the Caspar–Klug theory of virus structure were a distinctively biological law, it would not demonstrate that the evolutionary contingency thesis is not true of biological generalizations *in general.* Although Beatty

Nor do these responses define or examine further the meaning of Beatty's contingency or contingencies. Rather they just replace an unexamined or vague notion with another notion or idea that is equally vague. For instance, strong contingency does not appear to be a primitive concept. Thus, rather than being an *explanans* of why biological generalizations lack nomic necessity or lawlikeness, strong contingency is an *explanandum* that needs to be analyzed. The same goes for weak contingency. But with what should they be analyzed?

Sandra Mitchell's (1997, 2000, 2002) idea is that strong contingency should and can be analyzed in terms of stability of generalizations. On the other hand, what was called above weak contingency should be analyzed in terms of scopes of generalizations as suggested by C. Kenneth Waters (1998). Beatty's evolutionary contingency thesis can thus be paraphrased and analyzed in terms of stability and scope:

1) According to weak contingency, biological generalizations are riddled with *actual exceptions*, that is, their scope is far from universal
2) According to strong contingency, biological generalizations *lack the stability* that guarantee that they would hold in many or all possible background conditions

Sandra Mitchell understands stability to be something that deals with those background conditions on which the holding of a generalization is dependent. In other words, it is "a measure of the

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formulated the evolutionary contingency thesis to address all biological generalizations, the alternative interpretation, one that formulates the evolutionary contingency thesis to concern biological generalizations in general, is meaningful, important, and defensible as a thesis. The fact is that Morgan offers no reasons to rebut this latter, more moderate interpretation of Beatty's evolutionary contingency thesis. Second, Morgan does not define what he means by a (strong) contingency of biological generalizations, but treats contingency as if it were a monolithic or primitive notion. But to demonstrate that his example is not evolutionarily/strongly contingent as a generalization, Morgan should re-think his example of a law in the light of my development of strong contingency presented below.

range of conditions that are required for the relationship described by the law to hold" (Mitchell 2002: 346).

As a first elaboration of Mitchell's stability, call a generalization (or regularity) stable if it holds in many possible background conditions. An alternative phrasing would be that a generalization is stable if it holds during numerous interferences. Unstable generalizations break down under such conditions. Stability is a concept of degree as well. In other words, a generalization is more stable the more possible background conditions there are in which it holds. I take it that the above characterizations of stability capture the essentials of Mitchell's definition of stability presented above.

The connection between strong contingency and stability is that these are concepts of degree that are *inversely related* to one another: the more strongly contingent a generalization is, the less stability it has and vice versa. Stability considerations of generalizations are evaluated on the basis of how many background conditions there are and what kinds of background conditions the generalizations are dependent or contingent on. The more stable a generalization is, the less dependent it is in holding in these background conditions.

Yet, there is a problem in Mitchell's idea that strong contingency should be analyzed in terms of lack of stability of generalizations. The problem is that she treats stability as if it were a monolithic thing, whereas stability actually is a conflation concept. In one way, stability is not problematic as a conflation concept, because we have different, yet related stability concepts that have definite meanings, which are sometimes covered by one concept, albeit imprecisely. The situation would be different and problematic if these different meanings of stability had nothing in common or were vague.

I have identified seven related stability concepts or meanings of stability that I define as follows:

1) Constancy: *lack of changes* in a system despite changes in its background conditions,

2) Persistence: the *survival time* of a system when facing changes in its background conditions,

> 3) Inertia: the ability of a system to *resist* during changes in its background conditions,
> 4) Elasticity: the *speed of return* of a system after changes in its background conditions,
> 5) Amplitude: the *area or extent of equilibrium* of a system during changes in its background conditions,
> 6) Cyclical stability: the ability of a system *to return to a cycle or an oscillation* despite changes in its background conditions,
> 7) 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.

Sandra Mitchell's (2000: 252 and 2002: 346) definition of stability as a measure of the range of possible background conditions required for a generalization, relation, or regularity to hold is too general or non-committal in that it does not distinguish between different forms of stability and their meanings. It is moreover only once we look carefully at her examples of generalizations and their stability, when it appears that she understood stability vaguely in the sense of my constancy, inertia, and/or trajectory stability without distinguishing their meanings from one another (see also section 5.5).

Now, it is not the exact number of different forms of stability that is important in the present context. Rather, the important point concerns differences and commonalities that the forms of stabilities have. What is common to these forms of stability is that they *describe what is needed from a system or a generalization in order to hold during changes of its background conditions* (or interferences). Nevertheless, these forms of stability are also different. Whereas constancy, persistence, and inertia deal with the *endurance* of systems or generalizations during changes in the background conditions, elasticity, amplitude, and cyclic stability deal with *recovery* during changes in the background conditions. Trajectory stability could be understood as dealing with the *sensitivity* or *inevitability* of systems or generalizations during changes in the background conditions. Thus, we have different, but related concepts of stability.

Stability should not be confused with the property of generalizations that was called *scope* (see Darden 1996: 413-414, Cooper 1998: 578, 581, and especially Waters 1998). Scope – as I define it – *describes the actual application domain of a generalization in the past and/or present.* This domain includes those (dis)similar systems to which some generalization applies or has applied. Biological generalizations typically generalize about different taxa, features, entities, and so forth in different times and/or places. Many, although not all, biological generalizations evidently have narrow or limited scopes.[24] For example, the scope of each of Mendel's two rules is all or nearly all sexually reproducing taxa. On the other hand, those conditions, like the evolution of mitosis and meiosis, on which Mendel's rules are dependent in their holding are their background conditions in the above sense of stability.

Scope and stability are different properties of generalizations. For instance, a generalization that has a narrow scope could have a high degree of stability within this scope. The converse could also be true: a generalization that has a broad scope could have a low degree of stability within its scope. Scope is different from stability in that it deals with generality having to do with the *actual* distribution or range of (dis)similar systems to which a generalization applies or has been applied. Stability, on the other hand, has to do with the holding of a generalization in certain background conditions. In other words, scope is a non-modal concept and property of generalizations, whereas stability is a modal concept and property of generalizations because it has to do with considerations that deal with possibilities and counterfactuals.

Next, I shall address the issue of what implications weak and strong contingency have for the existence of biological laws.

Weak contingency appears to be true of biological generalizations in general. This is a serious problem only if one holds an account of laws according to which laws are (or from which follow) universal and exceptionless regularities. However, minority of

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[24] See, for instance, Skutch (1949), Connell (1961), Hagmeir and Stults (1964), Brown (1971), Haartman (1971), Holmes (1977), Rabenold (1979), Connell (1983), Ralls and Harvey (1985), McFadden (1986), Hairston (1989), Gaston (1996b), Rohde (1996), Alroy (1998), Gaston *et al.* (1998), Hecnar (1999), Ashton *et al.* (2000), and Freckleton *et al.* (2003).

philosophers and biologists today think of biological laws in these traditional terms. Likewise there are accounts of laws that tolerate exceptions (cf. chapter 3). Consequently, it can be claimed that weak contingency is not a serious difficulty for the lawlike status of biological generalizations. This is one reason why I do not discuss Waters' (1998) reply to the evolutionary contingency thesis here in detail.[25] Instead I shall proceed to strong contingency.

Despite what has just been said about weak contingency, strong contingency is a serious problem for biological generalizations' lawlikeness in general, regardless of whether one maintains an account of law according to which laws can be weakly contingent or not. Why is this so?

The main reason for above is that many philosophers and biologists, including John Beatty, think of lawlikeness/nomic necessity and strong contingency as contrary and perhaps even contradictory. Lawlikeness is likewise understood to be an "either/or" concept, that is, a concept that has no degrees (however, see also section 5.4). Consequently, for many, a generalization is either necessary/lawlike or it is contingent/accidental.

This conceptualization of lawlikeness is shared by almost all definitions of lawlikeness. For instance, Beatty's (1995: 46-47, 52, 63-64) idea seems to be that laws are necessary and non-contingent, but there could be degrees of contingency (or even contingencies) that non-laws display. If it is true that biological generalizations could have been otherwise had their background conditions been different, then such generalizations are strongly contingent. If they are strongly contingent, then they are not laws, because there are no degrees of lawlikeness or nomic necessity. Now, I agree with this kind of reasoning. At the same time, I think that we can do better in

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[25] Another reason is that it his reply is given in terms of scientific explanations. In other words, the issue here is not whether scientific explanations are possible without (universal) biological laws, which is the issue in Water's paper. Rather the issue here is on the implications and analysis of contingency of biological generalizations when it comes to the existence of laws.

analyzing how and why strong contingency should have damaging implications for the lawlikeness of biological generalizations.

I identified seven stability concepts or meanings of stability above. Accordingly, there are seven possible interpretations of Beatty's strong contingency in terms of the stability of generalizations. There is no need to discuss all of them here, since not all the forms of stability are plausible as candidates in the present context. For instance, it does not seem to be a plausible interpretation of strong contingency to analyze it in terms of *persistence* or *elasticity* of biological generalizations. Nor does analyzing strong contingency in terms of *amplitude* or *cyclical stability* do justice to Beatty's thesis. In other words, it is not the recovery or temporal aspects of endurance of biological generalizations during their interferences or changes in background conditions that Beatty was concerned with as is the case with persistence, elasticity, amplitude, and cyclical stability, but rather with their more *general endurance* (constancy or inertia) *and/or their sensitivity or inevitability* (trajectory stability).

With the three remaining concepts of stability we arrive at a clearer definition of what strong contingency means. The three plausible interpretations of Beatty's strong contingency are that strong contingency criticizes biological generalizations of lack of stability in the sense of *trajectory stability, constancy, or inertia.* The trajectory stability seems to be the most common interpretation of strong contingency. I deal with it first by arguing that it is the weakest interpretation of the three.

First, low degree of trajectory stability does not seem at all distinctive of biological generalizations or systems. There are non-biological systems that exhibit low degrees of trajectory stability, such as chaotic systems in general. While chaotic systems can be deterministic, they are extremely sensitive in their dynamics, behavior, and end results to the tiniest differences in initial conditions. Consequently, the existence of a low degree of this stability does not seem to be a sufficient condition to attribute any special nomological status to biological generalizations. Whether trajectory stability appears in biology with especially low degrees in contrast to other sciences is a topic that I will not take up. Nor does Beatty give us any argument or justification for this.

Second, the connection between low trajectory stability and lawlikeness is an open question. Considerations of trajectory stability are important when we are explaining, manipulating, and predicting. But the effect of a low degree of such stability is not that it makes a generalization non-lawlike, non-explanatory, or non-predictive, but rather that we have to be sensitive and careful when setting and studying its initial and boundary conditions. Third, we should not confuse the process with its results. The process, such as natural selection, could be lawlike, although varying in results if its initial and boundary conditions were varied (see Carrier 1995: 85-89, 93-97 for a similar reply). Although it is possible that natural selection might produce alien-looking life forms on a planet that has antecedent and boundary conditions different from ours, this difference in results does not provide reasons to doubt the putative lawlike status of natural selection itself.[26]

Fourth, there are other biological forces that might foster a system's other forms of stability or its scope despite the fact that the system has a low degree of trajectory stability, e.g., canalization or generative entrenchment (Wimsatt 2007: 133-145). Although on this planet it is true that hereditary information is carried (mostly) by nucleic acids, this seems to be "a frozen accident": a conditional fact of our planet's and life's history. Had the historical or other background conditions been different, other materials could have evolved to do the job. Thus, the ubiquity of the genetic code is an accident whose evolution could easily have been derailed. Yet this code has now become so entrenched as to be nearly impossible to change because so many other things depend on it. Much of what was just said is also true of Mendel's rules.

In my view, the points outlined above make the trajectory stability interpretation of strong contingency inconsequential insofar as the lawlikeness of biological generalizations is concerned. Curiously and unfortunately it is this interpretation of strong contingency that many apparently have adopted when they think of Beatty's evolutionary contingency thesis (cf., for instance, Carrier

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[26] What has just been said should not be interpreted as suggesting that natural selection is a lawlike process, however.

1995 and perhaps Morgan 2009; cf. also Powell 2010 and his discussion of Gould's macro-evolutionary contingency).

Yet, strong contingency seems to have damaging implications for biological generalizations' lawlikeness, and for generalizations of other sciences if these are similarly contingent,[27] if stability is understood in the sense of constancy and/or inertia. Generalizations with low and moderate degrees of these forms of stability only hold in special or limited background conditions, whereas laws are traditionally understood to be very stable or maximally stable generalizations that hold in most, if not in all, background conditions.

Low and moderate degrees of these forms of stability deprive generalizations of their necessity and nomic force. Apparently, with these goes the support of counterfactuals, the distinctive property of laws, according to which these govern not just what actually happens, but what would have happened under various background conditions that did not actually happen. Nor are unstable generalizations "projectable" in the sense that laws are traditionally thought to be.

One reason why these two latter forms of stability deprive generalizations their lawlike status is again that lawlikeness is traditionally understood to be a dichotomous thing. Accordingly, anything but the maximal degrees or very high degrees of these forms of stability deprive the lawlike status of a generalization, because with degrees come restrictions where and under what background conditions these hold. Another reason is that biological generalizations in general seem to have low or moderate degrees of the above forms of stability. In other words, for many biological generalizations it is true that had some background conditions been different in the past or in the present, they would not hold (in addition to Beatty 1980, 1981, 1995, 1997 and Mitchell 2000, 2002, see also Skutch 1949, Hutchinson 1953, 1961, Utida 1953, Savile 1960, Slobodkin 1964, Paine 1966, Paine and Vadas 1969, Dayton 1971, Wiens 1977, Crow 1979, Rabenold 1979, Sober 1984: 34-36, Duràn & Castilla 1985, Ruse 1988: 18-19, and Ghiselin 1989 for

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[27] Beatty (1995: 47, 63) suggest that it is not only biological generalizations that are strongly contingent but also those of other sciences, including physical and chemical ones. Cf. also Mitchell (2000: 250-259).

examples and discussion). The *ceteris paribus* law account literature is actually riddled with examples of this kind (see, for instance, Kincaid 1990: 77, Ereshefsky 1991, Weber 1999, and Lange 2005a).

Consequently, I suggest that Beatty's strong contingency represents a serious problem for biological generalizations' lawlikeness, because laws are commonly understood to be maximally or highly stable generalizations in the sense at least of constancy and/or inertia. And *if* biological generalizations display (moderate or low) degrees of these forms of stability, then they are accidental generalizations.[28] This is another sense in which my and Mitchell's examinations of Beatty's evolutionary contingency thesis differ for she does not think that the thesis has such implication. Sandra Mitchell has presented an alternative account of laws as a response Beatty's evolutionary contingency thesis. Her account turns the stability of biological generalizations into their virtue. I do not think that her account of laws is adequate to redeem the lawlike status of biological generalizations, owing to various difficulties in the account (see section 3.2).

I hope to have shown why Beatty's evolutionary contingency thesis is a rare case of success among the arguments against the existence of biological laws presented in the literature if biological generalizations are contingent in the ways depicted above.

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[28] I have treated constancy and inertia together here since their differences do not matter in the context. However, the two forms of stability are different, and their differences matter in other contexts. Constancy and inertia possibly have different (epistemic) functions in the context of scientific explanations, a topic that will be discussed elsewhere (cf. sections 4.6 and 5.6). The two forms apply to different systems as well. Constancy is meant to apply to systems that do not exhibit dynamics, whereas inertia is meant to apply to systems that exhibit some dynamics. In other words, the difference between constancy and inertia has to do with the difference between *non-change-relating* generalizations ("co-existence laws") that are stable in the sense of constancy and *change-relating generalizations* ("succession laws") that are stable in the sense of inertia (see also chapters 4 and 5).

2.7 Conclusions

I have analyzed nine arguments from various allegedly distinctive features of biological generalizations or phenomena to the absence of biological laws. Although my analysis of these arguments is perhaps not exhaustive, I nonetheless claim to have dealt with the most important of the arguments against biological laws.

If my analysis of these arguments is correct, then it seems to follow that there are no distinctively biological laws. Taken together, weak and strong contingency pose a threat, both to biological generalizations' lawlikeness and to their truth. Biological generalizations' strong contingency suggests that they are not lawlike, whereas their weak contingency suggests that they are not even accidentally true generalizations, owing to their open-ended set of exceptions that defy systematic and simple treatment. Thus, biological generalizations apparently hold only in limited spatio-temporal domains.

This seems to create a problem about the scientific explanations of the biological sciences, as have been noted by many scholars (e.g., Pietroski & Rey 1995 and Rosenberg 2001c). It seems that biologists provide with legitimate, successful, and autonomous scientific explanations. But how do they manage to do this if laws are necessary for scientific explanations as was claimed in the first chapter?

3

ON LAWS AND EXCEPTIONS

3.1 Genuine and Apparent Exceptions

In this chapter, I will analyze accounts of laws that tolerate exceptions. According to these accounts, biological generalizations could be lawlike with exceptions if the generalizations are pragmatically or paradigmatically similar to laws, inference tickets, *ceteris paribus* laws, or if they held with probabilities. Another topic in this chapter is the distinction between two kinds of exceptions, the genuine and the apparent.

Let us call an instance (Fa & $\neg Ga$) a *negative instance* of a generalization of a universal $\forall x(Fx \rightarrow Gx)$ or probabilistic $Pr(Gx/Fx) = r$ form. Let us call an instance (Fa & Ga) a *positive instance* of these generalizations. Instances ($\neg Fa$ & $\neg Ga$) and ($\neg Fa$ & Ga) are cases to which the above generalizations do not apply. Generalizations apply only to cases where their antecedents are satisfied.

What I call a *genuine exception* to a generalization is a falsifying or disconfirming negative instance of that generalization. If the antecedents of generalizations are as simple as in the generalizations $\forall x(Fx \rightarrow Gx)$ and $Pr(Gx/Fx) = r$, then the above negative instance (Fa & $\neg Ga$) represents a genuine exception. This is because the generalizations' antecedents are satisfied in the case of (Fa & $\neg Ga$), whereas their consequents are not. What I call an *apparent exception* is not a falsifying or disconfirming negative instance of a generalization, but its harmless negative instance, which only has the appearance of a genuine exception.

A claim frequently encountered in the literature on biological laws is that the antecedents of biological generalizations are not as simple as they are in the examples above. Rather, biological generalizations have complex antecedents that, in addition to the predicate term Fx, include qualifications or provisos that restrict the applicability of the generalizations. This claim maintains that negative instances of biological generalizations, such as $(Fa \,\&\, \neg Ga)$ are the apparent exceptions because the complex antecedent of biological generalizations is not satisfied.

The *ceteris paribus* account of laws is intended to establish that the negative instances of generalizations are not genuine exceptions, but only apparent exceptions to generalizations. The idea is that a protective clause – denote this with a predicate term CPx – is included in the statement of a law. The inclusion of this clause in antecedent of a statement of a law shows why negative instances of a *ceteris paribus* law are apparent exceptions rather than genuine ones, since they are instances $((Fa \,\&\, \neg CPa) \,\&\, \neg Ga)$ of a law that has the form $\forall x((Fx \,\&\, CPx) \rightarrow Gx)$ or $Pr(Gx/(Fx \,\&\, CPx)) = r$. Thus, *ceteris paribus* laws are riddled with apparent exceptions, which are *not* instances of these laws, owing to the fact that the laws' complex antecedents are not satisfied. Consequently, it could be claimed that biological generalizations are without genuine exceptions and possibly are even universally true if they are interpreted as *ceteris paribus* laws. Related idea is that what distinguishes the apparent exceptions from the genuine is that the apparent exceptions – in contrast to the genuine ones – can be explained and there is a corrective asymmetry in the sciences insofar as the legitimate explanations of exceptions to generalizations are concerned.

In this chapter I will claim the accounts of laws that tolerate exceptions, as suggested by Sandra Mitchell, Marc Lange, and many others, have difficulties in redeeming the lawlike status of biological generalizations. In the next section, I criticize two alternative accounts of laws suggested by Sandra Mitchell, which she has labeled the pragmatic and paradigmatic accounts. In section 3.3, I take up Marc Lange's account of laws as inference tickets. In section 3.4, I present the difficulties in interpreting biological generalizations as probabilistic laws. In section 3.5, I criticize the *ceteris paribus* account of laws. In section 3.6, I criticize the idea that explanations of exceptions to generalizations are exclusively or typically expressed in terms of the mechanisms and/or the vocabulary of the lower-level sciences.

3.2 Pragmatic and Paradigmatic Accounts of Laws

Sandra 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, which established the criteria that generalizations 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 biological generalizations – creates difficulties, suggesting that there are no biological laws.

As an alternative, a *paradigmatic* account is a descriptive or comparative definition of laws that takes some examples of generalizations – presumably from physics – as examples of laws. It then compares them and their properties to other generalizations such as biological ones to see how the latter live up to the standards suggested or described by the examples.[29]

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

Are the pragmatic and paradigmatic accounts of lawlikeness of any help in salvaging the lawlikeness of biological generalizations?

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[29] Mitchell refers to Martin Carrier (1995) as a supporter of a paradigmatic account of laws.

There are problems with Mitchell's proposed pragmatic account. The first is the question of whose roles of laws should we adopt and why? Moreover, what roles are used to evaluate the lawlike status of generalizations? Rather than addressing these problems, I propose that the central role of laws has to do with providing scientific explanations. That this is the central and perhaps even the proprietary role of laws is an idea shared by many philosophers of science, including Sandra Mitchell. Other roles have also been addressed in the literature, but since I have already addressed many of these (in section 1.5), I shall not take them up here. Moreover, it is an open question as to how laws function in many of these roles. In fact, it is an open question as to whether laws can function successfully in many of these other roles, many of which are also secondary in importance in the sense that by explicating what scientific explanations consist of, these other roles are reduced or even eliminated. Finally, the idea that the central role of laws is to provide scientific explanations is not only relatively well-developed as a philosophical idea, but also is an idea that has practical importance for scientists, as was argued in chapter 1.

Another, more serious difficulty with the pragmatic account is that Mitchell has not provided a (normative) model of scientific explanations. How are we to evaluate how well the generalizations fulfill the (central) roles of laws if no model has been given that provide explicit criteria for these roles?

On the other hand, Mitchell has carried out a preliminary work toward providing such an account. In this context she refers first and foremost to the stability of generalizations[30]:

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[30] Actually, Mitchell's account is multidimensional. In addition to stability, she refers to the degrees of abstraction, simplicity, and cognitive manageability as the other relevant dimensions of laws. Mitchell does not have much original to say about these other dimensions. Ultimately, she concentrates on stability and its analysis and devotes little effort to analyzing these other dimensions of laws. For these reasons the degrees of abstraction, simplicity, and cognitive manageability can be ignored in the discussion here. This does not mean that I am targeting a straw-man version of Mitchell's

> 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 generalizations. This is not her point, however.[31] Rather Mitchell's point is that stability is a central property of generalizations whose possession makes them explanatory.

The main difficulty with the pragmatic account of laws is that stability is a wrong property insofar as the explanatory power of generalizations is concerned. Moreover, the central idea of Mitchell's pragmatic account is incorrect, because there appear to be non-laws functioning in the role of laws. In other words, there are explanatory generalizations that are invariant *rather than* lawlike and/or stable (see also Woodward 2001: 13-17).[32] Since in chapters 4 and 5 I will argue

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account, however. Clearly, Mitchell thinks that stability is a central dimension of her pragmatic laws.

[31] Other authors have been engaged in just such an account (see section 5.4).

[32] Of course, we can stipulate the word "law" to mean whatever we wish. For instance, it could be claimed that if invariant generalizations 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

for the claims just made, I will proceed here to the paradigmatic account of laws.

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

Consider the ideal gas law. 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 (physical) 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 generalization (Salmon 1984: 121, 136, 227), not a causal or explanatory one, whereas others hold the opposite (Woodward 2000: 207-209, 218-219). While many think that the ideal gas law is an empirical generalization, some 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 & Roberts 1999: 461-462). Among those who think that the ideal gas law is a law, there are various ideas – even contrary or contradictory ones – about what kind of a law it is: a *ceteris paribus* law (Pietroski & Rey 1995: 82-83, 89-91, 97-98), a coexistence law (Hempel 1965:. 242 and 352), a succession law (Salmon 1984: 135-136, 227), a law about

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is a law regardless of what other properties the thing has." However, the fact is that invariant generalizations are quite unlikely to be laws, at least when compared to the ways in which most philosophers and scientists think of laws. Moreover, there might be other reasons to reserve the designation "laws" for generalizations that are not invariant and that have to do with such issues as induction, confirmation, theory structure (in physics), and so on.

disposition (Lipton 1999: 166, Cartwright 2002: 428, 430-433), and so on.

The point of the list above is to show 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, the point does not really concern the ideal gas law itself. A comparable situation holds for other (more fundamental) physical generalizations, such as Newton's laws of motion and gravitation, for which there are also different interpretations as to what kind of generalizations they are and what their central (lawlike) properties are.[33] In other words, different people not only view the same examples of physical generalizations as displaying different properties, but they also view different physical generalizations as examples of laws.

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 generalizations are that can then be compared to other generalizations in order to see whether they live up to the standards suggested or described by the paradigms of laws. Note that for the paradigmatic account, some consensus is required on what the examples of laws and their central properties are, for this is what *by definition* distinguishes the paradigmatic account from the normative account 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 (physical) lawlike generalizations of which all laws have some degree. Rather than discussing the idea here, I take it up and evaluate it in section 5.4.

What has been said about physical generalizations applies as well to biological generalizations: for instance, there is no consensus or agreement among philosophers (or biologists for that matter) on

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[33] See, for instance, 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.* (2001), and Smith (2002).

how **natural** selection should be interpreted or what kind of a generalization it is.[34]

If the situation is divided among philosophers, is it less varied among scientists? The answer is in the negative. For instance, ecologists have no consensus on the central properties of ecological lawlike generalizations and/or what the paradigmatic examples of ecological laws are. In fact, ecologists hold non-committal ideas about laws (as was discussed in section 1.4). I assume that the situation is similar among physicists as well. Thus, it is implausible that scientists help very much in this context.

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

3.3 The Inference Ticket Account of Laws

Traditional inference rule account of laws maintained that laws are not statements, as many logical positivists thought of laws (cf. section 1.3), but *rules of inference* that give justified inferential moves from the facts described by the antecedents of laws to facts described in their consequents without any indirect extra premises or statements, such as $\forall x(Fx \rightarrow Gx)$ in the background (see Schlick 1931/1961: 190[35]and Toulmin 1953: 88-93).

The inference that "had this object been copper, then it would have been an electric conductor" is justified, because the law "all copper objects are electrically conductive" works as a rule to validate it, whereas accidentally true generalizations do not work in this way. The inference rule account encountered some serious difficulties, not least because the account was coupled with the denial that laws are statements, but I shall not take up these issues here (see Nagel 1956, 1961: 137-140; Hempel 1965: 354-359).

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[34] Cf. Scriven (1959), Williams (1970: 362), Ruse (1973: 38-41), Peters (1976), Brandon (1978), Mills and Beatty (1979), Reed (1981), Rosenberg (1985: 126-129, 154-169, 211-225, 239-243), and Rosenberg and Kaplan (2005).

[35] The page number here refers to the English translation of the paper.

Marc Lange (1993a, 1993b, 1995) has developed the account further. Lange (1993b: 14) has dropped the "instrumentalist" idea of the traditional account and claimed instead that laws as inference rules are statements, that is, things capable of having truth-values. The result is that some of the previous difficulties of the inference rule account do not apply to Lange's account.

Since Lange's views of laws as inference rules are complex, I am not going to discuss all of them, simply because there is no space to do so. For instance, I will not discuss his views of inductive strategies in the context of laws. Here is how Lange describes a central aspect of his account of laws as inference rules:

> Whether scientists regard some claim as stating a natural law is revealed not by whether they call it a "law" but by how they use it. If a scientist takes some claim to be a law statement, then she uses it to perform various functions that she does not regard accidental generalizations as able to perform. Although philosophers have yet to state these functions precisely, they have long believed that these functions involve counterfactual conditional, scientific explanations, and inductive confirmation. (Lange 1995: 435.)[36]

Again, laws function as rules of inference, whereas accidental generalizations do not: laws justify inferences and therefore fulfill the functions of laws.

The above reminds me of Sandra Mitchell's pragmatic account discussed earlier, and if this is what Lange intends by his account, then both authors share some similar difficulties. Yet there is a further element in Lange's account of laws as inference rules, namely, the idea that only rules of inference that are "reliable" are laws, which gives some extra content to Lange's account:

> [A] law need not be associated straightforwardly with regularity. It may be associated with an inference rule that is 'reliable' – i.e., that leads to conclusions close enough to the truth for the intended purposes. (Lange 2002: 411.)

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[36] See also Lange (1993a: 243, 1993b: 14, and 2002: 412-413) for similar ideas and passages.

Lange is aware that biological laws – and perhaps even laws of physics – are not reliable rules of inference without provisos. Thus, Lange admits that biological laws are "contingent" (see Lange 1993a, 1995, 2002, and 2005a). This shows that they are not strict laws, but *ceteris paribus* laws (Lange 1993a, 2002). Thus, we can infer that an object is a G from the fact that it is an F on the basis of the law $\forall x(Fx \rightarrow Gx)$ only *ceteris paribus*.

The *ceteris paribus* account of laws raises several difficult problems that are unresolved, at least given the main theories and proposals of such laws in the literature (see section 3.5). 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 and 2002) has insisted that *ceteris paribus* laws might be pragmatically justified or that there exist "shared implicit and tacit scientific background knowledge" that is in need of no explication or exact determination that determines what the exact and intended meaning of a *ceteris paribus* clause is. It is this same background knowledge that then furnishes us with *reliable* rules of inference. Lange's appeal to "background knowledge" determining the meaning of *ceteris paribus* clauses postpones the problem to one of defining in more general and philosophically enlightening terms what this background knowledge consists of.[37]

These reasons suggest that Lange's inference rule account of laws does not offer an account that salvages the lawlikeness of biological generalizations. To Lange, there is more to lawlikeness than

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[37] Lange has presented many interesting examples of how laws function as reliable inference rules in specific contexts, given some audience, and so on (see Lange 1993a: 242-244, 1995: 440, 443-445, 2002: 411, 416). However, giving specific examples and cases of "reliability" of laws does not amount to giving a general, explicative, normative, and/or methodologically clarifying "theory" about these matters. Nor does it amount to giving *ceteris paribus* clauses general truth conditions or meanings.

the idea that laws function as reliable inference rules, namely, the idea that laws are collectively stable generalizations, which I take up in section 5.4. It is the latter idea and the account of laws that I consider to be more promising in providing a general account of (biological) laws rather than Lange's inference rule account of laws.

3.4 Probabilistic Laws

An anticipated idea is that biological generalizations should be interpreted as probabilistic laws.[38] This is a simple and straightforward solution to the problem of weak contingency or "exceptions" of biological generalizations (section 2.6). Whereas universal laws claim something about all *F*s and *G*s, probabilistic laws state what happens in a *definite proportion* of *F*s and *G*s. In other words, the scope of probabilistic laws is not universal, but tolerates *genuine* exceptions. Also many biological generalizations appear probabilistic, as biologists are aware. For instance, Bernhard Rensch (1959: 123-126, 1960: 107-110) reported percentages in which many ecological and evolutionary generalizations hold, including **Allen's**, **Bergmann's**, the **clutch** size, **Cope's**, **Dollo's**, and **Gloger's** rule (see appendix). However, I will argue that there is a serious problem in interpreting ecological and paleobiological generalizations as probabilistic laws.

In the context of probabilistic laws, the problem is not the weak contingency of biological generalizations for which the probabilistic law account offers a simple solution. Rather the problem is their strong contingency, which in the context of probabilistic laws emerges as a problem that biological generalizations evidently do not offer *stable probabilities with which these generalizations hold in different backgrounds*. In the given context the problem is that stable probabilities are required from probabilistic *lawlike* generalizations. Thus, instead of claiming that a definite or constant proportion of *F*s

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[38] A probabilistic law can be written as $Pr(Gx/Fx) = r$, which states that the probability of an *F* to be a *G* is *r* or that the frequency of *G*s among *F*s is *r* or that in any collection of sample *F*s, *r* percent will be *G*.

are *G*s in all, or at least most, background conditions to which a probabilistic law applies, biological generalizations display probabilities that evidently *vary* between and among different taxa, times, places, and so on.

Many studies show that some ecological rules, such as Bergmann's rule or the **latitudinal** diversity gradient, hold *at some percentage* for certain taxa, times, and/or places, whereas other studies show they hold *at other percentages* for other taxa, times, and/or places – or that they do not hold at all for some taxa, times, and/or place (see Davis 1938, Dale 1940, Rabenold 1979, Gaston 1996b, Hagmeier & Stults 1964, Ralls & Harvey 1985, Geist 1987, Ashton *et al.* 2000, Freckleton *et al.* 2003).The same is true for evolutionary (or paleobiological) rules, as well as for allometries and scaling laws. This is in contrast with physical probabilistic lawlike generalizations that evidently hold with stable probabilities that do not vary from one background to another, cf. the laws of radioactive decay.[39]

I am not the first to make this observation. Michael Ruse (1973: 60) had something similar in mind in the following passage:

> The theory of evolution cannot… assign a definite percentage to the number of exceptions to the evolutionary rules – for this reason it is perhaps better to regard them as "loose laws"…. rather than as statistical laws. The whole point about a statistical law like the H–W [Hardy–Weinberg] law is that the statistical part is just as necessary as the rest of the law – following form the H–W law is not only the fact the there will be three genotypes in future generations (say, A_1A_1, A_1A_2, A_2A_2), but also the fact that the distribution will be p^2 A_1A_1 : $2pq$ A_1A_2 : q^2 A_2A_2. In the case of evolutionary laws… no definite percentages follow from evolutionary theory. We

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[39] In developing the above argument, I had ecological and paleobiological generalizations in mind. Some authors have suggested that generalizations from evolutionary biology, such as what Brandon (2006) has dubbed as "the principle of drift," could be probabilistic laws. Although I think that the argument just presented applies to biological generalizations in general, I shall not argue for this here.

> cannot, for example, infer that exactly 30 per cent of nearctic animals will be exceptions to Bergmann's principle.

The problem is again that the probabilities of biological generalizations are not stable but vary from one background to another. Another way to put this problem is to the observation that biological generalizations seem to have an open-ended set of exceptions that defy systematic and simple treatment. This suggests that biological generalizations are not probabilistic strict laws but *ceteris paribus* laws. This is what I take that Ruse have in mind in the above passage when he calls them as "loose" rather than as statistical laws.

3.5 *Ceteris Paribus* Laws

Currently, the *ceteris paribus* account of laws is popular among philosophers in the special sciences. Marc Ereshefsky (1991: 65-67), Martin Carrier (1995: 88), and Marcel Weber (1999), to mention a few, are among those who have suggested that biological generalizations should be interpreted as *ceteris paribus* laws. Laws that *do not* contain *ceteris paribus* clauses are called "strict" laws in the literature. The idea of the *ceteris paribus* account of laws is that a *ceteris paribus* clause – denote this with the predicate term CPx –, is added to a statement of a universal or probabilistic strict law – as in $\forall x((Fx \& CPx) \rightarrow Gx)$ or $Pr(Gx/(Fx \& CPx)) = r$ –, which then specifies those conditions in which the law holds.[40]

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[40] There are other ways to include *ceteris paribus* clauses in statements of laws besides placing them in the antecedents. Nevertheless, I continue to speak as if *ceteris paribus* clauses were added in the antecedents of law statements, since this simplifies the discussion of c*eteris paribus* laws. Furthermore, the other ways are logically equivalent or logically "equisatisfiable." Besides, the problems of the *ceteris paribus* law account are independent of the different logical and formal ways we use to express or include *ceteris paribus* clauses in law statements.

A *ceteris paribus* clause is a protective, hedging, or qualifying proviso that prescribes the domain of a law outside of which the law does not necessarily hold. When a *ceteris paribus* clause of a *ceteris paribus* law is satisfied (its *cetera* are *paria*), then the law applies to a given situation. But when it is not satisfied (its *cetera* are not *paria*), the law does not necessarily apply to a given situation. The idea is to show that biological generalizations are not weakly contingent, because their "exceptions" come from situations to which biological generalizations *do not apply*. In other words, the exceptions of biological laws come from situations in which the laws' *cetera* are not *paria*.

The *ceteris paribus* account of laws purports to show that biological laws are riddled with exceptions that are apparent rather than genuine. Thus, a *ceteris paribus* law can even be riddled with an extreme number of exceptions as long as its *ceteris paribus* clause is not satisfied. Consider a situation in which the antecedent predicate, Fx, of a law applies, but law's *ceteris paribus* clause and consequent do not apply. This is not a genuine exception to a *ceteris paribus* law, but only an apparent exception, since the *ceteris paribus* law $\forall x((Fx \,\&\, CPx))\rightarrow Gx)$ or $Pr(Gx/(Fx \,\&\, CPx)) = r$ does not apply to the instance $((Fa \,\&\, \neg CPa) \,\&\, \neg Ga)$, the reason being that the law's complex antecedent is not satisfied. In other words, the law's *cetera* were not *paria*. An otherwise similar instance to which the *ceteris paribus* clause applies, $((Fa \,\&\, CPa) \,\&\, \neg Ga)$, is a genuine exception to a *ceteris paribus* law. Moreover, probabilistic *ceteris paribus* laws can exhibit unstable and varying probabilities as long as their *cetera* are not *paria*. These points suggest that we have an account that fits the appearances of biological generalizations.

A *ceteris paribus* clause of a *ceteris paribus* law should not be eliminable by a complete and finite list of external factors that could interfere with the law in question. In most cases, such an elimination or listing of factors cannot be done because we do not know all the interfering factors of a generalization, and/or the result would amount to a formulation of a law that is very complex in form. Yet if such an elimination or listing of factors can be done, and is carried out, then this amounts to a "lazy meaning" of *ceteris paribus* laws. In this case, we have a law that is *strictly complemented* and in which all of its interfering factors, Ix_i, are mentioned in a conjunct of their negations in the antecedent of a law statement, such as in $\forall x(Fx \,\&\, (\neg Ix_1 \,\&\, \neg Ix_2 \,\&\, \neg Ix_3 \,\&\, \ldots \,\&\, \neg Ix_n) \rightarrow Gx)$. We thus have a law with conditions of applicability fully specified, whereby the law applies to cases in which the listed factors, Ix_i, are absent. Strictly complemented laws are strict laws, and their problems are not those of *ceteris paribus* laws. Consequently, genuine *ceteris paribus* laws at least refer to some unknown interfering factors.

Suppose that the factors interfering in biological generalizations are so heterogeneous, complex, and numerous that at least in *practice* they defy simple and systematic treatment that would eliminate them by strict complementation. *Prima facie*, this is the right observation: many biological generalizations evidently have an open-ended set of exceptions that defy such a treatment, such as Gause's rule.[41] Appearances thus suggest that biological generalizations are or could be *ceteris paribus* laws. I wish to remain silent on the (metaphysical) issue of whether the number of interfering factors is so large, complex, or so heterogeneous in nature as to make the elimination of *ceteris paribus* clauses impossible in biology in principle. Some authors think that this is true of the special sciences in general, and some think it is even true of physics.[42]

The *ceteris paribus* law account is perhaps the most promising for salvaging the lawlike status of "contingent" biological generalizations. Regretfully, the account raises empirical, epistemological, and semantic problems that are unresolved, at least given the current accounts in the literature. In other words, it has proven to be a difficult problem to define what *ceteris paribus* clauses mean. If a more precise meaning is not found, then we have

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[41] The classic ecological reference here is Leslie *et al.* (1968) (see also sections 2.3, 2.4, and 2.6).

[42] See Beatty (1980: 552-553, 1995: 62-63), Cartwright (1983: 112, 1989: 206-212, but see also her 2002), Giere (1988: 40), Ereshefsky (1991: 66), Lange (1993a: 234-235), Pietroski and Rey (1995: 101-102), Schurz (2001a: 361), and Teller (2004: 733).

generalizations that are empirically, epistemologically, and semantically vague.

What do *ceteris paribus* clauses mean or how should we translate them into meaningful sentences? Evidently, the best non-literal translation of *ceteris paribus* clauses is "when some other (unknown) external factors remain absent and/or when some other (unknown) external factors remain constant" (cf. Schurz 1995, 2001a, 2002). In other words, *ceteris paribus* clauses cover the absence of or the lack of change in the unknown external interfering factors that are responsible for the law not applying in some conditions. Since this translation of *ceteris paribus* clauses is too general, non-committal, and ambiguous, we need to say something more specific about their meaning.

The central problem of the ceteris paribus law account is thus to give *ceteris paribus* clauses semantics or truth conditions. The problem involves not only giving formal truth conditions for *ceteris paribus* laws, but also to give *ceteris paribus* clauses definite content and meaning that allow us to distinguish legitimate uses of *ceteris paribus* clauses from illegitimate and *ad hoc* uses. Without such semantics, how can we tell whether a *ceteris paribus* law applies to a situation? In other words, we need an idea of how apparent exceptions can be distinguished from genuine ones. One possibility is that we have an apparent exception to a *ceteris paribus* law when the exception is explainable by factors or explanations that are causally and logically independent of exceptions and that explain or cause other things as well (cf. Pietroski & Rey 1995: 84-95). Another, related possibility is that explanations of exceptions are given in terms of "lower levels": an apparent exception to a *ceteris paribus* law comes from a situation in which the law's *completers* are interfered with by external factors (see Fodor 1991; see also section 3.6). Both suggestions, however, are problematic and riddled with counterexamples.

A related worry is that *ceteris paribus* clauses make generalizations "empirically vacuous." Empirically vacuous generalizations make trivially true or *ad hoc* claims, such as "all *F*s are *G*s, except when they are not," which are immune to empirical testing and disconfirmation. Empirically vacuous generalizations also lack explanatory and predictive power.

These are not the only problems with the *ceteris paribus* account. There is a danger that the account permits *accidentally true* generalizations to qualify as *ceteris paribus* laws. Thus, the account threatens to blur the distinction between laws and accidents. More worrisome is that some clear examples of *false, arbitrary, and accidental* generalizations – such as "all white substances are poisonous for us to consume" and "all spherical objects are good electrical conductors" – count as examples of *true ceteris paribus laws* according to the major accounts of *ceteris paribus* laws in the literature (for instance, on this basis, Mott 1992; Earman & Roberts 1999, and Woodward 2000: 247-251, 2002a: 307-313 criticize various accounts of *ceteris paribus* laws, such as those of Fodor 1991 and Pietroski & Rey 1995 discussed above).

I am not going to argue against different specific *ceteris paribus* accounts of laws,[43] since I have criticized them elsewhere (Raerinne 2003). In addition, there is already an extensive critical literature on the *ceteris paribus* account.[44] My claim is that all of the specific accounts presented in the literature run into the several problems mentioned above, and thus, none is a satisfactory account of laws.

The problems of the *ceteris paribus* account apply to another exception-tolerating account of laws, namely, the disposition account (see, for instance, Cartwright 1989: 141-182, 1995a, 1997, 2002; Horgan & Tienson 1990; Woodward 1992; Hüttemann 1996; Lipton 1999). The problem with the disposition account is that dispositions manifest themselves only when "when some other (unknown) external factors remain absent and/or when some other (unknown) external factors remain constant." Consequently, the epistemological, semantic, and empirical problems that plagued the *ceteris paribus* account similarly plague the disposition account. Without solutions to

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[43] See Cartwright (1983: 44-73, 2002), Hausman (1988), Kincaid (1990), Fodor (1991), Mott (1992), Lange (1993a, 2002, 2005a), Pietroski and Rey (1995), Schurz (1995, 2001a, 2001b, 2002), Silverberg (1996), Morreau (1999), Glymour (2002), and Spohn (2002).

[44] See Schiffer (1991), Mott (1992), Wallis (1994), Earman and Roberts (1999), Woodward (2000: 247-251, 2002a, 2003a: 307-311), Schurz (2001a, 2002), Earman *et al.*(2002), Mitchell (2002), and Smith (2002).

these, the disposition account is as unsatisfactory and unenlightening as a solution to the contingency of biological generalizations as the *ceteris paribus* account.

The meaning of *ceteris paribus* clauses in generalizations is typically locally, discipline-, and/or science-specific (cf. Lange 1993a, 2002; Levins 1998; Woodward 2006b: 13-15). For example, physicists are interested in the symmetry conditions of their generalizations (see Woodward 1992: 203-204 and Earman 2004), 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 generalizations hold under the changes of an agent's *information* as in Akerlof's lemon markets (see Sudgen 2002: 108-112). How are such conditions to be accounted for by general and abstract *ceteris paribus* clauses if these conditions are about locally and contextually-dependent things? Sandra Mitchell (2002: 339) has criticized the *ceteris paribus* account of laws from a similar angle:

> It is not particularly useful to say *that* laws are contingent or that they can be re-written as *ceteris paribus* generalizations, without detailing *what* kinds of conditions they depend upon and *how* that dependency works. Only by further articulating the differences rather than covering them over with a phrase denoting the existence of restrictions, can the nature of complex systems be taken seriously. The problem of laws in the special sciences is not just a feature of our epistemological failings; it is function of the nature of complexity displayed by the objects studied by the special sciences. Providing a more adequate understanding of laws in the special sciences requires a better taxonomy of contingency so that we can articulate the several ways in which laws are *not* "universal and exceptionless".

In lieu of the *ceteris paribus* account of laws and its solution to the "contingency" of biological generalizations in terms of abstract, schematic, and general *ceteris paribus* clauses, Mitchell suggests that we provide a taxonomy of the contingency that generalizations exhibit. I agree with her suggestion.

In fact, I provided a taxonomy of contingency in the previous chapter, where I argued that biological generalizations' weak and strong contingency should be analyzed in terms of scope and stability of generalizations. I suggest that we similarly analyze generalizations' *ceteris paribus* clauses or "domains," that is, we analyze them in terms of the scope and stability of the generalizations. I will continue the analysis of contingency of generalizations in chapter 5 by discussing the epistemic functions that stability and scope have in "generalizing" explanations.

3.6 Vertical and Horizontal Explanations of Exceptions

The basic idea behind **Gause's** rule is that species with similar niches, not to mention identical ones, cannot coexist in the same place for long periods of time. When faced with a situation that seems to be an exception to this rule, as happens with what appears to be a guild of birds of prey that hunt the same food, such as voles of the genus *Microtus*, an ecologist undertakes a closer investigation of the situation and the ecology of the species. As a consequence, it is not uncommon that competing species are found to be less similar in (auto)ecology than they first appeared (see, for instance, Lack 1945, 1946; Heatwole and Davis 1965; cf. the theory of **limiting** similarity). Thus, what appeared to be a genuine exception to Gause's rule is found to be an apparent exception, since the rule does not apply to cases in which there are important differences between the niches of competing species.

What makes an exception to a generalization apparent exception rather than genuine? Many philosophers (and scientists) maintain that if an exception to a generalization can be *explained* independently and in a non-*ad hoc* way, then the exception is apparent, not genuine (see, for instance, Pietroski & Rey 1995). A related and equally common idea is that the explanations for exceptions to generalizations come *exclusively or typically from the lower levels of mechanisms and/or are stated in the vocabulary of the lower-level sciences.*

This latter idea of the explanations of exceptions to generalizations is what is being called into question here.

Imagine a population of the species of the pocket gopher, *Thomomys bottae*, which is an anomaly with regard to Bergmann's rule (see appendix). Pocket gophers are rodents that dig up tunnel systems and spend considerable parts of their lives burrowing underground. The anomalous population lives in a mountainous area, where the soil is shallow and rocky. Suppose also that *lower altitude* populations of this species *at the same latitude* follow Bergmann's rule and thus are larger in body size.

We could try to discharge the problematic status of the anomalous population with the following evolutionary considerations (as in Davis 1938): at high altitudes, the critical factor for an organism's survival is typically the ambient temperature, especially during the cold season. Since the soil of the habitat of the anomalous population in question is shallow and rocky, its members cannot dig deep holes or build extensive tunnel systems in which to escape the harsh environmental conditions. Thus, there seems to be strong evolutionary pressure that selects for small(er) body size, for smaller-size organisms do not need such deep holes and extensive tunnel systems as their larger relatives in which to escape harsh environmental conditions. In addition, this pressure contradicts the selective advantages of Bergmann's rule (if there are any). In this way, an anomaly is rendered expected and understandable by considerations of a more fundamental, lower-level, or "basic" theory that accounts for the anomaly.

Jerry A. Fodor has been a strong advocate of the idea of finding more fundamental explanations of exceptions to generalizations; on several occasions he has stated something similar to the following (cf. Fodor 1974: 112, 1987: 4-6, 1989: 66-68, 75-7, 1991: 24-25):

> Exceptions to the generalizations of a special science [i.e., a non-physical science] are typically *inexplicable* from the point of view of (that is, in the vocabulary of) that science. That's one of the things that makes it a *special* science. But, of course, it may nevertheless be perfectly possible to explain the exceptions *in the vocabulary of some other science*. In the most

> familiar case, you go 'down' one or more levels and use the vocabulary of a more 'basic' science. (The current failed to run through the circuit because the terminals were oxidized; he no longer recognizes familiar objects because of a cerebral accident. And so forth.) The availability of this strategy is one of the things that the hierarchical arrangement of our sciences buys for us. Anyhow, to put the point succinctly, the same pattern that holds for the special sciences seems to hold for commonsense psychology as well. On the one hand, its ceteris paribus clauses are ineliminable from the point of view of its proprietary conceptual resources. But, on the other hand, we have – so far at least – no reason to doubt that they can be discharged in the vocabulary of some lower-level science (neurology, say, or biochemistry; at worst, physics.) (Fodor 1987: 6.)

Fodor is here generalizing an idea advanced by Donald Davidson (1970: 94-101), who claimed that there is a difference between generalizations that deal with physical phenomena and those dealing with mental phenomena. Generalizations that deal with the former are homonomic. Homonomic generalizations can be corrected and made more precise, and their exceptions can be explained within their own conceptual domain; in other words, they are distinctively physical and autonomous as generalizations. To homonomic generalizations Davidson contrasted heteronomic generalizations, which deal with mental phenomena.

Heteronomic generalizations cannot be corrected or made more precise, and their exceptions cannot be explained within their own conceptual domain; they need the vocabulary of other sciences and presumably those that reside at lower levels in order to have these things. Heteronomic generalizations are thus not autonomous, because they need the vocabulary of other sciences to better themselves:

> Where there are rough, but homonomic, laws, there are laws drawing on concepts from the same conceptual domain and upon which there is no improving in point of precision and comprehensiveness. We urged in the last section that such laws

> occur in the physical sciences. Physical theory promised to provide a comprehensive closed system guaranteed to yield a standardized, unique description of every physical event couched in a vocabulary amenable to law.
>
> It is not plausible that mental concepts alone can provide such a framework, simply because the mental does not, by our first principle, constitute a closed system. Too much happens to affect the mental that is not itself a systematic part of the mental. But if we combine this observation with the conclusion that no psychophysical statement is, or can be built into, a strict law, we have the Principle of the Anomalism of the Mental: there are no strict laws at all on the basis of which we can predict and explain mental phenomena. (Davidson 1970: 99.)

When the idea of the heteronomicity of mental phenomena is generalized to biological phenomena that moreover appear to be as "open" as mental phenomena (see section 2.4), which is the reason for the heteronomicity of the mental according to Davidson, and when this idea is connected with the observation that biological generalizations are riddled with exceptions (see section 2.3), one is easily led to the conclusion that explanations of exceptions should exclusively or typically be *vertical* or bottom-up in character in biology, as Fodor suggested in the passage cited above. Why? Simply and evidently because the default heuristics of many philosophers and biologists is reductionism (in the sense of Wimsatt 2006, 2007: 347-352).[45]

For instance, Michael E. Ruse (1970, 1973) set out to show that biology is a lawlike science. As one defense of his position, Ruse claimed that, although there are exceptions to biological generalizations, these exceptions are not damaging to biological generalizations' lawlike status, because they can be explained away by

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[45] Although I have nothing against reductionism as heuristics as long as its biases are remembered (cf. Wimsatt *ibid.*), the issue of why reductionism should be adapted to the context of the explanations of exceptions to generalizations has remained unaddressed, as I argue below.

making reference to lower-level mechanisms that implement biological generalizations. Ruse's examples (1970: 243-245, 1973: 13-14, 24-31) involve **Mendel's** rules (see appendix).

It is known that Mendel's rules are not without "negative instances." However, as later development in cytological and molecular biology has shown, negative instances of Mendel's rules are cases, in which the mechanisms of these rules do not apply or there are mechanistic stories at lower levels that account for the exceptions. I am referring to linkage, which is an apparent exception to Mendel's second rule. In linkage, different genes are located *on the same* chromosome, whereas Mendel's second rule applies only to genes at the same locus, i.e., to genes that are on *different* homologous chromosomes. Exceptions to Mendel's first rule – meiotic drive and non-disjunction (see Sandler & Novitski 1957; Crow 1979; Beatty 1981: 407-409, 1982: 210-213; Darden 1995: 143-145) – appear to be more genuine exceptions to Mendel's first rule than linkage, yet suppose for the sake of argument that there are lower-level explanations that show how these exceptions came about and that discharge their problematic status as genuine exceptions to the rule. Ruse thus evidently makes the claim that we have apparent, not genuine, exceptions to biological generalizations. He is therefore suggesting that biological generalizations are riddled with apparent exceptions (rather than genuine ones) and that, contrary to their appearances, biological generalizations are consequently exceptionless and perhaps even universal.

Recently, Daniel P. Steel (2008: 125-148) discussed of a similar "corrective asymmetry" between different levels in the sciences, according to which lower levels of mechanism account for exceptions and even correct for generalizations found at higher levels.

I object to the idea that corrections to generalizations in the special sciences should always be or typically be vertical in character. At least in community and population ecology, scientists explain away exceptions to their generalizations by using other concepts, causes, and mechanisms on the same level rather than those on lower-levels.

Let us call such explanations *horizontal* explanations of exceptions to generalizations in contrast to vertical explanations.[46]

That ecologists explain exceptions to their generalizations horizontally suggests that the idea of the heteronomicity of biological generalizations – which some think of as proving an argument against existing distinctively biological lawlike generalizations (see section 2.4) – can be questioned. It likewise suggests that the idea of vertical explanations of exceptions is not a necessary *and* sufficient condition for explanations of exceptions in the special sciences. The idea of vertical explanations of exceptions is also a central premise of many *ceteris paribus* accounts of laws (see section 3.5).

I wrote above as if it were the "(auto)ecology" of the species that account for the exceptions to Gause's rule in the case of birds of prey. If an explanation of an exception to Gause's rule were given in terms of the autoecology of these species, this could perhaps be interpreted as an instance of a vertical explanation of an exception to a synecological generalization (i.e., Gause's rule), in which the autoecology level can perhaps be viewed as situated at a lower level (of mechanisms) than the synecological level. But there are other, synecological explanations of exceptions to Gause's rule. The explanation I have in mind is the below "same level" explanation of the failure of Gause's rule, originally due to Robert T. Paine:

In (rocky) intertidal marine habitats the top predators (e.g., starfish and gastropod species of genera *Thais* and *Pisaster*) prey on different consumer species (the different mussels, barnacles, and other species of many genera). In such habitats, there is severe and intense competition among the *consumer species* for living space, which is the major and critical limiting factor. However, there is surprisingly high alpha or local diversity of the consumer species in many such habitats. It is surprisingly high, because, on the basis of Gause's rule, one would expect only a few species, given the keen competition for living space (cf. also Liebig's rule). What Robert T. Paine (1966)

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[46] I have adapted the terms vertical and horizontal explanations of exceptions from the excellent paper by Wim J. van der Steen and Harmke Kamminga (1991: 449-453). The authors call these *integrations* instead of *explanations*, but their idea is similar to mine.

experimentally discovered was that removing one (or more) of the top predator species (e.g., *Pisaster*) from such a habitat had the effect of *reducing* the alpha diversity of the consumer species: without a common predator species, a few (and perhaps even one) competitively dominant consumer species come to monopolize the living space by outcompeting other consumer species. Thus, Paine's explanation was that (the top or key-stone) predators can mediate the coexistence of their prey and maintain the local diversity of a community by keeping competing prey populations' densities or abundances below some level at which the competition would become so severe as to cause local extinctions. The removal of a predator not only reduced the alpha diversity of these species, but also the whole trophic structure of the manipulated habitat collapsed and was reduced to a simpler one. Other experiments have reported similar results (see Slobodkin 1964, Paine & Vadas 1969, and Durán & Castilla 1989).

These and similar experimental findings and explanations of exceptions to Gause's rule by Paine and others were later generalized and labeled the intermediate disturbance rule. According to this rule, intermediate levels of abiotic or biotic disturbances, such as predation, pathogens, aridity, storms, and fires are capable of mediating the coexistence of competitor species and thus maintaining the local diversity of a community at a relatively high level. Too small or infrequent disturbances lead to local extinctions of competitively inferior species by competitively dominant ones, whereas too intense or too frequent disturbances allow for the few species that are the most stress-tolerant to exclude other species from a habitat. In other words, local species diversity often peaks with the intermediate levels of disturbances of a habitat. Biotic and abiotic disturbances have the same effect, for instance, through reduction of population densities of competitor species, which counter the strong competitive effects between species.

The point of the above example is to suggest that ecologists explain away exceptions to generalizations by using ecological concepts, causes, or mechanisms on the same level rather than those on a lower level. I will give some other (same level) explanations of exceptions to Gause's rule in chapter 6. There are thus "horizontal" explanations of exceptions in ecology that makes ecology look homonomic in generalizations in contrast to what many previous authors have suggested about generalizations of the special sciences. Note that the above argument concerned *explanations* of exceptions, not their implementation or realization. Moreover, my claim is only that corrective asymmetry (or the idea of vertical explanations of exceptions to generalizations) is neither a sufficient and necessary condition nor a necessary condition for the explanations of exceptions or the corrections to generalizations.

The question of why corrective asymmetry should be adapted into the context of explanations of exceptions to generalizations will be examined some other time. This is because there do not appear to be many explicit arguments for its use in the literature. For instance, Daniel P. Steel (2008: 125-148) presents many insightful and intriguing ideas concerning both reduction and corrective asymmetry in biology. Nevertheless, Steel does not elaborate on his reasons for corrective asymmetry over and above the idea that corrective asymmetry follows from his general mechanistic approach to scientific explanation. Yet reference to this general explanatory strategy in itself does not answer or justify the question of why corrective asymmetry should be adapted in the first place in the context of explanations of exceptions to generalizations. At the same time, to maintain that the lower-level sciences are more trust worthy, reliable, and so on in their generalizations or explanations than are the higher-level ones does not constitute a valid reason for corrective asymmetry either, because this only beg the question. It suffices to point out that some reasons for corrective asymmetry in the context of explanations of exceptions evidently have to do with the idea that the lower-level sciences are more comprehensive, or less complex, or less open than the higher-level ones. The problem is that these reasons can be easily contested as I argued in chapter 2.

3.7 Conclusions

I have argued that accounts of laws that tolerate exceptions, as suggested by Sandra Mitchell, Marc Lange, and many others, are insufficient to redeem the lawlike status of biological generalizations. I have also suggested that ecological generalizations can be corrected and made more precise, and their exceptions can be explained within their own conceptual domain; in other words, these generalizations are *distinctively* ecological and autonomous insofar as their corrections and explanations of exceptions are concerned.

4

INVARIANCE

4.1 Introduction

As discussed in chapter 1, the covering law account of scientific explanation suggested a practically relevant, historically influential, and strong justification for the ecological laws debate, namely, the idea that laws are indispensable or necessary for scientific explanations. There were also serious problems with this account. For one thing, it failed to discriminate between explanatory relevant and irrelevant information. In light of this and other problems, it could be argued that reference to generalizations' lawlikeness is not a *sufficient* condition for scientific explanations. The problems of the covering law account have been known for decades, but it has been retained owing to the lack of an adequate alternative.

Another problem concerns the applicability of the covering law account to ecology. How are scientific explanations possible in ecology, given that there do not appear to be many – if any – ecological laws, as I argued in chapter 2? To answer this question, I will present and defend here a non-covering law account of scientific explanation – originally suggested by James Woodward – in which generalizations are explanatory if they are invariant rather than lawlike. I call this the interventionist account of causal scientific explanation.[47] It shows that reference to lawlikeness is not even a

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[47] The account is also called the manipulationist account, although interventionist account is a better name for it. The term manipulation seems to be associated with the idea that there is an agent carrying out the intervention. However, any process that fulfills the criteria discussed in section 4.2 counts as an intervention, regardless of whether it is based on the agency or the activities of humans. For instance, there are natural

necessary condition for scientific causal explanations. Thus, I argue that there is an adequate alternative to the covering law account, which I will defend with ecology in mind. This chapter also shows why I think the debate about ecological laws is a red herring.

I proceed as follows: I first present an interventionist account of scientific explanation according to which it is invariance rather than lawlikeness that furnishes generalizations with their explanatory power. I then apply the account to ecological generalizations to show that invariance under interventions as a criterion of explanatory relevance yields interesting interpretations of the explanatory status of many ecological generalizations, such as allometries, scaling laws, and the species–area rule. Ecologists should find this section interesting, because some of them rely on (unreliable and implicit) intuitions, when deciding by what criteria the explanatory status of ecological generalizations should be evaluated. In section 4.4, I discuss the lawlike status of the so-called allometries and scaling laws. In section 4.5, I distinguish between two kinds of causal explanations that I call simple causal claims and mechanistic explanations. I also review some recent definitions of mechanisms and argue that (representations of) mechanisms should be defined as invariant and modular. Section 4.6 concludes.

4.2 The Interventionist Account of Scientific Explanation

The main claim of this section is that invariance is the correct relation of explanatory relevance in the case of causal explanations, as has been argued by James Woodward (2000, 2001, 2003a, 2003b; see also Woodward & Hitchcock 2003; Hitchcock & Woodward 2003) whose

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experiments in which the interventions are those of nature, not of experimenters. In other words, "intervention" is not only broader in scope as a term than "manipulation," but also it is a more accurate term. Finally, with the name interventionist, I want to distance Woodward's non-reductive account of causal explanation from reductive accounts of causation that are called manipulationist accounts.

ideas I present in this section.[48] In the interventionist account, causes are difference-makers. One should understand causes and effects as (representable as) variables. Many vernacular causal locutions can be understood to be about binary-valued variables. Causes are difference-makers in that they can be intervened upon to manipulate or control their effects. A change in the value of a cause makes a difference in the value of its effect. This formulation extends to indeterministic contexts, where causes make a difference in the probability distribution of effects, such as when a drug makes a difference in the probability of the recovery of a patient.

What gets here defended is a realist account of explanation as well. Although explanations can be reconstructed as arguments, explanations are not explanatory as a result of their argumentative structure as they are, for instance, in Carl G. Hempel's (1965) covering law account. Rather, explanations are descriptions of objective dependency relations between things. And the real and independently existing dependency relations of the world determine whether an explanation is true or not. Explanations are explanatory and true if explanations describe correctly dependency relations.

An invariant generalization is one that continues to hold under a special change – called an *intervention* – that changes the value of its variables. For variable X to be explanatory with regard to variable Y, an invariant relation between the two is required in which interventions of the value of variable X change the value (or the probability distribution) of variable Y in accordance with the relation between the two variables. An invariant generalization describes what would happen to a value of a variable of a generalization if a value of one or more of its other variables were changed by an intervention. For instance, 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. The ideal gas law describes how changes in the pressure, volume, and temperature of gases depend on one another and how an intervention

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[48] The account has been developed, among others, by Petri Ylikoski (2001, 2007), Carl F. Craver (2007), and Ylikoski and Jaakko Kuorikoski (2010).

in one of the variables brings out changes in the others. In other words, the ideal gas law describes an invariant generalization.

An intervention is a manipulation affecting the value of *Y by changing the value of X*. It should not affect *Y* via a route failing to go through *X*. Nor should the intervention be correlated with the other causes of *Y*, except for those intermediate causes of *Y* – if there are such – that are between *X* and *Y*. As long as a process has the above properties, it is an intervention, regardless of whether it is based on the agency or activities of humans (for a more detailed account of interventions, see Woodward 2000: 199-204, 2003a: 94-111).

The interventionist account is a counterfactual account of scientific explanation that shows what would happen to variables of generalizations during interventions: how the value (or probability distribution) of *Y* would change during intervention in the value of variable *X* (see especially Woodward 2000: 205-209, 235-239, 2003a: 187-238). What is needed for explanations are *active* counterfactuals in the form, "if the value of the variable *X* of a generalization $Y_i = f(X_i)$ were changed by an intervention from x_1 to x_2, then the value of the variable *Y* would be changed from y_1 to y_2 in accordance with the relation $Y_i = f(X_i)$." The "stability" of a generalization under interventions in its variables is what matters in explanations. The ability to remain true under active – rather than passive – counterfactuals[49] distinguishes explanatory generalizations from non-explanatory ones:

> [T]he underlying idea of my account of causal explanation: we are in a position to explain when we have information that is relevant to manipulating, controlling, or changing nature, in an "in principle" manner of manipulation…. We have at least the beginnings of an explanation when we have identified factors or conditions such that manipulations or changes in those factors or conditions will produce changes in the outcome

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[49] Passive counterfactuals have the form "had this-or-that background condition been different, a generalization would still have held" (see chapter 5). I have adopted the terminology of active and passive counterfactuals from Woodward (2000: 235-239, 2001: 9-10, 2003a: 279-285) and Woodward and Hitchcock (2003: 19-21).

> being explained. Descriptive knowledge, by contrast, is knowledge that, although it may provide a basis for prediction, classification, or more or less unified representation or systematization, does not provide information potentially relevant to manipulation.(Woodward 2003a: 9-10.)

Explanations describe and exhibit how the phenomena described in *explananda* systematically depend upon the changes of *explanantia*. Invariant generalizations with their support of active counterfactuals provide answers to ”what-if-things-had-been-different” questions that furnish us with understanding and explanatory information about phenomena. Answers to these questions provide us with information about how the *explanandum* would have changed had we changed the initial or boundary conditions of an invariant generalization to “such-and-such” (see Woodward 2000: 209-214, 2003a: 187-194, 196-203).

In order for there to be an intervention and a possibility of a manipulation, at least some of the predicate terms of a generalization are required to be (representable as) variables (see Woodward 2000: 206-209, 2003a: 111-114). If there is no well-defined notion or idea of what it would mean to change a value(s) of a predicate term(s) of a generalization, or what it means to represent its predicate terms as variables, then the generalization is not invariant. Let us call such generalizations *non-change-relating*. The idea is that non-change-relating generalizations do not describe “dynamic” relations. Generalizations expressing static, qualitative, or categorical relations can often be viewed as non-change-relating.

Consider a generalization that is sometimes presented as a physical law, owing to its high degree of stability or constancy: “all noble gases are chemically inert.” Although this generalization is stable in the sense that it holds in many different background conditions – and supports passive counterfactuals because of its stability –, it is not invariant, because it does not allow for a (well-defined) change in its predicate terms. In fact, the generalization denies any changes in the properties of noble gases that could be used in the manipulation of their properties. A *necessary* condition for a generalization to count as explanatory and invariant is that it expresses a change-relating generalization (see Woodward 2003a:

245-254). [50] Although invariant generalizations are change-relating, not all change-relating generalizations are invariant.

Invariance is *a degree concept with a threshold* (see Woodward 2000: 214-222, 2003a: 257-265). There are generalizations that are not at all invariant, such as the above example of noble gases. Likewise, there are change-relating generalizations, such as correlations between factors that are joint effects of a common cause (i.e., confounding factors or cases of spurious causation), which are not invariant. A non-invariant, but change-relating relation between joint effects of a common cause breaks down during interventions in its effects. An example is the correlation between readings on a barometer and the occurrence of storms that correlate owing to their common cause, namely, the changes in atmospheric pressure: changing the value of one effect variable (a reading on a barometer) does not make any difference to the value of another effect variable (the occurrence of storms), although the correlation between the two can be very stable in the sense that it holds in many different background conditions. Only interventions in the value of a common cause make a difference to the values of the joint effects correlating.

Above the threshold of invariance there are more or less invariant generalizations. Woodward calls the *invariance domain* the set or range of changes over which a generalization is invariant – the range over which it supports active counterfactuals (see also Woodward 2000: 205-209 and 2003a: 239-314). This range need not be universal in the sense that during all the interventions on its variables, a generalization holds. Typically, generalizations fail to be invariant under extreme values of their variables and/or under some background conditions. The ideal gas law, for instance, breaks down under extreme pressure.

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[50] Taxonomy is riddled with examples of non-change-relating generalizations, such as "all crows are black," "all robins' eggs are greenish-blue," and "many species of the genus *Amanita* are (deadly) poisonous." The examples just mentioned are perfectly valid as classificatory and descriptive generalizations. In other words, "non-change-relating" is not a pejorative label for generalizations, but can refer to legitimate, non-explanatory, but classificatory/descriptive generalizations.

There is a connection between invariance domain and explanatory depth (see Hitchcock & Woodward 2003). The ideal gas law and van der Waal's force law both describe how changes in the pressure, volume, and temperature of gases depend on one another and how an intervention in one of the variables brings out changes in the others. However, van der Waal's force law has a larger invariance domain than the ideal gas law, since it holds in conditions in which the ideal gas law breaks down, such as under extreme pressure. Consequently, it could be suggested that van der Waal's force law provides deeper explanations about the behavior of gases than do the ideal gas laws, because van der Waal's force law answers more of the "what-if-things-had-been-different" questions about the behavior of gases, owing to its larger invariance domain.

As the above suggests, sometimes the invariance domain of a generalization is a proper subset of the invariance domain of another generalization. In such cases it is straightforward to claim that the latter generalization provides deeper explanations of their common *explanandum* than the former, because the latter answers more of the "what-if-things-had-been-different" questions about that particular *explanandum*. Nevertheless, such cases are possibly the exception rather than the rule; it is likely that invariance domains of many *explanantia* only intersect, which makes it more difficult to estimate which of the *explanantia* provides deeper explanations of their *explanandum* than other(s). Moreover, it is both the *nature* and the *range* of interventions that determine how shallow or deep are the explanations of generalizations offered (see Woodward 2000: 220-222, 2003a: 257-265). This is because certain interventions are considered privileged or more important than others in different sciences or disciplines in the sense that generalizations should remain invariant. Let me therefore suggest that one generalization provides deeper explanations than another of their common *explanandum* if it remains invariant under a wider and/or more important set or range of interventions. However, whether a generalization remains invariant during interventions is an objective fact that is unaffected by the considerations of importance we attach to interventions.[51]

Invariance is different from generalizations' lawlikeness (see Woodward 2000: 222-228, 2003a: 265-272). There is no requirement that invariant generalizations should contain only purely qualitative predicate terms, be universal, maximally or highly stable, or belong to a systematic web of other generalizations, as many philosophers have suggested about laws. A generalization can be invariant and explanatory *regardless* of its lawlike status.

Invariance is also different from stability (see chapter 5; see also Woodward 2001: 13-17, 2003a: 295-302, 2006b, 2010). There are stable generalizations that are non-invariant. For instance, many examples of change-relating correlations between joint effects of a common cause can be stable in the sense that they hold in many different background conditions. Nevertheless, they are non-invariant as generalizations. Stability gives us the support of passive counterfactuals, whereas invariance gives us the support of active ones. The function of invariance has to do with providing explanations, manipulations, and control, whereas stability provides reliability and extrapolatibility (as I argue in section 5.6).

The interventionist account has many advantageous features. It resolves the problems of explanatory irrelevance and asymmetry, which have plagued previous accounts of scientific explanation (see section 1.7). For instance, as an intervention, the hexing of a lump of table salt does not make any difference to its dissolving in water; that is, hexing is irrelevant for explaining why salt dissolves in water. It is the manipulation of the height of the flagpole that makes a difference in the length of the pole's shadow, not vice versa. The interventionist account allows us to speak of absences as omissions as causes and preventions as effects in contrast to some other accounts of scientific explanations (see Woodward 2003a: 86-91, 224-226), which is fortunate since such "negative facts" are typically treated as causes and effects in the biological sciences. Besides being applicable to type

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[51] For discussion of other dimensions of explanatory depth between different explanations of an *explanandum*, such as precision and factual accuracy, see Petri Ylikoski and Jaakko Kuorikoski (2010).

causal claims the account applies to token or singular causal claims, and evidently delivers the right judgments of the traditional problems of causal over-determination and pre-emption in this context (see Woodward 2003a: 74-86).

The account allows for explanations of phenomena that have low probabilities of occurrence. Untreated syphilis is the cause of and can be used to explain the development of paresis, although only a small percentage of syphilitics develop paresis. This is because the incidence of paresis is higher among syphilitics who have not been treated than among those who have been. For a variable to count as an indeterministic cause of an effect, it is not required that it raise (or lower) the probability of the occurrence of its effect in every background condition, only that the variable should do this under some of the interventions in some background conditions (see Woodward 2003a: 147-149). This last feature is especially fortunate, since many ecological generalizations seem to lack stable probabilities (see section 3.4), and many ecological causes evidently are not unanimous in their effects.

The interventionist account thus gives normatively right answers to many issues about explanations. This much cannot be said about any other account of scientific explanation.

4.3 Ecological Generalizations

Invariance under interventions as a criterion of explanatory relevance gives interesting interpretations of the explanatory status of many ecological generalizations,

According to the interventionist account, many (macro-)ecological generalizations are *not* explanatory, because they do not describe invariant relations. Putative examples include the *intraspecific* and *interspecific* patterns of **abundance** and distribution, the **canonical** distribution of abundances, and the **hollow** curve (see appendix), which represents phenomena, “patterns,” or *explananda* rather than *explanantia.* This is because the above generalizations are either non-change-relating or change-relating, but not invariant during interventions. This is despite the fact that many of the above generalizations could be stable generalizations, owing to their validity or holding in many different background conditions.

Why do I claim that these generalizations are not explanatory? Well, it appears to be difficult to determine exactly what variables should be manipulated in these generalizations and exactly what kinds of effects the manipulation of variables should have on other variables mentioned in these generalizations. This suggests that the generalizations are non-change-relating. However, even if there were a well-defined notion of what is to be manipulated and of the results of such manipulation, I suppose that many of the above generalizations would break down during interventions. Thus, even if these generalizations represented stable and change-relating generalizations, they are likely to be only joint effects correlating, owing to their common causes, rather than invariant generalizations.

The interventionist account gives experimentation and manipulation a central place in establishing and testing explanatory generalizations. The interventionist account seems to accord well with the intuitions and practices of many ecologists concerning explanation and experimentation (cf. Hairston 1989). Nevertheless, some biologists use non-manipulative and non-experimental methods, such as regression equations and other correlations, to study phenomena. These are widely used in ecology and paleobiology, for instance, under the rubric of "allometries and scaling laws."

In allometries and scaling laws body size or weight is treated as an independent variable of anatomical, physiological, morphological, behavioral, life historical, ecological, or paleobiological traits of taxa (see Newell 1949; Rensch 1960; Gould 1966; Clutton-Brock & Harvey 1983; Peters 1983; McKinney 1990; Brown 1995: especially 76-101; Marquet 2000; Marquet *et al.* 2005).

Allometries and scaling laws are regression equations represented as power equations in which one variable changes as a power of another. These take the form

$$Y = aW^b,$$

where Y is the dependent variable (or response), W represents the independent (or "explanatory") variable, a is the normalization constant, and b is the scaling exponent.[52]

Depending on the value of their scaling exponents, allometries and scaling laws are called either allometric ($b \neq 1$) or isometric ($b = 1$). Scaling exponents can take both negative and positive values. In general, the larger the value of b, the faster Y increases (if b is positive in value) or decreases (if b is negative in value) with increasing W. If the scaling exponent, b, is less than unity, then Y increases (or decreases if negative in value) more slowly than W. After logarithmic transformation the above equations become linear and yield straight lines on double log axes, where a gives the intercept or elevation of the regression line and b, its slope (see Fig. 1).

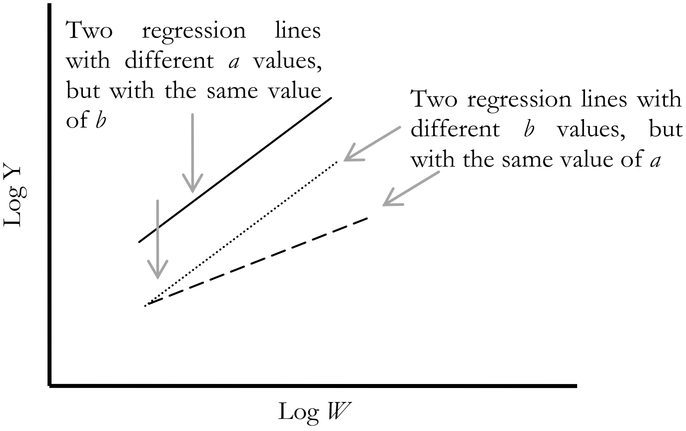

Fig. 1. Illustration of the effects of different values of constant *a* and *b* on regression lines. The constant *a* gives the elevation of the regression line, and *b* gives its slope.

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[52] I have omitted error terms that represent variation in the dependent variable, owing to other possible independent variables and measurement errors in the independent variable.

There is a plenitude of biological traits that correlate with body size, W, and can be represented as dependent variables, Y. Robert Henry Peters (1983) has provided hundreds of examples of allometries and scaling laws. Some of the more common are the following:

1) Energy use is unaffected by body size: according to the **energy equivalence** rule, the energy use of all species (populations) in each size class tends to be equal, scales as aW^{0}
2) **Fasting** endurance scales as $aW^{0.44}$ for mammals and between $aW^{0.40}$ and $aW^{0.60}$ in birds
3) The size of the **home** range of birds and mammals varies positively with body size, aW^{1}
4) The **inverse** scaling rule: the maximum density, D, of herbivorous mammals declines as their body size increases, $D = aW^{-0.75}$
5) The **thinning** rule for plants, where D is density, is $W = aD^{-1.33}$
6) The body size **frequency** distribution of a taxon: species number declines with body size
7) **Kleiber's** rule: basal metabolism, an estimate of the energy required by an individual for the basic processes of living, varies as $aW^{0.75}$
8) An Individual's total **energy consumption** varies as $aW^{0.75}$
9) In most mammal groups gut volume is isometric to W, aW^{1}
10) Blood volume varies as aW^{1}
11) Blood circulation time varies as $aW^{0.25}$
12) Heart rate varies as $aW^{-0.25}$
13) Respiratory rate varies as $aW^{-0.25}$
14) Mammal brain weight (excluding primates) varies as $aW^{0.70}$ ($a =$ 0.01)
15) Monkey brain weight varies as $aW^{0.66}$ ($a = 0.03$)
16) Brain weight in man varies as $aW^{0.66}$ ($a = 0.09$)
17) In mammals within species sociality increases with body size
18) Group behavior increases with body size in mammals

There are some interesting general patterns in these regressions (see Gould 1966 and Peters 1983). For instance, there is a constant scaling exponent, b (approximately 0.75) for regressions describing different kinds of biological *rates* that scale with W. Moreover, many biological *frequencies* scale as $aW^{-0.25}$, biological *times* as $aW^{0.25}$, and biological *volumes* as aW^{1}. The phenomenon that there are these constant scaling exponents is sometimes called the "similitude principle."

Below are some general reasons to be suspicious of explanatory and causal relevance of *some* allometries and scaling laws. There are other problems with these regression equations that I will not take up here. For instance, I do not discuss the statistical problems and the problems of fitting data to regressions that are relevant in interpreting allometries and scaling laws and that affect their reliability (see Gould 1966, Peters 1983: 10-21, McKinney 1990). Nor do I discuss the adaptive significance of allometries and scaling laws (see Rensch 1960, Bonner 1968, and Clutton-Brock & Harvey 1983).

Even though we know that correlation is not intimately or necessarily connected to causation, in practical terms this dictum is sometimes forgotten in the allometry and scaling law literature. In this literature, body size as an independent variable is sometimes claimed to explain (a major) part of the variation in the dependent variable. How much it explains is dependent on the indices of fit, e.g., on the value of r^2. According to the interventionst account defended, however, correlations by themselves are not explanatory, regardless of how strong and/or stable the connection between the correlating factors is. For a correlation to be explanatory there has to be an intervention during which the relation between factors remains invariant.

In many cases "body size" is used as an independent variable, owing to convenience. Body size is relatively easy to quantify, compare, and estimate from fossil parts and other field samples. As a result of its convenient features, body size is often not intended to be a cause variable, but a proxy for or a correlate of other features that are not so easy to quantify, compare, or estimate and which represent the real causes. This becomes a problem if body size is subsequently interpreted to be the cause variable. It is problematic because using body size in this manner is not only causally inaccurate and false, but also misleading insofar as we are searching for ways to intervene in nature. Another problem arises when the use of body size as a proxy hides the fact that the real causes of allometries and scaling laws are not well defined as variables (see below).

Sometimes it is not easy to understand how some allometries and scaling laws represent change-relating generalizations. Sometimes the *dependent* variable in allometries and scaling laws is such that it is difficult to understand what it means to change its value and/or what its different values would be. In other words, the problem is how dependent variables are to be represented as variables that have well-defined values. Consider, for instance, the two regressions presented above that relate body size to "within species sociality" and "group behavior." These generalizations are possibly non-change-relating, because what is meant by "within species sociality" or "group behavior" *as variables* appears not be well defined. If this is true, then it follows that it is unclear how changes in the value of an independent variable affect the dependent variables, simply owing to the fact that the dependent variables are not well defined.

What has been said about dependent variables applies to *independent* variables as well. As was just discussed, body size is often used as a proxy for the real causes of dependent variables of allometries and scaling laws that are not so easy to quantify, estimate, and so on. In fact, precisely because the real causes are not well-defined as variables, body size is apparently sometimes used as a *proxy*. Such use is inappropriate, since it hides the fact that the allometries and scaling laws in question are non-change relating as generalizations.

If a generalization cannot be tested for how it might behave during interventions in or manipulations with its variables, then the claims made about its explanatory status should be treated with suspicion. The unfortunate fact in this context is that testing whether ecological and paleobiological regression equations represent invariant generalizations is often difficult purely for scale-related reasons (cf. large ecosystems studies and paleobiological allometries and scaling laws) and owing to many ethical, technical, and other reasons.

When the problems above can be avoided, it is quite likely that many allometries and scaling laws turn out to be change-relating but *non*-invariant during interventions. Thus, even though many allometries and scaling laws represent change-relating generalizations, it is quite possible that they might be joint effects correlating, owing to their common causes – that is, accidents that hold because of their common causes. Even if we find that some of these generalizations are change-relating and invariant, allometries and scaling laws seem to offer rather shallow and superficial explanations – or "simple causal claims" – that need to be supplemented with information about the mechanisms that underlie them (see section 4.5).

In principle, I have nothing against the idea that regression equations may be causal or explanatory. Again, whether these relations remain invariant during their interventions is the objective criterion that determines whether they are explanatory or not. It is the naïve use of regressions encountered in the literature that I object to here.[53]

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[53] Another role of allometries and scaling laws is using body size as a predictive tool. It is trivial that "mere correlations" (or change-relating, but non-invariant generalizations) can be used to make successful predictions. In other words, a causal or explanatory interpretation of allometries and scaling laws is not a necessary condition for their functioning in making predictions. I have nothing against the idea that allometries and scaling laws function in making predictions. At the same time, this role should be adopted with some important qualifications. Body size as an independent variable typically "explains" only part of the variation in the dependent variable of allometries and scaling laws. The presence of this residual variation around the regression lines shows that, for successful and accurate predictions, other independent variables are needed in addition to body size. Moreover, the predictive power of regression equations can be more a function of their statistical fitting than the fact that the equations describe data or phenomena correctly. In other words, it is often not true that allometries and scaling laws can be used to make successful or accurate predictions. Furthermore, although mere correlations can be used to make predictions, it is not true that all the predictions are equally good, reliable, or illuminating. Mere correlations represent accidents that hold, owing to common causes and certain background conditions. Predictions based on invariant

There is, nevertheless, another important function that the regression studies mentioned above have. Many allometries and scaling laws should be understood as *elucidating phenomena from data* – they reveal, describe, and classify phenomena-to-be-explained rather than things that do the explaining. This function is not to be overlooked. For instance, if the energy equivalency rule is generally true (i.e., it has a large scope and/or is very stable), then it is interesting as an object of explanation that would have other interesting implications. As another example, homeotherms, poikilotherms, and unicellular organisms have different *a* values in equations that relate their metabolic rates to body size. In the equation for a basal metabolic rate (Kleiber's rule), which scales with body size as $aW^{0.75}$ in these taxa, the values of *a* are 4.1, 0.14, and 0.018 for homeotherms, poikilotherms, and unicellular organisms (Peters 1983: 29-30). Rather than indicating explanatory generalizations, these generalizations represent interesting objects of explanation. Why is it that unicellular organisms have the lowest values of *a* in such equations? How and why do homeotherms metabolize at a higher level (and thus seem to utilize and exhaust relatively more resources) than poikilotherms and unicellular organisms of similar size?

Does the claim that some allometries and scaling laws represent only phenomena-to-be-explained (or *explananda*) rather than things that do the explaining (or *explanantia*) cast a shadow over allometry and scaling law studies? No. A science or discipline without interesting *explananda* lacks the potential to progress and mature. In this respect these studies show a great potential.

It is easy to criticize the ideas presented above by suggesting that there is a "mathematical sense" of explanation according to

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generalizations are more than accidents, and invariant generalizations often lead to more reliable and illuminating predictions. In other words, mere correlations seem to provide the bases for fragile and unreliable predictions. Consequently, it is at least *desirable* to have predictions based on invariant generalizations. This shows that the issue concerning the explanatory status of allometries and scaling laws is relevant in the context of making predictions.

which allometries and scaling laws are explanatory, regardless of their explanatory status in the context of causal explanations. One problem with this suggestion is that it is difficult to explicate what this mathematical sense of explanation consists of or to develop this sense into a full-blown account of mathematical explanation (see, for instance, Sandborg 1998 and Mancosu 2008). There is one account of scientific explanation that perhaps could used, namely, the unification account by Philip Kitcher (1989). But Kitcher's account has faced serious difficulties in the context of non-mathematical explanations (see section 1.8), which suggests that it is defective as a general account of scientific explanation. Nor has the unification account been properly applied to mathematics to determine how well it accounts for the mathematical sense of explanation. In other words, the "mathematical sense" of explanation rests on undeveloped ideas and (unreliable) intuitions. Furthermore, the idea of a mathematical sense of explanations is evidently not first and foremost to show how regression equations could be explanatory "in the non-causal sense of the word," but rather to explain how and under which conditions such mathematical activities could be explanatory as providing proofs and solving equations. Finally, allometries and scaling laws should be dealt with in the context of *non*-mathematical explanations, since the authors who discuss them interpret allometries and scaling laws as providing non-mathematical explanations of phenomena; in other words, they are often interpreted to be about the causal relations between body size and the dependent variables mentioned in allometries and scaling laws. I have nothing against the idea that there should be mathematical explanations that differ from causal explanations. Another dissertation would be needed to explore the legitimacy of this account fully.

As another example, consider the **species–area** rule. According to this rule, the number of species varies with the area of an island or habitat, where this relation can be presented as a power equation, $S=cA^z$, in which S is the number of species of a given taxonomic group, A is the area of the island or habitat, and c and z are (fitted) constants. There appears to be an invariant relation between the variables area and species diversity (see Simberloff 1976b) despite the fact that the mechanism(s) behind the rule are disputed (see Simberloff 1974, 1976b; Diamond 1975; Connor & McCoy 1979; Gilbert 1980; Williamson 1989a; Lawton 1996). According to a rule of thumb, the manipulation of an area of an island or habitat that ten-fold it, doubles the species diversity. The explanatory status of the species–area rule does not depend on there being a generalization that is universally true that holds in many or most background conditions, and that has no exceptions (see Cook 1974, Gilbert 1980). In other words, it does not depend on the putative lawlike status of this rule as some authors believe (see Simberloff 1974; Lawton 1996, 1999; Lange 2005). Rather it depends on whether the rule is invariant during its interventions or not.

Some other putative examples of invariant ecological generalizations include the **area** rule of the equilibrium theory of **island biogeography**, the **distance** rule of the equilibrium theory of island biogeography, the **diversity–stability** rule, and the **endemicity** rule of the equilibrium theory of island biogeography. 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(s) of an island is increased (or decreased), its immigration rate is reduced (or increased). According to the **diversity–stability** rule, increased (alpha, beta, and/or gamma) diversity enhances (population, community, and/or ecosystem) stability.

4.4 Allometries and Scaling Laws Interpreted as Laws

Some biologists and philosophers think of allometries and scaling laws as representing biological laws because of their apparent generality and non-contingency (see Rensch 1960, Peters 1983, Marquet 2000, Marquet *et al.* 2005, Elgin 2006). This is a wrong interpretation as I demonstrate below.

Against the prevailing opinion among many scientists and philosophers that there are no *universal, exceptionless, strict, empirical, or* *distinctively biological* laws, Mehmet Elgin (2006) suggests that certain allometries and scaling laws could well be examples of such laws. For Elgin, allometries and scaling laws are *biophysical* laws: generalizations or statements of these that, in their antecedents, describe physical and chemical conditions, whereas their consequents describe biological properties that hold because the conditions mentioned in their antecedents hold (and are selected for):

> The form of such laws is as follows: given certain physical constraint *p* and under certain specifiable conditions *c*, all organisms exhibit behavior *b* where *p* is a physical property*, c* may be physical, chemical or biological property and *b* is a biological property. (Elgin 2006: 130.)

These biophysical laws of Elgin are allometries and scaling laws to which the above kinds of antecedents are added, and which, according to Elgin, yield biological lawlike generalizations. The idea seems to be that, say, **Kleiber's** rule or the **inverse** scaling rule holds because certain physical or chemical constraints or conditions, *p*, hold.

Elgin argues forcefully for the *biological distinctiveness* of these laws. This is understandable, since the problem in his position is that his examples of laws appear non-biological, owing to their non-biological antecedents. Thus, Elgin could not have shown that there are *biological* laws. I am not going to discuss this aspect of Elgin's project. Let us grant for the sake of the argument that his biophysical laws are distinctively biological. In lieu of arguing for their biological distinctiveness, Elgin should have argued for their *strictness, universality, lack of exceptions*, and *lawlikeness*, as I will show next.

First I will take up the strictness of Elgin's laws.

Elgin advocates a view that appears similar to Elliot Sober's answer (1997: S462-S465) to John Beatty's (1995) evolutionary contingency thesis. Responding to Beatty's evolutionary contingency thesis, Sober (1997: S462-S465) replied that Beatty had failed to recognize that laws do not hold outside their application domain. And when such a domain is added to a statement of law, the result is a statement that is not contingent. For instance, the **Hardy–Weinberg** rule is restricted to situations in which "there are no evolutionary forces present." Let "I" denote this applicability clause. When there are evolutionary forces present, the rule does not apply. Let us write this rule as a statement in a universal form, $\forall x(Fx \rightarrow Gx)$. According to Sober, we get *a non-contingent statement* if we add the clause "I" as an antecedent to the statement of Hardy–Weinberg rule. In other words, the statement "$I \rightarrow (\forall x(Fx \rightarrow Gx))$" *as a whole* is not contingent, although both of its components "I" and "$\forall x(Fx \rightarrow Gx)$" could be.

Although Sober's reply to the evolutionary contingency thesis is not the same as the *ceteris paribus* account of laws, the two are similar. In both, the applicability of a law is restricted by a protective clause that specifies in very general (and non-committal) terms those conditions under which the law in question is expected to apply to a situation. Likewise both intend to show that "contingency" of biological generalizations can be avoided by adding an applicability condition to the statements of laws.

If then, as Elgin's passage quoted above suggests, the idea of Elgin's laws can be written "$p \rightarrow (\forall x(cx \rightarrow bx))$," where p represents those physical and chemical conditions that guarantee the holding of the included biological (strict) generalization, $\forall x(cx \rightarrow bx)$, then apparently the resulting view is similar to *Sober's* view in which Elgin's p's are Sober's I's. In other words, biophysical laws hold and are non-contingent, according to Elgin, when and if their p's are satisfied, that is, certain non-biological conditions guarantee the holding of biological generalizations.

Not only is *Sober's* reply to the evolutionary contingency thesis similar to the *ceteris paribus* account of laws, but they also apparently share similar problems. This creates two difficulties for *Elgin's* view. First, it suggests that Elgin's laws are (similar to) *ceteris paribus* laws rather than strict ones because Elgin's view appears to be similar to Sober's view. However, this is contrary to what Elgin believes and suggests, because he seems to think that his biophysical laws are strict. Second, the problems that plague the *ceteris paribus* account of laws apparently plague Elgin's view as well, which suggest that Elgin's project is unlikely to succeed, because the *ceteris paribus* account of laws is encumbered by problems that we are unlikely to overcome (see section 3.5).

There is perhaps a way around these problems. Elgin's idea could be that his biophysical laws are strict after all, because their applicability domains or *ceteris paribus* clauses, that is, the *p*'s of Elgin's biophysical laws, can be discharged or eliminated at least in principle, since we can explain them or give explanations as to why they hold.

There is some evidence that the above is Elgin's strategy. For instance, Elgin claims that there are explanations for the constancy of scaling exponents of some allometries and scaling laws. In this context Elgin is referring to a phenomenon that can be called the similitude principle. I do not think that reference to this principle offers a good argument to avoid the problems mentioned above. However, before I take up my objections to this argument, I will first turn to the claim that allometries and scaling laws represent lawlike, universal, exceptionless, general, stable, non-contingent, and/ or constant generalizations.

Although allometries and scaling laws appear to be generalizations applying to many taxa, they are neither universal nor exceptionless. There are hundreds of examples of these generalizations in the literature. In fact there appear to be exceptions to all of them. Nor are these regressions universal in the sense that they apply to all taxa. There is a strong correlation between "density" and "body size" across motile taxa, a phenomenon that is called the **inverse** scaling rule. There is a similar strong correlation for plants and other sessile organisms between these two variables, but the relation is hypothesized to be the opposite: it is the density of populations that is treated as the independent variable affecting body size. And there seem to be good reasons why this **thinning** rule, rather than the inverse scaling rule, applies to sessile organisms.

Consider, for instance, green plants. There should be intense competition both within and between green plant species because all these compete for a limited set of critical resources (light, water, and similar nutrients). Thus, the thinning rule might hold because there is intense competition within and between the species for resources that these organisms cannot evade or avoid by migration (in their adult forms) as motile organisms can. The fact that in one generalization another's dependent variable is another's independent variable spells trouble only if one thinks along traditional lines of lawlikeness according to which laws are universal in scope.

Nor are the constants in allometries and scaling laws, namely the normalization constant, *a*, and the scaling exponent, *b*, truly constant, stable, or universal in character. In fact, these constants refer implicitly to *particulars* in values, that this, to the data from which they were derived. This is, however, not the point I want to make. My point is that these constants vary in value across different taxa and background conditions. These *a*'s and *b*'s in biological regressions are not immutable constants of nature.

For instance, homeotherms, poikilotherms, and unicellular organisms have different *a* values in equations that relate their metabolic rates to body size. For example, in the equation for a basal metabolic rate (**Kleiber's** rule), which scales with body size as $aW^{0.75}$ in these taxa, the values of *a* are 4.1, 0.14, and 0.018 for homeotherms, poikilotherms and unicellular organisms (Peters 1983: 29-30). Rather than indicating universal or covering laws with immutable constants, these values and their variance suggest that the generalizations represent interesting *explananda* (as was already argued in section 4.3). What has just been said is also true of other allometries and scaling laws: the values of their *a*'s vary across different taxa and/or background conditions.

But perhaps the variance of *a* values is not that revealing, since fixed or similar values of *b*'s give the appearance of qualitative similar trends in data, especially when the data are plotted on double-log axes. Yet, the variance in the values of *b*'s should be revealing: Variance in *b* values shows that there are *qualitatively different trends* among taxa and not just differences in the "elevation" in the regression lines. This is because the variance in *b* values amounts to different *slopes* in the regression lines (see Fig. 1). Thus, biological regression equations exhibit qualitatively different trends among taxa if there is variation in their *b*'s.

And there is considerable variation in the values of *b*'s in many regression equations. For instance, the above regressions for brain weights (section 4.3) vary among taxa with respect both to their normalization constants, *a*, and scaling exponents, *b*. The inverse scaling rule's *b* is said to be -0.75 for herbivorous mammals and/ or within trophic levels, but it is close to -1 for animals in general. These two regressions with different *b* values exhibit different phenomena with different implications, for instance, for the **energy equivalence** rule (see Damuth 1981, 1991; Peters 1983: 169-170; Brown 1995: 95-99; Marquet 2000, Marquet *et al.* 2005: 1754-1756).

These values of *a*'s and *b*'s in regression equations are not only taxa- or data-specific. In general, the value of *a*'s and *b*'s varies with respect to how one partitions the "reference class" in question. These reference classes can be divided more or less explicitly, e.g., by taking into account the taxonomy, latitude, ecology, metabolic classes, mode of reproduction or locomotion, levels of activities, trophic position, diet, habitat, ambient temperature and so on.

Again, that the values of *a* and *b* vary among many such partitions suggests that there are no universal or stable generalizations with immutable constants coming from such partitions nor are there perhaps qualitatively similar trends among them. Furthermore, in many cases it is the variations in data and its patterns that are more interesting as objects of explanations rather than the general and average or mean trends or the fitted regression lines and their constants (cf. Brown 1995 for examples).

Now I have admitted that there *are* some allometries and scaling laws that seem to have constant *b* values. I am referring to those that deal with biological rates, volumes and the like. In the previous section, I called this the similitude principle. Note, first, that the similitude principle is true, however, only as far as the *mean* values of *b*'s are considered; individual *b* values apparently do vary. But suppose for the sake of argument that *b* values in the regressions having to do with biological rates, volumes and so on, vary considerably less than in other allometries and scaling laws for which this principle is not true, that is, that do not deal with biological rates, volumes, times, and frequencies.

Elgin is aware that there are these constant values of *b*'s in some regressions. With this knowledge he could try to establish that some allometries and scaling laws are universal or strict because there are physical, chemical, and geometrical explanations for why these constant and general values in (the means of) *b*'s are found among different taxa. This brings us back to the idea of explaining applicability domains or *ceteris paribus* clauses of laws and showing how these could be strict laws with immutable constants. The fact is, however, that the importance and generality of the similitude principle is neither known nor well-established contrary to what Elgin assumes. In addition, the many explanations offered for this phenomenon have been found to be speculative or defective in the literature (cf., for instance, Peters 1983: 213-226). Unfortunately and revealingly, Elgin cites no critical discussion of the explanations of the similitude principle.

Moreover, these equations represent evolutionary contingent generalizations. In other words, allometries and scaling laws do encounter Beatty's (1995) evolutionary contingency thesis, which apparently threatens their lawlike status (section 2.6). In general and for instance, the coefficients or constants of allometries and scaling laws do not remain constant, but are subject to natural selection in values in contrast to what Elgin seems to assume (see Newell 1949: 123 and Gould 1966: 603-604, 616-621). Consider, for instance, a species of an ungulate for which there has been an evolutionary trend to increase body size and for which the size of the antlers scales or correlates strongly and positively with body size. Eventually this would lead to the size of the antlers of the species becoming so large as to become unwieldy and disadvantageous, as presumably happened with the Irish Elk, *Megaloceros giganteus*. In such a situation one would expect that there is strong selection pressure to change the value of b in the correlation between its body size and the size of its antlers, or otherwise the species becomes extinct.

The moral is that although for many animal taxa there has been a general evolutionary trend toward larger body size (a phenomenon that is known in the literature as **Cope's** rule), the destiny of the Irish Elk is an exception rather than the rule among them. That is, evolutionary pressure evidently has prevented their feet, tail, ears, bill, and other protruding body parts, which typically scale positively but *non-isometrically* with their body size, from increasing out of proportion and becoming disadvantageous to them.

I hope to have shown why we should not think of allometries and scaling laws are representing (strict or non-strict) biological laws.

Why is it that some think that these should represent laws in the first place? What is the justification behind such claims? The answer apparently lies in the view that many still have, namely, that laws are necessary for scientific explanation – an idea that I have questioned in this chapter.

4.5 Simple Causal Claims and Mechanistic Explanations

A frequently raised claim is that ecologists explain phenomena or patterns by mechanisms.[54] Yet ecologists have not given us definitions of mechanisms. For them, the concept is evidently pragmatic: mechanisms are things – whatever they are – doing the explaining of phenomena. Ecologists likewise speak of causes and processes as something explaining phenomena (or "patterns") without being explicit about what the causes or processes are.[55]

There is a distinction in the philosophical literature between two kinds of causal explanation, which I shall call "simple causal claims" and "mechanistic explanations." A simple causal claim describes the causal connection between the phenomenon-to-be-explained and the thing that does the explaining. It refers to a "phenomenological" or superficial causal explanation in which one has an invariant relation between variables, but no account – or mechanistic explanation – as to why or how the relation holds between the variables.

The account of explanation presented above describes how simple causal claims function by identifying what is required of a causal dependency relation to be explanatory. That is, simple causal claims need to be invariant during interventions. Describing a

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[54] See, for instance, Brown (1995: 119-187), Gaston (1996), Blackburn *et al.* (1999: 170-172), and Ashton *et al.* (2000: 406-410).

[55] In the interventionist account defended here, causes are difference-makers in that they can be intervened upon to manipulate or control their effects. Consequently, the species-area rule, for instance, can be called a causal generalization, if it is true that an intervention in an area of a habitat makes a difference in its species diversity.

mechanism of a phenomenon is not something that is contrary to describing what the causal dependency relation of a simple causal claim of that phenomenon is. Instead, a mechanistic explanation is a complement to a simple causal claim, since it describes *how* the dependency relation produces its phenomenon-to-be-explained.

A mechanistic explanation describes the internal causal structure of the phenomenon-to-be-explained. It describes the underlying mechanism *within* the system by showing how the system is constituted and how this produces the phenomenon-to-be-explained. Mechanistic explanations are causal and bottom-up explanations. Mechanistic explanations or representations of them are explanatory and true if they correctly describe the mechanisms in nature.

Recently, interest has revived in mechanisms in the philosophy of science. Previous philosophers had usually considered the concept to be primitive and had failed to define mechanisms (cf. Wimsatt 1976) and/or investigated mechanisms as the physical sciences in mind. For instance, Wesley C. Salmon (1984, 1989, 1994) gave no definition of mechanisms. Moreover, his causal processes interacting, which evidently constituted his mechanisms, were discussed with the physical sciences in mind. And there are serious doubts about whether his ideas concerning causal processes can be extended or applied to the biological sciences. Finally, there are serious difficulties with Salmon's causal-mechanical account of explanation that suggest it is not successful as an account (see Kitcher 1989: 459-477 and Hitchcock 1995). Thus, previous philosophers shed no light on the questions of what mechanisms are in biology or ecology and how they furnish us with explanations or understanding of phenomena there.

Fortunately, there are some recent (non-Salmonian) definitions of (biological) mechanisms in the philosophical literature. The most well-known is the one given by Peter Machamer, Lindley Darden, and Carl F. Craver (2000: 3):

> Mechanisms are entities and activities organized such that they are productive of regular change from start or set-up to finish or termination conditions.

I have no doubt that these authors give a more or less accurate *description* of mechanisms at least in such biological disciplines as neurobiology, molecular biology, cytology, and perhaps even ecology (see Pâslaru 2009). However, I have one problem with their definition; namely, they do not tie their definition to any account of scientific explanation that would tell *how or under what conditions (descriptions of) mechanisms are explanatory*. This makes their definition of mechanisms normatively shallow or unenlightening. This same problem applies to other recent definitions of mechanisms that are, in other respects, different. For instance, it applies to William Bechtel and Adele Abrahamsen's (2005) definition of mechanisms and to Stuart Glennan's (1996).[56]

Thus, the problem is not so much that of giving accurate definitions or descriptions of mechanisms in the biological sciences, but to come with an account of mechanistic explanation. As a solution to the latter problem, I suggest that we adopt James Woodward's definition of (representations of) mechanisms, which ties mechanisms to the account of scientific explanation presented

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[56] Now, it is true that, for instance, Stuart Glennan (2005) and Carl F. Craver (2007: 107-162) have, in their more recent and revised definitions of mechanisms, also defended "Woodwardian" definitions of mechanisms as I do below by emphasizing that the behavior of components of mechanisms should be describable by invariant generalizations. However, neither Glennan nor Craver have defended Woodward's modularity condition and neither of them discuss mechanistic explanations in the context of ecology as I do here. There is one paper that discusses mechanisms in ecology as well as their modularity, namely, Viorel Pâslaru's (2009). Pâslaru claims that Woodward's definition of mechanisms (to be defended in this section) is not sufficient for ecologists. Instead, he suggests, ecologists seek something similar to the definition of Machamer *et al.* (2000) as the correct description of their mechanisms. This observation may be correct. However, there is one problem in Pâslaru's paper. Pâslaru also seems to confuse the description or definition of mechanisms (and/or the fact that mechanistic explanations need to be "anchored" in entities, activities, and their organization) with the normative account of mechanistic explanation. The latter is discussed here, not the former.

above by emphasizing the modularity of components of mechanisms and invariance as the activity of the components:

> (MECH) a necessary condition for a representation to be an acceptable model of a mechanism is that the representation (i) describe an organized or structured set of parts or components, where (ii) the behavior of each component is described by a generalization that is invariant under interventions, and where (iii) the generalizations governing each component are also independently changeable, and where (iv) the representation allows us to see how, in virtue of (i), (ii) and (iii), the overall output of the mechanism will vary under manipulation of the input to each component and changes in the components themselves. (Woodward 2002: S375.)

One can think of a representation of a mechanism as a modular system of invariant generalizations or equations. Modularity can be understood to do with "the separability of different causal contributions to an overall effect" (Mitchell 2008: 699). In a modular system it is possible – at least in principle – to intervene in a variable of some generalization or equation without interfering or disturbing *other* generalizations or equations of that system. In other words, a component of a mechanism should be *independently changeable* with its other components (for a detailed presentation of the modularity condition, see Woodward 2003a: 327-342; see also Hausman & Woodward 1999, 2004; Cartwright 2002; Steel 2006).

Physical mechanisms are governed by physical forces. Consider physical component forces of Newtonian theory, such as gravitation and electricity, which have effects on the acceleration of massive bodies. As far as we know, these physical component forces are such that that we can intervene on one of these forces, say, gravitation (as in Newton's law of gravitation) without interfering with other such forces, say, electricity (as in Coulomb's law). In other words, it is possible to intervene on a mass of a system without affecting its charge. Thus, physical component forces are independently changeable or modular. I will assume that many ecological "component forces," such as competition and predation, are modular as well.

If a component of (a representation of) a mechanism is not modular under an intervention, then it is possible that one has misrepresented or mis-decomposed the mechanism into components. Likewise, a non-modular system cannot be used to provide answers to "what-if-things-had-been-different" questions that provide us with understanding and explanatory information about the phenomenon-to-be-explained. The reason for this is that in a non-modular system, interventions are not "surgical" in the sense that they would affect only the intervened-upon component, but they affect other components of the system as well, with unpredictable effects on the system's behavior.

There are counterexamples to the above definition. Some (biological) mechanisms are not modular in any general and/or absolute sense, but are non-modular under some interventions. Nancy Cartwright (2004: 807-811) and Sandra Mitchell (2008) think that this ruins the above definition. However, modularity as a condition should not be understood in an absolute or "either/or" sense, but as a degree condition of components, in contrast to above authors. It is likewise not a condition evaluated in isolation, but in comparison to interventions, for some interventions are more likely to be "structure-altering" than others (see Steel 2008: 154-160 for an interesting general discussion with examples). Thus, it is a mistake to think of modularity as a general, unconditional, and non-relative condition of (representations of) components of mechanisms or to claim that because some (biological) mechanisms are not modular under some interventions, modularity as a condition is questioned.

Examples of mechanisms and mechanistic explanations from philosophers of biology typically come from genetics, molecular biology, and neurobiology. We philosophers seem to be enthusiastic about such paradigmatic examples of mechanisms as photosynthesis (Tabery 2005: 4-8), the Krebs cycle (Perini 2005: 260-265), protein synthesis or the central dogma of molecular biology (Machamer et al. 2000: 18-21; Darden 2002, 2005), cellular respiration or metabolism (Bechtel & Abrahamsen 2005), Mendel's rules and Mendelian genetics (Glennan 2005: 446, Darden 2005), action potentials and synaptic plasticity (Craver 2002: S85-S88 and 2007; Bogen 2005), and the replication of HIV (Steel 2008: 55-58).

Many ecological mechanisms are not well known. In fact, most mechanistic explanations in ecology are undetermined by data or lacking in data (see Pianka 1966, whose paper is representative of ecological mechanistic explanations even today). Thus, many causal explanations in ecology are simple causal claims in the sense that there are no known or confirmed mechanistic explanations, for how the causes in these explanations produce their effects. This is true, for instance, of the species-area rule in which the "area" seems to be a cause of "species diversity," although we do know exactly why or how because the mechanism(s) behind this rule are disputed and under discussion. When ecology is contrasted with other biological disciplines, such as genetics, molecular biology, and neurobiology, in which the mechanistic aspects of explanations seem to be more prominent, ecological causal explanations appear to be "phenomenological" invariant generalizations whose mechanistic aspects remain to be discovered. This is the reason why I am unable to provide the reader with a paradigmatic example of an ecological mechanism or mechanistic explanation (but see Pâslaru 2009). Yet it is not my task to provide these, but rather it is the task of ecologists. My task was to discuss and defend an account of mechanistic explanation.

4.6 Conclusions

I have defended a non-covering law account of scientific explanation. According to it, it is the invariance of generalizations rather than their lawlikeness that gives generalizations explanatory power. An invariant generalization continues to hold or be valid under a special change – called an intervention – that changes the value of its variables. A generalization can be invariant and explanatory regardless of its lawlike status. There is no requirement that invariant generalizations should contain only purely qualitative predicate terms, be universal, maximally or highly stable, or belong to a systematic web of other generalizations, as many philosophers have suggested about laws. The interventionist account also provided some interesting interpretations of the explanatory status of ecological generalizations, such as allometries and scaling laws. The nature of mechanistic explanations in ecology was likewise discussed.

The interventionist account of explanation strengthens my argument for the same-level explanations of exceptions to ecological generalizations (section 3.6) by showing that explanations at higher levels can be explanatory to begin with. Whether a generalization remains invariant during its interventions is the criterion that determines whether it is explanatory. For a generalization to count as invariant during interventions, it does not make much difference whether it is a “macro,” “micro,” “higher-,” or “lower-”level generalization. Of course, there are sometimes reasons to prefer lower-level generalizations over higher-level ones. For instance, lower-level generalizations sometimes provide better or deeper explanations. However, this difference can be analyzed by saying that sometimes generalizations of lower levels (of mechanisms) have *larger* invariance domains (or stability domains) than higher-level ones – or that lower-level generalizations provide more “surgical” possibilities for manipulation and control. However, it is a mistake to conclude from this that higher-level generalizations are non-explanatory or non-invariant during their interventions because lower-level generalizations sometimes offer deeper or better explanations than higher-level ones.

The interventionist account covers causal explanation only, that is, the account is not an all-encompassing account of explanation, as some have mistakenly thought. Non-causal explanations, such as constitutive and mathematical explanations, are not dealt with here. By constitutive explanations I refer to explanations of property instantiations in which an explanation of a property of a system is given by its underlying nature. An example is an explanation of solubility of salt by reference to its molecular structure in which the latter explains and determines non-causally but asymmetrically the former. Another example is the explanation of noble gases’ chemical inertness by the fact that noble gases, such as helium, neon, and argon, have their outermost electron shell filled, and consequently, they cannot form bonds with other elements. Anatomy and histology are biological disciplines that are looking for constitutive explanations. Although in the case of constitutive explanations, there is a determination relation between "macro" and "micro" properties of a system, this determination relation is different from causal determination relation. Constitutive relations are *synchronic and componentially* related to their phenomena-to-be-explained, whereas causal relations are *diachronic and non-componentially* related to their phenomena-to-be-explained.

Yet it is possible that constitutive explanations are similar to causal explanations, although the two differ insofar as their determination relation is concerned. For instance, Carl F. Craver (2007: 139-160; see also his 2002) has made the suggestion that the explanatory relevance of constitutive explanations is based on their "mutual manipulability." This, in turn, suggests that some forms of non-causal explanation do not significantly differ from causal explanation. Furthermore, even if the scope of this paper is limited, an important species of explanation is nevertheless discussed, for causal explanations allow controlling, changing, and manipulating nature in contrast to some non-causal forms of explanation.

Finally, if the interventionist account can be defended successfully, then it would follow that there is no strong, practically relevant, or (historically) influential justification for the ecological laws debate, because laws are neither indispensable nor necessary for scientific explanations. This suggests that the ecological laws debate is a red herring.

5

STABILITY AND SCOPE[57]

5.1 Introduction

In this chapter, I will analyze such properties of generalizations as their scope and stability. I will also show how "contingent" biological generalizations function to provide us with reliable and generalizable scientific explanations.

James Bogen and James Woodward (1988; see also Woodward 1989) have articulated an interesting distinction between data and phenomena. What differentiates the two, say these authors, are their roles. Data function as *evidence* for or against phenomena, whereas phenomena function as *explananda*. Moreover, phenomena are recurrent, repeatable, and general, whereas data are less so. Often reliable data can be produced only in highly artificial or contrived conditions. Data also come in a variety of forms: graphs and curves; numbers, ratios, and measurements; reports of observations; readings of instruments; test scores; fossilized hard parts of organisms; and so forth.[58]

A major part of the discussions and controversies in scientific journals concerns the data and their reliability as evidence for or

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[57] A version of this chapter was presented at the Philosophy of Science Group/Trends and Tensions in Intellectual Integration seminar 30 September 2009 at the University of Helsinki.

[58] What is here called data is more exactly a "data model" (cf. Suppes 1962 and Harris 2003). Bogen and Woodward do not use this term, but it is what they have in mind. Data models are derived from "raw data" by manipulating, processing, and interpreting the latter by various means. The distinction between data and phenomena has interesting implications for theory testing, which I shall not discuss here (see Bogen and Woodward 2003).

against phenomena. For instance, in community ecology there is a famous controversy that is sometimes confused with being about a phenomenon, namely, the null or neutral models (or hypotheses) debate in the context of competition theory. The critics do not claim that the phenomenon of competition does not exist. Rather, they claim that there are no (statically significant and) reliable data for phenomena which are claimed by competition theorists to be the effects of interspecific competition, such as **Hutchinson's** rule, **character** displacement, and the **species–genus** ratio (see appendix).[59]

One admittedly fallible criterion for identifying a real phenomenon in data is to establish that the phenomenon is detectable via different and independent means, procedures, instruments, and so on. If the phenomenon is not "robustly detectable" in this sense, then we have reason to suspect that it is spurious or artificial (see Wimsatt 1981, 2006: 233-235; Bogen and Woodward 1988: 317; Culp 1994; Hudson 1999). I am not going to discuss the data and their reliability in detail, since it is difficult to be brief on the subject. Reliability of data as evidence for or against phenomena depends on many heterogeneous things that often are case- and study-specific, although there are also some general issues (see Bogen & Woodward 1988: 326-334, 2003: 244-249; Woodward 1989: 410-422). Besides, there is already considerable literature on the issue, and I shall not recapitulate those themes here (see, for instance, Wiens 1981, Hairston 1989, Arnqvist & Wooster 1995).

Yet there is one central property that phenomena do have, namely, stability. James Woodward observed something similar:

> [S]cientific investigation is typically carried on in a noisy environment; an environment in which the data we confront reflect the operation of many different causal factors, a

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[59] See Simberloff (1970, 1978, 1983, 1984), Grant (1972), Horn and May (1977), Wiens (1977, 1981), Connor and Simberloff (1979), Strong *et al.*(1979), Strong (1980), Simberloff and Boecklen (1981), Strong and Simberloff (1981), and Hastings (1987). Not all the authors mentioned are null or neutral modellers, but all of them criticize the data of competition theorists as being unreliable or deficient.

> number of which are due to the local, idiosyncratic features of the instruments we employ (including our senses) or the particular background situation in which we found ourselves. The problem of detecting a phenomenon is the problem of detecting a signal in this sea of noise, of identifying a relatively stable and invariant pattern of some simplicity and generality with recurrent features – a pattern which is not just an artifact of the particular detection techniques we employ or the local environment in which we operate. Problems of experimental design, of controlling for bias or error, of selecting appropriate techniques for measurement and of data analysis are, in effect, problems of tuning, or learning how to separate signal and noise in a reliable way. (Woodward 1989: 396-397.)

In the sciences, phenomena or *explananda* are often expressed as *stable generalizations*. This does not imply that generalizations or regularities are the only scientific phenomena, but they are my focus here. In ecology, phenomena or stable generalizations are called by a variety of names, the most common being "patterns." In paleobiology, phenomena are called evolutionary, anagenetic, or cladogenetic "trends."[60] Although some ecologists use the term "rule" interchangeably with patterns, I reserve the term rule for generalizations in general, whether these are phenomena (stable generalizations), *explanantia* (invariant generalizations), or something else.

I will investigate here the nature of the central property of phenomena, namely, stability. Although, Bogen and Woodward left

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[60] Examples of phenomena in ecology and paleobiology include evolutionary and ecological rules, such as **Bergmann's** and **Dollo's** rule; and such generalizations as intraspecific and interspecific patterns of **abundance** and distribution, the body size **frequency** distribution of a taxon, the **canonical** distribution of the abundances of species, the **latitudinal** diversity gradient, the **energy equivalence** rule, and the **hollow** curve (see appendix). These are examples of *explananda* rather than *explanantia* (or covering laws), because as generalizations they appear to be stable, but non-invariant (cf. section 4.3).

the concept undefined, I will show that there are different forms of stability. This is an important finding, given that stability is used to analyze the contingency of biological generalizations and some have suggested that stability should be identified with lawlikeness of generalizations. Furthermore, I will claim that stability is not a central or proprietary property of phenomena as suggested by Bogen and Woodward, but that it is also a central property of their *explanantia*. In other words, stability is a central property of explanatory generalizations insofar as their reliability and generalizability as *explanantia* are concerned.

I proceed as follows: In the next two sections I analyze the scope and stability of generalizations. My central claim is that stability is not a monolithic property of generalizations as previous philosophers have thought, but that there are different and related concepts or meanings for stability. In section 5.4, this finding is then used as a reason to criticize the idea that stability should be identified with lawlikeness. In section 5.5, I discuss and criticize the ideas of previous authors in more detail. In section 5.6, I discuss the different functions that stability and scope have in the context of "generalizing" scientific explanations. This shows that ecology or biology is capable of proving something more than "case studies" with invariant and stable generalizations of varying scopes.

5.2 Stability and Scope: A Preliminary Analysis

Sandra Mitchell (1997, 2000, 2002) has written extensively about the stability of (biological) generalizations. She understands stability to be something that deals with those background conditions on which the holding of a generalization depends. In other words, stability is "a measure of the range of conditions that are required for the relationship described by the law to hold" (Mitchell 2002: 346).

The more stable a generalization, the less dependent it is on background conditions to remain true. For instance, the generalization "all persisting lumps of pure uranium-235 have a mass of less than 1,000 kilograms" appears stable, because it depends on a small number of nuclear physical facts that are pervasive and deep facts about the universe. Unstable generalizations require a host of incidental conditions to remain true or to hold. Consider, for instance, such generalizations as "all persisting lumps of pure gold have a mass of less than 1,000 kilograms" and "all coins in my pocket are 50-cent pieces"; these statements hold true only if several incidental background conditions are in place.

As a first approximation, let us call a generalization (or regularity) stable if it holds in many possible background conditions.[61] An alternative phrasing would be that a generalization is stable if it holds during numerous interferences. Unstable generalizations break down under such conditions. Stability is a concept of degree as well. In other words, a generalization is more stable the more possible background conditions there are in which it holds. I take it that the above characterizations of stability capture the essentials of Mitchell's account of stability presented above. Let us call the range of possible background conditions over which a generalization continues to hold its *stability domain*.

Stability should not be confused with the property of generalizations that is called scope (cf. also Darden 1996: 413-414, Cooper 1998: 578, 581, and Waters 1998). Scope as I understand and define it gives the *application domain* of a generalization in the past and/or in the present. This domain includes all those (dis)similar systems to which some generalization applies or has applied. For example, both of **Mendel's** rules have their scope as (nearly) all sexually reproducing taxa. Conditions, such as the evolution of mitosis and meiosis, on which the validity or holding of Mendel's rules depend, are the background conditions in the above sense of stability. Other examples of scope include the following: *Some of the members of the lineage of horses* exhibited the pattern of **Cope's** rule *in the Miocene*. *Currently*, rain forests are *almost exclusively located at low latitudes*.

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[61] Of course, stability does not depend only on the number, but likewise on the nature of the background conditions. There are background conditions to which some generalizations (or rather regularities) are very sensitive to in their holding, whereas other conditions have milder impacts on their holding. Likewise, in certain sciences and disciplines certain background conditions are deemed as more important than others.

Although the scope sometimes works as data – e.g., the so-called checkerboard distribution of species of a guild to habitats is sometimes taken as evidence for the presence of interspecific competition – scope likewise works as phenomena. For example, why are *non*-Mendelian inheritance systems nearly non-existent among sexually reproducing diploid taxa?

Biological generalizations typically generalize about different taxa, features, entities, and so on in different times and/or places. Scope captures the idea that biological generalizations are distributed in their applicability to different taxa, places, epochs, and so on. Many biological generalizations evidently have narrow or limited scope (cf. section 2.6).[62] The important point is that a generalization that has a narrow scope could have a high degree of stability within this scope. The converse could also be true: a generalization that has a broad scope could have a low degree of stability within its scope. As an example, consider the criticism that **Rapoport's** rule (see appendix) has a narrow scope, that is, it is a local phenomenon at certain latitudes only (see Rohde 1996, Gaston *et al.* 1998, and Hecnar 1999). This criticism does not establish much about the extent of those background conditions on which the holding of this generalization depends. It is conceivable, although perhaps not plausible, that the rule is a very stable generalization within its scope. Moreover, as I argue below (section 5.6), criticizing generalizations of narrow or limited scope does not establish much insofar as the explanatory status of generalizations is concerned.

Although stability and scope both seem to deal with the generality of generalizations, they are also different in this respect. Scope deals with generality that has do with the actual distribution or range of (dis)similar systems to which a generalization applies or has applied. Stability, however, has to do with a holding of a generalization in possible background conditions. In other words, scope is a non-modal concept and property of generalizations, whereas stability is a modal concept and property of generalizations

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[62] Note that laws are typically thought of as having universal or unlimited scope. The same is true of stability: laws are often thought of as having maximal or very high degrees of stability (see sections 2.6 and 5.4).

because it has to do with considerations that deal with possibilities and counterfactuals. Stable generalizations provide support for *passive* counterfactuals rather than *active* one (see sections 4.2 and 5.6). Passive counterfactuals have the form "had this-or-that background condition been different, a generalization would still have held."[63]

5.3 Forms of Stability

A classic idea in ecology is that diversity should enhance the stability of ecological systems. Although the idea of this so-called **diversity–stability** rule (see appendix) at first appeared to be plausible and such that it could be used in conservation policies, theoretical and empirical studies came up with exceptions to the rule (e.g., natural monocultures of plants with considerable stability, see May 1975: 165-166) and studies also showed that conflicting and opposite results hold between the variables of "diversity" and "stability": sometimes diversity is a cause of stability, but sometimes it causes instability.

In the 1970s, it became evident that there might not be contradictions in the latter results for the diversity–stability rule. The reason was that different ecologists used different stability concepts when they studied the relation between diversity and stability (see Orians 1975 and Pimm 1984). It was found that diversity is a cause of some forms of stability, but also causes some forms of instability. This in turn suggests that there seems to be just not just one diversity–stability rule, but many rules with different stability concepts and different, even opposing relations holding between the variables in question.[64]

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[63] Active counterfactuals have the form, "if the value of the variable X of a generalization $Y_i = f(X_i)$ were changed by an intervention from x_1 to x_2, then the value of the variable Y would be changed from y_1 to y_2 in accordance with the relation $Y_i = f(X_i)$."

[64] Additional complexities for the diversity–stability rule(s) arise from the fact that there are many concepts of diversity as well. Moreover, while diversity could enhance *population* stability, it could also enhance *ecosystem* or

Thus, ecological systems exhibit different forms of stability. How many forms of stability are there? What are they? How do they differ? I answer these questions by distinguishing seven forms of stability defined as follows

1) *Constancy*: *lack of change* in a system despite changes in its background conditions.
2) *Persistence*: the *survival time* of a system when facing changes in its background conditions.
3) *Inertia* or resistance: the ability of a system to *resist change* in its background conditions.
4) *Elasticity* or resiliency: the *speed of return* of a system after changes in its background conditions.
5) *Amplitude* or domain of attraction: the *area or extent of equilibrium* of a system during changes in its background conditions; cf. local versus global stability of equilibrium, or positive, negative, and neutral equilibrium. A system could have several different amplitudes as is the case in systems with multiple (stable) equilibria (cf. Scheffer *et al.* 2001).
6) *Cyclical stability*: the ability of a system *to return to a cycle or an oscillation*, despite changes in its background conditions.
7) *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.

I have adapted the names and definitions of stability concepts from the ecological literature (see Orians 1975, Pimm 1984, and Grimm & Wissel 1997) with some modifications. Although ecologists disagree how to name different forms of stability, there seems to be agreement on their basic definitions. Thus, although some housecleaning is needed in the use of and names for stability concepts, ecologists do recognize there being different stability concepts and they do agree on what the basic meanings of the concepts are.

The above forms of stability appear to be different, but related and well-defined concepts that apply to systems and generalizations

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community instability. For discussion of these points, see Pimm (1984), Lehman and Tilman (2000), Mikkelson (1997, 2004), and Odenbaugh (2001).

about them. What is *common* to these forms of stability is that they *describe what is needed from a system or a generalization in order to hold during changes of its background conditions or interferences.* Nevertheless, the forms of stability are also different. Whereas constancy, persistence, and inertia deal with the *endurance* of systems or generalizations 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 generalizations during changes in the background conditions or interferences. Thus, we have different, but related concepts of stability. This suggests that stability is not a problematic conflation concept (see section 2.2).

Constancy, that is, remaining unchanged, is meant to apply to systems or generalizations that do not exhibit dynamics in contrast to inertia, which is meant to apply to systems or generalizations that exhibit some dynamics or active behavior. The point of distinguishing between constancy and inertia has to do with the distinction between non-change-relating and change-relating generalizations (see section 4.2). In other words, non-change-relating generalizations are stable in the sense of constancy and change-relating generalizations are stable in the sense of inertia. 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. This suggests that the forms of stability are not just continua properties, but also properties with thresholds and continua.[65]

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[65] My point is not that all the forms of stability just listed apply to *non-ecological* systems in addition to ecological ones; I shall, however, presume at least that most of them do apply to non-ecological systems. For instance, there are ergodic systems in thermodynamics that probably display most of the forms of stability listed above. Nor is my point here that all the forms of stability apply to *generalizations* about systems; I think that they do, but I have to leave the demonstration of this point to some other occasion. Nor is the important point here that there are *seven* forms of stability. There might be

My point here is to show that *stability is not a monolithic property of generalizations* as many evidently believe, but that there are different forms of stability that are distinct, yet related concepts that apply to systems and generalizations about those systems. And this is an important discovery insofar as the analysis of lawlikeness (and evolutionary contingency) with stability is concerned. Moreover, analyzing the forms of stability might help to illuminate how and why stability or stabilities are relevant in the context of scientific explanation.

5.4 Stability as Lawlikeness

Accounts of laws typically conceptualize lawlikeness as a dichotomous thing that is contrary to the "contingency" or "accidentality" of generalizations (see section 2.6). There is an account that conceptualizes lawlikeness differently, namely, the stability account of laws.

Because Sandra Mitchell is an advocate of a pragmatic account of laws (section 3.2), it is not fair to attribute the stability account of laws to her. Nor it is right to claim that she has replaced the dichotomous distinction between laws and accidental truths with a continuum of lawlikeness based on the continuum of stability as is also sometimes done. 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

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more forms of stability. It is also possible that some of the above stabilities are reducible to others as well. For instance, the main reason I have kept cyclical stability as a separate item rather than included it under amplitude is to emphasize that many biological ergodic systems display other kinds of stabilities over and above point equilibrium (see May 1972, 1975, 1976; see also section 6.5). In other words, the exact number of forms of stability is an open question. Finally, the issue in the present chapter is not whether there is an adequate *mathematical* notion of stability, such as the Lyapunov stability that captures the different aspects or different notions of stability in ecology (see Justus 2008).

lawlikeness with a continuum of stability (see 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 it, and moreover that lawlikeness is a continuum (e.g., Woodward 1992, Lipton 1999, Cooper 1998: 571-578, Mikkelson 2004: 124-125; see also section 5.5).

It seems that stability has many right properties to redeem the lawlike status of biological generalizations. Since stability is a degree, there might be degrees of lawlikeness. Consequently, even if some biological generalizations failed to achieve a maximal or a very high degree of stability – that is typically attributed to physical laws –, they nevertheless could count as lawlike if they had a high enough degree of stability. Likewise, the continuum nature of lawlikeness might explain why it has been so difficult to demarcate between laws and accidental truths. If stability is the right property to identify with lawlikeness and if it is a property with degrees, then the "contingency" of biological generalizations is not as serious a threat for their lawlike status as it would be if a dichotomous account of lawlikeness is held (cf. section 2.6). Besides, since all generalizations are more or less stable, the contingency of biological generalizations is not a sufficient condition to deny their lawlike status. To make such criticism potent it has to be shown that biological generalizations lack high degrees of stability in general and/or in contrast to the generalizations of other sciences. Finally, it is the ability of laws to support counterfactuals that is taken to be indicative of or a defining property of lawlikeness. And stable generalizations seem to support counterfactuals – which I called passive – having do to with the holding of generalizations in different background conditions.

As persuasive as these properties are for the identification of lawlikeness with stability, I have qualms about whether stability will be satisfactory in the given context.

To begin with, 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 fails to provide an account of lawlikeness, since it fails to distinguish between laws and non-laws. The central issue of any account of laws is to give a clear demarcation between laws and accidentally true generalizations.

Moreover, there was not just one stability, but several different forms of stability to identify with lawlikeness. Accordingly, there are several interpretations of the lawlikeness of (biological) generalizations in terms of their stability. There is no need to discuss all of them here, since not all are plausible candidates in the present context. For instance, it does not seem to be a plausible interpretation of lawlikeness to analyze it in terms of *persistence* or *elasticity* of generalizations, since these properties "only" concern temporal aspects of stability. But with the remaining five concepts we have somewhat plausible candidates of lawlikeness, since they have to do with more general aspects of endurance, recovery, or sensitivity of generalizations. In other words, lawlikeness can possibly be identified with constancy, inertia, amplitude, cyclical stability, and/or trajectory stability of generalizations.

Is it an advantage or a disadvantage of the stability account of laws that there are so many forms of stability to be identified with lawlikeness? On the one hand, it is perfectly conceivable that different sciences favor different forms of stability of generalizations as the most important one(s) in their domain. Consequently, it could be argued that there are different lawlikeness*es* in different sciences. This is an intriguing suggestion that has remained relatively 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. 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 five forms of stability just mentioned should be preferred as the lawlikeness of (biological) generalizations. Moreover, biological and non-biological examples of putative lawlike generalizations presented in the literature apparently display many of the above-mentioned forms of stability 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 forms of stability of generalizations and different sciences (or their disciplines). Nor it is true that different sciences favor different forms of stability of generalizations as the most important in their domain. Finally, generalizations about some science or discipline exhibit the above stabilities with heterogeneous and diverse patterns. To establish the points just made, consider ecology, its generalizations, and those forms of stability that ecological generalizations display.

For instance, there are ecological generalizations or theoretical systems that display many of the forms of stability mentioned. Consider **island biogeography**, which investigates such putative examples of ecological laws as **area**, **distance**, **endemicity**, and **species–area** rule (see appendix). A famous experiment of island biogeography consisted of small islands – of different sizes and from different distances from their faunal source area – that were "defaunated" (see MacArthur & Wilson 1967 and Wilson & Simberloff 1969). After the defaunation of the islands, re-colonization of fauna on the islands was monitored. The results of the experiment 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 a *trajectory stability* or succession towards it, there was no *constancy* or *inertia* in "species composition," at least in the first years, nor was there any trajectory stability toward it. In addition the speed of return (*elasticity*), inertia, and so on of species number and some other factors were studied in this and later experiments on island biogeography. At the same time, many allometries and scaling laws (see sections 4.3 and 4.4) exhibit only specific stability, such as inertia.

Thus, there are no systematic connections between the forms of stability of generalizations and different sciences. Instead, generalizations of some science display all forms of stability mentioned above. Nor are there reasons to think that some areas or disciplines of science, such as ecology deem only some of the forms of stability 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 many above and below generalizations are often presented in the literature as examples of putative laws, this spells trouble for the view and idea that some specific stability should be identified with the lawlikeness of biological generalizations.

There is another concern about the stability account of laws. The main reason behind the stability account is that its proponents apparently subscribe to a covering law account of scientific explanation (cf., for instance, 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 justification or motivation. Consider cases of spurious causation that, as generalizations, often display high degrees of stability, especially inertia. For instance, there is a stable and positive correlation between "readings on a barometer" and "the occurrence of storms" that is due to a common cause, namely, "changes in atmospheric pressure." This stable correlation 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, 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 ecological allometries and scaling laws. Although allometries and scaling laws as generalizations could be stable, many of them are apparently not explanatory, because they are cases of spurious causation. For instance, 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 appears to be a case of spurious causation (or one, which does not remain invariant under its interventions; see section 4.4). This suggests that many allometries and scaling laws do not represent causal or explanatory generalizations, but 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 often display high degrees of inertia or constancy, such as "all regions of space exhibit a cosmic background radiation of 2.7 degrees of Kelvin."

In a response, one could suggest that stability in the context of laws is something that the forms of stability have in common, such as it is a measure of the range of possible background conditions required for a generalization, relations or regularity to hold (cf. Mitchell 2000: 252 and 2002: 346). This would be an unenlightened account, because it simply replaces one vague or unanalyzed notion with another equally vague or unanalyzed notion. Nor would this response bypass the problems of the stability account of laws just mentioned. For instance, the response would not show that stable generalizations furnish us with explanations.

Let me take stock. The first problem of identifying stability with lawlikeness is not that there is a lack of stability in biological generalizations or generalizations of some other science, but that there are different forms of stability to identify with lawlikeness, which generalizations and examples of putative lawlike generalizations display by means of heterogeneous patterns. As a normative account of lawlikeness, stability is a concept that is *too diversely present* in (biological) generalizations. This is also a problem for the paradigmatic account of laws if it is used here to suggest that paradigmatic laws are stable generalizations (see section 3.2). The second problem of the stability account is that it counts as laws cases of "stable" spurious causation. Many of us think of these as paradigmatic cases of accidentally true generalizations, however. The third problem of the stability account of laws is that stability does not furnish generalizations with explanatory power, which is also the critical problem for the pragmatic account of laws discussed in chapter 3. This is a problem for the stability account of laws, because *lawlikeness* is commonly thought of a central property of generalizations in the context of scientific explanations. Finally, many biological generalizations seem to lack maximal and high degrees of stability (see also section 2.6).

5.5 Mitchell and Lange on Stability

Many philosophers have discussed stability in the context of generalizations.[66] Rather than going through all the articles and the ideas about stability in detail, I here concentrate on two different proposals, namely, Sandra Mitchell's and Marc Lange's. There are some general or common trends in the articles and authors that discuss the stability of generalizations. First, although explicit definitions of stability are missing, it becomes clear once we look at their examples of stability that the authors understood stability in the sense of my inertia and sometimes perhaps as constancy, which are not distinguished from one another (as in Woodward 1992, 2006b, 2010; Lange 1995, 2002, 2005a, 2005b; Mitchell 1997, 2000, 2002; Cooper 1998; Lipton 1999 and many others). Second, other stability concepts are typically neglected, with Mitchell (1997, 2000, 2002) being an exception; in addition to inertia, she also discusses trajectory stability. Third, some authors confuse stability with invariance, although the two are different properties of generalizations and moreover have different functions in scientific explanations (see section 5.6).

Sandra Mitchell (1997: S470-S473, 2000: 259-259, 2002) has dealt extensively with contingency and the stability of (biological) generalizations. The present chapter is intended as an elaboration on her ideas concerning the stability of generalizations and the function of stability in the context of scientific explanations. However, there are differences between us, which I briefly restate below.

Mitchell's definition of stability as a measure of the range of possible background conditions required for a generalization, relation, or regularity to hold (cf. Mitchell 2000: 252 and 2002: 346) and as a continuum is too non-committal in that it does not distinguish

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[66] See, for instance, Skyrms (1977), Schaffner (1980: 80, 1995), Cartwright (1983, 1989, 1995, 1997, 2002), Woodward (1992: 213, 201-206, 2006b, 2010), Carrier (1995), Lange (1995, 2002, 2005a, 2005b), Mitchell (1997: S470-S473, 2000: 250-259, 2002), Cooper (1998: 571-578), Lipton (1999: 163-166), Mikkelson (2004: 124-125), Elgin (2006: 124-130), Craver (2007: 65-72, 99-100), and Wimsatt (2007: 133-147).

between different forms of stability and their meanings as I do. Moreover, once we look at her examples of generalizations and their stability, it becomes clear that she understands stability in the sense of constancy, inertia, and/or trajectory stability – and she does so without distinguishing these concepts or properties from one another.

Her reply to John Beatty's evolutionary contingency thesis in terms of stability was dissimilar to mine as well, for she does not think that the thesis has such negative implications for the existence of biological laws as I claim (cf. section 2.6). Furthermore, Mitchell seems to conflate stability with invariance, and/or she refuses to distinguish between the two (see Mitchell 2002: 344-347). Finally, Mitchell's reference to stability in the context of her pragmatic laws is misplaced (section 3.2). Again, it is not the stability of generalizations, but their invariance that she should be looking for as distinctive of explanatory generalizations as I argued in chapter 4.

Recently, Marc Lange (2002: 412-421, 2005a: 396-402, 2005b) has referred to stability in the context of laws. In lieu of focusing on stability conditions of individual and separate generalizations as many others have done, 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 apparently to reinforce and supplement his inference rule account of laws (section 3.3) with considerations that deal with strong contingency of laws, where his *reliability* was a solution to laws' weak contingency.[67]

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

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[67] I will be concerned here only with the stability part of Lange's account of laws. Other papers that target Lange's law accounts (e.g., Alexander 2004) present more general criticism concerning Lange's ideas about the role and function of laws, for instance, in the context of inductive strategies.

domain over which laws are supposed to hold. Lange (2007: 471) expresses the basic idea of collective stability as follows:

> On this view, what makes the laws special, as far as their range of invariance is concerned, is that they are stable: *collectively*, taken as *a set*, the laws are as resilient as they could logically possibly be. All of the laws would still have held under *every* counterfactual supposition under which they *could* all still have held – every supposition with which they are all logically consistent. No set containing an accident can make that boast non-trivially. A stable set is maximally resilient under counterfactual perturbations; it has as much invariance under counterfactual suppositions as it could logically possibly have. (Lange 2005b: 424.)

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 generalizations 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 that is much 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 kilogram" is necessary (and lawlike), whereas the similar gold generalization is not.

Evidently, Lange developed the collective stability account first and foremost with physics in mind. However, he has applied the account to biology to show how the stability account could salvage the lawlike status of biological generalizations (see Lange 2004 and 2005a).

Lange (2005a) uses examples of biogeographical generalizations, such as the **area**, **distance**, **endemicity**, and **species–area** rule (see appendix) to show how ecological generalizations form a set of integrated laws that moreover could be independent in their collective stability from considerations that deal with physical counterfactual scenarios that would or could violate ecological laws. What makes ecological generalizations 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, they could be necessary (or lawlike), 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. In other words, 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 distant past by a large meteorite that destroyed all life permanently, then ecological laws would not have held on this planet. However, this scenario is not one that ecologists need to be worrying 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 generalizations 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, they are still necessary (or lawlike) generalizations. Collective stability seems to be a clever solution to John Beatty's (1995) strong contingency criticism of biological generalizations.

I have no criticism of Lange's (2005b) analytical or formal treatment of collective stability or lawlikeness. Nor do I claim that Lange is to be blamed because the collective stability account is a non-reductive, but non-circular account of laws (cf. Lange 1999a: 249-251), *provided* the account illuminates laws in other ways. There are other reasons that suggest his stability account is inadequate and/or inapplicable to biology. Moreover, I think that his idea of stability and laws' support of counterfactuals (or subjunctives) is not illuminating insofar as the explanatory status of generalizations is concerned.

Lange's treatment of and focus on collective stability leaves open what he means by the stability of *individual* or *isolated* generalizations. 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 generalizations need only be "reliable" rules of inference as discussed above (section 3.3). Nor does he give stabilities of individual generalizations any specific meaning(s), as I did in section 5.3.

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 supposed by Lange's stability account of laws. 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 about, first, 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. The only part of the synecological theory that has any coherence, is the competition theory. But even this theory appears reticulate and loose when it comes to the connections of its generalizations. Thus, it is far from clear that ecological generalizations form such 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 (current) ecological generalizations in contrast to what he seems to believe.

In addition, it is not clear how collective stability furnishes generalizations with their explanatory or predictive power – or how collective stability supplies us with "reliable inference rules" insofar as scientific explanations are concerned. Moreover, it appears that the explanatory and predictive power of generalizations depend on more "local" kinds of considerations than on collective stability (see section 5.6).

Besides, it seems that, as a solution to strong contingency, collective stability presupposes that the other laws that give the stability domain of some law cannot be "weakly contingent," because Lange apparently 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 exception-ridden, but reliable inference rule account of laws?

The problem is that exception-ridden generalizations admit exceptions to stability conditions of laws, whose stability domain the former determine, according to Lange. If there are exceptions to the law that charged bodies cannot travel at a speed faster than the speed of light, then how does this law constrain the stability of the Lorentz force law in the manner discussed above? The admittance of exception-ridden laws to a set of laws seem to imply 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 outcome seems to be a consequence of the fact that Lange treats stability as a feature that has *no degrees* (see 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. (2008: 83.)

Now, if there are exceptions to the law that charged bodies cannot travel at a speed faster than the speed of light, then it would seem to follow 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. Owing to the exception(s) of the former law, there is a nomically 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. Rather, it is an accidentally true generalization. Similarly, if there are exceptions to the laws of nuclear chain-reactions, then the implications seems to be that the construction of a lump of uranium-235 weighing over 1,000 kilograms is nomically possible after all – or at least that 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. That is, the uranium-235 generalization is not maximally resilient, and consequently, no law, but an accident. Furthermore, the argument above could perhaps be generalized to other examples of physical laws if it is true, as suggested by Lange (1993a, 2002), that all physical laws have exceptions. This in turn would suggest that there are no laws in (current) physics, a consequence that Lange would not be happy with. A similar argument can also be used to reason against the existence of biological laws, given that there appear to be exceptions to biological generalizations (section 2.3), which thus seems to suggest that biological generalizations are not maximally resilient within the integrated set of biological laws, and consequently, there are no biological laws.[68] Again, Lange's claim that laws need only be "reliable" is unenlightening and *ad hoc* in this context (see section 3.3 and Lange 2002: 413-414, 416 and 2004: 100-101).[69]

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[68] It could be asked, whether a more robust account of laws would have been produced had Lange treated collective stability as a degree. A degree account could be helpful when countering arguments to the kinds of counterexamples given above.

[69] In the context, Lange might refer to his suggestion that there are meta-laws, which "govern" ordinary laws (see 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,

Taken together, the points presented suggest that Lange's collective stability does not offer an account that could salvage the lawlike status of biological generalizations.

I have a different account of how individual generalizations are "stable," one that is being presented in this chapter. This account preserves many things that Lange wants to achieve with laws. The account shows how individual generalizations support counterfactuals and what kinds of counterfactuals they support. Lange's requirement that laws remain true in every counterfactual scenario that is nomically possible is not particularly illuminating in this respect. It likewise shows how biological generalizations furnish us with reliable scientific explanations. Moreover, the account shows how biological generalizations that refer to "particulars" can be explanatory (see Lange 1995). In other words, I do not think there is much wrong in thinking that there are reliable generalizations that function in the roles of laws. These are invariant and stable generalizations. These need not be laws, and they need not be collectively stable in contrast to Lange, however.

5.6 From Case Studies to Extrapolable and Unified Explanations

Some philosophers and biologists claim that biological explanations especially at the "middle levels" of biology, merely amount to case studies or natural history, that is, to explanations that cannot be generalized to other taxa, places, times, and so on over and above those from which the explanations originally came (see Simberloff 1982; Rosenberg 1985: 219-225; Shrader-Frechette 2001; Shrader-Frechette & McCoy 1993: 106-148, 1994). The reasons given are often the same or at least similar to the reasons given for why there are no distinctively biological laws, namely, that biological generalizations or phenomena are complex, unique, open, contingent, and so on (see chapter 2). In contrast, we have the defenders of the

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2002) admits that there are exceptions to *all* laws, including fundamental ones.

generalizability of biological explanations, who typically argue that biological explanations are expressed in terms of generalizations that include supervenient predicate terms that furnish us with "higher-level" generalizations claimed to be more lawlike and less "contingent" than those dealing with middle or lower levels (see Sober 1984: 47-59, 74-85, 95-96, 125-126, 1993: 73-77, 1997, 1999; Rosenberg 1985: 219-225, 2001c: 755, 758; Cooper 1990; Brown 1995; Carrier 1995; Lawton 1996, 1999; Mikkelson 2004). I do not find these defenses convincing for I have argued that reference to the lawlikeness of biological generalizations is not the right strategy for justifying their explanatory status.

Although it remains unclear to me exactly what case studies are,[70] I restrict the meaning of the term to those scientific explanations that are not generalizable in the sense of "extrapolation" and/or "projection" defined below. The problem in this section is similar to the problem that deals with the generalizability or "external validity" of experimental results (see Guala 1999, 2003; see also Hutchinson 1961, Wiens 1977, Hairston 1989).

In the first section of this chapter I discussed the idea that stability is a central property of *explananda*. However, stability is not an important property of *explananda* only, but also of their *explanantia*. I defended an account of scientific explanation in the previous chapter in which explanatory generalizations are invariant rather than lawlike. Invariant generalizations support *active* and the *same object counterfactuals* rather than *passive* and/or *other object* counterfactuals.[71] In other words, an invariant generalization provides us with an understanding about what would happen to a specific system if *some of its properties* were changed through intervention. What would happen to other, dissimilar systems under their similar changes is irrelevant to the explanatory status of an invariant generalization. Does this imply

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[70] Shrader-Frechette and McCoy (1993) list among the properties of case studies that they are bottom-up, quasi-experimental, inductive, particularistic, wholistic, and heuristic. Although some of these properties strike me as peculiar, I do not discuss them here.

[71] I have adapted the names of the counterfactuals from Woodward (2001: 9-10, 2003a: 279-285) and Woodward and Hitchcock (2003: 19-21).

that I am forced to admit that biology provides only case studies with no generalizable content? No. Stability and scope are relevant insofar as the generalizability of scientific explanations is concerned. However, the two have different functions.

With respect to the distinction between the *same* and *other object* counterfactuals, let me first make a distinction between two kinds of generalizability. What I call *extrapolation* has to do with the generalizability of explanations in terms of *passive and the same object* counterfactuals. Extrapolation deals with the generalizability of scientific explanations *to other possible background conditions with systems similar* to the original. What I call *projection* has to do with the generalizability of scientific explanations in terms of *passive and other object* counterfactuals. Projection deals with the generalizability of explanations *to other possible background conditions with dissimilar systems*. The similarity and dissimilarity referred to should be understood in terms of the similarity and dissimilarity of the causes and mechanisms of systems.

Although it is invariance – rather than stability – of generalizations that furnishes us with explanatory generalizations, there is an important function that stability of generalizations has in this context of explanations, namely, stability furnishes us with extrapolability and reliability of scientific explanations (cf. Mitchell 1997, 2002 and Woodward 2006b, 2010 whose ideas I elaborate on and synthesize here). Stability is an important property of generalizations insofar as their extrapolability and reliability are concerned, because stable generalizations furnish us with support for the counterfactuals I called passive. Passive counterfactuals have the form "had this-or-that background condition been different, the generalization would still have held."

I called the stability domain of a generalization the range or extent of background conditions or interferences over which it supports passive counterfactuals. A generalization could have many stability domains over which it is stable when stability is understood in the sense presented above. In the latter case, a more specific term for a stability domain can be used. When the context does not demand the use of a more specific term, I use the general term "stability domain" instead of "inertia domain" and the like. A stability domain appears to be a measure of a generalization's extrapolability and reliability. In general, the larger the stability domain of a generalization, the more (important) passive counterfactuals it supports, and accordingly, the more extrapolable and reliable the generalization 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. A lack of stability of a generalization has the effect of diminishing its extrapolability and reliability.[72]

The different forms of stability given above are measures of extrapolability and reliability. What was common to these forms of stability was that they described what is needed from a system or a generalization in order to hold during changes of its background conditions or interferences. At the same time they are somewhat different measures of extrapolability and reliability. Whereas constancy, persistence, and inertia deal with the endurance of systems or generalizations during changes in the background conditions or interferences, elasticity, amplitude, and cyclic stability deal with

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[72] There is a problem in explicating exactly what are the background conditions of a generalization over which it is supposed to hold. Although this is a question for which I have no general or formal solutions, I have claimed that considerations having to do with *ceteris paribus* clauses or "domains" of generalizations are local and discipline-specific in character (section 3.5). At the same time, my analysis of *ceteris paribus* clauses or domains of generalizations in terms of stability and scope shows that these issues can be conceptually clarified and analyzed. As I have further claimed, ecological generalizations can be corrected and made more precise, and their exceptions can be explained within their own conceptual domain (section 3.6). Consequently, it is possible to explicate *ceteris paribus* clauses or domains of ecological generalizations without "going down levels." Note that the latter does not imply that ecological generalizations can be "strictly complemented." Finally, as James Woodward (2000: 228-235, 2003a: 273-279) has argued, we can make use of generalizations that have vague domains if these domains are investigated *independently* of generalizations, whereas if the domains are included in (statements of) generalizations themselves (as in the *ceteris paribus* account of laws), we obtain generalizations that are vague and of little use in the context of scientific explanations.

recovery during changes in the background conditions or interferences. Trajectory stability could be understood as dealing with the sensitivity or inevitability of systems or generalizations during changes in the background conditions or interferences. Extrapolability and reliability of generalizations are no doubt dependent on the endurance, recovery, and/or sensitivity of generalizations under changes of their background conditions or interferences, that is, from their forms of stability.

For instance, the larger the inertia and constancy domain of a generalization, the larger and/or more important is the "set" of background conditions or interferences over which it holds. The larger the amplitude or cyclical stability domain of a generalization, the larger and/or more important is the set of background conditions or interferences over which the generalizations is capable of maintaining its equilibrium, cycle, or oscillation. The larger the trajectory stability domain of a generalization, the less sensitive is the generalization to the changes in its initial conditions. For instance, the **species–area** rule appears to be an invariant generalization (see section 4.3). The explanatory status of the rule does not depend on there being a generalization that holds in all or in most of the background conditions and that has no exceptions. Yet the rule could be generalizable and reliable if its relation between "the number of species" and the "area" remains invariant during changes in many of the (ecologically important) background conditions, such as in the biotic richness of the archipelagos to which it is applied (see also section 6.2).

Consequently, stable generalizations 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.[73] In this sense invariant and stable generalizations

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[73] My claim is not that all these forms of stability are equally important in this context. For instance, persistence and elasticity appear to be less interesting properties of generalizations insofar as generalizability and reliability of scientific explanations are concerned in contrast to constancy, inertia, amplitude, and trajectory stability. Furthermore, constancy was

function as many philosophers believe only laws function (see Lange 1993a, 1993b, 1995, 2002, 2005a, 2005b; and Mitchell 1997, 2000, 2002; see also Goodman 1954 and Brandon 1997). In addition just as there is a connection between the invariance domain and explanatory depth (section 4.2), there also seems to be a connection between the stability domain and explanatory depth. Explanations with high degrees of stability are often seen as deeper than explanations with low degrees of stability (cf. Hitchcock & Woodward 2003: 187-188 and Woodward 2006b, 2010, who seem to be have something similar in mind).

Yet stability does not establish much insofar as *projection* is concerned. There is no guarantee that generalizations dealing with dissimilar systems are similar in their support of passive counterfactuals. In fact, the default assumption seems to be that generalizations dealing with dissimilar systems are independent from one another insofar as their support of passive counterfactuals is concerned. There is thus no reason to think that generalizations dealing with dissimilar systems depend on similar background conditions in order to hold.

What about the function of *scope*? Scope does not appear be related to *extrapolation,* since scope furnishes us with information about the factual application domain of a generalization to (dis)similar systems rather than about the holding of a generalization in different background conditions. In fact, a generalization with a narrow scope could be extrapolable if it has a high degree of stability within its scope. Nor does the scope have much to do with how good or deep is an explanation furnished us by a generalization, because this has to do with how large is the invariance and/or stability domain of a generalization rather than the actual domain of application of that generalization (cf. Woodward 2000: 224-226, 2003a: 270; Hitchcock & Woodward 2003: 192-194 for similar ideas).

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defined to apply to non-change-relating generalizations. Although this implies that constancy is not an important form of stability in the context of causal explanations, there are non-causal explanations, such as constitutive explanations (see section 4.5), in which it could play an important role insofar as the generalizability of such explanations is concerned.

At the same time, scope appears to be related to *projection*. But in lieu of accounting for how projection is accomplished or on what properties of generalizations the support of other object counterfactuals depends, scope simply describes the systems to which a generalization applies or has applied. Consequently, scope does not illuminate the issue of projection. Regretfully, I do not have much to say about how the problem of projection is to be resolved. This is partly because other object counterfactuals involve changes in the identity of systems, which makes it difficult to estimate what consequences such drastic changes have (see Woodward & Hitchcock 2003: 20).[74]

Scope has a different, yet related function: it provides *unification* and *systematization* of (causal) knowledge (see also Waters 1998: 192-194). This *is* generalizability, but one that has more to do with classifications and descriptions than with explanations. Although it could be claimed that unification is not what makes explanations explanatory (see Barnes 1992, Waters 1998, Woodward 2003a: 362-367), unification can nevertheless be viewed as a serendipitous feature of causal explanations. Unification, for instance, allows for economical ways to present explanations. In other words, there are pragmatic and practical advantages in having unified explanations, especially in the sciences, where there appear to be different causes or mechanisms with similar effects, such as ecology. These points also suggest that projection is not a problem in the context of generalizing scientific causal explanations, because the scope or projectability of

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[74] Daniel Steel (2008) has made some other suggestions about what he calls "extrapolation." Steel has developed a mechanistic approach to "extrapolation" supplemented with inferences that he calls "comparative process tracing." He endeavours to show that it is possible to "extrapolate" qualitative claims about positive and negative causal relevance from one population to another population which "heterogeneous" to the original. This suggests that his "extrapolation" is what I call projection. It is unfortunate that Steel (2008) does not distinguish between what I call extrapolation and projection, since this makes it difficult to evaluate some of his developments and claims concerning his "extrapolation" and generalizability of mechanistic explanations.

generalizations has to do with the pragmatic and practical aspects of explanations rather than with how deep or good are the explanations offered by different generalizations with varying scopes.

5.8 Conclusions

In contrast to what previous philosophers have said about stability, I have suggested that it is not a monolithic property of generalizations but rather there are different, yet related concepts or meanings for stability. On this basis I criticized the idea that stability should be identified with lawlikeness. Although stability and scope seem to deal with the generality of generalizations, I argued that they differ in this respect. In my view, scope deals with generality that has do with the actual distribution or range of (dis)similar systems to which a generalization applies or has applied, whereas stability deals with generality that has to do with holding of a generalization in various possible background conditions.

I discussed the different functions that stability and scope have in the context of "generalizing" scientific explanations. Stability is an important property of generalizations insofar as their extrapolability and reliability are concerned, because stable generalizations furnish us with support of the counterfactuals I called passive, which have the form "had this-or-that background condition been different, a generalization would still have held." I called the stability domain of a generalization the range or extent of background conditions or interferences over which it supports passive counterfactuals. In general, the larger the stability domain of a generalization, the more (important) passive counterfactuals it supports, and accordingly, the more extrapolable and reliable the generalization 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. In this sense stable generalizations function as many philosophers believe only laws function.

I have argued that scope has a different, but related function, namely, to provide unification and systematization of (causal) knowledge. I suggested that more unified explanations are not deeper or better as explanations. Nevertheless, unification can be viewed as a serendipitous feature of causal explanations, owing to the practical and pragmatic advantages that unified or "projectable" explanations have in contrast to less unified explanations.

A choice that traditional law-centered views have forced upon us is the distinction between (universal) 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 ecological explanatory generalizations that are extrapolable and that unify or systematize our knowledge. Invariant and stable generalizations with varying scopes function in the above-described manner despite the fact that they need not be lawlike as generalizations and despite the fact that they are weakly and strongly contingent as generalizations.

6

ROBUSTNESS AND SENSITIVITY[75]

6.1 Scientific Models

The sciences employ different kinds of models, and these models serve various different functions (cf. Odenbaugh 2005). I will focus on the theoretical and mathematical scientific models that the theorists postulate to explain phenomena.[76] In such models, a causal generalization or a mechanism is modeled, and various modeling assumptions are made about it. I refer to these scientific models simply as models. For instance, models of competition theory – such as the **Lotka–Volterra** model of **interspecific** competition (see appendix) – are postulated to explain phenomena. Phenomena refer to stable patterns, such as local extinctions of populations of species, constant partitions of resources between species within a guild, checkerboard patterns of distribution of species on islands, **species–genus** ratios on islands, and so on.

Models contain simplifications, approximations, and idealizations, for which I use the collective term modeling assumptions.[77] Modeling assumptions sometimes make false, unrealistic, or inaccurate presumptions of their targets. These targets

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[75] A version of this chapter was presented at the Philosophy of Science Group/Trends and Tensions in Intellectual Integration seminar 17 November 2008 at the University of Helsinki.

[76] Note that I do not claim that these models are explanatory. Rather, I claim that these models are constructed with the aim at mind that they might provide explanations of phenomena.

[77] In this section, the term "modeling assumptions" is used ambiguously on purpose. Later on I discuss the kinds of modeling assumptions that exist and how these might affect the way we classify, analyze, and define analyses of robustness.

are typically natural systems. Modeling assumptions of theoretical and mathematical models concern, for instance, the type and number of equations in the model and the number and type of variables and parameters in equations. Modeling assumptions are used for various reasons. For instance, they are called for when a theorist seeks computationally tractable solutions.

The presence of modeling assumptions is one reason why some have questioned the representational accuracy, realism, and veracity of models. Moreover, many have criticized ecological models for lack of testability, lack of fit to data, and for giving inaccurate predictions (cf. Smith 1952, Pielou 1981, Hall 1988, and Peters 1991). In general, ecological models are often not amenable to direct empirical tests that provide clear results. Data are often equivocal, contested, or scarce, and parameter values of models are difficult to estimate or measure.

Above are important issues of scientific models to which I offer no solutions. Nor do I discuss them in detail. My point is that they are some of the main reasons why modelers engage in robustness and sensitivity analyses. It is because models are not amenable to rigorous empirical tests, data is equivocal, and so on that modelers sometimes choose to investigate models and their results by means of non-empirical investigations that I call robustness and sensitivity analyses.

I distinguish in this chapter, first, between robustness and sensitivity analyses. Second, I distinguish between two senses of robustness analyses, which I call *derivational* robustness and *sufficient parameter* robustness. All three – sensitivity and the two senses of robustness analyses – have different functions. The purpose of sensitivity analysis is to investigate the stability conditions of *a* model. The purpose of derivational robustness analysis is to investigate whether the results of similar models depend on certain modeling assumptions about models. Finally, the purpose of sufficient parameter robustness analysis is to warrant the use of unifying, abstract, and simple models of complex biological phenomena. Although robustness and sensitivity analyses are non-empirical investigations of models, they could help in developing, testing, and confirming models, since they have the potential to show which stability conditions and/or modeling assumptions the results of models depend on.

I proceed as follows. In section 6.2, I discuss sensitivity analyses with examples. In sections 6.3 and 6.4, I both present derivational and sufficient parameter robustness analyses and examine the taxonomies of robustness analyses of previous authors. In section 6.5, I give an example of how derivational robustness and sensitivity analyses are used in ecology in the context of interspecific competition models. In section 6.6, I discuss sufficient parameter robustness analyses with examples. Section 6.7 concludes.

6.2 Sensitivity Analyses

I call sensitivity analyses investigations in which *one* model is studied with regard to its stability conditions by *making changes and variations to the values of the model's parameters*.

Besides manipulating the values of *variables* of a modeled generalization or mechanism in a model to study its dynamic behavior – which amount to investigating the invariance conditions of that modeled generalization or mechanism – models are manipulated to study the behavior of the modeled generalization or mechanism under variation of its *parameter* values. The latter sometimes amounts to the investigation and simulation of the model's stability or background conditions; and to displaying how the modeled generalization or mechanism could function if its background conditions were varied in the devised manner.

As an example, consider the **species–area** rule that can be modeled as a power equation, $S=cA^z$, where S is the number of species in a given taxonomic group, A is the area of a habitat, and c and z are constants. There appears to be an invariant relation between the variables, "area of a habitat" and its "species diversity": according to a rule of thumb, manipulation or intervention in an area that increases it by ten-fold, doubles the species diversity of a habitat. The manipulation of area in a model of the species–area rule would amount to a non-empirical investigation of invariance conditions of the modeled generalization, such as "if an area of a habitat were varied or manipulated in value to such-and-such, its species diversity would have changed to such-and-such."

Although the parameter *z* of the species–area rule has some "canonical" values in the range between 0.15 and 0.35 (see, for instance, Preston 1962a, 1962b), its value depends on several different background conditions, such as whether the area in question is isolated or non-isolated as a habitat. Isolated habitats have higher *z* values than non-isolated habitats, and thus steeper slopes in the species–area curve, since *z* determines the slope of the curve. In isolated habitats, such as islands, there is a reduced rate of immigration of the species, and as a consequence they support comparatively fewer species than non-isolated habitats of similar size. The value of the constant *c* of the species–area rule likewise depends on many background conditions, such as the latitudinal or general biotic richness of the area in question and so on. With variation of the values of *z* and *c* in a model of the species–area rule we can study how the species–area rule could behave in different background conditions. For instance, if the value of *c* is varied in a sensitivity analysis of a model of the species–area rule, this can sometimes be viewed as changing the biotic richness background condition for the species–area rule, and this manipulation displays how the rule could behave under changes in its background conditions. Such manipulations would amount to a non-empirical investigation of the stability conditions of the modeled generalization or mechanism.

As another example of sensitivity analysis, Robert M. May (1974, 1976) demonstrated that simple, traditional non-linear models of population growth – such as the **logistic** and **exponential** models (see appendix) – 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 value. When *r* is set above its threshold value, the dynamics the above models exhibit is not that of classical point equilibrium but *n*-point cycles and even deterministic chaos, depending on how high the value of *r* is set. The results of May's sensitivity analyses of classical population growth models 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).

The result of May's sensitivity analyses received a great deal of attention among ecologists; they appeared to be 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 classical models on population growth.

There seems to be no biological generalization that has no exceptions to it. It is no wonder that the study of exceptions and the study of conditions under which exceptions become possible have interested biologists. This is true also of biological modelers: for instance, there are many studies that investigate how **Gause's** rule (see appendix) could have exceptions. These studies often use sensitivity analyses. Sensitivity analyses can show how a modeled causal generalization or mechanism could be broken up, and while doing this, we learn potentially something about the stability conditions of the causal core which is being modeled.

However, since it can be claimed that models include modeling assumptions, which sometimes make false, inaccurate, or unrealistic presumptions of their targets, it could be suggested that nothing straightforward can be claimed on the basis of a sensitivity analysis of a model. In other words, there is a worry that it is modeling assumptions rather than – what are supposed to be – the core explanatory aspects of models that are responsible for the results of models. There is an answer to this problem. If it can additionally be shown that the results of a sensitivity analysis are robust with respect to other, similar models, then something might have been learned from a sensitivity analysis that *prima facie* is not affected or biased by the caricature status of an individual model. This is because it can be claimed with some plausibility that it is the common causal core of models rather than their different modeling assumptions about this core that were responsible for the results.

It might be objected that my concept of sensitivity analysis is problematic. There are "radical" variations of a model's parameter values that seem to yield *new models* (of different phenomena) rather than being variations of an old model. For instance, when a modeler sets a value of a parameter of model – which is originally assumed to have non-zero values – to zero, this seem to amount to producing a new model rather than being a variation of the old model. As another example, setting the carrying capacity, *K*, of an environment to an extremely high value in an intraspecific competition model of population growth, such as the logistic population growth model discussed above, seems to amount to suggesting that the population in question is density *independent* in its growth or, in other words, that there is no intraspecific competition. Or consider a case in which both or one of the competition coefficients, α_{12} and α_{21}, in a model of interspecific competition, such as the Lotka–Volterra model discussed below, are changed from *negative* to *positive* in value. Such a manipulation of parameter values would amount to not modeling interspecific competition, but symbiosis or commensalism/parasitism!

At the same time, there seem to be examples of sensitivity analyses in which variations of models' parameter values preserve the identity of the model. Sometimes such analyses yield interesting results – especially when they are complemented by derivational robustness analyses – as I try to show below. In such studies modelers do not think that a new model is produced by variations but that an old one is studied to see what kinds of effects variations of the model's parameter values have on its "output values."

The problem here has to do with the identity of models. On what basis can one claim that the identity of a model has remained intact in sensitivity analyses? There is no space to discuss in detail how and on what criteria we should decide whether the identity of a model has changed in sensitivity analyses. Perhaps it suffices to say that (at least some central) parameters in models are not undetermined things, but things that come with a biological interpretation, meaning, and motivation that restrict the values that they take (cf. also Levins 1993:552-553). Thus, sensitivity is typically investigated over some range of parameter values. For instance, the parameter z in the model of the species-area rule has "canonical values" in the range between 0.15 and 0.35. The important point is that this range is determined separately for each model's parameter. These issues are moreover empirical and subject-matter specific. In a word, saying something general about these issues is difficult. Finally, a model's "substantial assumptions" partly determine the limits within which the parameters of the model can be varied in a sensitivity analysis. For instance, in a model of interspecific competition, it is presumed that species have negative effects on each other's growth rates or population numbers (otherwise, it would not be a model of interspecific competition, but of some other syn-ecological relationship between species), which limits the values the competition coefficients of the model can take; in other words, the coefficients have negative values. This suggests that at least some "radical" variations of parameter values discussed above should not be counted as sensitivity analyses, among other things, since some of the model's "substantial assumptions" are violated.

As non-empirical investigations of models, sensitivity analyses are not as philosophically interesting or puzzling as robustness analyses are. However, sensitivity analyses need to be discussed in the context of robustness analyses for two reasons. First, some authors have failed to distinguish between robustness and sensitivity analyses and even confused the two (see section 6.4). This is not only conceptually inaccurate, but misleading as well, for robustness and sensitivity analyses differ in functions. Second, although robustness and sensitivity analyses are different as non-empirical investigations of models, robustness and sensitivity analyses are also intertwined as investigations and can be used in a manner that they reinforce one another.

Sensitivity analysis is a term with different meanings. My definition is not intended to correspond to the activity in which the modeler investigates how the "(un)certainty" in the input of a model could affect the "(un)certainty" of its output, despite the fact that this is sometimes referred to in the literature as sensitivity analysis. Moreover, by sensitivity analysis I am not referring to perturbation analysis, which is a method for solving intractable equations, although this too is sometimes referred to as sensitivity analysis in the literature. Despite there being no generally shared definition of sensitivity analysis, the meaning of the term as it is used in this chapter should be clear.

6.3 Derivational and Sufficient Parameter Robustness Analyses

In robustness analysis one is investigating different models and their variations to see how the results of models are (not) affected by differences and variations of models.

As a general definition of robustness analyses I propose that they are *investigations of variations in modeling assumptions of different models of the same phenomenon in which the focus is on whether they produce similar or convergent results or not*. Because robustness analyses cover investigations of models under changes in their modeling assumptions, there are different ways to proceed in such investigations, because there are different ways to change modeling assumptions of models. Moreover and more importantly, there are different kinds of modeling assumptions that can be investigated in robustness analyses as to what effects their variations among or differences between models could have for the results of the models.

The above general definition is meant to apply to Richard Levins' robustness. I distinguish between two senses of "Levinsian" robustness, which are called derivational robustness (a term adopted from Woodward 2006a) and sufficient parameter robustness. Although Levins did not make such an exact distinction, it seems to be implicit in his examples of robustness analyses.

Robustness analysis is a search for similar or convergent results of different models under the variation of their modeling assumptions. What does one mean by the claim that models are different? According to my first sense of robustness, *derivational robustness*, different models are *similar* in the sense that they model the same or similar causal generalizations or mechanisms (which I here call the "common causal core" of models, borrowing the term from Weisberg), but different in the sense that they make different modeling assumptions about this core. For instance, there are models of interspecific competition in ecology that share a causal core, but that differ in modeling assumptions made about the core:

cf. different variations and versions of the classical Lotka–Volterra model.

Richard Levins was aware that models contain modeling assumptions that are open to suspicion, and that there is the possibility that the results of models depend on modeling assumptions rather than "on the essentials of the models." As a remedy he proposes robustness analysis:

> Therefore, we attempt to treat the same problem with several alternative models each with different simplifications but with a common biological assumption. Then, if these models, despite their different assumptions, lead to similar results, we have what we can call a robust theorem that is relatively free of the details of the model. Hence, our truth is the intersection of independent lies. (Levins 1966: 423.)

This is an often-cited passage from Levins. This passage fits with the above idea of derivational robustness, where Levins' "common biological assumption" is equivalent to my "common causal core" of models. This sense is also the one which many of the papers on robustness adopt (cf. Gibbard and Varian 1978: 674-677; Weisberg 2006a: 640-643, 2006b; Weisberg & Reisman 2008; Kuorikoski, Lehtinen & Marchionni 2010).

The second sense of robustness has to do with investigations of *dissimilar models*, that is, models that model different causal generalizations or mechanisms for one and the same phenomenon. In other words, such models differ in their causal core. This second sense of robustness, which is discussed under *sufficient parameter* robustness below, has been neglected in the literature.

Jaakko Kuorikoski, Aki Lehtinen, and Catherine Marchionni (2010) distinguish between two kinds of modeling assumptions, which they call tractability assumptions and substantial assumptions. The authors suggest that tractability assumptions are introduced into models through mathematical tractability, and they distinguish tractability assumptions from models' substantial assumptions, which define the causal core of models. This distinction allows the meaning of derivational and sufficient parameter robustness analyses to be more closely defined.

I suggest that we define *derivational* robustness analyses as investigations of how differences in the models' *tractability assumptions* could affect their results, while *sufficient parameter* robustness analyses could be defined as investigations of how differences in the models' *substantial assumptions* affect their results. The point of distinguishing sufficient parameter robustness analysis from derivational robustness analysis is to emphasize that robustness analyses are not simply investigations of how differences in "modeling assumptions" could affect the results into models, but also that there are different kinds of modeling assumptions. Consequently, there could be different kinds of robustness analyses as long as different kinds of modeling assumptions are investigated.[78]

In addition to Levins' robustness, there is William C. Wimsatt's robustness analysis as "multiple means of determination" (see Wimsatt 1980a: 306-315, 1980b, 1981, 1994: 210-220, 2007: 37-74; see also Culp 1994, Hudson 1999, and Calcott 2010). This concept appears to be different from the two "Levinsian" notions just discussed. Wimsatt's robustness analysis has been used in the context of evidence and data, hypothesis testing, objectification, calibration of instruments and results, realism, and so on. Kuorikoski *et al.* (2010) show how derivational robustness can be embedded within Wimsatt's scheme of robustness. Kuorikoski *et al.* show how what they and I have called derivational robustness can be interpreted as an example of Wimsatt's robustness and how derivational robustness analysis can be understood as an activity in model building (in economics) that supplies epistemic credence to the robust results of models.

I have not discussed the epistemic relevance of derivational robustness analysis, since it does not seem to be typical of biological modelers to be mainly engaged in investigating the effects that different tractability assumptions have on the results of models, such as Kuorikoski *et al.* (2010) suggest is the case with economic models and their robustness analyses. Rather, it is typical of biological

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[78] The taxonomy of modeling assumptions presented above is undoubtedly far from exhaustive. In fact, with a more detailed taxonomy of modeling assumptions (see, for instance, Mäki 2000 and Musgrave 2001), we could arrive at a more detailed taxonomy of robustness analyses of models.

modelers to investigate what effects different *substantial assumptions* could have on the results of the models. In other words, biological modelers investigate models with regard to their derivational and sufficient parameter robustness, based on two reasons that reflect the nature of the models' targets. First, there is variation within biological systems and, second, there appear to be alternative, redundant causes or mechanisms for many biological phenomena. Consequently, it would be difficult to argue that robustness analyses have significant epistemic implications for the robust results of biological models, since biological models' substantial assumptions appear less "substantial" and more "varied" in robustness analyses than they might be in the case of economic models and their robustness analyses.

I admit that the distinction between derivational and sufficient parameter robustness is slippery insofar as it rests on the distinction between similarity and dissimilarity of models' "causal core." Yet, this distinction is slippery, because similarity is a concept of degree rather than an absolute.

6.4 Previous Taxonomies of Robustness Analyses

Other authors have also provided taxonomies of robustness. I will comment upon two vis-à-vis my robustness and sensitivity analyses.

Michael Weisberg and Kenneth Reisman (2008) distinguish three kinds of robustness, parameter robustness (results surviving changes to the parameter set of a model), structural robustness (results surviving changes to the structure of the model), and representational robustness (results surviving changes to the whole representational framework in which the model has been framed).

The idea of their parameter robustness appears to be problematic, given my distinction between robustness and sensitivity analyses. I defined sensitivity analysis as having to do with variation of parameter values of *a* model. Sensitivity analyses differ from robustness analyses in that in the former only one model is studied and the focus is on the stability conditions of the model rather than on different modeling assumptions introduced into (dis)similar models and their effects on the results of the models. The point just outlined suggest that Weisberg's and Reisman's parameter robustness as an investigation of how "results survive changes to the parameter set of a model" should perhaps be viewed as an instance of sensitivity analysis rather than robustness analysis. In any event, the authors do not provide criteria according to which sensitivity analyses are to be distinguished from robustness analyses. This suggests that Weisberg and Reisman have not differentiated (parameter) robustness analysis sufficiently from other non-empirical investigations of models, such as sensitivity analysis, which in turn suggests that their definition of (parameter) robustness is insufficient.

Another problem with their characterization of parameter robustness analysis is that some "radical" variations of parameter values of models apparently produce *dissimilar* models of *different* phenomena rather than being investigations of similar models of the same phenomenon; cf. the example in section 6.2, in which one or both of the competition coefficients in a model of interspecific competition were changed from negative to positive in value. Again, this shows that Weisberg and Reisman owe us criteria by which robustness analysis can be distinguished from other non-empirical investigations and manipulations of models.

The idea of Weisberg's and Reisman's representational robustness is to investigate what effects different representational frameworks of a model could have on the results of the modeled causal core. The idea of their structural robustness is to investigate whether variation in the structure of a model makes a difference in the results of the modeled causal core. To my mind, Weisberg and Reisman have identified interesting and important scientific activities in these robustness analyses. One problem with structural robustness analysis is that the term "structure" appears to be non-committal in meaning insofar as it does not specify what kinds of differences in or variations among models we are interested in through such analyses. In fact, a similar criticism seems to apply to representational robustness analysis and its "representational framework" of a model.

The main problem with representational and structural robustness, however, is that both presuppose, without any justification or argument, that robustness analyses are investigations of how certain differences in or variations among *similar* models could affect their results. Note that without this assumption, their representational and structural robustness analyses would not function in the way Weisberg and Reisman suggest. For instance, ecologists model and simulate competition within and between species by using differential and difference equations, stochastic and deterministic models, and so on. Although these are models of the same phenomenon, which are presented by using different representational frameworks, the models can also be *dissimilar* insofar as their causal core is concerned. In such situations, representational robustness analysis would not function, simply because it could not be claimed that it is the differences in or variations among models' "representations" that are responsible for the differences in the results of such models, since the models also differ in their causal core. A similar argument can be made in the case of structural robustness analysis, that is, there are "structurally different" and dissimilar models of many ecological phenomena.

There would be no problem if derivational robustness analysis were the only game in town. However, as I have suggested, there seem to be robustness analyses that investigate *dissimilar* models. This is a problem for Weisberg and Reisman, because their definition of robustness analysis neglects the possibility that there might be different kinds of modeling assumptions in addition to tractability assumptions. These could be investigated in robustness analyses for the effects that their differences in or variations among models could have on the results of the models.

James Woodward (2006a) distinguishes four notions of robustness: inferential, measurement, derivational, and causal robustness. The problem with Woodward's taxonomy is in a sense the antithesis of that provided by Weisberg and Reisman. Whereas the latter two authors concentrate on a narrow sense of robustness analysis, Woodward extends the meaning of the term to cover all kinds of non-empirical investigations of models without giving much justification for this extended use.

As the name suggests, inferential robustness has to do with the sensitivity of an inference from a body of data to a conclusion that is reached by using different "assumptions." For instance, if different models give similar or convergent results, when there is suggestive but not conclusive evidence for the results, then *robustness* is thought to give good grounds for believing or inferring that the results are likewise true. If, however, different models lead to non-robust results, then this is seen as a reason to suspend belief regarding the results.

Inferential robustness seems to be what Steven Hecht Orzack and Elliot Sober (1993) interpreted robustness to be, when they accused Levins (1966) of proposing that robustness is a non-empirical way of confirming or testing models. Levins (1993) responded that this was not what he meant by robustness (as is discussed in section 6.5) and that confirmation and testing of models is different from robustness analysis but used in conjunction with it. In addition, Levins had good reasons to make this claim since, as Woodward (2006a: 219-231) demonstrates, inferential robustness seems to be demanding as a notion and not applicable to most of the cases where scientists use robustness analyses. In any event, this notion is different from Levins' sense of robustness analysis (or analyses) discussed here.

Woodward's measurement robustness corresponds partly to Wimsatt's sense of robustness, which I do not discuss in detail in this chapter (see the previous section, however). His causal robustness has to do with the size or range of invariance conditions of models. Rather than thinking of this as an instance of robustness analysis (or sensitivity analysis), I suggest that we view his causal robustness as an instance of non-empirical investigation of the invariance conditions of a model. In other words, Woodward has confused robustness analyses with other non-empirical investigations or manipulations of models.

The *idea* of his fourth notion of robustness, derivational robustness, corresponds to my derivational robustness. However, Woodward's *example* of this robustness could be interpreted to correspond to my sensitivity analysis, which suggests that Woodward has confused robustness and sensitivity analyses with one another; or alternatively failed to provide criteria for how we are to distinguish the two. As an example of derivational robustness, Woodward discusses the case in which the value of a parameter of a regression model is varied to see what implications this has for the results of that model.

6.5 Sensitivity and Derivational Robustness Analyses of a Lotka–Volterra Competition Model and Exceptions to Gause's Rule

The purpose of *derivational* robustness analysis is to investigate whether a model's *tractability* assumptions (do not) make a difference for the results of that model. Although modelers introduce falsehoods, inaccuracies, omissions, and so on into models in their modeling tractability assumptions in order to make models more tractable mathematically, analytically, computationally, and so on, it can be shown that these do not bias or affect the results of the models if there are similar models using different modeling assumptions that have similar or convergent results. Unless otherwise stated, the term modeling assumptions in this section refers to tractability assumptions.

However, demonstrating that a result is not derivationally robust is an important achievement as well. Such a demonstration has the potential to show which specific modeling assumption(s) that result depends on. This is important, since it is difficult to test and investigate each and every modeling assumption of models separately and individually to see how they affect the results. The same is true when results are robust: one can say that results appear not to be dependent on the different modeling assumptions of models but follow from their common causal or explanatory core without testing and investigating each and every assumption separately and individually. This shows how derivational robustness analyses can help in the context of testing of models. If results are not similar or convergent, one needs to isolate the assumptions of models that are responsible for non-convergence. With a non-robust result, testing of models becomes easier since individual assumptions of models can sometimes be tested and studied empirically.

The point of derivational robustness is not to show that models with robust results are more trustworthy, realistic, valid, or true given only the robustness of their results. Levins (1993: 554) himself wanted to distance robustness from such implications:

> Orzack and Sober [1993] are worried that the robustness strategy seems to propose a way to truth independent of observation. This is not the case. Observation enters first in the choice of the core model and the selection of plausible variable parts, and later in the testing of the predictions that follow from the core model. Multiple models sharing that same core [*C*] help to find the consequences of that core when we are unable to offer a general proof that *C* implies *R* [where *R* is a robust theorem of *C*]. Thus the search for robustness as understood here is a valid strategy for separating conclusions that depend on the common biological core of a model from simple simplifications, distortion and omissions introduced to facilitate the analysis, and for arriving at the implications of partial truths. The use of multiple models is a common practice for this reason, either to strengthen the conclusion or to guide us in looking for a general result.

Derivational robustness analyses show which modeling assumptions certain results of different models (do not) depend on. It does not furnish models or their results with truth values or confirmation, however (see also Forber 2010: 37-39).

As an example of how sensitivity and derivational robustness analyses function together, I will take up the classical model of interspecific competition and some results it suggests. My purpose is to show that some central and traditional results of this model are both sensitive and non-robust.

One central task of ecology is to account for the diversity of life. According to **Gause's** rule, there should not coexist in the same habitat or community many species that are in intense competition with their limiting resources, such as food, space, and nesting site. That is, species that have similar, let alone identical, niches cannot coexist at the same place for long periods of time. The classical model of interspecific competition is the **Lotka–Volterra** model. Gause's rule can be viewed as a theorem or a mathematical consequence of this model. The traditional idea and result of the classical Lotka–

Volterra model of interspecific competition is that the number of species competing for a set of limiting resources should not exceed the number of resources in that set – in equilibrium *n* sympatric species cannot coexist indefinitely on fewer than *n* resources. The converse is also thought to be true: *n* sympatric species that coexist indefinitely with fewer than *n* resources must have (evolved) different (realized) niches (cf. **limiting** similarity). Laboratory experiments apparently confirmed the mathematical and analytical results of the classical model of interspecific competition on which Gause's rule is based (see Park 1948, 1954, 1957, Leslie *et al.* 1968).

There are striking exceptions to the above idea, however. That is, there are ecologically similar species co-existing for long periods of time with few resources. For instance, many co-existing phytoplankton species live in bodies of waters and share a low number of resources; these resources are likewise very limiting as a result of their scarcity (see Hutchinson 1961). In such a situation, one would expect to find intense competition among phytoplankton species and local extinctions of all but one or very few of the species.

The explanation that G. Evelyn Hutchinson gave to this famous "paradox of the plankton" was that owing to their fluctuating environment, the competitive systems of the species do not reach their (point) equilibrium. I will return to this explanation below in the context of sufficient parameter robustness. In any case, exceptions to Gause's rule, such as the findings mentioned, inspired theoretical and mathematical ecologists to investigate conditions under which Gause's rule does not hold. I will limit my discussion to a few theoretical and mathematical studies that exhibit how derivational robustness and sensitivity analyses are used to provide, first, conditions under which such exceptions can occur and, second, demonstrations of parameter values or modeling assumptions, that these exceptions depend on.

It has been demonstrated by mathematical and theoretical ecologists that coexistence of *n* species with *fewer* than *n* resources is possible given certain parameter values of the classical competition model and/or given that some of the classical modeling assumptions of the Lotka–Volterra model are relaxed or replaced with other assumptions. Thus, "no coexistence of *n* species with fewer than *n* resources," that is, Gause's rule, is as a result *sensitive* to certain parameter values of the classical interspecific competition model, such as the values of intrinsic growth rates and/or competition coefficients (cf. Koch 1974a, 1974b, and Vandermeer *et al.* 2002). In addition, Gause's rule is a *non-robust* result of this model: coexistence of *n* species with fewer than *n* resources seems impossible owing to certain tractability assumptions of the classical model.

The idea codified in Gause's rule, which was suggested by the classical Lotka–Volterra interspecific competition model, depended on two general factors. First, it depended on certain (unrealistic) modeling assumptions of the Lotka–Volterra model, for instance, that there is a linearity assumption in species' intrinsic growth rates with the amount of resource(s) and that all the life history characteristics of species are described adequately by this coefficient; no time lags are allowed; environment is supposed to be spatially and temporally homogenous; there are no higher order interactions between species; migration is unimportant; and competition is the only important syn-ecological force that affects the system. All the assumptions just outlined are such that when some or one of them are relaxed, co-existence of competitors becomes possible especially if they are accompanied by the following factor. Second, in the classical model only certain stabilities – namely, point equilibrium of fixed population densities – of results were thought to exhibit coexistence, whereas today it is presumed that at their coexistence equilibrium competitive systems can and do exhibit other stabilities, especially cyclical stability (see Armstrong & McGehee 1980 and Chesson & Case 1986; cf. also Cuddington 2001).

Accordingly, it can be proposed that Gause's rule holds only for a certain range of parameter values and/or with certain tractability assumptions.[79] However, the classical model of interspecific

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[79] Interestingly, Arthur L. Koch (1974a) showed that his demonstration of co-existence of *two* competitors with *one* resource in a seasonally varying environment was a robust result of non-classical interspecific competition models. Another interesting case of robustness analysis is the **Volterra** rule (see appendix), which evidently is robust with regard to different Lotka–

competition can be helpful despite its being false or inapplicable: one can use this model – when its robustness and sensitivity are being analyzed – to point out reasons why the model fails to hold. Hutchinson (1961: 143) apparently held a similar view:

> [M]y main purpose has been to show how a certain theory, namely, that of competitive exclusion, can be used to examine a situation where its main conclusions seem to be empirically false. Just because the theory is analytically true and in a certain sense tautological, we can trust it in the work of trying to find out what has happened to cause its empirical falsification.

In the above analyses one has moved towards an explanation of why some models did not hold and why there were exceptions to them: Robustness and sensitivity analyses are powerful tools for studying the conditions and assumptions where models break down – and they are especially powerful in pointing out reasons as to why they do this. They show which conditions or assumptions the results of models depend on. For instance, when one has a non-robust result, it is sometimes possible to pinpoint the modeling assumptions that are responsible for this result or that are responsible for the non-robustness of this result. If the result is in conflict with the data, one can replace the corresponding modeling assumption(s) to resolve the conflict. In this sense, as William C. Wimsatt (1987) would put it, this process allows one to move toward truer or realistic models and theories.[80]

Volterra prey–predator models (see Weisberg 2006a: 642-643, 2006b; Weisberg & Reisman 2008).

[80] This does not imply that robustness (or sensitivity) analysis is a non-empirical way to confirm models, as was discussed above. The point here is that robustness and sensitivity analyses can be used in confirming, testing, and developing models, since these analyses can be used to show in which conditions and/or on what assumptions the results of the models do or do not depend.

6.6 Sufficient Parameters and Robustness Analyses

I have discussed robustness that dealt with models and results that had a common or similar causal core, but that made different tractability assumptions about this core. There appears to be another type of robustness analysis that has to do with investigations of *dissimilar* models, namely, models that differ in substantial assumptions. I will call this sufficient parameter robustness, since Richard Levins and William C. Wimsatt speak of sufficient parameters in the context of robustness analyses.

I will show how sufficient parameters, robustness, and the use of simple and abstract models of complex phenomena – themes that were and are still defended by Levins (1966, 1993, 1998, 2006; Levins & Lewontin 1980; see also Odenbaugh 2003, 2006) – are connected to one another.[81] Unless otherwise stated, the term modeling assumptions in this section refers to substantial assumptions.

One pervasive problem of the biological sciences is that there appear to be many different causes or mechanisms (at lower levels) that have similar or convergent results, and that we often do not know which of them is the actual cause or mechanism of a phenomenon – or which contribute or interact to produce it. One advantage of having sufficient parameter robust results of *dissimilar* models is that their robustness counts as a warrant to use unifying,

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[81] I will not take up the issue of different types of models or modeling. Nor will I discuss the related issue of impossibility of simultaneously maximizing models' generality, realism, and precision. These latter issues of Levins have interested many (cf. Orzack & Sober 1993; Odenbaugh 2003, 2006; Orzack 2005; Weisberg 2006a), whereas sufficient parameters and their connection to robustness have not. The only recent paper I know of that discusses sufficient parameters in detail is Rasmus Grønfeldt Winther (2006). His ideas have developed from a different angle than mine. Winther discusses how Levins' ideas of modeling, including his idea of sufficient parameters, are connected with the dialectic view of science as defended by Levins. This dialectic view of science and Levins' related criticism of reifying abstractions are not discussed in these pages.

abstract, and simple models of complex phenomena.[82] In lieu of building a model for each and every case where different mechanisms or causes are or could be at work, a modeler builds a model that is simple and abstract, yet robust or unifying in that it captures the common effect(s) of dissimilar models, which model or illustrate different causes or mechanisms with similar effects. One way to build such models is by constructing a "sufficient parameter" into a model (see Levins 1966; Wimsatt 1979: 360-361, 366, 1980a: 303-306, 1981: 149-151).

Levins himself clarified this concept first by an analogy:

> The thousand or so variables of our original equations can be reduced to manageable proportions by a process of abstraction whereby many terms enter into consideration only by way of a reduced number of higher-level entities. Thus, all the physiological interactions of genes in a genotype enter the models of population genetics only as part of "fitness." […]The multiplicity of species interactions is grouped in the vague notions of the ecological niche, niche overlap, niche breath, and competition coefficients. It is an essential ingredient in the concept of levels of phenomena that there exists a set of what, by analogy with the sufficient statistic, we can call sufficient parameters defined on a given level (say community) which are very much fewer in number of parameters on the lower level and which among them contain most of the important information about events on that level. (Levins 1966: 428.)

Levins (1966: 429) goes on to characterize the concept in a more precise way:

> The sufficient parameters may arise from the combination of results of more limited studies. In our robust theorem on niche

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[82] I am not suggesting that the above is the *only* advantage of having robust results of dissimilar models. However, the advantage just mentioned accords with Levins' ideas of modeling in biology (see Levins 1966 and Odenbaugh 2003, 2006), which is one of the main topics of this chapter insofar as robustness analyses are concerned.

> breadth [i.e., in an uncertain environment species will evolve broad niches and tend toward polymorphism but a certain and diverse environment leads to specializations] we found that temporal variation, patchiness of the environment, productivity of the habitat, and mode of hunting could all have similar effects, and they did this by way of their contribution to the uncertainty of the environment. Thus uncertainty emerges as a sufficient parameter.
>
> The sufficient parameter is a many-to-one transformation of lower-level phenomena. Therein lies its power and utility, but also a new source of imprecision. The many-to-one nature of "uncertainty" prevents us from going backward. If either temporal variation or patchiness or low productivity leads to uncertainty, the consequences of uncertainty alone cannot tell us whether the environment is variable or patchy or unproductive. Therefore we have lost information.

William C. Wimsatt (1980a: 304-305) developed the idea from a similar angle:

> A sufficient parameter is thus an index which, either for most purposes, or merely for the purposes at hand, captures the effect of variations in the lower level variables (usually only for certain ranges of the values of these variables) and can thus be substituted for them in the attempt to build simpler models of the upper level phenomena. It is related to the notion of a supervenient property widely discussed in the recent philosophical literature… except that whereas the latter involves a deductive and therefore exact relation between lower and upper level properties, the notion of a sufficient parameter is broader, involving a relation which is inexact, approximate and usually conditional. A sufficient parameter is a heuristic tool for dealing with complexity... The notion of supervenience … may be regarded as a kind of limiting case of a sufficient parameter, but it is I would argue, a relation which is seldom is ever found in the models of the science. Sufficient parameters, however, are frequent tools of scientists.

The characterizations just outlined are still quite non-committal as to what exactly a sufficient parameter is. To begin with, the name appears to be misleading since a variable as well as a parameter can count as a sufficient parameter, as Wimsatt notices. Also, what kind of "levels" are sufficient parameters about?[83] There are many other open questions and issues insofar as the concept and its use in the context of models are concerned. For instance, why are we justified in using sufficient parameters if we lose information when we use them? What is the relation between sufficient parameters and supervenient concepts or properties? Do sufficient parameters have anything to do with robustness analyses of models?

As an example of a sufficient parameter, consider "environmental heterogeneity." Environmental heterogeneity includes different kinds of environmental factors – spatial and/or temporal – that can prevent "ecologically similar" species from outcompeting one another. In other words, different instances of environmental heterogeneity have a robust result: they make exception to Gause's rule possible. In the high latitudes, for instance, there are *temporal* or seasonal variations of limiting resources of many species and guilds: nesting sites, amount of sunlight, minerals, nutrients, and so on. Roughly speaking, if temporal variation of these resources is faster – yet not too fast – than the time it takes for competitive systems to reach their equilibria, then temporal variation of resources can prevent competitive systems from reaching their competitive equilibria in which one or more species is locally outcompeted and displaced by other, ecologically more efficient species.[84] *Spatial* environmental heterogeneity works differently.

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[83] Reference to supervenience in this context is not helpful, because this relation is used in different ways in the literature. Moreover, there are doubts about the usefulness of supervenience as a determination relation (see Horgan 1993). Furthermore, it is not constitutive determination relations that are important in the given context (as reference to supervenience might suggest), but rather causal and mechanistic relations, as I argue below.

[84] The above is, in crude terms, the explanation that Hutchinson (1953, 1959, 1961) gave to his "paradox of the plankton" referred to above.

There might be (poor-quality) habitat patches that provide "refugee camps" for an inferior competitor species, and from which they recolonize the (high-quality) patches where they are being displaced by a better competitor. If vagility or the intrinsic growth rate of the former species is sufficiently high, and/or its has a broader environmental tolerance, this can lead to the co-existence of competitor species, despite the fact that the other species is superior insofar as competition and use of resources are concerned.

The point of a sufficient parameter is not that different instances of, for instance, environmental heterogeneity are different realizations of it. They are different realizations of it, but nothing important follows from this non-causal, constitutive fact. The same is true for the idea that a sufficient parameter is a supervenient concept that has a larger scope than the lower level realizations that it covers, and that owing to this larger scope it would furnish us with higher level (lawlike) explanations of phenomena. The point is that these different realizations are realized by *different* (lower level) *causes or mechanisms*, and they are modeled by different models. It is robustness of results of dissimilar models – which describe specific alternative (lower level) causes or mechanisms – that justifies using simple and abstract models with sufficient parameters.

Some mechanisms of environmental heterogeneity work by *lowering population densities* of competing species, which then lower the intensity of competition among species; others work by *changing the competition rankings* between the species, and so on. But as a result of their robustness, those individual models that describe different causes or mechanisms of spatial or temporal environmental heterogeneity can be described by an abstract, simple model with a sufficient parameter, such as "environmental heterogeneity" in which the differences mentioned are abstracted away, and the abstraction is "justified" as a result of the sufficient parameter robustness of the results of dissimilar models. Similarly, "area" in the **species–area** rule's model might function as a sufficient parameter in the sense that there are different (hypothetical and putative) mechanisms suggested in the literature as to how "area" affects "species diversity" (see Simberloff 1974, 1976b; Connor & McCoy 1979; Williamson 1989a; Lawton 1996).

As another instance, consider Levins' (1966) example of a robust theorem (referred to above): in an uncertain environment species will evolve broad niches and tend toward polymorphism, but a certain and diverse environment leads to specialization. There are different instances of environmental uncertainty, such as the temporal variation of the environment and the patchiness of the environment, which seem to have similar effects on the niches of species. The robustness of the results of different models of environmental uncertainty allows us to use "environmental uncertainty" as a sufficient parameter in an abstract model of this phenomenon. Levins (1966) establishes the above with three models, each of which makes different, substantial assumptions about species, their fitness, genetics, and so on. Since the three dissimilar models have the common result referred to above, this "justifies" using environmental uncertainty as a sufficient parameter in an abstract or general model of this phenomenon.

Sufficient parameters are rife in the biological sciences; consider, for instance, character displacement, the carrying capacity of the environment, *K*, competition coefficients, *a*, diversity, fitness, environmental uncertainty, intermediate disturbance, the intrinsic growth rate of a population, *r*, limiting similarity, predation efficiency, resources, species packing, and so on. Sufficient parameters in models function as abstract or general causal surrogates for different (lower level) causes or mechanisms that seem to have the same or similar effects in models of certain phenomena. A model with a sufficient parameter shows how modeled causes or mechanisms can be presented in a *unifying* theoretical framework which makes abstract the differences between different (modeled) causes or mechanisms with similar results.[85] In other words, if a result is sufficient parameter robust, the job of identifying the actual cause

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[85] Although it could be claimed that unification is not what makes an explanation explanatory, unification can be seen as a serendipitous feature of causal explanations owing to the pragmatic and practical advantages that unified explanations have. Unification, for instance, facilitates economical ways of presenting explanations.

or mechanism can be explanatory irrelevant for some modeling purposes.[86]

In the special sciences the strategy to use a *diverse* set of simple, robust, and abstract models to study complex phenomena is widespread (see Levins 1966; Odenbaugh 2003, 2006; and Boyd & Richerson 2005). Ecological theories can be seen as collections of such models. Simple and abstract models of one and the same phenomenon are used in ecology with other such models of *other* phenomena to gain understanding of complex phenomena of which the individual models are caricatures. There is one set of simple and abstract models that describes the potential effects of interspecific competition; another set of models is designed to describe the effects of predation, and so on. Theories of competition, niche, diversity, and abundance of species and populations form a more or less coupled set of simple and abstract theoretical ecological models that are developed and elaborated when new models for phenomena get proposed and when the previous models and their results are studied via robustness and sensitivity analyses.

6.7 Conclusions

Recent philosophical discussion has focused on Richard Levins' ideas of scientific modeling in biology and robustness analysis that date back to the 1960s. This chapter has contributed to this discussion by providing a sense of robustness analysis, called sufficient parameter robustness, a concept that so far has been neglected in the literature.

Robustness and sensitivity analyses were argued to be powerful tools for analyzing which conditions or assumptions the results of

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[86] Nothing in the above presupposes that for genuine or true explanations the identification of the actual cause or mechanism has become redundant owing to sufficient parameter robustness of the results of models. Besides hiding information about the actual cause or mechanism of the phenomenon in a model, information about alternative causes' and mechanisms' stability and invariance conditions is something that the use of sufficient parameters hides.

models depend on. Although robustness and sensitivity analyses are non-empirical investigations of models, they could help in testing and confirming models, since they have the potential to show which stability conditions and/or modeling assumptions the results of models depend on. In this sense, robustness and sensitivity analyses allow one to move toward truer or realistic ecological models and theories.

Robustness and sensitivity analyses are called by a variety of names. For instance, some modelers call the variation in the parameter values of a model robustness analysis rather than sensitivity analysis as I do. The point of this chapter was not how these terms are used in the sciences. Rather, the point was to develop definitions of non-empirical investigations of the models that I called robustness and sensitivity analyses and to facilitate a better understanding of these concepts. Providing better definitions and understanding of robustness and sensitivity analyses is important, as previous authors have concentrated on a narrow sense of robustness analysis, which they have inadequately distinguished from other non-empirical investigations of models, such as sensitivity analysis.

SUMMARY

- In chapter 1, I suggested that two important issues *prior* to the ecological laws debate have been treated carelessly: the definitions of laws and the justifications of them. The covering law account suggested an important justification for the ecological laws debate, namely, that the proprietary function of laws is to furnish us with scientific explanations. Thus, there is one (historically) influential justification for the laws debate, which is also strong enough, given the apparent paucity of ecological laws, to justify such a debate. This justification has practical relevance for ecologists as well.
- In chapter 2, I analyzed nine arguments for the absence of biological laws based on the alleged distinctive features of biological generalizations and phenomena. I claimed that the evolutionary contingency thesis by John Beatty (1995) presents a serious threat to the existence of biological laws, whereas other arguments were argued not to present serious problems for the lawlike status of biological generalizations.
- In chapter 3, I criticized several (non-traditional) accounts of laws as being insufficient to redeem the lawlike status of (strongly and weakly contingent) biological generalizations. I also suggested that ecological generalizations are *distinctively* ecological and autonomous insofar as their explanations of exceptions are concerned.
- In chapter 4, I presented and defended an account of scientific explanation in which generalizations are explanatory if they are *invariant*. This interventionist account of causal scientific explanation showed that reference to lawlikeness is not a *necessary* condition for scientific explanations. It also showed why the debate about ecological laws is a red herring, namely, because there is no justification for this debate in ecology. In other words, there are invariant ecological generalizations that are capable of functioning in the role of laws despite the fact that they need not be lawlike as generalizations.
- In chapter 5, I discussed the different functions that stability and scope have in the context of "generalizing" scientific explanations. I argued that stability is an important property of generalizations

insofar as their *extrapolability* and *reliability* are concerned. I argued that scope has a different, yet related function: it provides unification and systematization of (causal) knowledge. I also suggested that stability is not a monolithic property of generalizations, but that there are different, yet related concepts or meanings for stability. This was used as the basis for criticizing the idea that stability should be identified with lawlikeness.

- In chapter 6, I discussed non-empirical investigations of models that I called robustness and sensitivity analyses. I also suggested a new taxonomy of robustness and criticized some previous taxonomies of robustness vis-à-vis my taxonomy of robustness and sensitivity analyses. I showed that robustness and sensitivity analyses are powerful tools for studying where models break down. They are especially powerful in pointing out reasons for this breakdown, because they show on which stability conditions or modeling assumptions the results of models depend.

APPENDIX: BIOLOGICAL GENERALIZATIONS AND MODELS

Below, I have assembled a list of paleobiological, evolutionary, and ecological generalizations and models. The list is not representative of the biological disciplines mentioned. Yet, as a sample of what *philosophers* have taken to be examples of biological (lawlike) generalizations and models, it is a representative list. For the generalizations given here, I have used the word "rule" in the names to avoid the confusing terminological plurality that prevails in the literature. For instance, Gause's rule is called by a variety of names, such as Gause's (or Grinnell's) axiom, law, principle, and so on. The curves and graphs presented here are not based on any data; they are presented for illustrative purposes only. I have listed these generalizations and models in alphabetical order according to the parts of their names in bold.

The *intraspecific* pattern of **abundance** and distribution: abundance is highest at the center of each species' range and declines gradually and usually symmetrically toward the boundaries. Biologists: Brown (1995: 50-61)

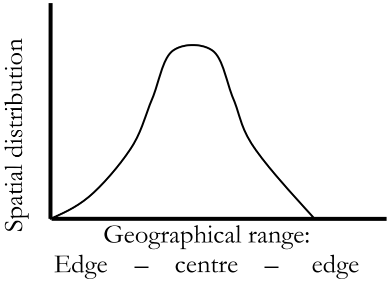

Fig. 2. The intraspecific pattern of abundance and distribution is a pattern of variation in the abundance of species across their geographic range within a latitudinal gradient.

The *interspecific* pattern of **abundance** and distribution: the abundant species tend to be widely distributed, while the rare species tend to have restricted ranges. Biologists: Brown (1995: 61-71); Lawton (1999: 188)

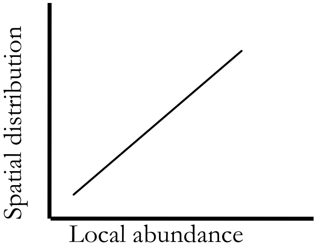

Fig. 3. The interspecific pattern of abundance and distribution is a pattern of a positive correlation between local abundance and the spatial distribution of species.

Allen's rule is a geographical or latitudinal gradient in body parts according to which the extremities (e.g., feet, tail, ears, bill, and other protruding body parts) of the members of a species of endothermic animals tend to be smaller in colder regions or at higher latitudes than members of the same species in warmer regions or at lower latitudes. See **Bergmann's** rule. Philosophers & biologists: Dale (1940); Mayr (1942: 90, 1956); Snow (1954); Scholander (1955, 1956); Irving (1959); Rensch (1959: 43, 1960: 109); Ray (1960); Barth (1966); Weaver & Ingram (1969); Peters (1991: 190); Beatty (1995: 57)

The **area** rule of the equilibrium theory of (see also) **island biogeography**: 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). The area rule should not be confused with the **species–area** rule. Philosophers & biologists: MacArthur & Wilson (1963, 1967: 19-67); Simberloff (1976b); Williamson (1989b)

Bergmann's rule is a geographical or latitudinal gradient in body size, according which the members of a species of endothermic animals are larger in their body size in colder regions or at higher latitudes than members of the same species in warmer regions or at lower latitudes. The traditional explanation for Bergmann's rule is that the increase of body size is due to selection for heat or energy conservation in cold climates. This follows from the reduction in "surface area to mass ratio" in animals that have larger body size, which is hypothesized to reduce their heat dissipation. A similar kind of reasoning applies also to **Allen's** rule. There are other versions of Bergmann's rule. For instance, some versions deal with variation in body size among species and/or in ectotherms. Philosophers: Ruse (1973: 59-61); Rosenberg (1985: 207-208, 215-216, 1987: 196); Giere (1988: 42-43); Ghiselin (1989: 61); Beatty (1995: 57-59); Carrier (1995: 87-88, 90). Biologists: Davis (1938); Dale (1940); Mayr (1942: 90, 1956); Newell (1949); Snow (1954); Scholander (1955, 1956); Irving (1959); Rensch (1959: 39-41, 43-46, 1960: 109); Ray (1960); Hagmeier & Stults (1964); Barth (1966); Lindsey (1966); Herreid & Kessel (1967); Bonner (1968); Rosenzweig (1968); Brown & Lee (1969); Kendeigh (1969); Weaver & Ingram (1969); James (1970); McNab (1971); Boyce (1979); Coleman (1979); Lindstedt & Boyce (1985); Ralls & Harvey (1985); Langvatn & Albon (1986); Geist (1987,1988); Paterson (1988); Dayan *et al.* (1991); Peters (1991: 190); Rhymer (1992); Steudel *et al.* (1994); Smith *et al.* (1995); Blackburn & Gaston (1996); Van Voorhies (1996, 1997); Mousseau (1997); Partridge & Coyne (1997); Blackburn *et al.* (1999); Gaston & Loder (1999); Ashton *et al.* (2000); Ashton (2001); Blackburn & Ruggiero (2001); Freckleton *et al.* (2003); Murray (2003)

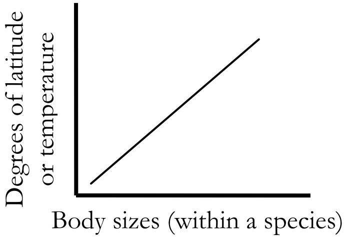

Fig. 4. Bergmann's rule is a biogeographic pattern of variation in body size.

The **biogenetic** rule (also known as the rule of recapitulation and von Baer's rule): ontogeny shows recapitulation of phylogeny. Philosophers & biologists: Rensch (1960: 96, 103); Ghiselin (1989); Beatty (1995: 67); Sterelny & Griffiths (1999: 365, 368)

The **canonical** distribution of abundances of species (also known as the approximately lognormal distribution of abundances of coexisting species, the canonical distribution of commonness and rarity, the distribution of abundance among species, and Preston's lognormal distribution): there are more moderately rare species than moderately common ones. In other words, ecological communities contain many relatively rare species and only a few very abundant ones. Biologists: Preston (1962a); Peters (1991: 19); Brown (1995: 88-89); Lawton (1999: 183, 187)

The **Caspar–Klug** theory is a hypothesis concerning the structure and organization of capsids in (simple) viruses. Philosophers: Morgan (2009)

The **central** dogma of molecular genetics: DNA is transcribed to RNA; and RNA is translated to proteins. Philosophers: Rosenberg (1985: 30, 2001b: 141-142, 2001c: 740); Darden (1995: 145-154, 1996: 410, 2002); Beatty (1997: S438)

Character displacement: an evolutionary phenomenon whereby interspecific competition causes species to become different (ecologically, morphologically, behaviorally, and so on) in regions where their ranges overlap (that is, where these are sympatric) and where they compete as compared to regions where they are not sympatric. See the theory of **limiting** similarity. Philosophers & biologists: Diver (1940); Brown & Wilson (1956); Schoener (1965); Grant (1966, 1968); Rosenzweig (1966); MacArthur & Levins (1967); Ashmole (1968); Holmes & Pitelka (1968); Schoener & Gorman (1968); Grant (1972); Pianka (1972); Lawlor & Maynard Smith (1976); Connell (1980); Hendrickson (1981); Strong & Simberloff (1981); Ralls & Harvey (1985); Cooper (1993: 364, 1998: 560-562, 580)

The **clutch** size rule (also known as the egg rule) is a geographical or latitudinal gradient in clutch size in birds, according to which the average clutch size of the members of a species increases with an increase in latitude. See also the **litter** size rule. Biologists: Mayr (1942); Lack (1947, 1948a, 1948b); Skutch (1949); Rensch (1959: 40, 43, 1960: 109-110); Ray (1960: 85); Cody (1966); Haartman (1971); Owen (1977); Boyce (1979); Brown (1995: 130); Murray (2001: 276-290)

Cope's rule (also known as Cope's first rule): there is a general evolutionary trend toward larger body size in many taxa. According to the traditional explanations of this rule, there are adaptive advantages conferred by a large body size. Philosophers: Ruse (1973: 158-159); Hull (1974: 81-82, 100, 1975: 265). Biologists: Mayr (1942: 293-294); Newell (1949); Rensch (1959: 206, 1960: 107-108); Gould (1966, 1970, 1980, 1988, 1997); Bonner (1968); Stanley (1973); Van Valen (1973a); Southwood *et al.* (1974); Dawkins & Krebs (1979: 502-503); Peters (1983: 184-196, 1991: 152-153); Brown & Maurer (1986); MacFadden (1986); McKinney (1990); Maurer *et al.* (1992); Damuth (1993); Arnold *et al.* (1995); Jablonski (1997); Alroy (1998); Ashton (2001); Knouft & Page (2003); Van Valkenburgh *et al.* (2004)

The **distance** rule of the equilibrium theory of (see also) **island biogeography**: 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(s) of an island is increased (or decreased), its immigration rate is reduced (or increased). Philosophers & biologists: MacArthur & Wilson (1963, 1967: 19-67); Cook (1974); Lloyd (1987); Castle (2001b: 556-569); Lange (2002: 419, 2005); Shrader-Frechette (2001: 514-515); Mikkelson (2001)

The **diversity–stability** rule (also known as the complexity–stability rule): increased (alpha, beta, and/or gamma) diversity enhances (population, community, and/or ecosystem) stability. Philosophers: McCoy & Shrader-Frechette (1992: 187-188); Shrader-Frechette & McCoy (1993: 3-5, 38-41, 48-51, 110, 175, 187, 188); Mikkelson (1997, 2004); Odenbaugh (2001, 2005); Sterelny (2006: 220-224); Justus (2008: 429-430). Biologists: MacArthur (1955); Hutchinson (1959); Connell & Orians (1964); Paine (1966); May (1975); Orians (1975); Connell (1978); Rabenold (1979); Simberloff (1980: 22); Pimm (1984); Peters (1991: 96-97, 161, 288); Lehman & Tilman (2000)

Dollo's rule (also known as the rule of irreversibility of evolution and the rule of phylogenetic irreversibility) is the idea that evolution cannot be reversed. Philosophers: Ruse (1973: 59, 61); Hull (1974: 81-82, 100); Rosenberg (1985: 207-208, 215-216). Biologists: Gregory (1936); Muller (1939); Mayr (1942: 295); Rensch (1959: 123-126); Gould (1970, 1980: 100-101); Ghiselin (1989); Peters (1991: 155); Lee & Shine (1998)

The **endemicity** rule of the equilibrium theory of (see also) **island biogeography**: the percentage of endemic species – that is, species that are indigenous only to a specific area – increases with the size of the island, which apparently follows from the **area** rule. Biologists: MacArthur & Wilson (1967: 173-174)

Individuals total **energy consumption** varies as about the three-fourths power of body size W, $aW^{0.75}$. Biologists: Van Valen (1973a); Brown (1995: 83); Marquet *et al.* (2005: 1752-1753)

The **energy equivalence** rule (also known as EER): energy use is unaffected by body size, that is, the energy use of all species (populations) in each size class tends to be equal, scales as aW^0. In other words, the energy used by different species is roughly equal and independent of their body masses. It has been suggested that since species' basal metabolic rate (cf. **Kleiber's** rule) and local density (cf. the **inverse** scaling rule) vary in directions opposite to body size, then it would follow that the energy used by the local population of a species could be independent of its body size. Biologists: Damuth (1981, 1991); Peters (1983: 164-183); Peters & Raelson (1984); Russo *et al.* (1993); Brown (1995: 95-99); Marquet (2000); Marquet *et al.* (2005)

According to the **exponential** population growth model, population growth is density *in*dependent, and it can be described by the equation

$$N_t = N_0 e^{rt},$$

where N_t is the population size at time t, N_0 is the initial size of the population, and r is the growth rate of the population, called the intrinsic rate of increase. There is a differential equation version of this model, which is not presented here. Philosophers & biologists: Smith (1952); Mikkelson (1997: 492); Turchin (2001: 18-19); Berryman (2003); Owen-Smith (2005)

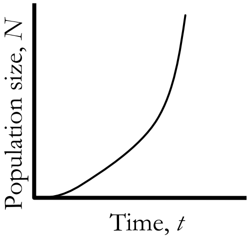

Fig. 5. Exponential growth model and the classical curve of the model.

Fasting endurance scales as $aW^{0.44}$ in mammals and between $aW^{0.40}$ and $aW^{0.60}$ in birds, where W is body size of a taxon. Biologists: Peters (1983: 41-34); Lindstedt & Boyce (1985)

The **founder** rule: a biased sample of a parent species' gene pool can lead to rapid changes in allele frequencies in a colonizing population and consequently to genotypic and phenotypic divergence from the parent population. The founder rule is supposed to be the mechanism of allopatric speciation; see **Mayr's** rule. Philosophers & biologists: Ruse (1973: 56-56, 1977: 645-646, 1988: 35); Hull (1974: 62-63); Sober (1984: 112); Rosenberg (1985: 149, 216); Ghiselin (1989: 57); Brandon (1990: 41); Plutynski (2001: 231)

The body size **frequency** distribution of a taxon (also known as the frequency distribution of body sizes among the species): species number declines with body size. In other words, there are more species that have small body sizes than species that have large body sizes. Biologists: Hutchinson & MacArthur (1959); Van Valen (1973a); McKinney (1990: 88-89); Maurer *et al.* (1992); Brown (1995: 77-88); Lawton (1999: 187); Knouft & Page (2003)

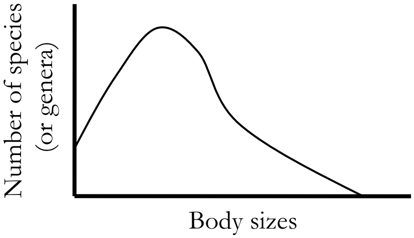

Fig. 6. The frequency distribution of body sizes among the species is positively skewed.

According to **Gause's** rule (also known as the competitive exclusion rule and Grinnell's rule), species that have similar, let alone identical, niches cannot coexist at the same place for long periods of time. In equilibrium, the number of species competing for limited set of resources should not exceed the number of resources in that set; in other words, in equilibrium *n* sympatric species cannot coexist indefinitely on fewer than *n* resources. The magnitude or intensity of competition between ecologically similar species is thought to be proportional to the degree of overlap in their niches or use of resources. Species that co-exist do so because there are "adequate" differences in their niches or use of resources (see the theory of **limiting** similarity and **Lotka–Volterra** models). Philosophers: Mertz & McCauley (1980); Thompson (1989: 292); Cooper (1990, 1993: 364, 1998: 560); Kincaid (1990: 77); Shrader-Frechette (1990a); Steen & Kamminga (1991: 450); Sterelny & Griffiths (1999: 256, 266); Weber (1999); Sterelny (2001: 449). Biologists: Beauchamp & Ullyott (1932); Gause (1934a, 1934b, 1936: 329, 1937); Lack (1945, 1946); Crombie (1947); Park (1948, 1954, 1957, 1962); Park & Frank (1950); Birch *et al.* (1951); Gilbert *et al.* (1952); Hutchinson (1953, 1959, 1961); Utida (1953); Neyman *et al.* (1956); Ross (1957, 1958); Savage (1958); Udvardy (1959); Cole (1960); Hardin (1960); Savile (1960); Connell (1961, 1983); Slobodkin (1964); MacArthur & Levins (1964); Heatwole & Davis (1965); Paine (1966); Birch & Ehrlich (1967); Leslie *et al.* (1968); McCown & Williams (1968); Taylor (1968); Vandermeer (1969); Paine & Vadas (1969); Park *et al.* (1970); Levin (1970, 1976); Dayton (1971); Jaeger (1971); Ayala (1972); Pianka (1972); Strobeck (1973); Koch (1974a, 1974b); Schoener (1974, 1989); Slatkin (1974); Means (1975); Menge & Sutherland (1976); Kaplan & Yorke (1977); Wiens (1977); Levins (1979); Armstong & McGehee (1980); Pielou (1981: 20-22); Simberloff (1982); Haila (1982); Hanski (1983); Shorrocks *et al.* (1984); Chesson (1985); Chesson & Case (1986); Durán & Castilla (1989); Peters (1991: 97); Namba & Takahashi (1993); Huisman & Weissing (2001); Vandermeer *et al.* (2002); Edmunds *et al.* (2003)

Gloger's rule: a color variation within an animal species that seems to follow differences in temperature and/or humidity of their climate. Biologists: Dobzhansky (1933); Dale (1940); Mayr (1942: 88-90); Snow (1954); Cowles (1958, 1959); Rensch (1959: 40, 43; 1960: 109)

The **Hardy–Weinberg** rule describes what will happen to gene and genotype frequencies in a population unaffected by evolutionary forces according to which both gene frequencies and genotype frequencies will remain constant in successive generations. Philosophers & biologists: Ruse (1970: 239-243, 1973: 33-38, 44-45, 1988: 18-19); Hull (1974: 57-58, 66); Ayala (1978: 135); Mills & Beatty (1979: 49-51); Sober (1980: 360, 381, 1984: 34-42, 1993: 71-72); Beatty (1981, 1995: 51-56); Rosenberg (1985: 132-133, 176); Ereshefsky (1991: 72-73); Cooper (1993: 372); Carrier (1995: 87-90); Brandon (1997: S454); Murray (2000: 405, 2001: 273, 278); Woodward (2001: 7-8, 11-13, 2003: 303-307); Elgin & Sober (2002: 443-444); Elgin (2003); Brandon (2006: 322-331); Hamilton (2007: 594)

The **hollow** curve (also known as the distribution of range sizes among species): there is a right-skewed species range size distribution, that is, most species have moderate to small range sizes and only a few have large range sizes. The curve was originally called hollow because its shape on linear axes is "hollow." Biologists: Brown (1995: 102-108); Gaston (1996a); Hecnar (1999); Lawton (1999: 187)

The size of the **home** range (of herbivorous mammals) varies positively with body size, aW^1, where W is body size. Biologists: Van Valen (1973a); Peters (1983: 164-183); Brown (1995: 122); Marquet *et al.* (2005)

Hutchinson's rule (also known as the 1.3 rule): a (hypothesized) constant of trophic or feeding structures between competing species. It has been suggested that a constant difference of approximately 1.3 in linear dimensions in trophic or feeding structures is sufficient to the permit coexistence of species. See also **limiting** similarity. Biologists: Hutchinson (1959); Klopfer & MacArthur (1961); Schoener (1965); Grant (1968); Holmes & Pitelka (1968); Schoener & Gorman (1968); Fenchel (1975); Pulliam (1975); Wilson (1975); Horn & May (1977); Maiorana (1978); Pearson & Mury (1979); Strong *et al.* (1979); Strong (1980); Simberloff & Boecklen (1981); Schoener (1989); Peters (1991: 217, 226)

According to the **intermediate** disturbance rule, intermediate levels of abiotic or biotic disturbances are capable of mediating the coexistence of competitor species and thus maintaining the local diversity of a community at a relatively high level. Too small or infrequent disturbances lead to local extinctions of competitively inferior species by competitively dominant ones, whereas too intense or too frequent disturbances allow for the few species that are the most stress-tolerant to exclude other species from a habitat. See also **Gause's** rule and **Lotka–Volterra**. Philosophers & biologists: Paine (1966); Paine & Vadas (1969); Dayton (1971); Connell (1978); Durán & Castilla (1989); Cooper (1998: 567-568, 578); Sterelny & Griffiths (1999: 267-268); Sterelny (2006: 217)

The **inverse** scaling rule (also known the distribution of population density as a function of body mass and size-abundance/density rule): the maximum density, D, of herbivorous mammals declines as their body size increases, $D = aW^{-0.75}$, where W is body size. In other words, small organisms typically attain greater population densities than larger ones. Biologists: Damuth (1981, 1991, 1993); Peters (1983: 164-183, 1991: 19-20, 26, 27, 29, 31- 33, 142, 190, 275); Peters & Raelson (1984); Juanes (1986); Marquet *et al.* (1990); Russo *et al.* (1993); Bohlin *et al.* (1994); Brown (1995: 91-95); Blackburn & Gaston (1997); Marquet (2000); Marquet *et al.* (2005)

The **island** rule (also known as Foster's rule): there are two versions of this rule. The *taxonomic* version claims that there is a tendency toward gigantism in insular rodents and marsupials, while there is a tendency for dwarfism in insular carnivores, lagomorphs, and artiodactyls. Insectivores, however, do not exhibit any clear pattern of insular body size. The *ataxonomic* version of the rule states that there is a trend toward gigantism in the smaller species and dwarfism in the larger species. Biologists: Foster (1964); Bonner (1968: 4-5); Wassersug *et al.* (1979); Angerbjorn (1985); Lomolino (1985); Maurer *et al.* (1992); Damuth (1993); Brown (1995)

The equilibrium theory of **island biogeography** is a model of insular biogeography. It represents the equilibrium number of species, $\hat{S}$, inhabiting an island at an intersection point between extinction (*E*) and immigration (*I*) curves of that island. The curves are hypothesized to be functions of the size of the island (extinction) and its distance from a source of dispersing species (immigration). See also the **area**, **distance,** and **endemicity** rules. Philosophers: Cooper (1990); Shrader-Frechette (1990b, 2001: 514-515); Shrader-Frechette & McCoy (1993: 69-79, 86-92, 99); Mikkelson (1997: 492); Sterelny & Griffiths (1999: 259-266); Sismondo (2000); Castle (2001a). Biologists: MacArthur & Wilson (1963, 1967); Hagmeier & Stults (1964); Simberloff (1969, 1974, 1976a, 1976b, 1976c); Brown (1971); Simberloff & Wilson (1969, 1970); Heatwole & Levins (1972); Cook (1974); Brown & Kodric-Brown (1977); Gilbert (1980); Peters (1991: 205-206); Williamson (1989a, 1989b); Haila (1990); Lawton (1999: 188)

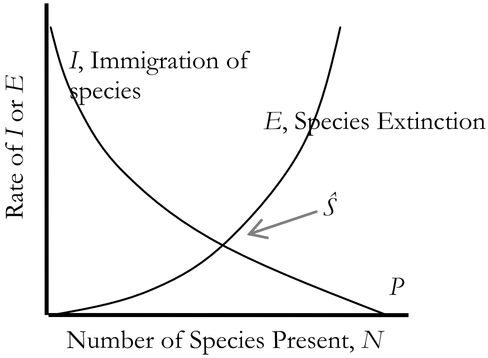

Fig. 7. An equilibrium model of an island. The equilibrium number of species, $\hat{S}$, is reached at the intersection point between the curve of the immigration of the new species and the curve of the extinction of the species from the island. P is the number of species in the "species pool." The point 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 is not constant.

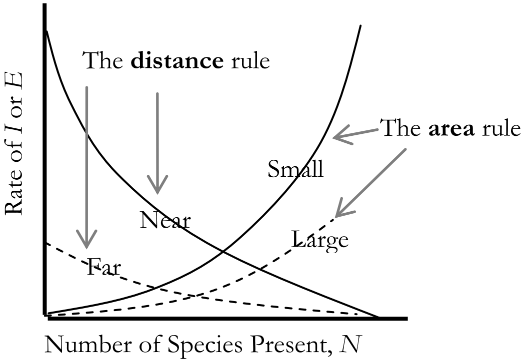

Fig. 8. An equilibrium model of islands at *varying distances* from the source area of immigrant species and of *varying size*. An increase in distance (near to far) lowers the immigration curve, I, while an increase in island size (small to large) lowers the extinction curve, E. Manipulation of "size" or "distance" of an island thus has an effect on what the equilibrium number of the species, $\hat{S}$, of that island will be.

Kleiber's rule: basal metabolism, an estimate of the energy required by an individual for the basic processes of living, varies as $aW^{0.75}$, where W is its body size. Philosophers & biologists: Gould (1966: 613); Van Valen (1973a); Peters (1983: 24-46); Mitchell (2000: 242); Colyvan & Ginzburg (2003: 651); Marquet *et al.* (2005); Elgin (2006); Hamilton (2007: 604)

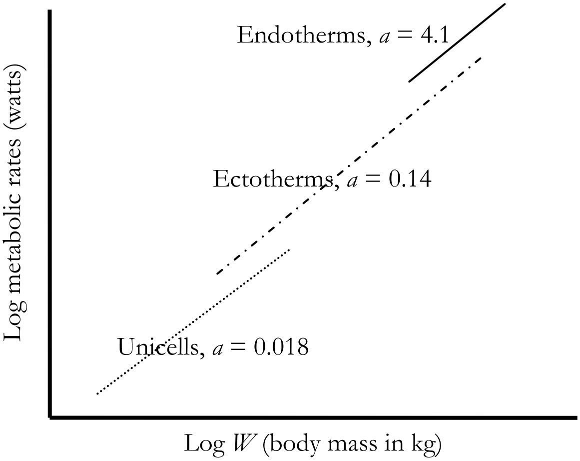

Fig. 9. Kleiber's rule: in organisms the basal metabolic rate is proportional to three-fourths the power of body size or weight. Note that although all three different major "metabolic groups" depicted here, endotherms, ectotherms and unicells, generally and approximately obey the rule, there are differences between them in the "elevation" of the allometric equation, that is, in the value of a constant a. These differences might be ecologically and evolutionarily important.

The **latitudinal** diversity gradient: a geographical or latitudinal gradient in species richness or diversity, according to which the number of species within a taxonomic group tends to increase with decreasing latitudes, i.e., diversity increases towards the equator and decreases towards the poles. Philosophers: Sterelny & Griffiths (1999: 272-273); Lange (2005). Biologists: Dobzhansky (1950); Fischer (1960); Klopfer & MacArthur (1960, 1961); Rensch (1960: 108-109);

Connell & Orias (1964); Hagmeier & Stults (1964); MacArthur (1964); Paine (1966); Pianka (1966); Menge & Sutherland (1976); Parsons & Bock (1979); Rabenold (1979); Stevens (1989); Rohde (1992, 1996, 1997, 1998); Blackburn & Gaston (1996); Gaston (1996b); Rosenzweig & Sandlin (1997); Santelices & Marquet (1998); Lawton (1999: 183, 188); Ashton (2001); Willig & Bloch (2006)

(von) **Liebig's** rule of the minimum: in plants and in some other groups, the growth of an individual or a population is controlled by the resource that is the scarcest, not by all of the available ones. Philosophers & biologists: Shrader-Frechette (1994: 34); Berryman (2003)

Under the umbrella of the theory of **limiting** similarity there are different but related ideas, such as **Hutchinson's** rule and **character** displacement. These ideas are related to one another via the notion that sufficient dissimilarities between competing species in niches or resource use could allow and permit for their coexistence; see **Gause's** rule.

The **litter** size rule: a geographical or latitudinal gradient in litter size in the members of mammal species similar to the **clutch** size rule. Biologists: Mayr (1942); Lack (1948b); Lord (1960); Cody (1966)

According to the **logistic** population growth model (also known as the density-dependent population growth model and the Verhulst(–Pearl) model), population growth rate is density dependent, that is, growth rate declines with population numbers, N, and reaches zero when the population numbers are at its environment's carrying capacity, K. The logistic growth model models *intraspecific* competition. The difference equation of the logistic population growth model has the form

$$N_{t+1} = N_t + N_t r(1-(N_t/ K)),$$

where K represents the upper limit of the number of individuals of a population that an environment can support and r is the growth rate of the population, also called the intrinsic rate of increase. If population numbers, N, exceed K, then the growth rate of the population becomes negative. The differential equation version of this model is not presented here. Philosophers: Wimsatt (1980a); Cooper (1993: 361-362); Shrader-Frechette (1994); Mikkelson (1997: 492); Weisberg (2006a: 638). Biologists: Gause (1934b); Smith (1952); May (1972: 645, 1976); Pielou (1981: 25-25); Hall (1988); Peters (1991: 52, 54-56); Murray (2000: 405); Turchin (2001: 20-21); Berryman (2003); Owen-Smith (2005)

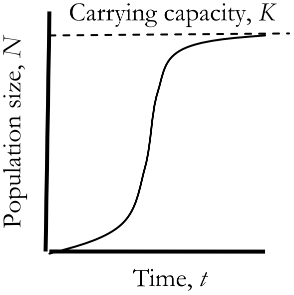

Fig. 10. The classical sigmoid or S-shaped curve of the logistic growth model

In **Lotka–Volterra** models, one have coupled differential equations. Different Lotka–Volterra equations are used to describe interactions between species belonging to *different* trophic levels (the predator—prey interaction model) and interactions between species belonging to the *same* trophic level (the interspecific competition model). The Lotka–Volterra **interspecific** competition model's equations are the following. The equation for the population growth of competitor species 1 is

$$dN_1/ dt = r_1N_1 ((K_1 - (N_1 - \alpha_{12}N_2))/ K_1).$$

The equation for the population growth of competitor species 2 is

$$dN_2/ dt = r_2N_2 ((K_2 - (N_2 - \alpha_{21}N_1))/ K_2).$$

In the equations, N_1 is the number of individuals of species 1 at time *t*, N_2 is the number of individuals of species 2 at time *t*, K_1 is the carrying capacity of species 1, K_2 is the carrying capacity of species 2,

r_1 is the intrinsic growth rate of species 1 (in absence of competition), r_2 is the intrinsic growth rate of species 2 (in absence of competition), and α's are competition coefficients, where α_{12} is the negative effect of species 1 on species 2 and α_{21} is the negative effect of species 2 on species 1.

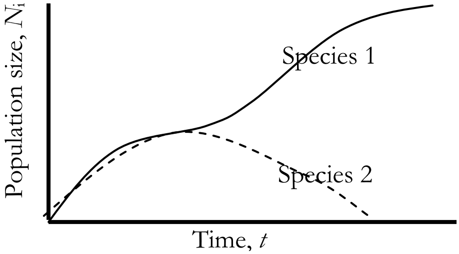

Fig. 11. The Lotka-Volterra interspecific competition model between two species. Species 2 is being locally outcompeted or excluded by species 1, for example, because species 1 out-harvested their common resource.

The classical Lotka–Volterra **prey–predator** model's equations are the following. Prey's growth equation is

$$dN_1/ dt = rN_1 - bN_1N_2$$

Predator's growth equation is

$$dN_2/ dt = ebN_1N_2 - cN_2$$

In the equations, r is the intrinsic growth rate of prey (in the absence of predation), c is the intrinsic death rate of predator (in the absence of their prey), b is the predation rate coefficient, e is predation efficiency, N_1 is the population size of prey at time t, and N_2 is the population size of predators at time t. These equations describe the dynamics in which populations of both prey and predators exhibit periodic oscillations.

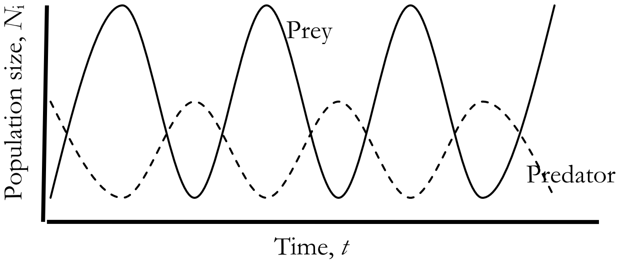

Fig. 12. The Lotka-Volterra predator-prey model: classical curves between predator and prey species whose cyclic populations oscillate in tandem

Philosophers: Rosenberg (1985: 214); Shrader-Frechette (1990a); Cooper (1993: 361-364); Sober (1993: 76-77); Carrier (1995); Mikkelson (1997: 492); Cuddington (2001); Odenbaugh (2005: 240-245); Weisberg (2006a: 638, 2006b); Justus (2008: 427-428); Weisberg & Reisman (2008). Biologists: Gause (1934a, 1934b); Smith (1952); Philip (1955); Strobeck (1973); May (1973: 646-647); Smale (1976); Peters (1976, 1991: 52, 164); Armstrong & McGehee (1980); Simberloff (1980: 18-19, 23-24); Pielou (1981: 18-22); Hall (1988); Namba & Takahashi (1993); Levins (1998: 575-576); Turchin (2001: 21-22); Owen-Smith (2005)

Mayr's rule (also known as allopatric speciation): new species evolve when a population is separated by geographic isolation from its parent population versus polyploidy and sympatric speciation. See the **founder** rule. Philosophers and biologists: Hull (1974: 50-51, 95); Ruse (1977: 645-646); Ghiselin (1987a: 129, 1989: 58); Sober (1993: 147); Beatty (1995: 67, 1997: S435-S436); Weber (1999: 88); Hamilton (2007: 594)

Mendel's first rule (also known as the rule of segregation): in sexually reproducing organisms the members of a pair of alleles of every gene segregate into different gametes during meiosis. Meiosis is the process by which diploid parents produce haploid gametes. Well-known exceptions to this rule are meiotic drive and non-disjunction. Philosophers & biologists: Sandler & Novitski (1957); Smart (1963); Ruse (1970: 243, 1973: 13-14, 24-26, 29-31); Darden & Maull (1977: 47 , 51-54); Ayala (1978: 134); Crow (1979); Beatty (1980: 555, 1981: 403-409, 1982: 209-213, 1995: 50-51, 54-56, 59-62); Rosenberg (1985: 31, 93, 108-109, 132-136, 212, 1997: S435, S440); Thompson (1989: 290); Ereshefsky (1991: 65-67); Steen & Kamminga (1991); Sober (1993: 107-108, 1997); Carrier (1995: 83-84); Mitchell (1997, 2000: 242, 260, 2002: 331); Waters (1998: 5); Sterelny & Griffiths (1999: 115, 117, 121-124, 365-366); Woodward (2000: 232, 2001: 7-8, 11-13, 2003a: 275-276, 303-307)

Mendel's second rule (also known as the rule of independent assortment): genes on different chromosomes assort independently of one another. In other words, non-allelic genes assort independently of one another in the offspring. A well-know exception to this rule is linkage. In linkage, different genes are located *on the same* chromosome, whereas Mendel's second rule applies only to genes at the same locus, i.e., to genes that are on *different* homologous chromosomes. Philosophers & biologists: Ruse (1970: 243, 1973: 13-14, 24-26, 29-31); Darden & Maull (1977: 47, 51 53-54); Rosenberg (1985: 31, 93, 108-109, 132-136, 212); Thompson (1989: 290); Darden (1995); Glennan (1996: 62); Beatty (1997: S435); Sterelny & Griffiths (1999: 115, 117, 121-124, 365-366); Waters (1998); Murray (2001); Hamilton (2007: 599)

The **miniaturization** rule: evolution of extremely small adult body sizes in some taxa. Biologists: Wassersug *et al.* (1979); Hanken & Wake (1993)

The rule of **natural** selection: "the survival and/or reproduction of the fittest." The problem with this formulation of the rule is related to how we define fitness in a way that does not turn the generalization into a circular or meaningless truism. There are many replies to this tautology charge of the rule. Philosophers & biologists: Scriven (1959); Smart (1963: 59); Williams (1970: 362, 1973a, 1973b);

Lewontin (1972); Ruse (1973: 38-41, 1977: 646-649, 1988: 19-21, 1989); Hull (1974: 65-69); Popper (1974: 137); Ferguson (1976); Peters (1976, 1978, 1991: 60-73); Caplan (1977); Castrodeza (1977); Stebbing (1977); Brandon (1978, 1990: 9-11, 140, 158, 1997: S454); Mills & Beatty (1979); Beatty (1980: 555, 1981); Reed (1981); Rosenberg (1982, 1985: 126-129, 154-169, 211, 212-216, 219-225, 239-243, 1987, 2001b); Byerly (1983); Sober (1984: 61-85, 1987: 222-223); Cooper (1988); Shrader-Frechette (1990a); Carrier (1995: 93-96); Lawton (1999: 178); Murray (2000: 403-405, 2001: 274-278); Rosenberg & Kaplan (2005)

The relation between **range size** and **body size**: there are few species of large (and possibly of very small) size with small geographical ranges. In other words, large (and possibly very small) sized species' geographical ranges tend to be large. Biologists: Brown (1995: 105-108)

Rapoport's rule: a latitudinal gradient of decreasing size of species ranges with decreasing latitude. In other words, the tendency is for latitudinal ranges of species to become smaller with decreasing latitude. It is hypothesized that species at high latitudes (and/or at cold climates) have adapted to more varied environmental conditions, which makes it possible for them to occupy wider ranges. Biologists: Pianka (1989); Stevens (1989, 1992); Brown (1995: 112-116); Blackburn & Gaston (1996); Rohde (1996); Gaston *et al.* (1998); Santelices & Marquet (1998); Hecnar (1999); Ashton (2001)

The **Red** Queen rule: the idea that the evolutionary progress of species (or taxa) is relative and temporary, owing to the fact that their biotic and abiotic environments are constantly changing and deteriorating. Examples of this rule are the arms race between species. Evidence for this rule is supposed to come from the pattern of Van **Valen's** rule of extinction. The rule tries to establish that evolution by natural selection is not an "improver," but that organism and species are only capable of maintaining their current states of adaptation and fitness by natural selection, which in turn implies that their extinction rates should be constant. Philosophers: Dawkins &

Krebs (1979); Sober (1984: 174); Wimsatt (1994: 254); Beatty (1995: 67); Sterelny (2001: 446, 450). Biologists: Van Valen (1973b); Raup (1975); Lewontin (1978: 159-160);

The relation between **specialization** and diversity: animals become more specialized as they become more diverse. Biologists: Simpson (1953); Cisne (1974)

The **species–area** rule (also known as the area–diversity rule): the number of species on an island or a habitat varies with the area of that island or habitat, where this relation can be presented as a power equation,

$$S=cA^z,$$

in which S is the number of species of a given taxonomic group, A is the area of the island or habitat, and c and z are (fitted) constants. The parameter z has some "canonical" values in the range 0.15 and 0.35. The value of z depends on several factors. The constant c is said to represent, among other things, the biotic richness of the archipelago in question or some larger region; see the **latitudinal** diversity gradient. The curve of this rule is often given in a log-log plot, so that z can be approximated as the slope of the curve. Philosophers: Cooper (1990, 1998); Shrader-Frechette (1990b, 2001: 514-515); Shrader-Frechette & McCoy (1993: 70, 72-73, 75, 89-92, 100, 285-287); Castle (2001a, 2001b: 556-569); Mikkelson (2001); Lange (2005a). Biologists: Preston (1962a, 1962b); MacArthur & Wilson (1967: 8-18); Brown (1971); Cook (1974); Simberloff (1974, 1976b); Diamond (1975); Connor & McCoy (1979); Gilbert (1980); Williamson (1989a, 1989b); Peters (1991: 188-189, 289, 294); Lawton (1996, 1999: 178, 188-189); Pounds & Puschendorf (2004)

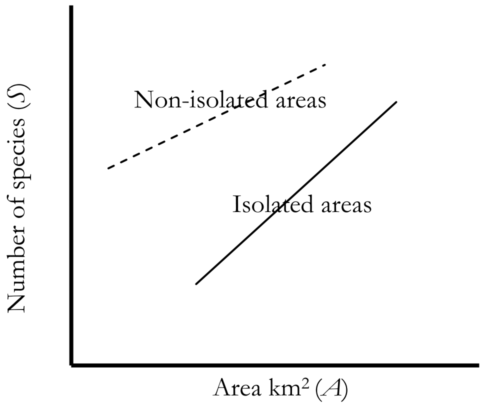

Fig. 13. The species–area rule ($S = cA^z$) plotted on logarithmic axes for isolated and non-isolated habitats. Isolated habitats have higher z values than non-isolated habitats, and thus steeper slopes in the species–area curve, since z determines the slope of the curve. Isolated habitats, such as islands, support fewer species than non-isolated areas of similar size.

On islands the **species–genus** ratio is lower than on the mainland from which the species was colonized. In other words, there are fewer sympatric congeneric species on an island than in the mainland areas of similar size. This decrease of the species–genus ratio on islands has been explained as an effect of interspecific competition that is supposed to be more intense on islands than on the mainland. See **Gause's** rule and the theory of **limiting** similarity. Philosophers: Sloep (1993). Biologists: Elton (1946); Hagmeier & Stults (1964); Grant (1966); Simberloff (1970, 1984); Van Valen (1973a); Strong (1980); Diamond & Gilpin (1982)

The **thinning** rule for plants is $W = aD^{-1.33}$, where D is density and where W is body size. In other words, in plants and some other sessile organisms, it is the density of populations that is treated as the independent variable affecting body size. See the **inverse** scaling rule. Biologists: Peters (1991: 45); Marquet (2000); Shrader-Frechette & McCoy (1993: 123); Marquet *et al.* (2005)

The rule of the **unspecialized** (also known as Cope's second rule): unspecialized species (i.e., generalists) tend to avoid extinction longer than specialized species. Philosophers: Hull (1974: 81-82, 100); Rosenberg (1985: 207-208, 215-216, 1987: 196). Biologists: Mayr (1942: 294-295); Rensch (1959: 236-237, 284, 1960: 108); Stanley (1973); Southwood *et al.* (1974); Flessa *et al.* (1975); Holmes (1977); van Valkenburgh *et al.* (2004)

Van **Valen's** rule of extinction: extinction rates for taxa are both constant and taxon specific. See the **Red** Queen rule. Biologists: Van Valen (1973b); Raup (1975)

The **Volterra** rule: any biotic or abiotic factor that both *increase* the death rate of predators and *decrease* the growth rate of their prey has the effect of *decreasing* the predator population size, whereas the population size of its prey *increases*. Philosophers & biologists: Odenbaugh (2005: 242); Levins (2006: 747); Weisberg (2006a: 642-643, 2006b); Weisberg & Reisman (2008)

Williston's rule (also known as the rule of anisomerism): a phylogenetic trend in which serial, repetitive, similar, or unspecialized traits, parts, or limbs in organisms evolve toward fewer numbers and more specialized functions. Philosophers & biologists: Gregory (1935); Rensch (1959: 291); Gould (1970: 207, 209, 1980); Cisne (1974); Ruse (1973: 58); Hull (1974: 81-82, 100); Waters (1998: 15); Sidor (2001)

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