

# On the dangers of making scientific models ontologically independent: taking Richard Levins' warnings seriously

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**Abstract** Levins and Lewontin have contributed significantly to our philosophical understanding of the structures, processes, and purposes of biological mathematical theorizing and modeling. Here I explore their separate and joint pleas to avoid making abstract and ideal scientific models ontologically independent by confusing or conflating our scientific models and the world. I differentiate two views of theorizing and modeling, *orthodox* and *dialectical*, in order to examine Levins and Lewontin's, among others, advocacy of the latter view. I compare the positions of these two views with respect to four points regarding ontological assumptions: (1) the origin of ontological assumptions, (2) the relation of such assumptions to the formal models of the same theory, (3) their use in integrating and negotiating different formal models of distinct theories, and (4) their employment in explanatory activity. *Dialectical* is here used in both its Hegelian–Marxist sense of *opposition* and *tension* between alternative positions and in its Platonic sense of *dialogue* between advocates of distinct theories. I investigate three case studies, from Levins and Lewontin as well as from a recent paper of mine, that show the relevance and power of the dialectical understanding of theorizing and modeling.

**Keywords** Dialectics · Ecology · Evolutionary genetics · Richard Levins · Models · Modeling · Theory · Ontological assumptions · Ontology

## Introduction

Richard Levins' recent auto-biographical presentation, "Living the 11th Thesis," elicited thunderous applause, and even some tears, upon delivery to the International Society for the History, Philosophy and Social Studies of Biology meetings, held in Guelph, Canada in July of 2005. As is so often the case when attempting to

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interpret the intentions of groups of people, contemporary or historical, it is extremely difficult to assess the reasons for what seemed to be a very strong emotional response to a piece that related politics, science, and sentiment. However, upon talking to some audience members after the presentation, it became clear that at least some attendees had enjoyed hearing a substantiated and substantial account of the deep interrelation between political activism and scholarship. It seems that a number of listeners reacted positively to one of Levins' main auto-biographical messages: "Rather than face a problem of combining activism and scholarship, I would have had a very difficult time trying to separate them" (Levins 2005, p. 1). The main goal of my article is to spell out what I take to be one of the key intellectually rich ways in which Levins has intertwined activism and scholarship: his warnings to biologists regarding the dangers of making abstract and idealized scientific formal models ontologically independent.

In what follows, I will first motivate two different views on biological mathematical modeling: the *orthodox* and the *dialectical* views. I discuss these positions in general and as they relate to Levins and Lewontin's work. Subsequently, I turn to three case studies of the way that abstract and ideal mathematical models are made ontologically independent: (1) Levins' notion of *sufficient parameter*, (2) Levins and Lewontin's discussion of the statistical analysis of gene interaction (epistasis), and (3) my analysis of theoretical imposition in the modeling of the evolution of cytoplasmic incompatibility caused by *Wolbachia*, an obligatory endosymbiont of many arthropod species (Winther 2006). In my conclusion, I follow the dialectical view by defending the importance of ontological assumptions in biological mathematical modeling.

### The orthodox and dialectical views of theorizing

Levins and his frequent co-author, Richard Lewontin, have increased our understanding of the structures, processes and purposes of theorizing and modeling. Particularly in *The Dialectical Biologist*, these two activist-scientists present critiques of what they take to be the usual way to interpret biological mathematical theorizing and modeling. I will call this interpretation the *orthodox* view. In opposition to this position, Levins and Lewontin develop what I will call the *dialectical* view.<sup>1</sup>

#### Motivating the distinction

I will contrast the orthodox and dialectical views with respect to their manner of conceptualizing ontological assumptions in biological mathematical theorizing and modeling. I understand such assumptions to be *presuppositions* concerning *the structures and processes, causes and effects, and organizational and functional hierarchies that are found in the world*. In other words, a scientific theory includes a set of beliefs and commitments about the nature of the world. In what follows, I will

<sup>1</sup> It is beyond the scope of this paper to discuss the related views found in the work of a number of other scholars who also emphasize the importance both of ontological components in our theories and of the process of theorizing as involving an imposition onto nature of those components. See, e.g., Marx *Das Kapital* v. 1 (Tucker 1978, pp. 320–321); Dewey 1929/1958, 1938 (e.g., 1929/1958, pp. 29–30); Kuhn 1970; Oyama 1985 (e.g., pp. 62–63); Smith 1996 (e.g., pp. 49–50) and Clapin 2002 (e.g., part 5 "On Smith," pp. 219–292).

discuss, with regards to biology, the respective commitments of the orthodox and dialectical view concerning the following four issues: (1) the origin of ontological assumptions, (2) the relation of such assumptions to the formal models of the same theory, (3) their use in integrating and negotiating different formal models of distinct theories, and (4) their employment in explanatory activity. The two views have radically different positions on each of these matters. It is also important to note that these are two *families* of views. Thus, I do not intend to use their respective commitments to each of these issues as either individually necessary or collectively sufficient properties for defining the views.

The orthodox *view*, as it applies to biology, holds that mathematical models are incomplete pictures of the world. I will now address each of the four issues in turn for this perspective.

With respect to the origin of ontological assumptions, the orthodox view claims that they are primarily abstracted and idealized from empirical information. Here, I employ Cartwright's distinction between abstraction and idealization (Cartwright 1989; see also Jones 2005). On the one hand, abstractions omit properties and relations in order to provide more general descriptions of a larger extension of cases. Abstractions about ontology are meant to be as representationally faithful to the world, in as compact a manner, as possible. Since they omit properties and relations such ontological assumptions cannot account for all the complex aspects of the world. However, the information they leave out can be filled in unproblematic when necessary. On the other hand, ontological presuppositions that are idealizations capture essential empirical aspects of complex situations even if that involves distorting, and making intentionally false claims about, the world. These distortions and false statements are intrinsic to the idealization and cannot be easily remedied. In short, ontological assumptions are produced from empirical information either by omission [i.e., abstractions] or by capturing one crucial aspect (at the expense of mishandling others) [i.e., idealizations].

Under the orthodox view, ontological assumptions are secondary to the formal models. Both of these are components of theory. Ontological assumptions (1) serve mainly as repositories of empirical information and, as a consequence, (2) specify the empirical conditions under which the formalizations apply. With respect to the first, they capture information that cannot be fully represented in the formalizations themselves, including the complete range of objects, properties and relations present in the real world. They are also dispensable and readily changed, should new empirical information make that necessary. Concerning the second, the ontological assumptions guide the correct employment of the formal models. So, to summarize, they provide metaphysical legitimacy for: (1) defining certain variables and parameters in particular ways, and for articulating them together in equations of certain sorts and (2) characterizing the conditions of application. Furthermore, both of these functions are interpreted by proponents of orthodox views as readily and straightforwardly satisfied. An example might help. When kin selection is considered an important part of the world, relatedness will be parameterized (e.g., Hamilton's  $r$ ) and kin selection models employed. However, if kin selection is not deemed crucial, the models used will not include parameters pertinent to such selection.

When we seek to integrate or unify distinct formal models, as is the case in the recent debates about levels of selection, ontological commitments can be readily altered should the mathematical manipulation require it. Put differently, under the orthodox view, negotiating among distinct models from different theories is

primarily a *formal* rather than an *ontological* matter (e.g., Dugatkin and Reeve 1994; Sterelny 1996; Kerr and Godfrey-Smith 2002). After all, the argument is made that the formalizations are representations of one reality so they must, ultimately, be consistent. Once the distinct formal models are negotiated, it is expected that ways will be found to readily unify, possibly even identify, the different sets of ontological commitments of each theory. Again, an important way of justifying this expectation is by appealing to one underlying and readily comprehensible world.

When we seek to explain, the orthodox view argues that the formal models alone do most of the work. The explanatory relation is between mathematical models and data. The data are seen as stemming directly and causally from the world. Ontological presuppositions exist primarily in the background, specifying the conditions of application. Should the explanation fail, the ontological assumptions can be transformed, in a coherent and clear manner, in order to assist the production of more reliable and explanatory models.

While accepting numerous elements of the orthodox view, Levins and Lewontin also critique it deeply in a number of ways. They develop the dialectical view of biological mathematical modeling. Under their interpretation, all aspects and activities of biological modeling and theorizing strongly and clearly involve ontological commitments. These assumptions play strongly active and creative roles in our modeling, explanation, and understanding of the world.<sup>2</sup> Below, I will articulate a number of ways in which these presuppositions are seen to be active and creative. A further aspect of their productive strength, as construed by the dialectical view, is that they are not easily changed or unified across theories. Therefore, they are in dialectical tension across theories *and* with the world. In what follows, I first discuss the four issues with respect to this view and, subsequently, characterize four reasons for baptizing it as “dialectical.”

Concerning the origin of ontological assumptions, the dialectical view agrees partially with the orthodox view in that it also takes ontological assumptions to be abstracted and idealized from empirical information. But, in addition, they are seen to be strongly shaped by the internal demands of the model and theory. The formation of these assumptions is a creative and inevitably biased task (e.g., Levins and Lewontin 1985). As we will see below for the case studies, the nature of new ontological assumptions is strongly influenced by pre-existing formal models and ontological assumptions. Furthermore, information left out of the ontological presuppositions cannot easily be regained (e.g., Levins 1968, p. 10). There is an important analogy here between the underdetermination of theory by the data and the underdetermination of ontological assumptions by the data. That is, the assumptions are underdetermined by the data and are significantly determined by the architecture and needs of the model and theory. The world is not a simple given from which a set of ontological commitments are neatly abstracted and idealized.

The relation between ontological presuppositions and formal models is complex under the dialectical view. In the orthodox view, there is a fairly close link between the world and ontological assumptions as well as between the world and the formalizations; the connection between the ontological assumptions and the formal models, however, is fairly weak and flexible. In contrast, in the dialectical view, the strongest coupling is precisely between the ontological assumptions and the formal

<sup>2</sup> One similarity between the two views is that they both accept that the world is categorically complex.

models. In addition, there is a circuitous link of either with the world. Let us explore this connectedness. Applying a particular mathematical technique or showing a mathematical representation, or both, is invariably associated with imposing a set of ontological commitments (e.g., Levins and Lewontin 1985, Chapter 6; Section ‘Levins and Lewontin on statistical methodology’ below). Thus, mathematical models, which involve techniques and representations, are suffused with ontological commitments. Levins and Lewontin believe that we would be well advised to make such commitments explicit and discuss them critically. Furthermore, as we saw above, new ontological assumptions must meet the internal demands of the formal models as well as be consistent with prior ontological presuppositions closely connected to these models. In short, it is very difficult to change the formalism without changing the ontological assumptions of a theory, and vice versa.<sup>3</sup>

In analyzing this tight relationship between formalisms and ontological assumptions, which is the second issue I am analyzing for the dialectical view, Levins’ appeal to activism comes to the fore. If it is indeed the case that a formal model is ineluctably densely connected with ontological commitments, then, rather than remain silent about these elements, deny them or remain oblivious to them, biologists should discuss them openly. Unfortunately, modelers often worry significantly more about the formalism than about the ontology associated with a theory. In order for biologists to understand the relationships among alternative sets of such commitments, they can empirically test such presuppositions, conceptually compare them or dialogue over them. One goal of these epistemic and social process is for biologists these to understand how elements of abstract and idealized theory are *made* ontologically independent. Particularly in a world torn by socioeconomic, public health, geopolitical, and ecological disasters, a clear awareness of the ontological commitments presupposed by a large variety of epidemiological, economic, political, and ecological models, as well as a reflective analysis of the consequences of such commitments, is incumbent. Scientists, Levins seems to suggest in his activism-scientific program, have a responsibility to aim for a more ontologically honest and transparent science.

The negotiation of different formal models—through coordinated, complementary or nested models (Levins 1966, 1968)—is importantly different in the dialectical view as compared to the orthodox view. Under the dialectical view, such negotiation involves making both the mathematics and the ontology commensurate. It is not sufficient to only find formal equivalences, as Robert Wilson also argues in an instructive paper on levels of selection:

...note that having a shared mathematical framework, or being represented by the very same equations, does not itself entail that two or more processes ‘cannot fundamentally differ from one another.’ This is because mathematical models capture just some aspects of the dynamics or kinematics of the pro-

<sup>3</sup> There is some “slop” between the two. For instance, the same formalism can be consistent with different (though almost certainly overlapping) sets of ontological assumptions. I suspect, though, that this is much more common in heavily formalized sciences—especially theoretical physics—than in biology. One of the central unresolved questions of quantum mechanics concerns the *interpretation* of theory. To what sorts of objects and processes does theory refer? Here, multiple radically different ontological interpretations are consistent with the same formal framework of quantum mechanics. In biology, however, I think that there is a much closer link between formalism and actual (as well as intended) ontological assumptions/ontological interpretation. I am grateful to Marie Svarre Nielsen for discussions on this matter.

cesses they model, and they serve *as* models of those aspects only given further assumptions [which can be ontological ones] not represented in the models themselves. (Wilson 2003, p. 538)

It is not always possible to unify or make ontological assumptions identical. Consequently, scientific disputes sometimes revolve around disagreements over ontological commitments rather than over the nature of the formal models. This is so even when these commitments are hidden, denied or forgotten.

There is a deeper point here too. Recall that formal models and ontological assumptions are deeply intertwined in the dialectical view. Thus, if for a particular case of distinct formal models, the former have been shown to be translatable, but the latter have not been made equivalent, it is important to ask whether the formal models have really been translated. I believe that it is important to ask such questions for the levels of selection debate in which there indeed seems to be formal translatability between genic selection and group selection models (Dugatkin and Reeve 1994; Kerr and Godfrey-Smith 2002). However, ontological assumptions of the two competing theories have by no means been made equivalent in this debate.

With respect to the final issue, explanation, the dialectical view claims that ontological commitments play a central role. First of all, there is a feedback relation between the world and the ontological assumptions/formal models. The way the world is taken to be depends significantly on the latter pair. On analogy with *theory-ladenness* of observation, data is *also* laden by *ontological assumptions*. Thus, the data to be explained is already influenced by ontological assumptions, themselves a component of theory. Furthermore, the dialectical view holds that ontological presuppositions are imposed upon the world, as well as upon the data representing it, in our explanations. This reification is often subsequently forgotten, but it is an essential aspect of our explanatory activity. In explaining, we employ ontological assumptions in addition to formal models.

Let us briefly summarize how Levins and Lewontin themselves articulate the dialectical view. Levins warns us: “the individual models, while they are essential for understanding reality, should not be confused with that reality itself” (Levins 1966, p. 431). What Levins (and Lewontin) find particularly ironic is that in myriad cases modelers unconsciously impose all sorts of ontological assumptions onto the data and, subsequently, believe and convince themselves that these assumptions exist independently in the world *per se*. Therefore, they understand the world to be that way. The models are “confused with [the] reality itself.” As they note, “Ideals are abstractions that have been transformed by fetishism and reification into realities with an independent ontological status” (Levins and Lewontin 1985, p. 150). Levins and Lewontin ask us to be critical about the process of taking abstract scientific theories, including their formal models and ontological commitments, to be inhabitants of the real world. They do not seek to eliminate this process since, as we shall see, it is inevitable. Instead, they believe that it should be made transparent by making it explicit and, subsequently, restrained to the extent possible by engaging a plurality of views.

There are at least four reasons for using the term “dialectical” to characterize this view of modeling. It would be useful to clarify these.<sup>4</sup> The first pair relates to the Hegelian–Marxist sense of dialectical as *opposition* and *tension* between alternative

<sup>4</sup> I gratefully acknowledge discussions with Faviola Rivera Castro on these points.

positions; the second pair concerns the Platonic sense of *dialogue* between advocates of distinct theories. In what follows, I state them together with the orthodox view's oppositional position on each of them:

- (1) As we shall see below, Levins, among others,<sup>5</sup> explicitly holds that there is a *tension between the complex world and our simple models of it*. Due to cognitive limitations, among other reasons, we simply cannot do justice to the complexity of the world. Thus, explanation and theoretical organization will always be limited—the world cannot be made sufficiently simple nor can theories be made sufficiently complex. Yet our theories interpret the world from their own perspective, and are often “confused” with the world. On the other hand, the orthodox view holds that theories are indeed often highly useful and accurate instruments to predict and explain complex phenomena; less frequently, it holds that theories can be very sophisticated and complex resources for making inferences about the structure of ontology.
- (2) Furthermore, *the ontological commitments in single simple theories and models do not easily or straightforwardly match the ontology in the complex world*. A single theory does not explain all the phenomena, and most theories contain ontological components not yet (or only partially) tested and corroborated. There is a tension between the ontology in these two “realms”. In addition, the ontology and formalism in the theory itself are deeply intertwined. On the other hand, the orthodox view emphasizes that ontological commitments in the theory and ontology in the world are highly congruent.
- (3) “Dia-” means “across” or “between” in Greek, whereas “-lectic” comes from “legein,” which means “to speak or converse”.<sup>6</sup> Thus, the view also refers to the Platonic dialectic of *dialogue between those endorsing alternative points of view*: (1) dialogue between the orthodox and dialectical views themselves, and (2) dialogue between alternative scientific theories (e.g., Fisherian and Wrightian perspectives on evolutionary genetics, Winther 2006). On the other hand, the orthodox view would diminish the importance of dialogue and, instead, stress empirical adequacy as well as mathematical translation as the only negotiation strategies possible across scientific theories.
- (4) Furthermore, *dialogue about ontological commitments* is accentuated. This dialogue is essential for overcoming differences and tensions among distinct theories. Such communication highlights and rescues the desirable ontological components of each theory, while discarding the undesirable ontological elements of each. The beneficial components are sometimes the same in different theories. On the other hand, the orthodox view emphasizes empirical testing and mathematical consistency as the only manner to rescue desirable, and discard undesirable, elements of theories.

### Levins and Lewontin on abstraction and idealism

Let us now turn to a much more detailed analysis of Levins and Lewontin's dialectical view of mathematical modeling in biology by focusing on their explicit

<sup>5</sup> See also, e.g., Cartwright (1983, 1999) and Wimsatt (1987).

<sup>6</sup> The word “logos” is also derived from the same Indo-European root “leg-”. Oxford English Dictionary.

analysis of abstraction and idealism. Perhaps the clearest exposition of their position on abstraction, and the dangers this process can entail, is found in their essay “Dialectics and Reductionism in Ecology.”<sup>7</sup> In this paper, they reject what they take to be extreme, and illegitimate, ideological positions in theoretical ecology: “mechanistic materialism” and “dialectical idealism” (Levins and Lewontin 1985, p. 133). A number of key, but wrong-headed, debates in ecology, Levins and Lewontin suggest, have their source in heated arguments between these two schools.

These debates are based on two confusions:<sup>8</sup> reductionism has been conflated with materialism, and the distinction between idealism and abstraction has been elided.<sup>9</sup> Through conscious diagnosis, and subsequent avoidance of these confusions by eschewing the very conditions of the debate, Levins and Lewontin seek to rescue useful elements from *both* mechanistic materialism and dialectical idealism. In order to fulfill this goal, they “develop implicitly a Marxist approach to the questions that have been raised in ecology” (Levins and Lewontin 1985, p. 133). Under this approach, materialism can be defended without advocating reductionism. Furthermore, abstractions can be found useful without committing oneself to unjustified idealism or illegitimate ideals.

The central theses of their Marxist approach are:

that nature is contradictory, that there is unity and interpenetration of the seemingly mutually exclusive, and that therefore the main issue for science is the study of that unity and contradiction, rather than the separation of elements, either to reject one or to assign it a relative importance. (Levins and Lewontin 1985, p. 133)

This study of unity and contradiction is to be done on nature as well as theory. For example, in evaluating the contradiction between mechanistic materialism and dialectical idealism, they note that these positions actually share an ontological commitment, which is itself a form of unity. The two views “share a common fault: they see ‘true causes’ as arising at one level only, with the other levels having epistemological but not ontological validity”. The views are diametrically opposed as to what they interpret as the “‘real’ objects”: for the former, these objects are the lowest-level constituent parts (e.g., individual species, or even organisms and molecules), for the latter they are the whole (e.g., the community). (Levins and Lewontin 1985, p. 135) In contrast to this mono-causal commitment, for Levins and Lewontin there are causally relevant objects at many levels: “the community is a contingent whole in reciprocal interaction with the lower- and higher-level wholes and not completely determined by them” (Levins and Lewontin 1985, p. 139). Thus, the commitment shared by the mechanistic materialists and dialectical idealists is simply false. Levins and Lewontin hold that ontological reductionism and holism are extreme views, the former of which should not be conflated with materialism (itself an acceptable thesis!). Let us now turn to their views on idealism and abstraction.

They discuss idealism in a section called “Abstraction and Idealism.” Examples from both physics and biology are used to support their argument that uncritical,

<sup>7</sup> This essay originally appeared in *Synthese* in 1980 (43) and was subsequently reprinted as Chapter 6 of their 1985 book.

<sup>8</sup> Levins and Lewontin mention one more: stochasticity and statistics have been conflated. I will not explore this confusion here.

<sup>9</sup> Their distinction between these terms is admittedly different from mine.



unquestioned, and unreflective abstraction can be extremely dangerous for our understanding of scientific theory and the world, as well as for our subsequent behavior based on such an understanding. When criticality, questioning, and reflection are absent from a process of abstraction, it is converted into an instance of idealism. They observe:

We can hardly have a serious discussion of a science without abstraction. What makes science materialist is that the process of abstraction is explicit and recognized as historically contingent within the science. Abstraction becomes destructive when the abstract is reified and when the historical process of abstraction is forgotten, so that the abstract descriptions are taken for descriptions of the actual objects. ...The problem for science is to understand the proper domain of explanation of each abstraction rather than become its prisoner.

Darwin's and Mendel's work, although great triumphs of materialist explanation in biology, are filled with abstractions (species, hereditary factors, natural selection, varieties, and so on). Abstraction is not itself idealist. The error of idealism is the belief that the ideals are unchanging and unchangeable essences that enter into actual relationships with each other in the real world. Ideals are abstractions that have been transformed by fetishism and reification into realities with an independent ontological status. (Levins and Lewontin 1985, pp. 149–150)

Science requires abstraction. This process and its products (scientific models) are necessary for scientific activity. Levins and Lewontin see nothing wrong with the fact that Darwin's and Mendel's work needed abstractions to frame and provide content to their 19th century materialist explanations and theories. Through the use of other examples from physics that they provide (such as Newton's abstractions in the *Principia*), it becomes clear that Levins and Lewontin accept the fundamental importance of abstraction in science. In fact, it would be surprising for them to claim otherwise given that they are two of the absolutely outstanding theorists of the second half of the 20th century in their respective fields, ecology and evolutionary genetics.

However, what they do take issue with—and this is where idealism enters the picture—is the pernicious effect the abstract (qua “ideals,” as they put it) can have when it “is reified and when the historical process of abstraction is forgotten.” When this occurs, the abstract has been made concrete (i.e., “abstract descriptions are taken for descriptions of the actual objects”) and the process of scientific inquiry involving social relations of various sorts is forgotten. The abstract has now been inappropriately reified (in an explanation, for example) and the dynamic process that caused this reification is neither admitted nor remembered.

Furthermore, these ideals are now taken as real objects in the world that can themselves be represented, modeled and theorized. That is, the fundamental error of idealism is the “belief that the ideals are unchanging and unchangeable essences that enter into actual relationships with each other in the real world.” These essences, which are the product of the reification of the abstract, are now interpreted as inhabiting the real world and as displaying unchangeable properties. To refer to Lewontin's work on this, causal *genetic programs* as well as organism-independent *niches* (which are sources of adaptive problems), for example, are also taken to be pre-existent causal essences in the ontology pertinent to, respectively, developmental biology and ecology. More generally, genetic programs and niches are also directly relevant to evolutionary theory. The inverse irony is that these reifications

are now considered the very ontological object of study of the theories and models. *In idealism, the consequences of theorizing are interpreted as pre-existing ontological sources of theoretical models.* In the final analysis, it is ideals, rather than abstractions, which Levins and Lewontin critique and reject:

To put the matter succinctly, what distinguishes abstractions from ideals is that abstractions are epistemological consequences of the attempt to order and predict real phenomena, while ideals are regarded as ontologically prior to their manifestation in objects. (Levins and Lewontin 1985, p. 152)

Ideals, not abstractions, are the dangerous and pernicious results of uncritical, unquestioned, and unreflective abstraction processes. A further analysis of their conception of abstraction and idealism would be worthwhile making in the context of Levins' paper (2006) and Ollman (2003), a book, which Levins cites enthusiastically.

Thus, the dialectical view emphasizes the importance of ontological commitments in the theorizing process. The commitments of a particular theory are tightly intertwined with the formal models and are not as closely linked to the world. Explanation involves imposition of ontological assumptions onto both the data and the world. Furthermore, according to the dialectical view, the theorizing process essentially involves a variety of contradictions. Let me mention three. The ontological assumptions in our simplifying theory do not match the ontology in the complex world. Different scientific theories are often in tension with each other in their modeling and explanation of the same part of the world. The orthodox view vs. the dialectical view of modeling are themselves in tension. These contradictions can be overcome not just through empirical testing or mathematical transformation but also, to a very important extent, through dialogue about ontological assumptions. Let us now see how, according to the dialectical view, these dialectical tensions, the activity of dialogue, and the link between ontological assumptions and formal models get played out in three case studies.

### Three case studies

I now turn to three examples for which I present an explicit dialectical analysis of the way scientists make abstract and ideal formal models ontologically independent: (1) Levins' notion of *sufficient parameter*, (2) Levins and Lewontin's discussion of the statistical analysis of gene interaction (epistasis), and (3) my analysis of theoretical imposition in the modeling of the evolution of cytoplasmic incompatibility caused by *Wolbachia*, an obligatory endosymbiont of many arthropod species (Winther 2006).

#### Levins on sufficient parameters

In his classic 1966 discussion of model building in population biology, Levins notes that one of the fundamental problems of providing intelligible models of rich and complex systems in nature is the sheer number of parameters needed to describe and explain natural processes. Employing such a large number of parameters overwhelms us cognitively and computationally.<sup>10</sup> Levins suggests solving this problem through the use of sufficient parameters:

<sup>10</sup> Particularly on the latter point, see Weisberg (2003); see also Odenbaugh (2007).

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 as part of “fitness.” (Levins 1966, pp. 427–428)

Through appropriate abstraction, we can usefully summarize some of the information of lower-level parameters into a higher-level parameter.

It is important to note that a “level” can be both an organizational level (e.g., organisms and genes) as well as a level of abstraction (e.g., a more abstract category which includes only some of the aspects of the more concrete categories). I suggest that given a number of the examples of sufficient parameters that he provides, the latter rather than the former understanding of level is the one that he emphasizes and studies. After all, models with sufficient parameters are “general” (Levins 1966, p. 429) and, elsewhere, he writes:

unlike the situation in formal mathematics, in science the general does not fully contain the particular as a special case. The loss of information in the process of ascending levels requires that auxiliary models be developed to return to the particular. Therefore, the “application” of a general model is not intellectually trivial, and the terms “higher” and “lower” refer not to the ranking of difficulties or of the scientists working at these levels but only levels of generality (Levins 1968, p. 8)

Thus, general and abstract sufficient parameters capture, in a useful manner, a limited amount of the information of a variety of more detailed parameters. Although sufficient parameters are valuable for a number of modeling purposes, a real loss of information occurs in their articulation. Levins sees this as the inevitable outcome, and precisely the point, of any abstraction process: generality gives us intelligibility and also implies that we sacrifice either realism or precision.

Before turning to Levins’ awareness of the dangers of making abstract sufficient parameters ontologically independent, I will briefly mention some of his examples of sufficient parameters. An important aim of Levins’ research project seems to have been to place ecology on solid theoretical foundations. Unlike evolutionary genetics, ecology did (does?) not have canonical equations or a small set of agreed-upon variables requisite for such equations. Discussing proposals of different sets of sufficient parameters could be a remedy for this situation. Although they can “arise directly from the mathematics and may lack intuitive meaning,” Levins suggests meaningful and useful sufficient parameters. Some of these “are formalizations of previously held but vague properties such as niche breadth” (Levins 1966, p. 428). *Environmental uncertainty*, for example:

arise[s] from the combination of results of more limited studies. In our robust theorem<sup>11</sup> on niche breadth we found that temporal variation, patchiness of the environment, productivity of the habitat, and mode of hunting could all have

<sup>11</sup> On robust theorems, Levins writes: “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 which is relatively free of the details of the model. Hence our truth is the intersection of independent lies” (Levins 1966, p. 423). For an analysis of Levins’ notion of *robustness*, see Wimsatt (1981) and Weisberg (2006).

similar effects and that they did this by way of their contribution to the uncertainty of the environment. (Levins 1966, p. 429)

Thus, this particular sufficient parameter summarizes a variety of lower-level already-abstracted parameters. In addition, he argues for a variety of sufficient parameters for both environmental and genetic properties. With respect to environmental conditions of an ecological and evolutionary study, he defends the employment of parameters such as: environmental range, uncertainty, grain, and temporal variance (Levins 1968, pp. 34–35). Concerning genetic circumstances, he argues that “the complexities of multiple systems with epistasis can be reduced to relatively few sufficient parameters”: genetic memory, delay, ridginess, and multiplicity of peaks [on a Wrightian adaptive surface] (Levins 1968: 94). Sets of such sufficient parameters would assist in placing ecology on firm theoretical ground.

Now I shall argue that Levins was well aware, during the end of the 60s, of the dangers of making abstract scientific mathematical models ontologically independent, as he was to explicitly observe a decade later with his regular co-author.<sup>12</sup> Levins critically and insightfully discusses the weaknesses of postulating sufficient parameters. In particular, he claims that general models using sufficient parameters have three “source[s] of imprecision”:

(1) they [the “general models”] omit factors that have small effects or which have large effects but only in rare cases; (2) they are vague about the exact form of mathematical functions in order to stress qualitative properties; (3) the many-to-one property of sufficient parameters destroys information about lower level events. Hence, the general models are necessary but not sufficient for understanding nature. For understanding is not achieved by generality alone, but by a relation between the general and the particular. (Levins 1966, pp. 429–230)

Note that these general models are “necessary but not sufficient” for “understanding nature.” Such models are not sufficient because they have certain imprecise elements, particularly the three he mentions. An accurate description of nature would require utilizing other sorts of models, especially ones that emphasized realism and precision at the expense of generality.

These sources of imprecision are an integral part of the interpretative and historical process of abstraction (and, possibly, idealization in my sense) required to formulate the (any) sufficient parameters. Therefore, ontological assumptions of all sorts can and do enter during abstraction. The danger lies in forgetting that this process has taken place. Subsequent to the abstraction, it is fairly easy to conclude, for example, that factors with small effects are really not there, (1), or that the destruction of information concerning lower level abstractions really is a consequence of the actual typological or causally unimportant nature of the processes already summarized by these lower level abstractions, (3). Put differently, there is a danger of implicitly justifying the higher level sufficient parameter based on a belief in the ontological absence and unimportance both of factors with small effects and of lower level abstractions. Although Levins does not make the point in exactly these terms, this reading of the consequences of the sources of imprecision in the articulation of general models makes sense in light of his concerns with activism and with

<sup>12</sup> Recall that their “Dialectics and Reductionism in Ecology” first appeared in 1980.

making science more ontologically transparent and honest. To be slightly anachronistic, Levins wants the sufficient parameters to be abstractions and not ideals (sensu Levins and Lewontin 1985).

My interpretation of Levins' caution with sufficient parameters is further bolstered by the conclusions Levins reaches in his 1966 paper. In his typically elegant and insightful manner, his last paragraph reads as follows:

The multiplicity of models is imposed by the contradictory demands of a complex, heterogeneous nature and a mind that can only cope with few variables at a time; by the contradictory desiderata of generality, realism, and precision; by the need to understand and also to control; even by the opposing esthetic standards which emphasize the stark simplicity and power of a general theorem as against the richness and the diversity of living nature. These conflicts are irreconcilable. Therefore, the alternative approaches even of contending schools are part of a larger mixed strategy. But the conflict is about method, not nature, for the individual models, while they are essential for understanding reality, should not be confused with that reality itself. (Levins 1966, p. 431)

Thus, while he clearly endorses a realism (i.e., “reality itself” exists), he understands that there is the perennial temptation and risk of confusing—and converting—scientific theories with/into reality. That is, the danger of making abstract and ideal models ontologically independent looms forever large. All the conflicts that he has diagnosed, including trade-offs among generality, precision, and realism, and even the (over)generalization of sufficient parameters, belong to the realm of modeling and theorizing, and not to reality itself. But, it is extremely easy to confuse methods and models with nature and reality. Alas, even with the self-critical employment of Levins' sophisticated account of models and modeling the risk of making them ontologically independent remains.

However, there is hope. Implicit in Levins' conclusions is the claim that scientists aware of the contradictory demands inherent in theory can use the dialectical (contradictory and dialogical) account of modeling to differentiate, as clearly as possible, ontological assumptions from actual ontology. They can also use this account to diagnose the relations between ontological assumptions and formalisms. This is the best method we have to avoid confusing theory and reality—it is certainly better than the orthodox account of theorizing. Scientists can become aware of their ontological commitments by adopting this view. Furthermore, such scientists are more self-critical. Because they see the intrinsic conflicts and pluralism present in theorizing, they are more likely to listen to those presenting very different theories and models, containing distinct ontological frameworks. In other words, Levins' realism does not imply that he thinks models lack ontological components. Furthermore, the dialectical account, employed by Levins, allows us to identify and understand our ontological views, especially as they feed into our abstractions of sufficient parameters. *Sufficient parameters are part and consequence of our theorizing efforts and should neither be confused nor conflated with reality (i.e., they should not be taken as pre-existing in the world).*

Levins and Lewontin on statistical methodology

In their co-authored article “Dialectics and Reductionism in Ecology,” as well as in Lewontin's work on evolutionary genetics, Levins and Lewontin state a general

concern they have with statistical methodology. As they put it, this methodology *reifies* statistical abstractions, thereby formulating unjustified causal claims. I will explore their general critiques of the reification of “main effects” in statistical analyses and, subsequently, turn to their, as well as Wimsatt’s and Michael Wade’s, proposals for rectifying the situation.

Regarding weaknesses of analysis of variance (ANOVA) as well as regression analysis methodology, Levins and Lewontin state:

...natural and social scientists persist in reifying the main effect and interaction variances [of an analysis of variance] that are calculated, converting them into measures of separate causes and static interaction of causes. Moreover, they act as if “main effects” were really “main” causes in the everyday English meaning if the word and as if interactions were really secondary in importance.

The most egregious examples of reification are in the use of multiple correlation and regression and of various forms of factor and principal components analysis by social scientists. Economists, sociologists, and especially psychologists believe that correlations between transformed orthogonal variables are a revelation of the “real” structure of the world. Biologists are apparently unaware that in constructing the correlation analysis itself they impose a model on the world. Their assumption is that they are approaching the data in a theory-free manner and that data will “speak to them” through the correlation analysis. (Levins and Lewontin 1985, pp. 155–156)

The reification here occurs when the very structure of the ANOVA model as well as of the regression analysis is imposed on, or “confused with”,<sup>13</sup> the causal structure of the world—e.g., (1) statistical “main effects” are seen as actual “main” material causes, and (2) real-world causal interactions are understood as statistical interactions between static statistical main effects.<sup>14</sup> Furthermore, in a correlation analysis, biologists are unaware that they are “impos[ing] a model on the world.”<sup>15</sup> These are mistakes and confusions—scientific formal models are interpreted as the causal ontology. In short, Levins and Lewontin are here pointing to the reification of statistical theory.<sup>16</sup>

What kind of alternative do they recommend? Do they suggest that we engage in a diversity of radically different reifications or that we somehow avoid reification altogether? I believe that they favor the former, and here I want to briefly mention an alternative statistical and analytic methodology that they defend. This

<sup>13</sup> See Levins (1966, p. 431).

<sup>14</sup> Wade (1992) also provides a useful discussion of precisely these points when he analyzes Fisher’s metaphor of ANOVA methodology and factorial design as a “questionnaire to Nature” (p. 42) which, for a variety of technical reasons including the statistical *power* of various tests, make one “more likely to discover main effects than... interactions” (p. 43). Now, if the questionnaire is biased in this manner, then users of the questionnaire will, upon receiving answers to their questions, illegitimately attribute properties (e.g., strong main causal effects) to nature that are actually outcomes and artifacts of statistical methodology.

<sup>15</sup> There are clear exceptions to this. Neyman et al. (1956) provides a sophisticated account of the complex interaction among theory, experiments, statistics, and data.

<sup>16</sup> Furthermore, Lewontin articulates this critique in more detail in his “The Analysis of Variance and the Analysis of Causes” [first published by Lewontin (1974); subsequently appeared as Chapter 4 in Levins and Lewontin (1985)], as well as in his 1975 paper co-authored with Marcus Feldman, “The Heritability Hang-Up.”

methodology is associated with a very different set of ontological commitments that emphasizes interactive relations among genes. It was first developed by Sewall Wright (and Richard Lewontin) and subsequently defended and articulated powerfully by Wimsatt and Wade, among others.

The non-aggregative mathematical methodology, to use Wimsatt's (1986) term, was probably first articulated in evolutionary genetics by Wright (e.g., Wright 1959, 1969). It was also clearly articulated in Lewontin (1974). In Chapter 6<sup>17</sup> of that book Lewontin describes the work he did with White<sup>18</sup> measuring the frequencies and fitnesses of two polymorphic inversions in the Australian grasshopper, *Moraba scurra*. The details are not important here. What key is that, as Lewontin convincingly showed, "a sufficient dimensionality for describing [and explaining] changes in the frequency of alleles at one locus must involve at least the frequencies of alleles at other loci that interact with it in determining fitness" (Lewontin 1974, p. 281). That is, changes in the frequency of alleles at one locus could not, in this case, be explained solely by the (frequency-dependent) fitnesses accruing to alleles at that locus alone. Rather, fitness was a function of the allele frequencies at two different loci. From this and other examples, Lewontin concludes:

an understanding of evolution along that one dimension requires *first* a synthetic treatment of the genotype and *then* an abstraction of the single system of interest from the complex mass. We cannot reverse the process, in general, building a theory of a complex system by the addition or aggregation of simple ones. (Lewontin 1974, p. 281)

That is, the frequency of all the loci interacting (epistatically) to produce fitness need to be taken into account in order to provide a dynamically sufficient explanation of the changes of gene frequency at each locus.

Wimsatt (1980) provides a powerful philosophical analysis of the sort of error Levins and Lewontin are pointing to in the theory surrounding gene effects. Regarding the systematic theoretical elimination of epistasis, he states: "Illegitimate assumptions of context-independence are a frequent error in reductionist analyses" (Wimsatt 1980, p. 157). In his discussion, Wimsatt is critiquing Williams' (1966) arguments which clearly fall in the category of error that Levins and Lewontin diagnose in statistical methodology concerning main and interaction effects. Wimsatt concludes his analysis of his concerns with Williams' methodology in the following manner:

Williams's remarks suggesting genetic reductionism are better seen as having more import as a kind of genetic bookkeeping than as promising a reductionistic theory of evolutionary change in terms of gene frequencies. The latter is a tempting mirage which vanishes upon closer inspection of the complexities and heuristics of the actual theory. (Wimsatt 1980, p. 158)

Similarly to Levins and Lewontin, Wimsatt concludes that tracking single-locus changes in gene frequencies is simply not dynamically sufficient for explaining evolutionary change whenever there is epistasis for fitness, as there often is in nature.

<sup>17</sup> The title of that chapter is "The Genome as the Unit of Selection."

<sup>18</sup> Lewontin and White (1960); see also Lewontin and Kojima (1960).

More recently, Goodnight and Wade (2000) have proposed an explicit methodology of the kind of modeling alluded to in Lewontin's book and Wimsatt's paper. They write:

If we were to offer a prescription for constructing such models [of epistasis], we would suggest building a model with *only* interactions between genes and, from it, *derive* the mean additive effects of the component genes and the variances about the means. In our view, the additive effect of a gene is strictly a statistical concept, a marginal value that summarizes the web of genetic interactions. It is not a free standing, independent property of the gene itself that can be treated in the theory like a constant with an assigned value. (Goodnight and Wade 2000, p. 319)

By stating that the additive effect is “strictly [merely?] a statistical concept” they imply that that is the *wrong* way to model, and subsequently explain, the highly interactive material system. For Goodnight and Wade, a model containing “*only* interactions between genes” would be a more (empirically and dynamically) adequate explanation of genetic causes in the material system. Goodnight and Wade's specific plea for a mathematical methodology honest to epistasis also includes a way to infer the marginal additive effect. This is done by systematically integrating (mathematically) the potentially complex functions of the gene effects at all other loci.<sup>19</sup> This inferential method is commensurate with Lewontin's comment above regarding the derivation of additive effects as “an abstraction of the single system of interest from the complex mass.”

Note that Goodnight and Wade's proposal also contains ontological assumptions regarding causation, the hierarchical ontological structure of biological systems, and so forth. Certain ontological presuppositions are made ontologically independent here too (even if they are more empirically adequate). Even though Goodnight and Wade are, together with Levins and Lewontin, more critical and aware of the reification of formal models, it seems impossible to avoid making abstract and ideal mathematical models ontologically independent.

But, again, there is hope. The novel ontological elements implied by their methodological proposals provide us with grounds to compare the strengths and weaknesses of different sets of ontological commitments [i.e., the atomistic aggregative one referred to in Levins and Lewontin (1985), pp. 155–156, versus the interactive non-aggregative one implied by Levins and Lewontin, Wimsatt, and Goodnight and Wade]. And, as I see it, the way to engage and potentially solve the process of making scientific models ontologically independent is neither by ignoring reification nor by trying to make it disappear. The former simply amounts to self-deception whereas the latter is impossible. Rather, a variety of different (types of) ontological impositions should be diagnosed and admitted. Then, they should, through dialogue, be compared and checked to see whether there is robustness across ontological commitments. Are the different commitments relevant under different conditions? Can they be generalized, translated or unified?

<sup>19</sup> Wade (pers. comm., January 27, 2006) makes this point in a clear manner: “The selection coefficients experienced by particular genes become functions of the frequencies of alleles at other genes or the frequencies of genotypes at other genes when there is epistasis. If you are tracking genotypes, then you might not try bothering to calculate the marginal fitnesses of alleles at specific genes—if you did, you would find that they are functions of allele or genotype frequencies at other loci.”



## My SMEO-P account of the mathematical modeling process

In what follows, I will present a sketch of a technical theoretical disagreement in evolutionary genetics that I have analyzed in detail elsewhere.<sup>20</sup> Michael Turelli and Steve Frank, two of the most important contemporary evolutionary geneticists, respectively adopt Fisherian and Wrightian theoretical perspectives. They develop radically different mathematical models to explain the nature and behavior of parasite-induced cytoplasmic incompatibility (CI). CI occurs when a sperm from a host with a particular strain of a specific parasite (e.g., *Wolbachia*) fertilizes an egg uninfected with that strain, thereby causing an unviable zygote.

A philosophical analysis of this case from the position of the dialectical view allows us to see how different formal models adopt different sets of ontological assumptions. As I show, the assumptions and the mathematical models are strongly intertwined. The former motivate and legitimate the sorts of parameters and variables employed in the models. Furthermore, these assumptions are clearly imposed on the data as well as on the world. My analysis takes seriously Levins' warnings about making scientific models ontologically independent. In fact, it is in the context of Levins and Lewontin's work that I articulate, and apply, my SMEO-P account of the modeling process [see citation from Levins and Lewontin (1985; Winther (2006, p. 221))]. My account allows us to explicitly track the way ontological assumptions get reified during the modeling process.

I will now turn to the details of my SMEO-P account, which provides a simplified and linear account of the modeling process. It emphasizes the active role that modeling, with its ontological commitments, plays in our understanding of real world ontology.

In the first *set-up* step, the theory provides the frame for setting up the model. Ontological assumptions of various sorts pick out what are interpreted as the important material structures and processes of the system under study. Parameters and variables are assigned to the key properties of the chosen structures and processes. The initial equations of the model, by presenting relations among variables and parameters, capture the relations among these properties. Turelli's Fisherian model assumes that selection is direct and individual-based—there is no kin, let alone group, selection.<sup>21</sup> Frank's Wrightian model presupposes that evolution often occurs in neighborhoods with kin structure.<sup>22</sup> In both cases, their initial parameterizations reflect and reveal the assumptions of their respective frames.

In the second *mathematical manipulation* step, these initial equations, which are seen as basic to the dynamics of the system, are manipulated. Sometimes, surprising results (e.g., unexpected final equations or equilibrium conditions) are derived. The kinds of approximations and idealizations made, and heuristics used, during the mathematical manipulation are also justified by the operative theory.

Let us very briefly compare their respective mathematical manipulations. Turelli's modeling only allows him to track individual-level fitness, measured in terms of the fecundity and subsequent average egg hatch rate of the strain characterizing the individual:

<sup>20</sup> Winther (2006). For further details please see that article.

<sup>21</sup> Turelli (1994).

<sup>22</sup> Frank (1997, 1998).

$$p_{i,t+1} = p_{i,t} F_i (1 - \mu_i) \bar{H}_i / \bar{W} \quad (1)$$

Here,  $F_i$  is the fecundity of strain  $i$  relative to strain 0 (uninfected females),  $\mu_i$  is the fraction of uninfected ova produced by infected females of strain  $i$  (i.e., this is a measure of the lack of fidelity of maternal transmission),  $p_{i,t}$  is the frequency of strain  $i$  in generation  $t$ ,  $\bar{H}_i$  is the average egg hatch rate of strain  $i$ , and  $\bar{W}$  is the mean fitness. This is a well-known population genetic form of selection on a haploid, uniparentally inherited gene, wherein  $F_i(1 - \mu_i)\bar{H}_i/\bar{W}$  is the relative fitness of strain  $i$ . Note the absence of *any* term tracking kin structure, either explicitly or through further manipulation.

On the other hand, Frank develops new formal techniques to measure kin structure. Here is his presentation of the relative fitness function for the parasite:

$$w(x, y) = [(1 - a - bx)(1 - \mu)] / [(1 - l)^2 + l(1 - a - by) + l(1 - l)(1 - y)]. \quad (2)$$

This fitness function measures the fitness of a parasite as a function of  $x$ , which is the continuous trait value of that parasite in the host;  $y$ , which is the average value of that same trait in neighbors with which the host female interacts;  $a$ , which is the absolute fitness cost the parasite exerts on every infected female;  $bx$ , which is the relative fitness cost the parasite has on its host in which  $b$  is a kind of cost term;  $(1 - \mu)$ , which is, as in Turelli's model, the transmission rate of the parasite; and  $l$ , which is the frequency of infection. Frank shows that kin structure can be measured by differentiating Eq. 2 with respect to  $x$  to get a  $dy/dx$  term, which elsewhere he argues is a measure of Hamilton's  $r$ , the coefficient of relatedness.<sup>23</sup> From their mathematical derivations, Turelli and Frank infer radically distinct models for the evolution of CI.

The third *explanatory* step concerns the formal model-data relation, in particular the way that the formal model is used to explain and increase understanding of the structures and processes of nature. There are two kinds of places in which theory imposes itself on data: (1) it strongly determines the form and content of the data and (2) it establishes the relation between itself and the data—i.e., it influences how the data actually bears on the theory as well as how the theory explains the data. In short, theory imposes itself on the data. And, since nature (reality) is seen as the cause of the data, theory *also* imposes itself on nature. In my analysis of the case study, I show how Turelli's mathematical formalisms and ontological commitments did not provide consistent explanations of the processes occurring in nature, whereas Frank's formalisms and ontological commitments provided (perhaps too?) powerful explanations.

The fourth, *objectifying*, step pertains to how nature itself, at the end of the modeling process, is interpreted. This step has been completed when the theory-driven understanding of data and nature is considered objective and theory-independent. The historical process of making abstract scientific formal models ontologically independent through the modeling process has been *forgotten* by the end of this step. The ontological commitments are now understood as really existing in, and as being causally efficacious of, nature. For Turelli, fitness really is individual-based, whereas for Frank, fitness really is intimately related to kin

<sup>23</sup> See Frank (1998).

structure. These comparisons then lead to what should be considered an additional step, rather than an alternative step as I argued in Winther (2006): *pluralize*.<sup>24</sup> In this step there is empirical testing, mathematical translation, and dialogue, among different perspectives in order to rescue the worthwhile aspects of each modelling methodology.

Note that the process of making abstract and ideal mathematical models ontologically independent occurs throughout the *entire* five-step process. Although the third step is where the actual reification occurs, the first two steps are crucial framing steps and the last step is where the consequences are expressed. In the fifth step the process is self-consciously analyzed and critiqued.

These three case studies present evidence of the pertinence and power of the dialectical view as a way of understanding modeling. In addition to evaluating theories through empirical data and mathematical equivalence, it is important to inquire into the nature of their ontological commitments. And it is crucial to develop *alternatives* to particular theories and models.

## Conclusions

As we have seen, the orthodox and dialectical views on theorizing and modeling have distinct commitments to the following issues: (1) the origin of ontological assumptions, (2) the relation of such assumptions to the formal models of the same theory, (3) their use in integrating and negotiating different formal models of distinct theories, (4) their employment in explanatory activity. The orthodox view minimizes the importance of ontological assumptions, sometimes to the extent that they are practically hidden. This corresponds to what it thinks happens in scientific practice. Furthermore, it underestimates both the underdetermination of theory and ontological assumptions by evidence, *and* the ladenness of observation by ontological commitments. This leads to inadequacies, on its part, in describing the process of theorizing.

The dialectical view, as I have attempted to show, takes underdetermination and the ladenness of observation by ontological assumptions very seriously. At least partially for political reasons, it points to the central role played by ontological commitments in theoretical structures and activities. These kinds of commitments go beyond empirical information and are closely tied to the formalisms of the theory. Furthermore, numerous differences, which cannot simply be overcome by empirical testing or the search for mathematical consistency, exist among theories with respect to their ontological assumptions. For example, we have seen how different assumptions regarding the structure of the world are present in each respective alternative of the following two pairs: (1) the aggregative versus the interactive methodologies for modeling gene interaction and, (2) Fisherian versus Wrightian perspectives. The differences between them are found not merely in empirical content/prediction or mathematical theory. In fact, it has been shown that in many respects the Fisherian and Wrightian perspectives are commensurate mathematically, and, furthermore, overlap in numerous empirical regards. However, the explanations stemming from distinct perspectives are different and can reify

<sup>24</sup> I am grateful to my student, Fabrizio Guerrero McManus, for pointing this out to me.

different sets of ontological commitments. Given the importance of ontological commitments, let us therefore search for, and dialogue about, them!

The dialectical view also points to important tensions and contradictions that exist in our scientific theories and theorizing activity. There is a tension between simple theory and complex world that cannot be easily resolved. Difficulties in making the ontological assumptions of any theory match the ontology of the world are rampant. There are also many stresses and strains across different theoretical alternatives: distinct theories are often at cross-purposes and speak past one another and sometimes they even collide in important respects. Finally there is a set of philosophical tensions between the orthodox and dialectical view as described in this article. Following Levins and Lewontin, I have defended the latter view, but understand the importance and strengths of the former. In addition, defending a view is not equivalent to denying its alternative. All these tensions first need to be admitted and diagnosed before they can be approached and overcome.

As I have also argued, dialogue is a key way of overcoming differences, including differences over empirical and mathematical matters. In a number of respects, it is the rational way to overcome differences between perspectives. Rather than remain satisfied with either silence or dialogue stand-stills, it is certainly better to engage in such open dialogue about ontology given that ontological commitments are an integral component of theory.<sup>25</sup>

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<sup>25</sup> Recent work in philosophy of science concerning social epistemology could be useful for further development of the ideas expressed in this paper. See, for example, Longino (1993, 2002); Hacking (2002) and Lloyd (2005).

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